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THE POLITICS OF METHOD IN THE HUMAN SCIENCES

Positivism and Its Epistemological Others

GEORGE STEINMETZ, EDITOR



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INTRODUCTION

Positivism and Its Others in the Social Sciences

GEORGE STEINMETZ

This collection explores the vicissitudes of positivism and its epistemological others in the contemporary human sciences. The volume's overarching goal is to provide a mapping of the contemporary human sciences from the standpoint of their explicit and especially their *implicit* epistemologies, asking about the differences and similarities among and within these disciplines' epistemological cultures. Taken together, the essays provide a portrait of epistemology and methodology (writ large) in the contemporary social sciences.[1] Given the contemporary conjuncture of epistemological crises and conflicts in the human sciences and the proliferation of nonpositivist alternatives, the present is an ideal moment for taking stock of the underlying assumptions of the human sciences. Only by making the epistemological stakes and disputes explicit will it become possible to heed the call to "open the social sciences" (Wallerstein et al. 1996).

This book also offers the rudiments of a comparative historical narrative of these disciplinary developments since the beginning of the twentieth century, with an emphasis on the period beginning with World War II. Recent writing has pointed not just to the present-day conjuncture of epistemological uncertainty but also to the middle decades of the twentieth century as critical moments in the transformation of the social sciences' deep culture. The varying epistemological effects and textures of these midcentury and contemporary changes, and the connections between the two periods, have yet to be interpreted.[2]

While providing an opportunity to compare disciplinary developments, this volume is also concerned with the interconnections among disciplines, their mutually imbricated developments. Many of the essays ex-

plore patterns of imitation and repulsion, the introjection and rejection of theories and practices from outside disciplines, the dynamics of self-definition and redrawing of disciplinary boundaries via constructions of disciplinary Others. Thus, while economics suffered from a sort of "physics envy" (Mirowski 1989), sociology suffered from a parallel "economics envy" (Somers, this volume). The political science subfield of *political theory* defined itself against its own "bad others"—positivism and sociology—during the 1950s (Hauptmann, this volume), while mainstream political science defined itself initially against classical political theory and increasingly in terms of rational choice and game theoretic models imported from economics. Psychoanalysis has worked the fraught boundary between the human sciences and their biomedical outside (Elliott, this volume). Many social historians have shifted from an admiring emulation of sociology during the 1960s and 1970s to theoretical approaches drawn from cultural anthropology, linguistics, and literary criticism (Sewell, this volume). In addition to these sociological and historical questions about the relations among the disciplines, this volume asks whether this overall configuration of disciplines makes sense. Is there an inherent social-ontological logic to this peculiar array of disciplines? Or, as some would argue, is our inherited disciplinary landscape merely the historical residue of colonialism and imperialism, long forgotten intellectual fashions, or the ephemeral priorities of states and corporations?

The other overarching aim of this book is to survey the landscape of alternatives to positivism in the human sciences; the entire second half is given over to this task. An earlier working title for this volume included the phrase "genealogies of positivism and postpositivism." From a certain perspective, postpositivism is little more than a description of the historical supersession of logical positivism and empiricism in the history of philosophy (H. Putnam 1990b). Yet, as some commentators and contributors to this collection observe, the term postpositivism is sometimes overly self-congratulatory and teleological. Indeed, the adjective postpositivist might be described as itself positivist, insofar as it suggests an image of scientific history as following a linear course in which positivism eventually becomes historically obsolete. The sheer fact that positivism was already declared anachronistic in the 1900s and 1930s (Lenin 1908/1927; Parsons 1937/1949) and again in the 1960s and 1970s (Adorno 1969/1976; Gouldner 1970; Giddens 1975, 1–22) cautions us against any finalist illusions in this realm. Many of the contributors to this volume track positivism's uncanny persistence in the human sciences up to the present moment. As Mirowski (this volume) reminds us, revised versions of positivism are alive and well

even in the philosophy of science. Harding (this volume), a founder of standpoint epistemology, cautions against a wholesale rejection of the positivist philosophical legacy. Hence my recourse to the more neutral language of "nonpositivism."

These considerations also steer us away from attempting to canonize any particular nonpositivist theory of knowledge. The word postpositivism indicates a greater degree of unanimity among the alternatives than actually exists in the world, or in this volume. Indeed, some antipositivists endorse a pluralization of knowledge cultures (as suggested by Bourdieu's "Vive la Crise!", 1988–89) rather than reconsolidation around a new orthodoxy. The only epistemological stance shared by all of the contributors to the present volume seems to be a desire to move beyond existing forms of social science positivism. The theoretical and methodological positions represented and performed here are diverse, encompassing standpoint theory, narrative analysis, critical realism, the ethnography and historical sociology of scientific fields and actor networks, critical or postmodern psychoanalytic theory, neo- and post-Marxism, poststructuralism, and a rejection of the fact/value dichotomy. The differences among the contributors' epistemic programs are in some respects as great as their shared distance from philosophical positivism.[3] The presentation here of partially incompatible alternatives differentiates this collection from previous volumes on postpositivism, which have tended to focus on one or another philosophical program (e.g., Archer et al. 1998; Lopez and Potter 2001; Pickering 1992; Law and Hassard 1999). This pragmatic approach makes sense if we accept the hypothesis that a revised positivism is alive and well in many of the social sciences.[4]

A Kaleidoscope of Disciplines and Their Epistemological Cultures

Nature is organized by simple universal laws of physics to which all other laws and principles can eventually be reduced.—Edward O. Wilson, *Consilience: The Unity of Knowledge*
Social science cannot ignore philosophical assumptions, since they will help to govern the focus of its attention.—Roger Trigg, *Understanding Social Science*

One of the guiding threads in this volume concerns the surprising longevity of positivism—especially in latent, unexamined, or unconscious forms—in the human sciences. Despite repeated attempts by social theorists and researchers to drive a stake through the heart of the vampire, the disciplines continue to experience a positivistic haunting. There is also a great deal of variation across the disciplines and historical epochs in the forms

of positivism and in the waxing and waning of positivist dominance (Knorr-Cetina 1991). A central goal of this collection is thus to allow the reader to explore the intimacies between positivism and the disciplines in different times and places and to facilitate other sorts of comparisons, including contrasts that track the migration and indigenization of concepts across social scientific space.

The essays in the first part of this book examine some of the main disciplines in the human sciences. The historical emphasis here is on the twentieth century, especially the period beginning with World War II; the geographical focus is on the United States due to that country's dominant position in the production and politics of social science knowledge.[5] The exceptions to this U.S. focus are mainly in the second part of the book, which is focused on *alternatives to positivism*. Some of these resources, not surprisingly, are generated outside the U.S. core, just as logical positivism originated not in the United States but in Europe. The division between more historical accounts of disciplines in part 1 and nonpositivist alternatives in part 2 does not mean that the first avoids discussion of earlier nonpositivist alternatives. For example, Keane analyzes the entire field of cultural anthropology as resistant to positivism throughout the twentieth century, while Mirowski discusses the philosophy of science of John Dewey. For the most part, however, the first part emphasizes not only historical accounts of the U.S. social sciences but also careers of positivist dominance in these disciplines.

Disciplinary Histories

Anthropology has shown the strongest divergence from modern epistemological versions of positivism. *History* is a discipline that has been deeply influenced by two versions of positivism, one oriented toward the search for general laws and the other emphasizing what historians call a positivist approach to source material. But the discipline of history is also widely described as having moved into a state of epistemic and methodological pluralism in more recent years. *Psychoanalysis* contained both scientistic-positivist and radically antipositivist potentials from the outset, as Anthony Elliott argues here. Recent theoretical developments in psychoanalysis have drawn out the nonpositivist elements, but the seemingly inexorable medicalization of psychological treatment and the ever increasing control of psychic health care by the pharmaceutical industry lends power to the scientistic tendencies. Diagnosing *economics* with respect to issues of epistemology turns out to be an extremely complex problem.

Economics is described by the contributors to this volume variously as dominated by epistemological positivism and empiricism, by a "depth realist"[6] (or "theoretically realist") epistemology, or by an antiempiricist idealism; alternatively, economists are said to combine realist and idealist approaches willfully and eclectically. By contrast, most writers seem to agree that U.S. *sociology* was captured by scientistic positivism during the postwar decades, even if different explanations have been offered for this disciplinary transformation. There is less agreement about the contemporary epistemological structure of the sociological field: some anticipate a breakup of positivist dominance; others see a shift in the *form* of positivism but not its prevalence. Most analysts of *political science* agree that positivism has been hegemonic throughout the postwar period, outside the political theory subfield, but the prevailing version of positivism has shifted over time. This collection also considers *area studies*, a set of interdisciplinary fields that occupied a central place in postwar U.S. research universities and that was connected in multifarious ways to the social sciences. The funding structures and policy aims of postwar area studies favored positivist approaches, as did the infrastructure of language training and the insistence on learning portable comparative lessons. At the same time, the inclusion of the language- and culture-oriented humanities in the overall mix of area studies introduced a potential for epistemic dissonance, as did the intrinsic focus of each speciality on a single sociocultural area (e.g., South Asia or Latin America). The final field examined here is the *philosophy of science* itself. As in many of the human sciences, the zenith of positivist dominance in the philosophy of science was during the two decades after 1945, even if most of its component building blocks were created much earlier. The philosophy of science is more than a metareflection on the sciences; it was also shaped by those sciences and by the broader sociopolitical environment.

Anthropology as an exception

The situation in cultural anthropology since the 1960s is often described in terms of a fragmentation into myriad different nonpositivist and "anti-science" positions.[7] In his contribution to this volume, Webb Keane does not reject this diagnosis of the present but questions whether this is best understood only as a post-1960s phenomenon. Keane argues that there has been a less visible but pervasive adherence among American cultural anthropologists to a version of nonpositivism since the days of Boas. He identifies a widely shared commitment to a quasi-Romantic syndrome that he calls the "epistemology of intimacy," a position centered on the

assumption of the incomparable singularity of cultures and the methodological necessity for self-interpretation as well as interpretation of the observed other. This epistemology also adheres to a belief that human agency is generally capable of defying "structural" constraints. U.S. cultural anthropology's common culture or philosophical center of gravity during the twentieth century stood in direct tension with the generalizing epistemology of positivism.

As George Stocking has argued, Boas himself was torn between a generalizing "scientific" approach to anthropology and a particularizing, historicizing stance that insisted that "in ethnology all is individuality" (Boas 1887b, 589). In addition to his natural science orientation, Boas was deeply influenced by neo-Kantian idealism and Wilhelm Dilthey's neo-Herderian hermeneutic historicism (Stocking 2001b, 37), arguing for a historical approach to geography and anthropology that aimed at "thorough *understanding*" (Boas 1887a, 138, emphasis added).[8] Boas was an adamant cultural relativist who held that "our ideas and conceptions are true only as far as our civilization goes" (1887b, 589). He insisted that the meaning of a given sign or cultural artifact varied according to context and actors' interpretations. And while he distinguished between physical science and cosmography, Boas applied the label "science" to both approaches. He also bequeathed the four-field arrangement to anthropology, which reserved a place for physical anthropology and archaeology alongside the cultural and linguistic branches. Boas was by no means a lone wolf; he and his students and allies controlled the American Anthropological Association. He suffered a "momentary setback" during and after World War I, when he was "censured and removed from office" in the Association for his antiwar activism, "pressured into resigning from the National Research Council" (created in 1916), and marginalized from the Galton Society, which was created in 1918 (Stocking 1968, 273, 289; Stocking 2001b, 314; Patterson 2002, 55–57). By the 1920s, however, the views of Boas and his students regained their leading position. Sociocultural anthropology dominated the discipline in quantitative terms as well as prestige: by 1932, more than 50 percent of the PhDs being awarded in the small field were on ethnological topics (Stocking 2001b, 289, 314–317; Patterson 2002, 64). Most of the leading departments in this period were dominated by Boasians.[9] Many new anthropology departments were created during the postwar decades (often by hiving themselves off from sociology departments), and they were typically founded by cultural anthropologists (Patterson 2002, 107). Those influenced by Boas radicalized some of the nonpositivist aspects of his approach, intensifying his bias toward studying

individual cases and interpreting them as complex wholes, rejecting the "scientific" quest for general laws, and emphasizing an approach that today might be thought of as a genealogical "history of the present": studying the contingent historical shaping of the contemporary sociocultural totality (Stocking 2001b, 42). Boas himself became increasingly skeptical about "the possibility of establishing valid categories for the comparison of cultural phenomena" and about "the possibility of establishing significant 'laws' in the cultural realm" (40). By the end of the 1920s, the object of anthropology for the Americans "became the construction or reconstruction of the uniqueness of individual cultures in relation to their histories" (Cohn 1981, 232). British-trained Bronislaw Malinowski agreed with Boas that the "goal, of which the Ethnographer should never lose sight," was to "grasp the native's point of view, his relation to life, to realize *his* vision of *his* world" (1922/1984, 25).

In the mid-1930s, however, " 'science' began to assert itself more strongly against 'history' " in anthropology, as in the other social science disciplines (Stocking 2001b, 45). During and after World War II anthropology saw a significant increase in funding opportunities from state and private sources, mainly oriented toward "big science" projects. Evolutionary theory made a comeback under various rubrics, including modernization theory. A collection of papers "from an international stock-taking symposium sponsored in 1952 by the Wenner-Gren Foundation and attended by eighty leading anthropologists" (318; see Kroeber 1953) had a largely positivist-scientistic tenor. The cultural ecologists, represented by such figures as Leslie White, Elman Service, and the young Marshall Sahlins, were pitted against what came to be called the symbolic anthropologists, whose most influential defenders were Clifford Geertz, Victor Turner, and David Schneider, in a "Manichean struggle" pitting the "emic" against the "etic" (Ortner 1984, 134). By the late 1960s, generalizing (though not quantitative) approaches appeared to be dominant in anthropology.[10] The crisis in the United States and the world at the end of the 1960s led to a resurgence of the post-Boasian cultural anthropologists, now mobilizing behind calls to "reinvent anthropology." The collection *Reinventing Anthropology* (Hymes 1972) "foreshadowed disciplinary developments of the next quarter century" (Stocking 2001b, 278). Among their concerns, the dissidents in anthropology at the time "shared in the then widespread disillusion with the 'positivistic scientism' of the anthropology and many other social science disciplines during the postwar period" (63).

The main disagreement among historians of twentieth-century U.S. anthropology, it seems to me, concerns the relative prominence and sig-

nificance of positivistic approaches during the 1945–1980 period. Hollinger (1996, 144 n. 32) agrees with Keane that even in this period anthropology largely resisted efforts to encompass it within the natural science model, in contrast to sociology and political science. What is clear is that it became impossible to speak of any postwar positivist predominance in anthropology by the 1970s at the latest. As Keane points out, anthropologists today almost instinctively reject any conception of doing science as a "depersonalizing gaze that separates subject from object" (Jean Comaroff and Comaroff 1992, 8; see also Clifford and Marcus 1986; Trouillot 1991). Among all of the social sciences, anthropology has continued to produce some of the most explicit critiques of comparison (see Povinelli 2001; Stoler 2001). Even if anthropology has lost its original object as modernity has "wiped out the empirical trace of the savage-object" (Trouillot 1991, 35), the field's inherited Boasian epistemological predilections predispose it to absorb postpositivist theories of singularity and incommensurability (Derrida 1995; Nancy 2000). The traditionally dominant methodical approach—intensive long-term fieldwork by an individual researcher—militates against the comparative design that is required by generalizing models of science, positivist or nonpositivist. The discipline's more recent moves in the direction of practice theory (Bourdieu 1977, 1990; Ortner 1984), multisited ethnography (Marcus 1995), discourse analysis (Clifford and Marcus 1986), and historical anthropology (Cohn 1987b; Axel 2002; Stoler 2002) have diverged in different ways from the natural science model.

Is it, then, correct to say that anthropology was "positivist in a positivist age" (Trouillot 1991, 29) during the twentieth century, and that it is nonpositivist in a nonpositivist age? The middle decades of the century, which saw a partial bending of anthropology in the direction of positivism, certainly were the heyday not only of government funding for appropriately configured social sciences, but also of logical positivism in the philosophy of science. The 1920s and 1930s, when the Boasians had their greatest triumph, were also less strictly positivist in the neighboring disciplines of sociology and political science (see Parsons 1937/1949; Steinmetz and Hauptmann, both this volume). Whether the current era is truly postpositivist is also open to interrogation. This question can be answered for the social sciences at large only through continuing discussion and observation. We should keep in mind the ongoing production of exceptions to the supposed rule of nonpositivism. Keane mentions the Human Relations Area Files, which started in 1949 with the ambition of collecting and coding comparable ethnographic data on all human cultures to facilitate

generalizing comparisons. This project still exists and continues to advocate a positivist-style approach to comparative cultural anthropology (Ember 1988, 2001). Yet these are not very popular topics for current anthropological dissertation research.[11]

Anthropology has also seen an ongoing internal backlash against Keane's "epistemology of intimacy," and major departments have split partly along these lines. Disputes over evolutionary genetics and what is now called sociobiology, from Boas to Sahlins (1976) and on to the recent controversies around Napoleon Chagnon's work on the Yanomami, revolve around core issues in the positivism/antipositivism debate: value-free science versus the interpenetration of facts and values; biological reductionism versus the ontological irreducibility of the social-cultural; the validity and ethics of the subject-object distinction, and so on.[12] The discipline's culturalist mainstream has been attacked by anthropological materialists who would limit study to *observable* behaviors (Wolf 1980). It thus seems advisable to consider anthropology as a field that is "structured in dominance" but by no means univocal, even if a neo-Boasian nonpositivist worldview prevails in a Bourdieuian sense. This configuration represents something of an extreme in the epistemological space of all of the human sciences.

The history of history

Positivism has been somewhat stronger in history than in anthropology. The U.S. history profession was long dominated by an empiricist epistemology, labeled "objectivism" by Peter Novick (1988), even if historians never wholeheartedly embraced the idea of positive laws of social behavior (Kloppenberg 1989).[13] As Kloppenberg (1022) notes, there were already "some historians intoxicated by the idea of becoming scientists" in the immediate postwar period. Furthermore, many New Left historians during the 1960s and 1970s were "anything but radical in their sometimes unselfconscious and sometimes defiant commitment to a naïve objectivist epistemology" (1023; see also Megill 1991). Positivist philosophers of science devoted a great deal of energy in this period to proposing ways that singular historical processes and events could be subsumed under (a series of) covering laws (Mandelbaum 1938; Hempel 1948/1965, 1974; Nagel 1961/1979, ch. 15). Their interventions did not lack influence among historians.

In his contribution to this volume, Bill Sewell focuses on the 1960s and 1970s and on the subfield of social history, which he helped to create and then to transform. Sewell points out that social history's political project of bringing to light the voices and invisible histories of the subaltern

classes led somewhat ironically to a reinforcement of a positivist or empiricist ethos.[14] Social history in this period was motivated by the laudable desire to study "new categories of people," especially ordinary people who had not left written records, and to ask "new questions about them" (Sewell, this volume). Along with these new substantive foci came a borrowing of methods from the social sciences as well as the epistemological approach typical in sociology and political science at the time. Sewell calls attention to the "new social history's uncritical objectivism, its presumptive preference for quantitative data, its default economic determinism, [and] its blindness to questions of meaning" (Sewell, this volume). Yet he lauds its "sense of the social," which was more "robust" than that of the subsequent cultural history.

With the consolidation of the "epistemological left" in academic historiography during the 1980s (Novick 1991, 703), however, this mixture of leftist populism and positivist epistemology began to seem strained and internally contradictory. The linguistic and cultural turns in history threw overboard most of the new social history and its social science methods and worldview, as well as the older commitment (see, e.g., Joan Scott 1988b; Megill 1989). Although social history had briefly become "hegemonic in the field," defining the very "terms of historiographical debate," cultural history now replaced it as the new orthodoxy (Sewell, this volume).[15] Novick paints a more pluralized picture of the post-1980s field, quoting a scriptural passage: "In those days there was no king in Israel; every man did that which was right in his own eyes." What Novick would define as objectivism lives on in several historiographic subfields. But it has become just one position among many.[16]

Even social history was never uniformly positivist, as Geoff Eley (this volume) reminds us. Eley associates social history with the goals of *explanation* and "grasping society as a whole." His main example of a social historian, E. P. Thompson, was connected to the cultural Marxism of the less economistic British New Left rather than positivistic social science. Thompson's (and Eley's) British social history thus differs in certain key respects from the U.S. version discussed by Sewell. Among other things, as Eley points out, Thompson was centrally concerned with cultural traditions and their complexities; Sewell notes that Thompson was "almost allergic" to quantification. With respect to the epistemological issues emphasized here, this means that Thompson's approach embraced an interpretivism that positivists usually reject because it is difficult to reconcile with deductivist covering laws.[17] Even if Thompson believed that "the development of capitalism was determining in the final analysis," his was clearly not a deductivist or positivist covering law version of Marxism.[18] This was evident both in the historical contingencies opened up by his emphasis on ideological constructions and transformations and in his rich description of the real alternatives to capitalism that were "championed by working-class communities" (Sewell, this volume; Sewell 1990; Thompson 1966).

Eley also alludes, however, to an image of historians becoming social scientists by "collecting, counting, and measuring data," which suggests that he too sees the earlier social history as epistemologically double-edged. Both Eley and Sewell thus reject the scientistic tendencies of the older social history while calling attention to that tradition's politically progressive and partially antipositivist potentials.

Eley's image of historians as accountants also recalls an alternate definition of positivism that one often encounters among historians and in the humanities. Ultimately, this is a methodological statement, but one that connotes an empiricist and antitheoretical unwillingness to depart from the narrowest reading of the sources, whether literary or archival (see Smail 2000).[19] Significantly, this version of positivism does not support any specific explanatory strategy but tends instead to eschew the goal of explanation altogether. This indicates an important difference from the dominant social science understanding of positivism. Indeed, there are similarities between the humanistic and historiographic version of positivism and anthropologists' hesitations about comparison and commensuration. Philosophical positivism, by contrast, requires the commensuration of events as a precondition for identifying "constant conjunctions." What is often called positivism in the humanities is opposed even to the level of theoretical abstraction required to commensurate events, and thus actually has partly nonpositivist entailments.[20] Traditional empiricist historiography thus shares some common ground with cultural anthropology.

The disciplines of anthropology and history thus represent two distinct disciplinary trajectories that have arrived at a similarly nonpositivist present-day condition, even if they differ in many other respects. This is especially remarkable in light of the fact that both disciplines were offered the opportunity to reconfigure themselves along the lines of the imagined natural sciences during the middle decades of the twentieth century. Positivism has been much more powerful and durable in the other fields explored here, even if it has undergone certain transformations and appeared in differing guises according to discipline and historical context.

Psychology and psychoanalysis

The associations between positivism and psychology were especially strong during the second half of the twentieth century, as indexed by the prevalence of behaviorism in that field (see Buckley 1989; L. Smith and Woodward 1996; L. Smith 1986). The subfield of social psychology has generally been just as hostile to nonpositivism.[21] Even to locate a frontier between positivism and nonpositivism in the fields concerned with the psychic, then, we are best advised to move beyond psychology into *psychoanalysis.*[22] Given its emphasis on the complex and unpredictable subterranean workings of the unconscious, one might expect psychoanalysis to have been less susceptible to the positivist temptation. Psychoanalysis is a prime example of a human science organized around depth-realist theoretical objects such as repression, the unconscious, fetish, and fantasy.[23] It is also an ideal arena for studying battles over social epistemology. Positivists—most prominently, Karl Popper—have been more interested in eliminating psychoanalysis than in assuming control of it or reconstructing it along the lines of the natural sciences. Psychoanalysis has been attacked from the start on empiricist methodological and epistemological grounds (Freud 1924/1961), and these criticisms have never really subsided.[24] Andrew Collier, in his contribution to this volume, points explicitly to psychoanalysis as one of the areas in which critical realism can offset some of the effects that "inadequate philosophical premises" have had on them.

As Anthony Elliott argues here, the institutions and sciences of the psychic—psychoanalysis, psychiatry, and psychology—have been attracted (especially in the United States) to a reduction of the psychic to a medicalizing "neuromechanical logic." They have promoted and attended to rationality as against fantasy, engaging in the "psychologization of desire." Elliott presents Freud himself as resisting or disavowing some of his own radically nonpositivist discoveries, above all, the irreducible dynamics of fantasy and unconscious sexuality and imagination. Freud was ostensibly committed to a naturalistic understanding of his own project, but Elliott argues that he also reached nonpositivist conclusions despite his original inclinations. Freud denied the possibility of using experimentation to test psychoanalysis or to measure concepts like libido, for example, and he resisted the idea of general laws of psychic development or uniform rules for treatment.[25] Freud's vigorous defense of lay analysis, as Russell Jacoby notes, was based on his prescient anticipation that "monopolization by medical doctors would degrade psychoanalysis into a specialty" (1983, 145; see Freud 1926/1959). Movements of psychoanalytic renewal from Lacan to Marcuse, and from feminist psychoanalysis (J. Mitchell and Rose 1982) to the contemporary Slovenian school (Žižek 1989), have repeatedly drawn sustenance from these sources in Freud. Focusing on Freud's unfinished 1895 paper "Project for a Scientific Psychology" (1895/1966), Elliott shows how both the biologistic and antipositivist potentials of psychoanalysis were present from the start. U.S. psychoanalysis aligned itself early on with the medical profession, which exposed it to the persistent pressures of a biologizing scientism. Clarence Oberndorf, one of the founders of American Freudianism, observed caustically that psychoanalysis, "once incorporated into medical schools . . . came to attract those who 'find security in conformity and propriety' " (1953, 127, 209–210). The flip side of this medicalization was "theoretical banalization" (Jacoby 1983, 23), meaning above all the marginalization of the unconscious and the erotic in favor of concepts located closer to the conscious or surface level, as in ego psychology.[26] This was part of a more general effort to provide a "strictly scientistic defense of psychoanalysis" (Whitebook 1999; see also Hale 1995, 2001).

No other field has had to confront such an onslaught of corrosive resistances. The broadest of these pressures are the general surrounding social conditions, which are conducive to an empiricist understanding of subjectivity. Elliott refers here to the "modernist tension between imagination and rationality" and "between imagination and specialized knowledge." Also important are the many ways capitalist modernity makes the sovereign, rational individual into the self-evident basic unit of social existence. A third factor is the presence of well-articulated theoretical approaches like behaviorism and brain chemistry around which scientistic-positivist resistance can be organized. Finally, there is the state, oriented especially in the past three decades toward cutting spending on mental health, and the pharmaceutical industry, perpetually oriented toward maximizing profits. These last two forces combine to replace the supposedly more expensive and less easily measured practices of psychoanalysis with pharmacological approaches to the management of symptoms (Roudinesco 2001). Although this displacement has been especially relentless in the recent past, it was already well under way in the middle of the twentieth century (Oberndorf 1953, 236–237). Criticisms of psychoanalytic practice by radicals like R. D. Laing, Michel Foucault (1980), and Deleuze and Guattari (1972/1983) ironically joined forces in this respect with the antipsychoanalytic campaigns of the health industry (Hale 1995).

Economics: a bifurcated discipline?

The field of economics has been paradigmatically committed to an atomizing ontology and a *deductivist* epistemology of lawlike generalizations,

according to Dan Breslau and Tony Lawson (both this volume). One of the sociointellectual conditions of possibility for consolidating this methodological approach was the construction of a delimited social object called "the economy" in the twentieth century (Mitchell, this volume). Tim Mitchell's essay here focuses on the pre–World War II period, Phil Mirowski's on the early and middle decades of the twentieth century, and Lawson and Breslau both emphasize the recent past and present state of the discipline.

Mitchell argues that social scientists invented the supposedly discrete spheres called the economy, the political system, the social system, and culture in the middle part of the twentieth century. He reminds us that Schumpeter thought that quantitative measures were especially adequate to the economic sciences due to the quantitative nature of the objects studied. The distinction between the national economy and external perturbances to the economy allowed economists to make generalizing statements, "if-then" statements, and predictions. Mitchell thus provides us with a succinct historical account of the processes by which positivism was able to become more plausible for economists. He also points out at the end of the essay that even mainstream economists in recent years have acknowledged the increasing proportion of economic activity that is *not* quantitatively measurable in any obvious way. The essay thus elegantly points to a sort of shearing pressure or contradiction between an established form of knowledge and its object. The question that remains open is whether these ongoing changes will actually unsettle entrenched forms of economics, and how. Breslau and Lawson address this more directly.

Positivism in economics has *not* tended to take the biologistic forms typical of Americanized psychoanalysis; nor has it eschewed all discussion of theoretical mechanisms, in contrast to psychological behaviorism. Mainstream economics does resemble psychological behaviorism, however, in its lack of interest in theorizing seriously about human rationality (Mirowski 1991, 2002) and its equation of science with the search for universal laws: the assumption of "regularity determinism" (Bhaskar 1975/1997, 69–71).[27] As Mirowski (1991) argues, the emergence of early game theory, including that of Von Neumann and Morgenstern (1944/1964), was related to the changing images of the natural world during the 1930s, including critiques of determinism and causality. Game theory could be described as nonpositivistic due to its open and apparently interactive imaging of the social, and to the possibility of multiple solutions to games, in contrast to more mechanical versions of regularity determinism. Yet Mirowski also shows that game theory's relationship to strategic military practices closed off certain epistemological possibilities. Actual applications of game theory in economics and strategic modeling emphasized static or nonrepeated games and singular solutions for games; subjectivity was construed as a rational black box. This realigned the approach with regularity determinism. As Lawson (this volume) and others (Yonay 1998; Weintraub 2002) argue, economic practice has been constituted above all by an a priori commitment to mathematicization, which militates against construing social processes as yielding a "myriad of incommensurate solution-concepts" (Mirowski 1991, 247); this is rooted in a deeper commitment to measurement and quantification (Porter 2001).[28]

Mirowski's contribution to the present volume situates the triumph of neoclassical economics in the years after 1940. This period also saw the ascendancy of a formulation I call methodological positivism in sociology, of logical positivism in the philosophy of science, and of behaviorism in psychology and political science. Of course, many of the raw materials for these postwar disciplinary formations originated in the interwar period or even earlier. Logical positivism traced its antecedents to the British empiricists, for example, and sociological positivism harkened back to Comte. But the period between World War II and the mid-1960s comes together as a rather coherent era for the social sciences, characterized by a relatively homogeneous regime of social science and an epistemological predilection for positivism.

When we turn to the present state of economics, however, it becomes more difficult to summarize in epistemological terms. Dan Breslau argues here that contemporary research by elite economists tends to be methodologically polymorphous, drawing at will on empirical-realist and aggressively antirealist forms of discourse. Lawson's analysis of mainstream economists supports this diagnosis, showing that economists alternate between depth-realist and antirealist/actualist (e.g., Friedman 1953b; Lucas 1986) formulations. Economists' more explicit and self-conscious methodological formulations may be more consistent than this, however. Breslau finds that the discipline is stratified between a dominant group of antirealist theoreticians and an empirically realist group of dominated econometricians and empirical researchers. He faults the "postautistic economics" movement (see below) for falling back into a naïve empiricism and for implicitly embracing its own dominated position. Yet, he concludes that a more adequate version of economics would attend to both empirical events and underlying causal structures (perhaps to structures of the sort identified by Breslau's own sociology of science, such as disciplinary or intellectual "fields"). The strategies associated with mainstream economics

as well as its "postautistic" challengers can thus be criticized for embracing either an antirealist and actualist theoreticism or its mirror image, an empiricist realism. Both strategies fail to move decisively beyond positivism. By the same token, both of the main postpositivist alternatives in economics, the analysis of rhetoric (Donald McCloskey 1985; Deirdre McCloskey 1994) and critical realism (Ehrbar 2002; Fleetwood 1999; Lawson 1997), can be faulted for reinscribing an inherited distinction between words and things that Breslau, like Latour (1999) and (post-)Wittgensteinian linguistics more generally, finds problematic. This division between words/rhetoric and things/realism is itself, Breslau suggests, a "product of the division of labor of knowing" in economics. Finally, Burawoy (this volume) reminds us that economics, like the communist parties of old, generates "rare but distinguished dissidents," but that these exceptions do not call into question the general organizational pattern.

Sociology: postpositivism eternally deferred?

U.S. sociology has been dominated by a positivist scientism throughout most of the postwar period, as Smelser (1986) argued, reaching something of an apotheosis during the decade of prosperity, social security, and unbridled optimism about science, the 1960s. U.S. positivism has encompassed a belief in the possibility of lawlike generalizations that are independent of time, space, and cultural meaning, the emulation of a misunderstood model of the natural sciences, and the deployment of philosophically misleading concepts such as middle-range theory, nomothetic (as opposed to idiographic) science, and falsification as a judgmental strategy (see Steinmetz and Chae 2002, and my own essay in this volume). Qualitative sociologists have often adopted a version of the Humean "constant conjunctions" model that is ironically more strictly positivist than the approaches adopted by most quantitative sociologists (e.g., Skocpol 1979; 1984, 378; compare Lieberson 1991). Mainstream sociology has also been characterized by a radically ahistorical sense of temporality and a disavowal of the narrative structures underpinning its ostensibly achronological, statistical approaches (Abbott 1992a, this volume). Abbott assails a reigning understanding of temporality that construes social processes as having a determinant endpoint and that therefore fit neatly into the inherited constant conjunctions model of explanation (see below). My essay offers a historical sociology of postwar U.S. sociology that reconstructs the macrosocial conditions that led sociologists to find it increasingly *plausible* to describe the social world in a positivist-scientistic manner. These constructed social "facts" were then used by positivistic camps in their "trials of strength" (Latour 1987, 78) with less positivistically inclined groups in the field. The goal was to transform the positivists' methodological, ontological, and epistemological preferences into seemingly obvious features of any sociology claiming to be a science. But none of this was a foregone conclusion: prewar U.S. sociology was far from unifed in epistemologic terms. Some of its leading figures explicitly opposed positivism and the natural science method. The question for sociology today concerns the likely effects of the fading of the macrosocial conditions that made positivism seem more immediately plausible than the alternatives, conditions I summarize under the heading of Fordism. The current reemergence of explicit epistemological dissent in sociology poses the question of whether the long period of positivist preeminence is finally coming to an end. Will the combination of post-Fordist social conditions and internal epistemic challenges succeed in dislodging positivist domination of the discipline, or will factors *internal* to sociology prolong positivism's life "unnaturally"? My essay sketches some of the possible implications of the transition to post-Fordism for sociologists' spontaneous social epistemologies and concludes that the grip of positivism on sociologists' imaginations may be weakening.

Margaret Somers's essay, on the other hand, provides a sobering counternarrative. Somers explores the recent penetration of sociology and some of the other social sciences by a reductively economistic understanding of the social encapsulated in the idea of "social capital." Somers's explanation of the growing popularity of this construction brings together intrascientific and environing social processes. With respect to the former, she points out that sociology has always looked to economics as a more successful social science. Sociologists were therefore thrilled when some economists began to adopt the ideas of social capital and the social embeddedness of markets. The idea of social capital was then reimported back into sociology (and political science) with the economists' stamp of approval, where it proceeded to displace genuinely relational concepts of the social. The proliferation of the idea of social capital, Somers argues, is actually at odds with taking the social seriously. Its use reinforces a central tenet of the postwar positivist settlement in sociology, which eschewed robust or depth-realist constructions of the social (Frisby and Sayer 1986). Somers argues further that the idea of social capital is theoretically incoherent, insofar as the social should be construed as a third term between the economy and the state (Arendt 1958). The social cannot be construed as a form of capital, because capital, at least in modern economic theory, is defined subjectively and atomistically, whereas the social has (at least partly) a transindividual character.

The permeation of sociology by the discourse of social capital may be an example of the successful resistance to pressures from the environing macrosocietal context that are undercutting positivism's instinctive plausibility. Positivism in sociology is finding new scientific allies to recruit in this effort. Additional concepts that were previously identified with nonpositivist sociology are being reappropriated by varying positivisms. The struggle over the ownership of the concept of "mechanism" is also suggestive of this dynamic. In the hands of critical realists, the idea of mechanism is distinctly unmechanistic and nonreductionist (see Bhaskar 1975/1997, 1979; Elster 1998; Mirowski 1988), but in other renderings of mechanism this is not the case (see Hedstrøm and Swedberg 1998). In short, sociological positivism may be able to successfully resist extrascientific developments urging the field to adopt more adequate epistemologies.

Political science and political theory

Both of the essays on political science in this volume agree that positivism has been all-powerful in the discipline throughout the postwar period. This began with the so-called behavioralist revolution in the 1950s, which explicitly adopted the language of empiricism and positivism and modeled the discipline "after the methodological assumptions of the natural sciences" (Easton 1965, 8; Gunnell 1975, 1–31; 1993, ch. 10; 1995, 924). Harold Laswell and Abraham Kaplan, in their 1950 manifesto for the behavioralist approach, insisted on "a thoroughgoing *empiricist* philosophy of science" and on "relating scientific ideas to materials ultimately accessible to direct observation" (xi–xii).[29] According to David Easton, another leading proponent of this movement, behavioralism emphasized "discoverable uniformities in political behavior, quantification, and value-freedom," all mainstays of the positivist tradition (1965, 17). Charles Lindblom argued years later that this movement "carried many political scientists toward a more *scientific* practice of their discipline" (1997, 231, my emphasis). Positive theory, introduced by William Riker, explicitly emulated the physical sciences (1962, 3–4). According to a recent overview, "The discipline has tended to accept implicitly a rather simple and, crucially, an uncontested set of positivist assumptions which have fundamentally stifled debate over both what the world is like and how we might explain it" (S. Smith 1996, 11). A content analysis by Schwarz-Shea and Yanow (2002, 457) of fourteen research methods texts used in political science found "a textual consensus on positivism as *the* mode of scientific research" in the discipline.[30] During the 1960s, rational choice theory began to replace behavioralism as the most powerful theoretical method in the discipline, and it currently reigns supreme.[31] As I argue below, many versions of rational choice theory are also epistemologically positivist.

Unlike sociology, political science has preserved a specific subfield in which positivism has held little attraction: political theory.[32] In this arena, normative analysis is the rule, rather than a forbidden activity, as Mihic, Engelmann, and Wingrove detail here. Members of the theory subfield have long defined their topic as politically engaged commentary and interpretation anchored in a canonical set of texts, as Emily Hauptmann (this volume) points out. Furthermore, they have portrayed their subfield in explicitly anti-positivist terms, especially in the 1950s and 1960s. Those in political theory cannot be said to be doing "the work of theorizing" for those located in subfields like comparative politics, international relations, or political behavior, however, because these fields deal with questions of "is" rather than "ought." In these other parts of the discipline, theory tends to mean "formal" or "positive" theory.

The essay by Mihic, Engelmann, and Wingrove argues that political science's postwar settlement—the division between a distinct subfield of political theory and the rest of the discipline—has had marked effects on the overall epistemological power structure and unconscious of the field. This division serves to reinforce positivist hegemony in the discipline as a whole, despite, or because of, the explicitly normative and antipositivist orientation of the political theory subfield. The authors begin from the premise that facts and values are not amenable to radical separation but should instead be seen as mutually constitutive.[33] Yet the very division between theory and the rest of the discipline strengthens this untenable dichotomy. More specifically, the discipline's institutional division contributes to the doctrines of *value neutrality*, "the presumption that normative commitments and/or assumptions can and should be set aside, or 'bracketed,' in the process of scientific analysis," and its mirror image, *fact neutrality*, "which presumes that data are ancillary to the main preoccupations of the analyst." This institutionalized separation allows most of political science to continue to propose universal lawlike statements with little concern for the imbrication of facts and values, and also permits political theory to proceed in pristine isolation from the real (recalling mainstream economics, as analyzed by both Lawson and Breslau in this volume).

Critics have argued variously that a political science organized first around behavioralism and later centered on positive theory (Riker 1962) and then rational choice theory both *undercuts* and tacitly *endorses* liberal democracy. In a sustained critique of a behavioralism he characterized as positivistic, Leo Strauss (1962) argued that there was an "unavowed com-

mitment built into the new political science" to a version of "liberal democracy" that was not discussed openly or impartially "with full consideration of all relevant pros and cons." By looking for "laws of political behavior to be discovered by means of data," Strauss continued, behavioralism put a premium on "the study of things which occur frequently now in democratic societies; neither those in their grave nor those behind the Curtains can respond to questionnaires or to interviews. Democracy is the tacit presupposition of the data; it does not have to become a theme; it can easily be forgotten. . . . [But] the laws of human behavior [that it discovers] are in fact laws of the behavior of human beings more or less molded by democracy; man is tacitly identified with democratic man" (326).

Rational choice theorists who did address the question of real alternatives to liberal democratic capitalism of the sort Strauss had in mind (e.g., some sort of socialism, as opposed to technical adjustments to the existing system) focused mainly on distributions of material resources and assets and on economistic cost-benefit calculations (compare Przeworski 1985), downplaying the specifically *political* considerations, arguments about the formation of preferences or values that Strauss, among others (e.g., Sahlins and the cultural anthropologists discussed here by Keane), saw as crucial and irreducible to economics or sociology. Rational choice theory, even of the Marxist variety, can thus be seen as tacitly reinforcing the existing *metapolitical* rules of the game even when it discusses transitions to noncapitalist *rules* (Roemer 1982). Rational choice theory can also be criticized for undermining those selfsame rules, of course, by emphasizing the economic irrationality of voting (as stated most famously by Mancur Olson 1965), the inefficiency of democratic decision making (Luke 1999, 347; Green and Shapiro 1994), and "the incoherence of the idea of popular sovereignty" (Mihic, Engelmann, and Wingrove, this volume).

Area studies

The funding and direction of social research by corporations and, especially after 1940, by the U.S. state played a crucial role in all of these disciplines, but it was more pronounced in some than others, and perhaps nowhere more than in area studies (Robin 2001; Miyoshi and Harootunian 2002; Patterson 2002, 115–132). For the most part, corporate and state funders have favored social research patterned on the natural sciences and focused on the production of general and empirical laws of behavior with practical applications. The emphasis on immediately applicable results has been relaxed in certain periods and places, but not the overarching preference for social science packaged according to positivist protocols.

The area studies fields have not promoted a single version of positivism, however. Instead, they have combined forms of objectivism, such as the position analyzed by Novick (1988) in history, with the search for covering laws. In addition, these two sorts of positivism can be related to different moments in the development of area studies. In his contribution to this volume, Michael Dutton analyzes the specific interdisciplinary field of Asian area studies. Dutton begins with the observation that Asian area studies has been resistant to theory in the name of a "truculent antitheoretical empiricism" (see also Rafael 1994, 101). Asian studies resembles other area fields that emerged in the nineteenth century in emulating a distorted version of the natural sciences and eradicating "unwanted signs of the heterogeneous that cannot be incorporated into the homogeneous world" of social science models (Dutton, this volume).

Dutton's central evidence for area studies' descriptivism concerns the privileged role of *translation* in its procedures and self-understandings. As Dutton points out, area studies' insertion into the positivist mainstream was abetted by its fetishization of acts of translation. Indeed, the basic empiricism of Asian studies flows directly from the "types of methods employed in language training itself." Rather than engaging with "people's way of thinking and feeling" (Dutton, quoting Michel Bréal) in terms of analysis of the disjunctures between surface and depth, area studies remained at the surface level and emphasized descriptive and applied research, often in the service of colonial power and, more recently, in the service of business. Whereas mainstream positivist social science has denigrated area studies as merely idiographic, the emphasis on translation actually suggests that cultures can be compared in a common metric. It is a small step from there to the search for empirical generalizations across time and space.

At the same time, Asian studies, like other area studies fields, "has the potential to send ripples of doubt through the dominant positivist social science 'stories.'" This intrinsic potential for epistemological dissent is lodged in area studies due to the difficulty of excluding from it the humanities and the more humanistic social sciences completely. Even if translation tends to inculcate the belief in ready comparability and generalization, it is rooted in philology, and some of the most dramatically nonpositivist epistemological programs have grown out of an engagement with classically philological themes. Although Oriental studies chose to become an empiricist "content provider" rather than following Freud, Nietzsche, and Marx into a radical interrogation of language, it has the potential for once again becoming "postpositivist" by rejoining earlier lines of inquiry.[34] The

nonpositivist promise of area fields also stems from the fact that each of them is concerned with a specific region and hence with unique sociohistorical totalities. The natural science persuasion in the human sciences, by contrast, has long been oriented toward dissolving social wholes into elements and substituting "names of variables for the names of social systems, such as Ghana, the United States, Africa, or Asia" (Przeworski and Teune 1970, 8). Area studies' ability to be "enchanted with difference" stands in sharp contrast to the procedures of nomothetic mainstream social science, for whom radical alterity is mere noise (or grist for "deviant case analysis"). Concerted efforts to remove the area focus from area studies have not been entirely successful, partly because a globally active hegemon often demands detailed empirical knowledge of very specific places, and partly due to refusal within the area studies community itself (D. Cohen 1997).

The philosophy of science

The final discipline examined here is the philosophy of science, whose twentieth-century U.S. history is examined by Philip Mirowski. Mirowski explores the shifting interplay between the philosophy of science and social change, looking specifically at relations among science, state, church, and corporation. He asks about earlier antecedents of the post-1980s acceptance of the social embeddedness of science. Like Dutton's reading of Asian area studies and my own depiction of sociology, Mirowski describes a partly nonpositivist past being overshadowed during the middle decades of the twentieth century by a positivism linked with the state and business. More specifically, he discusses the displacement of the pragmatists by the logical positivists in the U.S. after World War II. The question he asks here is "how it was that science came to be portrayed by philosophers as asocial and autarkic and value-free in the United States in the middle of the twentieth century." Mirowski argues that the dominant views of the society-science relationship in the 1914–1940, 1940–1980, and post-1980 periods can be understood in relation to "the types of environments in which scientific research was being prosecuted" and the types of social theories dominant in these eras. During the first period, John Dewey opposed the prevailing cult of the expert (itself a product of the concentration of scientific research in private corporations) with a dream of science dedicated to and embedded within the wider democratic community. In the second period, the funding and organization of U.S. science was assumed by the military and the state more broadly. The Operations Research (OR) profession that emerged in World War II "was a practical

response to the problems . . . of the military planning and organization of science" (Mirowski, this volume). Just as U.S. foreign policy was characterized by a mixture of "absence in principle and presence in practice" (Ulmen 2003, 26; C. Schmitt 2003, 255), the OR framework allowed scientists to construct a "delicate amalgam of engagement and aloofness": they could enjoy "military largesse" while gaining "a fair amount of latitude in evading direct control by the military" (Mirowski, this volume). Operations Research thus provided the template for the concept of the autarkic "scientific community" completely separate from society, a notion codified by Robert Merton and Michael Polanyi and by the other philosopher Mirowski examines here, Hans Reichenbach. According to Mirowski, the pact between OR and philosophers of science also accounts for various features of the new scientific regime, including its preference for mathematical formalisms and the "conflation of mathematical prowess with intellectual virtue"; its "insistence on a generic scientific method based on logic and probability and indifferently portable to any subject or discipline"; its antidemocratic elitism and "contempt for tradition"; its supposed value-freedom; and Reichenbach's (1951, 231) well-known distinction between the "context of discovery" and the "context of justification," which drove a wedge between philosophy and sociology. Various protocols that were worked out during World War II, including the fact that "grant overheads could buy off the principal investigator's academic obligations to his or her home institution," worked "in favor of treating the scientist as though he or she were a member of a community apart from the general run of intellectual life" (Mirowski, this volume). The logical positivist program in the United States thus "owed its good fortune to OR."

Elsewhere, Mirowski (forthcoming) characterizes the science regime of the more recent period, roughly since 1980, in terms of the outsourcing of research activities and the growth of a thicket of legal regulations and conflicts around intellectual property, the death of the idea of the freedom of the individual scientist, and the erosion of authorship. But most philosophers of science, he argues, have registered these drastic changes only indirectly via a partial acceptance of the "social dimensions of science." Biology and economics have replaced physics as the more "progressive" (i.e., profitable) sciences from which to derive epistemological generalizations. The exemplar of the new philosophy of science analyzed by Mirowski, Philip Kitcher, adopts the language of the social sciences most palatable to the new corporate clients—economics—and models all agents as "neoclassical rational choosers" with "preferences" in a self-avowed "methodological individualism."[35] Of course, some philosophers of science, includ-

ing the ones in this volume (Harding and Collier), reject this recharged positivism. Harding's standpoint theory has long acknowledged the importance of social (especially gender) locations for science; Collier's critical realism embraces what Bhaskar called "epistemic relativism"[36]—that is, a post-Kuhnian certainty that the choice of scientific theories is determined at least in part by "social" factors rather than the correspondence of a theory to its object of analysis.

Combined and Uneven Developments

As disciplines dominated by tenacious positivist or late positivist assumptions, sociology, political science, area studies, and even the philosophy of science provide strong contrast cases with anthropology and (post-1980s) history. But the epistemic cultures and latent underpinnings of these disciplines also differ in specific ways, many of them having to do with differing understandings of "theory." Sociological positivism has encompassed a doctrine of value-neutrality similar to the configuration in political science as described by Mihic, Engelmann, and Wingrove. Indeed, this assumption was codified by Max Weber, whose methodological essays were canonized in postwar U.S. sociology (see especially Weber 1949). Yet there has been no parallel mapping of facts and values onto disciplinary subsections of sociology; rather, there has been less space overall for theory tout court.[37] Partly due to this absence, the discussion of normative theory/values has been less sustained in sociology than in political science. Efforts to reintegrate values and facts within sociology have typically emerged in the framework of broader epistemic and theoretical disturbances in the field as a whole and not in any particular subfield.[38] By the same token, the sort of atheoretical quantitative description of statistical correlations between variables and outcomes that is typical of "journal sociology" (Abbott 1992a, this volume), and that was often the end product of the empirical materials provided by area studies specialists, seems somewhat less widespread in political science.

Other differences can be gleaned from a comparison of economics, history, political science, psychology/psychoanalysis, and sociology. Economics, as Lawson observes, has been primarily deductivist, whereas sociological positivism has more often been inductivist, discovering supposed regularities through the manipulation and massaging of statistical data (Hanushek and Jackson 1977, 1).[39] Explanations in economics and sociology rarely take biologistic forms nowadays (although this may be changing with the increasing availability of funding from the life sciences);

this boundary has been less taboo in psychology. Nor have economics and sociology tended to adopt explicitly *behaviorist* forms of explanation, a version of empiricism that eschews the unobservable theoretical object altogether, even as a causally salient black box. This differentiates both fields from psychology and political science. Rational choice theory has made fewer inroads into sociology than into political science, and almost none at all into history or anthropology. Yet, as Somers argues here, one of rational choice theory's "part objects"—social capital—has had enormous success recently in sociology, operating as a Trojan horse for mainstream economics.

The timing of shifts in epistemological focus also varies from one field to the next. Freud's epistemologically contradictory writings have allowed antipositivist tendencies to resurface periodically in psychoanalysis, challenging the biomedical mainstream. Sociology has been predominantly positivist since 1945, aside from a brief period of epistemological turbulence between the end of the 1960s and the mid-1970s. Philosophical positivism had a relatively brief heyday among some historians during the 1950s and 1960s, even if positivism understood as an atheoretical and empiricist unwillingness to depart from a narrow reading of archival facts has had a much stronger grip on that field. Economics has been much less prone to movements of self-criticism than anthropology and sociology, even if the current context of the "science wars" has allowed some dissent to emerge even there (see especially Hands 2001; Weintraub 2002; Dierdre McCloskey 1994; Mirowski 1989, 2002). Nonpositivist approaches are well entrenched in some of the "interdisciplines" like science and technology studies, cultural studies, and gender and race studies.

Alternatives to Positivism in and beyond the Disciplines

The reader of the essays in part 1 might justifiably be left asking, Quo vadis? Besides examining the differing textures and chronologies of the relationship between positivism and the human sciences, a second goal of this collection is to lay out some of the metatheoretical alternatives. All of the contributions to this volume, including those in the first part, discuss and exemplify nonpositivist approaches of one sort or another. The second part, however, provides more of a *tour d'horizon* of alternatives. As noted, this collection does not propound any specific nonpositivist alternative. There is little agreement even in the most solidly postpositivist sectors of the human sciences about epistemological questions beyond a common rejection of positivism, and this is partly true of the contributors

to this book as well. Nonetheless, these essays taken together conjure up a pluralistic postpositivist counterworld.

Positivism and antipositivism in the social sciences have both drawn sustenance from philosophy over the course of the past century. The first two essays in part 2 are by philosophers, and both of them are explicitly programmatic. Sandra Harding traces the development of standpoint epistemology, which emerged as a critique of positivism's "view from nowhere" and its doctrine of the unity of the sciences. Harding also touches on the Frankfurt School as a precursor to standpoint theory.[40] She explores the mutually constituting relationships between standpoint epistemology and the new social movements, especially in the context of the "Network Society" as described by Castells (1996/2000a). Harding also emphasizes some of the continuities between positivism and standpoint theory, reminding us that logical positivism encompassed a commitment to democracy, rationality, objectivity, and fairness.[41] The liberal left politics of early logical positivism are indisputable, even if this seems somewhat puzzling in light of the contemporary association between the epistemological and political lefts. As Carnap recalled, "All of us in the Circle were strongly interested in social and political progress" and "most of us, myself included, were socialists" (1963, 23).

Andrew Collier discusses the critical realist philosophy of science, which positions itself explicitly as an "underlaborer" for the sciences and as an alternative to both positivism and the various antipositivist positions it calls idealism or conventionalism. The *transcendental realist* half of this program develops an ontology of "hidden mechanisms, unexercised powers, and unrealized possibilities," going beyond the "actualist" proscription on disjunctures between the empirical and the real. This ontology "shows that there is a *plurality* of mechanisms conjointly determining events," meaning that constant conjunctions are an extremely rare exception in natural (nonlaboratory) settings, rather than the norm. As a result, prediction is demoted, becoming a rare event rather than some general goal for science. The "rainforestlike profusion of different kinds of reality" that critical realism's ontology takes for granted should be reflected in a "plurality of sciences." The *critical naturalist* half of this program contains a careful explication of the similarities and differences between the social and natural sciences and argues against both positivism and the actor-network approach to science studies (Latour 1999) with respect to their rejection of any differentiation between the two. Without denying that the study of the social can be quasi-scientific, critical naturalism specifies a set of specific differences between the objects of the natural and social sciences. A transcendental retroductive argument that asks "how any human activity in society is possible" allows critical realism to deduce the necessary existence of "two distinct kinds of being": on the one hand, "social institutions that preexist the agent," on the other, "an intentional [though not necessarily transparently rational] agent whose action presupposes and makes use of these institutions" but is not fully explained by them. Combined with other ontological peculiarities of the social, such as the time-, space-, practice-, and concept-dependence of social structures (Bhaskar 1979; Steinmetz 1998), the positivistic thesis of the unity of the sciences, at least in its simpler forms, is revealed as problematic. Ontological divisions in the realm of the human or social also suggest the rationality of different social sciences, or at least distinct theories mapped onto these distinct divisions (see also Burawoy, this volume, for a related critique of the idea of collapsing the social science disciplines).

Tony Lawson's essay presents a critical realist approach to the field of economics. It begins, however, with an analysis of the dominant orientation in economics toward mathematicization and deductivism. Lawson proposes that economists instead begin by developing substantive ontologies that recognize the openness of the social (including specifically economic practices) and thus the implausibility of deductive models. Like Lawson's, Dan Breslau's contribution also could have been placed in either the first or the second part of this book. Breslau presents the results of an ethnography of methodological discourse in contemporary economics. He finds that economists' discourse on method is structured in a way that corresponds to the overall structure of the field: theory dominates substantive inquiry, purity dominates worldliness, and antirealism dominates empirical realism. Elite economists are liberated from the demands of empirical verification and rationalize these privileges by defending an antiempiricist methodology. Dominated economists, by contrast, tend to articulate a moralistic respect for the concrete observable realities of economic life.

Andrew Abbott's essay analyzes the different conceptions of time in sociology and economics, focusing on the ubiquitous idea of "outcome." The fetishization of outcomes flows from an understanding of social science as oriented toward identifying stable concatenations of events. He juxtaposes this approach to a more processual, Bergsonian understanding of time. To establish the existence of these two models of temporality in sociology, Abbott examines the work of Angus Campbell and Paul Lazarsfeld, two leading postwar researchers, finding evidence of a more processual approach in the work of the latter. Abbott insists that social scien-

tists think seriously about the metaphysics of temporality that informs their work, rather than accepting the ontology that is suggested implicitly (or promoted explicitly) by a configuration of research around the explanation of fixed outcomes.

In the concluding essay of part 2, Geoff Eley surveys three salient epistemic/methodological moments in the writing of history during the past four decades, tracking the seismic shift from social to cultural history and beyond. Eley focuses on the politics and the overall "structure of feeling" that informed and emerged from these ways of writing history, following an arcing movement from the *optimism* of the early era of radical social history (exemplified by E. P. Thompson), through to the *disappointment* of the society- and class-based project of social history (exemplified by German historian Tim Mason). The third moment in this narrative, a self-reflexive approach to cultural history, is epitomized for Eley by Carolyn Steedman's *Landscape for a Good Woman* (1987). Eley concludes with a call for a "defiant" historiography, one that connects resistance in the present (specifically, resistance to the new neoliberalism, imperialism, and domestic authoritarianism) to thinking about the past. Like Sewell, Eley calls for a recovery of the more politically engaged spirit of the earlier social history, without abandoning the intervening lessons learned.

In the final essay in this volume, Michael Burawoy begins with three problems raised by the Gulbenkian Commission (Wallerstein et al. 1996). The first traces the connections between colonialism/imperialism and social science (e.g., Asad 1973; Connell 1997) and connects these to a universalistic denial of difference, crystallized by nineteenth-century social evolutionary theory (Stocking 1987) but persisting into the present. The second is a scientistic context-free positivism. Third is the supposedly arbitrary division of the disciplines, which can be overcome by unifying them. As Burawoy suggests, this utopian program of unification neglects the findings of science studies and the sociology of knowledge and the principle of epistemic relativism and proposes that the social sciences "can finally escape the stamp of the society they interpret." Burawoy proposes instead *provincializing* the social sciences in the sense of grounding them in their particular contexts of production and objects of knowledge. This involves asking explicitly normative questions about the *addressees* (internal and external to academic fields) and the *goals* (instrumental vs. reflexive) of disciplinary knowledge. Situating different styles of social science in a table generated by these two dimensions (audiences and goals), Burawoy suggests that the social sciences, such as sociology, are necessarily located at the "intersection of the humanities and natural sciences." Like Mihic et al. (this volume), this suggests that the human sciences need to avoid both value-neutrality and fact-neutrality. Social sciences have to both interrogate and criticize "factish" (Latour 1999) knowledge and avoid becoming fact-neutral in a gesture of Olympian disdain.

The current epistemological conjuncture of possibilities in the human sciences is thus exceedingly varied and complex. The contributions to this volume may help readers make sense of these complexities and sort out their own positions. Would-be nonpositivists embrace a wide array of philosophical positions nowadays. An almost equally wide range of *political* positions is imputed to both nonpositivists and positivists as well, belying the simple heuristic that opposes an "epistemological left" to an "epistemological right."[42] Further complicating the issue is the fact that social scientists designated as working in a positivist way often refuse this description of their work. Raymond Williams once remarked that positivism has become "a swear-word, by which nobody is swearing" (1983, 239). Williams acknowledged, however, that "the real argument is still there." Our next task, then, must be to provide a working definition of this "swear-word" to make sense of the "real argument" that encompasses it.

The Uncanny Persistence of Positivism

If we take in our hand any volume . . . let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames.—David Hume, *An Enquiry Concerning Human Understanding*

Thus in advancing we have insensibly discovered a new relation betwixt cause and effect. . . . This relation is their constant conjunction.—David Hume, *A Treatise of Human Nature*

Recently Dahrendorf implied that the positivism criticized by the Frankfurt School no longer existed. . . . Nevertheless, one should not lose sight of what continues to survive untouched in positivism. . . . Unified science has triumphantly ousted the schools as archaically qualitative entities.—Theodor Adorno, introduction to *The Positivist Dispute in German Sociology*

The humanists . . . suffer from feelings of inadequacy in a world dominated by statistics and technology . . . the radical and "critical" political theorists, like the ancient prophets, lay about them with anathemas against the behaviorists and positivists. . . . But their anti-professionalism must leave them in doubt whether they are scholars.—Gabriel Almond, "Separate Tables"

There are good reasons nevertheless to take positivism as one of our starting points here. Most important, there is ample evidence that positivism is alive and well in at least some of the human sciences, even if it

usually goes under different names nowadays, presenting itself in guises that differ from the versions popular between the 1920s and the 1960s. Thus, for every observer who insists that renewing the positivism debate is beating a dead horse there is another who identifies a resilient "positivist empiricism" (John Comaroff 1981–82, 144). The continuing hold of the positivist imagination can be felt in an emphasis on general, and usually empirical, laws; in doctrines of falsification or prediction; in a spontaneous preference for "parsimonious" explanations (forgetting that the first meaning of parsimonious, in the OED, for example, is "stingy") or for mathematical and statistical models; and in adherence to a caricatured view of the natural sciences as a role model. Each of the disciplinary-specific epistemological protests discussed in part 1 underscores the continuing existence of a robust, if updated (and sometimes camouflaged or unconscious) positivism.

Another reason for organizing a volume partly around positivism and nonpositivist alternatives is that positivism is still an important folk category among social scientists. On the one hand, positivism functions as an epithet among what Peter Novick (1991, 703) calls the "epistemological left." Understanding the differing ways this "bad other" is defined in the various disciplines can be useful in diagnosing and distinguishing their understanding of nonpositivist alternatives. On the other hand, a handful of scientists have somewhat defiantly embraced and defended positivism in recent years, sometimes under new labels such as "consilience" (E. Wilson 1998/1999).[43] Concepts like objectivism and scientism, lacking the august philosophical tradition of positivism, are genuine "swear-words" with no actual defenders. Before we can understand the epistemological reactions for and against positivism by contemporary participants in the human sciences, we need a stronger sense of the actually existing positivisms that have resided in each of the fields.

A final reason for beginning with the category of positivism is explicitly historical, at least if we understand history in terms of what Foucault called the "history of the present." Well into the 1950s positivism was still being explicitly defended by prominent philosophers (see Ayer 1959) and leading social scientists (e.g., Lundberg 1939a, 1955). Logical positivism or logical empiricism may have lost some of its luster by the 1950s, but there were few serious contenders. Positivism still constituted the telos of Bertrand Russell's widely read *History of Western Philosophy*, which was published in 1945. Many influential contributions continued to be made within the tradition, even if they increasingly avoided the language of positivism and empiricism (see Reichenbach 1951; Nagel 1961/1979; Schlipp 1963; Hempel 1948/1965; Stinchcombe 1968). Since the 1960s most social scientists have rejected any explicit association with positivist positions.[44] Hilary Putnam (1990b, 44) wrote at the beginning of the 1980s that Ayer's (1982) still partially positivist "style and spirit no longer speak to the concerns of practicing philosophers." Yet introductory philosophy courses today usually include sections on epistemology and treat precursors to twentieth-century positivism such as Locke, Hume, Comte, and John Stuart Mill. A. J. Ayer's classic collection, *Logical Positivism* (1959), is still in print. As Sandra Harding notes in her contribution to this volume, positivism's core beliefs continue to shape research disciplines as well as "our social institutions." The contemporary philosophy of social sciences has an important neopositivist wing,[45] even if this is only a subsection of a subfield of a discipline. A recent reassessment concludes that logical positivism "provided the working framework of most philosophers of science from roughly the 1930s to the 1960s" but that more recent projects in the philosophy and social study of science continue to be motivated by positivism's demise (Richardson 1996, 1).

Additional questions concern the forms, definitions, locations, and sources of positivism. Reconstructing the genealogy of twentieth-century positivism is crucial for alerting present-day readers to a core cluster of ideas that resurface periodically under different names and in varying guises. We can then ask why certain disciplines and periods have been more inclined to adopt a positivist self-understanding than others. Why have some social science fields been more eager to accept the resources offered to those who would adopt a natural science approach? What explains the differential timing, the variable ebbing and flowing, of positivist approaches in history, sociology, anthropology, psychoanalysis, economics, political science, and philosophy? Which contemporary epistemological positions can usefully be designated as (neo, or neo-neo) positivist, and how do they resemble and differ from the positivisms of previous eras? Are alternative terms like "objectivism" (R. Bernstein 1983; Novick 1988; Megill 1994; Keane, this volume), "theoretical realism" (Gunnell 1995; Somers 1998), "deductivism" (Lawson, this volume), "instrumentalism" (Gunnell 1995), or "mitigated positivism" (C. Taylor 1985b) better or more precise descriptions of the social scientific practices under discussion?

Before we begin to explore the answers to these questions, or to ask why positivism is perpetually disavowed and unconsciously embraced, it will be useful to provide a working definition. As both Collier and Lawson argue in their contributions to this volume, the version of positivism that is most relevant for discussions of contemporary social science is one

derived ultimately from Hume. It has been championed and developed further by John Stuart Mill, Ernst Mach, Karl Pearson, Rudolf Carnap, Moritz Schlick, Carl Hempel, Otto Neurath, Ernest Nagel, and Karl Popper. Positivism in this sense is above all a position within *epistemology*, even if it also entails implicit ontological assumptions, like any other theory of knowledge. Specifically, positivism insists that scientific explanations take the general form "if A then B" or more elaborate (including probabilistic and multivariate) versions of these Humean "constant conjunctions." As Hume wrote, "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*" (1748/1975, 76). Such statements presuppose the *invariance* of causal relationships.

Any theory of knowledge must also assume "that the nature of reality is such that it could be the object of knowledge of the required or specified sort" (Lawson, this volume). Causal invariance of the Humean sort is rendered possible by assuming the ontological *closure* of the observed system. Logical positivism in its original form made no claims about causal powers, of course, but was strictly a description of empirical correlations.[46] It was associated with an *empiricist* ontology, even if it made no explicit ontological claims, and was thus also called logical empiricism (Carnap 1928; 1963b, 870). According to empiricists, underlying causal structures or mechanisms either do not exist, or they are imperceptible and inaccessible like Kantian noumena, and are therefore off-limits for scientific statements. Even after Carnap and other logical positivists began at the end of the 1930s to acknowledge that scientific concepts could *not* be reduced "to the given, i.e. sense-data, or to observable properties of physical things" and to admit the possibility of "theoretical concepts" (Carnap 1956; also Carnap 1966, ch. 23), the latter were "regarded as mere devices for deriving the sentences that *really* state the empirical facts, namely the observation sentences" (H. Putnam 2002, 24).

One of the most significant developments in positivism's career during the second half of the twentieth century was the acceptance of theoretical terms as real entities. This allowed another revision that combined the Humean rule of invariance ("regularity determinism") with stronger notions of causation and the acceptance of unobservable, conceptual, or theoretical causal objects. The original empiricism of Hume and the logical positivists was thus severed from the epistemological commitment to constant conjunctions; these might now connect an unobservable cause to an observable event.[47] This version of positivism, which can be called a *depth-realist positivism*,[48] retains the basic positivist commitment to covering laws

as well as the ontological premise of all covering laws: system closure. Depth realism thus turns out to be compatible with positivism, as long as the relevant causal mechanisms or explanans are uniform across all instances of a given explanandum. Recall that earlier positivists held that "no fact is explainable without a law, which asserts a causal correlation, no matter whether the law is arrived at inductively as a generalization from instances or is deduced from a set of more general laws or theories" (Lloyd 1986, 48).[49] For Carnap (1966, 4), universal laws took the form (x) (Px * Qx). It was then a simple step to define the property P of x as an unobservable and theoretical term referring to a mechanism. Although the 1929 logical positivist manifesto had emphasized *both* positivism and empiricism (Der Wiener Kreis 1929/1973, 309),[50] the two were able to part ways. By the early 1960s the logical positivists "had more or less collectively developed and agreed" on a revised version of their approach that allowed systematic correspondence rules linking observables and nonobservables (Lloyd 1986, 51; Hempel 1948/1965), even if they could not agree on whether the theoretical postulates referred to "real but as yet undiscovered entities or mechanisms"—which would constitute a form of *positivist realism*, or *depth-realist positivism*—or merely to "useful fictions or instruments for deriving empirically testable statements (instrumentalism)" (Lloyd 1986, 53). What they retained was an updated version of the constant conjunctions model, now renamed (by Hempel) the deductive-nomological model. According to this D-N model, events are explained by being subsumed under a general law that can take both deductive and "inductive-statistical" forms (Hempel 1948/1965, 1966, 1974; S. Smith 1996, 15).

Positivism has therefore been neither monolithic nor immutable. It has continued to evolve from Hume to Comte (who coined the term), via Mach and the logical positivists, and on into the present (R. Miller 1987). Some social scientists continue to understand positivism in classical Humean terms as necessarily combining three features: regularity determinism, empiricism, and empirical testability (the "logic of justification").[51] This is the view that "*science* and *scientific* . . . are words that relate to only one kind of knowledge, i.e., to knowledge of what is observable" (Van Dyke 1960, quoted in C. Taylor 1985b, 58). To take a more recent example, Hassard (1993, 6) claims: "The crux of positivist inquiry is that we can only have true knowledge of *explicit* phenomena and the relations between them. Scientists should not make hypothetical inferences about the essence of the implicit structure of phenomena; they should instead identify phenomena which are systematically connected to one another by way of invariable and universal laws." This passage combines the doctrines of

empiricism (only "explicit phenomena" and not "implicit structures" can be the object of science) and regularity determinism ("invariable and universal laws"). But many contemporary social scientists link "implicit structures" causally to empirical phenomena under universal laws (depth-realist positivism), while others engage in an "actualist positivism," concocting theoretical statements with overtly fictitious concepts.

Depth-realist positivism can be found in a variety of different theoretical and methodological programs. Contemporary statistical approaches in the quantitative social sciences such as structural equation modeling often allow for latent (i.e., unobservable) constructs. These latent constructs are "indicated" by observable "explanatory" and "response" variables, that is, by variables meant to represent causes and effects (Jöreskog and Sörbom 1996).[52] If the relationships between latent and observed constructs are assumed to be constant, we can infer that the approach is premised on system closure. It can then be described as a version of depth-realist positivism.

Turning to substantive social theory, orthodox Marxism also often takes the form of a depth-realist positivism. Traditional Marxism posits some unitary underlying cause—the mode of production, social class, technological change, the organic composition of capital—that explains all instances of an empirical phenomenon (prices, revolutions, forms of state).[53] Depth-realist positivism also characterizes the subset of rational choice approaches that are realist about ontology but positivist about epistemology (see Green and Shapiro 1994, 30; Somers 1998).[54] Of course, many rational choice theorists are not realist at all about human rationality (their central explanans), but describe it as heuristic fiction (Friedmann 1953b). As Lawson (this volume) points out, many modern economic formulations "are couched in terms of categories that, though lying at the level of the actual, are not even real." At the same time, even these deliberately fictional constructs are connected to outcomes via general rules. If "perfect foresight is . . . widely acknowledged as a claim that is quite fictitious," for instance, it is also deployed in economic models as "a potential . . . that is *always* actualized" (my emphasis). The essence of the positivist stance, regularity determinism, is thus preserved in both the depth-realist and the willfully antirealist or idealist forms of rational choice theory (even as the latter rejects empiricism, empirical realism, and depth realism). In short, the ontological status of the concepts linked via a constant conjunction is less relevant for qualifying this approach as positivist than the supposed universality of the causal connection.

The common denominators of all positivist positions are thus *regularity determinism* and *system closure*. In light of this working definition, we can now return to the puzzle of positivism's unexpected and largely unacknowledged longevity in the social sciences. At the most general level, one might associate epistemological and ontological beliefs with structures and strategies of power, as suggested by theories about genealogies of discourse, ideology and hegemony, and the sociology of knowledge. But if claims to knowledge are also claims to power, what sort of power is this? Who is exercising it, and why? How does the specific arena of methodology and epistemology relate to knowledge more generally? Are external funding agencies able to mold scientists' basic epistemological and ontological beliefs (D. Ross 1991; Ahmad 1991; Fisher 1993)? Are the disciplines organized around a dominant epistemological *mentalité* or set of disciplining practices, a core episteme, as Foucault (1966/1970) claimed with respect to the human sciences? Mitchell (this volume) argues, for instance, that both orthodox economists and their critics agree that there really is such a thing as the economy, whose elements form a dynamic system that is separable from other systems and that could in principle be accurately represented. This suggests that disciplines can be characterized as *hegemonized*. Mihic et al. (this volume) argue the same with respect to political science. Are social scientific fields ever *paradigmatic* in the strong sense proposed by Kuhn (1962/1970, 1977)? If so, does this imply that a specific group—of scientists, class actors—controls the scientific field?

Sociological approaches suggest that the disciplines should be seen as structured *fields* within which actors internalize an epistemological or methodological habitus (Bourdieu 1981, 1984/1988, 2001). The image of a stratified, structured field implies that a multiplicity of positions, including epistemological ones, may well be thinkable and even *desirable* (because actors develop a "taste for necessity"; Bourdieu 1979/1984, 372–396), even if every location is not equally "profitable." Academic distinction or scientific capital might accrue to certain positions—epistemological, methodological, stylistic, and so on—more than others. Bourdieu also suggests that positivism is the "spontaneous epistemology" of the natural sciences, which themselves are always pressing up against the social sciences, urging them to emulate their greater success by adopting their implicit methodology (1981, 282). But a completely homogeneous scientific field is sociologically implausible, as it would not allow social actors to make claims to scientific capital or to exchange recognition of such claims with other actors. The very functioning of a social field (like science) depends on the existence of distinctions; an intellectual *Gleichschaltung* would make competition impossible. If the hierarchy of power in a scien-

tific field requires epistemological differences and is correlated with them, as Bourdieu argues, it may well contain nonpositivist as well as positivist positions. The actor-network approach developed by Latour and Callon also rejects the idea that all scientists necessarily "use the 'paradigm' in the same fashion, or that they mean the same thing when they refer to the underlying assumptions" (Yonay 1998, 23).

For each of these theorists, science is understood as an effect of power, a grid of inequality and domination, or, at the very least, as the product of strategic machinations that are driven by motives other than (or at least in addition to) discovering the truth. It is also possible, however, that positivism has been retained for reasons having little to do with scientists' quest for recognition, domination, distinction, or profits. Social-epistemic positions may represent cognitive responses to the structuring and restructuring of the social world, as suggested by neo-Marxist writers such as Jameson (1984, 1991) and Harvey (1989).[55] The determinative relationship between social structures and scientific knowledge does not entail any implications concerning the latter's cognitive adequacy or accuracy. Indeed, these writers tend to suggest that social knowledge describes social reality in oblique and distortive ways. But its content and form still do relate to social reality.[56] I refer to these positions as *social-epochal* or *macrosociological* accounts of social epistemology.

Some macrosociological accounts argue that positivism is perpetually reinforced and generated anew by the very character of the social arrangements of capitalist modernity (Horkheimer 1995). Others propose more historically specific accounts of positivist influence, linking social science epistemologies to the historical configurations of Fordism and post-Fordism (see Steinmetz 1999, forthcoming a; Steinmetz and Chae 2002). This particular social-epochal interpretation of social science epistemology originates with Marx, especially "The German Ideology." Adorno traced what he saw as the pervasiveness of positivism to the specific historical period of "administered capitalism," writing that "a social science which is both atomistic, and ascends through classification from the atoms to generalities, is the Medusan mirror to a society which is both atomized and organized according to abstract classificatory concepts, namely those of administration." For Adorno, positivism's categories were "latently the practical categories of the bourgeois class." Positivism's "elective affinity" with this class was to be found especially in the static, noncontradictory, repetitive character of modern administered capitalism. Positivism for Adorno represented an internalization of "the constraints exercised on thought by a totally socialized society," a sort of "puritanism of knowledge" (1969/1976, 56–59). Once one understands positivism as something akin to sexual repression it becomes more difficult to reduce that position to a mere veil for class interests. Indeed, Lukács (1968b) implies that bourgeois economists are themselves epistemic victims of the reified illusions of commodity capitalism which directs their attention toward empirical surfaces and away from real underlying structures.

Whatever the explanation for the current epistemological conjuncture in the human sciences—and this introduction cannot claim to legislate an answer for the diverse contributions to the present volume—it is a strange one indeed. A number of writers have referred to a positivist "haunting" of the sciences, depicting positivism's paradoxical power as a zombie-like refusal to stay buried (R. Miller 1987, 8; Elliott, this volume; Collier, this volume). In a slightly different register, positivism can be described as a kind of trauma from which the human sciences are still trying to recover. Like the macrosocial or epochal approach pioneered by Marx, Lukács, and Adorno, the sociopsychoanalytic one cannot be reduced to an account of scientists' strategically rational maneuvering or of the variable profitability of scientific positions.

The absence of any reference to positivism in the main title of this book stems partly from the conviction that positivism is too small a net in which to capture all of the problems addressed in this volume. Both the persistence and the critique of positivism can seem anachronistic in fields and disciplines for which positivism is now a strictly historical topic of analysis.[57] Some contributors to the interdisciplinary field of science studies argue that the entire category of epistemology is misleading and that efforts to distinguish it from ontology are miscast (Latour 1999).[58] Two of the essays here, for example (Lawson's and Breslau's), question whether positivism best describes mainstream economics, even if this discipline is often seen as one of the most recalcitrantly scientistic of the social disciplines.

But even if these intellectual disputes seem familiar from certain vantage points, one message of the essays in the first part of this book is that reinvigorated modes of positivism remain powerful in a wide swath of the social sciences. Indeed, positivism seems to be gaining strength in some sites. Political science, sociology, economics, psychology, and psychoanalysis all remain deeply enmeshed in a world that many in anthropology, history, and philosophy would locate firmly in their historical past. This explains how an era in which positivism is widely disparaged and barely recognizable might nonetheless give rise to a renewed *Positivismusstreit*.[59] The next section examines a series of such intellectual-political

upheavals in economics, political science, and sociology. These movements challenge what they describe as entrenched epistemic and methodological orthodoxies in their respective fields.

Social Science Wars: An Intellectual-Political Conjuncture

When a department is organized not around a discipline aimed at advancement of knowledge, but around a field associated with a social movement (as are many "women's studies" departments, or "African-American studies" departments), the criterion changes. It is no longer the criterion of how best to advance knowledge, but is instead the criterion of how best to advance the cause.—James S. Coleman, "The Power of Social Norms"

September 2000 saw the emergence of a movement based among a large number of economics students and faculty in France calling for a "post-autistic" economics and attacking the perceived hegemony of rational choice theory and econometrics within their field. Their petition demanded a "reform of the curriculum to incorporate a 'plurality of approaches adapted to the complexity of the object studied' " and decried the fact that "mathematics had become an end in itself, resulting in an 'autistic science with no relation to real life' " (Jacobsen 2001). This movement spread internationally to Britain and other parts of Europe, although the welcome has been noticeably cooler on U.S. soil. Indeed, from a certain angle, postautistic economics looks almost like an intellectual pendant to the broader antiglobalization movement and the widening sociocultural gap between Europe and the United States in many other realms. The neoclassical approaches against which the protest is directed are entrenched most powerfully in the United States and neoliberal NGOs and are understood to be helping to legitimate global inequality from which U.S.-based firms profit most. The field of economics thus sharply raises the question of the role of social movements in scientific conflict and change and points to differences in national and regional contexts (see Michael Burawoy's remarks in this volume).

The postautistic polemic also forces us to ask whether economics is indeed best characterized as *hypertheoretical* and oblivious to the empirical. Dan Breslau's contribution in this volume addresses the relations between the theoretical and the empirical in contemporary economic research via an ethnographic and textual study of the work of practicing economists. Breslau concludes that the most prestigious economists are able to take the intellectual high road of abstract theory and antirealism, but that less powerful economists tend to insist on the *moral* high ground

of empirical realism. This difference emerges sharply in economists' programmatic, methodological, and epistemological statements. In their actual research, however, elite economists are promiscuous in their combination of (antirealist) abstract theory and empirically realist data. Tony Lawson's paper supports some of the antiautistic movement's claims, arguing that mathematicization, rather than realism or explanatory capacity, is the common coin of professional economics. At the same time, Lawson, like Breslau, finds that elite economists appeal rhetorically to reality and empirical explanatory power whenever they find it convenient. At a different level, Tim Mitchell stresses the contribution of practical economic discourses to specifically *empiricist* social effects, as opposed to the hypertheoreticist effects that are implied by the language of the anti-autistic social movement. By staging an "effect of the real" in the representation of the economy, Mitchell argues, economics helps to produce the simpler, empiricist metaphysics that organizes our lives. The essays thus support the protesters' description of economics only in part, although none of them endorses the opposing position (e.g., Solow 2001) that finds nothing to criticize in contemporary economics.

In the United States, the most vigorous movement against entrenched social science orthodoxies in recent years has arisen in political science, sparked by the anonymous and eponymous "Mr. Perestroika" (see Mihic et al., this volume; also R. Smith 2002). The so-called glasnost-perestroika movement in political science called for the democratization and reform of the political scientists' professional association and flagship journal and criticized the discipline's preference for rational choice and game theory approaches, econometrics, and mathematical and statistical methods. This movement arose in the wake of concerted efforts during the 1990s to solidify the domination of the field by a basically positivist approach that could encompass both qualitative and quantitative methods (see King, Keohane, and Verba 1994). The perestroika movement has not produced a coherent epistemological alternative, however, but has instead forged a motley coalition of strange epistemic bedfellows. The movement has been directed mainly against the hegemony of a particular kind of theory (rational choice) and against a specific methodology centered on statistics and surveys. One of the most influential critiques of rational choice theory in political science, however, recommends a return to an empiricist covering law format (Green and Shapiro 1994, 31). Few of the dissidents have undermined the fundamental doctrines of fact- and value-neutrality that Mihic et al. (this volume) see as the bedrock of positivism in the field. These authors argue that the epistemological mainstreaming of political

science cannot be understood without attending to the overall intellectual configuration of the discipline, especially the definition of political theory as a particular subfield solely responsible for the values side of the "facts versus values" dichotomy.

U.S. sociology has also been riven in the recent past by debates around the methodological and sociological narrowness of its main journal, the *American Sociological Review*. An essay in the *Chronicle of Higher Education* in 1999 by the president of the American Sociological Association at that time, Joe Feagin, criticized the *ASR* and ended with a call for the profession to embrace the "self-reflexive tradition that is one of Sociology's recurring virtues" (1999, 86). Many sociologists felt that the Association ignored such calls by granting the editorship of the *ASR* to an "insider" department traditionally associated with more positivist and quantitative methods.[60] Even more unsettling for sociology's still dominant but largely unacknowledged positivism has been the emergence of epistemological challenges from a variety of corners in recent years. One of these is cultural sociology, which in some cases has rejected the scientistic insistence on treating social practices as "thinglike" and accessible to analysis without interpretation of their contextual meaning. Another challenge comes from the new sociology of science (see Shapin 1995 for a review). Given its commitment to the idea that scientific practice and theory choice is driven by more than simple correspondence with data or the truth, and its occasional claim that "there is no transcendent context of rational justification that renders some scientific hypotheses more credible than others" (Luke 1999, 345), recent work in the sociology of science embodies a frontal disagreement with the prevailing positivism. This subfield tends to reject the entire distinction between subject/scientist and object, and thus rejects not just positivism but the various critical realisms as well.[61] Not surprisingly, the sociology of science has become a flashpoint for opponents of Novick's "epistemological left" in the so-called science wars (e.g., Sokal and Bricmont 1998, ch. 5), although some view the Sokal affair as an aberration (e.g., Schabas 2002, 219). Other criticisms of positivism are associated with feminist and other new social movements and with the poststructuralist and postmodernist theories that have sometimes been linked to these movements. Methodological conservatives in sociology have regarded the emerging possibility of epistemological polycentrism as a threat to the field's always tenuous scientific status and have been especially vexed by the penetration of the field by social movements (see the epigraph to this section and Horowitz 1993, ch. 1).

Countering this epistemological blowback, the current president of the American Sociological Association, Michael Burawoy, has intervened with

a series of analyses of the connections between sociology and its publics (see Burawoy 2004 and his conclusion to this volume). These articles challenge the effort to (re)erect a firewall between sociology and its constitutive social "outside," which Burawoy sees as fully a part of its "inside." Positivism, according to Burawoy, is above all the "self-misunderstanding" of the "professional" sector of social science knowledge; it is the idea that "knowledge is and has to be autonomously produced" in "an autonomy without embeddedness." A vibrant sociology, according to Burawoy, "depends on reciprocal relations between professional sociology and both the self-reflexive critical branches of the field and the discipline's extra-academic audiences" (this volume).

This is by no means the first wave of epistemological upheaval in sociology's relatively short history. There have been at least three periods in which positivism became an explicit target of critique: around the turn of the twentieth century, in response to the doctrines of Mach, Pearson, Poincaré, and other influential positivists; during the 1930s, when logical positivism was criticized by such writers as Parsons (1937/1949) and Sellars (1939); and again in the 1960s and 1970s, which saw antipositivist movements that were closely tied to other cultural and political rebellions (Adorno et al. 1976; Gouldner 1970; Schwendinger and Schwendinger 1974; Giddens 1975).[62] The recurrence of these disturbances underscores the fact that they have failed to dislodge positivism permanently. In the present period there is more confusion and disagreement than in the past. Yet it is not at all evident that the current challenges will succeed in shifting the discipline's ontological-epistemethodological center of gravity. As Craig Calhoun (1996) has argued with respect to the initially antipositivist historical turn in sociology, the most dissonant and threatening aspects of such movements can be successfully "domesticated." My own paper in this collection examines a subset of these movements and finds that they have indeed been reconciled with sociological positivism to a surprising degree.

The configurations of dispute in economics, political science, and sociology are less alike than they might at first appear. The antiautistic movement in economics is directed in part against the dominance of abstract theory and mathematicized methods. In sociology, by contrast, it is abstract theory that has been especially taboo, due to the empiricist proscription on theoretical (or "unobservable") structures. And in a gesture reminiscent of sociological positivism, political scientist Charles Lindblom (1997, 243) refers to Joseph Schumpeter (!) as "more . . . a debater than . . . a scientist." Of course, theory has not so much been proscribed in political science as set off behind an intellectual *cordon*

sanitaire, according to Hauptmann and Mihic et al. And yet, neoclassical economics, rational choice theory, and game theory emphatically claim the right to make abstract assumptions and to posit the existence of theoretical objects, whether realist or antirealist (Somers, this volume).

We might anticipate similar expressions of epistemic discontent in the fields of psychology and psychoanalysis. After all, U.S. psychology was dominated throughout much of the twentieth century by one of the most extreme versions of an empiricist positivism: the behaviorist schools of John Watson and B. F. Skinner. As one recent collection of essays on psychology points out, however, there has been little consensus about the nature of positivism in the field or the character of its influence (Tolman 1992). Another psychologist notes that although there is "still argument over the precise extent to which psychology ever adopted or paid attention to the logical positivists," it is "less disputable . . . that positivist notions already prevalent in the late 19th century came to inform psychological research for much of [the 20th] century" (Stam 1992, 259). Despite the strong impact of positivism on psychology, or perhaps because of it, that field has not seen any explicit movements of epistemological dissatisfaction. In more recent decades, many psychologists have become interested in opening up the behaviorists' "black box" and started to introduce realist concepts and objects such as cognition, narrative, and the self that were declared off-limits by behaviorism.[63] But the furious campaign by pharmaceutical companies for the full-scale medicalization of psychopathology continues to reinforce the spontaneous ideologies of the self that are already generated by a neoliberal capitalism "unfettered" by solidaristic welfare policies and dedicated to the hyperindividualism of the market.[64] Just as social movements do not necessarily arise where grievances are most severe (Zald and McCarthy 1979), movements of intellectual renewal do not always emerge where they would seem to be most urgently needed.

As noted earlier, U.S. psychoanalysis aligned itself with a scientistic and medicalized approach that led it away from the theory's most significant depth-realist constructs, including the unconscious. Dissidents like Norman O. Brown, whose *Life against Death* (1959/1985) was published in the same year as C. Wright Mills's *Sociological Imagination* (1959), attempted to stave off the ongoing scientization of the field. But as Jacoby (1983, 135) argues, such isolated attempts to defend Freud against Freudianism failed to disturb the "theoretical sleep" of psychoanalysis. The 1960s saw a renewal of nonpositivist protests in psychoanalysis from figures like Marcuse (1964; see Zaretsky 2000). This era also saw the consolidation of the antiscientistic French school of psychoanalysis led by Lacan (Roudinesco 1990; Foucault 1994, 204). By the beginning of the 1980s, Jacoby (1983, 136) could sense that "the antipsychoanalytic wind [had] weakened." The intervening period has seen continuing opposition to psychoanalysis in the culture at large and in the fields of the psychic more narrowly, however. The proliferation of psychoanalytic theorizing in the humanities should not be seen as a sign of the theory's weakness, *pace* Jacoby, but neither does it gainsay the theory's institutional weakness.

The fields with perhaps the most corrosive effects on positivism are science studies and the new sociology and historiography of science. The very existence of the Strong Program in science studies represents a sort of historical paradox, of course. On the one hand, science and technology have become increasingly interwoven in all aspects of social life (Castells 1996/2000a; Mirowski 2002), and science's claims to a superordinate position in social, cultural, and philosophical debate have been raised ever more insistently as a result. At the same time, however, the intellectual orientation that Karl Mannheim (1929) called "unmasking" (*enthüllen*), the claim that one can unveil the social determinants of knowledge, has become more widespread. This debunking posture clashes with science's overweening claims to authority to constitute the unsettled and unsettling conjuncture of current debates around (social) scientific epistemology.[65] It is impossible to predict whether science studies will wither under the onslaught of big science; alternatively, the skepticism about science that feeds this field may continue to grow, or the two may remain at loggerheads.

The simultaneous focus on epistemological issues in scientific fields and in some of the antiorthodox social movements distinguishes the present moment from earlier waves of dissatisfaction with social science positivism. Earlier critiques of positivism were usually a subordinate part of wider political programs; one thinks of Lukács's *History and Class Consciousness* (1922/1968a) or Lenin's critique of Mach and the Machists (1908/1927).[66] Parsons may have argued against positivism in the 1930s, but this was by no means his main focus, and he seems to have abandoned the theme after the war. The polemic against Popperian positivism was only one of Adorno's many interests in the 1960s. By contrast, questions of science, epistemology, methodology, and technology have moved in recent years to the center of critical attention.

The Epistemological Unconscious: Politics of Method in the Human Sciences

> The overwhelming majority of philosophers regard as mental only the phenomena of consciousness. . . . What, then, can a philosopher say to a theory which, like psycho-analysis, asserts that on the contrary what is mental is itself *unconscious* . . . ?
>
> The strongest resistances . . . were not of an intellectual kind but arose from emotional sources. This explained their passionate character.—Freud, "Resistances to Psycho-Analysis"

The notion of an "epistemological unconscious" in the human sciences might seem jarring in light of the conventional associations of epistemology with explicit, conscious processes of generating and adjudicating knowledge claims.[67] Yet, as Tony Lawson reminds us, Alfred North Whitehead (1925, 71, quoted in Lawson, this volume; my emphasis) observed in 1925 that in every period "there will be some fundamental assumptions which adherents of all the variant systems within the epoch *unconsciously* presuppose." Pierre Bourdieu (1972/1977, 23, 92), a theorist who avoided explicit psychoanalytic theorizing and criticized certain versions of it, often used the adjective "unconscious" in describing the operation of the habitus, and this certainly extended to his view of the scientific habitus (1990, 55–56; 2001).[68]

We can define the unconscious as a deep structure encompassing processes and forms of knowledge that are not accessible to conscious awareness but that are nonetheless capable of patterning conscious thought and manifest practice. The concept then resonates with many alternative terms used by the authors in this volume to describe epistemic convergence in science: ideology, doxa, hegemony, disciplining, tacit understandings, knowledge postulates, and more. The concept of the unconscious in this context suggests that fundamental intellectual alignments within disciplines are partly generated and reproduced without the explicit agreement of the participants—violating the liberal self-understanding of science. Positivism may also offer more specific attractions. The positivist world of repeated conjunctions of events is, among other things, reassuringly stable. The psychoanalytic concept of the unconscious is necessary for analyzing the desire for knowledge: Freud's "drive for knowledge" (*Wisstrieb*) or "epistemophilia" (Moi 1988/1999). It may also shed light on some of the ways a discipline-specific habitus is inculcated through processes of identification with ideal egos and ego ideals (Žižek 1989), including identification with imagos of heroic figures (and impossible antiheroes) in one's own discipline and beyond.

The idea of a "politics of method" has several implications. As William Baxter (2002, 42–43) writes:

> Method is originally from Greek *méthodos*, a compound of *metá* "after" and *hodós* "way, road." Classical Greek *méthodos* originally means "following after, pursuit," hence (in philosophical contexts) "pursuit of knowledge, investigation"; by further extension, it refers to a plan or strategy for carrying out an investigation. . . . But for many nineteenth-century writers . . . *method* or *méthode* has a stronger and more specific sense which is probably to be traced to René Descartes. . . . The fourth of his *Regulae* is . . . "A method is necessary for investigating truth. . . . By method I mean certain and easy rules, *such that those who use them precisely will never suppose anything to be true which is false.*"

Method in this sense is a central site for the reinforcement of positivist hegemony in the social sciences. Tony Lawson argues, for instance, that the axiomatic privileging of mathematics in economics is inculcated in economists during their earliest training, coming *prior* to the substantive epistemology and ontology it supports.[69] Positivist assumptions are typically communicated to sociology graduate students not by reading Comte, Carnap, or Nagel but through practical training, *dressage*, in statistics classes.

Method in the narrower sense is relatively independent of epistemology, as illustrated by the elaboration of forthrightly positivist qualitative methods for sociology or political science (e.g., Esping-Andersen 1990; Mahoney and Rueschemeyer 2002). Epistemological-ontological positions and methodologies are typically "mutually implicating" through patterns of elective affinity (Schwarz-Shea and Yanow 2002, 460). One of the aims of this volume is to chart the differing articulations between methods in this sense—a focus on issues of technique or technology, writing and research, rhetoric, and disciplinary socialization—and broader assumptions about social knowledge and the social world (see also J. Nelson, Megill, and McCloskey 1987).

Another common thread in these essays is the *political* character of epistemology. Fredric Jameson (1981) famously analyzed the *political* unconscious of fictional texts. By the same token, the human sciences and their texts have tacit and explicit political (and antipolitical or depoliticizing) messages. Examples of the former include the separation of values from facts and the assumption of repeated conjunctions. Like the cyclical worldviews discussed by Bourdieu (1972/1977), positivist regularity determinism suggests that the modern world is itself unchanging, or changing only in predictable and predetermined ways. This is not only profoundly tranquilizing but also, for a modernist philosophical position, paradoxically "traditionalist."

All of the contributors to this volume have studied questions of sociocultural practice and change in different disciplinary, transdisciplinary, historical, and geospatial settings. They have analyzed academic or scientific knowledge and less elite knowledge cultures and discursive communities.[70] Some of the authors approach positivism and antipositivism via close readings of influential texts in their respective disciplines; others engage in ethnographies of the present-day human sciences; a third group focuses on elaborating alternatives to positivism. And because it is impossible to understand contemporary positivism and nonpositivism outside of particular histories and places, many of the essays approach.

One of the questions that immediately arose in organizing this volume was whether to privilege practioners' histories or "self-histories" (Mirowski 2002, 382–383) as opposed to accounts written by outsiders to the various discplines. Discussions of standpoint theory and scientific self-reflexivity have made it clear that outsiders do not necessarily have a *more* objective vision of their object; but neither do they always have a *worse* perspective. *Participants* in particular social practices have better "opportunities for knowledge" (New 1998); for the study of science this includes personal, specialized, and embodied training. There are also practical advantages to locating criticism of the social sciences *within* the specific sciences (see Burawoy, this volume). Students are likely to be exposed to their own discipline's orthodoxies, internal conflicts, and hegemonic imagined histories as part of their professional training. Yet the field's power structure may also subject them to systematic blindness, self-censorship, and pressures to intellectual conformity. There is no obvious progression in the history of science in which fields are first studied by insiders, followed by "maturation" and movement into the "autonomy" of professional history and sociology of those same sciences (*pace* Schabas 2002). Such autonomization may simply generate new forms of intradisciplinary heteronomy, as science studies generates its own internal orthodoxies.[71] The essays herein are mainly by practitioners of their own fields. This has the additional advantage of refuting the slur that "those who can, do science; those who can't prattle about its methodology" (Samuelson 1992, 240), for all of the contributors to this volume are conducting "scientific" work on topics other than their own disciplines as well. One of the disadvantages of self-histories, however, is that researchers on the various fields remain separated from one another. This volume attempts to overcome this problem.

Although focused on the United States, this volume also provides a starting point for thinking about the differences and relations between

social science in the United States and the rest of the world. Whether the interdisciplinary and chronological patterns that emerge here would hold in other national contexts remains an open question. Two relevant points are worth recording, however. On the one hand, a focus on the United States is crucial to the extent that contemporary scientific life worldwide continues to be dominated by U.S. universities and research institutes, for better or worse. It makes sense, then, to turn our attention first to the social scientific metropole before exploring the "reverberations of empire" (Stoler and Cooper 1997, 1). On the other, it would be worth exploring the hypothesis advanced in a different context by Fredric Jameson (1998b) concerning a growing divide between U.S. and European political culture and to direct this toward the problem of social scientific cultures.[72] Anyone familiar with, say, German or French social sciences will immediately be able to think of a series of differences from those fields in the United States. In psychoanalysis, the scientizing and antiscientistic tendencies developed at different moments and in different forms on the two sides of the Atlantic (Zaretsky 2004). Geoff Eley's essay in this volume suggests that British social history followed a political-epistemic trajectory that was quite different from its U.S. counterpart. Michael Burawoy discusses the "global division of social science labor" and the split between "opposition and attachment to" U.S. and "Western" forms of professional knowledge. Dutton's "Asian Area Studies" has a global genealogy and reach.

This collection is meant to stimulate comparisons over time and across disciplines and epistemologies. Because most of the essays develop more than a single argument—historical, descriptive, critical, programmatic—there was more than a single possible way to arrange them in the book. All of the essays engage in historical genealogies and criticize contemporary social scientific practice. All make recommendations for a non- or post-positivist social science. Even where alternatives to positivism are not the explicit topic they are performed or exemplified by these articles. Overall, the arrangement of the volume is a trajectory running from the past through the present and on to different possible futures. Without ignoring the "collapse of the fact/value dichotomy" (H. Putnam 2002), it is possible to say that this book is structured by an overarching movement from "is" (or "was") to "ought."

Notes

For their detailed comments on this introduction I would like to thank Webb Keane, Peggy Somers, four anonymous reviewers, and the indefatigable Raphael Allen.

1 I am using the term epistemology in the general sense of theories of knowledge of the world, not in the historically narrower sense of the sterile "quest for certainty" (Rorty 1979, 61), or for absolute truth based on "internal representations and the correct evaluations of those representations" (Rabinow 1986, 234), under this more capacious definition. Pragmatic postmodernism is (among other things) an epistemological position, as it makes claims about knowledge: knowledge has no stable foundations, but should be useful or edifying, and so on. As I am using the term methodology, it is also very broad, designating research design, case selection, techniques of analysis, and forms of presentation.

2 Research on this period is referred to in the rest of this introduction and in the other essays here. To cite just two recent historical accounts of scientific change focused on the middle of the twentieth century and emphasizing the effects of military funding and research priorities, see Mirowski (2002) and Robin (2001).

3 As Gunnell (1995, 930) points out, however, contemporary realism and antirealism both "abjure the legacy of positivism," even if adherents to the two positions often accuse the other camp of slipping back into positivism.

4 Of course, epistemological pluralism may end up reproducing "the underlying structures that sustain a uniform core" (Mihic et al., this volume), just as multicultural liberalism is delimited by a set of examined and unexamined strictures that determine which practices are thought to lie beyond the pale (see Povinelli 2002; Rasch 2003). Bourdieu (1981, 2001) recognized that the existence of a plurality of positions does not eliminate unequal hierarchies in fields of knowledge, suggesting instead that such pluralism is a condition of existence for any field.

5 Eley and Elliott focus on European texts; Collier's critical realist position is based mainly in Britain; Breslau's post-Mertonian sociology of science draws on European social theorists like Bourdieu and Latour; Dutton's analysis of Asian area studies spand the globe.

6 By depth realist I mean the same thing Gunnell (1995) means by theoretical realism, namely, the willingness to maintain theoretical claims about objects, structures, or mechanisms that cannot be reduced to observables (or even to potential observables; see R. Miller 1987).

7 See Ortner (1994) for a relatively approving summary along these lines and Wolf (1980) for a critique; Cohn (1981, 244) speaks of a "deconstruction" of the field. George Stocking (2001b) refers often to "sociocultural anthropology" to underscore the similarity between the United States, where the adjective "cultural" was used to refer to the dominant sector of the Boasian four-field definition of the discipline, and Britain, where the adjective "social" was used. Stocking's emphasis on common features of British and U.S. anthropology is generated out of a broad contrast with the French and German formations; he also attends to key figures like Radcliffe-Brown, who operated in more than one Anglophone setting. Because the present discussion is focused on U.S. developments, I use the term "cultural anthropology."

8 Although this article was written in English, Boas clearly had in mind the method of *Verstehen*, a term commonly translated as "understanding." Boas's essay is ostensibly about geography, but the term is used in the sense of the German geographical tradition associated with Karl (Carl) Ritter and Friedrich Ratzel. Ratzel was the author of a highly influential two-volume book called *Anthropogeographie* which argued against evolutionist theory and in favor of a diffusionist approach to cultural change. The topic of his "geography" was what in English was called ethnology (also called *Ethnologie* in German). Boas's later writings on ethnology shift to the more Anglophone usage of the term but continue to argue along the same epistemical lines as the 1887 essay.

9 Alongside Boas himself at Columbia, examples include Alfred Kroeber at Berkeley, who in 1915 was already strongly antibiologistic, arguing that "civilization" (later called "culture") "springs from the organic, but is independent of it," and opposed to geographic-materialist reductionism, insisting that "civilization reacts to civilization, not to geography" (1915, 284–285). Kroeber, like Boas, rejected all forms of social evolutionary theory, which had dominated anthropology during the nineteenth century and which resurfaced repeatedly in the twentieth (in forms like modernization theory), asserting as a premise that "all men are totally civilized" and that "the ranging of the portions of civilization in any sequence [is] always valueless" (286). Other influential Boasians included Robert Lowie (at Berkeley), Melville Herskovits (Northwestern), Edward Sapir (mainly Chicago and Yale), Ruth Benedict (Columbia), and Margaret Mead, who taught at a number of universities and was based at the American Museum of Natural History (Stocking 2001c, 45).

10 I disagree with Stocking's tendency to describe all comparative research as "scientistic" (e.g., Stocking 2001b, 46, 289). This seems to ignore the possibility of nonpositivist versions of comparison (see Steinmetz forthcoming b; Lawson 1999b).

11 This is based on personal communications from Webb Keane and Ann Stoler. The methodological trend toward *multisited ethnography* (Marcus 1995; Appadurai 1990) does not contradict this claim, because comparison here is interested in tracing a single cultural formation that is produced "in several locales" and "across and within multiple sites of activity" in ways that destabilize the distinction between the "local" and the "global" (Marcus 1995, 99, 96). Although "conventional controlled comparison in anthropology is indeed multi-sited . . . it operates on a linear spatial plane . . . comparisons are generated for homogeneously conceived units" (102). I would characterize this as a subtype of "depth-realist comparison" (Steinmetz forthcoming b) because, as Marcus says, there is still a common object of study (a person, community, etc.) across the multiple sites.

12 On Boas's deep antipathy to biological determinism and the overall "debiologization" of anthropology, see Stocking (2001b, 67, 315; 1968). On the recent debate, see Chagnon (1968), Tierney (2000), Geertz (2001), American Anthropological Association (2002), and the reviews in *Current Anthropology* 42, 2 (April 2001), especially Coronil (2001). See also Latour (1999), who focuses on soil scientists in the part of the Amazon inhabited, ironically, by the Yanomami. Latour distinguishes between his own "philosophical" analysis of these scientists and a "sociological" focus on colonialism, race, and gender.

13 In a response to critics, Novick (1991) denied that he was talking about history's *epistemological* stance, but most critics recognized that this was in fact precisely the topic of his book (see the reviews by Hollinger, Megill, and Ross in *American Historical Review* 1991). Novick was awarded the American Historical Association's most distinguished prize, yet, as Dorothy Ross (1991, 706) pointed out, his book "was not awarded the prize of the Social Science History Association." The implication is that at least one section of the historical discipline, the one most strongly associated with quantitative approaches to history, was less approving of Novick's analysis. Such disagreements can be examined in the reviews by Kloppenberg (1989) and Hexter (1991). Kloppenberg accuses Novick of

philosophical vagueness, but this seems to mask a more fundamental disagreement with his insistence on the relative autonomy of theory from empirical history. On the internal divisions among historians, see Abbott (2001a, especially ch. 4).

14 This is perhaps not so paradoxical, of course, if one recalls the leftism of the Russian Machists and Carnap's advocacy of socialism, not to mention the scientistic left's attack on postpositivism (e.g., Sokal and Bricmont 1998).

15 But see Eley (1996). There were many examples of nonsynchronicity, however, such as Baldwin's (1990), a historical study that seems to emulate mainstream sociology even as it was published a good decade after historians' cultural turn.

16 The linguistic turn in history or the new cultural history also occasioned, as Sewell (this volume) notes, a shift away from some of the more populist political themes of social history and a move back to a more elite-centered topic. For evidence that these methodological and political developments are not inextricably and necessarily bound together, however, one need look no further than Sewell's own *Work and Revolution in France* (1980), not to mention the work of Patrick Joyce, Craig Calhoun, Jacques Rancière, Bill Reddy, and others.

17 For a recent attempt to declare interpretation anathema to properly scientific comparative-historical research, see Mahoney and Rueschemeyer (2002).

18 Thompson (1978), of course, confusingly described Althussser's radically antirealist, antiempiricist, and antipositivist philosophy of science and social theory as positivist, muddying the waters considerably on this issue (see P. Anderson 1980).

19 Of course, some historians explicitly engage with the philosophical understanding of positivism; see Lloyd (1986), who discusses positivism in terms identical to those in the next section of this introduction.

20 Needless to say, this version of so-called positivism is superempiricist; see the next section for the distinction between positivism and empiricism.

21 See the special issue of the *Journal of the History of the Behavioral Sciences* 36, 4 (fall 2000).

22 We can pass over nonpsychoanalytic psychiatry here; psychiatry's positivist fixation on reductive and predictive generalization should surprise no one; see Scull (1999) for a review of recent historiography. I also ignore psychoanalysis's antithesis from the 1960s, antipsychiatry; see Will (1984) on that movement's relationship to positivism.

23 Norman O. Brown (1959/1985, 94) insisted that psychoanalysis claims to "break through phenomena to the hidden 'noumenal' reality."

24 The first landmark of positivist criticism of Freud was Popper's rejection in 1919 (see Popper 1983, 174). For a recent overview of the criticisms of psychoanalysis, see Forrester (1997); Grünbaum (1984) provides one of the most thoroughgoing nonpositivist critiques; see Frosh (1999) for a response to Grünbaum. Popper revised classical logical positivism, but his falsification doctrine was empiricist and positivist in its denial of a "vertical" stratification of real structures (the latter means that a causal structure such as the unconscious in Freudian theory may be prevented from being expressed at the level of the actual). Freud's central argument is that unconscious materials are *not* always or continuously expressed the same way at the empirical or symptomatic level. Any simple version of falsificationism is therefore inadequate to the ontological complexity of the world. Popper's theory is indeed *antiempiricist* in its embrace of conceptual causal structures; he insisted that his theory was a form of realism. But this tended to reduce to a form of *depth-realist positivism* due to its retention of the "constant conjunction" model (see below on the notion of depth-realist positivism, and Popper 1957/1991, 141–142 for an example of his acceptance of a deliberately unrealistic rational choice model of human practice). In 1957 he listed a number of "sociological laws or hypotheses," all of which took the form of constant conjunctions, and later insisted that even in the social sciences, "the basic logical schema of every explanation consists of a (logical) deductive inference whose premisses consist of a theory and some initial conditions" (1957/1991, 62–63; 1969/1976a, 100). He also defended experimentation and prediction as goals for the social sciences and opposed the "belief that the description of a social situation should involve the mental . . . states of everybody concerned (1957/1991, 94–95, 120–130, 140).

25 See Rustin (1991, 115–144; 1999) and Collier (1994), who discuss psychoanalysis as a critical realist form of science; Frosh (1999) draws out its distinctively antipositivist epistemology from a different point of view. Freud stressed individual variations in responses to general conditions as much as the general situations they face. As a result, despite modal conditions like the Oedipus conflict, it is impossible to generate general narratives of individual development.

26 In light of Jacoby's (1983, 15, 139) critique of theoretical banalization, it is ironic that he is so intolerant of newer nonpositivist directions in psychoanalytic theorizing. Jacoby fails to notice, or cannot abide the fact, that much of the creative political and theoretical energy he so misses in current medical psychoanalysis (141) has migrated to the humanities.

27 Lawson (this volume) suggests that the term "deductivism" may be a more appropriate description of mainstream economics than positivism, which he equates with empiricism. But he admits that mainstream economics does "appear to resonate" with positivism "in many ways." The terminological disagreement seems less important than Lawson's points about the centrality of mathematicization and regularity determinism in economics.

28 The reasons for economists' unwavering commitment to mathematicization are not developed here, but see Mirowski (1989) for one account of the influence of physics on economics and the resultant prioritization of mathematics. It is also important that many of the objects of economics (money, interest rates) themselves take an ontologically quantitative form, which lends greater credibility to the privileging of mathematical methods. Other sorts of social practice, from sex to speaking and writing a language, do not necessarily present themselves at the level of the "actual" in an already quantified form.

29 As Robin (2001) points out, Laswell propagated a mechanistic version of psychoanalysis during the prewar period, but by 1950, economics had replaced psychoanalysis as his ideal science (see also Gunnell 1993, 226).

30 Although Schwarz-Shea and Yanow (2002) never actually define positivism here, they seem to mean something quite similar to the formation I define in the next section.

31 See Emily Hauptmann's essay in this volume for a discussion of this rise to preeminence and of the question of its status as theory or method.

32 On positivism in the contemporary subfield of international relations, see Ashley (1984, 248–254); S. Smith, Booth, and Zalewski (1996). Positivism in the subfield of comparative politics is exemplified by Mahoney and Rueschemeyer (2002).

33 The most influential modern statement of the fact/value dichotomy is Hume's thesis that the "propositions, *is*, and *is not*" should never be "connected with an *ought*, or

ought not" (*A Treatise of Human Nature*, 1739–1740/1969 bk. 3, pt. 1, sect. 1). The argument *against* the fact/value dichotomy can be traced back to Aristotle. Discussing the classic Aristotelian "three intellectual virtues, *episteme, techne,* and *phronesis*" (the latter translated as "practical wisdom"), Flyvberg notes that "whereas *episteme* is found in the modern words 'epistemology' and 'epistemic,' and *techne* in 'technology' and 'technical,' it is indicative of the degree to which thinking in the social sciences has allowed itself to be colonized by natural and technical science that we today do not even have a word for the one intellectual virtue, *phronesis*, which Aristotle saw not only as the necessary basis for social and political inquiry, but as the most important of the intellectual virtues" (2001, 3; Aristotle, *Nicomachean Ethics*, 1140a, 24–30). Criticism of the fact/value dichotomy was developed further by Marx starting in the *Theses on Feuerbach* and by later Marxists (e.g., Lukács 1968b, 153–157); by the pragmatists Peirce, Dewey, James, and Mead (H. Putnam 2002, 30); by Leo Strauss (1962) and his followers; and by Adorno (1969/1976), Charles Taylor (1979, 1985b), and Roy Bhaskar (1979). For a recent discussion, see H. Putnam (2002); for an account of the necessary normativity of the social sciences, see Freitag (2001).

34 See, for example, Liu (1999). Liu (forthcoming) connects the desire for an unambiguous translatability with imperialism.

35 It is more than ironic in light of this adherence to methodological individualism that Kitcher (2000, S38) passes judgment on sociology, a discipline defined by its rejection of methodological individualism and its insistence on the ontological emergence of the social. For a sustained critique, see Mirowski (1996).

36 On Harding, see Pohlhaus (2002) and Lawson (1999b). Presumably, Mirowski might reject critical realism for retaining a neo-Reichenbachian distinction between what it calls "epistemic relativism" (which it accepts) and "judgmental realism" (which it rejects). The latter is not antisociological across the board, however, but only with respect to its claim that truth is "a value that is presupposed by all our doings as cognitive beings" (Collier 1994, 179). In effect, judgmental realism is a normative program but not one that claims to explain anything about the actually existing sciences. One also needs to ask whether it is not misleading to equate the critical realist critic of science with the hegemonic logical positivist. Critical realism does not want to restrict science to "tutored" legitimate agents or even to replace untutored with tutored ones, but to criticize the social conditions that produce systematically distorted knowledge. Thus, the parallel between its judgmental rationalism and Reichenbach's "context of justification" is only partial, stemming from a common ethical commitment to the idea of truth.

37 Even if most U.S. sociology departments have social theory courses and some have full-time theorists, and even if the American Sociological Association has a subsection devoted to theory, there is almost no systematic training of graduate students for this subfield and there are almost never entry-level jobs in social theory. The rare PhD in sociology with a theory dissertation often ends up working in another field. Existing theorists in U.S. sociology departments generally began their careers as empirical reseachers, or continue to combine theoretical and empirical work, or have been trained in other countries. The exceptions prove the rule.

38 I discuss these intellectual movements in my contribution to this volume; see Calhoun (1996) and the essays in Adams, Clemens, and Orloff (forthcoming) for detailed discussion of one of these challenges and of historical sociology.

39 The authors of this influential introduction to statistical methods single out economics as the only social science in which genuine deductivism is accepted (Hanushek and Jackson 1977, 1).

40 Adorno's criticique of positivism is dicussed below.

41 See Neurath (1931/1973, 356–371) for an early example of an explicit logical positivist Marxism, written long before "rational choice Marxism." On Neurath's views of the "social extraterritoriality of science," see Uebel (2000b).

42 I am grateful to Raphael Allen for helping me with this formulation.

43 See Jonathan Turner (1993) and Gerhard Lenski (1988) for defenses in sociology; for discussion, see J. Bryant (1992) and Despy-Meyer and Devriese (1999, 95–143). E. O. Wilson's (1999) treatise on the "unity of science" directly reprises the logical positivists' earlier slogan (see Carnap 1930–31/1959, 1934).

44 It seems to be precisely this unfashionableness that explains the periodic appearance of soi-disant positivists in sociology (J. Turner 1993), often among those whose positions are not truly positivist in any recognizable sense (e.g., R. Collins 1989; Stinchcombe 2002).

45 Private communication by Daniel Little, November 2003. See Kitcher (1993, 2002).

46 As Carnap (1963b, 870) observed, Schlick originally was a realist but abandoned realism as a result of "discussions in the [Vienna] Circle."

47 See, for instance, Green and Shapiro, who "insist that scientific advance comes only with developing theory—*that is, establishing the existence of covering laws*" (1994, 31, my emphasis). They add that these covering laws have to be "both general and empirical," thus seeming to slip back into a simple Humeanism. Shapiro (1990) gestured in an earlier book toward a "pragmatic realism" that transcended surface empiricism.

48 I call this depth-realist, rather than simply realist, in recognition of the fact that many empiricist positivists were in fact realist about the intransitive existence of the objects of science, contra Marxists like Lenin, who collapsed positivism and empiricism with idealism. But the original logical positivists were quite adamant about their opposition to the "metaphysics" of contemporary "critical realism" (which at the time was associated with Roy Wood Sellars), writing, "*For us, something is 'real' through being incorporated into the total structure of experience*" (i.e., empirical experience; see Der Wiener Kreis 1929/1973, 308; italics in original).

49 Thus, the discussion of induction versus deduction fails to get at the essential issue, which is a view of the (social) world as static and monologic (Adorno et al. 1976, 76).

50 This was true of all the logical positivists, many of whom also called themselves logical *empiricists* (H. Putnam 2002, ch. 1). Bhaskar (1975/1997, 1979, 1986) and Collier (1994, this volume) also distinguish positivism from empiricism. Sandra Harding (1999) points out that although the two positions have been closely interwoven in the course of philosophical history, they have also periodically diverged.

51 Sociologists also often equate positivism with Comte's positivism, which is understandable given his role in the history of the discipline; see C. Bryant (1975) on this conflation; Gouldner (1970) for an example of the elision; and Steinmetz and Chae (2002) for a critique of the latter.

52 Various unrealistic assumptions are also made about the distribution of random qualities or error terms.

53 This is why Althusser referred to Hegelian Marxism as essentialist: it reduced all phe-

nomena to epiphenomena of a common essence. See Althusser (1977), Althusser and Balibar (1968/1979).

54 Gunnell (1995) and Somers (1998) refer to this combination as "theoretical realism," which resonates with the logical positivists' notion of "theoretical terms." I prefer the term depth-realist positivism because positivists often refer to statements of constant conjunctions of empirical events as "theories."

55 This sort of explanation recalls the position Popper (1957/1991, 7) called "historism" (as opposed to "historicism"), one that explains "the differences between the various sociological doctrines and schools, by referring . . . to their connection with the predilections . . . prevailing in a particular historical period."

56 The actor-network approach argues that "facts" may be mobilized as "allies" by scientists engaged in "trials of strength." The argument I am making is that even social facts that are not mobilized by any particular group of scientists (as allies) can influence spontaneous social epistemologies.

57 But see Poovey (2001), who identifies an orientation toward the use of models in literary criticism that one might call positivist (although she does not use that word).

58 Latour (1999) argues that this distinction is an effort to ward off "mob rule," recalling Lakatos's (1970) critique of Kuhn. Some of the contributors to this volume deploy the epistemology-ontology distinction, but their critiques tend to be closer politically to what Latour would consider the "mob" position than that of the rulers (see especially the contributions by Sewell and Eley). Clearly, this is a fluid distinction, but without it we would not be able to track the emergence of phenomena such as the new depth-realist variants of positivism. As H. Putnam (2002) argues with respect to facts and values, distinctions are often less objectionable than dichotomies.

59 We should recall that some of the main participants in the 1960s positivism debate, including Popper, also refused to recognize themselves in that mirror (see the essays in Adorno et al. 1976).

60 Namely, the University of Wisconsin sociology department, which, as Sewell notes in his paper here, has long been the most "powerful and notoriously positivist" department and which is also regularly ranked at the very top of graduate sociology departments in the United States. Even though the journal's recent editor, Charles Camic, hardly represented the positivist mainstream of sociology, little seemed to change with the *ASR* during his stewardship, suggesting that the discipline (rather than the demonized Wisconsin department), was the source of the problem.

61 See Latour (1999), who argues implausibly that "we in science studies may be *the first to have found a way to free the sciences from politics*" (22). He offers a compelling critique of the thesis of the unbridgeable gulf between scientific subject and observed object, a criticism shared by many modern philosophies of science—though certainly not by standpoint theory (Gadamer 1975) or critical realism. Latour argues convincingly that facts are "clearly fabricated" by scientists, nonscientists, and nonhumans (15), but he fails to address the critical realist distinction between the ontological levels of the *real*—of structures that are indeed largely "intransitive" and thus independent of scientists—and the *actual* (events). Bhaskar (1975/1997) would agree that the latter, in scientific settings, are "clearly fabricated" by scientists. Indeed, he argues, the logic of the experiment is that it is a highly artificial construct intended to produce specific effects. Critical realism asks what *intransitive* properties of the world allow humans to fabricate certain facts but not others in the laboratory, a question that Latour's approach cannot

ask. Latour does claim to be moving toward a "more 'realistic realism'" (1999, 14). Science studies is certainly a form of realism, but it is generally an *empirical* realism, and not one that allows for an ontological stratification into the levels of the real and the actual (see the comments of Yonay 1998, 15, who even claims the mantle of positivism, though not a "traditional" version, for his constructionism). As Latour notes, what science studies does best is to pay "close attention to the details of scientific practice" (1999, 24). Latour's attraction to photography (which he introduces by writing somewhat naïvely that "a picture is worth a thousand words") seems to be motivated by photography's empirical-realist power, as he presents photographs as unvarnished records of "the facts" (at least in his 1999 book).

62 A lone voice in the 1950s was C. Wright Mills, whose *Sociological Imagination* (1959) resembled the arguments of the contemporary antiautistic economics movement in opposing both abstracted empiricism *and* grand theory.

63 For an excellent overview of the literature on narrative and the self, see Linde (1993); see also Steinmetz (1992).

64 For an example of Eli Lilly's brazen attack on opposition to medicalization as mere superstition or politics, compare the interview given to *The World* on Public Radio International in "Depressed in Japan," Nov. 19, 2002.

65 Another component of this critical conjuncture, one that is even more nondisciplinary than science studies, is cultural studies. The rise of cultural studies has even started to erode the long-enforced boundary between the humanities and the social sciences, at least in certain places. It is impossible to trace this shift to any single source, but fields like women's studies, minority studies, and the revitalized area studies point in a similar direction epistemologically (although European studies is a partial exception; see Steinmetz 2003a). The role of neo-Marxism in this history also should not be underestimated. Neo-Marxism simply ignored barriers between the humanities and the social sciences (I am thinking of journals like *New Left Review, Das Argument, Cultural Critique, Social Text,* and *Boundary 2*). A final critical component of this conjuncture is Foucault, whose radical antidisciplinarity and antipositivism was crucial in broadcasting a nonpositivist message across the humanities and social sciences. Evidence has been found in Foucault's writings to support almost every conceivable position, of course. One of his most influential books, *The Order of Things,* was concerned with what used to be called intellectual history, or what in another discipline is known as the sociology of knowledge. Foucault's thesis of the existence of a time-bound episteme ordering the various fields of knowledge in a given epoch may have proved objectionable even to Foucault himself in later years, due to its underestimation of the multiaccentuality of discursive formations (Steinmetz 2002), but it gave a fillip to a radicalized post-Kuhnian view of science as ordered by principles that are not readily or consciously available to participants in a given scientific arena. Foucault rejected the distinction between ideology and science that was still being maintained by writers like Althusser, strengthening the project of studying science critically and in its own right.

66 Lenin (1908/1927) discussed the "new positivism," equating it with his main target, idealism. He located the idealism of Mach and Richard Avenarius in a direct lineage with Berkeley and Hume. Lenin's own materialist philosophy of reflection boiled down to an empiricist rather than a depth realism, however. Lenin faults the idealists, for instance, for "not regard[ing] *sensations* as the true copy of this objective reality,

independent of man" (127, my emphasis). By contrast, Lukács's (1968b) approach was an explicit depth realism, organized around dialectical oppositions between essence and appearance, immediacy and mediation, and the capitalist irrationality of the whole as opposed to the limited rationality of its parts. Lukács suggested that scientistic social science provided a correct *phenomenal* description of capitalism, given the latter's calculating and fragmented character, which allowed for the local regularities codified in positivist general laws. As society became more rationalized, it did actually become somewhat more predictable. In response to this the (scientific) subject could then "pounce on opportunities created by the system of laws" and use them according to his "best interests" (130). Each bourgeois science, according to Lukács, was characterized by blindness to a particular area of reality—not because that area was impervious to knowledge but due to social reality's surface-level fragmentation and to the class bias of the bourgeois vision of the world.

67 Kant's analysis of the a priori categories of understanding is a closer approximation to the idea of an epistemological unconscious than positivist ideas of fully conscious knowledge production.

68 Bourdieu avoided a properly psychoanalytic understanding of the term unconscious, however (Steinmetz 2002).

69 See also the autobiographical observations by Sewell (this volume) and Weintraub (2002, ch. 7). Luke (1999) makes a similar argument for the "normalizing effects" of disciplinary practices in political science.

70 See Abbott (1999, 2001a), Breslau (1998), Burawoy (1979), Burawoy and Lukács (1992), Collier (1994), Dutton (1998), Eley (1980/1991, 1996, 2002), A. Elliott (2000), Engelmann (2003), Sandra Harding (1991), Hauptmann (1996), Keane (1997), Lawson (1999b), Mihic (1999), Mirowski (1989, 2002), T. Mitchell (2002), Sewell (1980, 1996), Somers (1996), Wingrove (2000).

71 It is revealing, for instance, that Schabas (2002, 219) is ostensibly concerned with gaining autonomy for a "mature" field of economic history while emphasizing that this would help historians of economics "garner more respect from the economics profession." A truly autonomous discipline would presumably be less concerned with gaining respect from exterior disciplines.

72 This question was recently posed in different terms with respect to the differing European and U.S. orientations to military hegemony (Kagan 2003; Meyerson 2003); see also Fourcade-Gourinchas (2003).

PART ONE

Positivism and Nonpositivism in Twentieth-Century Social Science