SYMPOSIUM ON HISTORICAL
SOCIOLOGY AND RATIONAL CHOICE
THEORY

"We’re No Angels": Realism, Rational
Choice, and Relationality in Social Science

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An alarm has been sounded that historical sociology is subverting
the theoretical aims of social science. Criticizing an array of widely
influential scholars, Kiser and Hechter propose that rational choice
theory can avoid the trend toward “empiricism” that results from
the import of history into sociology. Their position is based on theo-
retical realism—a radically antipositivist thesis that uses ontological
and theoretical postulates to theorize about reality beyond positive
appearance. A close examination of theoretical realism casts doubts
on the epistemological foundations of rational choice theory. Rela-
tional realism, the alternative introduced here, places greater em-
phasis on the pragmatic elements of explanation, supporting a more
relational, causal-historical, and problem-driven view of theory. A
renewed appreciation of what is defined as Kuhn’s historical episte-
mology provides the foundation for evaluating these competing re-
search programs.

Almost 40 years ago, in a remarkably prescient first sentence, T. S. Kuhn
sent shock waves through our intellectual landscape and forever trans-
formed it: “History, if viewed as a repository for more than anecdote or

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chronology, could produce a decisive transformation in the image of science by which we are now possessed” (Kuhn [1962] 1970, p. 1). More recently, in a stinging attack, two influential sociologists issued an alarm about just those social scientists who appear to have embraced Kuhn’s challenge most eagerly: “The role of general theory in comparative-historical sociology is under attack” (Kiser and Hechter 1991, p. 1). I juxtapose these contrary perspectives to strike a paradox: Kuhn, a physicist and philosopher-historian of science, turned the prevailing image of science on its head by demonstrating the wrongheaded denial of history in the construction of theory. Thirty years later, Kiser and Hechter build firmly on the foundations of Kuhn’s legacy to express alarm over the dire consequences of taking up Kuhn’s mandate; the profligate use of history, they charge, is subverting theory. This is a paradox that needs explaining: How is it that two historical sociologists can stand on Kuhn’s shoulders and denounce the use of history when it was Kuhn himself who changed our understanding of history by demonstrating its centrality in theory and the production of knowledge?

At a time when increasing numbers are taking “the historic turn in the human sciences” (McDonald 1996), this question touches on some of the most critical and contested issues facing not just historical sociology but the social sciences across the board. For one, Kiser and Hechter criticize all research that is not theory driven, or “guided” by “general theory” (1991, pp. 2, 16, 17, 22) and thus make it clear that their specific concerns about historical sociology are less important than their more general calling into question the worth of problem-driven theory building and their skepticism toward alternative research strategies. Second, Kiser and Hechter’s view is not an isolated idiosyncratic critique of comparative historical sociology but part of a wide-reaching research program of rational choice theory with many such targets against research too commonly characterized as antitheoretical, relativistic, or empiricist (Green and Shapiro 1994; Friedman 1996). That Kiser and Hechter invoke rational choice not merely to exemplify but as the only theory to date that meets their criteria (1991, p. 23) is thus another reason for the general significance of this discussion. Rational choice—and its ancillaries such as game theory and principal-agent theory—is among the fastest growing theories in social science and has itself changed the parameters of recent debate. Exam-

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ining specifically Kiser and Hechter's argument thus serves more generally to clarify the epistemological foundations of one of the most influential theoretical developments of our time. Finally, this discussion transcends a debate among historical sociologists because Kiser and Hechter do not chastise only a few well-known historical sociologists but some of the most highly respected scholars across the social sciences—not just Michael Mann, Theda Skocpol, and Charles Tilly, but also Reinhard Bendix, Kai Erickson, Anthony Giddens, Robert Nisbet, Arthur Stinchcombe, Guy Swanson, Immanuel Wallerstein, and no less than "contemporary survey research in sociology" (1991, p. 16). Surely if this wide spectrum of scholarship is threatening the discipline, we would all do well to take notice.

Kiser and Hechter charge that historical sociology is "empiricist," wrongly "inductivist," innocent of the standards for "adequate explanation," "novelistic," subversively "relativist," and "antitheoretical." Instead of waging a defense, already done forcefully by Quadagno and Knapp (1992) and Skocpol (1994), in this article my primary aim is to turn the tables and examine the foundations of Kiser and Hechter's own position: By what principles of knowledge do they justify their criticisms? And are we convinced by those principles? Since, moreover, Kiser and Hechter's accusations are spun on the terms of historical sociology being antitheoretical, neglectful of mechanisms, too descriptive and interpretative in its use of history, and naive of the necessity for deductivist general theory, then how we evaluate their criticisms fully depends on whether we accept their definitions of history, theory, explanation, and justification. The questions I put to Kiser and Hechter, then, following their own criticisms, follow:

1. What should count as an "adequate" social science explanation? If theorizing causal mechanisms is the criterion for success, and mechanisms are unobservable, how do we know a successful explanation when we cannot see those mechanism?
2. How do we judge among rival theories, and do Kiser and Hechter give us good reasons to believe in their theory?
3. And, ultimately, if "general theory" is the criterion for success, as Kiser and Hechter claim, and history the antithesis of theory, what counts as "theory" and "history" in social science?

At stake, I believe, is the epistemological status not just of history but of social science tout court. Are theories to be constructed and judged exclusively by theory-driven standards (as per Kiser and Hechter), or (as I insist) should theories be problem driven and judged by grappling with the difficult question of what—beyond the elegance of theory itself—makes an explanation convincing?
In this article I address these issues through a three-part strategy: first, a focus on Kuhn and the Kuhnian legacy; second, a critical focus on Kiser and Hechter (1991); and third, an outline of an alternative to Kiser and Hechter’s position. It is in examining Kiser and Hechter’s theory-centric justifications for rational choice theory that my argument about the paradox, and the importance, of the Kuhnian influence becomes so important. For rational choice’s current epistemological foundations—specifically its insistence on the necessity of, and the justification for, using an exclusively deductivist and exogenous (not subject to problematization) general theory to impute otherwise unobservable causal mechanisms—is part of a trajectory made possible largely by the Kuhnian revolution. I will argue additionally, and again paradoxically, that a slightly different trajectory out of the same Kuhnian corpus provides the most convincing alternative to Kiser and Hechter’s model. This alternative is based on a historical epistemology (Somers 1996b); it also supports theorizing causality—but with a conception of theory as problem driven, pragmatic, relational, and historical.

It is in considering these contending appropriations of Kuhn in theorizing causality that we confront the paradox that both positions stem from what philosophers call scientific realism—the thesis that unobservable phenomena (“theoretical entities”) such as atoms and quarks, market forces or individual intentionality, despite the absence of positive (observable) evidence for their existence are “real” enough to be used as explanatory in theoretical accounts. Scientific realism does not fit neatly into either side of the several dichotomies around which Kiser and Hechter frame their attack—between theory and history, between positivism and relativism, between their privileging of deductivism and their denigration of inductivism. But Kiser and Hechter have framed wrongly the terms of debate and their position in it, for these are not the fundamental issues at stake in their argument.

At issue in Kiser and Hechter’s rational choice–based attack on historical sociology is a less familiar but no less significant contestation over scientific realism. Induction and deduction are both logics of positivism with confirmation rules based on observation language; rational choice theory, by contrast, rests on a realist thesis, and its method of imputing causal mechanisms from ontological and theoretical postulates makes it

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2 I say the “current” epistemological foundations with full awareness (as I point out inter alia) that the roots of rational choice theory go back to 17th-century rationalism, 18th- and 19th-century classical political economy, 20th-century neoclassical economics and decision theory, etc. My argument, however, is not an intellectual history but one addressing the epistemological conditions of possibility for its current resurgence—and these are absolutely post-Kuhnian.
a militantly antipositivist enterprise. All versions of realism accept that causal mechanisms—despite being unobservable—must be used as the basis of explanatory theoretical accounts; but only rational choice realism generates those mechanisms using on “ontic methodology” (Salmon 1984) in which the causal mechanisms of social explanation are postulated a priori from the same general theory that “guides” their research. Thus while scientific realism may seem like an arcane concern to some, any talk of causality (Kiser and Hechter’s as well as my own) that reaches beyond correlation and statistical probability into the territory of mechanisms (“meaningful connection between events as the basic tool of description and analysis” [Coleman 1986, p. 1327]) cannot avoid confronting the challenges of realism.

Questions about what we can legitimately claim to be “real” have, in fact, always been at the epicenter of social science inquiry—not for reasons of metaphysical discourse, but for reasons of explanation and trust in a theory. The crux of it is twofold: First, how can we theorize about—that is, ascribe causality to—that which is unobservable, be it price mechanisms, maximizing preferences, class consciousness, value-driven intentionality, or domination? It is precisely because connective mechanisms are unobservable—unlike correlations of empirical indicators—that positivism has militantly rebuked their inclusion in the realm of scientific theory. That this rebuke has held firm in mainstream conceptions of science is incontrovertible—to wit, the ability of tobacco companies to withstand legal liability on the grounds that no positive evidence has yet been found to prove not just that smoking is associated with but actually causes cancer.³

As per Coleman (1990), rational choice rightly rejects this rebuke and rightly insists that conjunction and statistical probability cannot substitute for connective mechanisms in any true account of causality (see also Elster 1989). By insisting on mechanisms, however, rational choice must face up to the second issue at stake in realism, namely the long-standing positivist demand (also rightly justified) for accountability: What warrants accepting as true even well-structured and parsimonious explanations that rely on constructs that, since we cannot observe them, we have no reason to accept as true? It was for very good reasons that positivism took on the heavy burden of demanding evidence for theoretical claims;

³ If there is need for any further support for the long-standing epistemological link between the problem of the unobservability characteristic of mechanisms and the difficulty in convincing scientists of true causality, consider that as I write this in October 1996 it is front-page headline news in the International Herald Tribune that a “mechanism” has been discovered that actually demonstrates “for the first time” that smoking “causes lung cancer” (emphasis added).
without it, we are susceptible to all and any prejudice passing under the guise of science. Postivism’s challenge is still justified: Why we should believe in any social science theory—however elegant or parsimonious—that explains by reference to unobservables? This is a question to which we still are owed an answer and to which all theories that traffic in mechanisms must be accountable if they are not to thrive as mere prejudice.  

The purpose in bringing attention, then, to what I argue is rational choice’s underlying theoretical realism—the thesis that belief in an explanation depends on belief in the a priori theory from which it is imputed—is to call its advocates explicitly to account. Kiser and Hechter, in the end, dodge this responsibility: They give us no good reasons why we should believe their version of reality. The advantage of even asking for accountability, however, is that the question may provoke further epistemological deliberation on all sides of the issue; I certainly hope so. In the meantime, my view of rational choice’s theoretical realism shares much with Harrison White’s rather pugnacious conclusion that rational choice rests on “an underlying ontology of ‘spirits’ . . . upon angels, that is, upon spirits both disembodied and independent . . . [the] goals or preference orderings . . . essential to . . . rational choice, are appropriate and relevant only to entities which are inertial as well as isolate—angels, in short” (White 1992, p. 301). For those among us who are not angels, however, perhaps we need a realism for the rest of us.

My argument is in part devoted to a reinterpretation of Kuhn’s work. It is oddly unfashionable to focus on Kuhn these days—odd since The Structure of Scientific Revolutions is “probably the most influential work ever written in the philosophy of science” (Lloyd 1993, p. 207; Fuller 1992, p. 242). So much has Kuhn passed tacitly into our knowledge culture, however, that his work is sometimes less examined than it is invoked as the crucial point of reference in all discussion of recent transformations in the social sciences—from epistemology to postmodernism. A rethinking of Kuhn and a reconsideration of his legacy, therefore, may be even more important as its explicit retreat from graduate syllabi subjects it increasingly to appropriation without consideration. So I begin with an admittedly unorthodox argument: The legacy of Structure is the challenge to rethink theory-construction through the lens of a historical epistemology.

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4 My thinking about these issues has been enriched enormously through discussions with Martin Hollis.

5 Kuhn’s was not a lone voice among philosophers; he was in the tradition of Duhem ([1906] 1954), Koyre (1957), Quine (1963, 1969), and Polanyi (1958). Why Kuhn broke through to a wider audience is a question for the sociology of knowledge (but see Alexander 1982; Fuller 1992). Kuhn himself famously disclaimed any implications for the social sciences.
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The unorthodoxy is purposeful; on the occasion of rational choice theory’s appropriation of the Kuhnian legacy, it is fitting to revisit directly the question of Kuhn’s significance by exploring his paradoxical duality of influence.

THE KUHNIAN CHALLENGE: BEYOND INDUCTION VERSUS DEDUCTION

When Kuhn published Structure, the “‘image of science’ by which [philosophers were]... possessed” (Kuhn 1970, p. 1) was dominated by positivist epistemology. Epistemology, in the generic, is concerned with how we know, or why certain theories are justified and declared “winners” whereas others barely survive their birth; positivist epistemology, in particular, prescribes rules of logic and observational foundations for distinguishing true knowledge from wrongheaded speculations or mere belief. At the time of Kuhn’s intervention, positivists debated over whether induction or deduction was the correct logic for confirming a theory. Kuhn’s analysis of science showed the differences between them to be less important than their similarities. Most famously, he pointed to their mutual grounding in the foundational premise of modern science—the sharp delineation between theory and observation: between the self-evident, pre-theoretical existence of empirical reality on the one side, and what are unobserved, hence merely theoretical, “ideas” about reality on the other. For both the logics of induction and deduction, observation is the final adjudicator in confirming or disconfirming a theory.

Drawing from the history of science, Kuhn questioned the theory- and method-independent status of observation. In so doing he brought the term “paradigm” into common academic parlance to signify a worldview that frames not just particular theories but also the questions considered worth asking in the first place and the rules by which they can rationally be answered. Kuhn showed that what science has considered as confirming evidence has been influenced by what our dominant paradigms allow us to see and, most especially, to care about. This in turn suggested that the debate between induction and deduction could be seen as a false dichotomy held together by a misleading assumption that knowledge progresses by applying the correct logic of theory/evidence relations. Rather than winning or losing on the grounds of logical rules about evidence in confirmation, Kuhn showed those theories that actually have been accepted as explanatory are those that have unseated the available competi-

6 “Often the paradigm theory is implicated directly in the design of apparatus able to solve the problem” (Kuhn 1970, p. 27).
tors in the field (1970, pp. 144–45). History shows that it takes a theory to beat a theory.

THE KUHNIAN REVOLUTION: THEORETICAL OR HISTORICAL?

Kuhn proved to be most prescient; the impact of the "Kuhnian revolution" was cataclysmic (e.g., Gutting 1980). The transformation it brought, however, is neither obvious nor singular as Kuhn, like all influential thinkers, has been subject to a multiplicity of appropriations. There is Kuhn 1, the Kuhn accused of "relativism" and "mob psychology"; Kuhn 2, the Kuhn of the history of science and scientific revolutions; Kuhn 3, the theory-centric Kuhn; and Kuhn 4, the Kuhn who outlined a historical epistemology. Kuhn 1 and Kuhn 2, whose ideas are among the most debated topics in the last 25 years of scholarship, are not the focus of this article. Kuhn 3, the chief focus of my critique, is the Kuhn of rational choice theory. Kuhn 4, relatively neglected, is the Kuhn from which rational choice can be challenged and who provides a renewed appreciation for problem-driven research and the centrality of history in the construction of knowledge.

"Theory-centrism" describes the interpretation of Kuhn that focused almost exclusively on his apparent blurring of the fact/theory distinction—a distinction that "positivism made so important... it was certain to be denied" (Hacking 1983, p. 170). The instrument of denial was misreading Kuhn to have said that observation is nothing but theory, and science was thus exclusively theory driven. But theory-centrism wrongly conflates into one dimension what are three separate aspects of the postpositivist critique. The first is the quite reasonable, but banal, point that no research can take place without some proportion of deductive reasoning, especially to generate new experiments and new measuring instruments. To hold this position, however, is simply to recognize that we go armed with ideas and postulates not solely to test a theory about the phenomena under scrutiny but also to have principles for selecting data or to generate causal analogies (Stinchcombe 1978) that can be tested in turn. The second extends to the social sciences the problematic argument that since (in natural science) there is often no significant distinction between observable and unobservable entities, they can legitimately be used interchangeably in social theory. The third wrongly extends the justified critique of the possibility of pure theory-independent passive reporting of facts to all research practices not exclusively driven by deductivist logic.

7 We also do so to avoid suffering a complete "epistemological crisis" at every decision-juncture in our daily intellectual life (MacIntyre 1980).
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The flattening of these into one grand theory-centrism created a
gestaltlike change in the image of science: The positivist privileging of
"brute fact" was suddenly replaced with a new hegemony of "the primacy
of theory" (for correctives, see Pickering 1989, 1992b, 1993; Hacking 1983,
1992; Galison 1987, 1989; Gooding 1992; Gooding, Pinch, and Schaffer
1989). Suddenly it appeared that philosophers had closed the door to rea-
soning that did not accept what was now an aggressively antiempirical,
dogmatic version of deductivist logic—"theory proposes; theory disposes."
Overnight, science was rewritten to reflect this new theory-driven story
in which experimental practices were demeaned and observations de-
moted to mere "illustrations." When such new stories concluded with or-
dinary positivist morality tales about experiments "confirming the theo-
rists' speculations" they still converged safely with the empirical
foundations of deductivism. But it was an easy slide from there to the
near-total hegemony of theory in which talk of evidence and reasoning
other than theory driven came to be seen as empiricist, naive inductivism.
Declared by theory centrists to be nothing more than passive empiricism,
experiment—in fact long the driving force in the construction of phenom-
ena and never merely the passive reporting of facts—was now denied an
independent role in theory building (Hacking 1983, pp. 159–61, 171). Most
ironically, in light of Kuhn's original mandate, history—long the episte-
omological Other of positivism—became as much a casualty of the new
theory-centrism as it had been of pre-Kuhnian philosophy.

RETHINKING THE KUHNIAN LEGACY: A HISTORICAL
EPISTEMOLOGY

Kuhn's contribution, I think, should be seen elsewhere—as adumbrated
in the first sentence of Structure, where he argued against the exclusion

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8 Hacking, e.g., tells us of radio astronomers Penzias and Wilson who in 1965 exper-
imented on what they thought might be a meaningless phenomenon (because they
were not testing a hypothesis) but which held intrinsic interest to them—namely the
static found in transatlantic radio. After considerable experimentation, they deter-
mined that there was a uniform amount of energy in space. Meanwhile, physicists at
Bell Labs were theorizing about what came to be called the Big Bang theory. Only
the discovery of the experimental tests—conducted completely independently of any
hypotheses about the origins of the earth—could confirm the theorists' speculation
of a uniform temperature throughout space. They found it in the work of Penzias and
Wilson—among the few experimenters in physics to have been given a Nobel Prize.
Yet when the story appeared in textbooks it was rewritten to confirm to a theory-
dominated one in which the experimenters were represented as merely testing the
hypotheses the theorists had already generated (Hacking 1983, pp. 159–61).
of history, not of theory. He thus posed the challenge of a historical epistemology, a term I use to capture the idea that the history and development of a thing (and not just the logic of its construction) can tell you something fundamental about its nature. The term is purposefully oxymoronic: It intentionally challenges the assumed antihistorical quality of epistemology and instead proposes that all of our knowledge, our logics, our presuppositions, indeed our very reasoning practices, are indelibly (even if obscurely) marked with the signature of time. They are “history laden”—a phrase meant to evoke, to disturb, and to invert the well-known claim that all data are “theory laden”—and to draw attention to the less discussed inverse; namely, that all social and political theory is founded on presuppositional historical claims. In what follows I use the term to cover three neglected but crucially important aspects of the Kuhnian legacy: his implicit focus on problem-driven sources of knowledge; his pragmatist notion of articulation in research practice; and his demonstration (if not yet theorization) of path dependency and causal narrativity in explanatory structure.

Problem Formation in Theory Construction
As an element of his mandate to go beyond the induction/deduction dichotomy, Kuhn argued that theories are defeated only by competing theories—not just by disconfirming data. But he did not stop there; in keeping with his opening sentence he looked to the history of science to understand why certain theories were even considered candidates for truth in the first place (Kuhn 1970, pp. 144–59). There he found that the most significant

9 “The primacy of theory,” after all, would hardly in itself be a revolutionary position, always having been the rallying cry of deductivists (e.g., Popper [1934] 1959).
10 I discuss the concept of a historical epistemology elsewhere at greater length (Somers 1996b; Somers and Gibson 1994). One reviewer makes the suggestion that the term epistemology is too tied up with its own origins in the search for the transhistorical grounds for knowledge to be coupled coherently with historical. While I appreciate the accuracy of the comment, a useful distinction nonetheless can be made between epistemology in the generic sense—simply the study or the question of how we determine what counts as knowledge, regardless of how that question is answered—and the specific rules of positivist epistemology that have dominated in standard philosophy (see Rorty 1979). Nothing in my argument suggests any relativism or historicism—in the sense that concepts only have meaning in their original contexts; rather, because I believe that the question of knowledge is a generic question to which all theories must be accountable, it is justified to distinguish a generic use of epistemology from the history of one particular version. I find the conjoining of history and epistemology to be usefully jarring, hence prefacing the term as “purposefully oxymoronic.”
factor in determining the spectrum of competing theories was the set of contemporary questions the theories were vying to answer correctly. This laid the groundwork for what was perhaps Kuhn’s most important contribution—his challenge to positivism’s exclusion of the significance of problem formation in epistemology (Kuhn 1970, pp. 8–9).

Postivist philosophy of science had confined itself to one side of the fundamental distinction between what it called the logic of justification and the context of discovery (Reichenbach 1951, pp. 229–49; Hempel 1965, pp. 3–18; Popper [1939] 1959, pp. 42–59). Discovery is the context in which we both “discover” those problems deemed significant and generate the initial hypotheses we propose to explain them. It is thus the generative moment in which we discover and decide what is worthy of problematizing in the first place. Philosophers considered the context of discovery to be purely subjective (based on psychological propensities of the individual scientist, etc.) and thus of no bearing whatsoever on the validity of a hypothesis. The logic of justification, by contrast, is concerned only with the formal logic of method and the correct means for verifying and structuring hypotheses: Are they supported by the evidence? confirmed by experiment? corroborated by stringent testing? (Hacking 1983, p. 6). Since the question of truth and acceptability rested in justification alone, positivist philosophers charged that to confuse discovery with final adjudicating method and logic was a result of “mystical interpretation” (Reichenbach 1951, p. 231) or, even worse, the “genetic fallacy.” That meant wrongly mixing up history—the “origins” of a theory—with its final logical truth.

Kuhn’s use of history challenges as indefensible the separation between logic and discovery; doing so, he suggests, actually creates false ideas about how knowledge is constituted and truth decided upon:

I may even seem to have violated the very influential contemporary distinction between “the context of discovery” and the “context of justification.” Can anything more than profound confusion be indicated by [this] admixture of diverse fields and concerns? Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge. . . . Yet my attempts to apply, even *grosso modo*, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. (Kuhn 1970, pp. 8–9)

Once history shows us that theories are tested against competing theories, Kuhn argues that the logic/discovery distinction proves untenable. Instead, it is the historical context of discovery that determines the spectrum of theories available for comparison at any point in time. Why, for exam-
ple, at \( T_2 \) rather than at \( T_1 \), does theory A win out over the field of competitors \{A,B,C,\ldots\}? Kuhn shows that for historical reasons theory A could appear to be justified in the field of theories \{A,B,C,\ldots\} available for comparison, but look not at all justified if the field of available theories is or was \{A,D,F\} (1970, pp. 23, 94–110, 144–59).

Two key insights can be extrapolated from Kuhn’s focus on problem formation. First, because scientists ask different questions at different points in time—often because of the questions that have already been asked—different answers also satisfy them at different points in time. Theory A may beat B at time \( T_1 \) but lose to B at time \( T_2 \) because those questions considered historically compelling may have changed (1970, pp. 102–6). Thus, for example, Galileo’s science was accepted at the exact moment when changes in artillery made it urgent to figure out where cannonballs would land and how far they would travel with a given amount of gunpowder. Historical exigencies most commonly bring certain questions into existence in the first place. “Civilizational concerns” (Zald 1991) post-1989, for example, explain why social scientists now problematize democratization at a level of intensity completely absent only 10 years earlier, thus bringing new candidates (e.g., Putnam 1993) or recycling ones once thought moribund (e.g., social contractarianism) into the field of contending theories. This demonstrates that contained within all theory is the temporality of the question, and a confirmed theory at \( T_1 \) may well be subject to future disconfirmation at \( T_2 \) when pressed by a different kind of problem (Kuhn 1970, pp. 198–200, 209–10). This does not suggest that there is no best theory—of course, there is—rather, that a certain humility is necessary in the face of what is not an absolute triumph but an “epistemological gain” (Taylor 1989; Longino 1990).

Kuhn anticipated that skeptics would reply that it is an accident when we find the best theory because, if the full field of logically possible theories \{A,B,C,D,F\} had been up for comparison, there could have been only one winner; furthermore, that one winner would have won in relation to any subset of those theories (1970, pp. 171–72). An answer to these criticisms can be drawn from the second major implication of Kuhn’s focus on problem formation and the historicity of scientific justification: The adjudicative status of facts and evidence depends upon the temporal context in which it is discovered; indeed the epistemological significance (degree of importance in confirming or rejecting a theory) of the same pieces of fact and evidence could be completely at odds depending upon the historical moment and context. For example, potentially disconfirming facts or “anomalies”—pieces of evidence that are incorrigible vis-à-vis a theory/prediction—vary in their importance depending on the point in time the data are observed. In a period of “normal science” an anomaly is treated
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not as a refutation of the theory but as an invitation to further develop the theory to accommodate the observation or, alternatively, to sweep it under the rug, sight unseen. In a moment of scientific “crisis,” however, the very same piece of evidence can assume an entirely different historical significance and can be used to refute the theory, frequently creating confusion and disarray among its former defenders (Kuhn 1970, pp. 97–98). The status of anomalies is also affected by the historical discovery of new anomalies: If a theory is suddenly inundated by an onslaught of new anomalies some previous anomaly—once considered minor or merely a “problem of measurement”—may suddenly be used as major evidence to show how terribly a theory has failed (pp. 52–91).

Contrary to the prescriptive rules that exclude history from the criteria for the acceptance of knowledge, Kuhn’s historical investigations suggest that theories are rarely adjudicated primarily on the basis of the logics of justification—whether inductive or deductive (1970, pp. 3, 26, 94–95). Rather, the historical processes of problem formation appear to be driving knowledge: Only in response to a particular question are facts transformed into evidence at all, and that a particular question was being asked was itself a matter of historical moment (Kuhn 1970, p. 103; Collingwood [1939] 1970; Somers 1996b). Standard epistemology declared that matters of truth are decided by purely logical procedures that are unaffected by historical circumstance; subject-matter data may be historical, but the foundations for knowledge are themselves outside of time. Yet here was Kuhn showing that in practice the criteria for justification have varied historically: Why a theory is confirmed at one time and not another requires a historical and causal explanation, rather than a strictly logical one, as “changes in the standards governing permissible problems, concepts, and explanations can transform a science” (Kuhn 1970, p. 106).

Articulation in Research Practice

Kuhn’s demonstration of the epistemological centrality of problem formation and his critique of the induction/deduction dichotomy show the great failure of the theory-centric reading of Kuhn to distinguish between “empiricism” and the altogether different practice of experimental activities. In its conflation of experiment into a passive empiricism, theory-centrism mistakenly combined two separate activities—“reporting” (via passive representation of observations) and “doing” and “causing” (via intervening; see Hacking 1983, p. 173; Humphreys 1988, 1989)—into the single and justifiably vulnerable concept of passive accumulation of data. But Kuhn was equally critical of claims for disproportionately deductivist
logic, for history also demonstrates that the relative import of general
theory-driven testing has been radically exaggerated (1970, pp. 23–33).
The reason for this is simple: “There are seldom many areas in which a
scientific theory can be directly compared with evidence” (p. 26; emphasis
added). Science reveals many avenues to pathbreaking knowledge; testing
theories by observing only the phenomena designated by a preexisting
hypothesis is only one such avenue.

Indeed Kuhn suggested that the most common type of research practice
was what he called articulation: “A paradigm [theory] is rarely an object
for replication. Instead, like an accepted judicial decision in the common
law, it is an object for articulation and specification under new or more
stringent conditions” (1970, p. 23). The analogy to the common law is
telling, for like Oliver Wendell Holmes’s ([1881] 1963, p. 1) famous excori-
ation that law is made by judges rather than applied through logic (the
“life of the law is not logic but experience”), Kuhn is suggesting that new
theory is constructed not through applying the logic of deduction but
through articulation—the pragmatic interaction of theory with histori-
cally changing problems. In this effort to capture the practice centeredness
of the research process through the term articulation, Kuhn, like Holmes,
is challenging the priority of abstract rules of logic over the historical con-
struction of knowledge—apparently building from the original Greek
meaning of historia as practical “inquiry” (Oxford English Dictionary, 4th
ed., s.v. “history”). Experimenting through articulation “can resemble ex-
ploration” (Kuhn 1970, p. 29), “more than any other sort of normal re-
search, the problems of paradigm articulation are simultaneously theoretical
and experimental” (p. 33). Articulation includes “resolving some of its
[the paradigm–theory’s] residual ambiguities and permitting the solution
of problems to which it had previously only drawn attention . . . manipu-
lations of theory undertaken, not because the predictions in which they re-
sult are intrinsically valuable, but because they can be confronted directly
with experiment” (pp. 27, 30). In a direct challenge to the possible hege-
mony of either induction or deduction, he stresses that “the need for work
of this sort [articulation] arises from the immense difficulties often encoun-
tered in developing points of contact between a theory and nature” (p. 30;
emphasis added). Articulation can also radically transform accepted log-
ics. In Galileo’s time, for example, previously unencountered historical
and practical exigencies induced methodological instabilities; where scien-
tists were once satisfied with qualitative explanations, they may suddenly
have demanded accurate quantitative predictions with the introduction
of cannonballs. Kuhn’s discussion of the pragmatics of articulation was
brief and only suggestive; the challenge he posed was to take this sugges-
tive notion and nurture it, as per Hacking (1983) who returns to an “old-
fashioned concept of history, as history not of what we think but of what we do" (p. 17; see also Pickering 1992a).

Kuhn’s Explanatory Challenge

Kuhn is usually read as challenging only the logical rules of theory confirmation—hence the wrongheaded association of Kuhn with “irrationality” and “relativism” in science (e.g., Lakatos 1970). But recall that Kuhn demonstrated that the criteria for theory acceptance varied throughout history. He did so by constructing an empirical causal explanation that gave a convincing alternative theory for how and why science developed the way that it did. Thus while he did not explicitly prescribe an alternative set of rules for the correct structure of an adequate explanation, Kuhn’s explanatory account of Western science is so powerful that I believe it can be read as a performative demonstration and an answer to epistemology’s central methodological question: How does a set of hypotheses, if true, explain why something happened?

At the time of Kuhn’s writing philosophers almost uniformly advocated the deductive-nomological (D-N) method, or the “covering law” model of explanation, in which an explanation is derived as an instance of a conditional proposition stating empirical regularities (Hempel [1942] 1959, 1962, pp. 247–48; Popper 1959; Reichenbach 1951; Miller 1987; Outhwaite 1987; Salmon 1984). How something happens must follow deductively from the conjunction of sentences describing laws and initial conditions, which is expressed in the logical model of those conditions. Because theories are “covered” by deductive laws (hence the “covering law” model), prediction and explanation are considered to be subsumed under the same logical process; explaining the past and predicting the future involves the same operation. Most notable, and an easy source of confusion, about the covering law model is its emphatic rejection of causation unless subsumed under nomothetic (general and universal) law. Causation in this sense refers not to laws based on event regularities but to explanation of the specific mechanisms that cause X to lead to Y. According to a strict positive logic derived from Hume (1938), theorizing a true causal explanation based on spatiotemporal connective mechanisms is nonscientific because actual causal pathways can never be positively observed; only correlations or regular conjunctions of events can be empirically grounded in observation and thus supported by scientific law. By rejecting the scientific status of underlying spatiotemporal causal mechanisms (because they are neither lawlike nor empirical), classical positivism was able to combine under a

11 Inductive-statistical methodologies share a commitment to the deductive form of the covering law model of explanation.
single rubric of general law both prediction and explanation. But explanation in this epistemology is not an explanatory account of how something was actually caused; instead, it is a limited deduction from empirical regularities (Hempel 1959, 1965, p. 359).

Because it subsumes explanation under the rubric of predictive universal laws, the covering law model cannot disclose the underlying causal mechanisms underwriting chains of events (e.g., "enchainment"; Abbott 1993, 1995; Coleman 1986, p. 1327); it cannot allow for the contingency of outcomes or explain how temporal sequences, conjunctures, and spatial patterns matter to theory construction (e.g., Aminzade 1992; Griffin 1992, 1993); it searches only for similarities, not variations and processes (Tilly 1995c). The result is that time and space become an empty "picture frame" (the scope parameters) in which the real action—now assigned to laws—takes place. But the results can be nonsensical: "Can Newtonian dynamics really be derived from relativistic dynamics?" (Kuhn 1970, p. 101; original emphasis).

Kuhn's explanatory account of Western science can, by contrast, serve as an implicit template for understanding how causal explanations, to be effective, must be temporally and narratively constructed. By giving us a deep causal account—full of causal mechanisms—of the development of science, he introduced temporality and historicity as category of explanation and demonstrated the limits to the covering law model. A successful explanation, as Kuhn shows us, one intended to account for how and why scientific knowledge developed, could not be constructed through logical deductive entailment under universal laws but only through a causal historical narrative establishing causal chains and relational structures. A given theory not only has a history, but is a history—at each stage it bears the sedimentation and residue of its previous history, of a series of encounters with confirming or anomalous evidence (MacIntyre 1980). Kuhn described what he considered to be the teleological logic of the covering law approach as "stages toward a set goal, a permanent scientific truth" (1970, p. 173). His own causal historical account of science, by contrast, he depicted analogously to evolution as contingent, path dependent, and driven by spatiotemporal causal mechanisms and linkages—not by the "goal" of a universal law: "We are all deeply accustomed to

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12 Although its strict epistemological legitimacy has been considerably diminished in recent philosophical argument (Salmon 1988; Miller 1987; Cartwright 1983; Outhwaite 1988), as Steinmetz (1998) points out, a "watered-down" version is still dominant within American sociology. Its methods courses, statistics textbooks, and articles in leading journals all point to the same basic concept of theory as "constant conjunctions of events" and empirical generalizations expressed as universal statements of the covering law type (Steinmetz, 1998, p. 172); see esp. King, Keohane, and Verba (1992), Turner (1992), Wacquant (1993).
seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. But... *Origin of Species* recognized no goal set either by God or nature... Even the eye and hand of man... were products of a process that moved steadily *from* primitive beginnings but *toward* no goal... the evolution of scientific ideas... may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar” (pp. 171–73; emphasis in original).

Kuhn’s analysis gave new life to an old suspicion: That an explanation intended to account for how and why something happened—and not one generated only for predictive purposes—must be established through causal chains and mechanisms. Absent these linkages, the covering law model undermines its capacity to present a true explanation (Cartwright 1983, 1989). Striking implications for the epistemological status of temporality and process can thus be derived from Kuhn’s demonstration that a successful explanation cannot work through logical deductive entailment. Most notably, it makes clear that there are alternatives to the covering law model that cannot be dismissed as nontheoretical, particularizing, and “storytelling”—that is, “merely historical.” Put slightly differently, alternative methods such as causal narrativity and path dependence are historical, and Kuhn has given history a new and long overdue significance and epistemological stature in the construction of theory.

**WHAT IS HISTORY?**

When Kuhn wrote *Structure*, the dominant image of history in science as merely “anecdote or chronology” (Kuhn 1970, p. 1) was mirrored in the social sciences, long divided between interpretivism and positivism (Apel 1984; Hollinger 1980). Although apparently at complete odds, arguably what kept them joined in battle for so long were mutually caricatured depictions of each other. For both positivists and interpretivists, science was a matter of general laws and logical principles. For positivists that was its appeal; for interpretivists, the grounds for rejecting its applicability to the human sciences. Similarly, for positivists and interpretivists alike, history was historicist—hence beyond causal analysis. A historical epistemology implicitly knocks out the foundations of these very shared assumptions: Neither science nor history fit the images depicted by either side of the debate. Science does not exist outside its own historical condi-

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13 Gould (1989) and MacIntyre (1980) each capture the elegance of this approach, albeit in very different ways.
tions; those historical conditions, however, are themselves constituted by theoretical causal sequences and relational structures.

A historical epistemology thus invites us to think about history as a dimension of epistemology and thus as a constituent, rather than an illustrative, element in the conditions and the construction of knowledge: “History, we too often say, is a purely descriptive discipline. . . . [But] how could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?” (Kuhn 1970, pp. 8, 9). Like Stinchcombe (1978, 1984) earlier, Tilly has recently taken up this invitation in his use of the thick/thin metaphor to characterize as “thin” the use of history as no more than a “transparent medium carrying along more substantial causes,” whereas in “thick” history time is “drenched with causes that inhere in sequence, accumulation, contingency, and proximity” (1994, p. 270). Thick history is a causal participant in the construction of knowledge without which theory cannot explain the world. The implications of this expanded sense of history from thin to thick, from passive medium into the realm of epistemology, show not only that theories are inherently historical, but also that what we call “history” is inherently theoretical. Indeed it is the “thin” use of history that has “repeatedly led social scientists to the mistaken conclusion that historians are particularizers while social scientists are generalizers” (p. 270). Tilly thus pays tribute to Kuhn who first defrocked these caricatures and challenged us to develop epistemologies that no longer thrive on either one: Would we continue the work of liberating history from its “thin” status as “antitheoretical,” “purely descriptive,” and “particularizing”? Or would we fall back on easy prejudices and “an entire arsenal of [false] dichotomies” (Kuhn 1970, p. 8)?

The Failure to Meet Kuhn’s Challenge

Kiser and Hechter have taken the latter route. In their attack on historical sociology they rely relentlessly on the very caricatures Kuhn so forcefully challenged us to move beyond. Historical sociology is accused of (1) using naive inductivism rather than deductivism, and (2) doing “merely” descriptive narrative history rather than “general theory.” At first glance, it appears as if Kiser and Hechter are simply rehearsing an orthodox positivism, criticizing historical sociology for violating rules of logic and not producing adequately lawlike generalizations. That they are, however, forcefully antipositivist in their commitment to rational choice’s theory-driven logic of social analysis and causal mechanism, makes it clear that their position is more complex and more challenging. To assess the justification for this epistemological mix, we need to place Kiser and Hechter’s position in the context of scientific realism.
TWO REALISMS OUT OF KUHN: THEORETICAL AND RELATIONAL

One of the great paradoxes of the Kuhnian revolution is that it gave new life to an old idea. Scientific realism is the centuries’ old rationalist thesis that objects of knowledge exist and act independently of our direct knowledge of them, even when they are unobservable “theoretical entities,” such as electrons and atoms, or social structures, classes, and market forces (Blau 1979, 1986, 1989; Hacking 1983; Putnam 1975). To understand why so much hangs on this claim, it is worth remembering that the issue of observability has been a thorn in the heart of science’s basic aspirations since the 17th century. On the one hand, since Hume, science has insisted that for any knowledge claims to qualify as sound there must be sensory empirical proof; hence Comte’s, Mill’s, and Durkheim’s insistence on a positive sociology. At the same time, however, since Descartes science has also had the rationalist goal of understanding laws, forces, and structures that, even though beyond the senses, are nonetheless believed to be the real forces at work in the world—albeit a reality beyond appearance.14

The tension, of course, is that prioritizing positive evidence, versus rationalist or realist postulates, are competing goals. Do Durkheim’s “social facts” really exist—that is, explain—anything? We can observe the (apparent) results of racial discrimination, but how do we prove a causal claim about racial characteristics? Are Marx’s economic laws really true? We can count income distribution, for example, but how do we prove a causal claim about class exploitation? Are people really rational choosers? We can count low voter turnout, but how do we prove that the cause is really utility maximization or free ridership, as opposed, say, to registration difficulties put in the way of voting by state and county governments?

Although rationalism and positivism long vied for dominance, in the 20th century the victory had gone decisively to the logical positivists who insisted that knowledge had to be justified by what was called “theory-independent observation language”—that is, theory must hold up against what in the last instance can be observed empirically, not what might (in theory) really be deeper, unobservable forces. Counterintuitively, this positivist position is antirealist because it does not define as “real” anything beyond the observable (correlated with indicators or accessible to inference and measurement). This is not to say that antirealists accept no such thing as reality; simply that what is inaccessible is—for all scientific purposes—as good as “not-real,” since we have no empirical support for believing in it. Realists call this “actualism” as it “denies the existence of underlying structures which determine . . . events, and instead locates the

14 “Newton saw apples fall with his eyes but the force and law of gravity are not to be perceived” (Hollis 1994, p. 4).
succession of cause and effect at the level of events [empirical regularities]" (Collier 1994, p. 7).

At stake in the question of realism is thus a great deal more than talk of metaphysics; these are questions of what counts as knowledge, how to build an explanation, and what it takes to justify an explanation based upon unseen causes. Questions about the reality of unobservables clearly matter because most social theories attribute causality to—that is, theorize about—unobservable phenomena, for example, explaining the fall of the Berlin Wall by reference to a democratic "political culture." If theories can only be justified empirically, then what counts as a theory of democratization must be limited to inferences from statistical regularities about, say, opinion-pollled discontent and degrees of capital infusion. To be sure, attitudinal indicators and factor analysis can be used to translate unobservable theoretical entities into correlations; but correlations are not causes. So while we can count survey answers and numbers of firms, a force called "political culture" still remains inaccessible to the senses and so—for antirealists—cannot be attributed the status of a true cause.

Antirealism thus puts strict limits to what we can justifiably claim to be able to explain, grounding its criteria on what we can know positively to be real. Realism, by contrast, rejects these limits to what counts as theory and is the inverse in every respect—beginning with its postulate that little about the social or natural world makes sense unless one believes in the reality of unobservable forces. From this premise follows the difference over what counts as theory: Whereas antirealists limit explanation to the covering law model and exclude causality, realists insist that explanation can and must be causal in the sense of accounting for mechanisms, regardless of empirical access. Realism is clearly attractive because it allows us to extend the reach of theory beyond the limits set by positivism and to theorize about that which positivism excludes—the causal power of unobservable mechanisms.

By now it should be obvious just how important the Kuhnian revolution would have been for a rebirth of realism. Kuhn’s historical challenge to the absolute primacy of observation in adjudicating scientific truth gave new life to the premise that there is more to reality than what is observable or easily translated into empirical indicators. In fact, Kuhn’s demonstration that scientific theories are rarely disposed of due to lack of fit with data, and more often because of the appearance on the scene of a more powerful (more problem-generating) competing theory-paradigm, suggested that it is not only possible but justified to claim that theories about reality (rather than observation) were actually the prime movers of scientific knowledge. How else was it possible or justified for Newton to use the theory of gravity to explain a falling apple? At the end of the day, after all, despite all the strains and struggles to make the consequences

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or effects of an unobservable observable (preferably "calibratable"), gravity itself was and is unseen and thus remains a theoretical entity.

The monumental challenge that post-Kuhnian realism continues to face, however, is how to convince us why we should believe in an unobservable reality's putative existence. It is one thing to agree instinctively that there exist social and natural forces and dynamics beyond the access of empirical perception and instrumentation; it is wholly another thing to consider as more than prejudice any one theory about the nature of those social realities. Thus, for example, if faced with a theory that explains the collapse of the Soviet Union in terms of market forces, game-theoretic agency, or political culture, the antirealist asks, Why should anyone believe these claims to be true when they attach causal power to phenomena that we cannot see, feel, hear, or touch? As long as we continue to believe in theories dealing in unobservable phenomena (as in most social science), it is fair to ask for an account of how we can believe—reliably—in such causes (Holli 1994, p. 12).

It is on this issue of epistemic access that realism is radically divided; and I want to suggest this division can be interpreted as two contrasting routes out of Kuhn, each bearing differing traces of a contested Kuhnian legacy that has become part of our contemporary knowledge culture. On the one side runs theoretical realism. Following the theory-centric route, theoretical realism assumes that theories survive to the extent that they are true pictures of what is real, that this truth is guided by a general theory, channeled through deductive logic, and in the final instance confirmed by the same general theory—in this case defined as a set of ontological axioms about the unobservable generative mechanisms at work in the social world. Harking back to 17th-century Cartesian rationalism, building in part on Friedman's (1953) deductivist method of "positive economics," and bolstered by a theory-centric reading of Kuhn, theoretical realism posits theories to be notoriously "underdetermined by data" (Newton-Smith [1958] 1978, p. 72; Boyd 1973; Lakatos 1970, 1976, 1978), for example, perhaps the most influential representative of theoretical realism (and certainly the primary influence on Kiser and Hechter), represents himself as having improved on Popper in light of (a theory-centric) Kuhn: Competing theories should be adjudicated not on the basis of which one displays consistency with evidence, but on the basis of which one outpaces the evidence: The—well planned—building of pigeon holes must proceed must faster than the recording of facts which are to be housed in them (Lakatos 1970, p. 100).

I discuss below, however, the ways in which rational choice's theoretical realism with its belief in the truth of theories differs significantly from Friedman's instrumental "as if" approach to theoretical assumptions.
Lakatos describes the scientific project not as a paradigm but as a "research program" comprised of an exogenous "hard core" not subject to debate, problematization, or disconfirmation that, in practice, takes on the status of a metaphysic; this is what Kiser and Hechter and many rational choice theorists consider a general theory. It is protected by an outer "auxiliary protective belt" which receives implications/predictions from the hard core, translates them into hypotheses, and allows only these to be tested or to be open to new formulations and measurements (Lakatos 1978). The inner hard core, by contrast, remains pristine and fully protected from threats of "naive falsification." Thus, as we will see, rational choice theorists protect their general theoretical assumptions about individual utility maximization and intentionality-driven mechanisms within the hard core allowing only the implications for a specific case to be sent out into the potentially disconfirming world of testing.

Against "empiricist epistemology," theoretical realism thus follows Lakatos and assigns priority to a philosophically based framework of research from which explanation is logically inferred. In marked contrast to Friedman's explicitly instrumental use of "as if" assumptions to generate testable hypotheses, theoretical realism generates hypotheses from "a prior argument about social reality [which] should operate . . . as a framework for research and as a regulative principle akin to the principle of truth" (Lloyd 1986, p. 9; emphasis added). And in contrast to the standard positivist criteria for causality (correlations of indicators in the form of manifest events) theoretical realism rejects "the necessity of regular mechanical connection between events in favor of the idea of the essential causal powers of kinds of things" (p. 8; emphasis added). For theoretical realism the work of causal analysis is to "discover," at the most "general" level, the structures, powers, propensities, and liabilities of both persons and social structures so that general, lawlike statements about both of them can be made, statements that refer to the "essential but unobservable powers and tendencies of natural kinds" rather than to event-event regularities (p. 156). This conception of general theory thus marks theoretical realism's departure from both positivism and the historical post-Kuhnians: theory consists of identifying the essential and timeless properties of the social and natural world that will constitute its Lakatosian hard core.

Over the last decade, what can be fairly dubbed a relational and pragmatic realism has emerged as a post-Kuhnian philosophy of science that is squarely on the other side of the epistemological divide from theoretical realism. Steinmetz (1998) has made the criticism that he sees no compelling reason why Bhaskar's "critical realism" is not an adequate term for the kind of realism I call "relational and pragmatic." Although I generally do not endorse the proliferation of
servables—such as states, markets, or social classes—does not depend on
the rationality or truth of any given theory but upon practical evidence
of its causal impact on the relationships in which it is embedded. Follow-
ing the historical and more pragmatist reading of Kuhn, relational realists
believe that, while it is justifiable to theorize about unobservables, any
particular theory entailing theoretical phenomena is historically provi-
sional. For relational realism that means one can believe in the reality of
a phenomenon without necessarily believing in the absolute truth or ulti-
mate reality of any single theory that claims to explain it. Belief in a phe-
nomenon or an outcome instead depends on evidence of its causal, practi-
cal, and relational significance in time and in space, a practice that often
entails building models to represent theoretical entities and testing them
by observing effects and inferring by “abduction” to the existence of the
entities. But the practices need not be so formal: Why do relational realists
continue to believe in electrons when theory X has been proved wrong?
Because, regardless of the successive fate of competing theories of elec-
trons, and long after the current theory has been surpassed, supermarket
doors will still open automatically when we step on the rubber mat—
thanks to whatever reality the theoretical concepts of electron, and photon
now represent (Hacking 1990b, p. 356). Substitute the term society, domi-
nation, gender, or any variety of social concept, and the same argument
holds. For example, a relational realist would use pragmatist reasoning
to argue that despite the fate or fashion of any particular theoretical con-
cept such as “sex roles,” “sexual division of labor,” or “gender,”—each of
which represents a different causal conception of an unobservable postu-
lated reality—we have reason to believe in the causal force of that which
these terms variously attempt to signify largely for one reason: When we
dress a baby in blue, we can observe that people treat that baby differently
than when we dress that same baby in pink.

Relational realism is thus a “minimalist” realism (Humphreys 1988;
Longino 1990) in that it presumes that if one is going to be a realist at
all—that is, assign mind-independent status to elements of the world—
then, by definition (and humility), one must be agnostic about the absolute
truth of any given theory about the world. This is what Bhaskar means
when he distinguishes between the “intransitive objects of science . . .
and the changing (and theoretically-imbued) transitive objects which are
produced within science” (1986, p. 52). Belief in an entity or a phenomenon
may well outlast numerous conflicting and failed versions of a theory—
to wit, the perdurability of the belief in “society” despite a surfeit of failed
neologisms, in this case I find myself enough at odds with Bhaskar’s views (that cau-
sality is found in an entity’s essential properties) to require an alternate realism built
on relational and pragmatist premises.

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versions of social theory. Where the two realisms differ, then, is that while theoretical realism attributes an ontological truth to the theoretical phenomenon (e.g., the theory of electrons or the theory of market equilibrium), relational realism focuses on the relational effect of the phenomenon itself (e.g., the impact of the hypothesized electron on its environment or of the hypothesized market forces on an observable outcome).

Relational realism is pragmatic and relational because it believes phenomena to have causal properties only in virtue of relational evidence, and because such knowledge can only emerge through “the temporality of practice” (Pickering 1992a, pp. 2–3, 9; see also Camic and Xie 1994). It is the relational effect of a hypothesized phenomenon such as gender, sex roles, on a problematized outcome that makes us believe in its causal power and not because of any single theory. To be sure, regardless of whether they are social or natural scientists, relational realists believe in the importance of determining which theories more closely represent reality. But the relational realist does not base her preference for one causal claim over another on whether or not she has found a theory more lean and parsimonious than another. Science has shown us that some theoretical reality claims are false just because the entities in question are not really there—whatever the logical validity, beauty, or parsimony of the theories that originally invented them: Epicurean atomism is not true, there are no humors, nothing with negative weight exists, women’s wombs do not wander, “phlogiston is one with the witches and the dragons” (MacIntyre 1980, p. 72). But other causal claims have endured despite a succession of different theoretical positions to account for them. Social phenomena endure; but the “theoretical entities” that have purported to explain them are socially constructed—some more convincingly than others because they are more pragmatic and relational.

What’s Wrong with Theoretical Realism?
Kiser and Hechter are theoretical realists—and thus heirs of the unbalanced, theory-centric reading of Kuhn. My criticism, however, centers not just on their theoretical realism, but on their peculiar mixture of pre-Kuhnian antirealism in their dedication to the covering law model of universal regularities, with their postpositivist theory-centrism as expressed in their commitment to causal mechanisms. Each in itself is problematic, but in combination they operate in such tension that they generate incoherent grounds for critique.

"WHAT ADEQUATE EXPLANATIONS MUST ENTAIL"
Kiser and Hechter start out on the wrong foot: “Wide agreement exists across social science” about the “first requirement of an adequate explana-
tion—causality" (1991, p. 4). Contra Kiser and Hechter, no such consensus exists. Contestation over causality has been the greatest thorn in epistemology ever since Hume convinced philosophers that the most we can call an explanation is a statement of correlations, and the most we can say about correlations is that they are driven not by necessity (as with mechanisms) but by statistical probability—which does not claim to be a true account of the causal mechanisms that actually produce the observed effect. As any frustrated social scientist knows from the difficulties of trying to prove causality despite overwhelmingly strong statistical correlation between, say, capital punishment and black defendants/white victims, the battle to overcome the stigma of what classical positivist skepticism from Comte, to Mill, to Popper has considered the mere "psychological appeal" of "ideas" about causation (beyond conjunction) has by no means been won (Popper 1950, p. 722).17

Underlying these questions of method, as I stressed above, is positivism's antirealist epistemology: What counts as an explanation is limited by what we can claim to know empirically to be real enough to refer to as a cause in the first place. Questions of explanatory methodology are thus inexorably linked to deeply contested issues over realism and to difficult questions about how to explain by reference to causal mechanisms whose reality is based on forces that can neither be observed, nor manifested beyond correlated indicators, nor conjured up a priori. What "adequate explanation must entail" is thus hardly a matter of consensus.

Causal Relations and Causal Mechanisms

Seemingly unaware of these ambushes, Kiser and Hechter plunge headlong into this methodological thicket, asserting flatly—and wrongly—the existence of similar agreement over the structure of an adequate explanation, namely both causal relations and mechanisms (1991, p. 4). "Causal relations" refers here to the standard covering law model in which a true hypothesis must be analyzed logically in relationship to that which is to be explained as a generalization of constant conjunction: "In essence, causal explanation works by subsuming events under causal laws (Elster 1983, p. 26)" (Kiser and Hechter 1991, p. 6). With no attention to the fact that the covering law model flatly rejects explaining by causal mechanisms, they demand just that: "A complete explanation also must specify a mechanism that describes the process by which one variable influences the other" (p. 5). Hence the first glaring incoherence: Kiser and Hechter at-

1 Bertrand Russell once described the law of causality as "a relic of a bygone age" (quoted in Enrikin 1991, p. 110); Popper dismissed Weber as naive for not recognizing that causality can only be a feature of universal laws (1959, p. 722).
tempt to combine a covering law model logic based on empirical regularities with a method of imputed causal mechanisms—exactly that which covering law model theorists reject as part of a legitimate explanation. 18

Here Kiser and Hechter stumble badly. Deep into the terrain of what their original authority (Hume) declared metaphysics and theology (realist views about causal mechanisms), Kiser and Hechter nonetheless express a positivist concern: "Like causality itself, mechanisms are not directly observable. . . . How then are they to be imputed?" (p. 5). They suddenly exit the world of realism and climb back onto Hume’s stolid antirealist shoulders and announce they have solved the problem of the unobservability of mechanisms by invoking event uniformity: "Whenever the source of some event is unobserved, we should proceed on the hypothesis that it fits a pattern of causal uniformity. Causal uniformity implies the existence of a lawlike relationship that holds between events" (p. 6). The incoherence thus deepens: as Kiser and Hechter themselves have argued (pp. 4–10, 15–17), hypothesizing event uniformity (as in the covering law model) is not the same thing as finding causal mechanisms, just as demonstrating that childhood cancer is clustered in dense electrical power zones is not the same thing as demonstrating that (via direct causal sequences) concentrated electrical power causes cancer. Limiting an explanation to event uniformity is, after all, the only way to remain on empirical foundations.

Thus caught in the problem of trying to join the antirealism of the covering law model (which rejects the viability of causal mechanisms) to the realism of rational choice theory’s need for mechanisms, Kiser and Hechter’s only solution is to demand a strict inferential relationship in which causal mechanisms are “subsumed” under the causal law (1991, p. 6). Rather than solving the problem of how to find mechanisms, however, Kiser and Hechter appear to be simply waiving the problem away by retreating back into the safety of lawlike relations. Having first called mechanisms the “enduring focus of sociological explanations” (p. 15), they now want to conflate them into the uniformity of invariant laws. Struck by the awkwardness of this attempted merger of realism and antirealism, we might now ask: Whatever happened to the second requirement for adequate explanation—the causal mechanisms?

18 There are other reasons to object to the necessity of mechanisms; for the most powerful, see Stinchcombe (1991). What Kiser and Hechter really mean is that there is a consensus over the need for mechanisms among rational choice theorists (e.g., Cole-
quirements of positivism forced theorists from Hume to Popper to reject
the search for deep causality beyond conjunction and probability, and
whereas relational pragmatist realists use causal effects to infer theoretical
entities, theoretical realism provides Kiser and Hechter a deus ex machina
by making theory “see” what their senses cannot: with “general theory as
the basis for the imputation of causal mechanisms . . . mechanisms must
be imputed from general theories” (1991, pp. 5, 15, 16). Following the
Lakatosian route out of Kuhn (through Popper) into the “hard core” of
“general theory,” theoretical realism thus allows Kiser and Hechter to ac-
complish by theoretical fiat that which has for centuries confounded phi-
losophers—to bypass the epistemological problem of how to establish true
causal explanation of mechanisms in the face of the problem of unobserv-
ability: “Models and mechanisms can only come from general theory”
(p. 19).

What and Where Is General Theory?
We have arrived with anticipation at “general theory”—the fount of Kiser
and Hechter’s “adequate explanation.” But, search as we might, it turns
out that Kiser and Hechter never tell us what is general theory. To be
sure, throughout they refer to it with a sprinkling of accolades designed
to contrast with the weaknesses they impute to historical sociology—“the-
oretical boldness,” “scope,” “generality,” “analytic power,” and “parsimoni-
ous” (1991, pp. 9, 21). But the closest they ever come to a definition is to
repeat that general theory is the “source” from which causal laws and
mechanisms are to be derived. Indeed on page 17 we are presented not
with a general definition but with a single case of a general theory—ratio-
 nal choice theory—and then informed that, to date, it is the only such
case (p. 23). This presents a dilemma: The weaknesses, or for that matter
the strengths, of rational choice—or any particular substantive general
theory—is not the purported subject of Kiser and Hechter’s argument,
who instead give as their subject the formal criteria of methodology. Yet
since discussion of these are missing at what turns out to be the crux of
their entire argument, they have left us no choice but to use the particular-
ies of rational choice to develop a “general” conception of general theory.
We can thus add to Kiser and Hechter’s difficulties: (1) by paying only
the small price of an inconsistency in logic, they have smuggled in their
preferred theory in the guise of general and neutral standards of epistemo-
logical adequacy, (2) they have thus violated the rules of epistemology
and methodology, which, by definition, are required to define the general

man 1990; Elster 1989). The mistake is not an accident since they consistently use
“theory” synonymously with rational choice theory.
criteria for sound knowledge and should not be derived from any one substantive theory, (3) and with this rather imperial move of conflating the specifics of rational choice with the definition of general theory across the board, they have closed off debate and preempted any possibility of methodological pluralism (Anderson 1993).

General Theory: Agents as Mechanisms

With these obstacles in mind, I begin by surmising what general theory is not. It is not in itself (although it may be used to impute) a set of testable propositions, either generalized from historical comparisons or deduced from empirical hypotheses. And it should not be (at the risk of tautology) either models or mechanisms, since Kiser and Hechter tell us those derive from general theory. In fact, in keeping with the hybrid character of their overall approach, Kiser and Hechter's general theory turns out to be a peculiar mix of aspirations—combining a methodological commitment to general laws (usually built on a staunchly empirical methodology) with a radically antiempirical theoretical ontology. Specifically, rational choice's general theory is a definitional bundle of assumptions about the nature of reality—a body of axioms that postulate the essential causal properties and powers of the social world. These essential properties and powers are mental states embodied in the intentionality of the individual agent: "All rational choice explanations begin with the assumptions that individuals are purposive and intentional actors who pursue prespecified goals . . . in rational choice theories, interests are specified a priori" (pp. 19, 21). Given metatheoretical form by Lakatos's (1970) influential theoretical realism, these assumptions are the hard core of the rational choice research program.

This conception of general theory is a classic case of what is called an "ontic methodology" (Salmon 1984); it is the axial core of all theoretical realism. The term is itself a hybrid: First it refers to an explicit ontology—a postulate about the nature of the social world. In keeping with theoretical realism's dismissal of the positivist prohibitions against nonempirical postulates in social science explanation, the ontic approach explicitly opposes positivism's privileging of epistemology over ontology—strict rules required to justify a knowledge claim ruling out any claim to theory and knowledge based on postulated theoretical entities. Theoretical realism thus begins with what positivism excludes: an empirically unknowable, and unfalsifiable, ontological theory of reality as a causal structure—an a priori argument about social reality based on "the idea of the essential causal powers of kinds of things" (Lloyd 1986, p. 8). In keeping with its antipositivism, ontic approaches are inherently driven by theoretical realism's fundamental distinction between "appearance" and "reality"—a dis-
tinction as old as rationalism itself. Appearances are the mere “events” accessible to the senses of the of the observer; to mistake these for “reality” at the level of deep causal structure is to be empiricist. The theoretical realist, by contrast, privileges as the real source of causality the deep ontic structures and mechanisms of reality that are not accessible to the senses (Bhaskar 1979, 1986, 1989; Putnam 1997, pp. 181–84).

That rational choice has a core ontology is not in itself surprising; the protestations of some social scientists notwithstanding, all theories of knowledge make a more or less explicit ontological choice between either the individual or the social structure as the basic unit of social analysis (Alexander 1982). What makes Kiser and Hechter’s approach different from a simple ontology is their combination of ontology with methodology—specifically, with rational choice’s commitment to theorizing causal mechanisms. The result is not just a commitment to individualism, which could just as easily apply to interpretivist approaches. It is a commitment to a causal ontology in which agential intentionality is posited to be the a priori causal force/mechanism at work in the social world (Elster 1983, chaps. 1–3; 1989). An agent-centered ontology, to be sure, but more important is that as part of its very definition of reality it attributes exogenous causal power—via the property of mental intentionality—to its agential units of analysis. What completes the hybrid of an ontic methodology, then, is that a causal explanation (not just a postulated description) is not hypothesized but already built into the definition of social reality. This is a general theory—more precisely, an ontic explanatory presupposition less about agents as rational and more of agents as mechanisms.

Since all explanatory requirements flow from this presuppositional causal ontology, it is not rationality per se that predominates or even does the main explanatory work in rational choice’s general theory; much more central is the causal capacity attributed a priori to the individual agents through the essential disposition of intentionality—which is endowed with inherent and exogenous causal power. Such an inherently causal ontology that postulates its agential units to have exogenous causal powers and generative forces makes use of what Hempel (1965, p. 472) called “broadly dispositional traits”—a term that evokes the ontological causal necessity characteristic of certain dispositions. The dispositional traits of intentionality and purposiveness are the essential but unobservable powers and properties I have stressed to be at the core of theoretical realism. As such, they fit the category of what philosophers call “reasons as causes” (Hempel 1962; Rosenberg 1988, pp. 27–30). Because they are dispositional, these mechanisms cause consequences with lawlike and self-

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19 The locus classicus on “reasons as causes” is Hempel’s “Rational Action” (1962). For discussion see Dray (1957), Rosenberg (1988), and Bohman (1991).
propelling necessity: It would be no more possible (all things being equal) for the agents of rational choice theory not to be driven by their intentional purposes than it would be possible for glass not to break on impact—given its essential dispositional trait as “brittle.”

At the heart of this general theory, then, is an ontic definition of reality composed of discrete agential units each endowed a priori with the dispositional causal power of intentionality. The explanatory work of the theory is carried by this invariant causal mechanism of a dispositional agential intentionality that necessarily (in the absence of constraint) causes intents to convert into actions be they rational or nonrational. Kiser and Hechter’s general theory has thus emerged to be a hybrid straining to make the center hold: It aspires to the model of the covering law, but it is a law in which the crucial explanatory causal mechanism is an a priori theoretical and ontological entity that is postulated exogenously. Rosenberg (1988, p. 23) calls this kind of law “folk psychology.” Rather than hypothesized, discovered, problematized, deduced, induced, or generalized, the explanatory content of this law comes ready-made; causality is inscribed exogenously. Independent variables are usually hypothesized rather than postulated, but in rational choice theory intentionality as causality is not only defined in advance but defined as the universal cause of all known effects. The causal mechanisms built into rational choice theory are always the same: Agents equipped with intentional dispositions that, universally and necessarily, are charged with a self-propelling momentum from cause/intentionality to effect/action. General theory, then, for Kiser and Hechter is a theory of agents as mechanisms believed to constitute and, by general universal law, to embody in their consciousness the exogenous and ontological causal power and force of intentionality that drives the social world.

From General Theory Back to Laws and Mechanisms

We have now reached the apex of a journey in search of how to produce “an adequate explanation” (Kiser and Hechter 1991, p. 4). It began with a call for laws (constant conjunction), argued it necessary also to find deep causes (causal mechanisms), and, having faced the problem of the unobervability of mechanisms, retreated back to laws and from there elevated to “general theory”—an ontology with a built-in explanation of agents as mechanisms. But what we have now learned is that they are also the very

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20 According to rational choice theory, there will of course be constraints and counterforces that serve as obstacles in any given empirical example; but they will be just that—external constraints on an otherwise natural dispositional force. Hence the ceteris paribus clause. The example of brittleness is found in Bohman (1991, p. 20).
same mechanisms that Kiser and Hechter tell us are the central component of all adequate explanation. What now? As a set of given assumptions, general theory can hardly be accountable to the petty trials of empirical confirmation or problematization; instead, now that we have found the source of the models and mechanisms in this general theory, we are about to start descending again: “Beginning with these basic assumptions, rational choice political sociologists then apply one or more of the causal mechanisms derived from available [rational choice] theories—such as power-dependence theory, repeated game theory, optimal location theory, agency theory, and group solidarity theory—to the problem at hand” (p. 19).

This is remarkable: The very causal mechanisms required for an adequate explanation of a problem at hand are to be inferred from a general theory composed of assumptions about those same causal mechanisms. Since the causal mechanism of intentionality is already inscribed—regardless of the specific case or the outcome being problematized—Kiser and Hechter predefined what should be an empirical question, namely, the question of causality in any given case. The tautology is inexorable: At the level of microtheory, because individuals are constituted in essence by the causal trait of intentionality, it would be impossible to explain any given action without attributing causality to their dispositional intentionality. And at the macrosocial level, likewise, it would be impossible to explain social outcomes without attributing causality to agential intentionality. With causal mechanisms already inscribed in the theory from which they derive these very mechanisms, Kiser and Hechter’s ontic methodology gives new life to Plato’s lament about the search for hidden truth: If we know what we are looking for, we have already found it; if we do not know, we cannot recognize it when we do (see Hollis and Smith 1991, p. 408).

Having It Both Ways
Kiser and Hechter have taken us on a dizzying circular trip: they have told us that something called general theory can give us the causal essence of a phenomenon, and from this essence they are handily able to explain a social problem by imputing to it the same causal property of dispositional intentionality that already lies at the definitional core of the general theory. The tautology lies in the absence of an independent explanatory

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21 Tilly (1995c, p. 1595) spells out precisely the inexorable and tautological progression of this structure of explanation: “(1) assume a coherent, durable, self-propelling social unit; (2) attribute a general condition or process to that unit; (3) invoke or invent an invariant model of that condition or process; (4) explain the behavior of the unit on the basis of its conformity to that invariant model.” This is Tilly’s characterization
structure at least partially derived from the problem at hand. Because the explanation is already inscribed in the a priori definition of the problem, we are presented at the end with “little more . . . than the explication of a definition” (Tilly 1995c, p. 1595). Their commitment to laws notwithstanding, nothing could be more incompatible with standard positivist rules of deduction requiring testability and the capacity for falsification—or necessarily adjudicated by observation language.

When it comes to what counts as an adequate explanation, then, Kiser and Hechter try to have it both ways, in more ways than one. First, by combining the covering law model with the demand for causal mechanisms they aspire to the certainty of law with the elegance of theorizing deep causality. Then they try combining the whole mix with an axiomatic ontology. The problem is that the covering law model logic they rely on cannot be combined with theoretical realism’s presumption that once a theory is determined to be true, the terms of the general theory denote the entities that are causally responsible for what we observe. The incompatibility derives from rational choice’s theoretical realist definition of general theory as constituted by essential general properties, propensities, and powers—mechanisms, in short. By claiming to be able to explain by deriving both lawlike regularities/conjunctions as well as causal mechanisms from an axiomatic general theory, Kiser and Hechter are trying to combine positivism’s antirealism with rational choice’s ontic realism and in turn to deduce “how” mechanisms from this definitional theory—in advance of any empirical investigation—and not by engaging with the phenomenon itself. With this deeply anti-Humean/antipositivist solution in theoretical realism, Kiser and Hechter ultimately define general theory not by the Humean criteria they originally promised, but rather in anti-Humean realist terms as essential general properties, propensities, and powers. There is obviously no epistemological law that one must be Humean or positivist. Coherence, however, does require that since it is Kiser and Hechter’s own choice to begin with Hume, and in turn to take on board the problematic of observability, they cannot claim to have found a coherent solution in what is a radically anti-Humean conclusion—a general theory composed of favored axioms about the imputed nature of reality.

CAUSALITY AND HISTORY

It would be churlish not to note the advance in Kiser and Hechter’s desire for an adequate explanation to include mechanisms. In the covering law

of standard ways “to explain political processes”—not one that he applies specifically to theoretical realism. Yet it captures especially well the postpositivist deductivism of rational choice.

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model, mechanisms are excluded not only because of problems of observability, but also because causal processes are considered too historical and particularistic, thus insufficiently logical and generalizable (Popper 1959; Salmon 1984; Bohman 1991; Miller 1987). Theoretical realism, by contrast, does not rest content with positivism’s restrictive demands for observability as the criterion for explanation and insists, rightly, that cause cannot be ascertained by conjuncture alone; some understanding of mechanisms must be developed—how it is, for example, that the constant conjuncture of social revolutions on the one side and, on the other, a conjuncture of peasant rebellion and state breakdown, is actually a relationship of cause and effect? By insisting on mechanisms, then, Kiser and Hechter are to be commended for giving due importance to the processual and explanatory element of causality. Perhaps this is reason enough not to criticize the incompatibility of theoretical realism and classical positivism.

The apparent victory for the processual element, however, is Pyrrhic. Faced with the inability to observe causal processes, theoretical realism solves the problem by “observing” through the lens of theoretical assumptions the putative properties and propensities of unseen entities (e.g., the drive of human intentionality). No sooner do Kiser and Hechter turn to causal mechanisms than they declare that mechanisms can only be deduced from prior “omnitemporal, universal” laws, and then that both laws and mechanisms must find their source in a general theory whose analytic power lies precisely in its temporal abstraction (Kiser and Hechter 1991, pp. 6–7). This move allows their theoretical realism to reconcile its trafficking in metaphysics with the positivist goal of generating lawlike theory. The kinds of deductions about mechanisms that theoretical realism generates, given the definition of general theory as assumptions about essential properties internal to a contained entity, are deductions conforming to the same principles of omnitemporality, universality, and uniformity as those postulates of the general theory from whence they are derived. The result of this series of moves is an even stronger embrace than the original positivist one of the antitheoretical caricature of history.

For Kuhn, positivism wrongly neglected history; Kiser and Hechter take this temporal void to a level much higher than Kuhn ever envisioned. Whereas the standard covering law model conceptualizes time as “general linear reality” (Abbott 1988), Kiser and Hechter aspire to what can be seen as a nonlinear reality that does not so much linearize time as much as freeze it entirely. Theoretical realism, in fact, accomplishes a remarkable feat by solving the problem of causality not in the direction of Tilly’s notion of “thick” causal time but in the direction of even greater invariance. Thus Kiser and Hechter “rule out historical narratives as a basis for the imputation of causal mechanisms,” and instead demand “minimal
temporality” in selecting causal mechanisms (1991, pp. 6, 7). With this, theoretical realism in general and rational choice in particular have managed to resurrect what many had hoped would remain a long moribund false dichotomy between theory, science, and knowledge on the one side, and history, temporality, and narrative on the other. Surely it is ironic to find two historical sociologists telling us that the ideal explanatory structure is the one with the least temporality or “thinnest” conception of time—the very thing that has “repeatedly led social scientists to the mistaken conclusion that historians are particularizers while social scientists are generalizers” (Tilly 1994, p. 270).

HOW DO WE KNOW IF A THEORY IS TRUE?

Kiser and Hechter require an adequate explanation to be based on causal laws and mechanisms, but there is something missing in this definition—namely, a convincing reason for why we should believe in any given explanation. Explanations built on unobservable mechanisms with real causal powers operating with natural necessity require methods of confirmation very different from those built on observational correlations. But there is no trace of such an alternative approach to theory justification in Kiser and Hechter’s argument. Positivists have always told us not to trust any theoretical claims that did not address the epistemological problem of how we can know whether a theory is true. Even if they were wrong in pointing to observation as the only foundation for claims to knowledge, they were nonetheless right to demand epistemological accountability. Kiser and Hechter’s chain of inferences from general theory, to laws, to mechanisms may be logical, but in a post-Kuhnian universe logic is not enough to make a theory true. Kiser and Hechter still owe us an epistemology—a convincing account for why we should believe them.

In the same vein as their approach to an adequate explanation, the epistemology Kiser and Hechter do offer is cobbled together from a hybrid mix of pre-Kuhnian antirealist deductivism, and an antipositivist theoretical realism. The antirealist elements they use are: (a) a logic of justification limited to the dichotomy of induction versus deduction; (b) the uncritical acceptance of the superiority of Popper’s version of deductivist logic. Their theory-centric realist elements are (c) the conflation of history, experiment, observation, and problem-driven research with “empiricism”; (d) an epistemology that tells us it is theory that adjudicates truth. The elements do not cohere: One cannot be committed to b, a standard positivist

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Since that writing, rational choice theorists have discovered narrative—but only endorse a theory-driven version; see Bates et al. (1997), Kiser (1996).
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deductivist logic of confirmation, in which truth is confirmed through observation language, and also adhere to c, a postpositivist theory-centered view of justification that builds on a theory-laden conception of data. Nor can a, a spectrum of justification limited to induction or deduction, be coherently joined with d, a theoretical realist epistemology in which truth is ultimately adjudicated by theoretical presuppositions.

The Limits of Pre-Kuhnian Deductivism

Kiser and Hechter lambaste what they allege to be the empiricist and inductivist practices of historical sociology (1991, pp. 5–9, 12–17). The philosophy of science, they claim, has ruled that inductivism is methodologically unacceptable (pp. 12–15), leaving deductivist logic as the only valid means by which to generate and confirm theory. Even within its own terms, however, Kiser and Hechter’s argument is simply wrong. Deductivism does not hold this position of unequivocal triumph in the philosophy of science. To be sure, the theory-centric route out of Kuhn described above did, until very recently, dominate the history and philosophy of science and, superficially at least, seemed to converge with the primacy of deductivism (Hacking 1983, pp. 149–66; Miller 1987, pp. 453–61). But this has changed dramatically in recent years as revisionist demystifications of the textbook stories have converged with Kuhn’s original criticism of both deduction and induction to undermine a disproportionately deductivist-driven view of scientific advance (see, e.g., Camic and Xie 1994).

A critique of deductivism however, also entails epistemological doubts independent of such recent corrections in the historical record. For one, Popper’s criterion of falsification relies on “underspecified criteria” of what counts as decisive falsification criteria (Hull 1988; Bohman 1991; Hollis 1994; Miller 1987), just as it neglects the fact that theoretical hypotheses often make no claims about observation directly and thus cannot be tested except when supplemented with other hypotheses. This allows an escape clause for the hypothesis of choice by simply attaching blame for a recalcitrant observation to flawed measuring devices or to the faultiness of the supplemental hypothesis, thereby leaving the favored one intact (Quine 1963, 1969; Hollis 1994, pp. 79–80; Rosenberg 1988, p. 47). It is obvious in the social sciences, moreover, that the test of falsification—in which a single counterobservation can falsify a theory—is virtually never practiced (consider Marx’s theory of class formation; see Somers 1996a); and for good reason, given that more than one theoretical construction can almost always be placed on a body of evidence (Kuhn 1970, p. 76).

Equally important is the frequency with which well-established scientific theories survive and thrive despite bearing the burden of rather
mind-boggling implications: Until the last few decades, for example, the theory that matter is made of molecules, when combined with the best-established theories of force, implied that no floor could hold a person’s weight. Deductivism fails here because it contains no criteria for distinguishing between respectable reasons for defending a theory against such anomalies and dubious tactics—for example, dismissing anomalies based on insufficiently similar conditions, yet without specifying the criteria for sufficient similarity (Miller 1987, p. 232). Deductivism thus provides no way to prevent dogma from thriving quite comfortably under the guise of science.

The obverse side of the stubborn ability of most hypotheses to survive the test of falsification is the even more privileged—and problematic—status of corroboration accorded to those theories for which disconfirming data has not yet been found. The theoretical indeterminacy of data, however, makes suspect the idea that there will ever be firm consensus on what data counts as disconfirming in the first place—thus setting almost no limits to the criteria for corroboration. Even more hubristic are claims to successful corroboration based on what is, as of yet, merely an absence of counterevidence. On this principle, the more abstract the theory and the less amenable to empirical falsification, the more likely it is to be considered valid. That planets of stars other than the sun are Camembert moons, for example, could be considered corroborated in the absence of any actual observations to the contrary. Through failure to falsify, “All emeralds are green” and “All emeralds are grue” have both been corroborated (Miller 1987, pp. 235–36), just as failure to yet falsify “all post-Soviet capitalist regimes lead to socialism” allows it to stand as corroborated.

What these problems point to is the difficulty of proving or disproving a theory on purely logical grounds—the essential core of the deductivist claim to scientific superiority. Yet this is precisely what Kiser and Hechter do. Skocpol (1994, p. 323), for example, rightly points out that Kiser and Hechter attack her work “not by dealing with any of its substantive ideas or findings, but by showing—in very general logical terms—that it does not live up to what they claim, in the abstract, any ‘adequate explanation must entail.’ ” They likewise dismiss on logical grounds a vast array of substantive findings in historical sociology—ranging from world economies to contentious movements—on the grounds that these findings have been produced through the failed logics of “empiricism” and “induction” (Kiser and Hechter 1991).

In one (by now well-worn) such example of a logical rather than a substantive criticism, Kiser and Hechter attack historical sociologists for their lack of ability to achieve case independence (p. 13). In addition to ignoring the work in network theory that takes issue with the logic of this criticism, their attack also presupposes that cases are discovered as ready made,
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discrete entities—given in the nature of things, that is—rather than constructed along analytic parameters. In problem-driven research cases are not selected from an external spectrum of possibilities; cases are defined by the problem the researcher has set out to explain. Historical sociologists, by and large, thus draw the lines of independence between cases based upon what it is they are trying to explain; case selection is an analytic, not a logical, operation. The goal of the research design is to establish that an object is an adequate empirical unity for the limited purpose of the problem at hand. Thus Skocpol reminds us that she “could and did make comparisons” between cases she carved out as temporal “episodes” of sociopolitical conflict within single countries because she did not passively assume she could only compare discrete and predefined “countries” (1994, p. 322). Rather, she actively defined her cases as analytic temporal episodes precisely so that she could test her problem-driven hypothesis about the impact of Russia’s state apparatus as it weakened over time (1905–17). Similarly, both Putnam (1993) and I (Somers 1993) draw theoretical conclusions by comparing contrasting political cultures across different regions of a single country—cases that would be ruled nonindependent (because in the same national society) according to an abstract logic that leaves unproblematized the crucial question, What is a case? (Ragin and Becker 1992). In all of these examples, arguments from abstract logic overlook the valid findings that emerge when cases are configured according not to the abstract logic of deductivism, but to “the logic of the causal hypotheses being presented and tested” (Skocpol and Somers 1980, p. 194). Independence is defined analytically according to what it is that is being problematized and thus along substantive, not logical, dimensions. Following Marc Bloch’s (1934, p. 81) maxim about the comparative method, “Only the unity of problem makes a center.”

Postpositivist Deductivism

Kiser and Hechter specify three criteria to adjudicate among competing theories: “plausibility, reduction of time lags between cause and effect, and the empirical implications” (1991, p. 6). Despite appearances, each of these criteria, including the third, is premised neither on logical nor empirical grounds but on the theoretical ones of rational choice theory, thus suggesting that, for Kiser and Hechter, the test of a good theory is whether it meets the criteria of the particular theory favored by the researcher.

What counts as causal unity is an empirical question and so can change over the course of research. Very often one starts with commonsense causal unities—e.g., people have intentions—that with additional information can be transformed or extended to less commonsensical ones—e.g., firms have intentions.
This represents a hybrid of antipositivist theoretical realist deductivism the unworkability of which is manifested in the circular reasoning they employ in discussing these criteria. First there are criteria based on logic: “The success of such studies ultimately rests on the degree to which they meet the requirements of good causal explanations” (Hechter 1992, p. 368). Then, in light of the recognition that one of their logical criteria—providing causal mechanisms—relied on unobservables they turn to theoretical realism: “We cannot go out and collect the data [on causal mechanisms]. . . . On the contrary, this is what we need general theories for” (Hechter 1992, p. 368). This logic generates inferences that tautologically confirm their hypothesized ontology of the world, thus allowing theory to provide the data by which that same theory is judged.

Consider next the criterion for theoretical success being the greatest reduction of time lags between cause and effect. Citing Elster (1983, p. 24) Kiser and Hechter state that the best way to reduce the time lag between cause and effect is to replace macrovariables by microvariables or to link macrolevel variables using intervening microlevel ones (p. 7). But in this context microfoundations are not logical criteria; they are the central metatheoretical principles of rational choice (see, e.g., Elster 1989). It thus follows inexorably that their criticisms of historical sociology (p. 16)—historical sociology’s “inadequate microfoundations”—turn out to be neither logical nor empirical, but disagreements among rival theoretical views of the world. Kiser and Hechter are telling us, unabashedly, that the presence of microfoundations must be the general epistemological criterion for the success of a theory. In this instance, mine is not a complaint against rational choice; it is an complaint against an epistemology that adjudicates the success of any given theory by axioms derived from the very essence of that theory.

With their third criterion of testability, Kiser and Hechter now appear to follow Friedman’s (1953) “positive economics” in which axiomatic assumptions are just that—axiomatic “as if” heuristics, not meant to be realistic but designed to meet the instrumental test of producing testable predictions (see also Bell 1980). From this perspective it matters not that hypotheses are inspired by unrealistic assumptions as long as they are kept logically distinct from the methods of justification by which the hypothesized predictions are to be tested—thus falling back on Popper’s distinction between discovery and justification. But Kiser and Hechter

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24 Putnam (1993), not unsympathetic to rational choice, would disagree. After extensive testing he found the 14th century to be critical for his explanation of civic democracies, just as I found, after testing three competing hypotheses, that England’s 12th-century legal foundations were central to citizenship formation (Somers 1994b). And for another very long-run causal analysis see Tilly (1990).
are also postpositivist theoretical realists. Realists, following Kuhn, deny this distinction on the premise that in deciding which facts to select and transform into evidence we are already to a large degree deciding among rival theories (Hollis 1994, p. 79). Kiser and Hechter, moreover, make it clear that their use of general theory is much more than heuristic; it is the source of the causal mechanisms they will use to describe and explain the world. To concede their general theory as no more than heuristic or instrumental (in the Friedmannian sense) would be to concede to positivism’s antirealist foundational premise that true causal knowledge about the deep structures of reality is impossible to establish. With this concession would collapse the entire rational choice project.

This dilemma is not unique to Kiser and Hechter but characteristic of rational choice as a whole. When rational choice theorists justify the “as if” status of their assumptions on the grounds of instrumental utility in generating predictions, they inevitably contradict their simultaneous demands for theory structured by either general covering laws or by (true) causal mechanisms. Referring to the first incompatibility, Green and Shapiro point out that “either the development of general theory is justified on covering-law grounds (in which case it cannot legitimately be based on unrealistic assumptions), or the unrealistic is justified on instrumental grounds (in which case the particular mode of theory building is beside the point; testable predictions are what matter)” (1994, p. 31). And, with respect to the second, one cannot claim that theory is both the grounds for establishing a description of reality on the one hand and, on the other, a mere analytic language of heuristics for which the criteria of realism is unnecessary to meet its prediction-generating goals. As Friedman would be the first to concede, causal mechanisms telling us how something happened cannot be deduced from “as if” assumptions designed to produce only empirical regularities (Friedman 1953).

On close examination, even Kiser and Hechter’s criterion of testing via “empirical implications” turns out to be more theoretical than empirical as they appear to test the power of a theory to predict something the theory is already premised upon—the free rider problem is one such example (pp. 8–10). The limits of these overly theoretical criteria cannot be overcome by the claim that rational choice predictions test “retroductively” (Fiorina and Schepskle 1982, p. 63). One reason is that in the social sciences when theories are judged retrospectively, more than one almost always will be interpreted to predict consistently with the facts. Another is that the kind of consistency Kiser and Hechter claim is not one in which an empirical puzzle is explained by a theoretical model that fits both positive and negative cases of the same phenomenon. Rather the kind of prediction and testing that Kiser and Hechter use to exemplify the superiority of rational choice’s theory-driven approach over the problem-driven one of
most historical sociologists, is a post hoc one: Facts that are already known are explained in an account fashioned to be consistent with rational choice’s a priori postulates about essential (thus ultimately untestable) faculties and properties of theoretical entities such as intentionality. Such post hoc explanations that are consistent with a theory’s original assumptions amount to little more than the ability to rewrite in a “tribal language” (Skocpol 1994, p. 325) what others have laboriously discovered. Moreover, given the “lack of specificity about what it means to be a rational actor, it is not obvious what sorts of behaviors, in principle, could fail to be explained by some variant of rational choice theory” (Green and Shapiro 1994, p. 34; emphasis added). Telling a rational choice story about data that has already been observed hardly qualifies as passing the testability that Kiser and Hechter repeatedly demand of adequate theorizing. “Data that inspire a theory” we are reminded by Green and Shapiro (1994), “cannot . . . properly be used to test it, particularly when many post hoc accounts furnish the same prediction. Unless a given retroductive account is used to generate hypotheses that survive when tested against other phenomena, little of empirical significance has been established” (pp. 35–36).

Finally, Kiser and Hechter use yet another set of timeworn criteria for good theory—“parsimony,” “universality,” “scope,” and “boldness.” But these fare no better than the testability claims. Why should we prefer what are, after all, aesthetic qualities to, say, the “complexity, expansiveness, and historicity” advocated by Dessler (1989, p. 446)” or by Hirschman (1984) in his classic statement, “Against Parsimony”? The only convincing reason, as Hollis (1994) reminds us in referring to Friedman’s (1953) similar demand for parsimony, is that they believe these qualities actually capture the reality of the world being theorized—that is, the world really is parsimonious and invariant. One could take issue with this ontology, as indeed I do below, but certainly one can neither confirm nor deny it—thus making it a justification wholly at odds with Kiser and Hechter’s original premise that the ultimate test of a theory is its empirical testability.

To sum up: In their criteria for theory confirmation, Kiser and Hechter splice their postpositivist theory-centrism together with the positivist language of deduction. Their assumption that the only available alternatives in theory construction are the inductive/deductive dichotomy at first glance appears to make them merely strong positivists on the deductivist side of the debate. But standard deductivist logic confirms/tests a theory by observing the world—so much so that, as I have emphasized, it dismisses the possibility of justifying knowledge claims in the absence of

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15 “The richer and more comprehensive the underlying ontology, the better the theory” (Dessler 1989, p. 446).
observable evidence. Kiser and Hechter’s version of deductivism, by contrast, is the aggressively antipositivist deductivism of theoretical realism. It does not fit Popper’s requirements for theory testing by data (“conjectures and refutations”) for it is a deductivism in which hypotheses are not only guided but *adjudicated* on the basis of general theory. A post-Kuhnian theoretical realism combined with a pre-Kuhnian deductivism thus creates a hybrid suspended between, on the one side, what Kiser and Hechter call “empiricism,” and, on the other, a theory centrumism removed from the need to observe the world.

We have now come full circle. General theory is both the judge and the jury in an epistemology that, to paraphrase Hollis’s (1994, p. 36) remarks on rationalism more generally, makes the purpose of a theory (of action, e.g.) to find the essence of any given phenomenon (a given action) by defining that concept (of action) in a way that (inevitably) captures that essence. In the pattern of this rationalist epistemology, Kiser and Hechter dismiss what they define as historical sociologists’ overly great concern for accuracy and empirical detail as “naïve empiricism” when they compare it against the standard of their disproportionately deductivist view of theory. Here is an epistemology truly grounded in the assumption that the test of a good theory is its ability to trump observation on terms set by the same theory. A theory is thus to be admired when it “outpaces” the data that is to be placed in its pigeon holes (Lakatos 1970). The problem remains: We still have been given no good reasons to believe in it.

THEORETICAL REALISM: AN ONTOLOGY FOR THE ANGELS

In his recent discussion of how to explain political processes, Tilly (1995c) criticizes sociologists’ widespread use of “invariant models concerning self-motivating social units” (p. 1596). His criticism is important and unusual; in lieu of the common exclusion of ontology (“the nature of that which is to be known”) from discussions of social science methodology, Tilly insists that because ontological assumptions inevitably influence epistemology (“the conditions for the generation of knowledge”), it is crucial that those assumptions be “plausible” (p. 1602). In keeping with this approach and picking up on my discussion of rational choice’s “ontic methodology” as well as its use of “reasons as causes” and “agents as mechanisms” (see pp. 749–51 above), let us examine first how Kiser and

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25 Tilly is hardly alone in recognizing the centrality of ontological assumptions on methodology. See, e.g., Alexander (1982), Giddens (1984), and all of the literature on realism (e.g., Bhaskar 1979, 1986, 1989; Lloyd 1986, 1993).
Hechter relate their epistemology to their ontology, and in turn take seriously their own suggestion (1991, p. 6) to consider the "plausibility" of the ontology they do embrace.

In the first instance, rather than making their form of explanation dependent upon the character of the entities whose existence they are prepared to defend, Kiser and Hechter derive their ontology from their prior claims about what logical forms of explanation are acceptable. This strictly positivist approach reflects a three-tiered method that exhibits the following logic: (1) ontological claims ("X exists," "X has these properties") are licensed only once they have been validated by a general theory of X; (2) we can capture the true intrinsic character of X (i.e., the properties, including causal powers, that X possesses) only through causal relations that relate X's to Y's to Z's properties according to the covering law model; (3) since we already know X's ontological properties through the postulated causal assumptions applicable to any given X, it is not necessary to examine empirically the causal mechanisms through which Xs are related to Ys. (In rational choice theory, recall, causal mechanisms are held to be derivative from and entailed by X's a priori ontic postulates of intentionality.)

At odds with positivism's privileging of logic with which they started, Kiser and Hechter thus ultimately ground their logic on a realist premise—that laws and mechanisms must be imputed from general theory. Since, as I stressed above (pp. 749–52), their general theory turns out to be an exogenous ontology of the causal structure of reality, in this second instance they are donning their antipositivist theoretical realist hats and rejecting the priority of logic by putting their ontology first. If, as Tilly suggests, this influence of a prior ontology on one's epistemology is to some degree inevitable, then indeed we must ask, How "plausible" is their ontology?

The basic elements of rational choice are well known—that actors apply the standards of means-ends rationality, that they are self-interested, and they are wealth maximizers. These claims have been challenged forcefully at every turn and from every conceivable angle, and it is not my purpose to repeat those challenges. However, as I have stressed throughout, these substantive elements are not the foundational core of the theory. More important is that which is at the heart of Kiser and Hechter's ontic methodology. Recall the basic agential units of analysis in rational choice: Not just agents, qua actors, but agents that come already

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equipped with the essential properties of purposiveness and intentionality that will by dispositional necessity (Hempel 1965; and see above pp. 750–51) cause them to “pursue prespecified goals” (Kiser and Hechter 1991, p. 19). What emerges, then, is that the basic agential units are in themselves causal forces—what Tilly (1995b, p. 1595) calls “self-propelling” entities, and what I have called above “agents as mechanisms.” The theory that posits its causal mechanisms to be exogenously borne by human agents is also necessarily an ontology that assumes its agents to be inherently self-galvanized by self-contained autonomous mental states (reasons)—thus making them “coherent, durable, selfpropelling social units—monads” (p. 1602).

But it was no less that Karl Popper (1959) who famously took issue with what he called “essentialism”—a philosophy which looks to the “essence” of things for information about their “true” nature and behavior. An essentialist approach treats its objects of analysis as constituted by a set of inherent attributes—attributes intended to represent the essence of a thing. For Kiser and Hechter and rational choice, this essence is the exogenous causal power of intentionality. Essentialist ontologies, in this sense, are recognizably presocial, almost Hobbesian (Pizzorno 1986, 1991). They posit not only fixed solipsistic identities but ontological entities that are born preequipped to act through essential inherent causal mechanisms (reasons as causes) that drive action on their own autonomous momentum. Questions we would want to ask about this ontology include: How we can know or justify knowledge claims about the essence of unobservable mental states in the first place? Can the single putative property of intentionality define a human being, let alone explain or predict human action? How is it possible to claim social agency for an identity if its motivating force derives from apparently presocial or fixed categories constructed from exogenous attributes (Somers and Gibson 1994, p. 55)? And is it not possible that these “essential” identities are, as White (1992, p. 8) points out, “by-products of previous history adapted to current circumstance . . . not causes, but rather . . . spun after the fact as part of accounting for what has already happened”?

Thus postulated on a dispositional ontology of theoretical entities (states of mind) with a priori causal mechanisms (the intentionality of agents as mechanisms), I find implausible Kiser and Hechter’s perception of the social world—namely, that it really is composed of agents with essential and unchanging properties that operate independently of the very relationships by which they are constituted. The implausible implications of an ontology in which causality is dispositional, rather than investigated, point to a world of invariance, a “world in which whole structures and sequences repeat themselves time after time in essentially the same form”
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(Tilly 1995c, p. 1602). Kiser and Hechter’s ontology of agents-as-mechanisms invokes a world populated by “angels”—ontological entities that, implicitly, are not of this social world. Invariantly constituted by the singular dispositional drive of attributes and essential intentionality, these agents are defined as having discrete and autonomous states of mind constituted by causal mechanisms that are not only given a priori by theoretical assumptions, but inexorably do their work in isolation from other entities or processes. No wonder Kiser and Hechter are so confident of their predictive capacity: A world of angels possesses few surprises, founded as it is on invariant properties and self-propelling causal powers that are knowable a priori. To be sure, as Tilly ponders, “If the social world actually fell into neatly recurrent structures and processes . . . invariant model and the testing of deductive hypotheses would become more parsimonious and effective means of generating knowledge” (1995c, p. 1602; emphasis added). The problem, however, is that while this “would be a convenient world for theorists . . . it does not exist” p. 1596).28

Rational choice theorists seem to disagree. As theoretical realists they presume the possibility of “real and relatively atemporal” objects of analysis that lend themselves to “objective and progressively successful discoveries and explanations of them” (Lloyd 1986, p. 9). For theoretical realists this conception of the categorically inherent properties of reality is the final adjudicator of reason. This assumption generates logical inferences that tautologically confirm their hypothesized ontology of the world—its universal, omnitemporal nature.

Kiser and Hechter’s theoretical realist ontology is at root a metaphysics—an a priori belief that the social world really can be theorized as comprised of universal, invariant, entities with discrete self-propelling causal dispositions and agents as causal mechanisms. There is an irony in this: As realists, Kiser and Hechter very much believe that a real, mind-independent world exists “out there.” Yet, absent the relational, contingent, and causally indeterminate element of thick time and being, it becomes a thin world of pure idealized spirit (Stinchcombe 1978, p. 21). At the end of the day, one can only agree with White that Kiser and Hechter’s rational choice vision rests on “an underlying ontology of ‘spirits’ . . . upon angels, that is, upon spirits both disembodied and independent. . . . [The] goals or preference orderings . . . essential to . . . rational choice, are appropriate and relevant only to entities which are inertial as well as isolate—

28 Hence the great success of Green and Shapiro (1994) is to show that rational choice has failed empirically in at least three of the most important areas it has theorized in political science. See also Friedman (1996) for a range of such critical perspectives.
angels, in short” (White 1992, p. 301). A world of angels may be parsimoni-
ous and convenient to theorize, but—disappointing, to be sure—we’re no an-
gels.

RELATIONAL REALISM
Relational and pragmatic realism is a post-Kuhnian perspective for the
rest of us—those who are not angels. It should be understood as a mini-
malist realism—minimalist because it recognizes that the partial concept-
dependence of social life puts limits on the general realist premise of the
absolute mind-independent status of the social world; yet realist nonethe-
less, in contrast to hermeneutics or radical constructivism in that some
degree of concept-dependence does not in any way subvert the premise of
a social world that exists independently of our beliefs about it. Relational
realism thus has three limiting principles: First, belief in the causal power
of a theoretical social dynamic (e.g., gender, utility maximization, class
struggle), is independent from belief in any one particular theory. Follow-
ing Kuhn’s (1970, pp. 173–74) mandate to think of theory advance as
movement from present knowledge, rather than toward absolute truth,
this injunction endorses Taylor’s (1989) notion of “epistemic gain” in
which knowledge is understood to be limited to “movement from a prob-
lematic position to a more adequate one within a field of available alt-
ernatives” (see also Calhoun 1995, p. 36). Second, all theory, especially one
like rational choice that is premised on an a priori causal ontology, must
provide an epistemology—some very good reasons—for why we should
believe it (Hollis 1994; Hollis and Smith 1991, 1994). And third, there are
no universally valid principles of logical reasoning; there are only problem-
driven ones (Kuhn 1970; Lloyd 1993; Miller 1987, p. 486). Since space
prohibits me from detailing the complexities of relational pragmatic real-
ism, I merely sketch out and signal the markers of this route that has
begun to bear out the potential challenge of a historical epistemology.

A Relational Realist and Pragmatist Ontology
A relational realist and pragmatist ontology is for those of us who accept,
however unwillingly, the brutal fact that we and our social world are not
angelic, existing outside time and space, but living, breathing, changing,
dying creatures and entities, embedded in time and constituted—not
merely engaged—in relationships. Beginning with the postulate that we
are neither monads nor self-propelling entities but “contingent, transitory
connections among socially constructed identities” (Tilly 1995c, p. 1595),
a relational pragmatist ontology takes the basic units of social analysis to
be neither individual entities (agent, actor, person, firm) nor structural
wholes (society, order, social structure) but the relational processes of interaction between and among identities (Collins 1981; Pizzorno 1986, 1991, 1995; Stinchcombe 1991, 1992, 1993, 1995; Tilly 1995a, 1995b, 1995c). Insofar as professional historians have developed ways to represent these processual and relational characteristics of "the nature of that to be known," relational realists borrow freely from what has traditionally been historians' terrain—through the appropriation of narratives, for example, as a way to represent sequences and processes over time (Abbott 1992a, 1992b, 1993, 1995; Aminzade 1992; Griffin 1993; Somers 1994b, 1996b). They do this not because they embrace historians' norms as Kiser and Hechter accuse; if they choose narrative forms it is because they hold the ontological belief that the world is made up of things that are constituted through temporal and spatial relationships and thus must be represented in relational and narrative terms (Wallerstein 1991). Hence, for example, agency can be usefully represented through the concept of a narrative identity to signify it as "processual and relational . . . [it] embeds identities in time and spatial relationships," and analyzes all identities "in the context of relational matrices because they do not 'exist' outside of those matrices" (Somers and Gibson 1994, pp. 61, 65).

A relational ontology thus follows Popper's rejection of essentialism and instead looks at the basic units of the social world as relational identities constituted in relational configurations. In place of a language of essences and inherent causal properties, a relational realism substitutes a language of networks and relationships that are not predetermined but made the indeterminate objects of investigation. Relational subjects are not related to each other in the weak sense of being only empirically contiguous; they are ontologically related such that an identity can only be deciphered by virtue of its "place" in relationship to other identities in its web. What appear to rational choice theorists to be autonomous agents defined by their attributes are thus better reconceived as historically shifting sets of agential relationships contingently stabilized in sites.

One need not be a historical sociologist to embrace such a relational ontology: it entails no more than the premise that identities, and hence potential mechanisms, are constituted—not merely constrained—in variable relationships. Following Adams (in press), it does not require a rejection of rationality but simply treating the "disposition to act rationally as a variable rather than a postulate" (p. 26). Thus, as per Smelser (1992), it requires us to conduct research into "the question of the contextual conditions—motivational, informational, and institutional—under which maximization and rational calculation manifest themselves in 'pure' form, under which they assume different forms, and under which they break down" (p. 404). And of course what is true of the variability of rationality is equally true of relationality: "Relationships may be more or less bonded,
the experience of them may be more or less constricting or enabling... but this is a question of contingency... [for] relationality is an analytic variable instead of an ideal-type" (Somers and Gibson 1994, pp. 61, 65; emphasis in original).29

Relational Realism and Adequate Explanation

What counts as an adequate explanation for relational realism can be distinguished in several ways from theoretical realism in its convergence with elements of a post-Kuhnian historical epistemology. First, the basic mechanisms of causality are not within discrete agents, but in the pathways of agential interaction or what Stinchcombe (1991, 1992, 1993, 1995) calls and demonstrates to be contingent and empirically variable “situational mechanisms.” Relational realism thus takes on board rational choice’s criticism of functionalism’s neglect of mechanisms without reverting to the ontic methodology of what I have been calling agents-as-mechanisms. That causal mechanisms are in contingent relational pathways rather than in the minds of self-propelling agents is supported by the social sciences’ increasing recognition of the necessity of explaining outcomes through path dependency and sequence analysis (Abbott 1992b, 1995; Aminzade 1992; Arthur 1988, 1989; David 1985; Aminzade 1992; Tilly 1995a, 1995c; Putnam 1993; Sewell 1996; Stark 1992), and causal narrativity (Somers 1994b, 1996b). Adumbrated in Kuhn, and brilliantly demonstrated in David’s (1985) renowned economic analysis of the QWERTY keyboard, path dependence and causal narrativity incorporate a sense of thick causal time in which “for any given trajectory, past choices and temporally remote events can help to explain subsequent paths of development and contemporary outcomes” (Aminzade 1992, p. 462; see also Skocpol 1979). When incorporated with comparative trajectories (as is ideal) these methods allow us to hypothesize, theorize, and test the causal status of previously embedded institutional practices; 14th-century legal institutions, for ex-

ample, can be shown under certain initial and subsequent conditions to neither disappear nor become "lag effects" with the onset of capitalist markets in the 18th century, but to be causal factors in the development of 19th-century democratic institutions (Somers 1994b; see also Putnam 1993). Path dependence and causal narrativity suggest that earlier institutional processes are "sedimented" into the core of some of our most modern phenomena; they thus help to explain why some aspects of the social world, and not others, became modern in the first place. One of the most significant contributions of these processual and relational methodologies is that it makes it possible to theorize not just social change (long the sociologist's fixation), but the equally important phenomena of social durability and institutional patterns of resistance (e.g., the QWERTY keyboard, or the English common law). And although no one has done more than Abbott to demonstrate how an explanation that depicts mechanisms is best constructed through narrative sequential structures, it is telling that it was Mann (1986), Skocpol (1979), Stinchcombe (1978), and Tilly (1984, 1990)—four of Kiser and Hechter's core targets—who were among the earliest to demonstrate empirically and historically that outcomes could only be fully accounted for through causal explanations that incorporated degrees of path dependency.

The second aspect of a relational realist explanation decouples the two aspects of cause that form the centerpiece of Kiser and Hechter's argument—laws and mechanisms. Relational realism insists that "how" questions of causality (processes/mechanisms) cannot be deduced either from invariant laws (constant conjunctions/relations) or from the exogenous essential causal properties and powers that constitute Kiser and Hechter's notion of general theory. Relational realists justify uncoupling mechanisms from laws by (a) the contingent and indeterminate nature of the basic causal mechanism (the relational unit of interaction) and (b) the fact that the adequacy of an explanatory structure depends upon why a question is asked: Different research aims yield different explanatory demands. Laws may be feasible for prediction, but only causal mechanisms accounting for variation and relational linkages can explain how and why something actually happened. Neither fundamental laws nor imputed self-propelling causal properties and dispositions govern objects in time and space; they only govern objects in abstraction (Cartwright 1983). The answer to a "how" question instead demands explanatory and empirically contingent causal pathways. Hence there are the respective rules for the two types of accounts: Competing theoretical treatments—treatments that offer different general laws for the same phenomena—are encouraged in

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30 Paradoxically, that mechanisms cannot be deduced from laws is also the argument of Elster (1989).
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physics; by contrast, only a single causal story is allowed. Alternative causal stories compete in physics in a way in which covering law treatments do not. Causal stories are treated as if they are true or false, but which fundamental theories govern the phenomena is a matter of practicality (e.g., there are dozens of fundamental theorems for laser operations and scientists openly choose one or another depending on other factors; Cartwright 1983, pp. 11–12). And not surprisingly, although philosophers generally believe in laws and deny causes, the specificity of causal stories means that in actual practice physics works in just the reverse.

Let us take a classic example from economic theory. The fundamental law is based on microeconomic theory and it purports to explain how firms make decisions about prices: Because they are motivated to maximize profits, managers determine prices by setting output at a level where marginal costs and marginal revenues are equal. This may work as a powerful predictor about a relationship between managers and prices but it certainly does not give us an explanation for how prices are set. Why not? Because it is undisputed in economics that no manager or any economist has the slightest idea of what the marginal cost of producing something really is. For a cause to explain, the cause really has to exist; it has to be identified and exhibited. An “as if” underlying theoretical postulate is not a cause, it exhibits no evidence. No less than Milton Friedman has admitted that the actual thought processes of managers cannot in practice resemble this model. We have a covering law, but we do not have an explanation.

Since causality, unlike general laws, involves “stories” constituted of causal mechanisms and connections, the thick temporality of path dependence and causal narrativity makes them especially significant in the defense of history against the attacks of Kiser and Hechter. Take an example with which we are all familiar: It is a general covering law that to lose weight, you have to eat less. This suggests causal mechanisms—eating less causes the effect of losing weight—and it may also be a powerful predictor, but it is a correlation of events that does not in fact demonstrate the mechanisms that actually can explain how it happens. The law does not exhibit a cause. How do we exhibit a cause? We tell a story, a causal narrative about the causal pathways by which one class of events is actually affected by another. Since food has calories, and calories are energy, when we reduce our intake then the body has less energy to draw from external sources so it has to turn to internal sources of energy, which are stores of fat, and the body uses up fat when it draws that energy, and so forth. Along the way we may use general laws but they are not in themselves explanations for why and how, when we eat less, we lose weight. In science as well as sociology, an explanation that actually depicts causal mechanisms is always told in narrative form comprising statements with

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transitive verbs: "The reduction of energy caused the body to draw on other sources..." "A actually caused B to happen by means of the following mechanisms and processes..." Cause implies narrative and relational pathways. It is narrative because explanation must be embedded in time and move through time. Indeed the success of any explanation resides in its accounting for temporality and sequence.

Contra Kiser and Hechter, a relational realist explanation thus entails no trade-off between the false dichotomy of theory versus history. Following Kuhn's mandate to liberate history from its long-demeaned status in epistemology, relational realism is devoted fully to generating causal explanations but from an ontology based on relational mechanisms. At issue in proposing a causal theory based on causal narrativity and path dependency is not (as Kiser and Hechter insist) the choice to eschew causality and theory in favor of particularism; rather it is the choice to generate causal theories that build on the temporal processes that are at the heart of any notion of a causal mechanism (e.g., Coleman 1986, 1990). Relational realist methodologies demonstrate that when causal mechanisms are no longer "subsumed" (Kiser and Hechter 1991, p. 6) covering laws, we are left not with a naive particularism, as Kiser and Hechter suggest, but with Tilly's (1994) notion of time as one "drenched with causes that inhere in sequence, accumulation, contingency, and proximity" (p. 270).

Path dependence and causal narrativity converge with recent work in biology that suggests that many of our natural laws may hold because they can explain things by reference to a common history (Hull 1977, 1978, 1981). Comparing historical sociology to evolutionary biology more generally clarifies relational realism's explanatory model; indeed everything that Kiser and Hechter have said against the former would apply equally to the latter (Gould 1981, 1988, 1989; Hull 1988; Kauffman 1993). Evolutionary biology is narrative, historical, and lacks true invariant laws, yet is now understood to be the foundation of biology (along with genetic theory). It is, however, full of causal mechanisms and generalizing theory that explain how things happen and happened (Kauffman 1993). There are no laws for the extinction of species, for example, but there are indeed mechanisms: a comet hits and kills the dinosaurs thousands of years later. It was not, moreover, any intrinsic causal or essential property of the poor dinosaurs that made them extinct, nor anything intrinsic to comets that they extinguish dinosaurs; it was just the bad luck of the dinosaurs to have been at the wrong place at the wrong time. Yet the absence of law or exogenous properties does not prevent us from recovering causal generalization by comparing historical sequences and episodes—or what Gould (1989) calls "replaying the tape." Gould still requires that we test for causal attribution and employs a theory of comparative causality that he calls "consilience"—in which many independent sources "conspire" to demon-
strate the causal impact of historical patterns (see also Ragin 1987). The comparative test is crucial: In explaining outcome \( Y \) by a hypothesized historical pattern \( X \), consilience requires that if \( X \)'s earlier stages had not occurred or had developed differently, then \( Y \) either would not have happened at all or would have happened so differently that it would require a different explanation (see also Miller 1987 on counterfactuals). The method shows that \( Y \) makes sense and can be explained rigorously as the outcome of the causal processes of \( A \) through \( C \) plus \( X \); but "no law of nature enjoined" \( Y \), and any variant of \( Y \) emerging from differently configured antecedents would "have been equally explicable, though massively different in form and effect" (Gould 1989, pp. 278, 282–83, 288). The similarities between Gould's consilience and the thickly temporal demands of path dependence and causal narrativity are instructive.

How Do We Know if a Theory Is True? Problem-Driven Pragmatist Reasoning

Kuhn challenged the accepted premise that knowledge is neither primarily theory driven (deductive) nor data driven (inductive) but problem driven, thus moving us beyond the limited dichotomy of induction versus deduction. His challenge calls into question the legitimacy of rational choice theorists' common accusation that the only alternative to theory-centric deductivism is what they call the "empiricism" of "naive inductivism." Recent developments in science studies have taken up Kuhn's challenge and so cast further doubts on such an unbalanced overly theory-centric view of science and the production of knowledge. Increasingly, we are learning that Kuhn was right when he suggested that philosophy's long-standing privileging of theory over history and practice was little more than an "image" (Kuhn 1970, p. 1) based on idealized, rather than historical reconstructions of science. What has emerged is strikingly different: Hacking (1983, 1992) emphasizes "intervening" over "representing," Pickering (1989, 1990, 1992a, 1993) stresses "the temporality of practice," Humphreys (1988) characterizes the best kind of inquiry as "empirical realism," and Bohman (1991) sums up the long overdue recognition of pragmatism as "the new logic of social science." All follow Kuhn in rejecting philosophical images in favor of reconstructing the actual histories and practices of inquiry and knowledge production in science and the social sciences. The shift entails a move away from rationalist and a priori criteria for confirmation to "problems as they emerge in the theories and research of scientific practice itself" (Bohman 1991, p. 53). A more experimental and practice-centered view of theory confirmation than allowed by the either/or of deduction versus induction has thus gained scholarly recognition. Along with the emphasis being placed on the practical ele-
ments of inquiry has also come an overdue recognition of the diverse criteria that can justify (and have justified) theories (see esp. Pickering 1992). We now know that testing theories by observing only those phenomena designated by a preexisting hypothesis is only one among a wide variety of avenues along which science can lead us toward path-breaking knowledge. The relative import of general theory-driven logic has indeed been radically exaggerated (Galison 1987, 1989; Gooding et al. 1989; Gooding 1992; Pickering 1989, 1990, 1992a, 1993).

Kuhn prefigured the new trend. Beneath the false alternatives of induction and deduction he pointed to evidence of the more common practice of “articulation.” Acknowledging the significance of this alternative would, I believe, advance our understanding of scientific development. Hacking (1983) has been most influential in elaborating the notion of articulation. He adopts Kuhn’s term to explain how much more complex is the bridge between hypotheses and data than the limited either/or of deduction versus induction (Hacking 1983, pp. 213–16). Articulation more accurately represents the practice by which scientists “create the phenomena which then become the centerpieces of theory” (p. 220). Against a disproportionate theory-centrism, Hacking translates Kuhn’s term to mean the history “not of what we think but of what we do... because... reality has more to do with what we do in the world than with what we think about it” (p. 17). Following Hacking, the historically forged notion of articulation has been widely developed into a full-blown alternative practice-centered conception of theory construction in science (see esp. Pickering 1992b).

In social science, Stinchcombe (1978) foreshadowed this trend when he declared that “the dilemma between synthetic reasoned generality, tested against the facts, and historical uniqueness, a portrait of the facts, is a false dilemma. The way out of the dilemma is that portraits of the facts, combined with an intellectual operation of carefully drawn analogies, are roads to generality” (pp. 115–16; emphasis added). Western (1997, pp. 10–12) has summarized cogently the problem-driven approach as one that involves (a) a willingness to make strong use of substantive knowledge to guide assumptions, (b) admission of the heterogeneous sources of uncertainty associated with all data, and (c) building models to fit our substantive problems and not the other way around. The problem-driven and practice-centered approach has a new credibility not reducible to either induction or deduction; a disproportionate theory-driven adherence to deductivism is no longer feasible (see also Humphreys 1988).

Theorizing convincingly about mechanisms, then, is a task requiring neither pure induction nor pure deduction but one that demands devising diverse and creative ways (e.g., abduction or articulation) to answer the question of whether the theoretical entity being hypothesized can actually be demonstrated to have a relational effect on a specific problem: Are, for
example, the mechanisms triggered by Putnam’s (1993) differing degrees
of social capital really viable theorizations for explaining varying patterns
of democratization? Only through research practices deploying substance-
driven comparisons and problem-driven manipulations can we get an an-
swer to whether a “theoretical entity” such as social capital actually bears
what Abbott (1996, p. 873) calls “causal authority” when he asks: Does
something have an “independent standing as a site of causation, as a thing
with consequences . . . [an ability] to create an effect on the rest of the
social process that goes beyond effects . . . merely transmitted through the
causing entity from elsewhere?”

TAKING STOCK
I began by posing a puzzling paradox: How is it possible that Kiser and
Hechter’s attack on historical sociology could have its epistemological
roots in a recognizable post-Kuhnian philosophy of science and yet be at
militant odds with the very incorporation of history that Kuhn so elo-
quently advocated? Let me briefly crystallize the three-part argument
through which I have addressed this paradox. First, I suggested that there
have been radically competing interpretations of Kuhn’s argument and,
from these, it is possible to draw two post-Kuhnian branches of realism—
one theoretical, the other relational and pragmatic. Of these two routes
out of Kuhn, the long dominant one has been a theory-centric antipositiv-
ism that converged with Popper’s deductivism—even while it adamantly
refused standard deductivism’s ultimate foundations in the language of
observation. Deeply opposed to positivism’s antirealism, the theoretical
realist route out of Kuhn gave a new lease on life to an age-old rationalist
epistemology. Given Kuhn’s fundamental demonstration of the centrality
of history for epistemology, it is a great irony that among the major casual-
ties of theoretical realism is history—once again (qua Kiser and Hechter
1991) relegated to the status of “particularistic,” “antitheoretical” epistem-
ological Other to “real” theory.

The paradox that I have pointed to is that although seemingly anticipa-
ted by Friedman’s (1953) instrumentalist defense of unreal “as if” as-
sumptions, rational choice avows a noninstrumental belief in the reality
of its assumptions, and this is a feat only made possible by the theory-
centric version of the Kuhnian revolution. Once the “theory-laden” char-
acterization of observation had gained hegemony, not only induction but
even more, “mere history” became tainted with the empiricist label. With
this move the way was wide open for theoretical realism to claim a higher
form of epistemological privilege for the power of abstract reason, logic,
and a priori principles. Worries about empirical or historical grounding
are met with dismissals of “empiricism” and “antitheoreticism,” such as
those that drive Kiser and Hechter’s argument. Rational choice, the theoretical position from which Kiser and Hechter launch their attack, is the current foremost standard bearer of theoretical realism’s return to the dismissive attitude toward history that precipitated Kuhn’s original challenge. Stating, for example, that narrative is only useful for purely “illustrative” purposes, Kiser and Hechter invoke memories of Kuhn’s critique of an epistemology that limits the value of history to “chronology” and “anecdote.” Hence the paradox comes full circle in their assault on historical sociology.

Second, I have proposed a reading of Kuhn that allows us to develop an alternative branch of realism, one that follows his challenge to liberate history from its demeaned status as descriptor, anecdote, and mere illustration. Advocating a historical epistemology, I have tried to show that theory must be considered historical in a double sense: (1) What makes a theory true does not and cannot exist outside of the spectrum of historically conceivable questions of its time; it is only the questions we ask that bring into existence those theories that compete to be answers and it is only questions that transform random facts into confirming evidence. And (2) for any theory to provide an explanation requires a structure of causal linkages (mechanisms) and what Tilly (1994) calls a thick sense of history. Concomitantly, this means that history could no longer be caricatured as random particularities and excluded from the domain of true theory. Instead he showed history to bear within it the accumulation of questions that drive the production of knowledge; in turn, history gains epistemological stature as the stuff of sequences, processes, and relations that we can arrange into causal interventions and explanations based on path dependency and causal narrativity.

The third dimension of my argument has been to make explicit and to challenge the epistemological foundations of Kiser and Hechter’s rational choice theory. With respect to their criteria for what counts as an “adequate explanation,” Kiser and Hechter: (a) use a tautological methodology in which they impute causal mechanisms to the case at hand from an ontology already comprised of causal mechanisms (self-propelling agential intentionality); (b) they confound the covering law model’s rules for theory justification with rational choice’s realist reliance on inferred theoretical entities—hence mixing realism and antirealism with respect to observation rules; and (c) they create a hybrid standard for explanatory adequacy by conflating the abstract conjunctions of predictive laws with true causality—thus preempting the very process centeredness of the causal mechanisms that they advocate. In considering their criteria for justification, moreover, we deserve better reasons for why we should be convinced than mere appeals to the elegance and parsimony of their preferred general theory of rational choice. And with respect to their ontology, the problem
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is not the individualism, the attributed rationality, or the micro level of their basic unit of analysis, but their embrace of a causal ontic methodology: in advance of inquiry they ascribe to the individual agent exogenous a priori self-propelling causal properties that necessarily (deterministically) and universally will cause action. There is nothing wrong with an explanation based on intentionality; its causal powers in any given theory however, must be the result of research and inquiry, not of causal ontological postulates. A social science that presumes precisely that which should have to bear the test of demonstration—namely, the causal factors that best explain a given outcome or problem—is a social science that eliminates the contingency now recognized to be essential in causal theories (Lieberson 1991).

Kiser and Hechter, in sum, thus present a mixture of pre-Kuhnian deductivism and covering law model logic with a post-Kuhnian theory-centric causal ontology and commitment to causal mechanisms (see, e.g., 1991, p. 6). In the first instance, they rely explicitly on the authority of Hume. The price of starting with Hume, however, is that Hume insisted that experience gives us only observable correlations ("constant conjunctions"), and so little can be said about causal laws (except in terms of statistical probability) and nothing at all about causal mechanisms (except that none are observable). Since Kiser and Hechter are not willing to pay this price, they turn to theoretical realism and its claim that the unobservable can be inferred from general theory. This gives them license to build explanations based entirely on the premise of a universal causal ontology and a priori axioms of agents-as-mechanisms—explanations that are confirmed when their implications conform to the standards of justification set by theoretical realism.

CONCLUSION

Kuhn knew the long-standing caricature of the epistemological dichotomy between history and theory would not die easily; he even suggested that the "main obstacle" to the acceptance of his historical epistemology would likely be similar to the attacks on historically based theory that confronted Darwin's Origins of Species (Kuhn 1970, pp. 171–72). That Kiser and Hechter's chief accusation and interpretation of historical sociology is a "relativist" one shows just how prescient Kuhn was. Almost 20 years after The Structure of Scientific Revolutions, Stinchcombe (1978) recommended that the social sciences follow a path similar to Kuhn's: "There is nothing so likely to cause a misreading of this argument [history vs. theory is a false dichotomy] as twisting it back into the psychology of generality that it rejects. The Kantian versus Nietzsche (or positivist vs.
Dilthey) version of what epistemology is all about is so embedded in the historical origins of social science that an argument based on the supposition that this is the wrong question has a hard time trying to say what it is about” (p. 116). The recent “historic turn” in science studies and the philosophy of science would seemingly make it increasingly difficult to play an image-based and idealized version of the science card in criticizing history. Yet Kiser and Hechter’s attack on historical sociology as “antitheoretical” and “relativist” is but the latest manifestation of Stinchcombe’s observation of just how hard it is to say what an historical epistemology “is about.” Trundling out these same tired caricatures of both history and theory, they prove just how intractable the false dichotomy is, but one as indefensible today now as it has ever been.

Paradoxically, then, two leading historical sociologists are taking us right back to the antihistorical position that Kuhn originally challenged—free now, apparently, as post-Kuhians from the discipline of observation with which positivism was burdened. But positivism burdened itself for good reason with the discipline of the empirical—to escape the potential of epistemological tyranny, arbitrary dogma, and prejudice. In the absence of this discipline, the appeal to “theory” can be as much a weapon of mystification as the blunt sword of “empiricism.” The theoretical world that rational choice theory inhabits may well be, as Kiser and Hechter claim, “parsimonious,” “bold,” even elegant; but we need, nonetheless, justification for believing in it.

Kiser and Hechter are quick to scrutinize and to condemn the errors of historical sociologists. They have been less quick to examine and to question how their own epistemological strictures have histories, histories not of refinement towards greater absolute truth, but rather histories of deep contestation—histories not unlike those that both they and we study. Unfortunately, the histories that constitute some of our most important and significant explanatory rules are often completely invisible (Lieberson 1991); they have been naturalized to the point where Kiser and Hechter are able to confound general standards for what counts as theory and knowledge with the specificities of rational choice theory—and thus privilege theoretical realism over “mere history.” They have in effect preempted the benefits of deliberation among rival positions (clarifying presuppositions, broadening purview, etc.) and so averted the difficult questions inherent in the multiplicity of intents, purposes, and radically divergent assumptions characteristic of the social sciences today. But the consequences of their imperial claims are ironic: Kiser and Hechter have forcefully insisted on the postpositivist truisms that all observations are theory laden and that historical accounts are organized by theoretical categories. But perhaps Kiser and Hechter should ponder whether their own
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evaluative criteria are themselves not history laden, and thus contingent and embedded in a larger field of less angelic, more pluralist, epistemological possibilities.

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