"We're No Angels: Realism, Rational
Choice, and Relationality in Social
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Abstract

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 (1991) propose that *rational choice theory* can avoid the trend toward "empiricism" resulting from the import of history into sociology. K/H's position is based on *theoretical realism*--a radically antipositivist thesis that uses theoretical postulates to theorize about reality beyond positive appearance. Examining closely K/H's theoretical realism casts doubts on the epistemological foundations of rational choice theory. Recent developments in *relational realism*, by contrast, place greater emphasis on the practical elements of explanation, supporting a more relational, causal-historical, and problem-driven view of theory. A renewed appreciation of Kuhn's *historical epistemology* provides the foundation for evaluating these competing research programs.

30 years ago, in a remarkably prescient first sentence, T.S. Kuhn sent shock waves through our intellectual landscape--and forever transformed it: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed..." (Kuhn [1962] 1970:1).[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 1991:1). 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 (K/H) *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 and theory by demonstrating its centrality?

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, K/H criticize *all* research that is *not* theory-driven, or "guided" by "general theory" (p.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 all *problem*-driven research and their skepticism about what they call "empiricism." Second, K/H's 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 so-called "anti-theoretical empiricism" (Almond 1988; Hechter 1992; Shepsle 1992). That K/H invoke rational choice not merely to exemplify but as the *only* theory to date that meets their criteria (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. Examining specifically K/H'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 parochialism because K/H don't 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" (p.16). Surely if this wide spectrum of scholars is threatening the discipline, we would all do well to take

notice.

K/H charge historical sociology is "empiricist," wrongly "inductivist," innocent of the importance of "causal mechanisms in adequate explanation," and subversively "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 K/H's own position: By what principles of knowledge do *they* justify their criticisms? And are we convinced by those principles? Since, moreover, K/H's accusations are spun on the terms of historical sociology being "antitheoretical," neglectful of "mechanisms," too descriptive in their use of "history," and naive of the triumph of deductivist "general theory," then how we evaluate their criticisms fully depends on whether we accept their definitions of these terms in the first place. The questions I put to K/H, then, following their own criticisms, are: 1) What, indeed, *should* count as an "adequate" social science explanation? If theorizing causal mechanisms is the criteria for success, and mechanisms are unobservable, how do we know a successful explanation (i.e. based on mechanisms) when we cannot see them? 2) How do we judge among rival theories, and do *they* give us good reasons to believe in their theory? 3) And, ultimately, if "general theory" is the criterion for success, and "history" the antithesis of theory, what *counts* as "theory" and "history" in the first place? At stake, I believe, if we accept their answers to these questions is the epistemological status not just of history but of social science *tout cours*. Are theories to be constructed and judged exclusively by theory-driven standards (as per K/H), 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: the first, a focus on Kuhn and the Kuhnian legacy; the second, a critical focus on K/H (1991); and third, an outline of an alternative to K/H's position. It is in examining K/H'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 an trajectory made possible largely by the Kuhnian revolution.[2] I will argue additionally, and paradoxically, that a slightly different trajectory out of the same Kuhnian corpus provides the most convincing alternative to K/H's model. This alternative is based on a *historical epistemology*, and it also supports theorizing causality--but with a conception of theory not as either deductive or inductive but as problem-driven, 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 preferences, 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 K/H frame their attack--between theory and interpretation, between positivism and historicism, between their correct use of deductivism and the naive inductivism practiced in historical sociology. But K/H 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 K/H'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; rational choice theory, by contrast, rests on a realist thesis and is by definition of its insistence on imputing causal mechanisms from theoretical postulates a militantly antipositivist enterprise. Only realism accepts that causal mechanisms--despite being unobservable--can be used as the basis of explanatory theoretical accounts. Thus while scientific realism may seem like an arcane concern to some, any talk of causality (like that of K/H's and 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 1986:1327]) 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 two-fold: First, how can we theorize about--i.e. ascribe causality to--that which is unobservable, be it price mechanisms, maximizing preferences, class consciousness, 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 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 proabability 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 which, since we cannot observe them, we have no sure reason for believing in them? It was for very good reasons that positivism took on the heavy burden of demanding positive evidence for theoretical claims; without it, we are susceptible to all and any prejudice

passing under the guise of science. Their challenge is still justified: Why we should believe in any social science theory--however elegant or parsimonious--that explains by reference to unobservables, 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.[4]

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. K/H, in the end, dodge this responsibility: They give us no good reasons for why we should believe their version of reality. The advantage of even asking for accountability, however, is that the question may provoke further epistemological reflection 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 1992:301). Since most of us are not angels, perhaps we need a realism for the rest of us.

A Note on Kuhn: 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 *Structure* is "probably the most influential work ever written in the philosophy of science" (Lloyd 1993:207).[5] So much has Kuhn passed tacitly into our knowledge culture, however, that his work is sometimes less examined than 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 even be more important as its explicit retreat out of 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 an historical epistemology. The unorthodoxy is purposeful; on the occasion of his recent death it is fitting to revisit directly the question of Kuhn's significance by exploring what I propose to be crucial to current debates over rational choice theory--his paradoxical duality of influence.

I: THE KUHNIAN CHALLENGE: Beyond Induction versus Deduction

When Kuhn published *Structure*, the "image of science" by which [philosophers were]...possessed" 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 and empirical 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 the image 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 world view 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.[6] 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 theory-evidence confirmation, Kuhn showed those theories that actually have been accepted as explanatory are those which have unseated the available competitors in the field (p.144-45). *History shows that it takes a theory to beat a theory.*

The Kuhnian Revolution: A Theoretical or a Historical One?

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 an historical epistemology. Kuhn #1 and #2, among the most debated topics in the last twenty-five years of scholarship are not the focus of this article. Kuhn #3, the chief focus of my critique, I argue to be 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" (Kuhn #3) is the term for 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 1983:170). The instrument of denial was misreading Kuhn to have said that observation is nothing *but* theory, and science 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.[7] The second extends problematically to the social sciences the reasonable argument that since (in natural science) there is often no significant distinction between observable and unobservable entities, they can legitimately be used interchangeably in 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.

The flattening of these into one grand theory-centrism created a gestalt-like change in the image of science: The wrongheaded privileging of "brute fact" was suddenly replaced with a new hegemony of "the primacy of theory" (for correctives, see Pickering 1989, 1990, 1992a, 1992b, 1993; Hacking 1983, 1992; Galison 1987, 1989; Gooding 1992; Gooding et. al. 1989). Suddenly it appeared that philosophers had closed the door to reasoning that did not accept what was now an aggressively anti-empirical, 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 demoted to mere "illustrations."[8] When such new stories concluded with ordinary positivist morality tales about experiments "confirming the theorists' speculations" they still converged safely with the empirical foundations of deductivism. But it was an easy slide from there to the hegemony of theory *tout cours* 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--so long a driving force in the construction of phenomena not merely the passive reporting of facts--was now denied an independent role in theory building (Hacking 1983:159-161, 171). Most ironically, in light of Kuhn's original mandate, history--long the epistemological "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: AN HISTORICAL EPISTEMOLOGY

Kuhn's contribution, I think, should be seen elsewhere--as adumbrated in the first sentence of *Structure* where he argued *not* against the exclusion of theory, but of history.[9] He thus posed the challenge of an *historical epistemology*, a term I use to capture the deeply controversial thesis that the history 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 anti-historical quality of epistemology--the rules and criteria for valid standards of truth--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 focus on *problem-driven* sources of knowledge; his notion of *articulation* in research practice; and his implicit demonstration of *path-dependency* and *causal narrativity* in explanatory structure.[10]

(1) Problem-Formation in Theory Construction

As an element of his mandate to go beyond the induction/deduction dichotomy, Kuhn indeed argued that theories are defeated only by competing theories--not just disconfirming data. But he did not stop there; he looked to the history of science to understand why certain theories were even considered candidates for truth in the first place (1970:144-59). There he found that the most significant factor in determining the spectrum of competing theories was the set of contemporary questions to which the theories were vying to be the winning answer. 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 1970:8-9). Philosophers of positivism make a fundamental distinction between what they call the *logic of justification* and the *context of discovery* (Reichenbach 1962; Hempel 1966:3-18; Popper 1962:42-59). Discovery is the context in which we "discover" problems-to-be-answered, and the circumstances surrounding the initial formulation of the hypotheses we generate to answer them. It is 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 context of justification, by contrast, is limited to the formal logics of methodology, or the correct means for verifying and structuring hypotheses--are they supported by the evidence? confirmed by experiment? corroborated by stringent testing? (Hacking 1983:6). 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 to commit 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 created 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 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 1970:8-9). Once history showed that theories are tested against competing theories, the logic/discovery distinction proved untenable for Kuhn because it is the historical *context of discovery that determines the spectrum of theories available for comparison at any point in time*. Why, for example, at T2 rather than at T1, does theory A win out over the field of competitors [A, B, C...]? Kuhn's answer was that for historical reasons theory A could appear to be justified in the field of theories [A, B, C] available for comparison, but look not at all justified if the field of available theories is or was [A, D, F] (1970:23,94-110, 144-159).

Two key insights can be extrapolated from Kuhn's focus on problem-formation. First, because scientists ask different questions at different points in time--in part because of prior questions asked--different answers also satisfy them at different points in time. Theory [A] may beat [B] at time T1 but lose to [B] at time T2 because the questions may have changed (1970:102-106). 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 can bring certain questions into existence in the first place. "Civilizational concerns" (Zald 1990) post-1989, for example, explains why social scientists now problematize democratization at a level of intensity completely absence only ten 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. That means that contained within the theory is the *temporality of the question,* and a confirmed theory at T1 may well be subject to future disconfirmation at T2 when pressed by a different kind of problem. This does not suggest that there is no best theory; rather, that a certain humility is necessary in the face of any winning one (Kuhn 1970:198-200,209-210).

Arguably, Kuhn anticipated the reply from skeptics: 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 (1970:171-72). An answer to these criticisms can be drawn from the second major implication of his 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 a 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 not as a refutation of the theory but as an invita-

tion to further develop the theory to accommodate the observation (or 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 (1970:97-98). 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 how terribly a theory has failed (1970:52-91).

Contrary to the prescriptive rules that exclude history from questions about 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 (1970:3,26,94-95). Rather, the historical processes of problem-formation appears 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: 103; Collingwood 1970 [1939]; Somers 1996a). 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 1970:106).

(2) "Articulation" in Research Practice

Kuhn's demonstration of the epistemological centrality of problem-formation and his critique of the induction/deduction dichotomy shows the great failure of the theory-centric reading of Kuhn to distinguish between "empiricism" and the altogether different order of experimental activities. In this 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) (Hacking 1983:173; Humphreys 1988,1989) -- into the single and justifiably vulnerable concept of passive accumulation of data. Unnoticed, apparently, by the theory-centrists is that Kuhn was equally critical of claims for disproportionately deductivist logic, as history also demonstrates that the relative import of general theory-driven testing has been radically exaggerated (1970:23-33). 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 stringer conditions" (1970:23). The analogy to the common law is telling; for like O.W.Holmes' (1903:1) famous excoriation 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 the *encounter* of theory with historically changing problems. In this effort to capture the research process through the term "articulation," Kuhn, like Holmes, is challenging the priority of abstract rules of logic over the historical construction of knowledge--apparently building from the original meaning of "historical" as "inquiry" and "learning by doing" (OED 1954:567; Hacking 1983:4). Experimenting through articulation "can resemble exploration" (p.29), "more than any other sort of normal research, the problems of paradigm articulation are simultaneously theoretical and experimental" (Kuhn 1970:33). 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...manipulations of theory undertaken, not because the predictions in which they result are intrinsically valuable, but because they can be confronted directly with experiment" (p.27,30). In a direct challenge to the possible hegemony of either induction or deduction, he stresses that "the need for work of this sort [articulation] arises from the *immense difficulties often encountered in developing points of contact between a theory and nature*" (p.30, emphasis added). Not surprisingly, articulation can also radically transform accepted logics. In Galileo's time, for example, previously unencountered historical and practical exigencies induced methodological instabilities; where scientists were once satisfied with qualitative explanations, they may suddenly have demanded accurate quantitative predictions with the introduction of cannonballs. Kuhn's discussion of articulation was brief and only suggestive; the challenge he posed was to take this suggestive notion and nurture it.

(3) Kuhn's Explanatory Challenge

Kuhn is usually read as challenging only the logical rules of theory confirmation--hence the incessant worries about his fostering of "irrationality" and "relativism." But recall that Kuhn demonstrated that the criteria for theory acceptance had varied throughout history by constructing an *empirical causal explanation* that gave a convincing alternative account--an explanatory theory, in short--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 answer to epistemology's central methodological question, namely *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-nominological (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 1965:247-248; Cartwright 1983; Miller 1987; Outhwaite 1988; Salmon 1988).[11] 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, but too often unnoticed, about the D-N model, is its necessary and emphatic rejection of "causality" as a component of a nomothetic (general and theoretical) law. Causality in this sense refers *not* to laws based on regularities but to the specific *mechanism*s that causes X to lead to Y. According to a strict positive logic derived from Hume (1938), theorizing about causal mechanism was ruled non-scientific because actual causal pathways and mechanisms can never be positively observed; only correlations or regular conjunctions of events are scientifically grounded in observation. It was this rejection of underlying causal structures (because considered "theoretical entities") that made it possible to combine in a single set of conditional laws both prediction and explanation--explanation, in this account, was limited to a deduction from empirical regularities (Hempel 1965:359).[12]

By giving us a story about the development of science, Kuhn introduced temporality and historicity as category of explanation and demonstrates the limits to the covering law model. Because it subsumes temporal mechanisms under the rubric of predictive universal laws, it cannot disclose the underlying causal mechanisms underwriting chains of events (e.g. "enchainment," Abbott 1993, 1995; Coleman 1986:1327); it cannot allow for the contingency of outcomes or explain how sequences and spatial patterns matter (e.g. Aminzade 1992; 1992; Griffin 1992, 1993); it searches only for similarities, not variations and processes (Tilly 1995a). 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 could be nonsensical, as Kuhn ironizes: "Can Newtonian dynamics really be *derived* from relativistic dynamics?" (1970:101). 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. A successful explanation, 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 its bears the 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" (1970:173). His own causal historical account, by contrast, he depicted as "movement *from* beginnings but *toward* no goal": "We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by *nature* in advance. But ...can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time?" (p.172,171).

Kuhn's analysis gave new life to an old suspicion: That an explanation intended to account for *how* and *why* something happened--and not just for predictive purposes--can only be established through causal chains and mechanisms.[13] It has even been argued recently that the relational absences built into the covering law model actually undermine 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 could not work through logical deductive entailment. Most notably, it makes clear that there are alternatives to the covering law model that are *more* than non-theoretical, particularizing, and "story-telling"--that is, "merely historical." Put slightly differently, alternative methods such as path-dependence *are* historical, but Kuhn gave history a new epistemological status 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 1970:1) was mirrored in the social sciences, long divided between interpretism 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 conditions; those historical conditions, however, are themselves theoretical causal sequences and relational structures.

An historical epistemology thus invites us to think about history as a dimension of epistemology and thus a constituent, rather than an illustrative, element in the conditions for 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 1970:8,9). Like Stinchcombe (1978, 1984) earlier, Tilly (1994) 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"

(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 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" (Tilly 1994:270). Kuhn defrocked these caricatures and challenged us to develop epistemologies that no longer thrived 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 1970:8)?

HOW AND WHY K/H FAIL TO MEET KUHN'S CHALLENGE

K/H 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 K/H are simply rehearsing an orthodox positivism, criticizing historical sociology for violating rules of logic and not producing adequately law-like generalizations. That they are, however, forcefully *antipositivist* in their commitment to rational choice's *theory-driven* logic of social analysis and causal mechanisms, makes it clear that K/H's position is more complex, and more challenging. To assess the justification for this epistemological mix, we need to place K/H'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 rationalist thesis that objects of knowledge exist and act independently of our direct knowledge of them, even unobservable "theoretical entities" such as electrons and atoms, or social structures, classes, and market forces (Bhaskar 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 ever since the 17thc. On the one hand, ever 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, even before Hume (e.g. Descartes) science has always 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 beneath "appearance."[14] The tension, of course, is that the priorities of positive evidence, versus rationalist or *realist* understanding, 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 and/or free ridership, as opposed, say, to production of registration difficulties put in the way of voting by state and county governments?

Although they long vied for dominance,in the 20th century the victory had gone decisively to those who insisted that knowledge had to be justified by what was called "theory-independent observation language"--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 *anti-realist* 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 to us 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 succession of cause and effect at the level of events [empirical regularities]" (Collier 1994:7)

At stake in the question of realism is thus a great deal more than talk of metaphysics; these are questions of *how do we build an explanation* and what does it take to justify any given explanation based upon unseen causes. Questions about the reality of unobservables matter because most social theories attribute *causality* to--that is, *theorize* about-- unobservable phenomena, such as explaining the fall of the Berlin Wall by reference to a democratic "political culture." If theories can only be justified empirically, what counts as a theory of democratization must be limited to inferences from statistical regularities about, say, opinion-polled 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 "real" cause.

Antirealism thus puts strict limits to what we can justifiably claim to be able to explain on the basis of what we can know positively to be real. Realism, by contrast, adamantly 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 "objective" theory beyond the limits set by positivism, and allows us to theorize about that which positivism excludes--the causal

power of unobservable mechanisms.

By now it should be obvious just how significant would have been the Kuhnian revolution for 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 but 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 or effects of an unobservable observable (preferably "calibratable"), "gravity" itself was and is unseen and thus remains a theoretical entity.

The monumental challenge that postKuhnian realism continues to face, however, is how to convince us why we should believe in this 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; it is wholly another thing to consider as more than prejudice any particular 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 know--reliably--of such causes (Hollis 1994:12).

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 believes that theories survive to the extent that they are true pictures of what is real, and that this truth is guided by "general theory," channeled exclusively through deductive logic, but in the final instance confirmed by the same general theory--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,"[15] and bolstered by theory-centric postKuhnianism, for theoretical realism theories are notoriously "under determined by data" (Newton-Smith 1978:72; Boyd 1973). Lakatos (1970), for example, perhaps the most influential representative of theoretical realism (and certainly the primary influence on K/H), 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 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 I:100). 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 or what K/H and many rational choice theorists call 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 open to new formulations and measurements. 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 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 1986:9, 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 law-like 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 (Lloyd 1986:156). This conception of general theory thus marks theoretical realism's departure from both positivism and the historical postKuhnians: "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, a *relational* and *pragmatic realism* has emerged as a postKuhnian philosophy of science that is squarely on the other side of the epistemological divide from theoretical realism.[16] Relational realism is the thesis that belief in the causal power of an unobservables--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. Following the historical Kuhn, relational

realists believe that while it is justifiable to theorize about unobservables, any particular theory entailing theoretical phenomena is historically provisional. For relational realism that means one can believe in the reality of a phenomenon without necessarily believing in the absolute truth or "reality" of any single theory that claims to explain it. The belief in a phenomenon or an outcome instead depends on evidence of its causal 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 electrons, and long after whatever is 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 1990:356). Substitute the term society, domination, gender, or any variety of social concept, and the same argument holds. For example, a social science relational realist would argue that despite the fate or fashion of any particular theoretical term such as "sex roles," "sexual division of labor," or "gender,"--each of which represents a different causal conception of an unobservable postulated reality--we have reason to believe in the causal force of that which these terms variously attempt to signify as long as 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 identity to the world--then, by definition (and humility), one must be somewhat agnostic about the absolute truth of any given theory about the world. This is what Bhaskar (1986) means when he distinguishes between the "*intransitive* objects of science...and the changing (and theoretically-imbued) *transitive* objects which are produced within science" as a practice" (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 numerous failed versions of social theories. 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 of market forces), 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:2-3, 9; see also Camic and Xie 1994). It is the relational *effect* of an hypothesized phenomena such as gender, sex roles, etc. that makes us believe in it--*regardless of what we call it*--and not be-

cause of any single theory. To be sure, regardless of whether 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 preferring one causal claim over another on whether or not she has found a theory lean and parsimonious enough from which she can derive it logically. Science has shown us that some theoretical reality-claims are false just because the entities in question are not really there -- whatever their logical validity, beauty or parsimony: Epicurean atomism is not true, there are no humours, nothing with negative weight exists, women's wombs do not wander; "phlogiston is one with the witches and the dragons" (MacIntyre 1980:72). 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 purport to explain them are socially constructed--some better than others, I should add, because more relational.

What's wrong with theoretical realism?

K/H 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 pe-culiar 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 com-mitment to causal mechanisms. Each in itself is problematic but in combination they op-erate in such tension with each other that they generate incoherent grounds for critique.

K/H: "WHAT ADEQUATE EXPLANATIONS MUST ENTAIL"

K/H start out on a bad foot: "[W]ide agreement exists across social science" about the "first requirement of an adequate explanation--causality" (K/H 1991:4). Contra K/H, 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 expla-nation is a statement of correlations, and the most we can say about correlations is 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 explain how a (hypothesized) cause ac-tually produced the observed effect. As any frustrated social scientist knows from the dif-ficulties of trying to prove *causality* despite overwhelmingly strong statistical *correlation* between, say, capital punishment and black defendants/white victims, the battle to over-come the stigma of what positivist skeptics from Comte, to Mill, to Popper have called the mere "psychological appeal" of "ideas" about causation (beyond conjunction) has by no mean been won (Popper 1950:722; Comte 1830:14-16).[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 explan-atory 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 corre-

lated 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, K/H plunge headfirst 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 (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 1983:26]" (K/H 1991:6). Then, with no attention to the fact that the covering law model flatly *rejected* explaining by causal mechanisms, they then go on to 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: K/H attempt 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 rejected as part of a legitimate explanation.[18]

Here K/H stumble badly. Deep into the terrain of what their original authority (Hume) declared metaphysics and theology (realist views about causal mechanisms), K/H 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 again climb giddily onto Hume's stolid anti-realist shoulders and announce they have solved the problem of the unobservability of mechanisms by invoking *relations*: "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 K/H themselves have argued (p.4-10, 15-17), hypothesizing event uniformity (as in the covering law model) is *not* the same thing as finding causal mechanisms; just as in the face of inadequate exogeneity 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. Indeed limiting an explanation to event uniformity was the positivist's way to remain on empirical foundations.

But caught in the problem of trying to join the antirealism of covering law model (which rejects the viability of causal mechanisms) to the realism of rational choice theory's need for mechanisms, K/H's only solution is to demand a strict inferential relationship in which causal mechanisms are "subsumed" under the causal law (p. 6). Rather than solving the problem of how to find mechanisms, K/H appear to be simply waiving the problem away by retreating back into the safety of lawlike relations. Having first called them the "enduring focus of sociological explanations" [15]), they now want to conflate mechanisms 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 mechanisms?

Theoretical Realism: Overcoming the Limits of Observation

Enter *Theoretical* Realism--the thesis that unobservable underlying realities can be ascertained by *theoretical reason.* Whereas the empirical requirements of positivism forced theorists from Hume to Popper to reject the search for deep causality beyond conjunction and probability, and whereas relational realists use relational effects to observe theoretical entities, theoretical realism provides K/H a deus ex machina by making *theory* "see" what their senses cannot: "general theory [is the] basis for the imputation of causal mechanisms...mechanisms must be imputed from general theories" (K/H:5,15,16). Following the Lakatosian route out of Kuhn (through Popper) into the "hard core" of "general theory," theoretical realism thus allows K/H to accomplish by theoretical fiat that which has for centuries confounded philosophers--to bypass the epistemological problem of how to establish true causal explanation of mechanisms in the face of the problem of unobservability: "[M]odels 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 K/H's "adequate explanation." But--search as we might, it turns out that K/H *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--"theoretical boldness," "scope," "generality," "analytic power," and "parsimonious" (p.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 at p. 17 we are presented *not* with a general definition but with a single case of *a* general theory--rational 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 K/H's argument; according to K/H, it is the *formal* criteria of methodology that are. 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 particularities of rational choice to develop a "general" conception of general theory. We can thus add to K/H's difficulties that: 1) they have engaged a questionable maneouvre: 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 epistemological adequacy; 2) they have thus violated the rules of epistemology and methodology which, by definition, reflect general criteria for sound knowledge and are not 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 K/H told us those derive *from* general theory. In fact, in keeping with the hybrid character of their overall approach, K/H's general theory turns out to be a peculiar mix of aspirations--combining a methodological commitment to general laws (usually a staunchly empirical methodology) with a radically anti-empirical theoretical ontology: 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...[i]n rational choice theories, interests are specified a priori"(p.19, 21). Given metatheoretical form by Lakatos's (1971) 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), and it is an approach at the axial core of all theoretical realism. The term is itself a hybrid: First it refers to an explicit *ontology*--a thesis about the nature of the social world. In keeping with theoretical realism's rejection of what one realist calls the "long-standing neo-positivist injunctions against metaphysical postulates in the explanation of social actions" (Carlnaes:248), the ontic approach explicitly opposes positivism's privileging of epistemology over ontology (in which the question of whether we can really claim to know something takes precedence over the attraction of a postulated theoretical entity). Instead, it begins with what positivism excludes: An empirically unknowable, and unfalsifiable, theory of reality as a causal structure--"a prior argument about social reality" based on "the idea of the essential causal powers of kinds of things" (Lloyd 1986:8). In keeping with its antipositivism, ontic approaches are inherently driven by theoretical realism's fundamental distinction between "appearance" and "reality"--a distinction as old as rationalism itself. "Appearance" 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. Precisely because so much of what we now know really makes the world tick is in fact not empirically manifest, the theoretical realist, by contrast, privileges as the *real* source of causality those deep "ontic" structures and mechanisms of reality that are not accessible to the senses (Bhaskar 1978,1979,1986; Putnam 1997:181-84).

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 K/H's approach different than a simple ontology is the combination of the ontology with the methodology--specifically, with the rational choice 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--a priori--to be the fundamental causal force/mechanism at work in the social world* (Elster 1983,chs.1-3, 1986:12,16). 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 already built into the definition of social reality. This is a theory--more precisely, an ontic methodology--not just of agents as rational, but of *agents as mechanisms.*

Since all explanatory requirements flow from this presuppositional causal ontology, it is not rationality per se that does the main explanatory work in rational choice's general theory; more important is the causal capacity attributed to the individual agents through the *disposition* of intentionality--which is said to be inherently causal. Such an inherently causal ontology that postulates its agential units to have a priori causal powers and forces makes use of what Hempel (1965:472) called "broadly *dispositional traits*"--a term that evokes the 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. But what we have now learned is that they are *also* the very same *mechanisms* that K/H tell us are the central component of all adequate explanation, what philosophers call "reasons as causes" (Elster 1986:12; see also Rosenberg 1988:27-30).[19] Because they are dispositional, these mechanisms cause action with law-like necessity: It would be no more possible (all things being equal) for the agents of rational choice theory *not* to be driven by their purposes than it would be possible for glass not to break on impact--given its essential dispositional trait as "brittle."[20]

At the heart of this general theory, then, is a metaphysical vision of reality as an ontic structure comprised of agential units with necessary causal powers. The explanatory work is carried out by the invariant causal mechanism of agential intentionality that necessarily causes interests to convert into strategic actions. K/H's general theory has emerged to be a hybrid straining to make the center hold: It is built on a law that aspires to the covering law model of universality (intentionality causes action); but it is a law in which the crucial explanatory causal mechanisms are given exogenously. This is why Rosenberg (1988: 23) calls this kind of law "folk psychology": Rather than discovered (or problematized), deduced, induced, or generalized the content of this law comes ready-made; causality is inscribed exogenously. Independent variables are usually hyothesized rather than postulated, but in this case intentionality as causality is not only defined in ad-

vance but defined as the a priori cause of *all* known effects. The causal mechanisms built into rational choice theory are always the same: Agents equipped with causal dispositions that, universally and necessarily, are charged with a momentum from cause/intentionality to effect/strategic action. General theory, for K/H is a theory of *agents as mechanisms* believed to constitute and, by general universal law, to embody in their consciousness the essential causal powers and forces that drive 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" (K/H 1991:4). It began with a call for laws (causal relations), claimed it necessary also to find deep causes (causal mechanisms), and, having faced the problem of the unobservability of mechanisms, retreated back to laws and from there elevated to "general theory"--a ontology of agents as mechanisms. What now? As a set of "given" assumptions, general theory clearly is not accountable to the petty trials of empirical confirmation or problematization; rather, 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 sociologists then apply one or more of the causal mechanisms derived from available [rational choice] theories (e.g. repeated game theory, optimal location theory, agency theory...) to the problem at hand..." (p.19, emphasis added). This is remarkable: The very causal mechanisms required for an adequate explanation are to be imputed from a general theory comprised of assumptions about--causal *mechanisms*. Since causal mechanisms are *already* inscribed--regardless of any specific case or outcome being explored, K/H predefine what should be an empirical question, namely, the question of causality in any given historical case. The tautology is inexorable: At the level of micro-theory, for example, because their very essences are constituted by the causal trait of intentionality, it would be no more possible to explain the actions of individuals without reference to their essential intentional dispositions, than it would be possible to explain glass breaking without reference to its disposition of brittleness. And at the macro-theory level, it would be no more possible to explain social outcomes without reference to the agential property and disposition of intentionality than it would be to explain a broken glass bowl without reference to its constitutional brittleness. With causal mechanisms *already inscribed in the theory from which they derive these very mechanisms*, and relying on an ontic methodology, K/H give 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" (Hollis and Smith 1991:408).

Having it Both Ways

K/H have taken us on a dizzying circular trip: They have told us that something called general theory can give us the law or essence of a phenomenon, and from this essence they are handily able to explain their outcome by imputing to it the properties dictated by

the theory that has named these properties as the essence of its existence in the first place.[21] The tautology lies in the absence of an independent explanatory structure; the explanation is already inscribed in the a priori definition of the problem, which presents us at the end with "little more...than the explication of a definition" (Tilly 1995a:1595). Thus despite their commitment to laws, nothing could be more incompatible with either standard positivist rules of deductive logic in which the logic of generalization is required to be tested, perhaps falsified, in an observation language.

When it comes to what counts as an adequate explanation, then, K/H try to have it both ways, in more ways than one. First, by combining the covering law model with mechanisms they aspire to the certainty of positivity 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 which are causally responsible for what we observe. The incompatibility derives from theoretical realism's realist definition of general theory as essential general properties, propensities, and powers--mechanisms, in short. By claiming to be able "explain" by deriving law-like regularities/conjunctions from an axiomatic general theory, K/H 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, that is, of any empirical investigation, and *not by engaging with the phenomenon itself*. With this deeply anti-Humean/antipositivist solution in theoretical realism, K/H 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 no requirement that one must be Humean or positivist in social science methodologies; coherence, however, *does* require that since it is K/H'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 comprised of merely favored axioms about the imputed nature of reality.

CAUSALITY AND HISTORY

It would be churlish not to note the advance in K/H's desire for an adequate explanation to include mechanisms. In the covering law model, mechanisms were excluded not only because of problems of observability, but also because causal processes were seen to be too historical and particularistic, rather than logical and generalizable (Popper 1959; Salmon 1984; Bohman 1991; Miller 1987). Theoretical realism does not rest content with positivism's restrictive demands for observability as the criteria for true causality 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 me-

chanisms, it appears that K/H are recognizing the processual element of causality--thus hardly reason to worry about whether theoretical realism is compatible with classical positivism.

The victory, however, is Pyrrhic. Admitting to the same constraints as positivists in not being able to actually observe causal processes, theoretical realism "solves" the problem by "observing" through a set of theoretical assumptions the "properties and propensities" of unseen entities (e.g. the structure of human intentionality). Thus no sooner do K/H 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 (K/H 1991:6-7). This allows theoretical realism to reconcile its trafficking in metaphysics with the positivist goal of generating law-like 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; K/H take this temporal void to a level much higher than Kuhn ever envisioned. Whereas the standard covering law model conceptualizes time as a "general linear reality" (Abbott 1988), K/H aspire to what can be seen as a non-linear reality which 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 K/H "rule out historical narratives as a basis for the imputation of causal mechanisms," and instead demand "minimal temporality" in selecting causal mechanisms (p.6,7).[22] 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 1994:270).

K/H: HOW DO WE KNOW IF A THEORY IS TRUE?

K/H define an adequate explanation as one based on causal laws and mechanisms, but there is something missing in this definition--namely a convincing reason for why we should believe any given explanation. Recognizing the problems of observability in their requirements, K/H instead derive them from general theory, an exogenous bundle of gen-

eral assumptions about the equally unobservable properties, motivations, and causal powers of the social world--agents as mechanisms. Unobservable mechanisms with real causal powers, operating with natural necessity, however, require a fresh story about what the world is like and how we know. But there's no trace of such a story in K/H's argument. Positivists have always told us not to trust any theoretical claims that did not address the problem of knowledge. 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. K/H's chain of inferences from general theory, to laws, to mechanisms may be logical, but in a postKuhnian universe, logic does not make it true. K/H 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 K/H do offer is cobbled together from a hybrid mix of pre-Kuhnian anti-realist deductivism, and an antipositivist theoretical realism. The anti-realist elements they use are: A) a logic of justification limited to 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 deductivist logic of confirmation, in which truth is confirmed through observation language, and also adhere to C -- a postpositivist theory-centered view of justification which 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 first principles.

The Limits of Pre-Kuhnian Deductivism

K/H lambaste what they allege to be the empiricist and inductivist practices of historical sociology (pp. 5-9, 12-17). The philosophy of science, they claim, 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, it 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 1983:149-166; Miller 1987:453-461; Pickering 1992). But this has changed dramatically in recent years as revisionist demystifications of the text-book stories have converged with Kuhn's original criticism of both deduction and induction, undermining a disproportionately deductivist-driven view of scientific advance (e.g.,Camic and Xie 1994).

The critique of deductivism also entails epistemological doubts independent of such recent corrections in the historical record. For one, Popper's criterion of falsification relies on "under-specified criteria" of what counts as a 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 faulty measuring devices or to the faultiness of the supplemental hypothesis, leaving the favored one intact (Quine 1957; 1963,1969; Hollis 1994:79-80; Rosenberg 1988:47). It is overwhelmingly obvious in the social sciences, moreover, that the test of falsification--in which a single counter-observation to the results hypothesized serves to "falsify" the entire theory--is virtually *never* practiced (consider Marx's theory of class formation [Somers 1996b]); and for good reasons, given that more than one theoretical construction can almost always be placed on a body of evidence (Kuhn 1970:76).

Perhaps less known, but equally important, is the degree to which well-established scientific theories can survive and thrive despite bearing the burden of rather mind-boggling implications--that matter is made of molecules, for example, combined with the best-established theories of force, until the last few decades entailed 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 such as dismissing counter-instances as taken from insufficiently similar conditional situations, without specification of what would count as sufficient similarity (Miller 1987:232). 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, immediately makes suspect the idea that there will ever be firm consensus on what data *counts* as disconfirming in the first place-- thus allowing too much scope for theories to claim the status of having been corroborated. Even more conducive to theoretical hubris, corroboration as merely the absence of counter-evidence would mean that the more truly abstract the theory--and thus not amenable to concrete observations--the more likely it was to be considered valid; that planets of stars other than the sun are Camembert moons, for example, can be considered corroborated since we have no way to subject this hypothesis to the test of any actual observations. Through failure to falsify, then, "All emeralds are green" and "All emeralds are grue" have both been corroborated (Miller 1987:235-6), just as through failure to *yet* falsify, "all post-Soviet capitalist regimes lead to socialism" allows it to stand, technically, as corroborated.

What these problems consistently 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 K/H do. Skocpol (1994:323), for example, rightly

points out that K/H 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 what they claim, in the abstract, any 'adequate explanation must entail'." They likewise simply 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 "empiricism" and "induction" (K/H 1991:).

One (by now well-worn) such logical, not substantive, criticism attacks the ability of historical sociology to achieve case independence (K/H 1991:13). In addition to ignoring the enormous network theory literature that takes issue with these assumptions, this also presupposes the naive belief that cases are given in the nature of things rather than constructed along analytic parameters on the basis of problem-driven research specified not by some essential identity of the case but by the problem the researcher has set out to explain. Historical sociologists, by and large, draw the lines of independence between cases depending upon what it is they are trying to explain; case selection is an empirical historical not a logical operation. What one wants to establish is that an object is an empirical causal unity for the purpose at hand.[23] 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 her cases were not simply predefined as "countries" (p. 322) but drawn up analytically precisely to be able to test the causal impact of Russia's state apparatus as it weakened over time between 1905 and 1917. Similarly, Putnam (1993) and Somers (1993) draw theoretical conclusions by comparing contrasting political cultures across different regions of a single country--cases that would be ruled non-independent (because in the same national society) according to an abstract logic that leaves unproblematized the crucial question of "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 to "the logic of the causal hypotheses being presented and tested" (Skocpol and Somers 1980:194), that is, according to what it is that is being problematized. Following Marc Bloch's (1934:81) maxim: "Only the unity of problem makes a center," independence is defined analytically along substantive not logical dimensions in full recognition that the construction of cases is problem-, not logic-, driven in those studies whose findings have held up best over time.

Postpositivist Deductivism

K/H specify three criteria to adjudicate among competing theories: "plausibility, reduction of time lags between cause and effect, and the empirical implications" (K/H 1991:6). Each of these criteria, including the third, is *not* either a logical or an empirical criterion but a substantive theoretical one--in this case rational choice theory. The test of a good theory for K/H, then, is whether it meets the criteria of the particular theory favored by the researcher. This rather unworkable hybrid of antipositivist theoretical realist deductivism manifests itself in the circular reasoning K/H employ in their various

discussions of these criteria. First they present us with a criteria based on logic: "The success of such studies ultimately rests on the degrees to which they meet the requirements of good causal explanations" (Hechter 1992:368). Then, in light of the sobering recognition that one of their logical criteria--providing causal mechanisms--relies 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 1992:368). These assumptions generate logical inferences which tautologically confirm their hypothesized ontology of the world. Theory, in other words, provides the data by which that same theory is to be judged.

Consider again their criterion for theoretical success being the "greatest reduction of time lags between cause and effect." Citing Elster [1983, p.24] K/H state that the best way to reduce the time-lag between cause and effect is to replace macro-variables by micro-variables or to link macro-level variables using intervening micro-level ones (p. 7). But micro-foundations in the way they use them are not logical criteria; they are the central metatheoretical principles of rational choice (see e.g. Elster 1989). Hence the problems with historical sociology that K/H refer to (p.16)--historical sociology's "inadequate micro-foundations"--turn out to be not logical ones at all, let alone empirical ones, but disagreements among rival *theoretical* views of the world.[24] K/H 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 criteria of testability, K/H now appear to follow Friedman's (1953) conception of "positive economics" and claim that these 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 K/H are also post-positivist theoretical realists, for whom this distinction cannot no longer hold once it is recognized that in deciding on which facts to select and transform into evidence, we are already to a large degree deciding between rival theories (Hollis 1994:79). Moreover, as theoretical realists they believe that general theory is a great deal more than heuristics; it is more significantly 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 Friedmanite sense) would be to concede to positivism's anti-realist rejection of true causal knowledge about the deep structures of reality. With this concession would collapse the entire rational choice project.

Thus vindicating the unrealistic character of their assumptions on the instrumental

grounds of their utility in generating predictions cannot coexist fit with the simultaneous demands either for a theory structured by general laws, or for a (true) theory of causal mechanisms. Referring to the first incompatibility, Green and Shapiro (1994) 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 unrealism is justified on instrumental grounds (in which case the particular mode of theory building is beside the point; testable predictions are what matter)" (p. 31, emphasis added). And as for the second, one cannot claim that theory is *both* the grounds for establishing 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. In fact as Friedman would be the first to concede, causal mechanisms cannot be deduced from "as if" explanations which instead lend themselves to predicting empirical regularities, not causal accounts of "reality" (Friedman 1953).

Even K/H's criteria of testing via "empirical implications" turns out to be a theoretical, not an empirical test, since it means being able to demonstrate the power to predict something the theory *already* believes in--e.g. free rider problem (K/H: 8-10). It is not enough to claim, moreover, that their predictions work "retroductively" (Fiorina and Schepsle 1982:63), as rational choice often does. For one reason, I have emphasized that it is almost always the case in the social sciences that several theories predict consistently with the facts. For another, the kind of consistency being claimed here is not one in which an empirical puzzle--e.g. the occurrence of similar revolutionary outcomes in such vastly different times and places as the French, Russian, and Chinese revolutions--is suddenly explained by a theoretical model or hypotheses that fits consistently not only with each set of drastically different empirical data from these three cases, but also with other negative cases of the same phenomenon (e.g. Skocpol 1979, 1984). Rather, the kind of "prediction" and "testing" that K/H use to exemplify the superiority of rational choice's theory-driven approach over the problem-driven one of historical sociology, is a *post hoc* one in which facts that are already known are "explained" in a post hoc account fashioned to be consistent with the originally posited cluster of rational choice assumptions about essential and ultimately untestable faculties and properties of the theoretical world. Such post hoc explanations of already given phenomena consistent with the original hypothetical assumptions amounts to little more than the ability to rewrite in a "tribal language" (Skocpol 1994:325) 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 1994:34). Telling a rational choice story about data that's already been observed hardly qualifies as passing the "testability" that K/H 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 fur-

nish the same prediction. Unless a given retroductive accounts is used to generate hypotheses that survive when tested against other phenomena, little of empirical significance has been established" (pp.35-36).

Finally, K/H use yet another set of time-worn criteria for good theory--"parsimony," "universality," "scope," "boldness," etc. But these fare no better than their testability claims. Why, after all, should we prefer these aesthetic qualities to, say, the "complexity, expansiveness, and historicity" advocated by Dessler (1989:446),[25] or the now classic statement of Hirschman's (1984) "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 in the next section; but one can neither *confirm nor deny* it. That makes it a justification wholly at odds with K/H's original premise that the ultimate test of a theory is its testability.

To sum up this section: K/H's 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 using the unobservable world in theoretical accounts. K/H's version of deductivism, by contrast, is the aggressively antipositivist deductivism of theoretical realism. But as theoretical realism believes in unseen entities on the grounds of theoretical logic it does not fit Popper's requirements for theory-testing by data ("conjectures and refutations"); it is a deductivism in which hypotheses are not only "guided" (as discussed above) but *adjudicated* on the basis of general theory. Thus K/H's problems widen when they splice their post-positivist theory-centrism together with the positivist language of deduction. A postKuhnian theoretical realism combined with a pre-Kuhnian deductivism thus creates a hybrid suspended between what K/H call "empiricism," and a "theoreticism" 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 (1994:36) remarks on rationalism more generally, makes the purpose of a theory (of action, for example) 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. Hence K/H's dismissal of the concern for accuracy and empirical detail, and their penchant for dismissing as "naive empiricism" any alternatives to 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: To "outpace" the data that is to be placed in its pigeon holes (e.g. Lakatos 1971). The problem remains: We still have been given no good reasons to believe it.

THEORETICAL REALISM: AN ONTOLOGY FOR THE ANGELS

In his recent discussion of how "to explain political processes," Tilly (1995a) criticizes sociologists' widespread use of "invariant models concerning self-motivating social units"(p.1596). His criticism is important and unusual; in lieu of the traditional exclusion of ontology from discussions of social science methodology, Tilly is insisting that because there is, willy-nilly, an inevitable influence of ontological assumptions--"the nature of that which is to be known"--on epistemology--"the conditions for the generation of knowledge," it is important, above all, that those assumptions be "plausible" (Tilly 1995a:1602).[26] In keeping with this approach, let us examine first how K/H relate their epistemology to their ontology, and in turn take seriously K/H's own suggestion (1991:6) 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, K/H derive their ontology from their prior claims about what logical forms of explanation are acceptable. This strictly positivist approach reflects a 3-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 according to the covering law model; 3) because we already know x's ontological properties through the laws applicable to any given x, we need not bother ourselves with examining how x might be influenced by causal mechanisms through which x's are related to y's. Such mechanisms are held to be derivative from and entailed by x's causal laws.

At the same time, however, and completely at odds with the above, K/H tell us that their laws are necessarily imputed from general theory. Because, as I stressed earlier, 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, following both Tilly and K/H: How "plausible" is it?

The basic ontological assumptions of rational choice are well known--that all actors apply the standards of means-ends rationality, that they are self-interested, and they are wealth maximizers. These assumptions have been challenged forcefully at every turn and from every conceivable angle, and it is not my purpose to repeat those challenges.[27] Here, my focus returns to the implications of K/H's ontic methodology. Recall the basic agential units of analysis in rational choice: Not just agents, qua individuals, but agents that come already equipped with the essential properties of purposiveness and intentionality that will *by necessity* cause them to "pursue prespecified goals" (K/H 1991:19). What emerges, then, is that the basic agential units actually are *in themselves* causal forces--what

Tilly (1995a:1595) calls "self-propelling" entities. 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 and exogenously self-galvanized by self-contained autonomous mental states (reasons)--thus making them "coherent, durable, self-propelling social units--monads" (Tilly 1995a: 1602).

But it was no less that Karl Popper ([1934] 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 subjects of analysis as constituted by a set of inherent attributes posited under its rubric--attributes intended to represent its *essence*; hence for K/H and rational choice, the exogenous causal power of intentionality. Essentialist ontologies, in this sense, are pre-social, almost Hobbesian (Pizzorno 1990). They posit not only fixed solipsistic identities but identities that come equipped to act through the mechanisms of their own autonomous momentum. Questions we would want to ask about this ontology include: How we can know the "essence" of an unobservable mental state in the first place? Can the single 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 "essential" pre-social or fixed categories constructed from exogenous attributes (Somers and Gibson 1994:55)? And is it not possible that these "essential" identities are "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" (White 1992:8)?

Because theirs is an ontology that comes already inscribed with predetermined causal mechanisms, I find implausible K/H's perception of the social world--namely, that it really is comprised of agents with essential and unchanging properties that operate independently of the very relationships by which they are constituted. And I also find implausible their world of invariance, a "world in which whole structures and sequences repeat themselves time after time in essentially the same form" (Tilly 1995a:1602). K/H's ontology of agents-as-mechanisms invokes a world populated by "angels"--ontological entities which, implicitly, are not of this social world. They possess coherent attributes and essential properties whose actions are known in advance and measurable in autonomous, abstract, units. No wonder K/H are so confident of their predictive capacity: A world of angels possesses few surprises, founded as it is on invariant properties and attributes knowable a priori as the causal mechanisms that inexorably do their work in isolation from other entities or processes.[28] To be sure, as Tilly (1995a) ponders "if the social world actually fell into neatly recurrent structures and processes... invariant models, and deductive hypothesis-testing would become more parsimonious and effective means of generating knowledge"(p. 1602). The problem, however, is that while this "would be a convenient world for theorists...it does not exist" (ibid:1596).

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;9). For theoretical realists this conception of the categorically inherent properties of reality is the final adjudicator of reason. These assumptions generate logical inferences that tautologically confirm their hypothesized ontology of the world--its universal, omnitemporal nature. This suggests that, in the final analysis, K/H'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 essential and unchanging properties. The irony is that as realists, K/H do believe that a "real" material world exists "out there." Yet, when the relational, and contingent element of time and being is removed, it becomes a world of pure idealized spirit (Stinchcombe 1978:21). At the end of the day, one can only agree that K/H'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--angels, in short" (White 1992:301). A world of angels may be parsimonious and "convenient" to theorize, but as Tilly suggests above, it is hardly real.

RELATIONAL REALISM

Relational and pragmatic realism is a postKuhnian perspective for the rest of us--those who are not angels. As minimalist (Longino 1990; Humphreys 1988), relational realism has three limiting principles: First, belief in the causal power of a theoretical social dynamic (e.g.,gender, utility maximization, class struggle), must be independent from absolute belief in any one particular theory. Following Kuhn's (1970:173-74) mandate to think of theory advance as movement *from* present knowledge, rather than *toward* absolute truth, this injunction endorses Taylor's notion of "epistemic gain" in which knowledge is understood to be limited to "movement from a problematic position to a more adequate one within a field of available alternatives" (Calhoun 1995:36; Taylor 1989). Second, for all theory, especially one like rational choice that is premised on an a priori causal ontology, one is *owed* an epistemology--some very good reasons--for why we should believe it. And third, there are no universally valid principles of logical reasoning; there are only problem-driven ones (Kuhn 1970; Hesse 1980; Lloyd 1993; Miller 1987:486). Since space prohibits me from detailing the complexities of relational realism, 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 Ontology

A relational realist ontology is for those of use who *accept*, however unwillingly, the brutal fact that we and our social world are not angels, existing outside time and space,

but living, breathing, changing, dying creatures and entities, embedded in time and constituted through changing relationships. Beginning with the postulate that we are neither monads nor self-propelling entities but "contingent, transitory connections among socially constructed identities" (Tilly 1995a:1595), a relational realist 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 unit *process of interaction* between and among identities (Collins 1981; Pizzorno 1986,1991,1995; Stinchcombe 1991, 1992,1993,1995; Tilly 1995a,b,c). 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 1992,1993,1995; Aminzade 1992,1993; Griffin 1993; Somers 1992,1994,1996). They do not do this because they want to be "particularizers"; if they choose narrative forms it is because *they already believe* that the world is made up of things that are constituted through temporal and spatial relationships and thus must be represented in those 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 1994:61,65).

A relational ontology thus follows Popper's rejection of "essentialism" and instead looks at the basic units of the social world as relational identities embedded in relational configurations. In place of a language of essences and inherent properties, a relational realism substitutes a language of networks and relationships. Relational subjects are not related to each other in the weak sense of being only empirically contiguous; they are ontologically related such that identity can only be deciphered by virtue of its "place" in relationship to other identities in its web (Polanyi 1957b; Block and Somers 1984; White 1992; Somers 1993). What appear to be autonomous categories defined by their attributes are thus better reconceived as historically shifting sets of relationships contingently stabilized in sites. Hence Cassirer (1953): Things "are not assumed as independent existences present anterior to any relations, but...gain their whole being...first in and with the relations which are predicated of them. Such 'things' are terms of relations, and as such can never be 'given' in isolation..."(p.36, cited in Emirbayer 1996:5).

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 (forthcoming), 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); and, as per Smelser (1992), to conduct research into "the question of the contextual conditions--motivational,

information, 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 what is true of rationality is of course 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 1994:61,65).[29]

Relational Realism and "Adequate" Explanation

What counts as adequate explanation for relational realism can be distinguished in several ways from theoretical realism in its convergence with elements of a postKuhnian historical epistemology. First, the basic mechanism of causality is not within the single agent, 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." Thus relational realism takes on board rational choice's criticism of functionalism's neglect of mechanisms without reverting to what I have been calling "agents as mechanisms." If the basic causal mechanism is the contingent relational pathway, rather than the mind of the self-propelling agent, it is not surprising that the social sciences are increasingly recognizing the necessity of explaining outcomes through what is most commonly known as *path-dependency* and *sequence* analysis (Abbott 1983,1984,1992,1995; Aminzade 1992; Arthur 1989; David 1985; Aminzade 1992; Tilly 1995a, 1995b; Putnam 1993; Sewell 1996; Stark 1992; Steinmetz 1994), but also can be theorized as *causal narrativity* (Somers 1994b, 1996a). Adumbrated in Kuhn, and brilliantly demonstrated in David's (1985) renowned economic analysis of the QWERTY keyboard, path dependence is a theory of causality that incorporates the sense of 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 1992:462; see also Skocpol 1979). Path-dependence--especially when done through comparative trajectories (as is ideal)--allows us to hypothesize, theorize, and test the causal status of previously embedded institutional practices; 14th-century legal institutions, for example, can be shown *under certain initial and subsequent conditions* to not simply disappear or become "lag effects" with the onset of capitalist markets in the 18th century, but to become causal factors in the development of 19th-century democratic institutions (Somers 1994b; see also Putnam 1993). Path-dependence suggests that earlier institutional processes are thus "sedimented" into the core of some of our most modern phenomena; they thus help to explain why certain of those phenomena, and not others, became possible in the first place. One of the most significant contributions of path-dependency or causal narrativity is therefore that it makes it possible to theorize not just social change (long the sociologist's fixation), but the equally important--and neglected--phenomenon of social durability or institutional patterns of (seemingly inefficient)

resistance (e.g. the QWERTY keyboard, or the English common law). These kinds of findings and theorizations would not be possible using, say, a traditional variables approach in which each historical epoch is defined as a different case (Abbott 1992a). 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;1993), Stinchcombe (1978), and Tilly (1984;1990)--four of K/H'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 K/H's argument--laws and mechanisms. "How" questions (processes/mechanisms) cannot be deduced either from invariant laws (constant conjunctions/relations) or from the exogenous essential causal properties and powers that comprise K/H's notion of general theory.[30] The uncoupling of mechanisms from laws is justified 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 sorts of aims yield different explanatory demands.* Thus laws are feasible to use 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 properties/dispositions govern objects in time and space; they only govern objects in abstraction (Cartwright 1983). The answer to a "how" question, rather, demands explanatory and empirically contingent causal pathways. This is evident in the respective rules for the two types of accounts. Competing theoretical treatments--treatments which offer different general laws for the same phenomena--are encouraged in physics. By contrast, only a single causal story is allowed. Causal stories do not tell first one causal story then another according to their convenience. 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 convenience (e.g. there are dozens of fundamental theorems for laser operations and scientists openly choose one or another depending on other factors, Cartwright 1984 pp. 11-12). And not surprisingly, although philosophers generally believe in laws and deny causes, because of the specificity of causal stories actual practice in physics works in just the reverse.

Let's 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 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 doesn't give us an explanation for how prices are set. Why not? Because its an un-

disputed fact in economics that no manager or any economist has the slightest idea of what the marginal cost of producing something is. For a cause to explain, the cause really has to exist; it has to be identified and exhibited. An "as if" underlying theory is <u>not</u> a cause, it exhibits no evidence. No less than Milton Friedman has admitted that the actual throught processes of managers cannot in practice resemble this model. We have a covering law but we don't have an explanation.

Since causality, unlike generality, involves "stories", it is also the historicity of the causal explanation that is equally relevant for the defense of history against the attacks of the general theorists. 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 <u>suggests</u> causal mechanisms-- eating less causes the effect of losing weight--and it may also be a powerful predictor, but it doesn't in fact <u>explain</u> the mechanism of how that actually happens. The statement doesn't exhibit a cause. How do we exhibit a cause? We tell a story, a narrative about how a class of events is actually affected by something else. 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 it uses up fat when it draws that energy, etc. etc. Along the way we may use general laws but they aren't in themselves explanations for why, when we eat less, we lose weight. The moral of this story is that in science as well as sociology, an explanation which actual- ly depicts causal mechanisms is always told in narrative form. It is a set of sentences with 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. The historical and temporal dimension of comparative history is thus as important as its comparative component for it entails explantory narrative. It is narrative because the explantion is embedded in time, and moves through time. Indeed the success of any explanation resides in its accounting for temporality and sequence.

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 position of dismissive condescension, relational realism is devoted fully to the theoretical enterprise of causal explanation but from an ontology based on relational mechanisms. At issue in proposing a causal theory based on path dependency is not (as K/H insist) the choice to eschew generality in favor of particularism; rather it is the choice to find a causal approach to history that builds on the element of temporal process that is at the heart of any notion of a causal mechanism (e.g. Coleman 1986,1990). By example, rela- tional realism shows us that when we detach causal explanation from its "subsumed" place as a predicate to a covering predictive law, we are not left with a naive particularism, as K/H suggest, but with Tilly's (1994) notion of time as one "drenched with causes that in- here in sequence, accumulation, contingency, and proximity (italics added, p.270). Path dependence as a form of causality thus converges with recent work in biology that sug-

gests that many of our natural laws may hold *because* they explain things that share a common history (Hull 1977, 1978, 1981).

Comparing historical sociology to evolutionary biology more generally clarifies the relational realism explanatory model, as everything that K/H 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 form the foundation of biology, along with genetic theory. It is, however, full of causal mechanisms and generalizing theory that explain how things happened (Kauffman 1993). There are no laws for the extinction of species, for example, but there are indeed mechanisms: a comet hits...kills the dinosaurs thousands of years later. It was not, moreover, any intrinsic or essential properties 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 through historical comparison -- or what Gould (1989) calls "replaying the tape." Gould still requires that we test for causal attribution. He employs a kind of comparative causality that he calls "consilience" -- in which many independent sources "conspire" to indicate a particular historical pattern. The comparative test is crucial: If any of these 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; Tilly 1995a,1995b on counterfactual confirmation). This method shows that Y makes sense and can be explained "rigorously" as the outcome of the causal process of A through C plus X. But "no law of nature enjoined" this 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, p.278, 282-83, 288).

How do we know if a theory is true? Relational Realism and Problem-driven Reasoning

Kuhn showed that knowledge is neither primarily theory-driven (deductive) or data-driven (inductive) but *problem-driven*. This moves us beyond the limited choice of either induction or deduction and calls into question the legitimacy of deductivist theory-centric name-calling that attributes "empiricism" and "naive inductivism" to any kind of reasoning that is *not* theory-driven. Recent postKuhnian developments in science studies especially cast doubts on an unbalanced theory-driven view of science, knowledge, and the practices of inquiry. Whereas theory-driven epistemologies are constructed from idealized reconstructions of science, what Hacking (1983, 1992) calls an emphasis on "intervening," Pickering (1989, 1990, 1992a, 1993) stresses as "the temporality of practice", Humphreys (1988) "empirical realism," and Bohman (1991) "the new logic of social science," all follow Kuhn in rejecting philosophical images in favor of reconstructing the actual histories and practices of discoveries in science and the social sciences. The shift entails a move away

from philosophical criteria of confirmation to "problems as they emerge in the theories and research of scientific practice itself" (Bohman 1991:53). As a result new emphasis has been placed on the practical elements of inquiry, and on the diverse criteria that justify theories (see especially Pickering 1992). A more experimental and practice-centered view of theory confirmation than allowed by the either/or of deduction vs induction has thus emerged. Testing theories by observing only those phenomena designated by a pre-existing hypothesis is only one among a wide variety of avenues along which science leads us toward path breaking knowledge, suggesting that the relative import of general theory-driven logic has been radically exaggerated (Galison 1987, 1989; Gooding, et. al. 1989; Gooding 1992; Pickering 1989, 1990, 1992a, 1993).

Kuhn prefigured the new trend in pointing out that beneath the false alternatives of induction and deduction there was something called "history" and "articulation" that could significantly advance how we understand scientific development. Hacking (1983) has been most influential in his elaboration of the notion of articulation. He adopts Kuhn's term to represent a more complex bridge between hypotheses and data than the either/or of deduction or induction (pp. 213-16), and explains it as a more accurate representation of the practice in which scientists "create the phenomena which then become the center-pieces of theory" (1983:220). Against disproportionate theory-centrism, Hacking (1983:17) 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" (and see OED 1954:567). Since then, the historically-forged notion of articulation has been widely developed into a full-blown alternative *practice-centered* conception of theory construction in science (see especially Pickering 1992, 1995). In social science, Stinchcombe (1978) foreshadowed this trend when he said that his "argument is 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 (1996) 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, c) building models to fit our substantive problems, and not the other way around. Now that it is increasingly becoming recognized that the problem-driven approach has "a life of its own"--not reducible to either induction or deduction--a disproportionate and singular adherence to deductivism can no longer be feasible (see also Humphreys 1992).

Theorizing convincingly about mechanisms, then, is a task requiring neither pure induction nor pure deduction but one that requires devising diverse and creative ways (e.g. abduction) 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 the

mechanisms triggered by Putnam's (1993) differing degrees of social capital really viable theorizations for varying patterns of democratization? Only through research practices deploying substance-driven comparisons and problem-driven manipulations can we get an answer to whether a "theoretical entity" such as social capital actually what Abbott (1996:873) 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" (p.873; emphasis added)?

TAKING STOCK

I began by posing a puzzling paradox: How is it possible that K/H's attack on historical sociology could have its epistemological roots in a recognizable postKuhnian philosophy of science and yet to end up at militant odds with the very incorporation of history that Kuhn so eloquently advocated? Let me briefly crystallize the three-part argument through which I have addressed this paradox. First, I teased out what were competing interpretations of Kuhn's argument and, from these, drew parallel trajectories of two postKuhnian realisms--theoretical and relational. Of the two routes out of Kuhn I have examined, the dominant one was a "theory-centric" antipositivism that converged with Popper's deductivism even while it adamantly refused deductivism's ultimate foundations in the language of observation. Deeply opposed to positivism's antirealism, the theory-centric reading of Kuhn gave a new lease on life to age-old rationalist epistemology of *theoretical realism*. Given Kuhn's original challenge, the greatest irony is that among the major casualties of theory-centrism was history--once again relegated to the status of the "anti-theoretical" epistemological "Other" to science.

The paradox that I have tried to point to throughout is that although anticipated by Friedman's defense of predicting from unreal "as if" assumptions, rational choice's non-instrumental belief in the reality of its assumptions was a feat only made possible by the theory-centric version of the Kuhnian revolution. Once the "theory-laden" (hence suspect) character of pure observation had been asserted, not only induction with its ultimate foundations in observation but even more so "mere history" became tainted with the label of empiricist. With this the way was wide open for theoretical realism to claim a higher form of epistemological validity in the power of abstract reason, logic, and first principles. To counter with worries about the lack of empirical support for its claims is to be met with more dismissals of "empiricism" and "anti-theoreticism," such that drive K/H's argument. Rational choice, the theoretical position from which K/H 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. Conceding only that narrative may be useful for purely "illustrative" purposes, K/H invoke memories of Kuhn's critique of any claims to knowledge that limit 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 follows his challenge to liberate "history" from its contained status as descriptor, anecdote, or illustration. Adumbrating a historical epistemology he tried to show that theory must be considered historical in a double sense: 1) That 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) that for any theory to really provide an answer requires a structure of causal linkages and what Tilly later called the thick sense of history. Concomitantly, this meant that "what is history" could no longer be caricatured as random particularities excluded from the domain of true theory, eligible only as "anecdotes" or "illustrations." Instead he showed history to bear within it the problems that drive all knowledge, and to in turn be the stuff of sequences and relations that we can arrange into causal interventions and explanations.

The third dimension of my argument has thus been to make explicit and to challenge the epistemological foundations of K/H's rational choice theory. With respect to their criteria for what counts as an "adequate explanation," K/H: a) use a tautological methodology in which they impute mechanisms to the case at hand from an ontology comprised of--mechanisms; b) they confound the rules of covering law model with rational choice's rules for mechanisms--hence mixing realism and antirealism with respect to observation rules; and c) they confound predictive laws with true causality--thus preempting the very process-centered explanation for which they advocate. In considering their criteria for justification, we are deserve better reasons for why we should be convinced than merely appealing to elegance and parsimony. And with respect to their ontology, the problem is not primarily the individualism of their basic unit of analysis, but that they ascribe to this individual agent a set of a priori self-contained causal properties that necessarily (deterministically) propel actions independently of its embedded constitutive relationships--thus eliminating the contingency and probability now recognized to be essential in causal theories (Lieberson 1992). K/H thus present a mixture of pre-Kuhnian deductivism and covering law model logic with a postKuhnian theory-centrism and commitment to causal mechanisms. In the first instance, they rely explicitly on the authority of Hume. The price of starting with Hume, however, is that because Hume insisted that experience gives us only particulars and correlations ("constant conjunctions"), one cannot say much at all about causal laws except in terms of statistical significance and can nothing at all about causal mechanisms (except that there aren't any). Since K/H are not willing to pay this price, they turn to theoretical realism and its claim that what is unobservable is "real" by virtue of theoretical logic; this gives them license to try a tale about the truth of universal and a priori axioms ("general theory") imputed to causality as confirmed when its "implications" are confirmed by that same theory.

Conclusion

Kuhn knew the dichotomous caricatures between history and theory would not die easily and even suggested that the "main obstacle" to the acceptance of his argument would likely be similar to the attacks on his historical thinking that confronted Darwin's *Origins of Species* (1970:171-72). That the chief interpretation of Kuhn today is as a "relativist" today confirms just how right he was. Twenty years later, 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 [of history vs theory being a false dichotomy] as twisting it back into the psychology of generality that it rejects. The Kantian versus Nietzsche (or positivist versus 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)." Although recent work in the philosophy of science would suggest that the "historic turn" in science is making it impossible for theorists to play the science card any more in criticizing history, K/H's attack on historical sociology as "antitheoretical" is but the latest manifestation of just how hard it is to say what an historical epistemology is about. Trundling out the same tired caricatures of both history and theory, they prove just how intractable the false dichotomies are, and as indefensible now as they have ever been. Paradoxically, the result is that two leading historical sociologists are taking us right back to the anti-historical position that Kuhn originally challenged--free now, apparently, as "post-Kuhnians" 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 tyranny of arbitrary dogma and prejudice. In the absence of this discipline, the appeal to "theory" can be as much a mystification as the blunt sword of "empiricism." The theoretical world that rational choice theory inhabits may well be, as K/H's claim, "parsimonious" "bold," even elegant; but we are owed, nonetheless, justification for believing in it.

K/H are quick to scrutinize and condemn the errors of historical sociologists. They have been less quick to examine and question how their own epistemological strictures have histories, histories not of refinement towards greater absolute truth, but rather histories of 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 1992); they have been naturalized to the point where K/H are able to get away with confounding the formal logic of epistemology with the specificities of rational choice theory--thus making impossible any open debate over methodology--and then privilege theoretical realism over "mere history." They have in effect closed out the constructive benefits of engagement among rival positions (being forced to clarify presuppositions, broaden purview etc.) and averted the difficult questions of intents, purposes, and consequences of social science more generally.

But the consequences of their imperial claims are ironic: Kiser and Hechter have once again reminded us of the post-positivist "fact" that all facts are "theory-laden," and that histories are organized by theoretical categories. But shouldn't K/H wonder if their own evaluative criteria are themselves not *history-laden*, and thus contingent and embedded in a larger field of less angelic, more pluralist, possibilities?

Endnotes

1. This paper originated in a panel on history and theory at the *American Sociological Association* Meetings where Edgar Kiser, Michael Hechter, and myself had a stimulating discussion. Drafts have been improved by the comments of participants at seminars at the University of Chicago, and Northwestern University, as well as from many colleagues along the way, including Julia Adams, Renee Anspach, Mabel Berezin, Fred Block, Mustafa Emirbayer, Paula England, Walter Goldfrank, Martin Hollis, Ira Katznelson, Steven Lukes, John McCormick, Mark Mizruchi, John Padgett, Alessandro Pizzorno, Moishe Postone, Sonya Rose, William Sewell, Arpad Skakolczai, Marc Steinberg, George Steinmetz, Arthur Stinchcombe, Wolfgang Streeck, Charles Tilly, Bruce Western, Xu Xie, Mayer Zald, the anonymous reviewers, and the AJS editor. I thank especially Elizabeth Anderson, without whom this project would not have been possible. Financial support during the research and writing has been provided generously from the University of Michigan's Rackham Faculty Research Grant and Fellowship, the Office of the Vice-President for Research, and the LSA Faculty Assistance Fund, as well as from the European University Institute, Florence, Italy.

2. I say the "current" epistemological foundations with full awareness (as I point out *inter alia*) that the roots of RCT go back to 17thc. rationalism, 18th-19th century classical political economy, 20thc. neo-classical economics and decision theory, etc. My argument, however, is not an intellectual history but one addressing the epistemological conditions of possibility for its present resurgence--and these are absolutely post-Kuhnian.

3. If there is need for any further support for the longstanding epistemological link between the problem of observability characteristic of mechanisms and convincingly demonstrating true causality among scientists, consider that in October 1996 it is front page headline news in the *International Herald Tribune* that a "mechanism" has been discovered that actually demonstrates that *"for the first time"* that smoking "causes lung cancer" (*IHT*, Oct. 16, 1996:1).

4. My thinking about these issues has been enriched enormously through discussions with Martin Hollis as well as his own wonderful book on the topic (1994).

5. Kuhn's was not a lone voice among philosophers; he was in the tradition of Duhem (1954 [1906]), Koyre (1957), Quine (1963,1969), M. Polanyi (1958), and Hanson (1958). Why Kuhn broke through to a wider audience is a question for the sociology of knowledge (but see Alexander (1982). Kuhn himself famously disclaimed any implications for the social sciences.

6. "[O]ften the paradigm theory is implicated directly in the design of apparatus able to solve the problem" (Kuhn 1970:27).

7. We also do so to avoid suffering a complete "epistemological crisis" at every decision-juncture in our daily intellectual life (MacIntryre 1980).

8. Hacking (1983), for instance, tells us of radio astronomers Penzias and Wilson who in 1965 experimented on what they thought might be a meaningless phenomenon (because they were not hypothesis-testing) but which held *intrinsic interest* to them--namely the static found in transatlantic radio. After considerable experimentaion, they determined 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 Penzio and Wilson--among the few experimenters in physics to have been given a Nobel Prize. Yet when the story appeared in text-books it was *rewritten* to confirm to a theory-dominated one in which the experimenters were represented as *merely testing the already extant hypotheses of the theorists* (pp.159-161).

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 1959).

10. The concept of an historical epistemology is discussed in Somers (1990,1995a,b,1996) and 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 coherently coupled with "historical." While I appreciate the historical 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 indeed long been dominant (see Rorty 1979). Nothing in my argument suggests that I am a historicist--in the sense of believing that concepts only have meaning in their original specific contexts; rather, because I believe that the question of knowledge is a generic question to which all theories must be accountable, I find it justified to distinguish a generic and abstract use of epistemology from the history of one particular version of epistemology. Indeed I find the potential contradiction of history and epistemology to be usefully jarring, as evidenced in my prefacing the term as "purposefully oxymoronic"--something I develop at much greater length in Somers (1995a,b, 1996a),

11 Inductive-statistical (I-S) methodologies share a commitment to the deductive form of the covering law model of explanation.

12. Although its strict epistemological legitimacy has been considerably diminished in recent philosophical argument (Salmon 1988; Miller 1987; Cartwright 1983; Outhwaite 1988), as Steinmetz (forthcoming) 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 (p.3). See especially King et.al. (1992), Turner (1992), Wacquant (1993).

13. Gould (1989) and MacIntyre (1980) each capture the elegance of this approach, albeit in very different ways.

14. "Newton saw apples fall with his eyes but the force and law of gravity are not to be perceived" (Hollis 1994:4).

15. Although I discuss below 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.

16. Steinmetz (forthcoming) has made the reasonable criticism that he sees no compelling reason for why Bhaskar's "critical realism" is not an an adequate term for the kind of realism I call relational and pragmatic. Although I generally don't endorse the proliferation of neologisms, in this case I find myself enough at odds with Bhaskar's belief that causality is found in an entity's *essential properties* to require an explicit recognition of the *relational* dimension in my concept of realism.

17. Bertrand Russell once described the law of causality as "a relic of a bygone age" (cited in Entrikin 1991:110); Popper (1959) dismissed Weber as naive for not recognizing that causality can only be a feature of universal laws (p.722).

18. There are other reasons to object to the necessity of mechanisms; for the most powerful, see Stinchcombe (1991). What K/H really mean is that there is a consensus over the need for mechanisms among RCT theorists (e.g. Coleman 1990; Elster 1989). The mistake is not an accident since they use "theory" synonomously with RCT.

19. The locus classicus on "reasons as causes" is Hempel's "Rational Action" (1962). For discussion see Davidson (1980), Dray (1957), Rosenberg (1988), and Bohman (1991).

20. As RCT theorizes, there will of course be constraints and counter-forces that might present an obstacle in any given empirical example; but they will be just that--external constraints on a natural force. Hence the *ceteris paribus* clause. The example of brittleness is found in Bohman (1991:20).

21. Tilly (1995a: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 of standard ways "to explain political processes"--not one that he applies specifically to theoretical realism. Yet it captures especially well the post-positivist deductivism of rational choice.

22. Since that writing, rational choice theorists have discovered narrative--but only endorse a theory-driven version. See Bates et. al. (1997), Kiser (1996).

23. What counts as causal unity is an empirical question and so can change over the course of research. Very often one starts with common sense causal unities, e.g. people have intentions, that with additional information can be transformed or extended to less

common sensical ones, e.g. firms have intentions.

24. Putnam (1993), for example, would disagree. After extensively testing he decided that the roots are the critical explanation, just as Somers (1994) was able to demonstrate after testing three competing hypotheses that the long term one focusing on legal foundations was determinative. And for a very long-run analysis, see Tilly (1990).

25. "The richer and more comprehensive the underlying ontology,the better the theory" (p.446).

26. Tilly is hardly alone in recognizing the centrality of ontological assumptions on methodology, e.g. Alexander 1982, Archer 1988, Giddens 1984, and of course all of the literature on realism.

27. Among the most important are Anderson (1993), Block (1991), Hollis (1995), Green and Shapiro (1994), Pizzorno (1986,1991, 199), and White (1992). Two powerful recent critiques applied to specific areas are Adams (forthcoming), and Kim and Bearman (1997).

28. Although the unqualified 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).

29. Rejecting the either/or between individualism and holism, between agency and structure--but without resorting to the "mutuality" compromise--relational realism thus finds an analytic home in network theory (Bearman 1993, Bearman and Deane 1992; Breiger 1991; Burt 1980,1992; Laumann, Marsden, Prensky 1983; Padgett and Ansell 1993; Wellman 1988; White, et. al. 1975; White 1992,1993,1996; Wellman 1988), narrative/sequence analysis (Abbott 1992a, 1992b,1995; Aminzade 1992,1993; Griffin 1992, 1993; Somers 1994a,1996a; Steinmetz 1992; Stinchcombe 1978; Tilly 1995a,b,c), space-time structures (Aminzade 1992; Sewell 1996; Wallerstein 1991), relational identities (Pizzorno 1991, 1986; Somers 1992, 1994b; Tilly 1984,1995; White 1992), configurations (Skcopol 1984; Tilly 1984, 1988), and historical institutionalism (Steinmo et.al. 1992; March and Olsen 1990) each of which fits loosely under the general rubric of what Emirbayer (1996) has recently declared to be a *relational sociology.*

30. Paradoxically, that mechanisms cannot be deduced from laws is also the argument of Elster (1989); and see also Somers (1996a).

REFERENCES

Abbott, Andrew. 1988. "Transcending General Linear Reality." Sociological Theory 6:169-86.

Abbott, Andrew. 1992b. "From Causes to Events: Notes on Narrative Positivism." Sociological Methods and Research 20(4):428-55.

Abbott, Andrew. 1993. "Measure for Measure." Journal of Mathematical Sociology 18:203-14.

Abbott, Andrew. 1995. "Sequence Analysis: New Methods for Old Ideas." Annual Review of Sociology 21: 93-113.

Abbott, Andrew. 1996. "Things of Boundaries." Social Research 62: 857-82.

Achinstein, P. and O. Hannaway, eds. 1985. Observation, Experiment, and Hypothesis in Modern Physical Science. Cambridge, Mass.: MIT Bradford.

Adams, Julia. Forthcoming. "Culture in Rational-Choice Theories of State Formation." In State/Culture: New Approaches to the State in the Social Sciences, edited by George Steinmetz. Ithaca: Cornell University Press.

Alexander, Jeffrey C. 1982. Positivism, Presuppositions, and Current Controversies, Vol. 1 of Theoretical Logic in Sociology. Berkeley and Los Angeles: University of California Press.

Almond, Gabriel. A. 1988. "Separate Tables: Schools and Sects in Political Science." PS: Political Science and Politics 21:828-842.

Aminzade, Ron. 1992. "Historical Sociology and Time." Sociological Methods and Research 20(4):456-80.

Aminzade, Ron. 1994. Ballots and Barricades. Princeton, NJ: Princeton University Press.

Anderson, Elizabeth. 1993. Values in Economics. Cambridge, Mass.: Harvard University Press.

Arthur, Brian W. 1989. "Competing Technologies, Increasing Returns, and Lock-in By Historical Events." The Economic Journal 99: 116-31.

Arthur, Brian W. 1988. "Self-Reinforcing Mechanisms in Economics." In The Economy as an Evolving Complex System, ed. P. Anderson, K. Arrow, and D. Pines (Reading, MA: Addison Wesley).

Bates, Robert et. al. "Introduction," Analytic Narratives. Unpublished Ms.

Bearman, Peter. 1993. Relations into Rhetorics. New Brunswick, N.J.: Rutgers University Press.

Bhaskar, Roy. 1989. The Possibility of Naturalism, 2nd. ed. Hertforshire: Harvester Press.

Bhaskar, Roy. 1978. A Realist Theory of Science. Second Edition. Brighton, England: Harvester.

Bhaskar, Roy. 1986. Scientific Realism and Human Emancipation. London, England: Verso.

Block, Fred and Margaret Somers. 1984. "Beyond the Economistic Fallacy: The Holistic Social Science of Karl Polanyi." Pp. 47-84 in Vision and Method in Historical Sociology, edited by Theda Skocpol. New York: Cambridge University Press.

Bohman, James. 1991. The New Logic of Social Science. Cambridge,UK: Polity Press.

Boyd, Richard N. 1973. "Realism, Underdetermination, and a Causal Theory of Evidence." Nous 7:1-12.

Breiger, Ronald L. 1991. Explorations in Structural Analysis: Dual and Multiple Networks of Social Interaction. New York: Garland.

Burawoy, Michael. 1990. "Marxism as Science: Historical Challenges and Theoretical Growth." American Sociological Review 55:6:775-93.

Burt, Ronald S. 1992. Structural Holes: The Social Structure of Competition. Cambridge, Mass: Harvard University Press.

Calhoun, Craig. 1995. Critical Social Theory. Oxford: Blackwell Publ.

Camic, Charles and Yu Xie. 1994. "The Statistical Turn in American Social Science: Columbia University, 1890 to 1915." American Sociological Review 59: 773-805.

Campbell, Norman R. 1957 [1920]. Physics, the Elements. Reprinted in Foundations of Science: The Philosophy of Theory and Experiment. New York: Dover.

Carnap, Rudolf. 1966. An Introduction to the Philosophy of Science, edited by Martin Gardner. New York: Basic Books.

Cartwright, Nancy. 1983. How the Laws of Physics Lie. Oxford: Clarendon Press.

Cartwright, Nancy. 1989. Nature's Capacities and their Measurement. Oxford: Clarendon.

Cassirer, Ernst. 1953. Substance and Function. Trans. W. Curtis Swabey. New York: Dover.

Coleman, James. 1986. "Social Theory, Social Research, and a Theory of Action." American Journal of Sociology 91 (6).

Coleman, James. 1990. Foundations of Social Theory. Cambridge, MA: Harvard University Press.

Collier, Andrew. 1994. Critical Realism. New York: Verso.

Collins, Randall. 1981. "On the Microfoundations of Macrosociology. American Journal of Sociology 86: 984-1014.

Collingwood, R. G. 1979 [1939]. An Autobiography. Oxford: Oxford University Press.

Comte, August. 1830.

David, Paul. 1985. "Clio and the Economics of QWERTY." American Economic Review 75: 332-37.

Dessler, David. 1989. "What's At Stake in the Agent-Structure Debate?" International Organization 43: 441-73.

Duhem, Pierre. 1954 [1906]. The Aim and Structure of Physical Theory (trans. Wiener). Princeton: Princeton University Press.

Elster, Jon. 1982. "Belief, Bias, and Ideology." Pp. 123-48 in Rationality and Relativism, edited by M. Hollis and S. Lukes. Cambridge, Mass.: The MIT Press.

Elster, Jon. 1983. Explaining Technical Change. Cambridge, England: Cambridge University Press.

Elster, Jon. 1984 [1979]. Ulysses and the Sirens: Studies in Rationality and Irrationality.

Elster, Jon. 1989. Nuts and Bolts for the Social Sciences. Cambridge: Cambridge University Press.

Emirbayer, Mustafa and Jeff Goodwin. 1994. "Network Analysis, Culture, and the Problem of Agency." American Journal of Sociology 99: 1411-54.

Emirbayer, Mustafa. 1996. "Manifesto for a Relational Sociology" Unpublished Mss.

Entrikin, J. Nicholas. 1991. The Betweenness of Place: Towards a Geography of Modernity. Baltimore: The Johns Hopkins University Press.

Fetzer, James ed., 1988. Probability and Causality. Dordrecht: D. Reidel.

Fiorina, Morris P., and Kenneth A. Shepsle. 1982. "Equilibrium, Disequilibrium, and the General Possibility of a Science of Politics." In Political Equilibrium. Edited by Peter C. Ordeshook and Kenneth A. Shepsle. The Hague: Kluwer-Nijhoff.

Franklin, A. 1986. The Neglect of Experiment. Cambridge, England: Cambridge University Press.

Friedman, Jeffrey. 1996. The Rational Choice Controversy. New Haven: Yale University Press.

Friedman, Milton. 1953. "The Methodology of Positive Economics." In Essays in Positive Economics, edited by Milton Friedman. Chicago: University of Chicago Press.

Galison, Peter. 1987. How Experiments End. Chicago: University of Chicago Press.

Galison, Peter. 1989. "The Trading Zone: Coordinating Action and Belief." Mss. Stanford University.

Gooding, David, T. J. Pinch, and S. Schaffer, eds. 1989. The Uses of Experiment. Cambridge, England: Cambridge University Press.

Gooding, David. 1992. "Putting Agency Back into Experiment." Pp. 65-112 in Science as Practice and Culture, edited by A. Pickering. Chicago: University of Chicago Press.

Gould, Stephen Jay. 1981. The Mismeasure of Man. New York: Norton.

Gould, Stephen Jay. 1988. "Mighty Manchester." New York Review of Books, October 27.

Gould, Stephen Jay. 1989. Wonderful Life. Cambridge, Mass.: Harvard University Press.

Granovetter, Mark. 1985. "Economic Action and Social Structure: The Problem of Embeddedness." American Journal of Sociology 91:481-510.

Green, Donald and Ian Shapiro. 1994. The Pathologies of Rational Choice Theory. New Haven: Yale University Press.

Griffin, Larry. 1992. "Temporality, Events, and Explanation in Historical Sociology." Sociological Methods and Research 20:403-27.

Griffin, Larry. 1993. "Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology." American Journal of Sociology 99:

Gutting, Gary, ed. 1980. Paradigms and Revolutions. Notre Dame: University of Notre

Dame Press.

Hacking, Ian. 1982. "Language, Truth, and Reason." Pp. 48-66 in Rationality and Relativism, edited by M. Hollis and S. Lukes. Cambridge, Mass.: MIT Press.

Hacking, Ian. 1983. Representing and Intervening. Cambridge, England: Cambridge University Press.

Hacking, Ian. 1984. "Five Parables." Pp. 103-24 in Philosophy in History, edited by R. Rorty, J. B. Schneewind, and Q. Skinner. Cambridge, England: Cambridge University Press.

Hacking, Ian. 1988. "The Sociology of Knowledge about Child Abuse." Nous 22:53-63.

Hacking, Ian. 1990b. The Taming of Chance. Cambridge, England: Cambridge University Press.

Hacking, Ian. 1992. "The Self-Vindication of the Laboratory Sciences." Pp. 29-64 in Science as Practice and Culture, edited by A. Pickering. Chicago: University of Chicago Press.

Hanson, N. R. 1958. Patterns of Discovery. Cambridge, England: Cambridge University Press.

Hechter, Michael. 1992. "Rational Choice Theory and Historical Sociology." International Social Science Journal 133:367-73.

Hempel, Carl G. 1959 [1942]. "The Function of General Laws in History." Pp. 344-56 in Theories of History, edited by P. Gardiner. New York: Free Press.

Hempel, Carl G. 1965. "Aspects of Scientific Explanation." Pp. xx in Aspects of Scientific Explanation, edited by Carl G. Hempel. New York: Free Press.

Hempel, Carl G. and Paul Oppenheim. 1965 [1948]. "Studies in the Logic of Explanation." Pp. xx in Aspects of Scientific Explanation, edited by Carl G. Hempel. New York: Free Press.

Hesse, Mary. 1980. Revolutions and Reconstructions in the Philosophy of Science. Bloomington: Indiana University Press.

Hesse, Mary. 1974. The Structure of Scientific Inference. London: MacMillan.

Hirschman, Albert. 1984. "Against Parsimony." American Economic Papers and Procedings: 89-96.

Hollis, Martin. 1994. The Philsosophy of Social Science. Cambridge: Cambridge University Press.

Hollis, Martin and Steven Lukes, eds. 1982. Rationality and Relativism. Cambridge, Mass.: The MIT Press.

Hollis, Martin and Steve Smith. 1991. "Beware of Gurus: Structure and Action in International Relations." Review of International Studies 17: 393-410.

Hollis, Martin and Steve Smith. 1990. Explaining and Understanding International Relations. Oxford: Oxford University Press.

Hollis, Martin and Steve Smith. 1994. "Two Stories about tructure and Agency." Review of International Studies 20: 241-251.

Hull, David. 1977. "The Ontological Status of Species." P. 99 in Foundational Problems in the Special Sciences, part Two of the Proceedings of the Fifth International Congress of Logic, Methodology and the Philosophy of Science London, Ontario, Canada-1975, edited by R. Butts and J. Hintikka. Dordrecht, Netherlands: Reidel.

Hull, David. 1978. "A Matter of Individuality." Philosophy of Science 45:335-360.

Hull, David. 1981. "Discussion: Kitts and Kitts and Caplan on Species." Philosophy of Science 48:147.

Hull, David. 1988. Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science. Chicago: University of Chicago Press.

Humphreys, Paul. 1988. "Causal, Experimental, and Structural Realisms." Midwest Studies in Philosophy XII:241-252.

Humphreys, Paul. 1989. The Chances of Explanation. Princeton: Princeton University Press.

Kauffman, Stuart A. 1993. The Origins of Order: Self-Organization and Selection in Evolution. New York: Oxford University Press.

Keat, Russell and John Urry. 1975. Social Theory as Science. London: Routledge, Kegan Paul.

Keller, Evelyn Fox. 1985. Reflections on Gender and Science. New Haven, CT: Yale

University Press.

King, Gary, Robert O. Keohane, and Sidney Verba. Designing Social Inquiry: Scienctic Inference in Qualitative Research. Princeton: Princeton University Press.

Kiser, Edgar. 1995. "What Can Sociological Theories Predict?" American Journal of Sociology 100 (6):1611-1615.

Kiser, Edgar. 1996. "Narratives and Rational Choice." Politics and Society

Kiser, Edgar and Michael Hechter. 1991. "The Role of General Theory in Comparative-Historical Sociology." American Journal of Sociology 97:1-30.

Koyre, Alexandre. 1957. From the Closed World to the Infinite Universe. Baltimore: The Johns Hopkins University Press.

Kuhn, Thomas. 1970 [1962]. The Structure of Scientific Revolutions, Second Edition. Chicago: University of Chicago Press.

Kuhn, Thomas. 1977. The Essential Tension. Chicago: University of Chicago Press.

Lakatos, Imre and A. Musgrave. 1970. Criticism and the Growth of Knowledge. Cambridge, England: Cambridge University Press.

Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes." In Criticism and the Growth of Knowledge, edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press.

Lakatos, Imre. 1976. Proofs and Refutations: The Logic of Mathematical Discover. Cambridge, England: Cambridge University Press.

Lakatos, Imre. 1978. The Methodology of Scientific Research Programs. Cambridge: Cambridge University Press.

Latour, Bruno. 1987. Science in Action. Cambridge, Mass.: Harvard University Press.

Latour, Bruno. 1993. We Have Never Been Modern. Cambridge, Mass.: Harvard University Press.

Laudan, Larry. 1981. "A Confutation of Convergent Realism." Philosophy of Science 48: 19-49.

Laumann, Edward O., Peter V. Marsden, and David Prensky. 1983. "The Boundary Specification Problem in Network Analysis." Pp. 18-34 in Applied Network Analysis: A Methodological Introduction, edited by Ronald S. Burt and Michael J. Minor. Beverly Hills, CA: Sage.

Lenoir, T. and Y. Elkana, eds. 1988. Science in Context.

Leplin, Jarrett, ed. 1984. Scientific Realism. Berkeley: University of California Press.

Lewontin, Richard. 1984. "The Diversity of the Sciences." Paper presented at the 1984 Biennial Meeting of the Philosophy of Science Association.

Lewontin, Richard. 1992. Biology as Ideology: The Doctrine of DNA. New York: Harper.

Lieberson, Stanley. 1991. "Einstein, Renoir and Greeley: Some Thoughts about evidence in sociology." American Sociological Review 57: 1-15.

Lloyd, Christopher. 1986. Explanation in Social History. Oxford, England: Basil Blackwell.

Lloyd, Christopher. 1993. Structures of History. Oxord: Basil Blackwell.

Longino, Helen. 1990. Science as Social Knowledge: Values and objectivity in Scientific Inquiry. Princeton, N.J.: Princeton University Press.

MacIntyre, Alasdair. 1962. "A Mistake about Causality in Social Science." Pp. in Philosophy, Politics and Society (Second Series), edited by Peter Laslett and W. G. Runciman. Oxford, England: Basil Blackwell.

MacIntyre, Alasdair. 1980. "Epistemological Crises, Dramatic Narrative, and the Philosophy of Science." Pp. 54-74 in Paradigms and Revolutions, edited by G. Gutting. Notre Dame, Indiana: University of Notre Dame Press.

McDonald, Terrence. 1996. "The Historic Turn in the Human Sciences." Ann Arbor: University of Michigan Press.

Miller, Richard. 1987. Fact and Method. Princeton: Princeton University Press.

Mohr, John. 1994. "Soldiers, Mothers, Tramps, and Others: Discourse Roles in the 1907 New York City Charity Directory." Poetics 22:327-57.

Newton-Smith, W. 1978 [1958]. "The Underdetermination of Theory by Data." Proceedings of the Aristotlian Society, Supplementary Volume 52.

Norris 1995:

Outhwaite, W. 1987. "Laws and Explanations in Sociology." Pp. 157-83 in <u>Classic Disputes in Sociology</u>, edited by R. J. Anderson, J. A. Hughes, and W. W. Sharrock. London, England: Allen and Unwin.

Outhwaite, William. 1987. <u>New Philosophies of Social Science</u>. New York: St. Martin's Press.

Padgett, John F. and Christopher K. Ansell. 1993. "Robust Action and the Rise of the Medici, 1400-1434. <u>American Journal of Sociology</u> 98: 1259-1319.

Pickering, Andrew, ed. 1992b. <u>Science as Practice and Culture</u>. Chicago: University of Chicago Press.

Pickering, Andrew. 1984. <u>Constructing Quarks</u>. Edinburgh: Edinburgh University Press.

Pickering, Andrew. 1989. "Living in the Material World: On Realism and Experimental Practice." Pp. 275-297 in <u>The Uses of Experiment</u>, edited by David Gooding, T. J. Pinch, and S. Schaffer. Cambridge, England: Cambridge University Press.

Pickering, Andrew. 1990. "Knowledge, Practice, and Mere Construction." <u>Social Studies of Science</u> 20:652-729.

Pickering, Andrew. 1992a. "From Science as Knowledge to Science as Practice." Pp. 1-28 in <u>Science as Practice and Culture</u>, edited by A. Pickering. Chicago: University of Chicago Press.

Pickering, Andrew. 1993. "The Mangle of Practice: Agency and Emergence in the Sociology of Science." <u>American Journal of Sociology</u> 99(3):559-589.

Pizzorno, Alessandro. 1986. "Some Other Kinds of Otherness." Pp. 355-373 in <u>Development, Democracy, and the Art of Trespassing: Essays in Honor of Albert O. Hirschman</u>. Edited by Alejandro Foxley, Michael. S. McPherson, and Guillermo O'Donnell. Notre Dame, IN: University of Notre Dame Press.

Pizzorno, Alessandro. 1991. "On the Individualistic Theory of Social Order." Pp. 209-31 in <u>Social Theory for a Changing Society</u>, edited by Pierre Bourdieu and James S. Coleman. Boulder, CO: Westview Press.

Pizzorno, Alessandro. 1995. "On the Microfoundations of Social Change." Unpublished Mss.

Popper, Karl. 1959 [1934]. <u>The Logic of Scientific Discovery</u>. New York: Basic Books.

Putnam, Hilary. 1975. <u>Mind, Language and Reality</u>. Cambridge: Cambridge University Press.

Putnam, Hilary. 1997. "A Half Century of Philosophy, Viewed From Within." <u>Daedalus</u>: 12 (1): 175-208.

Putnam, Robert. 1993. <u>Making Democracy Work</u>. Princeton, N.J.: Princeton University Press.

Quadagno, Jill and Stan J. Knapp. 1992. "Have Historical Sociologists Forsaken Theory?: Thoughts on the History/Theory Relationship." <u>Sociological Methods and Research</u> 20(4):481-507.

Quine, Willard Van Orman. 1963. "Two Dogmas of Empiricism." In idem <u>From a Logical Point of View</u>. New York: Harper and Row.

Quine, Willard Van Orman. 1969. <u>Ontological Relativity and Other Essays</u>. New York: Columbia University Press.

Ragin, Charles. 1987. <u>The Comparative Method</u>. Berkeley: University of California Press.

Ragin, Charles and H. Blumer 1992. <u>What is a Case?</u> Cambridge, UK: Cambridge University Press.

Rosenberg, Alexander. 1988. <u>Philosophy of Social Science</u>. Oxford: Clarendon.

Ruse, M. 1971. "Is the Theory of Evolution Different?" <u>Scientia</u> 106:765-83.

Salmon, Wesley. 1984. <u>Scientific Explanation and the Causal Structure of the World</u>. Princeton: Princeton University Press.

Sayer, Andrew. 1992. <u>Method in Social Science: A Realist Approach</u>. 2nd ed. New York: Routledge.

Scott, John. 1991. <u>Social Network Analysis: A Handbook</u>. Newbury Park, CA: Sage.

Sewell, William H. Jr. 1996 "Three Temporalities: Toward a Sociology of the Event." In <u>The Historic Turn in the Human Sciences</u>, edited by Terrence J. McDonald. Ann Arbor: University of Michigan Press.

Shapin, Steven and Simon Schaffer. 1985. <u>Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life</u>. Princeton: Princeton University Press.

Shepsle, Kenneth A. 1990. "The Nature of Contemporary Political Science." PS: Political Science and Politics 23:40-42.

Skocpol, Theda. 1979. States and Social Revolutions. Cambridge: Cambridge University Press.

Skocpol, Theda. 1984. "Emerging Agendas and Recurrent Strategies in Historical Sociology." Pp. 356-391 in Vision and Method in Historical Sociology, edited by Theda Skocpol. New York: Cambridge University Press.

Skocpol, Theda. 1994. "Reflections on Recent Scholarship about Social Revolutions and How to Study Them." Pp.301-343 in Social Revolutions in the Modern World, edited by Theda Skocpol. Cambridge: Cambridge University Press.

Skocpol, Theda and Margaret R. Somers. 1980. "The Uses of Comparative History in Macrosocial Inquiry." Comparative Studies in Society and History 22:174-197.

Somers, Margaret R. 1990. "Historical Epistemologies, Knowledge Cultures, and Narrativity" CSST Working Paper.

Somers, Margaret R. 1993. "Citizenship and the Place of the Public Sphere: Law, Community and Political Culture in the Transition to Democracy" American Sociological Review 58: 587-620.

Somers, Margaret R. 1994a. "Narrative and the Constitution of Identity: A Relational and Network Approach." Theory and Society

Somers, Margaret R. 1994b. "Rights, Relationality, and Memberhip: Rethinking the Making and Meaning of Citizenship." Law and Social Inquiry 19 (1): 63-112.

Somers, Margaret R. 1996a. "Where is Sociology After the Historic Turn? Knowledge Cultures and Historical Epistemologies." In The Historic Turn in the Human Sciences, edited by Terrence J. McDonald. Ann Arbor: University of Michigan Press.

Somers, Margaret R. 1996b. "Class Formation and Capitalism." European Journal of Sociology 37 (1): 180-202.

Stark, David. 1992. "Path Dependence and Privatization Strategies in East Central Europe." East European Politics and Societies 6:17-54.

Steinmetz, George. 1992. "Reflections on the Role of Social Narratives in Working-class Formation." Social Science History 16:3:489-516.

Steinmetz, George. 1993. Regulating the Social: The Welfare State and Local Politics in Imperial Germany. Princeton: Princeton University Press.

Steinmetz, George. Forthcoming. "Critical Realism and Historical Sociology." Comparative Studies in Society and History.

Steinmo, Sven, Kathleen Thelen and Frank Longstreth. 1992. Structuring Politics. Cambridge: Cambridge University Press.

Stinchcombe, Arthur L. 1968. Constructing Social Theories. Chicago: University of Chicago Press.

Stinchcombe, Arthur L. 1978. Theoretical Methods in Social History. New York: Academic Press.

Stinchcombe, Arthur L. 1984. "Theory in Historical Work." Paper presented at Emory University.

Stinchcombe, Arthur L. 1990. Information and Organizations. Berkeley: University of California Press.

Stinchcombe, Arthur L. 1991. "The Conditions of Fruitfulness of Theorizing About Mechanisms in Social Science." Pp.24-41 in Social Theory and Social Policy: Essays in Honor of James S. Coleman, edited by Aage B. Sorensen and S. Spilerman. Westport, CN: Praeger.

Stinchcombe, Arthur L. 1992. "Simmel Systematized: James S. Coleman and the Social Forms of Purposive Action in his Foundations of Social Theory." Theory and Society 21:183-202.

Stinchcombe, Arthur L. 1993. "Freedom and Oppression of Slaves in the Eighteenth-Century Caribbean." American Sociological Review 59: 911-29.

Stinchcombe, Arthur L. 1995. Sugar Island Slavery in the Age of Enlightenment: The Political Economy of the Caribbean World." Princeton, NJ: Princeton University Press.

Tilly, Charles. 1984. Big Structures, Large Processes, Huge Comparisons. New York:

Russell Sage.

Tilly, Charles. 1988. "Misreading, then Rereading, Nineteenth-Century Social Change." Pp. 332-358 in Social Structures: A Network Approach, edited by B. Wellman and S. D. Berkowitz. Cambridge, England: Cambridge University Press.

Tilly, Charles. 1990. Coercion, Capital, and European Sates, AD 990-1990 Cambridge, Mass.: Basil Blackwell.

Tilly, Charles. 1994. "The Time of States" Social Research 61: 269-295.

Tilly, Charles. 1995a. "To Explain Political Processes." American Journal of Sociology 100 (6): 1594-1610.

Tilly, Charles. 1995b. Popular Contention in Great Britain: 1754-1828. Cambridge, MA: Harvard University Press.

Tilly, Charles. 1995c. "Macrosociology, Past and Future." Pp. 1-3 in The Relational Turn in Macrosociology: A Symposium. Working Paper No. 215: Center for Studies of Social Change, New School for Social Research.

Turner, Jonathan. 1992. "The Promise of Positivism." Pp.156-178 in Postmodernism and Social Theory, edited by Steven Seidman and David G. Wagner. Cambridge, MA: Blackwell.

Van Fraassen, Bas. 1980. The Scientific Image. Oxford: Basil Blackwell.

Van Fraassen, Bas. 1989. Laws and Symmetry. Oxford, England: Clarendon Press.

Wallerstein, Immanuel. 1991. Unthinking Social Science.

Wacquant, Luic J.D. 1993. "Positivism." Pp.495-98 in Twentieth-Century Social Thought, edited by William Outhwaite and Tom Bottomore. Oxford:Blackwell Publ.

Wasserman, Stanley and Katherine Faust. 1994. Social Network Analysis: Methods and Applications. Cambridge: Cambridge University Press.

Watson, James D. 1981 [1968]. The Double Helix: A Personal Account of the Discovery of the Structure of DNA, edited by G. S. Stent. New York: Norton.

Weber, Max. 1949 [1904]. "Objectivity in Social Science and Social Policy." P. 103 in The Methodology of the Social Sciences, edited and translated by E. A. Shils and H. A. Finch. New York:

Wellman, Barry and S. D. Berkowitz. 1988. Social Structures: A Network Approach. Cambridge, England: Cambridge University Press.

Wellman, Barry. 1988. "Structural Analysis: From Method and Metaphor to Theory and Substance." Pp. 19-61 in Social Structures: A Network Approach. Cambridge: Cambridge University Press.

Wendt, Alexander. 1987. "The Agent-Structure Problem in International Relations Theory." International Organization 41:335-70.

Western, Bruce. 1996. Unpublished Mss. European University Institute

White, Harrison C., Scott A. Boorman, and Ronald L. Breiger. 1976. "Social Structure from Multiple Networks I: Blockmodels of Roles and Positions." American Journal of Sociology 81(4):730-780.

White, Harrison C. 1992. Identity and Control: A Structural Theory of Social Action. Princeton, New Jersey: Princeton University Press.

White, Harrison C. 1993. Strategizing and Mobilization. Mss.

White, Harrison C. 1996. "Network Switchings and Bayesian Forks: Reconstructing the Social and Behavior Sciences." Social Research 62: 1035-63.

Wilson, Bryan R, ed. 1979. Rationality. Oxford, England: Basil Blackwell.

Zald, Mayer. 1991. "Sociology as a Discipline: Quasi-Science and Quasi-Humanities." The American Sociologist 22(3 & 4):165-187.