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THE POLITICS OF METHOD IN THE HUMAN SCIENCES

Positivism and Its Epistemological Others

GEORGE STEINMETZ, EDITOR



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ing at cross-purposes to society's collective interests (e.g. drug cartels, corruption rackets) can actually hinder economic and social development" (World Bank 2004).

12 See Block (1990) for a discussion of spot markets versus contracts. There is also an enormous literature on the role of trust in social capital; see especially Fukuyama (1995a), Gambetta (1988), Warren (1999).

13 A Marxist or even an institutionalist definition of capital would be very different. A coherent Marxist perspective could not take on board the notion of social capital in the first place because Marxism and institutionalism *begin* from the premise that capital is constitutively social in the *first* place (see, e.g., Block 1990; Fine 2001). For Marxism, the term social capital is thus redundant. For social capital to make sense, capital would have to first be depleted of its social nature only to be a feeble conception of social, now conceived as the aggregate of individual actions, choice, and decision-theoretics.

14 By external I include the educational skills of human capital. They do not count capital if they're simply knocking about in your brain while watching television, not being put to any productive use. Only the intention to invest them endows them with the status of human capital.

15 Between editions, Randall Collins's widely used *Three Sociological Traditions* (1985) became *Four Sociological Traditions* (1997) with the addition of utilitarianism.

16 I have long thought of social capital theory as a Rolodex approach to social relations and social action, but always in irony, playfulness, and even parody. Reality—without a trace of irony—now imitates the art of caricature: "We assume that individual social capital includes both intrinsic abilities . . . and the results of social capital investments [e.g., a large Rolodex]" (F438).

17 See Bowles and Gintis (2002) for a brilliant critique of the "economic approach."

18 The image of elephants brushing off flies in reference to the contempt economists have long held toward their sociological challenges was given to me by Robert Solow (personal communication, 1990).

19 There is a vast literature on social capital and development. See especially Evans (1996), Fine (2001), McMichael (1998), and Woolcock (1998).

20 Among the universe of "Putnamian" literature on his Italian research, especially important are Levi (1996) and Tarrow (1996).

21 Hegel, of course, reversed Locke and reinstated a triadic conception of social organization. But paradoxically, this did not create civil society as the sphere of citizenship but as the site of the market tout court. For the most important contemporary treatise on civil society in general and Hegel's understanding of it in particular, see J. Cohen and Arato (1992).

22 For a sampling of the relevant neoconservative literature, see also Demuth and Kristol (1995); Gershon (1996a, 1996 b); Habermas (1989); Nicholson (1989); Kristol (1995).

23 "Concerns about social capital thus represent a return to classical sociological preoccupations with community as the foundation of social solidarity and social cohesion" (Gamarnikow and Green 1999, 111). See also Bowles (1999) and N. Rose (1999, 2000).

24 On exit and voice, see Hirschman (1970).

SOCIOLOGY

Scientific Authority and the Transition to Post-Fordism: The Plausibility of Positivism in U.S. Sociology since 1945

GEORGE STEINMETZ

From the Cold War to the "Science Wars" in U.S. Sociology

Exoticize the domestic.—Pierre Bourdieu, *Homo Academicus*

It has become manifest that in each age there is a system of science which rests upon a set of assumptions, usually implicit and seldom questioned by the scientists of the time.—Robert Merton, "On Sociological Theories of the Middle Range"

According to accounts in the mass media and the academic press, the social sciences have experienced a sharp increase in epistemological uncertainty in recent years. Various writers have described or decried moves toward relativism, philosophical conventionalism, and skepticism about truth and science itself, including social science (see especially Sokal and Bricmont 1998).[1] Historians went through a linguistic or cultural turn starting in the 1980s, accompanied by a distancing from the more social-scientific models and methods that had characterized their disciplines in the 1960s and 1970s (Sewell, this volume; Bonnell and Hunt 1999). Among the various social sciences, cultural anthropology has seemed especially prone to doubts about the possibility of choosing rationally among competing theories and interpretations. Historians and cultural anthropologists often reject the ideal of explanation tout court.[2] Cultural anthropologists have expressed increasing skepticism about the possibility of understanding other cultures—the very goal that defined their field in earlier decades. To critics, these trends threaten the tenuous and only recently attained scientific status of these endeavors.

What about sociology? Viewed from the outside, it is tempting to see U.S. sociology as being similarly engulfed by skeptical waves of epistemic self-reflexivity. A *New York Times* report on the annual meetings of the

American Sociological Association in 2000 was written in much the same tone as reviews of Modern Language Association meetings during the 1990s, suggesting a discipline engulfed by political correctness that had lost touch with its scholarly calling.[3] A glance at the sociology section of any local bookstore similarly suggests that this discipline is a hotbed of social constructionism and allied forms of nonpositivism. Many first-year graduate students in sociology arrive with strong political agendas and commitments to standpoint epistemology. They clearly expect sociology to be friendly terrain.

It is slightly startling, then, to realize that the leading journals and departments in U.S. sociology have remained almost pristinely aloof from these battles that are supposed to be roiling them. Epistemological questioning and self-reflexivity are still rare in the pages of the main sociology journals in the United States, suggesting a discipline that is secure in its scientific identity and assumptions. In the leading journals and in the curricula of the leading departments, explicitly nonpositivist positions are only tentatively articulated, with the exception of a few subfields such as the sociology of science and culture and ethnic and minority studies. U.S. sociology still seems to be operating according to a basically positivist framework, perhaps a crypto-positivist one, if I can use that term without any conspiratorial connotations. This diagnosis is especially accurate if we define positivism in terms of the tradition running from Locke and Hume in the seventeenth and eighteenth centuries to John Stuart Mill, Ernst Mach, and Karl Pearson in the nineteenth, through the logical positivists and their sociological acolytes in the twentieth century (e.g., Lundberg 1939a, 1939b, 1955; Blalock 1964, 1969; Hanushek and Jackson 1977; and even Arthur Stinchcombe 1968 in his earlier phase). Positivist philosophers like Carl Hempel (1948/1965, 1966) and Karl Popper (a positivist malgré lui; see 1934/1992, 1976b; Adorno 1969/1976) were read by sociologists, and sometimes addressed them directly (e.g., Popper 1957/1991; Neurath 1931/1973; Nagel 1961/1979, ch. 13).[4]

It has been impossible for mainstream sociology to completely ignore the long-term decline of positivism as a vital position in philosophy or the more contemporary epistemological upheaval in the human sciences. There have been repeated intellectual insurgencies and challenges since the late 1960s against the version of positivism that became dominant in sociology after World War II. One effect of these successive, overlapping, and partially cumulative waves of criticism was that the term positivism itself (and allied terms such as empiricism, discussed below) took on overwhelmingly negative and pejorative connotations. Mainstream sociology has mounted three kinds of response to these critics in recent decades. First, many sociologists have continued to work in the same way as before but have simply renamed their positions or ceased to describe their own work in epistemological terms at all. A second response has been to update sociological positivism in ways recommended by Carnap and the logical positivists after 1939 (see the introduction to this volume and H. Putnam 2002), integrating theoretical and unobservable constructs into a covering law framework. This involved the elaboration of universal social laws linking theoretical constructs to empirical events. A third response has been to separate the antipositivist critics' substantive topics of research from their epistemological challenges and to formulate positivist approaches to the new topics. Finally, some sociologists have taken the criticisms seriously and re-examined their positions.[5]

Because the first response is the most typical, positivist positions have become much more difficult to track since 1945. This differs from the situation in the 1930s, when positivism was openly defended and vigorously debated in U.S. sociology. Yet, even where the explicit label is disavowed or partial amendments are made to the basic position, it is still possible to characterize much of the field today as fundamentally positivist. This framework has remained dominant despite the antipositivist wave of the late 1960s and early 1970s, even if it is no longer "doxic." A new situation has arisen in the past decade, however, in which positivism is again forced to defend itself openly as a kind of disciplinary *orthodoxy* rather than simply being taken for granted as the way things are done (doxa).[6]

The initial paradox driving this essay is thus the depth of the positivist unconscious in U.S. sociology and the fact that this discipline has been penetrated to a lesser extent by antipositivist and postpositivist positions than some of the other human sciences.[7] To understand U.S. sociology's rather tenacious positivism, I argue, we need to reconstruct the earlier epistemological conflicts in the discipline and their resolutions, especially since the middle of the twentieth century. The period 1945–1965 was a foundational moment whose effects continue to be felt, even as many of the social conditions that originally supported the triumph of positivism in sociology have fallen away.

The first section of the paper offers a brief working definition of *methodological positivism*,[8] the specific cluster of ontological, epistemological, and methodological assumptions that has prevailed in U.S. sociology for the past half century. Drawing on discussions in the critical realist philosophy of science, I define this position as a combination of an empiricist

ontology, a positivist epistemology, and a scientistic version of naturalism. The reason for initially defining this conglomerate object in such abstract terms is that it allows me to identify positivist positions even where writers eschew the self-description. It also permits me to distinguish different sorts of breaks with the earlier orthodoxy. It should be noted, however, that this is not an "ideal type" definition but one that attempts to extract the essential elements of a scientific formation from the writings and practices of sociologists.

The core of the paper is the second part, which proposes a historical sociology of the consolidation of positivist dominance in U.S. sociology during the cold war, roughly 1945 to 1989, with a focus on the first two decades. I argue that this success resulted from a contingent conjuncture of events and social structures located both in the discipline and beyond it. Although I do not ignore the role of the university and scientific fields and the flows of private and public funding, my focus is one that has been ignored in the existing work on the history of sociology: the impact of large-scale social structural processes and cultural discourses on sociologists' sense of the plausibility of different ways of thinking about the social. A useful framework for making sense of patterns of macrosocial and discursive change and their implications for perceptions of social ontology and epistemology is *regulation theory*.[9] Methodological positivism's picture of the social resonated in various ways with the Fordist mode of societal regulation, which also crystallized in the two decades after the war. While various Marxists have thematized for a general correspondence between positivist epistemology and capitalism (Lukács 1922/1968a) or late capitalism (Adorno 1969/1976), I am arguing for a much tighter and more historically specific fit between Fordism and the specific features of the version of methodological positivism that became dominant in sociology during the 1950s. Indeed, it is possible that the current post-Fordist mode of regulation, while thoroughly capitalist, favors *postpositivist* over positivist forms of social epistemology, a hypothesis that I explore in the conclusion.

My account also attends briefly to the growth of state funding for social research, which gave backing to the positivist wing that already existed in U.S. sociology before the war. The circulation of key actors between government agencies and sociology departments was critical in underwriting the ascendance of methodological positivism at this time (S. Turner and Turner 1990). A final factor contributing to the consolidation of positivist orthodoxy was the emotional-political landscape of the middle decades of the century, which brought certain critiques of normal positive science into disagreeable association with fascism and communism.[10]

The main outcome of the postwar conjuncture was the successful institutionalization of a system of some explicit, but most of them implicit, rules that guided the socialization of sociologists, the creation of knowledge, the channeling of resources, and the drawing of disciplinary boundaries. These rules were firmly consolidated despite scattered and quixotic resistance (e.g., Mills 1959).

In the last section of this essay I briefly discuss a series of challenges to methodological positivism that have emerged in sociology since the late 1960s, beginning with neo-Marxism and (second-wave) feminism, passing through the historical and cultural turns, and culminating in the current "epistemological" turn. This discussion parallels the section that precedes it in asking how field-specific and broader sociocultural conditions shaped the emergence and fate of these challengers to methodological positivism. Although none of these movements has succeeded yet in dislodging the central position of methodological positivism, I consider three possible future scenarios for sociology. In one, the current discontent with positivism will bundle together the intellectual resources generated by the earlier critical movements and spark a shift from quantity into quality. Another future would find nonpositivist approaches taking their place alongside methodologically positivist ones in an epistemically fragmented and pluralistic field. But this is unlikely to be a stable solution, as most fields tend to reconsolidate along an axis of dominant versus dominated poles. A third possibility is that the current epistemological turn will suffer the same fate as earlier moments of critique. Arguments that are incommensurate with methodological positivism will be ignored or ridiculed, or nonpositivists will be herded into a marginal subfield that has science itself as its specific object, creating a sort of quarantine zone for epistemological questions.

My own approach to historical reconstruction attempts to exemplify a methodological counterposition—a ridge riding, to use Alvin Gouldner's term—between the Scylla of positivism and the Charybdis of conventionalist constructivism. Key terms in this approach are *contingency* and *conjuncture*. This method, associated inter alia with contemporary critical realism, avoids idealism by embracing causal determinism and by refusing to equate "concept dependency" (defined below) with the view that society consists "entirely of concepts" (New 1998, 359).[11] It suggests that one can combine post-Kuhnian *epistemological relativism*, that is, the recogni-

tion that science is a thoroughly social and historical process, with *judgmental rationalism* about theory choice.[12]

The Contours of Methodological Positivism in Postwar U.S. Sociology

In this section I draw on the (mainly critical realist) philosophy of science for definitions of positivism, empiricism, and scientistic naturalism to provide a framework for analyzing the ongoing practices and methodological statements of U.S. sociologists. Without a systematic framework for diagnosing the underlying ontological and epistemological assumptions of social research, any classification of the latter becomes descriptive and arbitrary. In addition to the more abstract literature on science, my working definition of methodological positivism is extracted from programmatic and empirical work by sociologists, none of which can be analyzed in any detail here.[13]

Defining positivism in philosophical terms allows us to avoid using the term as an epithet. Positivism, as Sandra Harding emphasizes in her contribution to this volume, is a well-established position with a distinguished tradition (Ayer 1959; Kolakowski 1966/1968; Giere and Richardson 1996). This legacy has special importance for sociology due to the role of Auguste Comte (1975) in coining not only the term positivism but also "sociology" itself (Halfpenny 1982; Scharff 1995). Yet self-identification as positivist has become exceedingly rare in sociology.[14] The more popular philosophical position in current sociology, at least for those who explicitly embrace one, is some version of "constructivism" or "constructionism," even if, as Sergio Sismondo (1993, 1996) and Ian Hacking (1999) have shown, these terms are used to cover a vast array of often mutually exclusive viewpoints. Many constructivist views are coextensive with the widely accepted idea that human practices are not completely determined by biology or other physical causes. More typical than affiliation with constructivism or any other philosophical position, however, is simple avoidance of all philosophical terminology. The main source for the communication of positivist orthodoxy to sociology students for many years has therefore not been explicitly philosophical texts but instructions in statistics and methods construed as purely technical matter.[15] The widespread hostility to more general discussions, the reluctance to engage in them, is suggestive of their central importance (*pace* Foucault). A coherent definition of positivism will allow us to identify the metaphysical assumptions underlying texts that present themselves as unconcerned with such and to distinguish between sociologists' explicit "methodological" descriptions of their own positions and the assumptions implicitly informing their research.[16]

Methodological positivism can mainly be defined as a cluster of specific ontological and epistemological positions. This framework has also tended historically to endorse specific research techniques, but it is by no means limited to quantitative methods.[17] Methodological positivism contains first of all a set of empiricist *ontological* assumptions about the nature of social reality, objects, and causality. Closely articulated with these ontological foundations but nonetheless distinct are properly positivist *epistemological* precepts concerning the way social facts can be known.[18] The third component is a *scientistic-naturalist* belief in the identity of the social and natural sciences. Scientism has important implications for ontology and epistemology and for methods and modes of presentation.

Empiricism can best be understood as a position within ontology according to which "there is no real difference between 'essence' and 'phenomenon' " (Kolakowski 1966/1968, 3). This results in a proscription on theoretical, abstract, and unobservable objects or entities. Psychological behaviorism, which claims that "we do not need to worry about what actors think they are doing to explain their behaviour" (S. Smith 1996, 36), is thus empiricist. Crucially, empiricism can be both realist and antirealist about the objects it accepts into scientific statements. Indeed, much of the confusion about the status of *critical* realism by those unfamiliar with it results from a conflation of realism with *empirical* realism. Even Rudolf Carnap, one of the central proponents of logical positivism, insisted, "I am also a realist," so long as " 'realism' is understood as preference for the reistic language" (which he defined as "language describing intersubjectively *observable*, spatio-temporally localized things or events"; 1963b, 870, my emphasis). As Collier (1994, 26) points out, empirical realism is simply "realism about the concrete results of experience—an empirical realist believes that chickens and blizzards and magnetic fields exist independently of the observer." Empirical realism is a label applied not just to anyone who believes that concrete objects are real, however, but also to "one who also *denies* the reality of *underlying* mechanisms, structures, etc., which don't appear in experience, but cause phenomena that do." Those who accept the existence of such underlying and determining structures, objects, or mechanisms are *depth realists*. Empiricism thus stands opposed to depth realism, which begins from a vertically "stratified" picture of reality. Such a vertical stratification allows for disjunctures between the level of underlying causal structures and the level of observable phe-

nomena. Empiricism is therefore better described not as antirealist but as opposed to depth realism.

Positivism is a venerable position in epistemology that scientific explanations take the general form "if A then B," or more elaborate versions of these Humean "constant conjunctions." The logical positivists and their successors (Hempel, Popper) renamed these "covering laws." Once again, ontology and epistemology appear to imply one another almost inexorably. Just as empiricist ontology restricted the range of permissible knowledge statements, so positivist epistemology suggests that reality is ontologically structured as a closed system rather than an open one, as this alone would make constant conjunctions possible. If the cause (explanans) is uniform across all instances of a given explanandum, it can be either empirical, as in classical logical positivism, or theoretical, as in later versions of logical positivism and social science adaptions thereof (Ayer 1946; Blalock 1964, 19; Hanushek and Jackson 1977).

This is where the difference between generic depth realism and "transcendental" realism enters in, allowing us to specify the ontological limit conditions of positivist epistemology. The first limit condition is the openness of the system, in this case, the openness of the social system. How can we establish this openness? Bhaskar's (1986, 110) transcendental deduction of the ontological conditions of possibility of the natural science experiment points to the necessary existence of a "rainforestlike profusion" of causal mechanisms (a *horizontal* stratification); this suggests that a multiplicity of mechanisms will typically combine in conjunctural ways to produce empirical events (Collier, this volume). Of course, we cannot conduct true experiments in the social sciences both for the fundamental reason that it is impossible to bracket the effectivity of all causal mechanisms other than those of scientific interest and for the ethical-practical reason that it is indefensible to try to "rerun" the most significant events of human history, which are usually tragic. This means that the openness of the social cannot be *deduced* in the same way as in the natural sciences. But we do know that the level of the social is *emergent* from the biological and physical (Collier 1994, 107–118, 156–160). Regardless of what we do or do not know about the internal structuring of the social, then, it is safe to assume that the genesis of social events is at least as complex as the production of natural events. The sheer mass of evidence concerning social scientists' inability to predict world-historical events—most recently, the collapse of communism and the attacks of September 11, 2001—is also suggestive of social system openness. This failure to predict should not be read as a sign of these sciences' "immaturity," as they are perfectly capable

of generating convincing conjunctural explanations of past events. Generic depth realism is compatible with ontologies of system closure, but transcendental or critical realism is not.

Sociology's methodological positivism has also embraced a strong version of scientific naturalism, that is, the claim that the social world can be studied in the same general manner as the natural world (Bhaskar 1979). *Scientism* is a more stringent variant of naturalism that "claims a complete unity" between the natural and social sciences (Bhaskar 1994, 89). The logical positivist thesis of the unity of the sciences (Carnap 1934; Hacking 1996) was taken literally by these sociologists.

Scientism has supplemental implications for ontology, epistemology, and methodology. Because quantification, experiment, and prediction are central to the natural sciences, it is assumed that these are also de rigeur for sociology. With regard to social ontology, scientism led sociologists to overlook the *concept, time,* and *space dependence* of social structures, as it held that natural causal structures were not variable across time, space, and interactional contexts.

Concept dependency means that social practices and structures are inextricably bound up with peoples' interpretations of the world and their own activities. The thesis of concept dependency, in other words, is opposed to the *dichotomy* of things (including material human practices) and words/ideas, even if it is willing to distinguish between the two analytically. This blending of things and words/ideas differentiates the natural from the human sciences and undermines any across-the-board scientism. With respect to this point at least there is agreement between the hermeneutic tradition (Dilthey 1910; Gadamer 1975) and poststructuralism (Laclau and Mouffe 1987) in the social sciences. One can recall Charles Taylor's (1979/1987) effective Saussurian image of the meaningful dimension of human practice and its material substrate as being as inseparable as two sides of a sheet of paper. Geertz's (1973d, 6–7) famous example of the indeterminate meaning of a rapid contraction of the eyelids makes a similar point. Without reconstructing the preinterpreted meaning of social practices, we literally will not be able to determine what kinds of practices they are, vitiating any attempt at social explanation, much less comparison.

The denial of concept dependency appears to receive support from the real world insofar as most natural phenomena are not concept-dependent in the same way as are human phenomena. Hacking (1995, 390; 1999) speaks of the *looping effects* of theoretical classifications on social realities, a "feedback mechanism" that distinguishes natural and human kinds. Natural objects are "intransitive," in Bhaskar's terms; or as Hacking puts it,

quarks "do not form an *interactive kind*"; they "are not aware that they are quarks and are not altered simply by being classified as quarks" (1999, 32).

Sociology's scientism also led to an underestimation of the *relationality* of social structures, something always emphasized by critical realism, and of the time and space dependence of those structures. Methodological positivism's orientation toward general laws received its warrant from the apparent unchangeability of causal mechanisms in the natural world across time and space.[19] Natural scientists acknowledge that mechanisms like natural selection may not be valid across all time, of course, and physicists went even farther in the twentieth century (H. Putnam 1990c). But the idea that different places, cultures, or historical epochs might be organized differently, that modern European or U.S. analytic categories might not apply to the feudal past or to Chinese or African society, was anathema to mainstream methodological positivism.[20]

A further aspect of sociological scientism was its adherence to the fact/value dichotomy (discussed by Mihic, Engelmann, and Wingrove in this volume). Value-freedom (*Wertfreiheit*) was enshrined in some of the earliest statements of sociology's mission (Weber 1949).[21] It was reasserted after World War II and in subsequent periods of epistemological-political unrest (e.g., Coleman 1992).[22]

Research methods in the narrower sense are selected and limited by these scientistic assumptions. The prominence of quantitative methods in the natural sciences led social scientists to privilege them, even though qualitative data and methods are also fully compatible with positivism and empiricism (e.g., Barton and Lazarsfeld 1956), just as quantitative data and methods are compatible with nonpositivist epistemologies (e.g., Bourdieu 1979/1984). Nor should it be assumed that quantitative methods are necessarily associated with empiricism. Hayek already endorsed quantification for depth-realist reasons, writing that "the main importance of [the] quantitative nature of most natural sciences is [not] their greater precision" but rather "substituting for the classification of events which our senses provide another based on relations established by systematic testing and experiment" (1942, 275). As mentioned in the introduction to this volume, contemporary statistical approaches allow for "latent" (theoretical-unobservable) constructs.

The full-fledged methodological positivist paradigm did, in fact, privilege quantification. But in sociology (as in political science, but perhaps not in economics, as Lawson argues here), the quantitative/mathematical rule has been easier to bend than the positivist commitment to covering laws. There has also been a continual return to the idea of experimentation in the social sciences, even though both critical realists and some philosophical positivists have rejected true experiments as ontologically impossible in these fields.

Methodological positivism in sociology has also privileged specific ways of representing social reality. Articles have been preferred to books; statistical and tabular forms to textual ones; analytic genres of writing to narrative or experimental forms; and written forms of inscription to visual ones. Authorial voice is disdained as subjective and the voiceless style felt to be more scientific. Multiple rather than single authorship is the norm (Hollinger 1996, 151, n. 64), providing a synecdoche of the "scientific community" of peers discussed by Michael Polanyi, Robert Merton, Thomas Kuhn, and others (see Mirowski, this volume).

In sum, methodological positivism combines positivism, empiricism, and scientism; the last encompasses the fact/value dichotomy, mimicry of hard science styles of presentation, a preference for quantification, and a belief in the concept, space, and time independence of social mechanisms. This cluster of premises has broadly defined the dominant position in U.S. sociology since the 1950s. But the specific combination of assumptions has varied across subperiod, subfield, setting, from one sociologist to the next, and even within individual texts.[23] The common denominator, the belief whose abandonment would signal a fundamental change in this formation, is the orientation to regularity determinism or covering laws. All of the other features may define methodological positivism's center of gravity, its modal type, but they are less essential.

What does this definition leave out? Some would argue that mainstream U.S. sociology in this period was unified as much by a specific understanding of its object domain, by adherence to a shared theoretical orientation, or by its orientation to a set of canonical texts. Yet methodological positivism was more significant for the practical unification of the sociological field. As Frisby and Sayer (1986) have argued, mainstream sociology has actually had a very weak conception of its supposed subject matter, society or the social. No general object of analysis united the discipline, despite the best efforts of the Parsonians and Marxists, who pursued parallel goals with respect to this issue. It is paradoxical that a field supposedly defined by the social has tended to adopt an atomized view of human life (Emirbayer 1997).

Nor was sociology integrated by adherence to any specific substantive theoretical paradigm. While Parsonianism may have unified many U.S. sociologists professionally and politically in the 1950s and early 1960s, it did not lend more than a superficial concurrence to their actual research

and writing. There were as many dissenters as adherents to the supposedly all-powerful Parsons and even more who simply seemed oblivious. Kingsley Davis's 1959 presidential address to the American Sociological Association argued that structural-functional analysis was neither an agreed upon method nor a distinct theory but simply a "myth" or another name for explaining social phenomena socially (Davis 1960).[24] George Homans displaced Parsons in the sociology citation counts in the second half of the 1960s (Chriss 1995, 48–49). Of course, theoretical diversity is quite compatible with dominance by one or another specific theory. Epistemology and method were more fundamental than theory. Indeed, methodological positivism could be reconciled with theories ranging from Marx to Parsons.

A final alternative is to read the history of U.S. sociology in terms of canon formation. Prewar theorists like Sorokin (1928) and Parsons (1937/1968) actively excluded Marx and Engels from any claim to a place in sociology's canon.[25] Connell (1997) has argued that U.S. sociology was unified after 1945 by the creation of a canon that suppressed an earlier orientation toward the colonized and non-Western world, narrowing the discipline's vision to Eurocentric classics and metropolitan problems. One problem with this interpretation is that most sociologists after the war were little concerned with theoretical texts per se. Merton (1936/1968c) included exculpatory comments for his atheoretical colleagues, and Lazarsfeld (cited in Mills 1959, 60) juxtaposed the unspecialized, individual "social philosophers" who wrote books to the practical and scientific sociologists. Yet Connell (1997, 1511) is certainly correct in his claim that U.S. sociology became "centered on difference and disorder within the metropole" and turned away from problems of "global expansion and colonization," and that the construction of a European-theory-centered canon focused on problems of *European* modernity was related to this move. As I argue below, this narrowing of sociology's focus to Europe and the United States was related to the consolidation of methodological positivism, specifically to the division between the "nomothetic" social science disciplines and the "idiographic" fields of area studies.

Although few sociologists nowadays embrace the positivist label, many defend or rely on positions that closely match the definition of methodological positivism set out here.[26] This should not surprise us. As Jürgen Habermas (1969/1976, 1971) has noted, positivism was committed from Comte onward to avoiding all talk of epistemology and metaphysics and portraying itself as having transcended philosophy altogether. Deconstructionists have argued that the dominant pole in such binary oppositions (positivist versus nonpositivist) has the luxury of remaining unmarked. Methodological positivism became so prevalent in sociology during the 1950s and 1960s that it marginalized not only other positions but also philosophical discussion per se. C. Wright Mills's *The Sociological Imagination* (1959) is perhaps the only example of a direct and sustained critique of methodological positivism from within U.S. sociology during the 1950s (and even that book makes compromises with empiricism).

By speaking of the dominance of positivism I do not want to suggest that other approaches have been absent or unthinkable. The image I want to convey instead is closer to Bourdieu's notion of a "field" of diverse positions and viewpoints that is clearly "structured in dominance." As Bourdieu (1981, 260) argues, "A survey on power in the scientific field could perfectly well consist of . . . epistemological questions alone."[27] Certain ideological-methodological positions have more (field-specific) scientific capital than others. All players in the field recognize a common definition of scientific authority and competence, of distinguished and less distinguished positions, even if they develop a taste for their own dominated position within this hierarchy (see Bourdieu 1981, 1984/1988, 1985, 1986).[28] The resources that have allowed sociology to slowly begin thinking its way out of methodological positivism have often been literally foreign, emerging from other national contexts or disciplines. "Indigenous" sources for these arguments were also available, but the fact that these positions had been subordinated for so long lent them an air of lifelessness and defeat, leading critics to look for alternative backing elsewhere.[29]

U.S. Sociology's Postwar *Refondazione* (Refoundation) and the Consolidation of Methodological Positivist Orthodoxy

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.—Charles Lindblom, "Political Science in the 1940s and 1950s"

A fruitful starting point for a sociology of sociology is thus the idea of the scientific field. A *settled* field is internally heterogeneous but at the same time biased toward a shared definition of distinction. The agreement by all actors on a certain definition of value is based on dynamics of mutual *recognition* (Steinmetz 2002). There is thus a basic, if tacit, level of agreement within settled fields. This consensus undergirds disagreements about taste, which are rooted in variations of individual and class/group habitus and capital. This means we should never expect absolute epistemological

homogeneity within a scientific field. The presence of nonpositivist positions is fully compatible with positivist dominance (and vice versa).

Why do certain epistemological and theoretical preferences become entrenched within a given scientific field? This is similar to the question broached by Kuhn (1962/1970) in his famous intervention on scientific paradigms and scientific revolutions, except that Kuhn was more concerned with full-fledged theories than with more diffuse assumptions. The determinants of scientific doxa can be described heuristically as external or internal to the scientific field (or alternatively, as macroscopic versus microscopic). In explanations of the development of science, the adjective "external" has typically been used to refer to all those influences on science that are not intellectual (R. Hall 1963; Breslau 2003). "Internalist" historical analyses are those concerned with the correspondence between a scientific theory and its object, and that hold that "scientists themselves come gradually to perceive this trajectory over the course of history as they 'learn to learn' about the world" (Fuller 1993, 34). I draw the boundary between internal and external influences at a slightly different point, however, with the *inside* encompassing subfields, disciplines, universities, and research institutions, in addition to the storied point of contact between the scientist and his or her object. The *outside* then refers to all other sociocultural factors that influence science, including branches of the government concerned with science and private funding agencies, as well as macrosocial external factors like those associated with Fordism and post-Fordism.

In much of the recent sociology of science there has been a tendency to privilege what I am calling the internal level—the field of science itself and its immediate extensions—rather than its relations with its environment (but see Merton 1936/1968c; Gouldner 1965, 1970). Sociological research on science in the 1950s and early 1960s was strongly focused on the idea of the scientific community as "a kind of social and cultural system with its own powers of self-maintenance and self-regulation" (Shils 1954, cited in Hollinger 1996, 109). Although Kuhn may have been less convinced than earlier writers of the "judgmental rationality" of the choices of the scientific community, he still emphasized the latter as driving theory choice. Indeed, by breaking with the idea that theory choice is driven by correspondence with its object, Kuhn severed yet another link between scientific theory and science's outside. Sociologists have grown increasingly skeptical about the "gentlemanly" (or "communal") qualities of this "scientific community," but many of them continue to train their gaze on the internal features of science. They have emphasized competition among

scientists (Ben-David 1960), patronage and gate keeping (Cole and Cole 1973, 1983), the enlisting of allies (Latour 1987),[30] personality conflicts (Abbott 1999), and the pressures of professional legitimation[31] in accounting for scientific behavior and change. Luhmann's (1989, ch. 12) theory, in which the border between autopoeitic science and its social environment becomes densely compacted and difficult to permeate except via indirect perturbation, is the apotheosis of this science-centered view of science.

How can internalist approaches explain fundamental agreement in scientific fields? One possibility, suggested by Camic (1995; Camic and Xie 1994), is that new sciences tend to emulate the currently most prestigious discipline. The problem is that other newcomer disciplines that emerged at the end of the nineteenth century, most notably anthropology, did not follow the same path as sociology (Stocking 2001a; Zimmerman 2001; Keane, this volume). Moreover, if we place sociology in a higher-order metafield of the sciences, it is not transhistorically obvious that sociologists would necessarily experience "physics envy" or "economics envy." In earlier periods, and in other national settings, philosophy, not physics (or economics), has figured as the queen of the disciplines. Camic's account thus throws us back on a historically and logically prior question: Why do fields differ in their selection of disciplinary "ego ideals"?

Bourdieu's approach seems to promise some level of generality with respect to these questions. For Bourdieu (1979/1984), the most distinguished positions in any given field will be the ones that exhibit the greatest "distance from necessity." In the arts this seems to point to more abstract or minimalist forms, or to a style of viewing that privileges form over content (but see Barthes 1980). In the field of sports (Wacquant 2000), distance from necessity accrues to bodily movements that are more artificial and are distinct from street gestures, or to games that are wasteful of time (sailing) and space (golf). But how could this line of reasoning account for the prestige of positivism in sociology? While *empiricism* is more concrete than depth realism, and thus in certain respects closer to "necessity," *positivism* can be seen as less concerned with the messy details of the individual case, which it subsumes under a general covering law. Quantification, as Hayek (1942, 275) suggested, can be described as a substitute for the cruder "classification of events which our senses provide." Although "distance from necessity" need not be defined in aesthetic terms, it is notable that sociological positivists spontaneously adhere to an aestheticizing language of "elegance" when describing statistical models or "parsimonious" explanations.

And so on. The problem with this Bourdieuian approach is its port-

manteau and ad hoc 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 theory is best equipped to account for the workings of consolidated, settled fields, but its analysis of the origins or substantive contents of any given "settlement" is less compelling. At best, it indicates what kinds of additional ideological framing are likely to be directed at practices and perceptions whose distinction is *already* being promoted. It points to the general kinds of arguments that will be mobilized by anyone trying to control a field. But Bourdieu cannot explain why certain definitions of distinction will be more successful than others.[32]

We are again thrown back on the problem: Why was positivism so successful in its bid for leadership and distinction after 1945? Obviously, economic (as well as political and social) capital can enhance the value of cultural capital. Research on the politics of the cold war and academia focuses on the role of the national security state in promoting and suppressing certain kinds of science and forging a military-academic complex (Leslie 1993; Robin 2001; Chomsky et al. 1997). Historians of postwar U.S. social science have often traced its scientistic turn to the massive influx of resources offered by private foundations and government agencies to those who were willing to configure their work along the lines of the natural sciences (Gouldner 1970; Schwendinger and Schwendinger 1974; S. Turner and Turner 1990; Ross 1991; Ahmad 1991; Fisher 1993; Kleinman 1995). While this was undoubtedly crucial, there is still "a good bit of disagreement and uncertainty about just what effect the interests of federal and private patrons have on the intellectual shape of . . . science and scholarship" (Hollinger 1996, 142, n. 23).[33] One reason this account is obviously inadequate is that resources were already available during the prewar period, at least from private foundations, and still exist today. Yet the social science disciplines, including sociology, have varied across this period in their level of adherence to methodological positivism as well as theoretical fashions. Taken alone, funding cannot account for the waxing and waning of methodological positivism.[34]

The externalist theoretical approaches that I called "social-epochal" or "macrosocial" in the introduction to this volume offer an alternative, or at least a necessary supplement, to the currently popular approaches to the study of social science. Social-epochal approaches need not negate the existence and causal importance of intrascientific dynamics like competition, hierarchy, enlisting allies, and "trials of strength," but they shed light on the sources of the more widespread and often implicit ideas shared by all of the actors in a settled scientific field. Nor do such accounts have to ignore the role of class, but they are less interested in correlating particular class positions with specific ideologies.[35] Instead, they look at the production of more general ideological forms, the scientific aspects of the "cultural dominant" (Jameson 1984).

An early example of this approach is Lukács's analysis of reification as a universal condition besetting the minds of capitalists and workers alike, even if the latter have at least a chance of extricating themselves from it. Historians and sociologists have also dealt with "external" determinants of spontaneous scientific consensus. Following Max Weber, Merton (1936/1968b) traced the "enhanced cultivation of science" and the specific "attitudes of scientists to their work" in later seventeenth-century England to the ascetic ethic of Puritanism.[36] Foucault (1966/1970) traced the sources of writing in a wide array of classical scientific fields to a period-specific deep structure, which he called *episteme*. Shapin's (1994) study of the same period highlighted the role of the ideology of a community of *gentlemen* and of trust in the origins of natural science. Haraway (1989) ties the history of twentieth-century primatology to broader histories of capitalism, race, and gender. Ulrich Beck's (1986/1992; Beck, Giddens, and Lash 1994) theory of risk society and reflexive modernization takes a broadly similar analytic approach. Beck traces the new social risks largely to the runaway logics of contemporary capitalism, but he views their impact as more general than the older forms of exploitation and domination, which were stratified by social class. New types of risk, including pollution, nuclear proliferation, and heightened levels of competition and individualization, are spread across the entire social surface rather than being specific to any one group (Beck 1986/1992, 53; Heitmeyer 1992). Novel forms of anxious subjectivity and resistance arise in response. Beck's approach is relevant in the present context because he discusses the impact of "risk society" on scientific ideologies and practices. The expansion of science and its dangers encourages critique, demystification, and greater reflexivity with regard to expert practice and science.[37]

In another paper (Steinmetz 2004b), I show that most of the discursive and institutional building blocks for methodological positivism were already in place in U.S. sociology before 1940. As sociology began to constitute itself as an academic field in U.S. universities between the 1890s and the 1920s, there were two existing routes to disciplinization, one represented by the humanities and the other by the natural sciences. Many of

the early founders of the field, including Giddings at Columbia and Albion Small at Chicago, embraced a scientific naturalism (Vidich 1985, ch. 8).[38] The Laura Spelman Rockefeller Foundation promoted a scientistic, policy-oriented approach in the 1920s and 1930s, making large grants to Chicago, Columbia, Harvard, Minnesota, North Carolina, and other universities and departments (Bulmer 1982; Fisher 1993; D. Ross 1991).[39] A recent historical study supports Donald Fischer's claim that the Rockefeller Foundation promoted a positivist form of social science, one that would appeal to business and the state, with an orientation toward practical applications and prediction (Ahmad 1991). Logical positivism was also dominant in Anglophone philosophy between 1930 and 1960. To the extent that sociologists attended to philosophical discussions at all—and they did so to a greater extent before 1945 than afterward—they tended to be channeled toward positivism.

Despite these favorable preconditions, methodological positivism did not dominate U.S. sociology departments during the 1920s and 1930s. Two explicit adherents of positivism observed in the pages of the *American Sociological Review* in 1939, "Sociology is at present divided in two conflicting schools. One believes that sociology must adopt principles and methods of the physical and biological sciences. The other maintains that sociology must use its own methods, since these are best adapted to its subject matter" (Levine and Dornblum 1939, 381).

The articles in the *American Journal of Sociology* and *American Sociological Review* (beginning in 1936) during the 1930s reveal a great epistemological and methodological diversity in comparison with subsequent decades. As Connell (1997) has remarked with respect to sociology in general before World War II, there was more interest in the colonized global periphery, although this was already tapering off in the 1930s. In a single year, 1939, the *ASR* included articles by a prominent antipositivist philosopher, Roy Wood Sellars (along with a response by an explicitly positivist sociologist, George Lundberg) and a historical analysis of "total war" by the German "émigré" sociologist Hans Speier, as well as essays on "Republican Turkey" and Brazil; the *AJS* carried a piece by Maurice Halbwachs on "Individual Consciousness and Collective Mind" and essays on Brazil and Hawaii (a quasi-colonial U.S. territory at this time). There were also examples in 1939 of inspired amateurism, reflecting sociology's lower degree of professionalization at this time, including C. Wright Mills's "Language, Logic, and Culture" (published while Mills was still an undergraduate at Texas). And in November 1939 the *AJS* published an entire special issue on psychoanalysis and sociology, which included essays by

A. L. Kroeber, Harold Laswell, Karen Horney, and Kenneth Burke (writing on "Freud and the Analysis of Poetry"), along with several medical doctors, psychiatrists, and, of course, sociologists. This collection suggests not only a lower level of anxiety about disciplinary boundaries than in subsequent periods (especially since the 1960s), but also an openness to the depth-realist theoretical concepts of psychoanalysis. As one of the contributors remarked, "Sociology is sufficiently mature to adopt the methods of contemporary psychological science," adding, "Among the latter, the phenomenon which Freud called the return of the repressed is of particular importance to sociology" (Zilboorg 1939, 341).[40] By the 1960s, however, the category of "the return of the repressed" was, to put it mildly, absent in most sociological analysis.

Leading sociologists rejected positivism during the 1930s, including Parsons and Howard P. Becker.[41] Parsons explicitly attacked "the positivistic-utilitarian tradition" at the beginning of *The Structure of Social Action* in 1937. Parsons's student, Robert Merton, wrote 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 doctrine of middle-range theory for which he became known in the postwar period (Steinmetz and Chae 2002). Influential figures in other fields, such as Friedrich von Hayek, warned against the danger of "scientism," which he defined as a "slavish imitation of the method and language of Science" and a "mechanical and uncritical application of habits of thought to fields different from those in which they have been formed" (1942, 268).

At the end of the 1930s, then, nonpositivists still had an important place in U.S. sociology, which was fragmented, polycentric, and methodologically unsettled (see S. Turner and Turner 1990, 75). Private foundation resources had not been sufficient to usher in an unalloyed triumph of positivism during the interwar period. Mounting anxieties around Nazism and Soviet communism were not yet focused on questions of epistemology and science.[42] Parsons opened his discussion of the positivist tradition in 1937 with the question "Why has it died"? and was less worried about empiricism than about a rising current of "irrationalism" that insisted that "sociology is an art" and threatened to abandon "scientific standards" altogether (1937/1949, 2: 774).

Soon after the war, however, the discipline of sociology experienced a dramatic turn toward methodological positivism. In the pages that follow, I want to reconstruct the external and intradisciplinary underpinnings of this installation of a new epistemological orthodoxy. In the spirit of the

critique of positivism, my account of these developments is conjunctural and figurational. I have already dealt here and in the introduction with some of the intellectual raw materials for social science positivism, which can be traced back as far as Hume. I therefore emphasize the novel forces after the war, without meaning to suggest that the preexisting conditions did not play a crucial role as well.

The most important and all-encompassing of the new conditions after the war was the social formation that retrospectively has come to be known as Fordism. The Fordist security state relied to a greater extent than previous forms of governmentality on the skills of sociologists, and this entailed a greatly enhanced level of public funding for social research. By the 1950s and early 1960s, the intellectual, financial, and political forces that had existed before 1945 combined with the ideological effects of the newly consolidated Fordist mode of regulation to sweep methodological positivism to triumph. The heightened level of anxiety around totalitarian irrationalism also played a role in this rise to dominance, as did the field-internal dynamics that have been so central to recent writing on science.

The Resonance of Fordism with the Positivist Vision of the Social

Regulation theory has described Fordism as an integral form of *societalization* (*Vergesellschaftung*),[43] that is, as a temporarily stabilized system for producing and reproducing (capitalist) society "despite and because of its conflictual and contradictory character" (Lipietz 1985, 109). Specifically, Fordism was geared toward maintaining a certain balance between capital accumulation and consumption. This emphasized a combination of mass production and consumption, which were associated with the system pioneered by Henry Ford; Ford's name therefore became associated with the more encompassing social form. Along with mass production and mass consumption of highly standardized products, Fordism also entailed some or all of the following elements: Keynesian-style policies aimed at smoothing economic cycles; welfare-statist programs that partially socialized the costs of reproducing labor power; collaborative relations between organized labor and management; mass culture offering homogeneous fare and tending to erode older distinctions between highbrow and lowbrow culture, converging in the middle; a family form emphasizing the male single breadwinner, at least as a sociocultural ideal; privatized single-family housing based on patterns of urbanization tending toward suburbanization (Graham and Marvin 2001); and a tendency for economic and social practices to be located within the "container" of the nation-state and evenly distributed across its surface (N. Brenner 1998; Peck and Tickell 1994). Each nationally specific "mode of growth" (Jessop 1989) emphasized some of the elements from this menu of Fordist practices and omitted others.[44]

Although Europeans often spoke of Fordism during the interwar period, they were usually referring to Henry Ford's system of high wages and assembly line production (e.g., Gottl-Ottlilienfeld 1924; Erfurth 1929). In his essay "Americanism and Fordism" (1929–35/1971a), however, Antonio Gramsci discerned the emergence of a full-fledged social system centered on the Taylorist and Fordist methods of production. Only in the 1950s was this "private sector" system articulated with systematic government policies. A full-fledged Fordist "mode of regulation" then began to crystallize in the United States, and somewhat later in Europe, culminating in the 1960s (see also Aglietta 1987; Boyer 1990; Lipietz 1987; Jessop 1990, 1999, 2003).

Regulation theory is concerned with the emergence, effects, and dissolution of modes of regulation such as Fordism and post-Fordism. The putative effect of a mode of regulation is to stabilize capitalist society, specifically, to provide the conditions for the accumulation of capital. But there are no guarantees that any solution to a capitalist crisis will be the optimal one, or even that it will even necessarily emerge at all in a given period. The centerpiece of regulation theory is its emphasis on crisis and contingency. Unlike functionalism and traditional Marxism, regulation theory stresses the improbability, or unlikelihood, of successful social reproduction (Demirović 2003, 46) as a result of the crisis-ridden, entropic dynamism of capitalist society. Elements of modes of regulation emerge in an uncoordinated and often chaotic manner that is different in each period and site. Another specific difference from traditional Marxism is the assumption that regulatory modes are provisional and will inevitably fall apart.[45] Regulation theory is also antireductionist: even within its contingent logics, it does not privilege a single causal mechanism, economic or otherwise, in explaining the origins or the effects of a mode of regulation.[46] Given its rejection of covering laws and prediction, regulation theory is "condemned, nolens volens, to ex-post observation" (Raza and Brand 2003, 10), that is, to an analysis of the (distant and more recent) capitalist past. It is not surprising, therefore, that the regulationist analysis of Fordism has become more complex and compelling as the Fordist period recedes in history, while discussions of post-Fordism are still somewhat confused and tentative.

The various patterns grouped by regulation theory under the heading

of Fordism helped to render positivist approaches to social explanation more plausible both to sociologists and to other people exposed and attentive to the new logics governing activity in the advanced capitalist world. By contrast, positivism had been a less plausible description of the meta-conditions of society in the context of the deeply unstable conditions of the interwar period. After the war, however, social reality appeared increasingly to fit the positivist expectation that social practices could be subsumed under covering laws, that is, fall 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 and controllable. In sharp contrast to the crisis conditions of the interwar years, orderly postwar Fordist societalization resonated with positivist notions of repetition.

The effects of postwar Fordism on the enhanced plausibility of sociological positivism can be summarized under five broad categories:

Science and the Fordist state

The first is the greatly enhanced role of science (including social science) in the Fordist form of governmentality. The 1940s and 1950s were not the first period in which significant funds were offered to sociologists willing to reconfigure their work in the image of the natural sciences, but the postwar period saw an unprecedented influx of new resources from the state sector to universities and think tanks and in the guise of contract research.[47] In 1958, government at all levels contributed only about 27 percent of all social science funds, but by 1976 the rate was about 56 percent (Useem 1976; Alpert 1961). In 1966, the federal government spent $26,621,000 on "basic and applied sociological research" alone (McCartney 1971, 385). In addition to the U.S. state's cold war support for research on counterinsurgency, which channeled copious resources to social scientists (Robin 2001), the domestic Keynesian dimensions of the security state relied on social science to track the economy, regulate business cycles, survey the population, and bring social practices into line with the regularities of mass production and mass consumption (Hirsch 1980, 1983, 1988).

The National Science Foundation was only one of the many military and civilian government agencies that funded sociologists' research, but there are good reasons for placing it at the center of this story. The NSF, created in 1950, was the single largest source of funding for sociological research. It was also crucial because of its orientation toward "pure science," which made it more immediately relevant than think tank research

for defining the level of scientific capital of each form of sociological activity. Leading figures recognized this conjuncture of politics and sciences in the immediate postwar period as a critical opportunity to enhance sociology's standing and put it on a more "scientific" footing. In a paper read before the American Sociological Association meetings in Cleveland in March 1946, Phil Hauser of the University of Chicago called attention to the spectacular increase in the prestige and political usefulness of science resulting from "the blinding flash of the atomic bomb" (379). Turning to the bills for NSF funding that were being debated in Congress, Hauser asked, "Are the social sciences ready?" Inclusion in the new funding opportunities, he argued, required that sociology "meet the requirements of good design of experiment." This also entailed the manufacture of "social laws" and predictions (382). Talcott Parsons wrote a paper two years later for the Social Science Research Council as "part of a political maneuver to place the social sciences in a posture to obtain federal funds," in which he "touted the contributions of empirical social science" and argued that "the same philosophical principles that guided the natural sciences were at the heart of the social sciences" (Klausner and Lidz 1986, vii).

Resources for positivistic forms of sociology were greatly enhanced by the expansion of the NSF mandate in 1954 to begin supporting the social sciences, along with the availability of funds from other agencies such as the National Institutes of Health (created in 1930), the National Institute of Mental Health (1946), and a thicket of think tanks funded by the military, greatly enhanced the resources for positivistic forms of sociology. A series of articles in the *American Sociological Review* between 1954 and 1957 by Harry Alpert (1954, 1955a, 1955b, 1957; see also Lundberg 1947), a sociologist working at the NSF, laid out the conditions under which sociologists would be eligible for NSF funding.[48] The first criterion 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 1955b, 656, my emphasis). This criterion was specified in naturalist terms as implying the "convergence of the natural sciences and the social sciences." The debates since the late nineteenth century around the proper epistemological logic and research methods for sociology were thus swept summarily under the rug. Attention to the "national interest" constituted the third criterion for funding, suggesting at the very least that sociologists would be expected to draw predictive lessons from their research. To underscore the payoff awaiting those who were willing to translate their research into the required idiom, Alpert listed some of the projects already

funded on a trial basis. All had adapted their approach to fit the framework either of the NSF's Biological and Medical Sciences Division or its Mathematical, Physical, and Engineering Sciences Division. This underlined two possible forms of scientism that would be available to sociologists in the decades to come, one characteristic of the "physics envy" period and another looking forward to the present-day infatuation with the life sciences. Many sociologists were ready to pursue Latour's "second principle" of "science in action," according to which scientists "speak in the name of new allies that they have shaped and enrolled" and "add these unexpected resources to tip the balance of force in their favor" (1987, 259). The subordinate status under which sociology entered this brave new world of federal funding was underscored by the overall tone of Alpert's essays: the "Foundation," he wrote, "*utilizes* sociology in the pursuit of its policy and operating responsibilities"—referring to surveys—and "to a very limited extent, it supports them financially" (1955b, 654). The article concluded with a barely veiled 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" (660).

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" (55).[49] Before 1930, only 1.3 percent of articles in the *AJS* acknowledged financial support from any outside source at all. Between 1945 and 1949, the percentage of articles in the *AJS*, *ASR*, and *Social Forces* acknowledging outside funding was 17.4, and by the 1960–64 period this figure had risen to 52.5 (McCartney 1971, 387–388). Sociologists were being offered unprecedented new resources and powerful allies.

The integration of sociology into the Fordist domestic and foreign policy scientific infrastructure seemed paradoxically to validate the claim that science was "value-free": social scientists could conceive of themselves as a separate and autarkic scientific community only after they had freed themselves from the prewar corporate organization of science, dominated by private funding (see Mirowski, this volume). As autonomous professionals, social scientists were released from responsibility for the ways policymakers would use their research. State Fordism thus buttressed the value/fact dichotomy, a mainstay of methodological positivism. By the

1960s, social policies with historical origins in socialist, corporatist, or religious traditions were being implemented by "postideological" governments. Sociologists offered their morally neutral services to their clients at the center of the welfare state. By 1967, Lazarsfeld, Sewell, and Wilensky could discuss sociology's relationship to its environment as generally one of scientist and "client" (x).

Fordist economy

The second way 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 especially affected the middle wage-earning classes (including sociologists), made it seem more conceivable that social practices did in fact repeat themselves in ways that could be represented with general covering laws, statistical models, replicable experiments, and reliable predictions. The steady improvement of the standard of living and the thickening of the welfare statist safety net lent credence to the idea of general social laws. Wage earners in general, including the better-paid sectors of the working class, were able for the first time in the history of capitalism to develop a horizon of stable expectations concerning their own future, enhanced by relatively generous protection against the risks of unemployment, sickness, and poverty in old age. The social ontology of the Fordist subject was aligned with *security*. The real wages of wage earners rose over the long run in tandem with increasing productivity (Demirović 2003, 47), rather than stagnating or declining regardless of the tendency of profits. Historical analysis became less significant for sociology once the world had become more "synchronic" and history had supposedly ended. Thus, Bernard Brodie of the Rand Corporation could write in 1949 of the need to "go beyond history" (474) through the application of economic models.

Culture and the Fordist ideoscape

A *culture* that was increasingly replicated across regions and social groups and classes began to make economic and developmental-modernization models seem more convincing, predicated as they were on a picture of universal, interchangeable subjectivity. Arjun Appadurai's (1996a) notion of an *ideoscape* suggests that ideologies will be understood differently, depending on one's position in social space (similar to a landscape that looks entirely different depending on the viewer's spatial location).[50] As noted above, some social scientists resisted the turn to methodological

positivism even during the 1950s; others were positioned to profit from the new perspective. But even those who initially resisted positivism were more likely to gravitate toward it, as it resonated increasingly with their experience of their own sociotechnical world. The increasingly depthless culture of Fordism (Hirsch and Roth 1986) seemed to substantiate the behaviorist-empiricist models. Social-psychological experiments in which conclusions about human behavior were drawn from research on undergraduates at U.S. colleges began to seem less far-fetched, at least to social scientists.[51] The positivist impetus to abandon all analysis of subjective meaning resonated with a wider culture that really did seem to have transcended ideology. The postwar U.S. postideological character was demonstrated in numerous sociological studies of voting and military life (Shils and Janowitz 1948; Lazarsfeld, Berelson, and Gaudet 1948/1968; Stouffer 1949; Berelson, Lazarsfeld, and McPhee 1954; Converse 1964) and reinforced by discussions of "the end of ideology" (Bell 1960). Interpretation of culture in the sense of a deep structure (Lévi-Strauss) and the concept of the unconscious were both to be avoided.[52]

Fordist geospace

The refusal of a role for hermeneutic or cultural interpretation in sociology was paired with a denial of the importance of *spatial* or geographic difference. Two aspects of Fordism's spatial regime were especially significant for social epistemology, encouraging an understanding of sociology as based on covering laws and universal mechanisms. First, the concentration of economic development and transactions on the scalar level of the nation-state encouraged social scientists to take this scale for granted as their unit of analysis. The containment of most social practices within the boundaries of the nation-state made it seem more obvious that these practices could be accurately described by general laws. By contrast, where practices are situated in multiple and shifting sites and confound simple dichotomies of the local and the national, the treatment of countries as necessarily commensurable and self-contained units of comparative analysis becomes much less convincing (Marcus 1995).

Fordism's relative homogenization of domestic space also underwrote the positivist disavowal of the spatial variability of causal mechanisms. This tendency to seek global laws of human behavior, to disavow cultural difference (Bhabha 1994a), was strictly opposed to the humanist and historical-hermeneutic emphasis on the unique and idiosyncratic. For behavioralists in the 1950s, including those wedded to a scientistic version of psychoanalysis, there were no salient differences between the United States and the rest of the world. It therefore made perfect sense for a University of Washington sociologist (Stuart Dodd) to carry out research for the Korean War effort of the Air Force on the effectiveness of air-dropped leaflets in rural towns in Washington State (Project Revere). Rural Washingtonians stood in for Korean peasants (see DeFleur and Larsen 1958; Robin 2001, 100). Modernization theory treated the poorer parts of the world simply as less developed versions of the United States and "foreign nationals as 'underdeveloped Americans,'" in the words of one anthropologist (E. Hall 1959, 13). Sometimes it was impossible to overlook the fact that culture and ideology were impeding "modernization," as in Vietnam. But the eventual telos for all nations was identical: a condition exemplified by the United States, located beyond ideology and culture, in which social practice was reducible to the actions of rational individuals pursuing wealth, status, and power (Pletsch 1981). As a leading sociological modernization theorist insisted, there "cannot be one social science for the study of one's own country and a different one for the study of other nations" (Inkeles 1951, 269). Identical models were applied to U.S. inner-city rioters, campus antiwar protesters, and Vietnamese peasants (e.g., Popkin 1979).[53]

Fordist imperialism

A final structural characteristic of this period that was relevant for the fate of methodological positivism in sociology was the emerging role of the United States as imperialist global hegemon. U.S. imperialism cannot be subsumed under Fordism, even if it was entwined with it; indeed, we are currently in the midst of a rearticulation of U.S. imperialism with post-Fordism (Steinmetz 2003b).[54] Two aspects of the cold war global configuration tended to work *in favor* of positivism: the U.S. orientation toward an "imperialism of free trade," and the government emphasis on counter-insurgency research, spawned at that time by the rivalry with the Soviet Union.

U.S. imperial hegemony after World War II was oriented toward making the world into an open capitalist market. The United States increasingly backed an imperialist but anticolonial stance, recalling the post–World War I Wilsonian era but now backed by unprecedented military predominance. This reflected the shift in U.S. priorities toward anticommunism and containment (Louis and Robinson 1993) and a preference for free trade as against the more protectionist style of relationships between colonies and their metropoles (Schmitt 1950/2003; Gallagher and Robinson 1953; Bergesen and Schoenberg 1980). Because the United States gener-

ally eschewed direct colonization it was not compelled to enforce a direct racial "rule of difference" (Chatterjee 1993) in its peripheral dependencies, which were treated as ostensibly self-governing.[55] Unlike colonial powers, for whom assimilation "taken to its extreme meant, quite simply, the ending of colonialism" (Sartre 1956/2001, 46), U.S. imperialism was directed toward a convergence of peripheral polities, cultures, and economic models with the American way.[56] Without gainsaying the exploitative, manipulative, and punctually violent character of U.S. imperialism, we should not ignore the fact that the postwar United States had fewer opportunities than the more directly 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 sciences for the colonized (anthropology) and the colonizer (sociology, political science, etc.). By contrast, the new U.S.-dominated postwar global regime reinforced the positivist program of using a single model to analyze both "other nations" and "one's own country."[57]

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" (Pletsch 1981, 568), a major substantive focus of social science research was counterinsurgency. As Robin (2001) shows, the Korean and Vietnam wars were both accompanied by rapid expansions of social science contract research. Lest one think this involved only social scientists outside the research universities we need only recall Project Camelot, which involved many of the most prestigious U.S. social scientists in the early 1960s. Project Camelot's stated objective, according to the official description issued in 1964 by the Office of the Director of the Special Operations Research Office, was to "determine the feasibility of developing a general social systems model which would make it possible to predict and influence politically significant aspects of social change in the developing nations" (Horowitz 1967, 47; see also Herman 1996, ch. 6). A book like 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 (1960, 166, xii).

Other aspects of this new U.S.-dominated world order were corrosive of the positivist unity of science program. Once the periphery is regarded from the standpoint of *production* rather than trade it takes on a very different appearance. First of all, Fordism was just one of the many forms of societalization, and usually a minor one, in the world system's margins (Alnasseri 2003). The social complexity that resulted from the articulation of "modes of regulation" and "modes of production" in the peripheries seemed to defy generalizing social science laws. These real differences made the application of universal models to peripheral societies less compelling. The complex cultural hybridity in postcolonial sites made generalization even more difficult, whether one was analyzing resistance to development or the microlevel processes of converting labor power into labor.[58]

Generalizing approaches were also continually undercut by the pervasive "three worlds" model (Pletsch 1981), which was predicated on the division between the communist, advanced capitalist, and postcolonial parts of the world system. This map called attention to the instability of the third category, which was permanently susceptible to the lures of communism, generally considered to be an "ideological" system rather than a "rational-natural" one.[59] The Third World was associated in this scheme with "tradition, culture, religion, irrationality, underdevelopment, overpopulation, political chaos, and so on" (Pletsch 1981, 574; Steinmetz 1999).

Another barrier to the universalization of generalizing models was racism. A common response to the scientific difficulties of encompassing the periphery within general developmental schemas has been to fall back on older discourses of cultural essentialism. A team of social scientists sponsored by the Human Relations Research Office (an army-funded think tank) to study the Korean and Chinese POWs at Koje Island during the Korean War pointed to "unchanging behavioral traits of 'orientals' " (authoritarianism, adaptability, an orientation to short-range calculations and personal bargaining, etc.) as reasons for communist organizational success. These categories were derived from "material on Korean and Chinese culture contained in existing studies, so far as it is consistent with the data of the existing study," and especially from the work of Lucien Pye, "America's foremost orientalist of the period" (Meyers and Bradbury in Bradbury, Meyers, and Biderman 1968, 281, n. 115; Robin 2001, 149). Similarly, some 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" (Kahn 1968, 340–341). This flew directly in the face of the universal picture of human behavior. Huntington's *Political Order in Changing Societies* (1968) broke with the generic story about modernization by arguing for cross-national differences in the adoption of selected parts of the modernity package, partly in an effort to justify U.S. support for "unmodern"—that is, undemocratic—client regimes. As became clear in subsequent publications, this was connected to an essentialist differentialism according to which Muslims and Africans are said to refuse modernity for inherent cultural reasons (Huntington 1996; Harrison and Huntington 2000; see also Lewis 2002).

The partial resistance of the Third World to regularity determinism thus marked a limit condition, even in the golden age of Fordism, to the reach of social science positivism. It is thus perhaps no coincidence that sociology turned away from the periphery, and also no coincidence that the discipline dedicated to studying the colonized world, anthropology, was constitutionally unable to become positivist and continued to emphasize relatively ungeneralizable research (Stocking 2001a, 317; Keane, this volume). Even some well-funded social scientists who set out with a generalizing take on the (post)colonial periphery experienced an epistemological change of heart. For some participants in the Camelot Project the mid-1960s marked a turning point at which they recognized the futility of the "search for universal laws of human behavior" (Robin 2001, 214, discussing testimony to that effect by Gabriel Almond, Kalman Silvert, and Jessie Bernard). Methodological critiques of modernization theory began to appear in the second half of the 1960s (e.g., Bendix 1967; D. Tripp 1973).

Similarly, postwar area studies represented a compromise formation and a site of struggle between "generalists" and "particularists." Area studies originated as an effort to adapt "first world social science to particularistic second and third world contexts" (Pletsch 1981, 582). Yet, at the end of the 1960s, the "largest proportion of area specialists [still came from] history, anthropology, language and literature, and the particularistic segments of political science" (Lambert 1973, 3). Area studies retained connections to the interpretive, linguistic, and humanistic approaches that had been so contemptuously forsaken by methodological positivism.

These idiosyncratic and linguistic approaches were necessarily preferable to the generalizing approaches (Dutton, this volume). Indeed, there was continuity in some respects between the "tradition versus modernity" distinction and the earlier and more explicitly racist Victorian approaches, from a period in which humanistic and scientific approaches were less polarized. Tradition was an undifferentiated category functioning only as a "before" to modernity in the new schemas, and in this respect it played a role similar to the "summational statements" about the Orient in older Orientalist discourse (Said 1978, 255) or the raciological generalizations of the eighteenth and nineteenth centuries (S. Gould 1996). Some writers have therefore been able to charge that modernization theory was simply a "cleaned up" and more respectable version of earlier languages of "primitivism" and social Darwinism (Mazrui 1968; Pletsch 1981, 575). The failure of essentialism to accurately perceive cultural difference is as unsatisfactory as the outright denial of difference. More recently, area studies has started to move beyond both the essentialist and the difference-denying approaches (Gaonkar 2001; Eisenstadt 2000). The reasons for this movement beyond methodological positivism are discussed in the concluding section of this paper. As in sociology, however, it is significant that the positivist dream of replacing place-names by the names of general "variables" has not disappeared even in area studies.[60]

Even as the U.S. emphasis on free trade imperialism pressed toward a universalizing form of positivism in the postwar period, other forces undercut the tendency to enfold the periphery into general laws. We should not attribute this resistance solely to the object itself, for this would seem to accept the self-description of the advanced capitalist core as having transcended culture and ideology. Sociological discourse on the non-West was multivocal even during the Fordist period, fluctuating between nomothetic approaches that embraced the periphery into a universal framework and essentialisms that attributed unique and timeless traits to the (post)colonial other. The survival of nonnomothetic perspectives was related to the complex internal organization of postcolonial social systems, the instability of their political boundaries, the competition between the United States and the Soviet Union, the persistence of colonial and Orientalist representations of the non-Western Other, and the relative autonomy of area studies from the more positivistic social science disciplines.

My aim in this section has been to trace some of the ways Fordism resonated with, and directly promoted, U.S. sociology's positivist epistemic unconscious starting in 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 and ontologies directly *reflect* social reality. On the one hand, the Fordist mode of regulation was accompanied by a series of self-interpretations that guided the way it was experienced.[61] The main components of U.S. Fordism—stabilization through demand management, homogenization of consumer tastes, ris-

ing worker incomes pegged to productivity, collaborative labor unions, nation-state centrism—were constantly broadcast to the denizens of the Fordist metropolises 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 ratify their approach.

Totalitarianism and Social Science Positivism

A further element of U.S. culture that tended to strengthen the positivist position in the social sciences was the discussion of the role of science in the rise of Nazism and totalitarianism. This was related to global Fordism but not reducible to it. Writing in 1941 when the target of his reference could not be mistaken, Marcuse accused the sociological positivism derived from Comte of bearing "the seeds of a philosophic justification of authoritarianism" by repudiating metaphysics and thereby also "man's claim to alter and reorganize his social institutions in accordance with his rational will" (Marcuse 1941/1960, 342, 344; see also Bannister 1992). Horkheimer and Adorno's 1944 *Dialectic of Enlightenment* (1986) opened with a Heideggerian critique of the Enlightenment and moved from there through a series of dialectically unfolding steps to industrial capitalist modernity, anti-Semitism, and Nazism. This text was the fount of an entire field of interpretations of fascism and totalitarianism as products of (a distorted) scientific modernity starting in the 1960s, when the Frankfurt School was rediscovered (see Arato and Gebhart 1988), and somewhat later with poststructuralism. Hollinger recalls that the literary cohort that was attacked by C. P. Snow in 1959 for their supposed proximity to fascism "regrouped, and came out some years later . . . to attack science itself as crypto-fascist" (1996, 166, 171). Much of this was "under the cover" of postmodernism and Michel Foucault, who published the memorable and provocative phrase "Knowledge is not made for understanding; it is made for cutting" (1977b, 154).

In the context of the cold war competition with the Soviet Union and in the immediate aftermath of Nazism and the Holocaust, however, it was difficult for antifascists to criticize modern science. The prestige of science had increased so dramatically that such critiques seemed quixotic if not treasonable. The exponential expansion of think tanks and research contracts in this period is extremely relevant here. This military-intellectual matrix integrated a huge number of social scientists into a culture that connected the struggle against the totalitarian enemy with emulation of the natural sciences and disdain for humanistic and historical approaches. Discussions of the assault on science shifted from the Nazi to the Soviet

example (e.g., M. Polanyi 1945). In a study of voting by Lazarsfeld et al. (1948/1968), for instance, the authors pointedly refused to blame capitalism or communication technology for extremist psychopathologies in modern society (Robin 2001, 63), insisting instead that a robust fabric of primary groups and opinion leaders filtered the commodified messages emanating from the mass media.

Antifascism was at least as strongly associated with positivist as with nonpositivist positions. As Hollinger remarks, the "faith that science and democracy are indissolubly bound up in a single cultural mode . . . was passionately reaffirmed in the late 1930s and early 1940s by enemies of the Third Reich" (1996, 81). The suggestion that it was precisely the *lack* of a modern scientific culture that had contributed to fascism retroactively cast suspicion on the entire nonpositivist tradition running from Hegel and the German Romantics to Marx, Dilthey, and the critical theorists of the interwar period. This narrative was doubly damaging because most of the antipositivist thinkers had arisen in a German-speaking context and could therefore be connected to German exceptionalism at least through guilt by association. According to a version of the exceptionalist theory popular at the time, Nazism had resulted mainly from irrationalist trends in the German philosophical tradition.[62]

After 1945, Americans began to associate Soviet oppression with hostility to science. Because the word "science" in this period was usually understood as a description of the natural sciences offered by the logical positivists, this only bolstered any opposition to antipositivist criticism. It took someone with the intellectual and moral stature of Adorno to reopen the debate with his 1957 essay on "Sociology and Empirical Research" and his 1962 critique of Popper, "Logic of the Social Sciences."[63] Of course, even then Popper responded by calling the Frankfurt School "irrationalist and intelligence-destroying" and putting it in the same camp as the "enemies of the open society" (1976b, 289, 300; 1962/1971). Being called an enemy of the open society for criticizing positivism would perhaps not get you fired or investigated by COINTELPRO or the Verfassungsschutz (Office for the Protection of the Constitution, or the West German equivalent of the FBI), but it still had a chilling effect. Moreover, none of this was translated into English until the mid-1970s.

The Long Reign of Methodological Positivism

One result of this overdetermined conjuncture after 1945 was the solid implantation of methodological positivism as doxa in the sociological

discipline.[64] Despite differences of taste or viewpoint, most of the players in the field seem to have recognized common stakes and agreed on legitimate definitions of field-specific cultural capital. Reputational, social, and economic capital in sociology tended to accrue to more positivist/empiricist/scientistic positions. Fluency in positivist methodological positions began to function as a field-specific form of scientific prestige. Even those who disagreed with the positivist position tended to collude in its dominance; those who did not adjust to this new regime were rechanneled willy-nilly into less rigidly positivist disciplines or into poorly regarded sociology departments. Sociology became a well-structured field at last.

The concept of the scientific field implies that alternative positions are dominated but not necessarily unthinkable. The entrenchment of positivist orthodoxy should be understood as a multifaceted process of rearticulating categories and definitions, graduate curricula, and funding priorities, and of redefining standard models for journal articles. Of course, social theory has taught us that the repetitive "physical" engagement in a practice that one disapproves of, or with which one disagrees "internally," can still have profound effects on habitus (Bourdieu) or take the form of an "ideological interpellation" (Althusser). Or, in Orwell's (1936/1968) description of a colonial official: "He wears a mask, and his face grows to fit it."[65] These unperceived effects of physical training can be as lasting and thoroughgoing as a deliberate conversion experience or a conscious "paradigm shift." Indeed, nonpositivist positions began to seem unscientific, unprofessional, or nonsociological partly as result of such training.[66]

There may indeed have been little knowledge of epistemological alternatives among sociology PhDs who were socialized into the discipline during the 1950s and 1960s, until the critical movements emerged in the later 1960s. But there were dissonant voices even in the heart of the cold war period. A short list of sociological critics of methodological positivism would have to include C. Wright Mills, Hans Gerth, Howard P. Becker, Howard S. Becker, Reinhard Bendix, Alvin Gouldner, and Theodor Adorno (and starting in the early 1960s, Jürgen Habermas). All of these critics lost some of their scientific authority in relation to the purveyors of the new dominant paradigm. Critics of the sociological mainstream during the 1950s and 1960s like Mills and Gouldner attacked quantitative methods and Parsonian grand theory even while they seemed to endorse other features of methodological positivism.[67] Some opponents of methodological positivism were "excommunicated," others "converted," and still others criticized the mainstream while reproducing some of its central premises. The role of Parsons, who shifted from being a critic of positivism to promoting it, is best understood as that of an insider critic.[68] Indeed, the self-proclaimed positivist sociologist George A. Lundberg pointed in 1964 to "convergences in the theoretical positions of Parsons and S. C. Dodd" (of leaflet-dropping fame), whose systems were "hitherto assumed by many" to be "incompatible" (169–170).

Conclusion: Toward a Third *Positivismusstreit*?

I have argued that Fordism was a key ingredient in the processes that tipped the hand of constituencies long bent on making methodological positivism dominant in sociology. Fordism helped to forge a form of social science that was acultural, ahistorical, and individualist with respect to its basic units of analysis and oriented toward general laws, replication, prediction, and value-freedom. The Fordist security state drew heavily on social knowledge packaged in a positivist format and pumped massive amounts of money into research organized this way.

The period between 1945 and 1965 was a powerful foundational moment in sociology whose effects continue to be felt today. Graduate students in the leading departments are still required to take statistics courses but not to study textual interpretation. The NSF still tends to support mainly quantitative research in sociology. Methodological concepts such as scope conditions, path dependency, and interviewer effects seem to acknowledge problems with positivism, but they retain the idea of general laws, social processes that are not shaped by history, and interviews purged of subjectivity. Some sociological subfields are only now beginning to question the adequacy of modernization theory. Although there is no space to treat the intervening period between the 1960s and the present in any detail, I want to consider briefly the effects of two processes that might have been expected to have undermined the reigning methodological positivism since then. The first is a series of epistemological challenges to the discipline's positivist mainstream since the late 1960s. The second is the collapse of Fordism (which began almost as soon as it reached its fullest elaboration) and the ongoing transition to post-Fordism.

A wave of challenges to the positivist metaposition began to appear just a few years after it became enshrined as orthodoxy. One year after the publication of the book *The Uses of Sociology* (Lazarsfeld et al. 1967) an essay appeared entitled (ironically, of course) "Tuer les sociologues" (Kill the sociologists) by Daniel Cohn-Bendit and other sociology students at Nanterre, translated into English under the less bellicose title "Why Sociologists?" (Cohn-Bendit et al. 1969). Although hardly a theoretical tour

de force, this essay signaled the onset of a long period of challenges to the positivist mainstream in sociology, some of them cumulative, many of them recuperated. In 1968 a caucus was formed within the American Sociological Association called the Sociology Liberation Movement. Gouldner's *Coming Crisis*, a frontal assault on positivism, was published in 1970, followed by a series of (self-)critical studies of the history of U.S. sociology.[69]

The challenges to methodological positivism in the late 1960s and 1970s can be seen as a sort of intellectual pendant to the broader rejection of Fordist homogenization by the first wave of "new social movements" in that same era. Just as the anti-Fordist new social movements criticized capitalism while simultaneously providing models for a resolution of its crisis (Mayer 1988; Mayer and Roth 1995), the intellectual challenges to positivism during the 1960s and 1970s rejected positivism while generating intellectual resources for repairing some of its intellectually tattered loose ends. A depth-realist positivism modeled on mechanistic Marxism became palatable to some sociologists and was workable as a unifying ground for a refurbished discipline.

A similar set of parallels can be drawn between external and intradisciplinary movements in the 1970s and 1980s. The new social movements of these decades were now responding to the final unraveling of Fordism rather than the pathologies that accompanied its era of greatest glory;[70] by the same token, the turn to history within sociology in the 1970s echoed the disappearance of the Fordist standardization of time, space, and culture. The relative decline in social science funding at the end of the 1970s corresponded to a crisis of confidence in big science sociology. The new cultural studies/cultural sociology of the 1980s and 1990s can in turn be related to the emergence of post-Fordist, globalized regulation and a version of capitalism that was increasingly centered on culture and self-promoting ("entrepreneurial") forms of subjectivity. The current epistemological turn, finally, has emerged contemporaneously with the consolidation of post-Fordism, indexing the central role of science, technology, and information in that formation (along lines first sketched by Lyotard 1979/1984).[71]

As with the account of methodological positivism in the previous section, the emergence of these critical movements can be understood only against the overall macrosociological and historical context. Although the earliest critical movements in the 1960s contained fundamental critiques of the dominant mode of sociology, they were also in many ways defined by the Fordist conditions that had originally underwritten methodological

positivism. The latest critical movements, by contrast, have come into existence in a post-Fordist environment. Unlike the earlier criticisms, they do not share social "conditions of birth" with most of their positivist opponents. Nor do post-Fordist social conditions resonate as strongly with positivism as the earlier mode of regulation. While the intradisciplinary conditions may still disproportionately reward positivism, this is less the case with the world beyond the academy.

In their most radical forms, neo-Marxist, feminist, historical, and cultural sociology were corrosive of the central ontological and epistemological premises of methodological positivism. Neo-Marxism and feminism attacked empiricist ontology, historical sociology provided tools for dismantling the positivist emphasis on invariant and time-independent laws, and the cultural turn reasserted the importance of concept dependence. The paradox is that each of these challenges began to be recuperated almost as soon as it appeared. As with the 1950s and 1960s there were, of course, many exceptions. But just as Craig Calhoun (1996) has argued that there was a domestication of historical sociology, the epistemological challenges of these other critical movements were largely defused. These movements tended to attack only a subset of the main tenets of positivism or to scale back their epistemic agendas and advocate instead for the study of previously ignored objects. Sociologists began to produce positivist studies of class and gender and to use history as little more than a means of increasing the number of data points (Sewell 1996). Another strategy of (self-)containment was to translate a critical movement into the promotion of a new "independent variable" that needed to be thrown into the analysis (as in the now shopworn "bringing *x* back in" trope; see Homans 1964). Culture was redefined as a delimited set of objects rather than an optic for understanding the reflexivity and concept dependency of social life in general. U.S. sociology has not, until recently, seen many signs of a genuine crisis at the fundamental level of its philosophical and methodological assumptions, despite the proliferation of ostensibly dissident theoretical approaches and labels.

If I am correct that social epistemologies are undergirded not just by intradisciplinary mechanisms and funding patterns but also by the ways macrocultural conditions reverberate or collide with them, one might expect the cultural and epistemological turns to have some staying power. Each resonates in important ways with the flexible and information-centered world of the post-Fordist "new economy." Alvin Gouldner's (1970) prediction three decades ago that U.S. sociology would move through a period of crisis characterized by the collapse of theoretical

hegemony to a less positivist and more reflexive polycentrism might then turn out to be correct, if premature.[72]

The most recent dissenting movement in sociology is the turn to epistemology and science itself.[73] The contributions of the current epistemological challenge to the possible dismantling of positivism are twofold. First, it intervenes much more centrally in the fact-value debate than the earlier movements. Second, by clarifying the basic stakes at issue, this epistemological turn may make it possible to perceive the complementarity of the earlier challenges, to bundle their critical potentials, and to ward off efforts to lasso them back into a recharged positivist orthodoxy. Recall that methodological positivism itself represented a joining together of empiricist ontology, positivist epistemology, and scientism. The epistemological turn, by clarifying the basic stakes at issue, might make visible the complementarity of the Marxist/feminist, historical, and cultural turns. Given my critique of the notion of invariant patterns of social behavior, it would be perverse to try to predict what direction sociology will take. But one possible future, at least, is postpositivist.

A second scenario, however, acknowledges the deep and abiding implantation of methodological positivism in disciplinary institutions, patterns of socialization, and the habituses of individual sociologists, and its reinforcement by extra-academic social structures other than the mode of regulation. The state, private organizations, and even social movements continue to demand social knowledge packaged in positivist formats. The reasons for this preference are complex. It may be that there is a built-in affinity between projects of societal regulation per se and positivist knowledge formats. The impetus behind projects of "flexibilizing" capitalism does encompass criticisms of the positivist forms of knowledge that undergirded less flexible models of production. An entire array of critiques of the Fordist welfare-security state, some of which have been built into post-Fordism, proposed alternative forms of social knowledge and intervention. At the same time, post-Fordist policies of privatization, localization, and flexibilization are oriented toward logics of recommodifying labor power and redisciplining the poor. Economic flexibilization has involved first and foremost the imposition of a *uniform* neoliberal model on all places and situations. The concept of social capital, which is linked to neoliberal policies of privatization, has been applied in an "imperialist" way, running roughshod over disciplinary distinctions (Somers, this volume). Post-Fordist interventions thus seem to rely as much as Fordism did on decontextualized economic models of human subjectivity.[74]

As for politics, post-Fordism has not led to the complete elimination of state-organized forms of regulation or to "spontaneous" forms of societalization organized by "invisible hand" mechanisms—despite the barrage of discourse and policy since 1980 organized around "deregulation" (Prasad 2000). Instead, new forms of state regulation have emerged in a series of wrenching moves and in the wake of the "creative destruction" of earlier schemas. The state is still the most important center, the primus inter pares, for organizing projects of societal regulation. This has become even more evident since the beginning of the Bush administration's new imperialism and war on terrorism (Steinmetz 2003b).[75] This is not to ignore shifts in emphasis and scale since the 1980s, including the increasing importance of parastatal and nongovernmental centers as organizers of social regulation.[76] But even in the current period, the state, considered at all of its scalar levels, is still the superordinate coordinator even for privately financed regulatory initiatives, and it continues to organize most of the familiar legal, infrastructural, and welfare-statist interventions. Indeed, during the 1990s, when the post-Fordist regime seemed to achieve a sort of consolidation, state funding for science again began to increase in the United States.[77] Of course it is still an open question whether this form of governance will rely to the same extent as its predecessor on positivist forms of social science.

Post-Fordist governance may continue to privilege positivistically packaged social science knowledge even as the global conditions of post-Fordism make methodological positivism less spontaneously convincing.[78] Positivists are also able to reproduce themselves intellectually through the academic equivalent of artificial fertility, defying the "nature" of environing social trends. Whether "society's nature" or the discipline's "second nature" will prevail is impossible to predict.

Notes

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1 Conventionalism, as I use the term here, includes standpoint theory, constructivism and constructionism (Hacking 1999), and epistemological idealism. Strictly speaking, conventionalism is the doctrine that the success or failure of scientific theories in achieving acceptance is based on convention, that is, on considerations other than the correspondence between theory and its object. "Idealism" is the term for the view articulated most famously by Berkeley (1734/1998) that denies the existence (or the

accessibility to the observer, which has the same consequences) of an external reality existing "independently of our theoretical beliefs and concepts" (Keat and Urry 1975, 5; also see Bhaskar 1975/1997, who speaks of "conventionalist super-idealism").

2 Keane (this volume) suggests that cultural anthropology in the United States has been less beguiled by positivism than the other social science disciplines, even during the strong push for scientific respectability during the first two-thirds of the twentieth century (but see Wax 1997 for a different assessment). *Cultural* relativism, a normative stance that has characterized U.S. cultural anthropology throughout most of the twentieth century, should be distinguished from *epistemological* relativism, which critical realism accepts, and judgmental relativism, which it rejects (Bhaskar 1986; Collier, this volume).

3 See Walter Goodman, "Thinkers Who Would Be Doers See Social Injustice Wherever They Turn," *New York Times*, August 19, 2000. Recalling Richard Bernstein's reports on the MLA meetings in the 1990s, this one is exercised mainly by a suggested loss of objectivity. See Bernstein, "The Rising Hegemony of the Politically Correct," *New York Times*, October 28, 1990.

4 Lundberg (1955) in the 1950s is typical in disavowing the label positivist, which he had openly embraced before the war (1939a), and in characterizing his own viewpoint more neutrally as that of "natural science." Even during the 1950s positivism was not so secure as to be *doxic*, at least when it presented itself in overt philosophical and methodological terms. Nagel (1961/1979) also eschews the term positivism. But see Riemer (1953) for evidence that sociologists in the 1950s clearly recognized the philosophical basis of Lundberg's position.

5 An example of such rethinking is Arthur Stinchcombe, whose *Constructing Social Theories* (1968) was largely positivist but whose *Theoretical Methods in Social History* (1978) represented a completely different epistemological position (see also Stinchcombe 1991). Eclectic and complex, Stinchcombe's work has reflected and contributed to the critical theoretical movements (including historical sociology) that intervened in the decade between the two books, while also constituting a target for some of them.

6 See Bourdieu (1972/1977) for the use of the terms doxa and orthodoxy in this context.

7 Positivism as defined below is even more entrenched in economics, demography, political science, and psychology. Economics and demography were already established and accepted as *sciences* when sociology, anthropology, and political science were still facing what Camic (1995; Camic and Xie 1994; see also Breslau 2003) calls the "newcomers' dilemma" in the mid-twentieth century, the dilemma of proving themselves as science by emulating the established natural sciences while simultaneously differentiating themselves from each other. Political science is similar to sociology with respect to the conditions it faced in the mid-twentieth century and the path that it followed as a result. The different ways the critical challenges to positivism since the 1960s have played themselves out in political science seem to stem largely from the former discipline's different position vis-á-vis the imperial U.S. state. Sociology has been articulated with, and drawn financial support and personnel from, the lower-prestige branches of the welfare state (see below). Political science has been integrated into the more prestigious foreign policy branches, as revealed most recently by the "Straussian" influence on the U.S. government's imperialist interventions (Atlas 2003). The various social sciences have also differed with respect to the postundergraduate careers and policy-relevant academic departments with which they are associated. Here again, sociology has been connected to fields with lower cultural capital, such as social work, while political science has become a feeder for law schools. None of these claims can be generalized to countries other than the United States, of course.

8 I have found only one example of related use of this term, in an essay by a symbolic interactionist (Littrell 1997).

9 In other work I have proposed regulation-theoretic accounts of the rise of social policy and the welfare state (Steinmetz 1993), new social movements (1994), and right-wing social movements (1997b; forthcoming a). See Jessop (1990, 1999) and Brand and Raza (2003) for detailed recent discussions of regulation theory.

10 Of course, logical positivism itself was associated with the left, as Sandra Harding (this volume) reminds us. Some of the key contributors to methodological positivism in postwar U.S. sociology were associated with the old left, including Phil Hauser and Leslie Kish. Alvin Gouldner was a left-wing sociologist who seems almost to have kept his epistemological criticisms under wraps until the McCarthyite cold war began to thaw in the 1960s (see Gouldner 1962, 1970). Only since the 1970s and 1980s has the "epistemological left" (Novick 1991) differentiated itself clearly and sharply from the political left.

11 On critical realism, see Bhaskar (1979, 1989, 1994, 1975/1997), Collier (1994), Steinmetz (1998), Steinmetz and Chae (2002).

12 In terms of evidence not discussed here, my own participation in three U.S. sociology departments (Wisconsin, Chicago, and Michigan) and my personal socialization into a natural science version of positivism are important. As with Sewell's paper (this volume), mine is thus a form of autocritique. During the heyday of methodological positivism (and still today, to a lesser extent), sociologists believed that it was more scientific to work on topics in which they had *less* personal involvement and investment. This premise stemmed from a scientistic emulation of the natural sciences, which were thought to have successfully separated knowledge from values. The primary source of social distortion in social research was construed as bias, located in researchers' minds. It was correct that the relationship between the researcher and his or her object of research was more complex in the social sciences than in the natural sciences, given the *recursive* relations between the two. Yet the rejection of "value-laden" science was applied unsystematically only to some stages of the scientific process and not others. Social scientists seem to have believed, for instance, that they could not be as objective about themselves or their own discipline as about other scientists and other disciplines. Yet few argued at this time that middle-class sociologists' class position might prevent them from writing objectively about the lower classes, or that metropolitan observers' associations with colonizing or imperial states could hinder their ability to generate dispassionate knowledge about colonized or postcolonial peoples.

13 In a separate article (Steinmetz forthcoming) and in a book in preparation, I provide more detailed readings of core texts from the golden age of methodological positivism and the more recent period.

14 There are a few sociologists who still openly defend and embrace positivism, including Jonathan Turner, Gerhard Lenski (1988), Guy Swanson (see Bryant 1992), and, in his most recent work, Arthur Stinchcombe (2002). Griswold (1990) lightheartedly defended a "provisional, provincial positivism." Abbott (1992a) proposed what he called a "narrative positivism," but he combines this positivism with interpretivism in an eclec-

tic mix (1990) and offers some of the most trenchant critiques of mainstream sociological statistical method (1988b, 1992b, this volume). A recent Belgian conference discussed plural positivisms in sociology (see Despy-Meyer and Devriese 1999, 95–143).

15 See, for example, Blalock (1964, 1969) and the other key texts from the 1960s and early 1970s cited by Gartrell and Gartrell (1996, 144); for a critique of Blalock, see R. Miller (1987, 240–241, n. 11). Lazarsfeld and Rosenberg (1955) still included a section in their methods text on the "philosophy of the social sciences"—one that was explicitly positivist (see especially the entry by Hans Zetterberg in Lazarsfeld and Rosenberg 1955, 533–554).

16 See, for example, R. Collins (1989, 129), who implicitly stakes out a nonpositivist, even critical realist, position that contradicts his explicit endorsement of positivism there. Collins's arguments in this essay generally line up with realism, even critical realism, and against positivism. Yet he makes only one reference to a realist philosopher (H. Putnam 1983) and none to writers like Peirce, Harré, Bhaskar, Hacking, Collier, or Miller.

17 See Layder (1988) for an interesting discussion of the interdependencies between specific epistemologies, theoretical discourses, and methods.

18 As I argued in the introduction to this volume, the relationship between ontology and epistemology should be viewed as a distinction, not a dichotomy.

19 To the extent that sociologists began talking about *scope conditions*, they were acknowledging time dependence, even if the very need to coin such a term reflected a baseline scientism.

20 This refusal of the time and space dependence of concepts is usually implicit, as in theories of economic change that apply the same concepts to the ancient or medieval world as to the capitalist present (Rostow 1960; G. Cohen 1978). In the most self-conscious statements of methodological positivism, however, it is explicit; one thinks of Popper's (1957/1991) critique of "historicism" or the statements of some of the modernization theorists, like Inkeles (1951), discussed in the next section. See also the quote by Becker (1996, 6), quoted by Somers in her contribution to this volume, about the applicability of the concept of social capital to "the distant past."

21 On the political conditions in Wilhelmine Germany that led to German sociologists' rapid convergence around the doctrine of value-freedom (*Wertfreiheit*), see Rammstedt (1988); see also Abel (1929); for a recent discussion of Weber's "positivism," see Hekman (1994).

22 See also "Campus Life: Scholars' Group, Accused of Bias, Divides Faculty," *New York Times*, October 21, 1990.

23 In another essay I track the epistemic multivocality, or wavering, in a recent methodological treatise that attempts to stake out a depth-realist positivism (Steinmetz 2004b; see Mahoney and Rueschemeyer 2002).

24 The fact that Davis goes on to equate causal analysis with "discovering relationships among *phenomena*" (my emphasis) suggests again that methodological positivism was at least as influential as functionalism during the supposed heyday of Parsonianism. There was also an effort to render Parsons's theory compatible with positivist research methods (see note 69 in this essay).

25 Sorokin (1928, 727n, 545, quoted in Merton 1936/1968a, 25–26) actually argued that Franklin Giddings was a "pioneer of . . . world sociology," while insisting that Marx added nothing new and that "what is original is far from scientific" in Marx.

26 H. Friedrich (1992) criticizes certain self-proclaimed antipositivist approaches for their "cryptopositivism." King et al.'s *Designing Social Inquiry* (1994) defends a contemporary empiricist/positivist position while eschewing the label (see Mihic et al., this volume; Steinmetz 1998).

27 Bourdieu clearly had mainstream U.S. sociology in mind, as it figured prominently at the beginning of the paragraph from which this quotation is taken.

28 "Distinction" is field-specific. One can hardly ignore the fact that positivism and scientism have been less than distinguished positions in other fields, for instance, the arts, since the early twentieth century. In philosophy, positivism has had no eminent defenders for decades. Although it seems obvious to some writers (e.g., Bhaskar 1989, ch. 4; Bourdieu 1981, 282) that positivism is the "spontaneous ideology" of the natural sciences, I am less certain that positivism will continue to be the most influential position in sociology (see the conclusion to the current essay).

29 Marxism, feminism, historical and cultural sociology, critical race theory, and the sociology of science—probably the main critical movements in U.S. sociology during the past thirty years—all had precursors in the discipline before the consolidation of methodological positivism.

30 Paradoxically, Latour's (1987, 1999) approach ends up looking internalist even though he rejects the dichotomies of human and nonhuman, society and nature, epistemology and ontology, words and things, and so on.

31 With respect to the discipline of sociology, Camic (1995; Camic and Xie 1994) has argued that early U.S. sociologists were motivated by the scientific "newcomers' dilemma" to emulate more established scientific disciplines. In his work on Parsons, Camic (1987, 426; 1989) stresses the immediate university field, that is, the "Harvard scene."

32 Bourdieu's view of science is conventionalist in its rejection of the view that epistemologies or theories gain prestige simply because they are more adequate to their object. This applies only to the social sciences, however, if only by default, because Bourdieu does not extend his theory to the natural sciences.

33 Hollinger's (1996, 125–128) own account of "Academic Culture at the University of Michigan" and at U.S. research universities more generally in the key 1945–62 period focuses on the quantum leap in government funding after 1945 and suggests that it had strong effects on the resultant culture of "academic entrepreneurship" and on the research products.

34 Robin's (2001, 220–225) excellent book, for instance, provides a careful account of the role of military think tanks in promoting U.S. social science behavioralism, but it does not account for the *crisis* of this framework from the mid-1960s on except as a seemingly natural response to its political and epistemological inadequacies.

35 At more concrete levels of practice, the overall fabric of commonsense or agreed-upon culture will, of course, give way to patterned differences, and these will be correlated with social class or other dimensions of social hierarchy.

36 Merton's widely cited and often revised and reprinted "Note on Science and Democracy" (1942) focuses on "communism" and disinterestedness as transhistorical features of the scientific field, suggesting again than his historicizing focus was more externalist than internalist (see also Hall 1963).

37 See also Habermas (1970) and, for an earlier sociological diagnosis of the "anti-science movement" and widespread "latent and active hostility to science," Merton (1938, 336).

38 Albion Small, the first professor of sociology in the United States and founder of the first U.S. sociology department and journal, the *American Journal of Sociology*, is a slightly more ambiguous case. Small studied in Germany and published a book on the German Cameralists (1909), a topical choice that signaled an interest in the less positivist pole of the nineteenth-century intellectual division in Germany between historical economics and the "Manchester School." But he also published a book on Adam Smith and modern sociology (1907), and was influenced by the Austrian social Darwinist Gustav Ratzenhofer, both of which align him with the mainstream of sociological scientism. On Small, see Dibble (1975); on Giddings, see Camic and Xie (1994); on both, see O'Connor (1942).

39 Between 1922 and 1929, Chicago received the most money from the Laura Spelman Rockefeller Memorial and the Social Science Research Council (which itself received a great deal of its money from the Rockefeller Foundation): $3.5 million in institutional grants, including direct staff support, along with the "lion's share of the fellowship money and a new social sciences building" (D. Ross 1991, 402).

40 Of course, the fact that seven of the eleven contributors were listed as MDs underscored the ongoing medicalization of U.S. psychoanalysis (discussed in the introduction to this volume), which played a role in transforming the field in a more scientistic direction.

41 Gouldner's *Coming Crisis of Western Sociology* (1970) labels Parsons a positivist. This evaluation is based on placing Parsons politically in a lineage running back to Comte, the discipline's original positivist. But Gouldner ignores the redefinitions of positivism that had occurred during the twentieth century (see C. Bryant 1989).

42 This is not to say that sociologists were not concerned with this problem before the war and the Holocaust. Already in 1938 Merton discussed the ways "hostility toward science" in Nazi Germany was threatening to "curtail scientific activity" (321–322; see Hollinger 1996, 83).

43 *Vergesellschaftung* is defined as the various "complex social processes in and through which specific institutional orders and their broader social preconditions are secured." One premise of this concept is that "the existence of 'society' cannot be taken for granted: it must be constituted and reproduced through more or less precarious social processes and practices which articulate diverse social relations to produce a 'society effect' " (Jessop 1990, 4–5).

44 The fact that Fordism was centered on the scalar level of the nation-state allows us to privilege this unit of analysis and comparison. That this cannot be taken for granted in the discussion of post-Fordist regulatory projects, which take place on a variety of different scales, is a commonplace in current discussions (see Brand and Raza 2003).

45 Some regulation theorists see this as a problem. Discussions of whether regulation theory's usefulness is limited to analysis of the Fordist epoch are often tinged with regret. This is perhaps not surprising, given that most regulation theorists are economists (in France) or political scientists (in Germany) rather than historians, and are therefore focused on the present. Indeed, one regulation theorist suggests that we should "forget regulation theory" if it were only of "historical interest" (Röttger 2003, 39; 2001).

46 Indeed, even though the earliest regulation-theoretic interventions were by economists, they underscored the reciprocally determining relations between economics and politics (Röttger 2003, 18–19). More recent work in this tradition has encompassed all sorts of social practices without privileging one or the other (see Jessop 1990).

47 For accounts of the growth of public funding for science and sociology, see Geiger (1993), Gieryn (1999, 65–114), Kleinman (1995), J. Wilson (1983), Klausner and Lidz (1986), Larsen (1992), Featherman and Vinovskis (2001). On the specific military connections of this research money, see Needell (1983), Robin (2001).

48 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 his aim "was to build up, inductively, general laws of social life through the accumulation of statistical findings." *Suicide* was privileged over *The Elementary Forms* (1912/1955) or *Primitive Classification* (1903/1963).

49 Of course, Shils played both sides of the street epistemologically, collaborating with Janowitz on the Wehrmacht study (1948) that denied the importance of ideology, and even coining the infamous phrase "end of ideology" (1955).

50 I am using "culture" in the broadest possible sense (see my introduction to *State/Culture* 1999) and "ideoscape," following Appadurai (1996b, 36), to refer to "concatenations of images" that are "often directly political."

51 I should note again that many of my arguments here are based on personal experience as well as reading the literature. In this case, I can report on a social psychology experiment in which I participated as a research assistant, in which the responses to pornographic films shown to male college undergraduates taking introductory psychology were discussed as indicative of the general human response to pornography. See Linz, Donnerstein, and Penrod (1988), which refers to male college students as generic "subjects" throughout. In parallel, Mueller and Donnerstein (1983, 62) use female college students "enrolled in an introductory psychology course" as stand-ins for women in general.

52 According to the postwar Parsonian division of academic labor, anthropologists were to focus on "cultural" (post)colonial peoples while sociologists focused on "social" practices in the metropoles.

53 Robin (2001, 189–199) traces this shift to the desire by the "softer" social scientists at Rand to collaborate with the economists and physicists, but it is unclear why they would not have felt this urge much earlier. The reasons for this shift need not detain us here, however, as both behavioralism and rational choice represented alternative forms of (depth-realist) positivism.

54 The first generation of regulation theorists was relatively uninterested in geopolitics. Aglietta's (1987) treatment of the "U.S. experience" 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 proceeds to bracket this global aspect entirely (32). Lipietz (1987) discusses only the economic aspects of "global Fordism." Some of the essays in Brand and Raza (2003) begin to correct this shortcoming with respect to *post*-Fordism, as do Hardt and Negri (2000; but see Steinmetz 2003b). The regulationist literature still has not specified the geopolitical dimensions of Fordism, even though Aglietta claimed to be constructing "a set of concepts as a precondition of such investigations."

55 Competition with the Soviet Union for the allegiance of the Third World was another major impetus for the United States to distance itself officially from overt racism in this period, for instance, by integrating its army units during the Korean War.

56 See Steinmetz (2003b) for a discussion of the distinction between imperialism and

colonialism with respect to current U.S. geopolitical strategy. There was, of course, a huge literature in the nineteenth century that constructed non-Western cultures as earlier stages of European civilization (Stocking 1987), which legitimated colonization while simultaneously suggesting a certain kinship with the colonized (Steinmetz 2003c). In most colonies, however (including British), the colonized were treated as ineradicably inferior, or as assimilable only in the very longue dureé.

57 U.S. anthropologists had always been different from European anthropologists, of course, because the U.S. colonial empire was small and short-lived and the exact status of North American indigenous peoples with respect to the category of colonialism was unclear.

58 The problematic character of this conversion of labor power into labor is a "structural" reason for the lesser impact of positivism on studies of the labor process, from Mayo (1933) to Willis (1977) and Burawoy (1979).

59 A better illustration of this cold war assumption than theoretical texts on the end of ideology is the discussion in John Le Carré's novel *The Spy Who Came in from the Cold* (1963/1991, 114) between the British agent Leamas and the East German lawyer and spymaster Fielder. When Fielder asks about the philosophical motives of the British spies, Leamas replies, "What do you mean, a philosophy?" adding, "We're not Marxists, we're nothing. Just people." Fielder asks, "What makes them do it then?" and Leamas insists again, "Perhaps they don't even know; don't even care."

60 This is based on discussions with the current and former directors of the International Institute at the University of Michigan, David William Cohen and Michael Kennedy, and with the current and former directors of the Social Science Research Council, Craig Calhoun and David Featherman.

61 Of course, this was not a regulation-theoretic guidebook; that would not be logically impossible, however. Indeed, one of the directions regulation theory has taken in recent years is to propose itself as a policy science for capitalist crisis management (Röttger 2003).

62 See Lukács (1954/1981), Stern (1961), Plessner (1959), and Mosse (1964). For discussion of this tradition, see Blackbourn and Eley (1985) and Steinmetz (1997a).

63 Both were reprinted in the 1969 *Positivismusstreit* collection, translated into English in 1976 (Adorno et al. 1976).

64 Abbott (1999, 9) argues that there was a split between a positivist mainstream led by Columbia and a "Europeanized" theoretical orientation associated with Parsons and Harvard in the postwar period. Hollinger (1996) contrasts both Columbia and Harvard with a more firmly positivist University of Michigan in the same period. I hypothesize instead that there was a division of labor between these (and other) sociology departments, but with all of them agreeing on the same deeper set of underlying premises. The fact that Lazarsfeld and Rosenberg (1955) addressed philosophy of science issues more directly than Festinger and Katz (1953), for instance (Hollinger 1996, 149, n. 53), does not indicate any epistemological disagreement between the two books, but rather simply a difference between a more and less explicit positivism.

65 See especially Althusser's comments on Pascal (1971b, 168, 169) and Pascal's *Pensées*, sections 3 and 4, especially ¶282.

66 The view of David Riesman (best known for his 1950 book *The Lonely Crowd*) as unscientific that crystallized in this period is confirmed more recently by Clark (1996), who calls Riesman a "journalist." This is just one of a large array of epithets used to denigrate nonpositivists. Riesman was able to attract large-scale funding for his research in the early postwar period, however (Abbott 1999).

67 Mills rejected positivism and scientism but embraced empiricism; Gouldner's *The Coming Crisis of Western Sociology* (1970) also contained empiricist elements (Steinmetz and Chae 2002). Glaser and Strauss (1967, 3), whose book is often recalled as a deviation from mainstream orthodoxy, referred to behavioral data and described the "job of theory in sociology" as "enabling prediction" and social control.

68 Even in the 1930s Parsons wrote that the goal of any analytical science was the discovery of "analytical laws" that state "*a uniform mode of relationship between* the values of *two or more* analytical *elements*" (1937/1949, 622, my emphasis). And despite Parsons's emphasis on values or norms, he fell into an objectivist analysis of the subjective, as Camic (1989, 64–69) notes, reducing it to the single category of the means-ends schema. After the war, Parsons began building various bridges between his theory and empirical research by deriving testable hypotheses of the positivist variety. He even adopted the language of "variables," writing in 1953, "Our attempt is to take the value system as the independent variable . . ." (630). Parsons encouraged a disciplinary division of labor between theory and empiricist research.

His theories were not the most obvious choice for a positivistic sociology, however, even if one was convinced that they could be tested empirically. The almost complete disappearance of Parsons from sociological research and teaching after the 1960s, and his reemergence as a topic for specialists in theory during the 1980s, suggests that he was not, in fact, a crucial component part of the positivist *dispositif*. His continuing influence during the two decades after the war was due not to his compatibility with positivism, which was always questionable. As Chriss (1995, 48–49) argues, one of the reasons for the rising popularity of Homans in the second half of the 1960s and his displacement of Parsons in the citation counts by this time was that his microsociological approach was much more amenable to quantitative testing and experimental design (see also Klausner and Lidz 1986). Gouldner suggested that Parsons's theories, especially in their postwar "structural-functional" guise, could be read as an attempt to mend a social whole that had been fragmented by capitalist modernity. Parsonian theory, according to this reading, was attractive less for its match with positivism or any other guiding metaphysical commitments than for its fulfillment of an ideological function. A more likely reason for Parsons's attractiveness has to do with his contribution to a discipline lacking an object. His work after the war, beginning with *The Social System* (1951), allowed U.S. sociology to rediscover its lost object realm, the social, which has perennially been considered too metaphysical and European. Yet sociologists could not hope to compete with the economists without a proper object. But Parsons reinvented sociology's object just at the moment when positivism was imposing a ban on such abstractions. Positivist sociology won the battle for pseudo-scientific respectability, but discipline's familiar symptoms of dissipation, fragmentation, and triviality are in part long-term consequences of the absence of a proper concept of the social.

Parsons's student Robert Merton played a paradoxically central role in translating Parsonian grand theory into terms compatible with methodological positivism. Beginning in the late 1940s, Merton developed a "program for concentration on 'theories of the middle range,'" as Parsons summarized it (1937/1968, 1: ix). In a paper on "The Bearing of Sociological Theory on Empirical Research," Merton argued that there was

"no logical reason" for generalizers like Parsons to be ranged against the "hardy band" of empirical researchers (1948/1968d, 139). Merton's extremely influential paper on "theories of the middle range" (1936/1968b, 46) suggested that broad theories could not be "effectively developed before a great mass of basic observations has been accumulated," thereby turning Parsons's earlier theoreticism on its head. Although Merton did insist that such middle-range theories were "more than . . . mere empirical generalization" (41), they were not necessarily theories of real causal mechanisms. By acknowledging that a given middle-range theory could be "consistent" with a wide range of divergent "broad theoretical orientations" (such as Marxism, functionalism, and behaviorism), Merton pushed his conception of middle-range theory very close to empiricism. Indeed, middle-range theories were summarized as "verifiable statements of relationships between specified *variables*" (52; emphasis added), and not as pictures or models of theoretical mechanisms that might or might not give rise to events at the level of the empirical. In scientistic-naturalist style, Merton asserted that the long-range goal of the social sciences was to become like the natural sciences, even if sociologists were not yet ready to "compare biceps with their bigger brothers" (47). As a scion of Parsons and tenured professor at the Columbia department in the period of its greatest glory, Merton's intervention gave a green light to sociologists who hoped to operationalize Parsons's apparently abstruse concepts, to convert them into empirical generalizations—and essentially to continue working as before.

69 One of the most interesting studies from this period is Schwendinger and Schwendinger (1974). See also Herpin (1973) and Ladner (1973). The years 1970, 1973, and 1976 were the high points for books on this topic until the end of the 1980s, when the new sociology and history of science began to bear fruit.

70 I am thinking about the ways the new social movements of the 1970s and 1980s represented, on the one hand, cries of despair and symptoms of the destruction, atomization, and fragmentation wrought by the dismantling of Fordist protections (the decline of labor unions and the welfare state) and on the other hand, models of a post-Fordist form of sociality (social movements organized around niche subcultures, multiculturalism, and identity politics). Whether or not one supported these movements there is little question that the collapse of Fordism was one of their conditions of emergence and existence.

71 I offer a more detailed account of these theoretical-epistemic turns in sociology, especially the historic turn, in Steinmetz (2004b).

72 Gouldner understood positivism in very different terms from those used here, focusing on political conservatism. But his book also criticized tendencies toward "methodological empiricism" (1970, 454), defined as a form of "value free, high science sociology that . . . serves to defocalize the ideological dimensions of decision-making" and that is "congenial . . . to an 'engineering' or managerial position, in which the client specifies the ends to be pursued while the sociologist provides the means" (104). Like Mills, he criticized the ideas of the sufficiency of technique (Gouldner 1973, 111) and becoming the king's advisor (see Mills 1959, 180; Gouldner 1970, 445).

73 I have found one reference to the "epistemological turn in feminist theory" (Hallberg 1989) and another to the "epistemic turn" used in the sense discussed here (Cerullo 1994).

74 See the comments of Jeffrey Sachs (in Harrison and Huntington 2000, 29–43), who tellingly rejects even the conservative version of culturalism peddled by this volume.

75 The Bush administration's approach to overseas imperialism does not necessarily imply a need for positivist social science along the lines of the 1950s and 1960s, however. In certain respects this new imperialism is much more open to what it would describe as a more flexible, rapid, and improvisational approach to military matters—consider the lack of planning for "nation-building" in Iraq after the 2003 war and the shortage of military manpower and resources. There is more than a superficial resemblance between the military's approach to war and post-Fordist "just in time production." By the same token, the Bush administration's defiance of established realist theories of international relations and its "faith-based" policies suggest a lack of interest in social science per se.

76 I include local and state governments in the internally variegated category of the state here; for further elaboration of this point, see Steinmetz (1993).

77 After a nadir during the Reagan years, federal funding again increased, albeit with new emphases. Whereas the NIH and NIMH were at the core earlier, the life sciences have become crucial, and sociology is again being encouraged to repackage itself along the lines of another set of natural sciences (witness the rise of interest in sociobiology in recent years). See "Before Leaving Health Agency, Shalala Offers a Little Advice on a Big Job," *New York Times*, January 16, 2001, 16. Even more recently, funding has again dipped.

78 The massive uptick of interest in rational choice theory in political science and parts of sociology in recent years cannot be explained by "economics envy," because that was always present. One possible explanation is that rational choice seems to put the agentic individual at its center, corresponding to the felt increase in demands on the individual to be self-promoting and self-reflexive, while simultaneously promising to allow behavior to be predicted.