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REMAKING MODERNITY Politics, History, and Sociology

EDITED BY

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In memory of our friend,
Roger V. Gould

GEORGE STEINMETZ

The Epistemological Unconscious of U.S. Sociology and the Transition to Post-Fordism: The Case of Historical Sociology

The Domestication of Historical Sociology?

In a much discussed essay, Craig Calhoun (1996) argued that American historical sociology was "domesticated" in its search for professional legitimacy. Sociologists' turn to history during the 1970s and 1980s initially challenged some of mainstream sociology's entrenched methodological and epistemological assumptions. Yet historical sociologists, according to Calhoun, elected to "play on the turf of their mainstream colleagues, not just in placing an emphasis on empirical research ahead of theory and epistemological critique" (p. 309), but also in promoting a research framework that was compatible with the dominant meta-position in postwar American sociology. In this chapter I analyze this position, which I call *methodological positivism*.

This chapter first examines the consolidation of methodological positivism as the *epistemological unconscious* of American sociology in the postwar period and then looks at the historic turn in American sociology since the 1960s, exploring its partial domestication and its contemporary condition, which I will describe as epistemically unsettled and far less monolithic than Calhoun predicted in 1996. In trying to understand how historical sociology had moved through stages of historical critique, partial recuperation, and present dispersion, I propose a *historical sociology of sociological knowledge*,

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focusing on the period since World War II and placing historical sociological research within the broader framework of postwar American sociology. My account differs from others by emphasizing the changing *structures of plausibility* of different ways of thinking about the social. It is, to paraphrase Pierre Bourdieu, an analysis of the "spontaneous epistemology" of sociologists qua social actors. Drawing on regulation theory (Jessop 1999, 2001; Steinmetz 1993, 1994b, 1997b), I will argue that the patterning of social life by modes of regulation—specifically by Fordism and later by post-Fordism (both of which are defined and discussed below)—has important implications for sociologists' conceptions of social epistemology. Concepts like "capitalism" and "modernity" are too broad-gauged to explain medium-term shifts in sociologists' metaphysical and epistemological assumptions, despite the claims of Marxists like Lukács (1968: 110–148), who saw modern "bourgeois" philosophy as a product of reified consciousness per se. After all, Marxism, logical positivism, and post-structuralism are *all* historical products of one and the same "capitalist modernity." By attending to the shifting structures of social regulation, we can make sense of the specific *timing* of epistemological challenges within social thinking and sociology, including the upsurge of adherence to positivism after 1945 and the rise of the various non-positivist movements since the late 1960s. We can also understand something about the *depth of sympathy* for these movements and their particular *emphases*. My overarching theoretical model thus concerns the impact of modes of regulation and societalization on the relative plausibility of different social epistemologies.

This is not to deny that the factors emphasized in earlier work by historians and sociologists on the rise of positivism explain a great deal about the dynamics of domesticating historical sociology. Such forces include the pressures of professional legitimation and the priorities of funding agencies on sociologists (Kleinman 1995; D. Ross 1991; Schwendinger and Schwendinger 1974; S. P. Turner and Turner 1990). Other factors include the "hysteresis" of epistemological habitus (Bourdieu 1984); the dynamics of patronage and gatekeeping (S. Cole 1983; H. A. Zuckerman and Merton 1973); and enlisting allies (Latour 1987). Although these factors have been especially important in accounting for the ongoing *recuperation* of critical intellectual movements in the discipline of sociology, they cannot explain the emergence of these movements or their specific substantive focus. Nor can these approaches make sense of the partial expansion of non-positivist forms of sociology during the past decade, both within historical sociology and in other subfields. Professional legitimacy and funding priorities have *not* shifted to non-positivist forms in this period. Although a "taste for necessity" (Bourdieu 1984: ch. 7)

might account for the adoption of non-positivist epistemologies among dominated fractions of sociology, this structural mechanism cannot explain the *increase* in non-positivist positions over time.

As noted, this chapter begins by reconstructing the consolidation of American sociology's *epistemological unconscious* around the assumptions of methodological positivism (summarized in table 1) in the three decades between 1945 and 1975. I then examine the historical turn in sociology during the 1970s, emphasizing its connections to the shifting structures of social plausibility, which were themselves related to the ongoing collapse of Fordism. Like the other critical movements in American sociology that initially challenged methodological positivism, historical sociology was indeed, as Calhoun argued, partly recuperated. But the *shearing pressure* between the social structures of plausibility and the recuperative enticements in the scientific field has become more intense in recent years. One result is that even texts that intend to defend positivist orthodoxy are often torn between positivist and non-positivist positions, becoming incoherent and internally contradictory as a result. The conclusion analyzes one such exemplary text in detail. The entire field of sociology in the United States is not moving in a post-positivist direction; rather, the methodologically positivist position that used to be *doxic* has been forced to become more explicit and *orthodox* or else has diluted its claims, taking on board some elements of non-positivism (see Bourdieu 1977: 167–169 on the distinction among doxa, orthodoxy, and heterodoxy).

Methodological Positivism Defined

The intellectual formation I am calling *methodological positivism* contains, first of all, a set of *ontological* assumptions about the nature of social reality, objects, and causality. Closely articulated with these ontological foundations is a set of *epistemological* precepts concerning the way in which social facts can be known. A third element of this formation is a *scientistic-naturalist* belief in the unity of the social and the natural sciences. A final set of assumptions concerns social science methodology and the form in which social science should present itself. *Methodological positivism* can be juxtaposed to several other understandings of social science, including critical realism and conventionalist idealism.[1] These contending formations and their individual components are summarized in table 1.

1. 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 the object of knowledge. Bourdieu (1988) is conventionalist insofar as his account of the

This is not an ideal-typical definition but a realist one that attempts to capture the central elements of the actual practices of sociologists.[2] Disaggregating the ontological, epistemological, and methodological dimensions of this formation makes it possible to distinguish the different kinds of *partial* breaks with methodological positivism that have characterized historical sociology in different periods since the 1970s. Carefully defining this conglomerate position also makes it possible to identify latent positivist positions *symptomatically* within texts that avoid the explicit language of positivism. Such avoidance of the language of positivism has been the usual situation in sociology since the 1950s.

Empiricism is an *ontological* position according to which "there is no real difference between 'essence' and 'phenomenon'" (Kolakowski 1968: 3). Empiricism therefore rejects the invocation of theoretical, abstract, and unobservable entities. Empiricism suggests that the explanatory elements of an explanation—the explanans—are located at the same phenomenal or actual level of reality as their explananda (Hempel and Oppenheimer 1948).[3] Empiricism holds that reality, or at least any reality that can be legitimately included in a scientific account, is observable using current observation technologies (R. W. Miller 1987: 359–363). As Andrew Collier pointed out, empiricism in the social sciences is most often expressed as the denial of the existence, plausibility, or usefulness of conceiving of "underlying structures which determine events, and instead locates the succession of cause and effect at the level of events" (1994: 7). Empiricism is not necessarily associated with any concepts of causality—hence the need to distinguish between empiricism as a position within ontology and positivism as a position within epistemology. Indeed, Foucault and his followers often converge with older versions of empiricism in their proscriptions on "depth hermeneutics" and causality itself.[4] Actualism/empiricism can thus be contrasted with a depth realism, which begins from a vertically stratified picture of reality. Differentiating the levels of the empirical and real allows for disjunctures (*counter-*

domination of specific scientific fields focuses exclusively on considerations of power (capital) and not on the correspondence between scientific beliefs and the objects of which they are theories. Bourdieu (1990) recognized correspondence only as a sort of utopian horizon.

2. The theoretical basis for my definition of methodological positivism is critical realism (Archer 1995; Bhaskar 1979, 1986, 1989, 1994, 1997; A. Collier 1994; A. Sayer 1992; Steinmetz 1998; Steinmetz and Chae 2002).

3. Bhaskar (1997: ch. 2) called this proscription on ontological depth *actualism*; Kolakowski (1968) called it *phenomenalism*.

4. See, for example, Foucault, whose "archaeological" approach "reveals relations" that are "*not* intended to . . . isolate mechanisms of causality" (1972: 162; my emphasis).

phenomenality) between underlying causal mechanisms or deep structures and observable phenomena.[5] Depth realism also allows for a *horizontal* stratification of causal mechanisms, suggesting that in *open systems* like the social, a multiplicity of mechanisms will typically combine in conjunctural ways to produce any empirical event (Bhaskar 1986: 110; A. Collier forthcoming).[6]

Positivism is a well-established philosophical position with particular importance for sociology due to the role of Auguste Comte in coining and popularizing the terms "sociology" and "positivism" (Comte 1975 [1830–1842]); Giere and Richardson, eds. 1996; Halfpenny 1982; Kolakowski 1968; Scharff 1995). Yet self-identification as positivist has come to seem unfashionable, even curmudgeonly, within sociology, something best avoided (Despy-Meyer and Devriese, eds. 1999: 95–143). More widespread than identification with positivism or any other explicit epistemological position is the simple avoidance of all such discussions. Indeed, the central sites for the communication of positivism to sociology students for many years have been introductory textbooks on sociology, statistics, and methods (e.g., Blalock 1964; Hanushek and Jackson 1977; Lundberg, Schrag, and Larsen 1954).[7] The communication of methodological orthodoxy is also embedded or embodied within research monographs and papers. Defining positivism in philosophical terms allows us to avoid using the term as an epithet and to distinguish between sociologists' explicit or manifest descriptions of their position and the latent assumptions expressed in their texts.

Positivism, by contrast, is a position within *epistemology*. It insists that scientific explanation take the general form "if A, then B" or some more elaborate version of the Humean "Constant Conjunction of Events." According to Hume, "we may define a cause to be *an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second*" (1975 [1748]: 76; emphasis in original). Such statements presuppose the *invariance* of empirical relationships. Invariance is rendered possible in

5. Although the term "mechanism" runs the risk of sounding mechanistic, it is preferable to all of the alternatives, such as "deep structure," with its roots in French structuralism, and "causal entities," with its Platonic connotations. The conceptual goal in making such a distinction is to signal a difference between (a) relatively enduring social structures that are capable of being reproduced unintentionally and without the conscious awareness of social actors, and (b) the empirical effects these structures are capable of producing. The best illustration of the difference between "events" and "mechanisms" is the psychoanalytic imagery of the generation of symptoms by the unconscious (Rustin 1991, 1999).

6. J. R. Hall complained that "it remains for metaphysical realism [including critical realism] to offer the social ontology that it would claim to warrant" (1990: 344). Bhaskar's (1979) critical realism does, in fact, sketch out the basic lineaments of such a social ontology.

7. Key texts from the 1960s and early 1970s are cited in Gartrell and Gartrell (1996: 144); for a critical discussion of Blalock, see R. W. Miller (1987: 240–241 n. 11).

Table 1 Different Understandings of Social Science

Premises about core aspects of Social Science	Three approaches to social science: Methodological Positivism	Critical Realism	Conventionalist Idealism
Epistemology	Positivism Search for constant conjunctions of events (i.e., general laws)	Conjunctural and contingent causality is assumed to be the norm in open systems Distinguish between theory and explanation	Conventionalism Theories are accepted or rejected due to convention, not to correspondence with their objects No rules for explanation
Ontology	Empiricism Assume irrelevance or nonexistence of non-phenomenal level Assume concept-independence of practice Assume time-independence of social-theoretical concepts Assume space-independence of social concepts	Transcendental Realism Assume disjuncture between phenomenal and real levels Assume concept-dependence of practice Assume time and space dependence of social concepts	Idealism Theories of the world create states of affairs in the world
Stance on Scientific Naturalism	Scientism Natural and social sciences are identical Values and facts are radically distinct Sociology as a technical *Hilfswissenschaft*	Critical Naturalism Social sciences differ from natural ones: social mechanisms are only relatively enduring Social and natural sciences similar insofar as social and natural systems are both open ones	Critical Anti-Naturalism Social sciences are assumed to be radically different from natural sciences, although some versions are conventionalist in treating the natural sciences as equally socially determined
Methodology	Hempel's Deductive-nomological model Privilege given to quantitative methods and experimentation	Denies fact/value distinction Methods should be appropriate to the object of study	"Anything Goes"

two different ways. One is the ontological assumption of phenomenalism or actualism. But it is possible to retain the rule of invariance even if one embraces depth realism—that is, if one breaks with empiricism—as long as the relevant underlying explanans are uniform across all instances of a given explanandum. This sort of *depth-realist positivism* (not presented in table 1) can be found in some versions of traditional or Hegelian Marxism (see Althusser 1990 [1965]; Postone 1993), those versions that posit a unitary cause of all instances of a given type of empirical event. As Somers (1998) has argued, this hybrid position is also characteristic of many rational choice approaches that are realist about ontology but positivist about epistemology.[8] Although empiricism and positivism have been closely interwoven historically, they have also periodically diverged (Harding 1999). Some rational choice theorists have also moved away from assumptions of general or universal laws, however (see Elster 1998), while others embed models of rational decision making within broader contexts of contingent cultural rules (Laitin 1986, 1999).

As noted, the third constituent element of methodological positivism is its adherence to a strong version of scientific naturalism. "Naturalism" in this context refers not to aesthetics but to the philosophical assumption that the social world can be studied in the same manner as the natural one. *Scientism* is a more stringent variant of naturalism that "claims a complete unity" between the natural and social sciences (Bhaskar 1994: 89). Scientism is therefore closely linked to assumptions about ontology and epistemology, but it has additional implications for sociological methodology (row 4 in table 1) and ontology (row 2 in table 1). Due to the central place of quantification, experiment, and prediction in the natural sciences and because natural science is often incorrectly assumed by sociologists to be both empiricist and positivist, many social scientists have assumed that these are appropriate and feasible goals for their own work.

Sociological scientism had three consequential implications for social ontology. Scientism militates against the recognition of the *concept-*, *time-*, and *space-dependence* of social structures and practices. The notion of "concept-dependency" refers here to the claim that human practices and social structures do not exist independently of human theories about them. Social practices are not "brute facts" (C. Taylor 1975: ch. 3; 1979), in other words. Without taking the signifying dimensions of social practices into account, we literally cannot tell what sort of behaviors they are, as Geertz famously illustrated with his discussion of the indeterminate and context-bound meaning

8. Of course many rational choice theorists are not realist about the concept of human rationality but describe it as a heuristic device and a fiction (e.g., Friedman 1953).

ism (Mihic, Engelmann, and Wingrove 2005).[10] Arguments about the possible connection between positivist notions of value-freedom and the totalitarian "banality of evil" (Adorno and Horkheimer 1986 [1944]; Arendt 1965) were already largely dismissed or ignored during the cold war as antipositivism was brought into unsavory correlation with fascism.

Research methods in the narrower sense are both selected and limited by these three basic scientistic assumptions (see table 1, row 4). Qualitative data and methods are compatible with positivism and empiricism, just as quantitative data and methods are in some cases compatible with non-positivist (realist or idealist) epistemologies. Sociology's particular understanding of the natural sciences led it to privilege quantitative methods, however, as well as experimentation, even though true experiments are logically impossible within open systems like the social.[11] That quantitative methods do not flow necessarily from a commitment to positivism and empiricism underscores the fact that the actually existing set of rules is a contingently hammered-together *dispositif* or assemblage of rules.[12]

Finally, particular ways of representing the social textually and visually tended to be associated with methodological positivism. Statistical and tabular forms of presentation are understood as preferable to textuality; narrative or experimental forms of textuality were widely dismissed as unscientific. Just as vision itself was denigrated in twentieth-century thought and diminished specifically within social theory (Jay 1993; Woodiwiss 2001), visual forms and media (other than tables and statistics) were dismissed in sociology as amateurish. One seemingly trivial but still revealing example of the scientism of postwar sociological modes of representation is the way in which the *American Sociological Review* (ASR) has emulated "hard science" journals by presenting its text as two tightly wrapped columns on each page. A more general representational issue is the shift from books and book chapters to short journal articles as the field's defining format. Ceteris paribus, short articles do not allow authors to develop complex narratives or arguments that interweave multiple strands of causality. A final issue is the marginalization of authorial voice. The implicit message that individual

10. For a recent example, consider the criticism of linkages between social movements and academic research associated with the National Association of Scholars (see Coleman 1992).

11. See Camic and Xie (1994) on the emergence of statistics as a discipline-defining technique at the turn of the twentieth century. The timing of their argument is not antithetical to mine, since I differentiate between the creation of the raw materials for a later positivist orthodoxy during the pre–World War II decades and the actual consolidation of that orthodoxy. (They also focus specifically on Columbia and not on the entire field of American sociology.)

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

voice or interpretation was less significant than standardized analytical techniques and modes of representation was reinforced by sociologists' increasing emulation of the "hard science" practice of publishing articles with a rotating list of authors drawn from a large group. The distrust of authorial voice as subjective was initially linked to the distinction between facts and values, but the voiceless style soon became established in sociology and was reproduced without reference to its original philosophical-political motives.

The combination of positivism, empiricism, and scientism is best described as methodological positivism's center of gravity rather than its common denominator. The cluster's constitutive parts have been differentially emphasized in various periods, sociological subfields, departments, and texts. In recent years, for instance, mainstream American sociology has clung more to positivism and scientism than to empiricism, which has been rejected even by many quantitative sociologists. These differing versions of methodological positivism can still be seen as having family resemblances with one another, as comprising a single category despite their differences of emphasis. If there is a common denominator, it is positivist epistemology, which has allowed even historical, Marxist, and cultural sociology to be presented in the form of closed-system generalizations.

American Sociology's Postwar *Rifondazione* (Refounding) and the Consolidation of Methodological Positivist Orthodoxy

One of the first issues that needs to be addressed in a reconstruction of the genealogy of positivist orthodoxy in U.S. sociology during the postwar period is the fact that many of its discursive and material-institutional building blocks were already in place before World War II.[13] Many of the nineteenth-century founders of sociology worshiped at the altar of the natural sciences. The specific cluster of naturalism, empiricism, and positivism had been promoted by Comte, whose "hierarchy of the positive sciences" placed mathematics, astronomy, physics, chemistry, and physiology before "social physics" (Comte 1975 [1830–1842]: 101).[14] Spencer (1972) assumed that natu-

13. This section is a much condensed version of an analytical historical sociology of the postwar period on which I elaborate in more detail in Steinmetz (forthcoming a). (I use the term *Rifondazione* in a loose analogy to preservation of traditionally communist positions by the Italian Communist Refoundation Party (Partito della Rifondazione Comunista), formed in 1991 after the dissolution of the old Italian Communist Party. Similarly, American sociology consolidated a positivist orthodoxy in the postwar period, just as philosophers of science were beginning to move away from positivism.

14. Although Comte introduced the term "positivism," the differences between his definition and the one that prevailed in the twentieth century are extensive (C. Bryant 1985).

ral species provided a grid for taxonomizing different types of societies, and Durkheim (1915: 486) elevated science itself to an object of quasi-religious worship. The philosophical writings of Ernst Mach (1886), the godfather of logical positivism, were extremely important for early American sociology. Founders of American sociology such as Franklin Giddings drew on Mach's empiricist theory, according to which science and knowledge in general were based entirely on sense impressions (see also Bannister 1987: 72–73; D. Ross 1991: 227; Toulmin 1969: 33–35). Karl Pearson, who promoted a positivist, empiricist, and naturalistic understanding of the social sciences, was also influential. Pearson's 1892 *The Grammar of Science* was adopted by many of the first generation of American sociologists in their self-transformation from reformers and social evolutionists into social technicians (Bannister 1987: 151; G. Levine 1996). In that book, Pearson reproduced a sketch by Mach of the view from inside the scientist's head, looking out at the world, to illustrate the argument that sense perception was the sole source of knowledge (see fig. 1). Pearson also argued that science found its "fullest expression" in statistics (Bannister 1987: 151). The scientific naturalism of early founders of American sociology, like Giddings at Columbia University or Albion Small at the University of Chicago, was explicit (Dibble 1975; W. T. O'Connor 1942; Vidich 1985: ch. 8).

Positivist approaches were also actively promoted by the Laura Spelman Rockefeller Foundation during the 1920s and 1930s, under the guise of making sociology less theoretical and political and more scientific, applied, and similar to the natural sciences. Major grants were made to a number of sociology departments, the list of which is startlingly similar to the roster of top-ranking departments today (Bulmer 1982; Fisher 1993; D. Ross 1991). Ahmad (1991) supports Fisher's (1993) conclusion that the Laura Spelman Rockefeller Foundation rewarded a positivist version of social science with an orientation toward predictions and practical applications for business and the state (but compare Bulmer 1982, 1984).

Nevertheless, American sociology was epistemically unsettled during the entire period from the Gilded Age to 1945, lacking any consensus about criteria for scientific authority and distinction. Many leading figures in early American sociology, including Sumner, Mead, Cooley, Veblen, Parsons, and Howard P. Becker, rejected some version of positivism. Non-positivists had a voice in the *American Journal of Sociology* (AJS) and the ASR, which began publication in 1936. In 1939, the ASR carried a critique of Comtean positivism by the founder of an earlier version of critical realism, the philosopher Roy Wood Sellars (1916). The first two volumes of ASR also carried essays on cultural theory, psychoanalysis, Lenin's theory of revolution, and the topic of "imagination in social science." Even the Sociological Research

Figure 1

Association, an elite, invitation-only professional group that was formed in 1936 in response to battles within the American Sociological Society between "value-free" positivists and "humanistic" social activists, was itself divided between theoretical and more positivist wings (Bannister 1987: 189, 218). At the end of the 1930s, American sociology was not yet a "scientific field" in the Bourdieuian sense of the term—that is, it was a fragmented field, not one in which all actors shared a set of criteria, conscious or unconscious, defining scientific capital (Bourdieu 1985, 2001; S. P. Turner and Turner 1990: 75).

Parsons began his discussion of the positivist tradition in 1937 with the question, "Why has it died?" Parsons (1993) also produced some of his most historical and non-positivist work in his wartime writing on Germany and Nazism. Ironically, this was the swan song of anti-positivism in American sociology. In defiance of Parsons's performative speech act, positivism came roaring back to life immediately after the war and soon came to dominate

American sociology.[15] Indeed, it persisted as *doxa* until sometime between the late 1960s and the mid-1970s and still functions as *orthodoxy* within the field today. How can we account for this rapid and overwhelming consolidation of positivist doxa?

Scientific Captial, Modes of Regulation, and Ideological Resonance

Bourdieu's notion of the field (*champ*) is a useful starting point for analyzing postwar changes in U.S. sociology. A field contains diverse positions and viewpoints but is nonetheless clearly structured, such that certain positions are recognized by the dominant and the dominated alike as the most *distinguished* (see Bourdieu 1981, 1984, 1985, 1986, 1988). Bourdieu notes that epistemological differences are a central axis of distinction within scientific fields, writing that "a survey on power in the scientific field could perfectly well consist of . . . epistemological questions alone" (1981).[16] Certain methodological positions have more *scientific capital* than others, all other things being equal. All participants in a well-structured field recognize a common definition of scientific authority and competence, a hierarchy of distinguished and less distinguished positions, even as the dominated develop a somewhat perverse taste for their own domination. My argument, for which I can provide only partial evidence here, is that sociological scientific authority increasingly accrued to methodologically positivist positions during the postwar decades.

Unlike earlier anthropological notions of culture, this imagery of structured fields does not require any assumption of cultural uniformity. Unlike Gramscian hegemony, it does not suggest that alternative positions are unthinkable. Instead, it suggests that alternative *valuations* of heterodox positions are unworkable. Epistemological alternatives were never absent in postwar American sociology. Marxism, cultural sociology, and the sociology of science all had prewar precursors, and all of them continued to lead at least a desultory existence throughout the cold war period.

It is also important to keep in mind that what counts as distinguished is field-specific. What counted as cultural capital within sociology certainly would not have been recognized as such in the broader arena of American cultural life during the cold war. Positivism has had no eminent

15. In the field of psychology, by contrast, positivism already flourished during the war as a result of wartime policies (Herman 1996); here, behaviorism was stronger than epistemically analagous positions within sociology (Buckley 1989).

16. Interestingly, Bourdieu is discussing American sociology here.

defenders in philosophy for many decades; already in the early 1950s philosophers observed that "the words 'positivist' and 'positivism' seem to be in general disfavour . . . as a result of incriminating overtones which accrued to them in the twenties and thirties" (J. W. Smith 1952: 190). The dominance of positivism within sociology might also seem to contradict Bourdieu's general thesis that distinguished positions tend to exhibit a greater degree of "distance from necessity." Yet we should not necessarily define such distance in aesthetic terms. *Empiricism* is indeed more concrete than realist ontologies, and thus in one respect it is closer to "necessity" in Bourdieu's sense. From another perspective, however, *positivism* understands itself as less concerned with details than the other epistemological positions. Positivism's *abstractness* applies to the explanans as well as the explanandum. By declaring itself uninterested in the idiographic—that is, in the details (or even the identity) of the specific case—positivism keeps the concrete at arm's length. In the social sciences this typically involves replacing place names or proper names with the names of variables. And by emphasizing constant conjunctions, positivism avoids the concrete "messiness" of explanatory strategies that are open to constantly changing concatenations of mechanisms.

This is not to suggest that non-positivist approaches could not have defined themselves as distinguished. Emphasis could have been placed on the fact that simple, parsimonious explanations are actually closer to common-sense understandings of the social, for example. So why was methodological positivism so successful in its bid for leadership? Although Bourdieu helps us to clarify the general sorts of arguments that must be used by *any* position trying to govern a given field, his approach cannot explain why specific definitions of distinction are more successful than others in any given time and place. Obviously economic (and social) capital can enhance the value of cultural capital, at least indirectly. The resources offered by private and government sources to sociologists who agreed to configure their work along the lines of the natural sciences played a key role. But these resources were also available during the prewar period, and they still exist today, yet their ability to shape the discipline has varied over this entire period.

Bourdieu's theory allows us to understand the operation of cultural capital in consolidated, settled fields, but it is poorly equipped to explain why particular ways of describing society come to be recognized as more distinguished than others. Bourdieu's relatively ahistorical and conventionalist account of science needs to be reformulated in order to understand the changing *conditions of plausibility* of different ontologies and epistemologies

of the social.[17] To explain why certain visions of the social seem more intuitively reasonable than others, to understand why some intellectual challenges are able to emerge and seize the imaginations of social scientists despite entrenched interests, habituses, and the preferences of external agencies, we need to consider the overall context of social regularities. It is possible to argue that certain ways of seeing the social world are *empowered* in particular historical epochs by their resonance with that world without thereby reducing them to a simple reflection of some external reality (Peirce 1955: ch. 7).

This suggests that we need a theory of the aspects of the social worlds that are most relevant to social epistemology. We also require a theory that is able to make epochal distinctions within the broad period of capitalist modernity, if social structures are to account for social epistemologies, given that the latter have changed and evolved over the timeframes designated as "modernity" and "capitalism." Regulation theory is one of the few social perspectives able to fulfill both of these requirements. The distinction between Fordism and post-Fordism within this framework (see below) underscores the theory's goal of periodizing analytically *within* the broader epoch of capitalism. Regulation theory has also emphasized the influence of system-level regularities on subjectivity and culture (Hardt and Negri 2000; Harvey 1989; Hirsch and Roth 1986; Steinmetz 1994a, 1994b, 1997b). We can extend this to the investigation of system-level effects on spontaneous social epistemologies.

My argument does not reduce to a reflectionist theory of social perception in which ideologies are understood as mirrors or as partial, blurry, or inverted images of reality. A mirror (or *camera obscura*) approach would involve the absurd claim that post-Fordist restructuring is leading social actors to become spontaneous regulation theorists (if we assume, for the sake of argument, that regulation theory best captures these ongoing social changes). Instead, as theorists such as Jameson (1984) and Roudinesco (2001) have argued, contemporary social transformations are more likely to be expressed in subjective forms such as feelings of ephemerality and depthlessness. By arguing that positivism and post-structuralism are the spontaneous social epistemologies of the Fordist and post-Fordist eras respectively, for instance, one is not claiming that these are *adequate* forms of knowledge.

The connection between social epistemology and Fordism should not be understood as a difference between a dependent cultural realm and an independent acultural one but as a relationship of *resonance* between two different formations of practice. Resonance suggests formal and structural homologies rather than any sort of direct mimesis. Regulation theory avoids a reifying language according to which Fordism or post-Fordism would be reducible to brute material or narrowly economic practices (Jessop 1999). The concept of modes of regulation is centered on rates of profit and exploitation, but it encompasses an entire array of patterned cultural, social, spatial, temporal, and political practices as well. It is these broader aspects of Fordism and post-Fordism that are most relevant to explaining the fluctuating degrees of plausibility of positivist and non-positivist epistemology in sociology. Social knowledge may register the shifts in these regulatory modes in oblique and indirect ways. To take a simple example: social crisis may be more resonant with epistemologies that emphasize *discontinuity* than with epistemologies insisting on repeated, general social laws (Bourdieu 1977: 170; 1984: 168). By attending to the changing modes of regulation and societalization, it is possible to periodize the waxing and waning influence of spontaneous social epistemologies.

17. While the present chapter *supplements* Bourdieu's approach via a theoretical account of macro-social patterns of societalization, elsewhere I have reformulated Bourdieu's underlying theory of subjectivity in psychoanalytic terms (Steinmetz 2002, 2003a, forthcoming b).

The determinants of sociologists' social epistemologies can be heuristically described as being external or internal to the scientific field. In explanations of the development of science, the adjective "external" has typically been used to refer to all influences on science that do not relate to the intellectual question of the fit between theory and object (Breslau 2003; R. A. Hall 1963). I draw the boundary between internal and external influences at a different point, with the *inside* encompassing subfields, disciplines, universities, research institutions, and funding agencies. The *outside* then refers to other sociocultural factors that influence science, as well as branches of the government concerned with science.[18] This is not to deny that each of these separate levels may have its own distinctive fieldlike properties. But the sociology of sociology has not paid enough attention to the extra-scientific sources of social epistemologies, focusing instead on intra-scientific dynamics and on interventions by government and private funding agencies (Ahmad 1991; Fisher 1993; S. P. Turner and Turner 1990; D. Ross 1991). The impact of broader social structures on sociologists' epistemological leanings—a field of inquiry suggested by Marx and Engels in *The German Ideology* (1970)—has been left largely unexamined.

The Bourdieuian theory of fields and scientific capital, combined with a regulation-theoretic sociology of social knowledge, allows us to move beyond the usual focus on material blandishments and sanctions in explaining

18. Examples of such external sources would include Puritanism and pietism in Merton's (1936) classic study of seventeenth century science and the social role of the "gentleman" in Shapin's (1994) study of the same period.

the dominance of methodological positivism and the timing of its consolidation. We can now understand how the intellectual raw materials of methodological positivism could have existed for many decades before becoming dominant. Fordism may have lent credence to methodological positivism, but it did not create it.[19] By the same token, the post-Fordist mode of regulation that is currently being consolidated resonates with non-positivist forms of social knowledge production without having given rise to those forms, and it may help explain the stepwise and piecemeal dissolution of positivist doxa in recent years.[20]

The Postwar Conjuncture and Positivist Social Science

The postwar period saw an enormous rise in resources for social science research from the state sector (Featherman and Vinovskis 2001; Gieryn 1999: 65–114; Klausner 1986; Kleinman 1995; Larsen 1992; J. T. Wilson 1983). Leading sociological figures, from Talcott Parsons to Philip Hauser, understood this conjuncture as an opportunity to establish sociology more firmly as a discipline. This would require epistemological convergence, however. The direction of this convergence was suggested initially by the National Science Foundation (NSF), which made funding available to sociologists provided they were willing to generate "social laws" and predictions (Hauser 1946: 382). Parsons wrote a paper in 1948 for the Social Science Research Council that argued that "the same philosophical principles that guided the natural sciences were at the heart of the social sciences" (Klausner and Lidz, eds. 1986: viii). In 1954, the NSF mandate was expanded to include the social sciences. Conditions were laid out defining which sociologists would be permitted to receive funding (Alpert 1954, 1955a, 1955b, 1957; see also Lundberg 1947). The first condition was "the *criterion of science*" specified to mean the "convergence of the natural sciences and the social sciences" (Alpert 1955a: 656). Sociologists were cautioned that access to future funding depended "largely on their capacity to prove themselves by their deeds" (p. 660). During the years 1945–1949, the proportion of articles in AJS, ASR, and *Social Forces* acknowledging outside sources of funding was 17.4 percent; by 1960–1964 this figure had risen to 52.5 percent (McCartney 1971: 388).

The positivist camp was strengthened by other institutional factors (Featherman and Vinovskis 2001). The wartime mobilization brought social scientists into the ambit of the state (Kleinman 1995: ch. 3). After the war many new recruits were brought into sociology from government agencies and hard science disciplines, tending to reinforce methodological positivism (S. P. Turner and Turner 1990: 86–88). Another critical factor was the rise of freestanding research institutions such as the National Opinion Research Center in Chicago and the Institute for Social Research at the University of Michigan (Frantilla 1998). These new research institutes were often physically separated from the extant sociology departments, but they were still able to intervene in the ongoing redefinition of the discipline's identity.

The scope of these postwar ambitions was expressed in a 1948 paper by Paul Lazarsfeld that C. Wright Mills (1959: 59–63) quoted extensively. Students were to stop studying "the history of institutions and ideas" and focus on "the concrete behavior of peoples" and contemporary events—an empiricist preference. Lazarsfeld called for "studying social situations and problems which repeat themselves rather than those which occur only once"—a direct restatement of the positivist definition of science as the search for constant conjunctions. Methodologically, sociology was to cease being an artisanal activity practiced by "the individual observer" and to become more like "organized, full-fledged empirical science" (p. 61). And "basic techniques for finding explanations," according to Lazarsfeld, were "statistical" (p. 63).

What explains this powerful surge of methodological positivism after World War II? While some of the influences were internal to social science—the influx of funding and recruits oriented toward a natural science model, the creation of freestanding centers of "abstracted empiricism" (Mills 1959: ch. 3)—other factors were external. The positivist position was encouraged, first, by an association of anti-science irrationalism with the rise of Nazism and Soviet totalitarianism. Adorno and Horkheimer's 1944 *Dialectic of Enlightenment* (1986) had moved dialectically from a Heideggerian critique of positivism to an explanation of anti-Semitism and Nazism. In the context of the wartime triumph of science and the postwar confrontation with the Soviet Union, however, it became more difficult for anti-fascists to criticize the spirit of modern science itself. Indeed, it was widely argued that it was precisely the *lack* of a modern scientific culture that had contributed to fascism. Suspicion was now cast backward retroactively on the entire non-positivist tradition, running from Hegel and the German Romantics through to Marx, Dilthey, Nietzsche, and the critical theorists of the inter-war period. This was doubly damaging to anti-positivist arguments, since most of these thinkers had emerged from the German-speaking context and could therefore be linked, if only by a vague sort of "guilt by association," with German exceptionalism and hence with [illegible]ism (Steinmetz 1997a).

19. For a similar argument about the ways Fordism "bundles" and reinforces specific forms of practice without actually giving rise to them, see Steinmetz (1994a).

20. See P. Anderson (1998), for example, who traces the origins of "post-modernism" as an aesthetic category to the 1930s and as an epochal category to 1954.

The 1950s thus saw an accumulation of pressures conducive to positivism. Nonetheless, methodological positivism became dominant only at the end of the 1950s and during the 1960s, after a considerable period in which its basic picture of the social had been ratified by the consolidated Fordist mode of regulation. It is to those processes of ratification, or resonance, that I will now turn.

The Resonance of Fordism with the Positivist Social Imaginary

Fordism can be understood as a contingently arrived-at regulatory dispositif, a mixture of social, cultural, political, and economic arrangements that provided a temporary solution to the problems of capitalist instability and profits.[21] Fordism arose in the United States and elsewhere in the advanced capitalist world through processes of trial and error, and not due to some functional logic or central coordination. As Fordism proved attractive to a wide array of powerful actors and institutions, they began to orient themselves toward reproducing it, and it prevailed as a dominant form of social regulation for several decades. Although Fordism was never all-encompassing, its logics had a wide reach and were able to shape the subjectivities and practices of many people (Aglietta 1987; Boyer 1990; Gramsci 1971; Lipietz 1987).

Fordism's dominance in the 1950s and 1960s was not unconnected to the cold war boom in science funding. The Fordist state relied to a greater extent than earlier forms of regulation on positivist social science to track the economy, regulate business cycles and labor markets, survey the population, and bring social practices into conformity with the regularities of mass production and mass consumption. It is more than ironic in this respect that, just as Henry Ford's social system in southeastern Michigan was a microcosm of the all-encompassing postwar mode of regulation, so Ford had created his own Sociological Department in 1913 to promote Americanization, health, and a wholesome family life among his workers through inquisitorial visits to workers' homes and paternalistic interventions (Nevins 1957, vol. 2: 332–354).

American sociology and Fordism were thus connected from the start, and sociologists were among the creators of Fordism as a mode of regulation.

21. See Aglietta (1987) for the first, more economistic statement of regulation theory and a definition of Fordism. The important thing to remember in the current context is that Fordism is not the same as Ford, even if some of the elements of the Fordist social formation were strongly associated with Henry Ford in the U.S. context (high wages, the assembly line, vertically integrated production, attempts to police and control workers' private lives, etc.). Fordism as a social formation is associated most strongly with the postwar period, culminating in the 1960s in most of Western Europe and North America.

But I want to focus on the causal arrows running from Fordism to sociology. Specifically, the array of sociocultural changes that regulation theory gathers under the heading of Fordism contributed to the *plausibility* of positivist approaches to sociological research and writing. The state-organized wartime regime in which many sociologists were involved provided a glimpse of this more orderly life that emerged in the 1950s and 1960s. The world wrought by Fordism seemed increasingly to correspond to a positivist representation of social phenomena as repeated and invariant across time and space, as rational, acultural, predictable, and controllable.

The first aspect of American Fordism that resonated with positivism was the management of economic crisis and the corresponding sense of the end of (economic) history. Fordism seemed to bring capitalist instability under control through a combination of high wages, Keynesian demand stabilization, and social benefits. The muting of capitalist crisis made it increasingly plausible that social practices really were repeatable in ways that could be captured by statistical models and replicable experiments. One could imagine that a wide range of human practices could be construed as constant conjunctions of events while ignoring the historical conditions of possibility of this patterning. The specifically Keynesian-Fordist strategies for dampening the effects of the business cycle lent support to theoretical approaches premised on synchronic rather than diachronic analysis.[22] In contrast to the chaotic social conditions of the Great Depression, organized Fordist societalization seemed to be coherently serial and cyclically repetitive.

A second way in which Fordism shaped sociologists' spontaneous perceptions of social ontology involved the homogenization of consumer tastes, subjectivities, and everyday life. Behaviorist and economic models of psychology, surveys, and statistical approaches seemed appropriate to an increasingly flattened out and standardized culture. The infantilization of the consumer-citizen (Adorno and Horkheimer 1986 [1944]; Berlant 1997) helped to convince social scientists, who were consumers and citizens themselves, that individual consumers and producers were in fact interchangeable. It made increasing sense for undergraduate social psychology laboratory subjects to stand in for the generic American. American intercollegiate football could be compared to "tribal" African cultures and described as another example of a "culture complex" (Lundberg, Schrag, and Larsen

22. Even under Fordism, history could not be ignored completely, of course, given the nonsynchronized spaces of communism and the nonaligned post-colonial world. But modernization theories folded these recalcitrant worlds into the dominant narrative defining them as earlier stages of the normative Western trajectory.

1954; 196–197). Surveys and statistical modeling approaches to human behavior both assumed and rhetorically implied that the units of analysis—individuals—were fundamentally the same with respect to their decision-making processes. And as subjectivities and social practices became stabilized, *prediction* itself started to seem feasible.

The third implication of Fordism for sociologists' spontaneous social philosophy relates to its synchronization of the scale of activities within the contours of the nation-state. As Jessop (1999) and other regulation theorists have argued, Fordism organized economic development and economic flows mainly within and between national states. Neil Brenner (1998) demonstrates that Fordism effected a relative reduction in levels of uneven development within the contours of the national territory. The resulting taken-for-grantedness of the *nation-state space* as the fundamental frame, or container, for social practices made it easier to believe that social events actually did occur in constant conjunctions. By contrast, the idea of social regularities is much less plausible where each type or dimension of social practice has a different spatial reach and location, as is increasingly the case under "glocalizing" post-Fordism, with its myraid spatial reshufflings (N. Brenner 1999; S. Graham and Marvin 2001).[23] Fordism's geosocial character was thus conducive in myriad ways to methodological positivism.

The importance of the spatial organization of capitalism and social life more generally for the social sciences can also be seen in the division between the putatively nomothetic social science disciplines and the supposedly idiographic fields of area studies. Although spatial scale was relatively settled within the capitalist core countries (and the East European socialist ones) during the Fordist era, it remained less so in the so-called Third World. Seemingly continual changes in the location of international borders were one aspect of peripheral spatial uncertainty. The naturalness of the nation-state as the obvious scale for economic, political, and cultural practices was continually called into question in the Third World by international labor migrations and anti-systemic movements (pan-Arabism, pan-Africanism, communism) that partly transcended national boundaries. The nation-state was not only too small to capture the main dynamics of the periphery; it was also too big. Social activities in the periphery often seemed to be more local in their focus, and economic development continued to be more uneven within nation-states. These differences undergirded the division between area studies and the generalizing, supposedly context-free social sciences. Prediction seemed less conceivable in a Third World because Fordism was absent or only partially instantiated there (Lipietz 1987). Other factors played a role in staving off positivist dominance within area studies, including the pervasive view of the non-West as less rational and more cultural; this was conducive to hermeneutic methodological approaches (see Bierstedt 1949: 590; Steinmetz 1999).[24] Positivist forms of social science knowledge that promised to fold the non-West into a generalizing explanatory framework were not entirely lacking, of course, especially where U.S. economic and security interests were at stake.

In sum, while the Fordist state directly subsidized positivist regimes of social knowledge, Fordism as a mode of social regulation and way of life provided sustenance to the positivist social imagination.

Sociology Becomes a Full-Fledged Field

The outcome of this postwar conjuncture was that U.S. sociology had become a well-structured field by the early 1960s. Despite differences of taste or viewpoint, all of the players in a field agree on common stakes and legitimate definitions of field-specific capital. Reputational, social, and economic capital in sociology tended now to accrue to more positivist positions. Fluency in positivist methodological positions began to function as a field-specific form of scientific capital. Even those who rejected positivism often colluded in its rise to preeminence. Talcott Parsons, who had forcefully opposed positivism in the 1930s (and who is currently associated with an anti-positivist strand of social theory; see J. C. Alexander 1982), accommodated to the new doxa.[25] Those who did not conform often "emigrated" to less influential sociology departments (Abbott 1999).

A system of rules that guided the creation of knowledge, the channeling of resources, and the inscription of boundaries within sociology and against other disciplines was thus institutionalized and internalized. The initial period of consolidation in the 1940s and 1950s was still characterized by occasional skirmishing over foundational questions, but by the early 1960s these

23. "Glocal" refers to the mixing up of the global and local scalar levels in contemporary post-Fordism, as opposed to the "containerization" of most social practices within the scale of the nation-state under Fordism (P. J. Taylor 2003).

24. The orientation of area studies fields toward a fetishization of language and "translation" problematics promoted an atheoretical approach that critics of these fields sometimes label "positivism" (Dutton 2002); this is not equivalent to positivism as defined here.

25. I am not following Gouldner (1970) here; see Steinmetz and Chae (2002) for a critique of his conception of American sociological positivism. Elsewhere (Steinmetz forthcoming a), I discuss Parsons's prewar and postwar theories in detail, finding an increasing compatibility with methodological positivism. Robert Merton played a key role in translating Parsonian grand theory into terms that were compatible with methodological positivism, especially with his highly influential empiricist-positivist notion of "middle range" theory.

rules had become largely habitual and commonsensical. Positivist methodological positions won the battle to define field-specific cultural capital, becoming doxic.

Historical Sociology and Other Challenges to Methodological Positivism since the 1960s

The dissident movements that emerged within sociology during the late 1960s marked the first challenge to the discipline's postwar common sense. The incipient collapse of Fordism after the economic and oil crisis of 1973 entailed the disappearance of the social regularities that had indirectly supported a spontaneous positivism in the vision of sociologists. Yet methodological positivism remained dominant in the field, if not doxic, well into the 1990s. American sociology remained well structured as a field, with scientific capital continuing to accrue to more positivist positions. The dominance of this framework was still recognized by most sociologists, whether or not they stood to profit from it (S. Cole 1983: 137). But the emergence of post-Fordism created a disjuncture between the overall structuring of social-epistemological plausibility and the continuing adherence of much of the discipline to the older model. In recent years, methodological positivism has been forced to become more explicit and orthodox or else has relinquished one or more of the tenets that had been central.

Historical sociology is one of a series of challenges to methodological positivism that began in the late 1960s. There have been at least four such movements: critical sociology (neo-Marxism and feminism and, somewhat later, critical race theory); historical sociology, starting in the second half of the 1970s; cultural sociology, reemerging in the 1980s; and the epistemological turn, which was located especially within science studies but not restricted to them, in the 1990s.[26] Each of these movements initially rejected one or more of the mainstays of methodological positivism. Although each of them was partially recuperated by positivism, they were not fully domesticated, *pace* Calhoun, and have been able to recover some of their nonpositivist potential in recent years. There is enough space here to discuss only the second of these challenges, the historical turn in sociology. But the

26. In addition to these four challenges, Gouldner (1970) pointed to the work of Goffman and Garfinkel as having the potential, along with humanistic Marxism, to displace positivism and to introduce a more reflexive approach into American sociology. Both of these theorists provided important sources of continuity with the hermeneutic tradition, and Goffman (1974) was adopted in the 1980s by social movement theorists who were trying to think their way out of their subfield's entrenched aculturalism (Snow et al. 1986).

contributions of the other movements to the critique of methodological orthodoxy can be briefly summarized.[27]

The first challenge came from *neo-Marxism, feminism/gender studies,* and *critical race studies.* Neo-Marxist sociology emerged just a few years after methodological positivism had become enshrined as doxa. Some of the earliest Marxist interventions attacked empiricist ontology by insisting on the legitimacy of unobservable causal mechanisms and theoretical concepts like reification, commodity fetishism, surplus value extraction and the value form, the political unconscious, modes of production, and ideological interpellation. Some neo-Marxists also rejected the scientistic assumption of the temporal invariance of theoretical concepts, arguing that their own central explanatory concepts (such as social class) were time-dependent and not necessarily applicable across all social formations. Feminists and critical race theorists also deployed theoretical, nonempiricist concepts such as patriarchy and mimicry. Theorists called attention to the social construction and geohistorical variability (time dependence) of gender, race, and sexuality.

The *historical turn* in sociology since the 1970s (discussed by the editors of this volume as the "second wave" of historical sociology), like neo-Marxism and feminism, promised to make sociologists aware of the historical mutability of their theoretical categories. Unlike Marxism and feminism, it also emphasized the ubiquity of conjunctural and contingent causality. In formal terms, historical sociology was open to *narrative emplotment.* There was also an emphasis on historicity itself, on time and process, that had been almost entirely lacking during the preceding period (Aminzade 1992; Sewell 1996b). These messages about contingency, conjuncture, figurational analysis, narrative, the historicity of concepts, temporal process, and the category-dependence of social practice ran up against the deeply entrenched dominant epistemic habitus in sociology.

The *cultural turn* in sociology since the 1980s signals a broad-gauged interest in the role of signification and subjectivity in all types of social practice, along with a reinvigoration of the older sociological tradition of studying more narrowly defined cultural "objects" (Griswold 1987; Wuthnow 1989). The main critique of methodological positivism emanating from the recent cultural turn, familiar from earlier interventions dating back to the nineteenth-century *Methodenstreit* (methods dispute), is the insistence that human practice is always an entanglement of meanings and material

27. See Steinmetz (forthcoming a) for further elaboration and references.

substrates.[28] In ontological terms, this militates against the scientistic assumption of concept-independence and against behaviorist or one-sidedly materialist theories. Insofar as it analyzed subjectivity and culture as mutable, diverse, and complex, the culturalist turn discouraged sociologists from looking for trans-historically valid conjunctions of events.

The ongoing *critical epistemological upheaval* within the contemporary human sciences, including sociology, is centered around a reinvigoration of interest in philosophy and social theory, a renewal of Critical Theory's emphasis on self-reflexivity, and the specific subfield of the new science studies.[29] Many of the participants in this literature reject the entire set of assumptions of methodological positivism, although they are divided with respect to alternatives. Some embrace the ideas presented in the second or third columns in table 1 (critical realism or conventionalist idealism). More explicit defenses of positivism, empiricism, and scientism are also emerging in response to this challenge (e.g., Stinchcombe 2002; J. H. Turner 1993). The central axis of debate generally pits defenders of various sorts of philosophical realism against an array of constructivist, conventionalist, and idealist positions that are radically skeptical about the possibility of choosing rationally among contending theories (i.e., that embrace the position known as judgmental relativism; see Bhaskar 1986, 1997 [1975]). This configuration of debate is quite different from that found in the first half of the twentieth century, which was dominated by disputes between positivism and various forms of realism.

Like the rise of methodological positivism during the postwar period, we can understand the emergence of positions critical of positivism only by considering the ways they have been encouraged by the overall mode of societalization. Both the timing of these critical movements and their particular substantive emphases partly reflect the environing structures of social regulation. Again, this is not to downplay the significance of material rewards, allies, patronage, gatekeeping, or sheer habit. These factors have been especially central to the domestication of critical turns (Calhoun 1996; Flacks 1989: 355; Gouldner 1970: 445; Wright 1994: 1–15). But it is difficult to see how they could explain the *emergence* of new paradigms. A competition-based theory of science can account for the continual generation of new challengers, but it cannot explain the specific *contents* of their challenges, except in a formalistic way, as the simple negation or inversion of earlier

28. The Methodenstreit, or "methods dispute," pitted natural science against humanities as models for the social sciences; see Dilthey (1910).

29. See Shapin (1995) for an overview of science studies and the sociology of science.

positions.[30] Nor can a purely internalist approach explain why the current epistemological challenge seems to be less easily assimilable than the critical, historical, and cultural turns. This resistence by the epistemological turn to cooptation cannot be traced to the nature of the object of study itself, since American sociology demonstrated in earlier periods that science could be studied in a positivist manner. Indeed, the second half of the 1960s was a crucial watershed for a shift toward a sociology of science focused on the quantifiable aspects of science, such as citations, discoveries, and rewards, and away from the more historical and interpretive emphasis that still characterized this field in the first half of the 1960s (see Barber 1962; Ben-David 1960a, 1960b, 1971; Ben-David and Collins 1966).

The differences in the trajectories of the four critical turns can be understood in terms of the shifting conjunctures of internal and external determinants. The early neo-Marxists and second-wave feminists in sociology confronted a positivist doxa that was still buttressed by the conforming social regularities of Fordism, whose collapse did not really begin until the 1970s. These critical movements encompassed a fundamental critique of sociology's dominant epistemology, but at the same time, they themselves had been shaped by the Fordist conditions that underwrote methodological positivism. These critical challenges were contemporaneous with, and in many ways parallel to, the first wave of new social movements. Social movements of the late 1960s and early 1970s such as anti-consumerism and environmentalism were directed *against* aspects of Fordism, but in many ways they were also shaped by the very forms of subjectivity that Fordism had generated. Fordism was most likely to become a central target of widespread, vehement protest among those who had experienced at first hand the degrading side of standardization and mass consumerism and whose subjectivity was correspondingly shaped by it (Hirsch and Roth 1986; Steinmetz 1994a, b).[31] Similarly, the critical sociologists of the period before the mid-1970s were motivated by their opposition to a disciplinary formation that had shaped their own intellectual formation. At the same time, this superego dimension of original symbolic identifications introduced re-

30. Neither Karl Popper nor Thomas Kuhn provides a social account of the contents of new theories or paradigms. For Kuhn, a paradigm shift is stimulated by the accumulation of anomalies. But since he is concerned with the natural and physical world, which he assumes is the *same* world before and after the scientific revolution (1970: 129), he cannot use that world to explain the contents of the new paradigm.

31. Conversely, certain strands in the current anti-globalization and far right movements look back nostalgically to a Fordism they never knew, enhancing its positive virtues (see Steinmetz forthcoming c).

cuperative pressures into the confrontation. And the ongoing regularities of domestic Fordism overshadowed the challenges to regulated order emanating from the Third World (Vietnam), reinforcing the positivist social worldview for many critical sociologists.

Taken together, these factors hampered the ability of the early critical antipositivism to capture the imagination of most sociologists. It was on this terrain, coupled with the internal dynamics of hiring, firing, funding, and direct political repression, that the initial critical turns were partly reintegrated. Marxism's epistemological critique of positivism and scientism was divorced from its substantive claims, some of which found their way into mainstream sociological research under the guise of "independent variables." This was perhaps a political victory, but it was an epistemological setback.

The style of Marxist and feminist research that became prevalent in American sociology was thus oriented toward the search for *constant conjunctions of events*—or constant conjunctions between underlying mechanisms and surface events (the position I call depth-realist positivism; see Steinmetz 2004). Marxists and feminists tended to assume away the horizontal openness of the social, rejecting the ontological assumption of a multiplicity of causal mechanisms in favor of arguments for the causal primacy of a single mechanism (capital, class, or patriarchy) or a constant subset of several mechanisms (as in so-called dual-systems theory; see Hartmann 1981).

The most recent critical movements, by contrast, have arisen in a thoroughly post-Fordist environment (A. Amin, ed. 1994; Harvey 1989; Lipietz 1992). Unlike the earlier movements, these critics do not share social conditions of birth with their main opponents. Nor do the new post-Fordist conditions seem to confirm or conform to the positivist perspective to the same extent as Fordism. In many ways, post-Fordism is actually more compatible with non-positivist than with positivist social epistemologies. Post-Fordism has entailed a new set of demands on the individual personality, which is compelled to become more adaptable, flexible, self-promoting, and reflexive and to be able to read social practices hermeneutically, as texts, in order simply to function properly in everyday life (U. Beck, Giddens, and Lash 1994; Lash and Urry 1994; Wernick 1991). In methodological terms, these developments tend to direct people's attention toward the concept-dependence of social practice.

The attempt to define "culture" restrictively as a delimited range of objects is undercut by the generalized "becoming cultural of the economic, and the becoming economic of the cultural" in this more recent period (Jameson 1998: 58, 63). In other words, conditions internal to the sociological discipline may still disproportionately reward positivism, but the world that sociology purports to study does not provide the same level of resonant affirmation. Critical sociology would thus not appear to be as susceptible to recuperation as the neo-Marxism and feminism of the 1960s and 1970s.

The idea of "domestication" is too simple and sweeping to describe what happened to the earlier critical movements in U.S. sociology. First of all, none of these developments was internally homogeneous. Not even during the heyday of multivariate Marxism and quasi-experimental historical sociology in the 1970s and 1980s did historical sociologists adopt the positivist approach en masse. Furthermore, many books and articles that seemed to adopt a discourse that was superficially positivist were punctuated by moments or latent levels that were less positivist.[32] Second, we have to distinguish these critical movements' conditions of emergence and early reincorporation from their subsequent trajectories. In more recent years, for instance, Marxist sociology has rearticulated itself through the lenses of post-structuralism, semiotics, narrative analysis, and Lacanian psychoanalysis and rediscovered the anti-positivist traditions of the Frankfurt school. Gender and queer studies, both influenced by Foucault, have emerged out of feminist sociology, and there has been some rapprochement among sociological feminism, post-structuralism, and psychoanalysis. Cultural sociology is moving beyond its earlier restrictive focus on so-called cultural objects, with some practioners returning to the hermeneutic tradition and others embracing the approaches of Bourdieu or Birmingham-style cultural studies, all of which were formerly found mainly in Europe and Britain. The new visual sociology (Woodiwiss 2001) breaks with the early frameworks emphasis on text, number, and diagram.

The Historical Turn in Sociology

The wave of historical sociological research that began in the 1970s had a handful of antecedents during the postwar period, including Bendix (1964), Lipset (1963), Moore (1966), Roth (1963), Smelser (1959b), Swanson (1960, 1967), Tilly (1964), and Wallerstein, ed. (1966). But historical sociology was not recognized as a distinct subfield in this period. The ASR carried almost no historical articles between 1945 and 1970 and historical sociologists such as Smelser never before the end of the war (in 1944 and February 1945). Only at the end of the 1970s did sociology start opening up to intellectual traditions and methodological strategies associated with the field of history. The importance of this opening should not be underestimated. Historians'

32. This includes, according to Burawoy (1989) and Sewell (1996b), Skocpol (1979), which manifestly adopted an "experimental" methodology. Burawoy argued that Skocpol's text in fact adopted a "conjunctural analysis in which political crises have different causes" despite her manifest adherence to the positivist "method of difference and agreement" (1989: 772).

approaches had been defined as antithetical to sociology since the late-nineteenth-century Methodenstreit. Even during the interwar period, few American sociologists were as historically oriented as European sociologists like Weber and Elias.[33]

Since there was only a handful of American sociologists who could be "recovered" for the historical opening in the 1970s and early 1980s, courses in historical sociology typically contained a large proportion of European "classics" and works by historians. One result of the lack of an indigenous tradition, paradoxically, was a greater willingness to read work written before or outside of American sociology's positivist culture. And these external sources provided crucial resources for unthinking positivism. Although Weber had insisted on what critical realists call the concept-dependency of social practice—the unavoidable hermeneutic dimension of social analysis—this had been lost or suppressed in the American translation of his work. Some of the discussions around culture, language, and practice that were beginning among historians in the 1970s and 1980s, for instance (e.g., Sewell 1980), seeped into sociology.

Historians offered sociologists important lessons about the historicity of conceptual categories. This was especially visible in work on class formation. Sociologists tried to make sense of E. P. Thompson's insistence that social class was not some simple material structure but rather something that "happens when some men, as a result of common experiences . . . feel or articulate the identity of their interests as between themselves, and as against other men" (1966: 9).

The opening to historiography also promised to make sociologists more aware of the horizontal openness of the social and hence of the ubiquity of conjunctural and contingent causality. Drawing a series of methodological lessons from this first period of historical sociology, the sociologist Philip Abrams (1982: ch. 9) recognized the special significance of the category of *contingency*. In a direct refutation of the distinction between a supposedly idiographic historiography and a more nomothetic sociology, Abrams (1982: 195–196; 1972, 1980) pointed out that the individual event was amenable to (sociological) explanation, even if this explanation involved a plurality of mechanisms operating in a contingent conjunction. As Abrams noted, "what is unique about an event is the conjunction of elements it embodies" (1982: 197). During the same period, Norbert Elias's (1978) proposal for a "figurational sociology" translated a central epistemological lesson from the

33. One of the major exceptions was Merton, who published historical essays on science and Puritanism during the second half of the 1930s (e.g., Merton 1936).

field of history into sociology by arguing for "figurations" rather than "invariant laws."[34]

Somewhat later, historical sociologists began to discuss narrative emplotment as a distinct and sometimes privileged means of presenting complex historical conjunctures and representing the braiding together of the meaningful and material aspects of social life.[35] From historians such as Louis Mink (1987), they learned about the unique cognitive and interpretive contributions of narrative form, understanding the importance of reading beneath the textual surface for the "content of the form" (Hayden White 1973, 1987) or the textual "unconscious" (Jameson 1981). This helped historical sociologists to *unlearn* the simplifying accounts of narrative that had been proffered in earlier decades by positivist philosophers—arguments that reduced complex narratives to a series of simple lawlike statements or probabilistic generalizations (e.g., Hempel 1966; Mandelbaum 1961; Nagel 1979 [1961]: ch. 15).

These lessons about the historicity of concepts, the concept-dependency of social practice, contingency, conjuncture, figurational analysis, and narrative ran up against American sociology's entrenched positivism. During the foundational postwar period, figures such as Lazarsfeld had insisted on the hoary distinction between history and social science and between idiographic and nomothetic knowledge. Even Parsons, despite his critique of positivism, empiricism, and scientism, insisted again and again on the distinction between "the historical and the analytical sciences" (1937, vol. 2: 759–760). Rather than learning from historiography, many postwar sociologists assumed that they had lessons to provide historians.[36] An entire lexicon of terms was elaborated to keep "idiographic" history at arm's length. Work that paid too much attention to the meaningful dimensions of social practice was referred to as "wishy-washy," "loosey-goosey," "fuzzy," "soft," "parasitical," "airy-fairy," and, mirabile dictu, "poetry." Writing that attended to sequence and conjuncture was often dismissed as "journalism" and "just-so stories."

How was the "second wave" of historical sociology related to broader de-

34. On conjuncture and contingency, see also Itzkowitz (1996), Sewell (1996b), and especially Althusser (1990, 1994) for the concepts of conjuncture and "aleatory materialism."

35. See the special issue of *Social Science History* (16:3, 1992) on narrative.

36. Compare the tutelary approach of Lipset (1968) or postwar sociologists' favorite philosopher, Hempel (1966), with the comments about the relationship between the disciplines by G. S. Jones (1976). Even Immanuel Wallerstein, who has contributed to current discussions of "unthinking" the limits of positivist social science, continues to use the concept of the "idiographic" when discussing historiography (1991: 252).

velopments in the discipline and society at large? Periodization is crucial here. Dissertations and books began to appear in numbers sufficient to speak of a "second wave" of historical sociology only at the end of the 1970s.[37] Some sociologists became *more* historical during the 1970s. *The Formation of National States in Western Europe* (C. Tilly, ed.), published in 1975, was not organized around the search for a uniform model of state formation but offered instead a series of sensitizing concepts. Wallerstein's *The Modern World-System* (1974) may have presented its arguments in a realist-positivist mode, but its complex historical narrative undermined any simple monocausal argumentative structure.[38] Indeed, the so-called waves of research in historical sociology have to be seen as referring as much to epistemic formations as to generations or individuals.[39] But a serious boom in historical sociology began only after a half-decade of socioeconomic crisis in the United States, a period that saw the unraveling of the Fordist mode of regulation and a postwar nadir of American imperialist projection overseas (Steinmetz 2003b). In this respect, and in contrast to the earlier neo-Marxist and feminist movements, historical sociology was faced with a mainstream episteme whose sociopolitical conditions of support were already beginning to crumble. The patterned Fordist regularities of time, subjectivity, and space, which had ratified sociologists' positivist worldview in earlier decades, were disappearing. Under these conditions, timeless sociological laws—statements of regular conjunctions of social events—seemed inherently less believable. With respect to more immediate supports for positivism, the state and private foundations also were scaling back their demands for social science research in the late 1970s and 1980s, in contrast to the full-bore welfare-warfare state clientelism that social scientists had experienced during the 1960s.[40]

As with the earlier wave of critical sociology in the 1960s, there was a

37. This involved people such as Aminzade (1981), Arjomand (1984), Block (1977), Bonnell (1983), Calhoun (1982), D. Clawson (1980), P. B. Evans (1979), Lachmann (1987), McAdam (1982), Sewell (1980), Skocpol (1979), Starr (1982), Traugott (1985), Trimberger (1978), and Zaret (1985). See Adams, Clemens, and Orloff (this volume) for the idea of a "second wave" of historical sociology.

38. Bendix (1978) is another example from this period of an opening to history, but Bendix had already been exceptional on this count, orienting himself more toward the Weberian tradition and historiography (compare Bendix 1960, 1964).

39. One of the most historical studies in this period, Schwendinger and Schwendinger (1974), was concerned with the discipline of sociology itself. Historical books in this period included Chirot (1976), Hechter (1975), Paige (1975), and Schwartz (1976). Paige's work is an interesting seismograph of change. His 1968 dissertation was concerned with the 1967 urban riots in Newark and Detroit. Paige's (1975) Sorokin Award–winning book, by contrast, was more historical and nonempiricist in its use of the concept of the mode of production; at the same time, however, it was mono-causalist and acultural. His most recent book (1997) is multi-causal and concerned with cultural meaning.

40. See the special issue of *American Sociologist* (17:4, November 1982) on financing sociological research.

structural parallel during the late 1970s and the 1980s between the historical turn in sociology and social movements in society at large. This was the era of a generation that had arrived "after the revolt" (R. Mohr 1992), too late for the optimistic upheaval of the 1960s. These social movements unfolded in a period of double-digit unemployment and academic retrenchment, behind the paradoxically historicist slogan, "no future." As Louis Menand remarked, "the economic value of a college degree began to fall" around 1975, and "the income differential between college graduates and high school graduates dropped from 61 percent to 48 percent" over the decade (2001a: 4; compare E. A. Duffy and Goldberg 1998; Lazerson 1998). This period also saw a shift from the more united politics of the old and new left, both of them committed to the idea of a "revolutionary subject," to the fragmented politics of the shifting coalitions, continually rearticulated identities, and nontotalizing "snake-like" forms of protest (Hardt and Negri 2000: 57–58; Laclau and Mouffe 1985).

These social and political developments were paralleled by a turn within sociology toward more conjunctural ways of thinking about social change. The decline of triumphalist social narratives of progress that resulted from the economic recession, the energy crisis, Watergate, the U.S. loss of the Vietnam War, and the splintering of the left was echoed intellectually not just by a post-modernist "suspicion of grand narratives" (Lyotard 1984 [1979]), but also by sociologists' turn toward history. The grand narrative of sociology's own unfolding as a cumulative science had become as unconvincing as the modernization-theoretic and Marxist meta-narratives of social development. The turn toward history thus represented an embrace of anti-teleological and ironic modes of thought—even if historical sociologists sometimes took detours through historical methods and meta-narratives that historians already considered outmoded. The turn to history also meant that sociologists were seeking rapprochement not with one of the "harder" sciences, as had been the case during each of the discipline's earlier phases, but with a more humanistic discipline. History had not enjoyed the largesse of the "post-historical" Fordist science-and-security state and had produced some of the harshest critics of sociological scientism during that period.

Despite social conditions conducive to the historical turn, however, a period of domestication set in quite quickly, as noted by Calhoun (1996). The epistemological disruptions coming from the new historical sociology were countered by efforts to make the subfield conform with existing conventions. This involved, first, forcing historical research back into the procrustean bed of the covering-law format. Methodological discussions within historical sociology were dominated for years by single-mechanism explanations or models in which a single interaction term was repeated

across contexts. Theda Skocpol influentially recommended that qualitative historical sociologists adopt John Stuart Mill's "Method of Difference and Agreement" and seek "invariant causal configurations" (Skocpol 1984a: 378; 1979; also Orloff and Skocpol 1984; Skocpol and Somers 1980). This promised to provide qualitative historical sociologists with a simulacrum of quantitative statistical design, although quantitative sociologists gleefully called the historical sociologists' bluff, insisting on the inferiority and logical flaws of these small-n comparisons in comparison to standard statistical models (Goldthorpe 1991; Lieberson 1991; Steinmetz 2004). It also meant that historical sociologists were rejecting notions of the openness of the social system and eschewing methods attuned to shifting conjunctural overdetermination. Compared to the method of difference and agreement, multivariate statistical methods were much better equipped for dealing with complex interaction terms; indeed, this was one reason some historical sociologists turned to quantitative methods. In doing so, however, they reinforced a tendency to relate to history as a source of data. They were also unable to deal with shifting conjunctures (but see Ragin 1987).

There was also a tendency to redefine historical sociology in empiricist and anti-realist terms. This was partly due to the adoption of statistical methods, which tended to collapse indicators into the mechanisms that they were supposed to measure—no matter how often statistical methodologists warned against this. Similarly, Skocpol's method of induction tended to "[reduce] causal processes to causal associations," as Burawoy (1989: 783) noted, pushing it away from a realism of mechanisms. Finally, historical sociologists tended to accept the mainstream refusal of "interpretivism," which they often associated with earlier essentializing and politically conservative cultural approaches. The adoption of statistical methods played a role here as well, since culture is intrinsically resistant to measurement.

Although domestication was largely self-imposed, historical sociologists were also facing a still powerful positivist mainstream. Research funding, publication in the main journals, and hiring and promotion decisions continued to be dominated by this approach. Indeed, some major sociology departments moved even further in this direction during the 1980s, as Neil Smelser (1986) pointed out. The initial disruption to sociological positivism sparked by the trembling foundations of Fordist normalcy during the 1970s seemed to have been contained by the early 1980s. And without equating the three waves of historical sociology with generations, we must not overlook the continuing domination of most sociology departments during the 1980s by the generation that had come of age intellectually during the 1950s and 1960s.

Despite these recuperations, however, historical sociology was not completely reconciled with methodological positivism. Some sociologists from the so-called second wave continued to relate in an unencumbered way to historicity and historians and to take seriously ideas such as conjuncture, context, narrative, persistence, and culture. Studies of working-class politics by historical sociologists in this period, for example, were often culturally or semiotically oriented (see Aminzade 1981; Calhoun 1982; Sewell 1980). Ronald Aminzade's 1981 book was organized around a cultural-Marxist theoretical framework of hegemony and was methodologically similar to the work of social historians; Calhoun (1982) and Sewell (1980) were organized around theoretical discussions in cultural anthropology and social history. A background outside the mainstream of American sociology may have been the differentia specifica in some of these cases.[41] The subfield of class formation was also protected from positivism to some extent by the fact that many influential books on the subject, even during the 1950s and 1960s, rejected aculturalism (compare Gouldner 1954; Thompson 1966; Willis 1977), in contrast to research on topics like state formation or social policy.

The Unsettled State of Historical Sociology Today

The cluster of social changes signaled by the concept of post-Fordism has tended to push sociologists' spontaneous social epistemologies away from the assumptions of positivism. The sheer accumulation of critical movements within sociology, furthermore, has helped to preserve the vision of a less positivist discipline. Yet entrenched disciplinary interests and habits continue to pull in the opposite direction. This straining in opposing directions produces multi-accentual and internally contradictory texts and unexpected epistemological hybrids.

A recent attempt to take stock of comparative-historical analysis by Mahoney and Rueschemeyer (2003) underscores both the persistence of meth-

41. Sewell's Ph.D. was in history rather than sociology, despite his strong positivist background (see Sewell's own comments on this in Sewell forthcoming), and Calhoun had earned an Oxford D.Phil. in social sciences, with an anthropologist and a historian as advisers (personal communication). At the same time, Sewell was a member of the Sociology Department at the University of Michigan between 1985 and 1990 (with an appointment in history as well); Calhoun was appointed in sociology departments at the University of North Carolina and New York University. Aminzade earned his Ph.D. in sociology at Michigan, with Charles Tilly as adviser. As noted above, Tilly was one of just a handful of American sociologists whose work became increasingly oriented toward historiography and historians during the 1970s.

odological positivism, and the changes in sensibility since the 1980s.[12] To illustrate the polycentrism and multi-accentuality of present-day American historical sociology one might also have selected the introduction to the present collection, but it is located at a greater distance from sociology's positivist center of gravity.

At the manifest level, Mahoney and Rueschemeyer's text is an attempt to channel the subfield back toward the predilections of the "second wave." Yet a closer examination of the text reveals a certain level of epistemic multivocality in which the manifest positivist aspects are combined with latent non-positivist ones. This epistemological dualism reflects the increasing presence of non-positivist alternatives in other subfields of sociology, as well as the collapse of the environing social regularities associated with Fordism and the emergence of post-Fordism. Despite Calhoun's pessimistic diagnosis, then, it appears that the project of domestication has not been entirely successful, even among the would-be domesticators. Although the recuperative processes identified by Calhoun are obviously still operating, the dialogism of Mahoney and Rueschemeyer's text is indicative of an unsettled field—unsettled in the sense of not being united around shared recognition of a particular definition of scientific capital or "distinguished" types of research.

Skocpol's (1984a) concluding essay in *Vision and Method in Historical Sociology* serves as the template for Mahoney and Rueschemeyer's article, making it instructive to map the continuities and deviations from the earlier text to the present one. Mahoney and Rueschemeyer follow Skocpol explicitly in singling out three features of historical sociology for praise: *attention to temporal processes*, *causal explanation*, and *comparison*.

Temporality/Historicity

The first facet of Mahoney and Rueschemeyer's definition of "comparative-historical analysis" is an *emphasis on processes over time* (2003: 8). This seems, on the one hand, to reveal a difference from the static, pseudo-

12. I will treat Mahoney and Rueschemeyer's text with respect to historical sociology, ignoring the fact that part of their agenda is to conjure up an object intermediate between sociology and political science that they call comparative historical analysis. This seems fair since both authors are sociologists. I should also note that an early reviewer of this article asked me to discuss Mahoney and Rueschemeyer's chapter, while a subsequent reviewer wondered why I would devote so much space to it. Hopefully this treatment falls somewhere in between their expectations. My sense is that Mahoney and Rueschemeyer's piece does exemplify the way in which the multi-accentuality of epistemological discussions in contemporary sociology can undercut arguments that attempt to hew a more unified positivist line.

experimental model of historical sociology advocated earlier. Yet the authors draw back from acknowledging the full epistemological consequences of a genuinely historical approach to temporality. They seem to feel compelled, for example, to avoid established historiographic terms for temporal processes, preferring scientistic alternatives such as "path dependence." Historians might well ask why they should replace their own rich lexicon, which includes notions such as persistence, continuity, memory, process, and the return of the repressed—not to mention historicity and "history" itself—with this rather rebarbative phrase. More to the point, "path dependence" makes sense as a distinct concept only if one retains some background belief in a "normal" path of development from which deviations may be plotted. But one of the fundamental arguments derived from critical realism—and a commonplace among most historians—is the untenability of any notion of "normal" and "deviant" development paths (see Blackbourn and Eley 1985; Steinmetz 1997a). Historical events are *always* produced by contingent conjunctures of causal mechanisms. Earlier conjunctures influence the intensity or particular value of any given mechanism in the present; they also co-determine whether a particular mechanism will be suppressed or expressed. What this means is that *all* events are partly shaped by earlier conjunctures, via historical "paths." Path dependency is thus a synonym for historical change *tout court*. It is only necessary as a separate term if one retains some belief in general social laws. As a figure of thought, path dependency has the structure of a fetish (Freud 1963 [1927]), with the subject's anxiety in this case being fixated on proximity to the humanistic, less scientific field of history. Like a fetish, the idea of path dependency disavows the sociologist's identity with the historian while at the same time covertly acknowledging it.

Winking and Blinking: Erklärendes Verstehen (explanatory interpretation) and Verstehendes Erklären (interpretive explanation)

Mahoney and Rueschemeyer's discussion of *theory and explanation* reveals a similarly uneasy mingling of positivism and non-positivism. On the non-positivist side of the ledger, they decry the "poverty of universalizing theoretical approaches," which are said to generate "ahistorical concepts and propositions that are often too general to be usefully applied in explanation" (2003: 7). Against general (that is, reductionist) theory, they advocate a pluralistic approach to explanation, as well as a "dialogue between theory and history" in many iterations of analysis and a combination of "induction and deduction" as concepts are formed and refined in light of evidence (ibid.: 20). Although they do not spell out what they mean by either "the-

develops his famous example of the rapid contraction of an eyelid, a gesture that is completely illegible at the purely behavioral level—is it a wink, a twitch, a parody of someone else's wink or twitch, a rehearsal of such a parody, a fake winking or blinking, or what? If empirical description is impossible without interpretation, the same must be true, ipso facto, of explanation. Explanation and interpretation are complementary dimensions of all social analysis.

The impetus to polarize interpretation and explanation, like the contrary insistence that the two are inextricably linked, reaches back much farther into sociology's history than any recent confrontation between science and post-modernism, to the debates among Dilthey, Windelband, and other participants in the nineteenth-century German methods dispute and also to the "positivist dispute" of the 1960s (Adorno et al. 1976). Sociologists are routinely taught about Weber's struggle to rearticulate the sundered strands of interpretive (*verstehende*) hermeneutics and the "causal-explanatory" approach that he associated with "rationalistic interpretations" (1978, vol. 1: 7). Weber's alternative to this binarism was "explanatory understanding" (*erklärendes Verstehen*). This formula actually sounds quite similar to Geertz's "interpretive science." We should not retreat backward epistemologically to positions that flourished before Weber but try to advance beyond his still unsatisfactory solutions.[43]

Once we turn our attention to the present-day context, we can start to reconstruct the animus motivating Mahoney and Rueschemeyer to divide the Red Sea of interpretation and explanation. The authors equate interpretation with the "cultural or linguistic turn," and in a jarring string of epithets, they call cultural analysis anti-scientific, post-modern, nonrigorous, unsystematic, speculative, "willfully selective" with regard to evidence, and even lazy.[44] The authors warn ominously of the "danger" to "young researchers" of "not taking sides" against the cultural-interpretivist temptation (Mahoney and Rueschemeyer 2003: 22–23). Most of these charges, reminiscent of popular media reporting on the "science wars" in the 1990s and of many other moral campaigns, are themselves too "willful" to warrant

43. Weber's famous methodological solution of the *ideal type* differs from a critical realist position, for example, insofar as it does not seek first to resolve a complex event into its components through abstraction (A. Collier 1994: 163–164; A. Sayer 1992), proceeding from there, via the process of retroduction, to descriptions of regular relations between these events and some underlying, postulated mechanisms (Bhaskar 1986: 68). The telos of concept formation for critical realism might be the *real type* rather than the ideal type, even if it is acknowledged that knowledge is necessarily provisional and theory choice inevitably shaped by social factors.

44. Interestingly, rational choice theory is associated with "ambition" (Mahoney and Rueschemeyer 2003: 7) rather than "case," even if it is rejected on substantive grounds.

much of a response. But some of them are helpfully diagnostic. Mahoney and Rueschemeyer trace the interpretivists' "willful selectivity" in the use of evidence to a "preoccupation" with the "critique of power" (p. 23). Here we can detect in a highly reduced form that old chestnut, "value-free" social science. In the general context of vehement attacks on "anti-science post-modernism," such a taunt automatically dredges up the entire polemic against fields "associated with a social movement," in which "a 'politically correct' agenda of research" arises because "the criterion of how best to advance knowledge has been replaced by the criterion of how best to advance the cause of the movement" (Coleman 1992: 8).

Mahoney and Rueschemeyer also misleadingly elide "post-modernism" with post-structuralism and equate the latter with judgmental relativism. While post-structuralism does reject the possibility of choosing rationally among contending interpretations, post-modernism is best construed not as an epistemological position but as the name of a historical epoch or cultural and subjective condition. Various analysts have traced post-modernity causally to "realist" mechanisms such as late capitalism, post-Fordism, or time-space compression; post-structuralism is itself sometimes included as part of this overarching post-modern condition that needs explaining (Harvey 1989; Jameson 1984, 1991; also Hardt and Negri 2000).

The most peculiar aspect of Mahoney and Rueschemeyer's critique, however, is the contrast between the strangled rhetoric of their discussion of the cultural turn and the measured tone they adopt when discussing the two other alternatives to comparative-historical analysis. In a striking concession to rational choice theory, for instance, they write that their differences do "not revolve around the assumption that actors are rational" (2003: 20–21). This throwaway comment completely overlooks the legions of sociologists and political scientists who have problematized this exact assumption. Subtle critiques of rational choice theory have been published by one of the founders of the *Journal of Historical Sociology* (D. Sayer 1987), by a recent chairperson of the comparative-historical section of the American Sociological Association (Somers 1998), and by one of the editors of this volume (Adams 1999). Historical sociologists since Elias (1994 [1939]) have analyzed the social operations of forms of subjectivity other than formal rationality, whether unconscious, affective, or habitual (Adams 1994, forthcoming; Berezin 1999; Bourdieu 1977; Steinmetz 2002, 2003a).

Apples and Oranges: Toward a Non-Cannibalistic Comparativism

The third criterion Mahoney and Rueschemeyer use to identify comparative-historical analysis is "contextualized comparison [,] typically limited to a

small number of cases" (2003: 14)—a concept they draw from Richard Locke and Kathleen Thelen (1995). This format is juxtaposed with two other types: "statistical studies of large numbers of countries" and so-called *idiographic* studies (Mahoney and Rueschemeyer 2003: 13, 17). Their critique of the first studies (Mahoney and Rueschemeyer 2003: 13, 17). Their critique of the first approach is well taken; the problem lies at the other delimiting boundary. Why is "comparison" a necessary component of any and all "analytic" historical research? It is critical to their project to define an area called *comparative-historical analysis* and to mark a distance from merely *historical* sociology.

The key term here is *comparison*. Comparison would seem to be logically indispensable to assessing the plausibility of theory for critical realism (see Lawson 1997; Steinmetz 2004). Investigation of a single case provides less assurance that the causal mechanisms of interest actually exist. So what is wrong with Mahoney and Rueschemeyer's argument?

On the one hand, I am struck by the bad faith of sociologists who continue to construct a lineage of comparative macro-history purged of historians' contributions. Mahoney and Rueschemeyer do not include a single book or article by a historian in their category of comparative-historical analysis. It is as if our discipline believed that Max Weber invented history and that it did not reappear until sociologists became interested again in the 1960s. In fact, comparative-historical social science originated in history, and most of the classics are by historians. One has only to think of Ranke's universal history and his works on French, German, English, Spanish, Roman, and Ottoman history. One immediately thinks of classics by Perry Anderson, Marc Bloch, Fernand Braudel, Robert Brenner, Otto Hintze, Eric Hobs-bawm, and Ernst Kantorowicz, and of more recent comparative work by Geoff Eley, Michael Geyer, Jürgen Kocka, Charles Maier, and Jürgen Os-terhammel. The longest lived journal of comparative-historical social stud-ies, *Comparative Studies in Society and History* (1958–present), has been edited by historians.

The bigger problem, however, lies with Mahoney and Rueschemeyer's attempt to restrict properly "scientific" macro-social inquiry to the com-parative type. Against this, we should recognize that the ontological and epistemological characteristics of social life make the case study the pre-condition for any comparative assessment of theory. Within an open system like the social, and in contrast to artificially closed systems like the scientific experiment, empirical events are inevitably multiply overdetermined by a plurality of conjuncturally interacting causal mechanisms. Because genuine experiments cannot be conducted in the human sciences, one has no other choice than to investigate the expression of underlying causal mechanisms within complex constellations in concert with other mechanisms. Moreover,

the mechanisms making up the formative constellation will vary from case to case. Causal mechanisms in social sciences usually exist in hybrid rather than a simple, uncombined form (Bhaskar 1986: 111–113). An empirical contrast may reflect two differing combinations of the same set of mechanisms, rather than the presence or absence of a single mechanism. Due to *counter-phenomenality*, an empirical contrast may also result, not from the variable presence of a single mechanism, but from its varying ability to be expressed at the level of the actual. Both of these considerations undermine the notion that a comparison of two or more cases is necessarily more conclusive than a case study.

Social events are unique or unrepeatable in the sense that they cannot be explained in terms of a universal law, even if there are partial regularities at the level of events (Lawson 1997). The usual deployment of the terms "idiographic" and "nomothetic" is thus an incoherent intellectual survival—or if we must retain these terms, then we have to admit that social explanation is necessarily idiographic. One can gain access to causal mechanisms only through the study of unique events, specific individuals, etc. Since the basic object of an explanation is the individual event or case, it makes no sense to set case studies against explanation. Indeed, excluding case studies from historical sociology would be equivalent to excluding explanation from historical sociology.

Social knowledge necessarily involves movement between case studies and theory; and comparisons among case studies. In Peirce's terms, discovering any regularity requires movement from induction to retroduction to "deductions from retroductive suggestions" (Peirce 1931–1932, vol. 2: 491). *Explanation* concerns the unique phenomenon, *theory* concerns underlying mechanisms, and the *evaluation of theory* requires an accumulation of cases—that is, comparison. Comparison may therefore be "indispensable" given an "analytic interest in causal explanation," as Mahoney and Rueschemeyer claim, but it cannot be *identified* with explanation. Indeed, there is no philosophical program, *not even positivism*, in which the two are equated. For Popper, to "give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, held together with certain singular statements" (1992 [1934]: 59; emphasis in original). A single event, in other words, *can* be explained. Similarly, Nagel insisted that "explanations may be offered for individual occurrences" (1979 [1961]: 15) and developed an example involving an individual historical event (pp. 552 passim).

It is also unclear exactly what sort of comparative method Mahoney and Rueschemeyer endorse. They recommend "contextualized comparison,"

which is summarized as a method of exploring "how variables may have different causal effects across heterogeneous contexts" (2003: 13). This procedure would seem to be compatible with critical realism, as long as there is no assumption that causal mechanisms are empirical or that explanation is concerned with constant conjunctions of events involving clusters of causal factors. Yet the authors also endorse "macro-causal analysis," which suggests an understanding of explanation as involving constant relations of dependence between clusters of determining causal factors and specific outcomes. For critical realism, by contrast, comparison might involve a group of empirical phenomena having little in common at the empirical level other than the hypothetical impact of a given underlying causal mechanism. The goal of explanation would be to investigate the vicissitudes of the underlying causal mechanism in differing empirical contexts (Steinmetz 2004).

I am not making the statisticians' criticism of selection on the dependent variable. There is nothing wrong with organizing comparisons around phenomena that are similar at the empirical level; this is an alternative to comparing phenomena that have little in common at the empirical level other than the putative effect of some causal mechanism of interest. Indeed, many historians would prefer comparisons among entities that are historically connected to one another in this way (Bloch 1953; Grew 1980; Sewell 1967). There may also be ethical or political reasons for organizing a comparison among phenomenally similar events or objects.

Others have rejected the idea of comparison from the standpoint of incommensurability (see Keane forthcoming; Lambek 1991; Nancy 2000; Povinelli 2001; Yengoyan forthcoming). These arguments cannot be dismissed as the ravings of a "camp" of politically driven, unsystematic, anti-science postmodernists—especially since this argument can be traced back historically at least as far as Kant's theory of the "sublime" and German Romanticism. Due to the concept-dependence of human action, comparison necessarily involves problems of translation into a meta-language. Translation may repress the incommensurable aspects of the translated culture or force differing cases into categories determined by the more powerful. Conceptual categories used in comparisons with the non-West often treat parochial or geohistorically limited categories as if they were universal (Chakrabarty 2000).

It is obvious that "difficulties of translation" will arise between theorists of incommensurability and positivistically inclined social science. Like positivists, critical realists are willing to deploy theoretical categories and languages to designate mechanisms that may not be consciously perceived by the actors in question. But critical realism does not require that mechanisms be universal to all societies. Nor does it insist that the empirical level of events

be redescribable in a theoretical meta-language. By contrast, positivism and so-called macro-causal analysis seem willing to redescribe disparate empirical events using universal conceptual categories. For critical realism, abstract conceptual language would be deployed when discussing putative causal mechanisms, but not necessarily with respect to the empirical level of events.[45] This breaks with the "cannibalistic" tradition of positivist comparison in which the concrete historical individual is disaggregated into a heap of variables and devoured.

L'Objet Petit n: Big N's and Bigger Cases

If sociology as a field suffers from a kind of "economics envy," historical sociology has often displayed signs of an inferiority complex vis-à-vis the more positivistic sectors of the discipline. The domination of the field by methodological positivism in earlier decades accounts in large part for the self-recuperative tendency of the so-called second wave. There is also more than a hint of this identification with the aggressor in Mahoney and Rueschemeyer's article. At one point they seem to concede that historical sociologists'—their own—work entails "some reduction of ambition" (2003: 7). Elsewhere, they describe comparative-historical sociology as a "bargain," making it sound like the Wal-Mart of social science (p. 11). But a historical explanation that weaves together a large number of causal factors, attempting to place the proper weight on each of them and to trace their intricate imbrications and overdeterminations—like Rueschemeyer's own study with Stephens and Stephens (1992) of the relations between capitalist development and democracy—is every bit as ambitious as the most universal theory. Mahoney and Rueschemeyer both are certainly aware that such research is no "bargain" for those who undertake it.

The most striking indication of a sense of historical sociology's dominated status is the authors' clamorous identification with "big" and "important" questions. Comparative-historical analysis is defined by its focus on "large-scale outcomes" (Mahoney and Rueschemeyer 2003: 5). Although there have certainly been good political reasons in the past for historical sociologists to emphasize topics such as revolution or the rise of capitalism, the continuing insistence on a narrowly delimited set of topics is a dead

45. This does not contradict the above discussion of Geertz and concept-dependency. The translation of an eyelid contraction into a wink, for instance, is a translation into a sign that is itself also culturally specific. On a different, more abstract level, winking may also be explained in terms of theories of communication, twitching in terms of psychological theories, etc.

weight on the shoulders of current researchers. Charles Tilly's *Big Structures, Large Processes, Huge Comparisons* opens with the sentence, "We bear the nineteenth century like an incubus" (1984: 1). The incubus pressing down upon us today has a more recent origin in the second wave of historical sociology, which was overwhelmingly informed by Marxism or by a Weberianism that was itself a reaction-formation to Marxism.

The insistent mobilization of adjectives such as "big" and "important" also suggests the competitive pressures of a discipline that is still perceived as dominated by quantitative sociologists with "big N's." This seems to be an almost masochistic sort of "symbolic identification" (Lacan 1991: 134; Laplanche and Ponatalis 1973 [1967]: 144), an identification with an *external location* from which historical sociologists think they are being observed. We should instead be seeking a more pleasurable "imaginary identification" with an image of " 'what we would like to be' " (Žižek 1989: 105). What many of us would like to be is students of a small number of cases. This could be called historical sociology *à la recherche de l'objet petit a*. The quantitative connotations of "big" in this discussion cannot be dissociated from the socio-historical context of Fordist big science, the era in which methodological positivism came to the fore. Both the wielders of the "big N" and their counterparts, with their equally "big" and "macroscopic" cases (Skocpol 2003), seem somehow fixated on the gigantism of this now bygone Fordist era. This gigantesque sociology seems more at home in the Russian Magnitogorsk or in Komsomolsk-on-Amur in the Russian Far East, or in Ford's River Rouge, than in the post-Fordist twenty-first century.

Delimiting "big" topics in a commonsensical way seems especially inappropriate within a branch of sociology that defines itself as historical. One need only compare the topics that were considered important in American sociology four decades ago to topics that are currently big, such as culture, nationalism, space, post-colonialism, sexuality, human rights, religion, and globalization; the current themes were nearly invisible in American sociology during the middle decades of the cold war. While adjectives like "big" and "important" serve to mark off a distance from the merely fashionable, the selection of themes in historical sociology has always been presentist—it is no surprise, for instance, that historical studies of revolution became popular during a period of worldwide revolution.

Something has changed between 1984, when Skocpol's *Vision and Method* appeared, and the present. The newly destabilized conditions seem to be compelling some social scientists to insist more dogmatically on the positivist position, given that it is no longer doxic. Indeed, their version is not even an *orthodox* one, but rather a new mixture of elements. The text's multivocality is suggestive of the epistemologically unsettled character of the field. The prominence of methodological positivism within this hybrid mix, however, underscores that position's continuing power in the discipline as a whole.[46]

Conclusion: Post-Positivism Eternally Deferred?

In this chapter I have emphasized the conformity of Fordism with the methodologically positivist social worldview and the conformity of post Fordism with certain aspects of the historical, cultural, critical, and epistemological turns in sociology. The non-positivist elements in these intellectual movements resonate in particular ways with the flexibilized and information-centered society of the post-Fordist "new economy" and might therefore be expected to have some staying power. Positivism would also seem to be weakened by the decline in state funding for research, the waning of postwar fears of totalitarianism, and the collapse of Fordist regularities in sociocultural life. One possible prediction, then, would be a continuing weakening of positivism.

The analysis of modes of regulation is not just another name for social control or disciplining, however, but is embedded within an overarching analysis of capitalism. And while regulation theory has jettisoned the earlier Marxist functionalism and teleology that seemed to guarantee that capitalism would resolve its crises in a quasi-automatic fashion, in favor of a "Marxism without guarantees" (S. Hall 1983), it is actually more focused than earlier Marxisms on the notions of contradiction and crisis. The point is that while post-Fordism may be more improvisational, flexible, self-reflexive, deterritorializing, and culture-centered—tendencies that seem to resonate with postpositivist social epistemologies—it is no less oriented than earlier modes of regulation toward increasing the rate of extraction of surplus value. Although the post-Fordist strategies of deregulating and flexibilizing production and markets and recommodifying labor differ from earlier Fordist strategies of shoring up accumulation, these aspects of post-Fordism could be seen as resonating in different ways with positivist forms of social knowledge. Economic flexibilization has involved the imposition of uniform neo-liberal economic models to all places and situations. These projects depend upon a decontextualized economic model of human sub-

46. Mahoney and Rueschemeyer's (2003) text contrasts interestingly with similar programmatic statements in political science, such as G. King, Keohane, and Verba (1994), which makes fewer concessions to non-positivism (see Mihic, Engelmann, and Wingrove forthcoming; Wedeen forthcoming).

jectivity and behavior, even if it is a model of "rational man" rather than the "behaviorist man" of the Fordist era. Post-Fordist states continue to favor positivistically packaged social science, as do private foundations and social movements, even as the overarching social structures of this new mode of regulation render methodological positivism less and less plausible.[47]

And while this paper has emphasized the overall external social context, especially the mode of regulation, as a determinant of sociologists' spontaneous social epistemologies, one cannot overlook the abiding and profound implantation of methodological positivism within disciplinary institutions, patterns of socialization, and scientific habituses. Sociology is relatively autonomous from external pressures and events, like any other field. One can expect a lag, or *non-simultaneity*, between the unraveling of Fordism and the disruption of methodological positivism within sociology due to the *hysteresis of the scientific habitus* (to paraphrase Bourdieu)—that is, the fact that intellectual paradigms are not simply intangible discourses but are instead embodied practices.

Another scenario, then, would see positivism changing its form without disappearing. The increased interest in rational choice theory in political science and parts of sociology, for instance, places the active social agent at the center of social analysis, corresponding to the felt increase in demands on the individual to be self-promoting and self-reflexive. At the same time, rational choice analysis promotes a universal model of subjectivity and retains an orientation toward the prediction of social action. The shift from a behaviorist black-box image of subjectivity to a universalizing view of the subject as agent resonates with the changed structures of plausibility in post-Fordism.

Given my acknowledgment of the contradictory epistemic signals sent by post-Fordism and my more general critique of the notion of invariant patterns of social behavior, it would be foolish and paradoxical to predict which direction (historical) sociology will take. But one plausible scenario, at least, includes an increased space for non-positivist alternatives. Gouldner's (1970) prediction three decades ago that American sociology would move

47. Even in the current period, the state (considered at all of its scalar levels) is still the superordinate coordinator for regulatory initiatives, and it continues to organize most of the familiar legal, infrastructural, and welfare-statist interventions. During the 1990s, when the post-Fordist regime seemed to attain a certain level of consolidation, state funding for science began again to increase but with new emphases. Where the National Institutes of Health, National Institute of Mental Health, and the National Science Foundation were previously at the core of government funding for social science, the life sciences now became more central. The social sciences are again being encouraged to repackage themselves along the lines of the natural sciences; witness the recently renewed interest in "sociobiology" and genetic explanations of behavior.

through a period of crisis characterized by the collapse of theoretical hegemony to a less positivist and more reflexive polycentrism might turn out to have been correct, if premature (see also J. C. Alexander 1982). Historical sociology, along with critical sociology, cultural studies, and the sociology of science, would have contributed in no small part to such a future.