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# Explanation in Historical Sociology: Narrative, General Theory, and Historically Specific Theory<sup>1</sup>

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Sociologists seem doomed to fight the *methodenstreit* again and again. For every practitioner of intelligent division of labor or multidimensional research, there are a dozen advocates of one against the other.

Reopening the debate, Kiser and Hechter (1991) argue that historical sociologists have modeled themselves too much on historians, seduced by the desire to understand particular cases and the temptation to find knowledge through induction rather than general theory. In response, Skocpol (1994) denies that this argument sticks to her, and suggests that in any case it says little about specific explanations as distinct from abstract standards. Quadagno and Knapp (1992) argue that Kiser and Hechter have too narrow a notion of theory and that they fail to see how narrative history can help to build theory. Somers (in this issue) defends narrative as a dimension of theory itself. Along with the other critics, Somers also responds vigorously to the way in which Kiser and Hechter combine an ostensibly neutral, abstract argument for general theory with advocacy for rational choice theory. Some of Kiser and Hechter's position on general theory—such as the claim that “all good sociological explanations” must contain separate arguments about the motives of individual actors—thus derives more from the substance of rational choice theory than from methodological principles independent of specific theories. Somers accepts the argumentative gambit offered by Kiser and Hechter: to debate approaches to historical sociology as a replay of the *methodenstreit*, reworked in line with new trends in the philosophy of science.

This is unfortunate, though Kiser and Hechter invite it. They argue not simply about what will make for better sociological knowledge but also on the more philosophical terrain of “what adequate explanations must entail” (1991, p. 4). The result is to bring into often confusing combination three different kinds of argument: (1) about general epistemological and

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scientific principles, (2) about the role of theory in historical sociology, and (3) about the merits or demerits of rational choice theory as an exemplar of general theory. I will have the least to say about the last of these, since I think the merits of rational choice theory are best debated in terms of the new knowledge produced by its use rather than in terms of abstract sermons about what constitutes a good sort of theory.

The heart of the argument is about what counts as a good explanation and how such explanations might be pursued. Kiser and Hechter emphasize the centrality of causal explanations based on deductions from general theories that include abstract models, covering laws, and mechanisms. They denigrate other approaches to scientific knowledge. In this they follow mainstream philosophers from a generation or two ago, but they also lay claim to the more recently fashionable label of “realist” philosophy of science. Somers challenges this with a broader and more complex reading of (and to some extent intervention in) contemporary debates in philosophy of science and with her own claim to be a realist.<sup>2</sup> Resort to philosophy, however, does not bring clarity to the exchange (even though clarity is one of the main goods philosophers of science ostensibly have on offer). This is largely because the colloquy between Kiser and Hechter and Som-

<sup>2</sup> Somers designates Kiser and Hechter’s position “theoretical realism” and her own “relational realism.” In response, Kiser and Hechter claim that there is a clear “standard usage” among philosophers as to the term “realist” and that it fits their usage. Readers should be wary of attempts to settle arguments by claiming to discern clear standard usages in other disciplines. Kiser and Hechter do not make much use of the philosophical realist who has probably had the most influential engagement with sociology, Bhaskar (see, esp. 1978, 1986). As their only recent authority on the position, they cite Bunge (1996). He is indeed a prolific advocate of realism, but far from suggesting a standard usage, he finds it necessary to describe his position as “critical realism” to distinguish it from the many other claimants to the rhetorically desirable realist label. Putnam (1987) refers to the “many faces of realism.” Generic realism, for what it is worth, is a philosophical position rejecting both extreme forms of idealism (“the world is just my dream”) and thoroughgoing empiricism (the inductivism that Popper [1979] called “the bucket theory of the mind”; see also Miller 1985, pp. 101–17). Along with common sense, it holds that what is real is not limited to either sensory experience or our internally developed ideas. It acknowledges nonobservables as part of reality. The point in philosophy of science is to give theory (and beliefs and conjectures that may not quite add up to theories) standing alongside empirical inductions. At least in most versions, then, progress in knowledge is held to depend on both new observations and new thought about available observations. Thought (including theory) may play a role in producing useful observations; some thoughts (conjectures) are also corrigible by such observations; these are the ones most useful to science. Realism does not entail a Popperian belief in the progressive corrigibility of theories by empirical refutations, but the latter is consistent with and typical of a realist position. At the same time, realism suggests that the human world is always partly the product of our thought (and hence our culture); we cannot grasp it as a purely external reality (see Putnam 1987).

ers replicates divisions among philosophers, including notably that between “analytic” and both “Continental” and pragmatist approaches.<sup>3</sup> Philosophers are very smart people, and their inability to find a way out of this long-standing division reflects, among other things, the enormous power that differing perspectives, criteria of judgment, and decisions as to what questions to ask have over the production of knowledge.

Here, I wish to clarify some of what is at stake for the relationship among history, theory, and sociological knowledge. I wish also to argue against the rather extreme “either/or” choice between history and theory, description and explanation, narrative and causal analysis that readers might draw from the debate as currently presented.

#### RAMPANT INDUCTIVISM IN HISTORICAL SOCIOLOGY?

According to Kiser and Hechter (in this issue, p. 786), “Whereas historical sociologists traditionally had insisted on distinguishing social science from history, now many sought to do away with the boundary between these disciplines.” This boundary Kiser and Hechter see as that between idiographic and nomothetic approaches, induction and deduction, particular descriptions and general theories. This was always tendentious language, and presumed that a clear distinction could be drawn between the two kinds of inquiry. It grasps some differences between the disciplines, but it also distorts; certainly it is a caricature of what contemporary historians do to say that they seek only descriptions of historical particulars. It also implies objections to a wide range of sociological work, identifying as mainstream only a very specific kind of explanatory enterprise—and consigning not just narrative historical work but most qualitative sociology and much survey research to the descriptivist, idiographic, inductive periphery.

Kiser and Hechter draw their evidence for an antitheoretical turn from some abstract arguments against certain uses of theory rather than from

<sup>3</sup> In the article published as part of this debate, Kiser and Hechter show themselves to be less naive readers of philosophy of science than Somers suggests, but they do appear to have limited their reading to publications that suited their tastes, and to have grasped less well the divisions among philosophers. In an early draft of their critique of Somers, Kiser and Hechter referenced Searle (1993) to support their contention that “the vast majority of American philosophy departments refused to endorse views of the sort that Somers espouses.” The vast majority of American sociology departments refuse to endorse rational choice theory, but one doubts whether this persuades Kiser and Hechter that it is false. In any case, “American” is a key word in the quoted sentence. The “analytic” taste in philosophy is especially Anglo-Saxon. “Views of the sort that Somers espouses” are given more respect by philosophers elsewhere.

an examination of actual research. In fact, recent historical sociology draws on theory and frequently advances it. Consider Gould's (1995) use of network theory, Goldstone's (1991) of structural and demographic theory, and Goodwin's (1997) of theory of emotions as well as networks (to take only prominent authors whose names begin with *G*).

Since the inception of the discipline, historical sociologists have theorized historical patterns of continuity and change in both evolutionary and nonevolutionary models. Sociologists have also turned to history for data by which to test more limited theories and have developed theories partly through induction from historical scholarship. The critical discussions of theory that Kiser and Hechter cite actually come from a period of resurgence in historical sociology. Though important work earlier in the postwar period contributed to this resurgence (Smith 1991), the sub-field only gained critical mass and institutionalization in the 1970s and early 1980s. That it did so then reflected widespread theoretical concerns: to address social change and power relations; to restore agency to sociological analysis; to "denaturalize" social conditions by seeing paths that shaped them and ways in which they might have been otherwise. Reaction against functionalism, an attempt to overcome the marginalization of Marxism, and new readings of Weber all influenced this renewal of historical inquiry within sociology. So did a desire to pursue theory by pursuing history.

Historical sociologists recognized that extant theories of many important dimensions of modern society were based on implicit historical premises that needed to be reconsidered. Modernization theory, for example, had incorporated an account of early modern European history, which it then used to generate propositions for the study of social change in the rest of the world. It was not enough to test the propositions so generated (many of which did indeed prove dubious); it was important to reconsider the premises themselves on the basis of newer historical research. In a literal and important sense, then, the goal of much of the new historical sociology was to change the basic inductions that formed part of the core of sociological theories and provided the basis from which deductions were made. For this purpose, it was important that sociologists turn to history with an agenda that went beyond testing propositions derived from general theory. Of course, part of what enabled new historical research to generate new findings was change in analytic perspectives—such as greater concern for "history from below" and the role of power and conflict in shaping social change.

Explicitly or inexplicitly, this challenged some of the post-*methodenstreit* understandings of an older generation. Many had treated the social world simply as so many neutral instances of naturally occurring phenomena. For the newer historical sociologists, especially Marxists and Weberi-

ans, the contemporary social world was a specific configuration produced by historical struggles. In other words, the social world as it presented itself at one time was a particular, a singular conjuncture, as much as it was a cumulation of cases of more general processes. To say, for example, that it was predominantly capitalist was not only to indicate what theory would fit best with many replicable empirical data within it, but it was also to characterize the whole and to locate it in history. This was a theoretical claim.

Over time, historical sociology became domesticated as a style of research in American sociology (Calhoun 1997*b*). It lost its theoretical agenda (and the political agenda that shaped the interest of many in the 1970s and 1980s). Many sociologists turned to historical research, but it became increasingly hard to identify any common theory or approach to theory as dominant in or unifying of historical sociology. This is somewhat ironic, since much of the impetus for the rise of historical sociology came from theoretical issues and arguments, but it reflects the extent to which the legitimation of historical sociology as a subfield within the discipline was achieved by portraying it (misleadingly) as a method.

Rather than emphasizing sociology's substantive need for history—the need for social theory to be intrinsically historical—Skocpol, Tilly, and others argued that historical sociology should be accepted because it was or could be comparably rigorous to other forms of empirical sociology. Moreover, it was the only way to fill certain important gaps in empirical knowledge—for example, of rare events or phenomena that only happen over a long time span. Tilly (1984) emphasized the operationalization of quantitative sociological research and analytic methods for historical use; he engaged in important comparative studies, but he also warned against overreliance on a comparative strategy. Skocpol (1979; Skocpol and Somers 1980) placed a distinctive stress on comparison. While Skocpol's (1979) cases happened to be historical, her analytic method was less distinctively so. Indeed, her later analysis of the creation of state welfare programs in the United States was much more intrinsically historical (Skocpol 1992).

Emphasis on method obscured the importance of theory. Still, it is not entirely clear why Kiser and Hechter present their argument mainly as a challenge to historical sociology. In the first place, if they are right about the desirability of pursuing generalizable knowledge through identifying lawlike relationships and causal mechanisms, then this applies to all sociology (and, indeed, to all science). Second, historical sociology seems neither an extreme nor a typical case by which to make the argument for this version of general theory; symbolic interactionist fieldwork might qualify better as the former, survey research as the latter. Third, and most important, Kiser and Hechter do not address in a sustained way the specificity

of historical research or the place of historical reasoning in theory.<sup>4</sup> Doing so would have made the connection to historical sociology clearer.

Kiser and Hechter (1991, pp. 1–2) are concerned mainly that “comparative-historical sociologists are tending to move from arguments against *specific* theories to arguments against *theory in general*” (emphasis in original). But they are not interested in “theory in general,” that is, in the range of intellectual products and orientations sociologists call theory, so much as in what they call “general theory.” By this they refer to formulations of explicit assumptions, abstract scope conditions, and models that include both lawlike statements of relations and causal mechanisms. Such theories are to be the basis for deducing potentially testable propositions, including causal explanations of events. Their generality inheres both in the lawlike statements of relationships (which are ideally universal and at least uniform—in the sense that similar causes always produce similar effects) and in the mechanisms that are abstract enough to be used in analyzing multiple concrete events.<sup>5</sup> Preference for this sort of theory is

<sup>4</sup> The illustration Kiser and Hechter (1991) offer, based on studies by Skocpol and Mann, considers variations in state autonomy as a question abstracted from historical context. Contrast this to studying the development of the modern state as a historically specific process (which indeed both Skocpol and Mann do).

<sup>5</sup> Most of what Kiser and Hechter discuss has been a familiar part of texts and classes in theory construction for some time. The partial exception is their strong emphasis on causal mechanisms. Here they join in a vogue for reviving an idea historically linked to notions of strict causality, but using it in the context of more probabilistic models. Rational choice theory has played an important role in encouraging more emphasis on causal mechanisms alongside covering laws or causal relationships. Kiser and Hechter are actually somewhat vague about what is involved here, and their language shifts a bit between seeing the mechanisms as necessarily deduced from lawlike statements, and as simply being lawlike statements but of a different order from relationships of implication or correlation. One way of seeing what is at stake is to recognize that classical ideas of causation considered it as a relationship of force; all causes were agents exerting power over patients. This account ran into trouble in the 17th and 18th centuries, especially among empiricists. Failure to observe causes operating as external relations of force led to a variety of theoretical responses. Among these was Hume’s argument that what we observe is the spatiotemporal conjunction of events, not the necessary connections that previous theorists understood as causes. The idea of causality then became, for Hume, a matter of psychological imputation rather than an empirical observation. The notion of mechanisms came to prominence as an attempt to identify the ways in which connections were actually formed between objects and is especially important to “realist” theories willing to discuss causation even when it cannot be directly observed. Such mechanisms come in two forms: actual energy transfers and triggers or signals that do not involve substantial material force. Many important causes in social life are of the latter sort, which means that there can be no presumption of a “proportionate” material relationship between cause and effect (Bunge 1996, p. 31). This is one reason why social life is full of nonlinear relationships (see Abbott 1988). These do not invalidate causal analysis, but they make it hard to use some traditional ways of reasoning about causation.

the basis for Kiser and Hechter's (1991, p. 2) complaint that the "new wave" of historical sociologists is too inductive: "The structure of their arguments . . . tends not to be implicative (involving deductive logic), but conjunctive (involving the use of coherent narrative)."

Behind this preference lie two distinct though related philosophical controversies. One is the classic question of the *methodenstreit*, whether the human sciences need be distinguished methodologically from the physical and biological sciences. Arguments for such a distinction are generally grounded in three claims. First, human nature is never fully independent from culture and is modified by historical change. Second, we do not understand human action adequately by explaining it solely in terms of external antecedent causal conditions; such factors need to be supplemented at least by inquiry into motives and purposes and the pursuit of empathic understanding (*verstehen*). Third, many of the important events concerning humanity happen only once or very rarely and thus cannot be explained by theories focused on patterns in repetitious events such as those of nature.

The second controversy concerns the problem of induction. Kiser and Hechter use the same term to designate both the belief that basic categories of thought (like causality) derive from psychological experience and the attempt to make empirical generalizations basic premises of social theory. The former is inductivism in a strict epistemological sense, the latter a more general sort of empiricism. Arguments against the former are not necessarily decisive against the latter.

The status of causal reasoning is a classic issue that has bedeviled philosophers since Hume. Hume ([1748] 1902) famously argued that all pure empirical induction could yield was the knowledge that events were conjoined in space and time. The idea of cause could not be given by experience alone. Hume was content simply to focus on empirical generalizations without reliance on the notion of an unobservable causation. Defending causality and the greater certainty of lawlike knowledge, idealists were prepared to argue then that an idea of causation was given to the human mind a priori. They, more than empiricists, are ancestors of today's "realists." Durkheim ([1912] 1965) famously accepted the argument that individual experience could not produce basic categories of thought, but held that causality and the others were not simply a priori, but given by society. Eventually, Popper ([1934] 1959, 1972) and others saw that part of the intractability of the problem lay in the very empiricist presuppositions with which Hume and others began. In Popper's phrase, they worked with a "bucket theory of mind," believing that there is nothing in our intellect that has not entered it through the senses.

Popper argued that we should distinguish psychological questions (e.g.,



about how we learn) from logical questions (such as whether an inference is valid). As a logical question, the problem of induction concerns centrally how we can determine the truth or falsity “of universal laws relative to some ‘given’ test statements” (Popper 1972, p. 8).<sup>6</sup> Test statements are hypothetical singular descriptions of observable events; they may be judged true or false on the basis of experience (in science, usually experience guided by observational technique). They establish good reasons for accepting universal laws as true, though of course the latter remain conjectures subject to possible future refutation.<sup>7</sup> Whether empirical conjunctures suggest causal connections to us remains a psychological matter; logically they are important as tests for propositions derived from conjectural laws, not the basis for such hypothetical statements. We may gain a great deal of knowledge from empirical generalization thus, but in itself this will not enable us to arrive at universal laws.

Kiser and Hechter maintain Popper’s sharp distinction between the context of discovery and the context of justification, but are somewhat confusing in this regard. They purport to describe, for example, “the failure of induction to suggest causal mechanisms” (1991, p. 15). This is an extreme overstatement, however, since many causal mechanisms have been suggested to scientists of all sorts (including historical sociologists) by direct inductive observation (combined with some reasoning). Kiser and Hechter are right, however, that causal mechanisms cannot be specified on the basis of induction alone. Because they are not directly observable, their specification depends on inference from indirect tests. It is the latter in which Popperian scientists are interested. It does not matter to them so much how social scientists get their ideas as how they test them. Kiser and Hechter might reply that they would prefer that explanatory ideas be deductions from general theories, but it is unlikely that all new ideas as to causal mechanisms will be produced this way (even if one thinks this is the best way to formulate them). It would be more accurate to say that for the view they express it does not matter whether scientists get their ideas by deduction or by inspiration while taking a shower. What matters is that they are able to account for those ideas in terms of the deductive structures of general theories so that their possible falsification can contribute to the advance of theory.

<sup>6</sup> “Given” is in inverted commas because Popper wishes to call attention to a further class of problems pertaining to the empirical validity of test statements, including whether we can speak of experienced instances prior to the theories that define them as instances of test statements.

<sup>7</sup> This is one of the key distinctions between Popper’s critical realism and positivism. Positivists commonly hold that certain truths are established outside this process of infinite corrigibility.

By contrast, Somers (p. 732), in line with Kuhn and recent science studies, would argue that the idea of a sharp separation between contexts of discovery and justification is misleading. It allows for scientists to represent their work as fitting more fully into canonical models of inquiry than it usually does, and it systematically underplays the concrete histories in which new data, understandings, and theories are produced. If in fact changes in theories are always historically produced in a field of available alternatives, then formal justifications in terms of covering law theories and the like are at best partial accounts of why the changes have occurred. One must account also for what has been studied and what has not, for what sorts of observations could be gathered under given research conditions, and so on. Which topics are considered interesting, which archives are open, what technologies aid access? There is no reason to assume that these are simply uniform filters on progress; they shape sharp turns in the orientation of knowledge as well as variations in its rate of accumulation. Accounts of such factors may take narrative form. It is worth noting, though, that similar strictures apply to narrative. It is not sufficient simply to tell the story of what has occurred without inquiring into the conditions of these empirical events and the factors that selected this actual history from other possible ones, and into the rhetorical tastes and practices that guided the construction of the narrative.

#### GENERAL THEORY AND HISTORICAL EXPLANATION

When historical sociologists speak of induction, they seldom mean to enter into debates on basic epistemological problems. Proponents usually mean, rather, to suggest that the amount of insight or “explanation” provided by highly general theories is modest, partly because such theories do too much violence to the specifics of the historical patterns. These proponents propose to build knowledge of those patterns “upward” from empirical specifics.<sup>8</sup> Opponents, like Kiser and Hechter (1991, p. 9), charge that this

<sup>8</sup> As it happens, many of the “inductivist” historical sociologists rely heavily on the published works of historians to produce the empirical descriptions from which they work. The issue is not precisely nearness to archives, nor even nearness to facts, but something more like breadth and depth of knowledge of conditions and events. Thus Stinchcombe’s (1978) call for historical sociologists to pay less attention to general theories and more to analogies between empirical instances is a call for attention to complexity of patterns in richly understood cases, and comparisons among such cases, rather than fitting of such cases into abstract theories. But note here that Stinchcombe works with a sense of general theory different from that held by Kiser and Hechter. The theories to which he (and many of the so-called inductivists in historical sociology) object are theories of “whole events” or even whole historical processes. We may learn less from a general theory of revolutions, thus, than from detailed comparison of a

is a sign of “the predominance of historians’ norms” in historical sociology, with the result that “scope (generality) and analytic power have been minimized and descriptive accuracy has become the predominant criterion for constructing and judging explanations.” Their complaint, in other words, is less that historical sociologists are attempting wrongly to produce general theories by means of induction than that they are dispensing with general theory in favor of empirical detail.

This empiricism, it would seem, is what Kiser and Hechter mean by acting too much like historians. It is indeed a stylistic quirk of historians to point to the particulars of cases they know well in order to challenge attempts at generalizations; this makes them threatening book reviewers for historical sociologists (see Kiser and Hechter 1991, pp. 23–24). Inspired by 19th-century German “historical science,” American and some other historians took on the project of studying the past “as it really happened” (Novick 1988). This became a hallmark of objectivism and a challenge to all who would read the concerns, values, and indeed theories of a later age into historical accounts. This was central, for example, to what E. P. Thompson meant when he proposed “to rescue the poor stockinger, the Luddite cropper, the ‘obsolete’ hand-loom weaver, the ‘utopian’ artisan, and even the deluded follower of Joanna Southcott, from the enormous condescension of posterity” (1968, p. 13) and also when he later railed against “the poverty of theory” (1978). But despite an ideology of “nothing but the facts,” historians seldom engage in radical inductivism. They do not simply walk arbitrarily into archives and demand documents, storing up facts as the experience dictates. They seek to test existing claims and extend understanding to new (or hitherto unstudied) events. They treat studies of specific towns and social movements, fertility rates and royal succession crises as cases to be compared as well as appreciated in their specificity. They have theories, even when they choose to express them implicitly rather than explicitly. Above all, in light of the present debate, they write narratives that are hardly exercises in induction or indeed in pure empiricism.

Whatever theory historians use to organize their narrative explanations, however, is commonly left implicit. Historical sociologists have related to history partly by making such theory more explicit, subjecting it to logical

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handful of revolutions (see Skocpol 1978). But this is a claim about the “context of discovery” rather than the “context of justification.” In urging us to seek analogies, Stinchcombe might be understood to be suggesting a procedure well suited for identifying causal mechanisms, as Kiser and Hechter would like, rather than arguing against these. But like Robert Merton (1968) in advocating middle range theory, he is indicating that we are at an early stage of our quest and ought not to leap to premature attempts to theorize complex wholes.

scrutiny, and challenging it substantively. Conversely, they have also related to theory by challenging its historical presuppositions. As I suggested at the outset, and as Somers (p. 731) argues, "all social and political theory is founded on presuppositional historical claims." Though these may figure in the "core" of the theory (in the sense of Lakatos [1978]) and thus not be rendered as propositions subject to empirical test, they may be challenged directly as to their plausibility. Argument on such points is appropriately based on historical research.

When Kiser and Hechter refer to explanation, they take it for granted that this means causal explanation in a covering law model.<sup>9</sup> At least for the purposes of social science, they do not consider it an explanation to say what something means, or what its social function is, or by what sequence of events it came to exist or take on its present form. The kind of causal explanations that interest Kiser and Hechter are explanations for the occurrence of "facts," understood as highly replicable observations generated under specific theoretically defined conditions (see Bunge 1996, p. 137). In adopting the covering law model for explanation, they necessarily side with the position that there is no deep difference between the human and the natural or physical sciences. One side in the *methodenstreit* argued that historical change, cultural difference, and individual action made such a difference in human affairs that explanation could not proceed in the same form as it did in physical sciences or even in biology (though biology was understood to be more like history and social science than, say, physics or chemistry were). The basic idea of explanation through covering laws is that an event is explained by subsuming it under universal laws. More specifically, an explanation requires both statements of conditions (such as scope of application) and laws (which, as we saw in discussing Popper, take the form of well grounded but still always contingent hypotheses). The structure of a covering law explanation is virtually identical with that of a prediction.<sup>10</sup> Not everything in the theory is directly testable, of course, only the empirical statements that are deduced from it. Its core of assumptions, including basic mechanisms and models, can only be tested indirectly through tests of the propositions they yield.<sup>11</sup>

<sup>9</sup> The covering law model has many adherents who differ on other specifics. It is linked especially with Popper, Hempel, Mandelbaum, and Nagel, each of whom has written specifically about its application in historical explanation. Hempel (1942) wrote perhaps the decisive article in the ascendancy of the covering law model in the philosophy of history.

<sup>10</sup> See Bunge (1996, chap. 5) for a discussion of the relationship between explanation and prediction, including reasons why they are not precisely identical.

<sup>11</sup> There is a substantial literature concerning what counts as testing and how one may (or may not) evaluate "progress" in scientific theories. Lakatos (1978) introduced the idea of a "progressive" scientific research program to describe the capacity of a given

Kiser and Hechter (especially in the discussion published here) associate deviation from covering law explanations with the recent rise of “relativist” science studies, but in fact dissent is long standing. There have been sympathetic critics who held that the standard of universality set up by the protagonists of covering law explanations was much too stringent. A sharper challenge has come from adherents of varying forms of narrative explanation (Ricoeur 1984). Thinking of natural more than human history, Goudge (1958) argued that historical explanations commonly, and appropriately, rely not on laws but on the specification of a number of conditions that are jointly sufficient to account for an event when combined in a coherent narrative sequence. Gallie (1968) maintained that even joint sufficiency was too strong a criterion; it is enough that the temporally prior necessary conditions be specified. Rather than deducing a class of similar events from covering laws and instancial conditions, thus, one explains historical events by placing them in a sequence of statements of conditions. This is the distinction between “implication” and “conjunction” to which Kiser and Hechter allude. Lest it be thought that “conjunctive” explanation is mere recourse to Humean association between temporally contiguous events, note that the criterion of narrative coherence sets quite different standards, even though they are not of the covering law sort. Stories come in different genres and a range of disciplines and constraints shape the formulation of narratives—explanatory or otherwise. Danto (1965), for example, holds that the involvement of a subject in a story is crucial to narrative explanation. In any case, a story is neither a regression of variables against time nor a mere record of events. The identification of a causal sequence “intrudes” into narrative to give it direction (Gallie 1968, p. 110).

Somers enters the fray with a more substantive defense of narrative and a further challenge to covering law explanation. Before proceeding to this, however, we would do well to be a bit more probing about what covering law explanation and the use of general theory might mean in actual historical sociology.

Kiser and Hechter are less than clear on this point partly because they do not distinguish adequately between two claims about the importance of general theory in historical sociology. One is that general theory is valuable as the source of explanations in historical sociology because it allows us to understand better whatever substantive topic of inquiry motivated us in the first place. The examples Kiser and Hechter give generally pertain to this idea of value. Their major claim about rational choice theory

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theory to continue to guide research that produces progressively more successful empirical explanations, while suffering only modest revision at some distance from its core.

is that it will be useful to historical sociologists, not that it needs historical sociology to advance as a theory. The other claim, however, follows even more basically from the covering law approach to science. This is that general theory should be employed in historical sociology not so much to advance immediate historical understanding (though we hope this will happen) as to advance general theory itself by bringing it under risk of refutation and stimulation of further creativity by distinctive empirical data.

In this connection, it is interesting to note the critique of historical sociology offered by John Goldthorpe (1991), almost simultaneously to that of Kiser and Hechter's first foray (and surprisingly not mentioned in the present debate by either them or by Somers). Goldthorpe is also an adherent to a covering law approach to explanation and to the idea that the development of good general theory is the main object of sociological research. He agrees with Kiser and Hechter that many historical sociologists are inappropriately blurring the boundary between history and sociology (somewhat confusingly taking Skocpol as a primary example, though she has been a strong defender of the disciplinary distinction). Despite the similar premises, however, Goldthorpe reaches a more or less opposite conclusion. The burden of his argument is that sociologists have little business doing historical research. If they do it well they will simply be historians, though the odds of this are small given sociologists' poor training for doing historical research. More important, historical research is poorly suited to generating the observations needed to advance general theory by providing empirical refutations or confirmations for its propositions. What is needed for the development of more or less universal, lawlike theoretical statements is a large number of replicable observations, preferably gathered under controlled conditions. According to Goldthorpe, historical facts are inferences from relics, while the preferred facts of social science are the results of new, more perspicuous, more complete and repeatable observations. "History may serve as, so to speak, a 'residual category' for sociology, marking the point at which sociologists, in invoking 'history,' thereby curb their impulse to generalize or, in other words, to explain sociologically, and accept the role of the specific and of the contingent as framing—that is, as providing both the setting and the limits—of their own analyses" (Goldthorpe 1991, p. 14). History is more about scope conditions than causal generalization, thus. The more we seek the latter, the better off we are gathering contemporary data.

Why then turn to historical inquiry? Kiser and Hechter give examples of recent historical sociology in which general theory (rational choice) is employed to good effect, but these do not answer the question. That is, Kiser and Hechter give us no reason to think that the studies were decisive for advancing the general theory as such—for example, for developing

new micromechanisms that would change analysts' views of choice processes.<sup>12</sup> Rather, rational choice theory is shown to be useful for answering certain particular questions about events and phenomena that are of interest for other reasons. One might be interested in certain historical events or processes for their own sake, or one might care about a specific historical narrative—say the history of one's own nation—which would be advanced by a better understanding of certain events or processes. Presumably, however, these sound too much like historians' reasons for Kiser and Hechter's taste. This is basically where Kiser and Hechter leave us: rational choice theory is argued to be useful in analyzing problems the interest and importance of which it cannot in itself explain.

Alternatively, one might be interested in history because one wished to advance theory that was itself intrinsically historical. That is, one might be interested in a better theory of capitalism and thus understand capitalism not simply as recurrent events and processes but as a historically specific and situated phenomenon that not only shapes an epoch but is shaped by other historical changes and by phases in its development. In any everyday sense, this would seem an extremely general theoretical project, but it is not a project of developing general theory in Kiser and Hechter's sense. Their sense identifies generality with replicability; that is, specifying events or processes that will occur over and over again in many cases when causal conditions are met. This latter approach allows for the study of many processes within capitalism—investment practices, inflation, labor regimes, and technological obsolescence. It does not allow for the study of historical capitalism as such.

Theory that is intrinsically historical is not quite theory in Kiser and Hechter's sense. Marx's *Capital*, Durkheim's *Division of Labor*, and Weber's *Protestant Ethic* are among the many works that do not qualify. Of course, instead of just dropping them from the canon of sociological theory, we could reanalyze each, trying to find within it a residuum of useful propositions that could be recast in an ahistorical, covering law

<sup>12</sup> Kiser and Hechter (p. 797 above) do assert that translating historical scope conditions into abstract ones allows for insights developed through inquiry into the past to be applicable to the present and future, and indeed that this is "the most important contribution that historical sociology can make to the discipline as a whole." This does not answer the question of why one would need to turn to historical cases to gain these insights. It also seems to underestimate the complexity of the task of translating historical into abstract scope conditions and the extent to which choice of theoretical languages is likely to influence this. Most important, this presumes the capacity to abstract cases altogether out of history. This is most plausible when one is concerned with frequently replicated phenomena; it completely misses problems of historical singularities (like the rise of capitalism) significant for an entire epoch. Of course, the frequently replicated phenomena may be parts of more complex and rarer large-scale phenomena and may shed light on them.

manner.<sup>13</sup> Nonetheless, this would both do violence to these major works and eliminate much of what has given them enduring theoretical importance.

My point is not to dismiss the approach to theory that Kiser and Hechter advocate—either in general metatheoretical terms or in the specific example of rational choice theory. Rather, I want to suggest that it is simply not broad enough to encompass the full range of meanings of theory, the different ways in which theory is important to sociological understanding, or the reasons why sociologists turn to theory. It is a useful part of the disciplinary tool kit, better for some kinds of intellectual projects and problems than others, but poorly understood as definitive of theory (or science or the advancement of knowledge) in general.

#### NARRATIVE AND HISTORICAL EPISTEMOLOGY

Somers challenges Kiser and Hechter on many fronts and raises a variety of concerns. Centrally, she argues that theory cannot escape history and both misunderstands itself and introduces potential distortions into its work if it tries to do so. The kind of general theory that Kiser and Hechter advocate is, as we have seen, basically ahistorical (or, to put it more positively, omnitemporal). This has two implications. One is that, while this sort of theory can contribute to historical explanations, it does not address the specifically historical dimensions of theory or explanatory problems. The other is that theory of this sort tends to leave unexamined the “presuppositional historical claims” that inform it (Somers, p. 731). One of the points of Somers’s historical epistemology is to keep attention focused on the embeddedness of all knowledge (as of all social life) in history. This implies continually reexamining the ways in which both the standpoint of the present and the specific histories of theory development shape what we ask and do not ask and therefore what we know and do not know.<sup>14</sup>

<sup>13</sup> Elster (1985) has done something of this in the case of Marx. For a contrasting view, in which Marx appears as a historical and critical theorist rather than an immature rational choice theorist, see Postone (1993).

<sup>14</sup> There is a close link here to arguments about the inescapability of hermeneutic interpretation and the necessary limits to covering law theories and scientific prediction. As Taylor’s ([1971] 1985, p. 48) classic statement goes, man is a “self-defining animal.” “With changes in his self definition go changes in what man is, such that he has to be understood in different terms. But the conceptual mutations in human history can and frequently do produce conceptual webs which are incommensurable, that is, where the terms can’t be defined in relation to a common stratum of expressions.” Elsewhere (Callhoun 1995) I have argued that such cultural incommensurability is better understood as involving practices than simply terms. The very process of creating our contemporary webs of meaning—including our theories—implicates us in some rather than other relationships to projects of historical understanding.



Somers's argument is rooted in a crucial point derived from Kuhn: the history of the development of science explains as much about its current patterns of knowledge as do its attempts at theoretical justification of its knowledge claims. Theories—or paradigms—are seldom falsified (let alone proved true). Rather, scientists are persuaded to shift from one theory to another available one.<sup>15</sup> This does not mean that there is no such thing as falsification, rather that only fairly narrow empirical claims get falsified. Falsification of these claims casts doubts on underlying theoretical structures from which they may be deduced or to which they may be important. However, this is seldom if ever decisive in the actual abandonment of theory. Most reasonably robust theories are indefinitely revisable, and in any case an alternative theory needs to be presented that can succeed in competition with the one previously held. This may be a new theory or, as often happens in social science, the recycling of an old, previously widely abandoned theory, now presented in modified form. This last is precisely what happened with the redeployment of rational choice theory in sociology (no longer called utilitarianism and shorn of some of the assumptions that would have tied it to that early incarnation). Functionalism has likewise come back into vogue after deep crisis. Marxism may rise again, and so forth.

The significance of these points is more far-reaching than at first appears. For example, they raise questions about the idea of scientific progress. There are clearly new achievements in scientific understanding and in technological capacities to manipulate the external world that are derived from science. No serious scholar doubts this, and the charge of relativism hurled at post-Kuhnian science studies is at least overstated if not a red herring.<sup>16</sup> The problem is not that science does not help people to do new things. It is rather that it is hard if not impossible to arrive at a defensible metatheoretical position from which to assert that the succession of theoretical understandings represents a linear "progress." A commonly stated ideal is that a new theory should be able not only to succeed in all the explanatory tasks where a previous theory succeeded, but also to succeed where the old theory failed and perhaps even explain why the old theory failed. Even in the paradigm case of relativity theory versus

<sup>15</sup> In the following discussion, in accord with widespread sociological usage, I will generally use the word "theories" for the complex structures of theories and research approaches that Kuhn termed paradigms and not attempt to restrict it to a narrower sense of specific theories within a broader paradigm.

<sup>16</sup> In a typical overstatement, Kiser and Hechter (p. 788) accuse Somers of holding that "science is nothing but a social construction," as though she implied that all social constructions deserved equal epistemological credence. In claiming that science is a social construction, she never claims that there is nothing more to say about it or its efficacy, nor does she take a radically relativist (as distinct from relational) position.

Newtonian mechanics, it is not clear that this is a good account of what happened. In any case—and this is important to Somers’s argument—it is a narrative account of what happened, not a set of changes in understanding deducible from general laws of either nature or human reasoning.<sup>17</sup>

When theories contend and one gains more widespread adherence, this is seldom based on a confrontation between each of the theories in its entirety. Rather, certain parts or features of each theory are brought to the fore and become crucial to the assessment of their relative explanatory power. Such assessment takes place, moreover, in relation to specific explanatory problems. These problems are produced by research projects that direct attention in some possible directions rather than others and that focus attention on certain aspects of phenomena and not others. It is often in the choice and framing of research problems that extrascientific forces (e.g., funding agencies, governments, employers) have their greatest effects. Extrascientific efforts to promote particular solutions to problems are likely to be much less influential in the long run—at least where relatively open scientific practice is involved. There are also reasons within science but outside theory that help determine which problems become the foci of research and the basis for contention among theories. For example, available research technologies and the distribution of experience with different material practices among scientists and laboratories may shape what scientists choose to study. Scientific knowledge comes not simply out of theories and tests but out of the “mangle of practice” (Pickering 1997). There is a potentially infinite range of research problems, so the confrontation between theories is played out on an arbitrary subset. The subset may be historically explicable but is unlikely to be amenable to satisfactory justification within any theory let alone across theories. Thus which problems scientists study (and which they do not) has an impact on the direction of theoretical development. Even where there is clear improvement in the ability to solve the particular problems addressed, we have no basis for assessing this in relation to the problems not addressed.<sup>18</sup>

For these (and related) reasons, scholars in science studies since Kuhn have argued that it is important to study the context of discovery as well as efforts to justify knowledge claims. The context of discovery determines

<sup>17</sup> Kuhn ([1962] 1970) did suggest that “rational reconstruction” could save the narrative history of change in scientific theories from being subject only to hermeneutic interpretation. Denying this is basic to both a more thoroughgoing pragmatism (Rorty) and many so-called postmodernist challenges (e.g., Lyotard).

<sup>18</sup> This holds without accepting Somers’s much stronger (and more contentious) statement that “there are no universally valid principles of logical reasoning; there are only problem-driven ones” (p. 766).

both the range of theories available for comparison at any one time and the specific research practices through which empirical observations may be generated. Part of what Somers challenges in Kiser and Hechter is their adherence to a discussion of theory and theory testing based mainly on justification and their neglect of the significance of this dimension of Kuhn's argument.<sup>19</sup>

In a closely related point, Somers emphasizes Kuhn's argument that it is much more difficult than most philosophy (and ideology) of science suggests to "articulate" or develop "points of contact between a theory and nature" (Kuhn 1970, p. 30; quoted by Somers, p. 735 above). It is conceptually as well as sometimes materially hard to generate observations that are genuinely effective in deciding for one theory and against another.<sup>20</sup> As Somers points out, neither induction nor deduction solves the problem. What exists between theory and research practice is not a relationship of simple testing but one of sustained mutual engagement.

#### CAUSALITY

Kiser and Hechter (1991, p. 4) begin, Somers stresses, with a dubious empirical claim. They assert that there is "wide agreement . . . across social science" that causality is "the first requirement of an adequate explanation." Somers rightly responds that no such agreement exists, though I think we might qualify this by noting that many social scientists, perhaps even a majority, think casually that explanation is basically a matter of identifying causes. The problem is that they do not go beyond such casual thinking or they would run into the many difficulties with the assertion. Somers perhaps does not distinguish clearly enough three kinds of difficulties: (1) Different things are meant by explanation. (2) Different things are meant by causation. (3) Even where social scientists definitely define explanation as adducing causes, and mean the same thing by causes, such causes are notoriously hard to prove.

<sup>19</sup> I think this is also what Quadagno and Knapp (1992, p. 482) are getting at when they write that it makes sense to "derive theoretical explanations from detailed narrative histories." Kiser and Hechter (pp. 795–96) mock them for asserting this, and for asserting that theory should provide researchers with questions, not just with answers to be tested. Obviously, Quadagno and Knapp cannot mean the same logical operation by "derive" as do Kiser and Hechter or anyone else speaking about deductive theorizing. But they are within the bounds of reasonable English usage, and they call attention to the difficulty of making sense of the role of theory without addressing the practical production of knowledge, the context of discovery.

<sup>20</sup> The issue is partly sheer difficulty, partly the tendency to be biased by theoretical starting point. Somers (p. 760) approvingly cites Hollis (1994, p. 79): "In deciding on which facts to select and transform into evidence, we are already to a large degree deciding between rival theories."

*Explanation.*—The equation of explanation with causality has indeed become widespread and is especially widespread in Anglo-Saxon philosophy of science and scientific discourse. It is less widely shared in other philosophical traditions and in ordinary language. Many social scientists are influenced by other philosophical traditions, and quite a few even speak ordinary language. Some, therefore, believe they can “explain” something by saying what it means or how it works, not just what caused it (or what it causes). This view is particularly salient in relation to historical inquiries (as to cross-cultural ones) because considerable effort may be required to grasp the meanings that objects, phenomena, ideas, and other people have for actors in very different social and cultural settings (see Calhoun 1995, chaps. 2–3). As terms of art, scholars sometimes distinguish achieving understanding, in the sense just evoked, from explaining. This follows the distinction between *verstehen* and *erklärung* formulated during the *methodenstreit*. The point of the distinction was to contrast the attempt to produce knowledge through interpreting how historical actors thought and felt, their practical orientations as well as their explicitly avowed intentions, and the attempt to produce knowledge through study of external causal linkages. Understanding was held to be important (by some) because the human sciences dealt with sentient beings who took actions based on the meanings they found in or imputed to their worlds. Explanation was held to be different not because it was more empirical, but because it was more objective—including in the literal sense of treating people as objects rather than actors (in other words, making no distinction between action and behavior). This notion of causal explanation achieved its apogee in social science (especially psychology and sociology) in the era of behaviorism.

Covering laws and mechanisms are both examples of explaining by adverting external factors to account for behavior (or other events). The interesting, perhaps even ironic, twist introduced by Kiser and Hechter (along with Elster [1985], Coleman [1984], and many others) is to emphasize causal explanation in an ostensibly voluntaristic theory. At least in many versions, rational choice theory treats human beings as actors with some degree of agency and makes reference to their motives and not merely the external causes evoked by those who linked the notion of cause with that of explanation during and after the *methodenstreit*.<sup>21</sup> On Som-

<sup>21</sup> Bentham was a strict behaviorist. This is a difference between some versions of modern rational choice theory and classical utilitarianism. Rational choice theory in this sense tries to combine what philosophers have sometimes distinguished as a logic of natural relations and a logic of action (Dray 1964). Kiser and Hechter (pp. 789, 800) specifically identify their theory with intentional action rather than a more deterministic logic of interests and argue that because of the importance of individual action, sociologists “must employ a different methodology than that used in the natural

ers's reading (pp. 749, 752 above), Kiser and Hechter offer a theory of "agents as mechanisms."<sup>22</sup> In doing so, they not only introduce rather simplistic ontological claims about what people are like; more surprisingly, they withdraw the apparent voluntarism of the theory. This is so, she suggests, because they hold that mechanisms must be deducible from general theories, so if an agent is a mechanism she cannot have much real agency. The demand for microfoundations as part of all fully successful theoretical explanations starts out looking like an opening to human reason (as in the *verstehen* side of the *methodenstreit*) but (if the view of the agent is as narrow as Somers suggests) ends up introducing a substantial determinism and possibly an element of tautology into the theory.

Thinking of explanation only in terms of causality, and of theory only in terms of the general theories that generate causal explanations, leaves out many important efforts to achieve relatively general understandings of social life. I have alluded already to the reasons why one might wish to formulate intrinsically historical theories. A further example may be clarifying specifically in relationship to causality. Some abstract categories are important in theory, research, and everyday social life because they constitute dimensions of the reality we study and in which we live. Money is such a constitutive abstraction; its creation is a collectively achieved performative investiture of value. The business corporation exists and is treated in law as an artificial person (and so, in many respects, is the state; see Coleman 1984). Nations are not simply found in external terms but created in human history (though not necessarily through such discrete events as the idea of "invention" implies). Their creation is partly based

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sciences." They do not say what this different methodology is; however, though they do imply that it remains objectivist, rather than being strongly interpretative. They maintain a strong distinction between individuals and contexts (p. 800) and insist on "a social world existing outside the minds and sense perceptions of its participants" (p. 789), and thus apparently not intersubjectively constructed.

<sup>22</sup> Somers changes her account somewhat on p. 764 when she endorses Tilly's (1995, p. 1602) account of rational choice theory's treatment of actors as "coherent, durable, self-propelling social units-monads." At this point she is interested (reasonably) in emphasizing the lack of a strong sense of the agents as social. She shifts, however, from describing the agents as being mechanisms to describing them as the bearers of mechanisms. In either case, they are causal forces in themselves, but in the second case one could not conclude that Kiser and Hechter had taken away apparent voluntarism as clearly as one could in the former. A somewhat related issue arises a page later. Somers (p. 764) accuses Kiser and Hechter of "essentialism." It seems to me that they (and most rational choice theorists) have not actually offered a substantial enough account of how they understand human agents for us to decide on this charge. It may be, however, that the only way for rational choice theory to escape the charge of essentialism is to relax its standards for microfoundations. If the agents are indeed mechanisms, or have inherent causal power by virtue of fixed dispositions, then the charge of essentialism is more likely to stick.

on a rhetoric or discursive formation that makes it meaningful for people to speak and think about and become emotionally committed to “their” nations (Calhoun 1997a). To paraphrase W. I. Thomas, each of these phenomena becomes real because we treat it as real in our actions. The existence of money, business corporations, and nations is basic to the modern world. Explaining how this is so would seem to be important for sociological theory.<sup>23</sup> But is this project of explanation graspable entirely through covering laws and causal mechanisms? Part of what we want to understand is the transformation of the world by these new phenomena that are created in and through culture. Explanation here may be achieved partly through narrative, as Somers suggests, but this does not exhaust it either. Critical inquiry into cultural categories may produce theoretical explanations that are neither simply causal nor simply narrative.<sup>24</sup>

*Causation.*—Even in the heyday of covering law theory, one of its foremost defenders could write of the principle of causality that “there is no generally accepted standard formulation of it, nor is there general agreement as to what it affirms” (Nagel 1961, p. 316). Most of what passes for causal analysis in the social sciences is in fact identification of more or less “weak implication” between statistical variables (Boudon 1974). It is very different from arguments as to strict causation. The issue here is not specific to history; in fact, highly particularistic historical arguments may sometimes come closer to the logic of strict causality than most quantitative social science. Somers (pp. 746–47; original emphasis) implies that it is an error for Kiser and Hechter to “attempt to combine a covering law model logic based on *empirical* regularities with a method of *imputed*

<sup>23</sup> As Somers notes, Kiser and Hechter make a good deal of the inability of comparative historical sociology to achieve independence of cases. Their solution is to turn to lower level, frequently repeated phenomena where they believe this is less of a problem. Somers (pp. 757–58) accuses them of presupposing (if not quite of holding) “that cases are discovered as ready made, discreet entities—given in the nature of things, that is—rather than constructed along analytic parameters.” While this may not be altogether fair, it is certainly true that Kiser and Hechter offer little account of how one would make the construction of cases an object of study, facilitating inquiry not just into theory and its operationalization, but into the way the categories used by many theories reflect the historical constitution of aspects of the world they observe (as, e.g., by assuming nations, or events situated in national histories, as the units of comparison).

<sup>24</sup> One may argue that the first chapter of *Capital*, vol. 1 (Marx 1867), is a theoretical account of such constitutive categories, of how the commodity form and abstract labor constitute capitalism. This is not per se an account of the historical origins of capitalism—the sequence of steps by which it came into being. Nor is it a covering law theory of either capitalism or the various features Marx identifies as central. Nor further is it an account of the commodity form or abstract labor as causal mechanisms producing capitalism. Rather, it is a theoretical statement of what capital (the basic term in capitalism) is. In this sense, but not in Kiser and Hechter’s, it is an explanation.

causal mechanisms.”<sup>25</sup> This is not necessarily so. It is true that positivists attempted to dispense with reference to unobservable causal mechanisms, but not that all covering law theorists follow them in this. Moreover, until the 1960s, most covering law theories were theories of strict causation. Nagel (1961) and Hempel (1962) led the way in accepting probabilistic and statistical laws in place of universal ones. An important result of this, however, doesn’t get the attention it deserves from Kiser and Hechter. This is the fact that when “laws” are probabilistic, it is no longer possible in the ordinary logical sense to deduce an explanandum from an explanans, though one may infer probability.

*Problems of proof.*—Proving causation is a formidable challenge; it is met to varying degrees of satisfaction often enough that it remains meaningful to speak of causal analysis in social science, but it is met in such different ways that care is required before assuming common meaning. Some will accept a causal argument as complete if it consists of necessary but insufficient causes, others require necessity plus sufficiency. Some hold that causality can link only events, others use the term to describe links between properties or conditions and events. There is also the difficulty that most phenomena in social life admit of multiple causes (under any meaning of the term). Identical events, moreover, may result from completely different causal determinations (though some still dispute this, sticking perhaps to Comte’s positivist dictum that there should be a one-to-one match between causes and effects). Not least of all, there is the question of determinism. Indeterminacy in the form of chance operates alongside causality even in physics. As Wilhelm Dilthey and Heinrich Rickert noted a century ago, in addition, to the extent that human beings are held to be actors with free will, able to shape history by their intentions, a different sort of indeterminacy is also introduced. This by no means invalidates causal analysis. It merely means that “causation is just one mode of determination, along with self-determination (or spontaneity), chance, and purpose” (Bunge 1996, p. 33).

<sup>25</sup> Somers (p. 775) also says that Kiser and Hechter “create a hybrid standard for explanatory adequacy by conflating the abstract conjunctions of predictive laws with true causality.” This seems too strong, though their writing is ambiguous. Rather, I read them as arguing for making use of both predictive, covering-law-style arguments and attempts to identify causal mechanisms in the effort to produce fuller explanations. There seems to be no contradiction in this so long as one keeps them conceptually distinct. Admittedly, Kiser and Hechter are sometimes unclear about how these fit together at the level of philosophical underpinnings. In particular, they do not address the question of what the implications are if (as I think is the case) they see mechanisms working in a probabilistic rather than a strict causation model. This would increase complexity by an order of magnitude and reduce the likely payoff to deductive theorizing—at least until vastly more is known.

CONCLUSION

I have suggested that the methodological divide drawn in this debate is starker than is either widespread in or valuable for the practice of historical sociology. There is room for both general theory and narrative in the explanation of sociologically significant historical phenomena. Moreover, these two do not exhaust useful approaches to such explanation. In particular, though I have not taken the space to develop a positive argument, I have indicated the importance of intrinsically historical theory. This includes both theory that is about the shape of history, its disjunctures as well as its continuities, and theory that is self-conscious about its own historical specificity and that of the phenomena it studies. It is striking that Kiser and Hechter advocate a form of theory that can produce explanations of microprocesses within important historical phenomena, but that will tend to leave their historical importance as such to be accounted for in other ways. Narrative can do some of the work, as Somers suggests, but there is also need for the kind of historical sociology that both learns from and problematizes narrative and that theorizes in nonnarrative forms.

Kiser and Hechter's 1991 article gets us off to a problematic start in thinking about explanation in historical sociology by posing the question in terms of sociology versus history, nomothetic-deductive theory versus inductive or particularizing empirical inquiry. This not only polarizes; it misstates the issues. The fact that Kiser and Hechter are open to arguments based on statistical implication as well as strict causal universality already takes them some distance from the strong ideal of nomothetic-deductive theory as formulated in the *methodenstreit*. By the same token, the terms "idiographic particularism" and "inductivism" poorly describe actual historical research. In the more moderate passages of their contribution published here, Kiser and Hechter indicate that they did not mean to argue one-sidedly that the sort of work they advocated based on general theory was the only good approach to sociological explanation. Their concern, they say, was for the limits of a historical sociology that refused to use such theory. Let us hope they are more worried than they need to be.

One of the drawbacks to thinking in terms of such binary oppositions as general theory versus narrative is that we are not encouraged to examine the questions of whether the strategies may complement each other or whether there are analytic problems that may call for one more than the other. As a familiar example, consider Habermas's (1984, 1988) famous (if problematic) distinction of *lifeworld* from *system*. Part of Habermas's theoretical project was to distinguish dimensions of social life that could be understood well in agent-centered accounts (including, implicitly, nar-



ratives) and those that would be missed or systematically distorted if only understood in such a way. Agent-centered hermeneutic accounts are needed to make sense of friendship; they do a much less adequate job in accounting for the operation of markets (though they are perhaps not irrelevant).

To understand the global expansion of capitalism, for example, is a historical problem. It is not a problem for historians rather than sociologists, because it is basic to the constitution of the contemporary social world and almost everything we study in that world. But it is a problem embedded in time, not just in the absolute but as constituted by the relations among changes in different parts of the system. Some of this will be grasped only by narrative accounts, especially where the experience or intentions of agents are crucial. Global capitalism also includes and indeed is conditioned upon a variety of narrower processes. Some of these can be (and are being) studied fruitfully by means of general theories more or less like those Kiser and Hechter advocate. Others are better addressed through middle range theories, in Merton's (1968) sense. But global capitalism is also importantly a singularity, not merely a cumulation of different instances of smaller, frequently repeated phenomena. It is important for sociology to theorize it as such. This argues for a composite explanatory strategy in which narrative and mechanistic or covering law explanations can each play a part. Nonetheless, an explanation of what capitalism is and how it works will exceed both narrative and discrete causal mechanisms or covering laws.

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