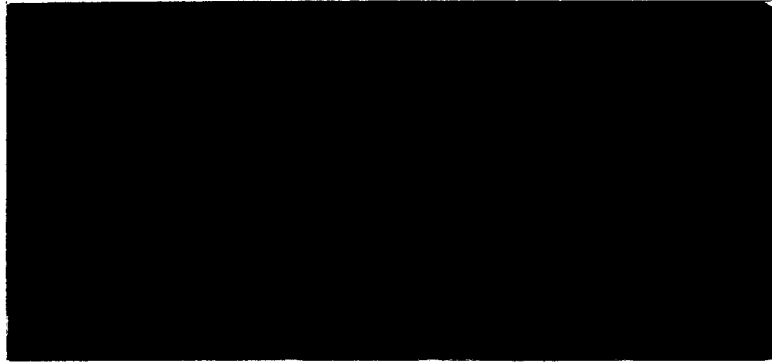




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**"Narrativity, Culture, and Causality:
Toward A New Historical
Epistemology or Where is Sociology
After the New Historic Turn?"**

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Narrativity, culture, and causality: Toward a new Historical Epistemology
or
Where is Sociology after the Historic Turn?

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Introduction

In the last decade the social sciences have turned with enthusiasm to history. In anthropology and economics as in sociology and political science, much of the best work studies data from the past or evolves methods for examining social processes over time. Yet the fundamental issues that have troubled social science thinking since its inception seem strangely unchanged by this "historic turn." To be sure, we have recognized that a link like that between social class and revolution changes historically. And this has allowed us to see that causal propositions--the syntax of social life--must vary with time. But we have been less quick to examine how our theoretical semantics--terms like "agency" and "action"--and our very epistemological foundations themselves have a history, a history not of refinement towards greater truth, but rather of undending contest, a history not unlike those we study.

Appreciating this deeper epistemological potential of history demands an "historical and narrative epistemology." This is a way of investigating sociological problems founded on the principle that all our knowledge, our logics, our theories, and our reasoning practices are "history-laden." Historicizing these practices by looking closely at their rootedness in what I will be calling "knowledge cultures" may begin to effect some deeper changes on sociology as a whole. This is the task of an historical epistemology. It is an epistemology applying equally to the historical recounting of past particulars and to the sociological theorizing of past and present generalities. And it is most notably an epistemology singularly unconcerned with disciplinary boundaries.

My chief aim in this paper is to explore the impact of sociology's recent "historic turn" on our practices and convictions about a) how to make knowledge about both b) what is the nature of society and c) why social processes happen in the ways they do. I will do this by looking at some of sociology's fundamental controversies before and after the historic turn. Following in the irreverent tradition of Barrington Moore, I am asking -- so what? What difference has it made? More specifically, what has been the impact of the encounter with history on ways we try to understand the social world? Evaluative questions should be posed on both substantive and theoretical criteria. The former asks about the substantive contributions of the focus on historical topics: Have historical studies on, for instance, the division of labour in 19th century English factories enhanced our understanding of the social basis of gender inequality? The latter criteria are "metatheoretical." I could just as well use the word "philosophical" (and will, interchangeably.) Either term is used simply to distinguish between two different aspects of social inquiry. On the one side is metatheoretical inquiry, which reflects on how and why sociology uses certain standards to construct social knowledge in general. And on the other is explanatory inquiry, which seeks to develop particular theories about substantive topics-- theories of public opinion, for example, or of class formation.

Using metatheoretical criteria, one would want to ask questions about the historic turn's contributions to three essential areas--1) to our ways of constructing and establishing knowledge about society; 2) to our understanding of the nature of society and social beings; and 3) to the validity of the specific ways we use to explain social outcomes and processes. These are three of the most deeply divisive problems in sociology and the social sciences as a whole. They represent first, issues of epistemology, which addresses the problem of how we can believe we really know about the world, and why we believe in these foundations for knowledge. I will call this the problem of "ways of knowing." The second represents issues of social ontology (in this context the term can be used interchangeably with social or philosophical anthropology), which addresses the problem not of how to know about society, but of what actually is the nature of social reality: What is society? What is a social actor? I will call this the problem of "ways of being." And the third represents issues of methodology, which addresses viability of different explanatory strategies. I will call this the problem of "ways of explaining."

The bulk of this paper will focus on the relationship between historical sociology and these three sociological problems about how to know (epistemology) about the nature of society (being) and how to make sense of it (explaining.) Regardless of whether we deal explicitly these three problems, they are the foundational and contested "fault-lines" of the historic turn. Questions about this relationship would include: Has there been an epistemological transformation in our thinking about the social world as scholars have boldly transgressed disciplinary boundaries? How significant has historical work been to the general area of social theory? Has any consensus been achieved over whether there are central problems unifying the concerns of those who work at the boundaries of history and sociology? Are the problems of each discipline simply exported onto new territory in the interdisciplinary encounter, or does the historic turn provide explanatory tools previously unavailable to the single disciplinary approach?

The story of sociology's historic turn suggests why both these criteria-- substantive and philosophical--are important. Historical sociology began to reemerge in the 1960's with the perceived interrelationship between a substantive conservatism (especially modernization theory) and the prevailing methodologies of mainstream sociology. The intellectual inspiration for this view was above all C.W. Mills.² Mills's definitive contribution was to have linked together substance and epistemology. He did this by criticizing the political implications of "radical

empiricism" and "grand theory" alike but, most importantly, by demonstrating how these implications were inseparable from sociology's anti-historicalism.³ At the same time the activism of the 1960's inspired general interest in the topics of social change, revolution, and class analysis. Hardly new of course, these topics were central to modernization studies--arguably the most prestigious area of the social and political sciences in the 1950's and 60's.⁴ But what made the difference was a snowballing awareness of the link between the conservatism of modernization theory and the recognition that its approach was deeply anti-historical. It was this that sparked a sudden sense of urgency about the need to develop historical methodologies for macrosociological analysis. In this respect Barrington Moore's comparative historical analysis of the varying routes to the "modern world" truly set the terms for sociology's recent historic turn--the emphasis on macrostructural and class transformation, and what Skocpol and I later called his "macroanalytic" comparative historical methodology. And these have continued to be the terms for both the developments as well as the criticisms that have characterized the sub-field of historical sociology.⁵

But there is also a second reason why philosophical and metatheoretical criteria are essential that is at once analytically independent of this particular story and at the same time convincingly demonstrated by the story. It is not a very popular idea among most historical sociologists. This is that ideas about the nature of the social world, and about how to best gain knowledge about that nature, undergird the ways we go about explaining any given social process or issue. This first part is not the one that most historical sociologists do not like; probably most agree with it. What is less popular are the implications of this: that philosophy and metatheory matter because they have consequences for any and all sociological argument. And for this reason they cannot be simply ignored, dismissed, or "left to the metatheorists". I hope to make a case for these implications in this paper. To be clear, I am not suggesting they are the only things that matter, nor that they matter deterministically. But I am arguing that whether we are investigating love and emotions or demographic statistics we begin from philosophical ideas about the nature of the social world and our beliefs about what ways of investigating that world make the most sense to us. And this beginning point willy-nilly will be embedded in the answers with which we end. Whether there is inherent value in these issues (metatheory for metatheory's sake?) is an entirely different question. Here, I introduce metatheory simply because it matters so much to what historical sociology creates.

And the evaluation? Substantively, the sub-field has been an unqualified success. It would be hard to understate the importance of the last twenty five years of historical sociology in enlarging our understanding of the social world. At the risk of doing a laundry list version of these gains, I think it adequate to simply assert that our understanding, indeed our awareness, of the historicity and variations in such areas as class and gender, revolutions and state-formation, religion and cultural identity, political economy and colonialism has been hugely enlarged by work in historical sociology.

I do not believe the same can be said for our attempts to historicize the methodologies which inform our substantive analysis. We have a long way to go in changing, indeed challenging, the terms (and the stalemates) of the issues that fundamentally have divided the social sciences for over two centuries ago--the three problems of knowing, being, and explaining.⁶ Paradoxically, I believe this is because of the limited exploring we have done into the significance of history itself--not as a subject matter but as an epistemology. In light of why the historic turn was originally sparked (the connection between substance and epistemology) it seems incumbent on us to push further into this potential significance through a direct confrontation between history and metatheory.

I propose that we carry out this confrontation through the development and use of what I will call an historical epistemology. It may not be the only way, but it is surely one important way, to rethink the historicity of the historic turn. The approach rests on an undoubtedly contentious assumption, but one that nevertheless has emerged from my own work in historical sociology.⁷ This is that sociology's perduring philosophical fault-lines are built into the original social science project. They are the product of the fusion between a "revolutionary epistemology" on the one side, and an 18th and 19th century reading of contemporary history on the other. Sociology's philosophical foundations emerged from this particular rendering of a particular historical moment through a particular epistemological filter. That moment was the social drama of the macro-historical rupture between "tradition" and "modernity" as it was perceived by the historical sociologists who were the 18th and 19th century's social theorists. The epistemological filter was the revolutionary social naturalism of 18th century combined with a revamped 17th century social ontology of the self. Classical modernization theory was both the outcome and the great and lasting invention of this complex fusion of history and epistemology. Not merely one a number of subfields in social science, it is its virtual epicenter. Endowed with generality, this highly particularistic historical narrative was abstracted into the foundations of what we still know as social science. These foundations inevitably reflect the available analytic discourse of the then revolutionary epistemology; but their core consists of "frozen" fragments of that historical narrative distilled into theoretical presuppositions.

It follows from this that the historic turn and epistemology need each other. Historical sociology needs to further probe into the metatheoretical foundations of our substantive arguments. This probing, must not be solely philosophical. Instead we must recognize and so directly reconsider many of the same historical processes encoded in modern social science and theory. This historical task at the same time means experimenting with new conceptual schemas critical of (or at least different from) the assumptions undergirding classical social theory. Since the historical and the conceptual tasks each presuppose the other, both must proceed at once. This is the challenge of an historical epistemology--to deconstruct and reconstruct the fusion of history and social theory.

That, of course, is not the goal of this single paper! Rather, here I will use the idea of an historical epistemology in a more limited way: first to try to understand why historical sociology has not significantly transformed the terms of sociology's long-standing controversies and stalemates; and second, to make speculative suggestions about how an historical epistemology suggests new ways of thinking about these problems. The underlying theme of this paper follows from my experimenting with an historical epistemology. This is that social science and its internal hermeneutic critics is a way of thinking and reasoning about the social world. But it is just one way. For it is rooted in what I will call a knowledge culture, a term meant to suggest the historicity--and thus the contingency--of any given way of thinking and reasoning. This is not an argument from the field of the sociology of knowledge, for a knowledge culture is not the surrounding "context" in which theories are born and so shaped. Nor is a knowledge culture is a specific set of beliefs or "truths." It is the actual practices of thinking and reasoning that constitute our full spectrum of conceptual and practical possibilities. The historicity of a knowledge culture is thus constitutive to our very "presuppositional" categories and ways of reasoning. And because it is constitutive, this spectrum so ultimately delimits what is nonetheless a huge range of contestation about ways of knowing, being, and explaining. More important than any particular truth, a knowledge culture is constitutive to what even conceive of as possible candidates for being problematized as "true-or-false." .8

It follows from this that the fault-lines of social science ought to be explored through the historicity of its knowledge culture. To do so, I will look at three dimensions to that culture, all which reflect the centrality of narrativity to historical epistemology. The first is the context of discovery, or how issues are "discovered" and deemed problematic. The significance of discovery is hidden unless we look at knowledge construction as a narrative process. Discovery constitutes both the prehistory of methodologies on one side of this process, and the carrier of ontological notions of social anthropology on the other. It thus forms a crucial window into how we make sense of the social world. The second dimension is the spectrum of specifically ontological beliefs which precede our problematizing about the world. How we presuppose the nature of the social world is shaped by the historicity and the narrativity of our knowledge culture. Classical modernization theory is one of the most powerful narratives that has informs our range of debate over our premises about society. An historical epistemology can help us look at the consequences of that impact by the means of a genetic analysis of these ontologies. The term I use for this is borrowed from the philosopher of science Ian Hacking. It is the imperative to "take a look"⁹ at the historicity of our categories and constructs. The third dimension is that of the context of justification--the range of specific methodologies that are legitimate to social science. I believe none of these methodologies are adequately historical--the historic turn notwithstanding. But explanation can be potentially historicized through a "narrative causality" constructed on contingency, sequence, temporality, and meaning.

Part I of this paper examines the roots and developments of social science logic and its constitutive problems, the ongoing debates over these problems in philosophy and social science, and how the recent historic turns in both fields have affected the terms of debate. Part II is a discussion of the concept of a knowledge culture. It forms a bridge to Part III where I will explore the three dimensions of sociology's knowledge culture--the contexts of discovery, ontology, and justification. My intention is not to resolve fundamental debates--indeed I do not believe "resolution" would be either possible or an intellectual advance. I do think it worth while, however, to use these contested issues as a window into the impact of the historic turn in sociology and as a way of thinking about the place of history in our attempts to explain the social world. The terms I use in this paper are just that--terms that I use knowing full well that the definitions I propose are simplified and abstract. Not everyone has taste for such abstractions. There is no reason they should, and I certainly do not intend to argue for (nor do I believe in) the transformation of sociology into philosophy. But my motivation is unabashed: it is to make a case against an all too common dismissal by historical sociologists of philosophical issues as mere "mere metatheory."

PART I

Social Science and its Dilemmas

Like the remarkable fable that inaugurated its birth, the logic of modern social science is rather simple. Both are utopian narratives of society's emancipation from history. William Townsend, the late eighteenth century English statesman wrote a social parable about the isle of Juan Fernandez (xxxx). The island (it had been made famous in England by the mythical R. Crusoe) was populated only by goats and dogs (men and women). According to reigning Hobbesian assumptions, these allegorical people should have had brutish, nasty, and very short lives in the absence of institutional authority. Townsend, however, endowed the island with natural harmony through a perfect balance of population and food. He did not explain this by what we might today identify as an Orwellian allegory in which order is maintained through bureaucratization or political tyranny. Rather the fable's utopianism was precisely in its inverse postulate: no state or artificial law was necessary to maintain the equilibrium. Townsend argued this by borrowing a revolutionary new metaphysics--the laws of nature--from a revolutionary new epistemology--natural science. He combined

these into a new science of society to conceptually liberate the social world from from political or social authority and the claim's of its most articulate apologists, Hobbesian and Lockean political theory. This mini-society flourished precisely because it was left to its natural laws freed what he viewed as the chains of politics and religion. We were able to know the truth about this society because we had access to scientific foundations.

Classical social science was born of this revolutionary epistemology constructed upon a myth and a metaphor about a unified social system with its parts expressive of an innerworking autonomous logic. Social thinkers of the late eighteenth century appropriated Townsend's anti-institutional naturalism as the core metaphor of a new science of society. Prevailing Hobbesian assumptions yielded to a social utopianism and radical naturalism: for Hobbes, society needed a state because human were like beasts; for Townsend, it seems that natural law sufficed because humans were beasts. Liberated from the burdensome traditions of the past--elegant in its parsimonious simplicity--the revolutionary science of "society" had arrived.¹⁰

Perhaps inevitably, two great problems were built into this naturalistic metaphor and its scientific foundations--the problem of knowing and the problem of being. The first hinged on the appropriation of a scientific logic as the grounds for our knowledge about the social world. If science is the criteria for knowledge, and science is a method of "discovering" objective laws of nature (now of society) that exist independently of us--unaffected by our ideas about them, unaffected by our being humans--where does the capacity for human understanding (*verstehen*) fit in to establishing knowledge? Does it make a difference that the subjects are humans, not objects, and thus they have consciousness? Should this not affect our method for obtaining knowledge? What about the sociologists's understanding and the capacity to empathize--as fellow humans--with the subjects of our inquiry? What about the capacity to discern meaning, not just behavior? These were are all problems of the foundations for social knowledge.

The second problem is ontological--What is society? b) What is a social actor? The problem was how or even whether this systemic notion of society could be reconciled with an anthropology that included moral agency rather than mere behavioralism, and individualism as well as social wholism. If society made up of humans, and humans have free will to act, how is the capacity for agency accounted for? If society is a systemic structure, where how does action fit in? Devised to solve the problem of how there could be any order in society at all, the solution created a more intractable problem. Sociology has been left fundamentally divided over the relative import of action and order.

These problems of knowing and being are the axial issues of contestation in sociology. The epistemological impasse hinged upon a fundamental dualism. Should the social sciences adopt the same standards of knowledge as the natural sciences, or should social knowledge be something altogether different by virtue of the subjectivity uniquely characteristic of the object of study in the "human sciences?" I hope to show that this dichotomy looks the way it does because both sides of the debate share a conception of the scientific method--the one best represented by the "covering law" philosophies of Carnap, Popper, and Hempel. The following convictions fall under this rubric: 1) natural science logic is the foundation for social knowledge because it can attain truth through a fully logical structure unaffected by historical circumstance; subject matter data may be historical, but the foundations for knowledge are themselves outside of time; 2) there is no logical relationship between the context of discovery and the context of justification--the former is merely a curiosity while the latter is the grounds on which knowledge production stands; 3) there is an absolute distinction between observation and theory; theories may be used to formulate conjectures (Popper) but only observations--that which we

can empirically contact--provide the foundations for knowledge; concomitantly, theoretical entities (electrons, social structure, the ego) do not exist since the only things we can truly know are those things we can observe; 4) causality and explanation are not scientific concepts since they are not found in nature; an explanation can only be identified as such if logically deduced as a subset from a logical and predictive proposition using a priori rules; this is rarely possible to do since "causality" by itself is seen as local, not logical.

Within this common rubric, two important distinctions exist in methodology: inductivist and deductivist logic. To the four points above Rudolph Carnap, the most important inductivist of the 20th century, added that the criteria for scientific knowledge were 1) the verifiability of propositions; 2) proceeding from observation to generalization, and from these up to analogies, hypotheses and eventually testable theories.¹¹ For Karl Popper, however, falsification was the justifying criteria: a proposition was grounded only if it could prove to be false.¹² Popper also rejected Carnap's inductivism and argued instead for deductive logic--the process of conjecture and refutation: develop hypotheses, make deductions regarding causality derived from those propositions, test those deductions against observations.¹³ If they prove true, test some more; if they are false, revise or dispose of the hypothesis. Philosophers of the "historical sciences" engaged in parallel debates.¹⁴ Carl Hempel, the most notable, insisted that an historical explanation could only be justified if it was an instance of an a priori law that "covered" that case (Hempel, 1942). An explanation could have no logical autonomy, it had to be a subset of a conditional logical law although the methodology could be either deductive-nomological or inductive-statistical.¹⁵ The same rules of a priori parallel logic, however, maintain for both.¹⁶

The major hermeneutic counter-tradition can be traced to romanticism and most significantly to Dilthey and Weber who argued that the human sciences are constitutively different from natural science.¹⁷ In the 20th century Collingwood and Dray carried on the interpretivist tradition within the philosophy of history,¹⁸ while Hans Gadamer and Charles Taylor are probably most known among contemporary philosophers. All of these men argued that the human sciences must not only explain historical processes from the subjective understanding of the agents. They also insisted to varying extents on the methodology of verstehen and the significance of cultural anthropology.¹⁹

Up until the time of the recent historic turn in philosophy and the philosophy of science, these are the dichotomous terms of the epistemological debate. To date, every advance in the defense of scientific logic--whether inductivist or deductive-nomological--has been matched by new innovations and defenses of the interpretive approach. It appears that the two positions are stalemated on first principles. But I want to argue that the stalemate actually rests on deeper agreements. First, in response to the two questions of justification--as to when a given set of propositions adequately explain why something happened; and when does a body of data confirm a proposition which can then be used to explain?--the covering law advocate makes explanation and confirmation a matter of general laws and a priori logical principles. The hermeneuticist also accepts and endorses this covering law model as both appropriate for and a correct definition of the natural sciences. This is the first point of agreement: that scientific knowledge is in fact founded on that which the covering law philosophers claim it to be. This may be more significant than the disagreement over the extension of the model to the human and social sciences.²⁰

The second point of agreement is deeper and more worrisome. Both approaches advocate generalizing and a priori rules of explanation once their respective criteria for truthful propositions have been fulfilled. For positivists this is more obvious: a true hypothesis need only be analyzed logically in relationship to that

which is to be explained. For the hermeneuticists, the hypothesis is examined closely to discern whether it satisfies the test of certain a priori principles of understanding and subjectivity. Both positions have fixed standards of judgment which are not subjected to empirical reformulations.²¹ A third (seemingly paradoxical) point of agreement is that both positions reject causal analysis. For the covering law philosophers, as I explained above (in #4 of their rubric) the reason is that on its own causal explanation is too historical; to be justified, it must be logical deduction of a general conditional proposition. For interpretativists, the reason is because they accept uncritically their opponents' redefinition of cause as a logical subset of a general law. Cause, by this definition, of course excludes the central interpretativist concerns--meaning, locality, and contextuality--and so must be rejected. Rejecting cause altogether, however, is itself problematic. In its explicit absence, implicit and ad hoc "causes" inevitably sneak into arguments. And at the same time, the vacuum left from the explicit anti-causalism is too often filled by residual implications of cultural stability that may actually resemble the logic of covering laws. Missing a chance to potentially reclaim and redefine altogether causal analysis, it begins to look like both sides of the dichotomy share more than a degree of anti-historicalism.

The Historic Turn in Philosophy

Like almost every other discipline, the philosophy of science and social science has taken a dramatic "historic turn" in the recent decades. In 1960, Thomas Kuhn published a book with a now famous first sentence: "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."²² That image, he argued, is false; it presents as absolute and timeless its foundations for knowledge. He did not suggest that science presents as absolute any particular piece of knowledge; but that this an instrumental problem only. In principle the right method may ultimately know truth since our foundations are not historical but logical. Among the gauntlets he threw down to the covering law philosophers, that knowledge only exists in time and is historical was Kuhn's most overarching. This appeal to historicity also included the claims that all data is "theory-driven" (there can be no foolproof distinction between observation and theory); science is not cumulatively progressive; science consists of paradigmatic "puzzle-solving" in which theories are not refuted by anomalies but rather adjusted for "measurement problems"; and that scientific "progress" occurs through paradigmatic revolutions in communities of scientific practitioners.

The impact of the "Kuhnian revolution" was cataclysmic--his historical challenge has been one of the most influential and controversial theories in 20th century scholarship: Scientific knowledge is not refuted by facts or anomalies; historical and sociological conditions determine the life history of a paradigm. It was this aspect of Kuhn's work that has been most criticized for its relativism. He was accused of ascribing irrationality, relativism, and "mob psychology" to the scientific community.²³ Kuhn did not consider himself "irrational" because he was not questioning the traditional conception of theory itself. Rather he was challenging positivist notions of how and why theories are accepted or "disproven".²⁴ Nor did he believe that because theory couldn't be separated from observation that that made all scientific observation useless, let alone scientific theories, any more than the fact that anthropological data, say, is theory-driven makes anthropological any less important. This is not the place to discuss the Kuhnian debate.²⁵ I simply want to emphasize those four points of the post-Kuhnian historic turn directly relevant to my discussion of the historic turn in sociology (here I am committing a gross injustice by conflating into a single "historic turn" in science and philosophy some of Kuhn's ideas with those of his own post-Kuhnian critics).

- 1) Re: ways of knowing (epistemology): the current foundations of scientific

knowledge are historical rather than a sign of increasing access to truth. 2) Re: ways of being (ontology): the metaphysics of science are also historical: theoretical entities such as electrons and quarks are socially constructed. That does not mean the objects of physics are historically constituted--physical entities do exist, indeed long after the theory of electrons has been surpassed, supermarket doors will still be opening automatically thanks to the natural force the concept of "electron" now represents.²⁶ 3) Re: ways of knowing (again): knowledge construction is a dual process: the context of discovery--or how we ask questions about the world--is inseparable from that of justification.²⁷ 4) Re: ways of explaining (methodology): I also want to emphasize that which Kuhn and the post-Kuhnians did not argue: that theory itself must be historical in method and criteria; they did not suggest that an adequate theory should be built on a method that incorporates time, contingency, sequence. Before I take up the implications for sociology of this historic turn, let me address that turn in sociology.

The Historic Turn in Sociology

One of the hallmarks of the recent historic turn in sociology has been its reflections on the project of constructing an historical and comparative methodology.²⁸ It is thus all the more indicative of just how stubborn are the metatheoretical impasses to note that there seems to be very little difference in the terms of epistemological debate between sociology before and after the historic turn. Despite the deepening of our knowledge of the past we in historical sociology have generally been caught in the same dichotomies--only now exported on to historical data. Consider first "ways of being." Although historical sociologists tend to avoid explicit metatheoretical issues, debate over the relative import of agency vs structure in explaining social processes is always more or less implicit.²⁹ One of the more prominent ones is that between Bill Sewell and Theda Skocpol in their discussion over the French Revolution and the dispute over structural vs agency-centered conceptions of society.³⁰ ³¹ The burgeoning of new debate over the viability of rational choice analysis among historical sociologists indicates that the anthropology and ontology of the both society and the social actor virtually not change in the terms of debate since the original social science logic: Is society a systemic whole irreducible to its actors, or is society an aggregate of individual actions or preferences? Is the social actor constituted collectively or as an individual who enters into society? What is notable about these discussions is not their lack of resolution in over two centuries ("resolution" I would worry about)³² Rather it is that the arguments and the terms of opposition are the same. When Marx wrote: "men make history...but not under the conditions they choose" he brilliantly summed up the perduring ontological fault-line of social science--before and since his time.

But it is debates over method that have predominated in historical sociological discourse. One way into the terms of this debate is through a glance back at an article Theda Skocpol and I wrote in 1980.³³ We suggested distinguished among three strategies of comparative historical sociology: 1) "parallel comparisons"--the use of comparisons to demonstrate repeatedly evidence for a general theory (Wallerstein, Eisenstadt); 2) "contrast-oriented"--the use of comparisons to highlight the unique and contextually meaningful features of each case (Bendix, Geertz); and 3) "macro-analytic"--the use of comparisons to explain different outcomes and regularities (B. Moore, Skocpol).³⁴ Skocpol and I were explicit advocates of the macro-analytic approach and it is to that position that most of the criticisms have been addressed. The first set of criticisms came from the logic of statistical probability: they rejected the "imperfect empiricism" of the macro-analytic approach--it suffers from too small a sample, and too large a universe of potential independent variables.³⁵

A few years later (in defense of macroanalysis) I responded that the problem is not that the sample is too small. It is that since the method creates causal

configurations (comprised of different aspects of cases), even with a small number of cases the number of possible configurational "conjunctures" is potentially overwhelming.³⁶ Indeed any loss in "scientific precision" is more than compensated by the greater intelligibility resulting from deeper subject-matter knowledge. By intelligible I meant meaningful, and used as an example Almond and Pie's comparisons of national pride. An abundance of precision, but little subject-matter knowledge, led them to conclude that Italians have a comparatively minimal sense of political identity--something easily discounted by more contextual research.³⁷ As Geertz reminds us, a wink and a blink look the same out of context.³⁸

It is precisely this concern with context that motivates the second set of criticisms from the interpretativist position. Because they want to stress the particularities of and the meaning within each case, they reject the configurational strategy of macroanalysis. My response to this was there is no reason why meaning should suffer from this strategy. Indeed it is possible that this approach potentially could make culture and meaning more significant by identifying which specific aspects of a cultural and meaningful context can be more strongly linked than others to the patterns or outcomes under investigation.³⁹ I suggested also that virtually all of the interpretavist tradition in historical sociology, moreover, rejects what they define as "causal analysis" in favor on using an ideal-typical methodology.⁴⁰ As a result we end up with arguments about "deviations" from the (ahistorical) ideal-type rather than about variations. Just as philosophy has shown us the impossibility of "raw empiricism", we must similarly question the possibility of reconstructing any meaningful historical trajectories without the implicit or explicit use of explanatory argument. The problem is that the interpretativist historical sociologists accept and thus reject the covering-law definition of causal analysis.⁴¹

Finally, and most recently Edgar Kiser and Michael Hechter have issued an especially strong criticism from what would fit the "parallel" method in the Skocpol-Somers terminology.⁴² They argue that sociology is being undermined as serious theoretical research by the "empiricism" and "historicism" of historical sociology. Only deductivism can produce real theory (as opposed to "mere history") which they define as general covering laws which equally explain the past and predict the future. Explanation is thus entailed within the logic of parsimonious conditional propositions.⁴³

I have reviewed this methodological debate at length for several reasons. First, I wanted to observe that disagreements seem fundamentally untouched by the fact that the substantive content of the scholarship is historical. Albeit in simplified form, the three approaches can nonetheless be identified as variants of the Popper-Carnap debate between deductivism (parallel comparisons) and inductivism (macro-analytic) on the covering law side, and interpretativism or hermeneutic sociology (contrast-oriented) on the other. Indeed they represent the same dividing lines that have marked discussion in the social sciences since its inception.⁴⁴ Consider the macro-analytic strategy: although using the adverb "analytic" and bearing few resemblances to Carnap, the method is a straight forward application of J.S. Mill's inductivist methods of difference and similarity.⁴⁵ Not surprisingly, it is similarly vulnerable to the same criticisms. The Kiser and Hechter position is an explicit recapitulation of Popper (with Lakatosian modifications) restating the case for primacy of a priori general theory and the subsumption of cause to an instance of the conditional proposition.⁴⁶ And the contextualists make the hermeneutic rejection of causal analysis on the assumption that causality entails general laws and thus precludes meaning.

Second, in my own response to the critics of macroanalysis it is notable and significant that each of my defenses entailed a move toward the third (previously

Second, in my own response to the critics of macroanalysis it is notable and significant that each of my defenses entailed a move toward the third (previously rejected) position. Thus my reply to the statisticians sounded suspiciously interpretativist; to the interpretativists, suspiciously positivist; and to the deductivists, suspiciously inductivist and interpretativist. Third, I have wanted to show that we in historical sociology seem to have been singularly untouched by the historic turn in the very exemplar for our discipline--science itself!47

It would be wrong to say that we have failed at resolving fundamental issues since "resolution" would not, in my mind, be a sign of success. And it would be silly to understate the value of these current methodological discussions. But in light of the striking continuity of theme--and it would be possible to document a parallel set of dual tracts for the intervening years, especially among European historical sociologists--in Parts II and III, I will try to answer the one overriding question with which I at least am left:

Where are the epistemological identity crises we in historical sociology should be having?

PART TWO

Knowledge cultures: the historicity of reasoning practices

Why, when we have turned to history in the hopes of working our way out of some of the intractable problems of social science logic, have the same fundamental oppositions over laws, realism, context, causality, variables, understanding, action and order appeared nonetheless doggedly? I don't believe the source of the stalemate is in the superiority or inferiority of either pole of the dichotomies. Rather, it lies in our common participation in a knowledge culture. The fault-lines and problems each position attempts to solve are not independent of the reasoning processes we use to try to find solutions. Both the problems and our reasoning are constitutive to the knowledge culture in which we all participate. So too, therefore, are the range of possible solutions. As members of a shared culture of reasoning, our attempts to solve its internal conundrums will reproduce them, and in defense of a position on either side of the dichotomies will move us only closer to the other side. To support this argument in the case of sociology's historic turn, I will first more generally present my view of a knowledge culture.

The resurgence of social theory has been another recent trend among sociologists (indeed the growth of the social theory section of the American Sociological Association has outpaced that of historical sociology in less than five years.) The theoretical revival has produced a renewed attention to the epistemological and the ontological foundations of the discipline. In a healthy response to what Alexander has called the "positivist persuasion" in sociology, theorists have taken on the task of "post-positivist" sociology. The problem, however, is that they have interpreted post-positivist philosophy as representing the opposite end of an epistemological continuum between "theory-driven" and "data-driven" poles (see Alexander 1983). As long as these are perceived to be the parameters of possibility, even a direct confrontation with metatheory and philosophy will not escape the circular dichotomies of social science.

Many social theorists have wrestled brilliantly, for example, with the challenge of reconceptualizing theories of action and order. They most notably have attempted to escape the voluntarist...structuralist dichotomy by developing new concepts-- structuration and multidimensionality are two.48 The theorists rightly point to the importance of "presuppositional" categories. But their efforts notwithstanding, it is not hard to recognize these theoretical attempts to once again be variants of "men make history....but"

The social theorists and the historical sociologists alike need to follow the example of the historic turn in science in order to step out of our circular arguments. We must rethink the epistemological place of history.

Theories, epistemologies, ontologies, philosophical anthropologies, metatheories (including mine), questions, conceptual frameworks, methodologies and data always bear within them the signature of their passages through time. Both theory and observation, and everything in between the two poles, is "history-laden" because all thinking and all reasoning practices and all logic and all research takes place within time and space. The parameters of a given time and space constitute a knowledge culture. A close synonym is what Hacking has called a "style of reasoning" (a term he borrows from Crombie) but it misses the practical and organizational dimensions of a cultural environment. I use the term knowledge culture to mean an environment with a broad spectrum of contested truths and falsehoods (theories of the world, beliefs about how to justify knowledge, problems-to-be-solved, and so on) which is nonetheless delimited by that culture's ways of thinking and reasoning. These parameters do not specify anything even so broad as "truth." What they do establish are the boundaries of what is even conceivable within our historical imagination.

A knowledge culture is both a part of history, and has a history (more likely, many histories.) Various ones might include the Greeks' embeddedness in Euclidean thought; the 17th century's in experimental method; the 19th century's discovery of statistical reasoning. What binds together these various examples is not that each one had a certain or even distinct notion of truths, or science, or even methodologies and epistemologies. Indeed each of those would be highly contested within a given knowledge culture. That does mean there might not be what Foucault has called a "regime of truth" dominating a particular time or place. But that is both an historical and an empirical question to be investigated; it is not constitutive of the concept itself. What is constitutive of the single concept is the existence of a spectrum of conceptual and practical possibility--which establishes the parameters of what will even be a candidate for reasoning practices at all. All knowledge cultures share the trait of being limited only by the very range of what is to be considered at all for the question of whether "is it true or false?" Or "shall we test this with statistics or field work?" Or "is the modern self an alienated autonomous self, or a communitarian one?" Or to come at it from the other direction, the concept of a knowledge culture helps us to understand that certain problems wouldn't even exist, and so would not be subject to debate, if they were within a given spectrum of reasoning. This is what historicizes our thought.

The idea of a knowledge culture and its historical process can be seen in the following sequence (diagrammed next page):

1. Thought operates within a knowledge culture--a way of thinking and reasoning that produces a capacious but delimited spectrum of conceptual possibilities. Knowledge cultures are products of history; every aspect of the culture is "history-laden."

2. The conceptual imagination is limited by this spectrum, so the range of consideration about ontologies, philosophical anthropologies, even "reality" is historically contingent.

3. This spectrum limits the conceptualization of propositions about what may be thought about as either true-or-false, and what constitutes a question to be asked. The range of what is problematized is thus historically contingent.

4. The range of possible ways of explaining problems and questions depends on a concept of explanation; explanation too is thus historically contingent.

Before specifying the components of a knowledge culture, let me first be careful to say what I do not mean by the term. First, and for several reasons, a knowledge

culture is not equivalent to Kuhn's paradigm. Paradigms, at least in Kuhn's model, are tightly coherent constellations of theories, methods, and problems. This is too coherent to be a useful historical tool for understanding epistemologies and social ontologies which always cut across numerous particular theories and methods. Within a given knowledge culture, people may vehemently argue about what is true and indeed never settle the issue. These people are unlikely to work together in a tight scientific community such as those that characterized Kuhn's paradigms. That they agree, however, on those things even to be considered for the question of truth-or-falseness is because they share a knowledge culture and a spectrum of conceptual possibilities. Just how capacious is that culture, just how many rival interpretations of truth, can coexist may be unknowable. What we do know is that there are certain things simply not in the running in any given knowledge culture.

A knowledge culture is also different from Kuhn's notion of paradigm in that while it cannot admit of future conceptualizations, or even those that are part of co-existing but never connected knowledge cultures, it is very much tied up with previous ones. The past is not at all "incommensurable", in Kuhn's and Feyerband's expression. For a new knowledge culture to develop it must not only have new and different ways of thinking and reasoning; it must also have a way of explaining both the original coherence and the current incoherence of the previous ways of thinking. For Kuhn, for instance, Einstein is closer to Aristotle than to Newton because of the former's ontology of mechanics.⁴⁹ But it is far more convincing that Einstein's triumph was that he provided an ontology which explained why "from the standpoint of an approach to truth" Newtonian mechanics is superior to Aristotlian. Why? Because regardless of how Aristotlianism might have developed, it is virtually impossible that its problems regarding time would produce the questions to which relativity is the answer. A history which moved "from Aristotelianism directly to relativistic physics" is not an "imaginable history."⁵⁰ Physics, like all other forms of theory and method and truth, presupposes history. Kuhn's historicity is limited to the sociological history of any given paradigm. We must look between paradigms for more than "incommensurability" and empty space to make sense of thought.⁵¹

To imagine any conceptual schematas as changing historically to the point of incommensurability with the past also assumes an what I would deem an untenable ontology--namely that any reasoning practice could be context-free enough to call into question everything within its own culture. This is the fiction of Descartes' own story; its appeal to us still reflects the appeal of its underlying conception of the self as totally free. But we learn from Hume's own story that such an attempt is the ingredient for mental breakdown, not for philosophical breakthrough.⁵² MacIntyre has cleverly suggested that this view of ontological ahistoricity would make empiricism a disease and paranoia a reasonable epistemology, since only the latter has a conceptual framework (the world is out to get me) through which to experience that world.⁵³

A knowledge culture is also not a form of logic like induction or deduction. Logic entails the skill of jumping "from truth to truth." ⁵⁴ Its' goal is to justify already extant classes of propositions. A knowledge culture brings into being new classes within a new spectrum of propositions, ontologies, questions, possibilities. And a knowledge culture is similarly distinct from Lakatos' "research programme" for it has no substantive "hard core" which perdures despite peripheral challenges to the periphery. A way of thinking and reasoning is a practice, not an argument, and a practice can entail numerous substantive even metaphysical principles. Finally, a knowledge culture is not a set of truths and meanings which make two different cultures incommensurable until we learn each other's languages of truth and meaning. To communicate or to understand requires not a new vocabulary but to understand the practices and processes of reasoning characteristic of another culture. Thus translation is not enough; and it is conceivable that reasoning practices can be

shared across different conceptual languages. Instead we would need to understand how it is that another knowledge culture came to problematize that which they did in those vocabularies they do use.

A knowledge culture is thus ultimately not a practice of reasoning discrete from the world, one that confronts data as something separate and then processes that data. I don't know how the relationship between thought and world actually works, or whether they are even two distinct entities which can enter into a relationship. But the concept of a knowledge culture is meant to embody both; the world and a culture's reasoning practices are too historically intermeshed for a representational conception of knowledge to be tenable. This sequence of arguments about the historical contingency of thought, however, does not mean we may not have good reasons for having our own standards of truth and reason. To say that the existence of a spectrum of possibilities is historically contingent is not to say that within that many propositions may in fact be deemed "true" on the basis of evidence. What it does suggest is certain degree of agnosticism concerning their foundations.

A knowledge culture makes epistemology historical rather than either logical or interpretativist. An historical epistemology in turn explains why the spectrum of conceivable ontologies; the range of conceivable questions-to-be asked and problems-to-be solved; and the range of possible justifications for theories and propositions are all also historically contingent. The contingency is mutual as well: Hacking has shown how the emergence of statistics was the result of questions people came to ask at a particular time, only because at that time they had started to define society and the individual in certain ways. And that they had done this was because of a style of reasoning that had come into existence historically--and had not been there before.

The Knowledge Culture of Historical Sociology

I believe the limits to sociology's historic turn are the limits of our shared knowledge culture. This claim can be supported in several ways, first by noticing the increasing attempts to join methods so that a "little meaning" is combined with a "little science." This is done skillfully, for example, by Charles Ragin in his The Comparative Method.⁵⁵ Similar reconciliations between action and order are evident in theoretical discussions of social ontologies. Hybrid terms such as structuration and multidimensionality now characterize the literature of historical sociology.

A second support for this claim for a common knowledge culture goes back to an earlier point above: despite the differences and the vehemence of opposition, there are fundamentally shared assumptions among the positions. I have suggested that the epistemological foundations of both the inductivist and deductivist versions of the covering law as well as the hermeneutic traditions are both anti-historical in their orientation toward a priori rules of justification and their mutual opposition to causality (as defined by positivism). Similar shared assumptions exist in the debates over ontologies and ways of being. Regardless of which position one takes on the debate over the self, for example, communitarian and rational actor perspectives both presuppose the existence of an essential human nature. Similarly, both perspectives on society--the aggregationist and the wholistic--share the assumption that this social entity is analytically independent of its social agents and thus can be represented as an entity. In both cases "society" is a social fact, not an historical one and there is an essential "something" out there that exists prior to our categorization of it--regardless of whether that turns out to be meaning-centered, communal, individualist, and so on.⁵⁶ It is important to note that I am not saying whether these positions are right or wrong. What I am trying to insist upon is the common reasoning assumptions underlying the arguments.

PART III

TOWARD A NEW HISTORICAL EPISTEMOLOGY

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If this argument for the inherent limitations in social science logic and its alternatives is at all convincing, how might we begin to look differently at the issues at stake? How might the historic turn in sociology challenge the limits of its own environment. One answer to these questions lies in our having another look--albeit a critical one--at the historic turn in the very field which has been the model for so many of our persistent conundrums, the philosophy of science and even science itself. This historic turn deserves the attention of historical sociologists. It has implications for rethinking our most contested and stubborn problems of explanation, confirmation, and concept formation. To be sure, there are important reasons for why we have not thought more about these implications.⁵⁷ What it has done, however, is provide important clues which suggest rich opportunities. There is much to be learned in looking at the lessons, the limitations, and the possibilities of that historic turn. I have dwelt at length on the first of these--the historicity of any given epistemology. In the rest of the paper I will discuss the remaining three: 1) the insistence upon the centrality of the context of discovery as well as that of justification, the importance of questions; 2) the recognition that while observation is theory-laden, no less is metatheory and ontology itself historically constituted; our conceptual vocabulary of "presuppositions" is historical and thus action and order are not presuppositional; we need a new way of rethinking our conceptual frameworks based on the centrality of history and culture rather than the autonomy of either facts or presupposition; and finally 3) the need to develop a causal narrativity--a reclaimed notion of causality based on narrativity and the centrality of meaning, sequence, and contingency rather than universality and predictive law.

Contexts of Discovery and Justification

Philosophers of science make a fundamental distinction between the context of discovery and that of justification.⁵⁸ It is the latter, the context of justification, that most of us think and worry about most of the time--namely, what best justifies and confirms our theories. How to do social science? Virtually the entirety of academic ire among scientists is exercised over questions of validity, verification, and justification: is it reasonable, supported by evidence, confirmed by experiment, corroborated by stringent testing, and so on. We in social science then go on to argue about the value of quantitative versus qualitative methods, survey versus historical methods, etc. The context of discovery, by contrast, is neglected. It is the context in which we "discover" the questions we ask in our research, in which we define that which is considered problematic in the first place (or what is necessary to be explained), and in which we select our conceptual vocabulary to formulate that question. Discovery thus comprises the pre-history of theory construction.

The chief reason for this neglect is that one of the pillars of philosophical thinking has been the insistence on a rigid separation between discovery and justification. Popper, Carnap, and Hempel alike charged that the context of discovery had nothing to do with what was actually discovered and to think otherwise was to commit the "genetic fallacy." That meant it mixed up history with scientific logic.⁵⁹ Questions are considered quirky, often born of "mere curiosity", sociological, historical, or even psychological circumstances (such as Kepler's sun-worshipping habits). Because they emerge from idiosyncratic and psychological interests, questions and conjectures cannot be located as part of scientific discourse. To be sure, if not for Kepler's peculiar commitment to sun-worshipping we would not have the scientific explanations that we do have. But the fundamental logic of Kepler's theory is untouched by the fact of its origins.⁶⁰ The randomness of interest and intrigue with mystery creates an equal randomness in how we problematize the questions we ask of the world; logic, by contrast, can never be random.

It was Collingwood who first challenged this claim for the autonomy of justification. He argued for the existence of a "question-answer" relationship between discovery and justification in both scientific and social explanation. He believed that any attempt at empirical explanation was doomed unless we know what question we are trying to explain; what it is that we are asking will determine where and how and on what grounds and with what evidence we look for an explanation. Just as importantly, it will determine what we leave out of the explanatory inquiry. In his most famous example Collingwood demonstrates how a general question about a stalled car--"what is wrong with my car?"--looks inconsistent with the answer we are most likely to state in the actual attempt to get the car started--"my spark plugs are fine." That answer, he points out, is a response to a more specific question--"are my spark plugs broken?" In this latter question hundreds of other potential causal variables are indeed neglected but it can be argued that the answer is at least "true" for the question and the variables (spark plugs) about which have been hypothesized. Collingwood used this "question-answer" epistemology to challenge the realist argument that explanatory statements exist outside of their relationship to particular questions.⁶¹

Kuhn gave an even more serious blow to the separation of discovery from justification from within the history of science itself. Since for Kuhn science is in time and is essentially historical, there could be no epistemological separation of discovery from justification. He argued that scientific problems are hardly serendipitous. Rather a standard set of problems are part of "normal science" and come from paradigms. The context of discovery is thus "paradigm-laden," according to Kuhn, and it is the character of a paradigm that specifies our questions. And this brings us back to history since the acceptance and overturning of scientific paradigms, the narrative of paradigmatic change was of course the central focus of Kuhn's work on scientific revolutions--or the historical conditions under which paradigms are overthrown and replaced by new ones.

Influenced by Kuhn but wanting to preserve Popper, it was Imre Lakatos who has perhaps most accurately explained why some questions are asked and not others, indeed why scientific change is so difficult. Lakatos describes the scientific project not as a paradigm but as a "research program" comprised of a negative and positive heuristic. The hard core is not open to debate, it takes on the status of a metaphysic. It is protected by an outer protective belt which is open to new formulations and measurements. Although Lakatos believes scientific progress is about the transformation of the auxiliary, a transformation which develops out of new questions posed by anomalies, his description more accurately explains social scientific stability and the extraordinary perdurability of so many of our "hard core" assumptions.⁶²

Questions and historical epistemology

Historical sociologists' reflections on theory and method have been impoverished by the singular focus on justification and the non-recognition of the importance of context of discovery.⁶³ We have not been epistemologically self-conscious enough about this dual nature of theory construction. To be fair, we now know that thanks to Carnap and Popper the discovery process was shunted off as mere historical curiosity by pre-Kuhnian philosophers as well. But we can ill afford to reproduce this anti-historical approach to theoretical development. We must recognize that questions are indeed epistemologically prior to answers and they can't be treated as residuals. Since justification is not independent of discovery, one important avenue into thinking about our stubborn debates over method and justification is via questions. Here we have much to learn from survey researchers such as the pioneering work of Howard Schuman.⁶⁴ There is no such thing as an explanation that doesn't contain within it a prior question, or what the French call a

"problematique." The very language, shape and content of our explanations are forged by the questions which inform them. Indeed explanations are only called into existence by prior questions.⁶⁵

Our research agendas are thus not shaped by method and data but by the formulation of the problem to be explained. From this perspective it begins to look like attending to questions will give us a very important route into thinking about the context of justification and the dilemmas of social science logic--a route substantially more historical than through direct theoretical encounter. Attention to context of discovery will also lead us to the difficult issues of ontology and being, action and order. For although the connection of questions to answers is more obvious, what has been unremarked upon is what I believe to be the link on the other side of question-formation, namely our conceptual frameworks. After all, if questions precede explanation, what precedes questions? Where do questions come from? One recent and convincing answer is that questions come from civilizational concerns (Zald). I think that this is very much the case, but through a rather more indirect route than this argument suggests.

Questions are born of historical and conceptual ontologies. Because they are historical, they lack of coherence or "stability" and carry with them an inherent set of problems and conundrums. Moreover, these ontologies will be in ongoing competition or contestation with other ontologies--primarily over winning intellectual consensus over ongoing points of crucial difference.⁶⁶ These internal problems and external debates are wholly products of the conceptual frameworks and their assumptions. Indeed they are only considered problems because of the content of these ontologies. Thus these problems inevitably carry within them the assumptions of their framework, and the questions we ask in social science are themselves inherently ontological, metatheoretical, or to put it simply, contain a priori decisions about how we understand the social world to be constituted.

The shaping of problems by ontologies works in two ways: first it is unquestionable that ontologies of the world will inform the development of the world (Adam Smith's, for example, was as powerful as any more "material" force) and thus be constitutive to the "worldly" things we take to be problems for research. And second, ontologies of the world will shape both the way that contemporaries framed their questions about their world--and thus the questions we inherit and work from, as well as shape the very vocabulary through which we frame our current questions and problems. Questions, then, are the lynchpin of theory construction. On the one side they lead us to answers, and on the other to they flow from fundamental assumptions. The implications of this are that we must develop ways of examining each of these three "moments" of theory construction. This is the task of an historical epistemology.

Problematizing Questions

A crucial aspect of the pre-Kuhnian conception of science is that questions are stable, what changes are the answers as we are better able to gain access to and accurately measure the world. If our conjectures are so randomly conceived, there is no very convincing explanation for how certain questions become more or less dominant at different times, how certain questions are more or less legitimate at different times, and certainly for the necessary narrative of how science progresses over time. If this is true, then we have no way to judge the value of our questions even though we may recognize their crucial epistemological role in the explanatory process. We are thrown back to a focus only on logic, explanation, soundness of method, and justification--exactly where Popper and Carnap believed the focus should be.

By contrast, an historical and narrative epistemology would begin by looking directly at questions, at problem-formation. A crucial task is to explore how, and

when, and why questions change over time--one way to put it would be the "rise and fall of questions." Exploring why certain concepts become problematic at certain times (the "discovery" of poverty, for example, in late eighteenth century England), why certain questions once considered in the realm of ethics or theology are transformed into questions of economics or morality, demands not only a deep understanding of the context of these questions. More important, it demands understanding the internal logic of our questions in order to know why their answers would resonate at certain times, and not others, to certain authorities, and not others.⁶⁷ Looking at the rise and fall of questions will teach much about the rise and fall of answers, and thus theories.

If our exploration of the rise and fall of questions is to have meaning, it must be more than a contextual narrative. We would want to know how certain ontologies distinguish between relevant and irrelevant questions as well as superior and inferior methods for answering them. In social science we distinguish between valid questions about empirical causality and invalid questions about normative causality. Educational achievement, for example, constitutes a standard explanatory variable in hypotheses about the causes of poverty, but we do not consider questions about the link between a society's level of, say, social justice to be a comparable hypothetical variable (as legitimate as we may agree it to be for philosophical inquiry.) This distinction is not a judgment about whether people should be concerned about justice; indeed that justice is an irrelevant criteria has no bearing on the normative implications for the discipline. There is instead a "paradigmatic" reason for why this question is ruled out: 1) justice cannot be measured; it cannot even be defined, so we cannot ask about it; 2) the history of the paradigm of social science has excluded issues of moral philosophy from its problematics. These assumptions so determine the spectrum within which the problems of social science research operate.

Through the context of discovery history willy-nilly enters into the logic of both science and social science argumentation, and into the conundrums of being and knowing. Since questions are so important, and questions come from conceptual and ontological frameworks which carry within them ongoing problematics, the real challenge is to problematize our problems. I suggest this can be approached by deconstructing and historicizing our conceptual frameworks, categories, and vocabularies--our thinking about ways of being.

Ways of being: the historicity of ontologies

The goal of a narrative epistemology is to explore the process by which those problems which have such a formative place in theory construction get identified as such--in time and over time. This means examining the cultural and social construction of conceptual ontologies, and in turn to examine the internal logic of their categories and assumptions as they unfolded historically. The goal is not primarily to understand "why" in sense of locating a sociological environment; it is more to understand "how" competing sets of ontologies of identity, political life, society, and so on gain currency and shape the empirical problems we encounter as historical sociologists.

The idea of a knowledge culture suggests that the very ontologies that shape our questions are contingent and historical. Thinking about this ontological dilemma from an historical and narrative epistemology leads to a different strategy for thinking about the ontological stalemate: namely, that our theoretical renewal cannot proceed without an historical one. We need to look at the encoding of category by history. Here I am not speaking of looking for context. Rather I am suggesting that most of what we in sociology treat as abstract or "presuppositional" categories--subject and object, agent and structure for example--carry within them "frozen" historical arguments which have been "abstracted" into our familiar general

categories. To "unfreeze" requires an "undoing" and that requires history. The theoretical task is thus an historical one: we cannot overcome the historicity of our theory without a new look at the history it encodes. But we cannot reread history without a new conceptual framework--at least tentative. Since each task requires the other, both must proceed at once.⁶⁸

From Ian Hacking, I lift the phrase "taking a look" to characterize this kind of project--one which joins history and epistemology. It denotes a form of conceptual analysis that I first used in my work on English working class politics and citizenship--one which developed from my recognition that the available conceptual vocabulary of class, social change, politics, and citizenship were all encoded with the very readings of that history I was now criticizing. I am calling for a genetic accounting of conceptual configurations which is neither intellectual history, nor the sociology of knowledge.⁶⁹ I am not asking whether these ideas are "true" or "false", but rather how and to what effect ideas and ontologies are even considered either true or false, how they gain and lose their currency and resonance. This means exploring the cultural, historical and narrative construction of concepts. What makes it narrative is that this kind of history is not a chronicle of events; instead like a narrative structure it tries to find those reasoning practices embedded in the conceptual constructions that allowed meaning and resonance over time. Many of Foucault's investigations, Charles Tilly's discussion of the nineteenth-century, as well as Charles Taylor's and Alasdair MacIntyre's books on the construction of the self could all serve as exemplars.⁷⁰

The whole premise of this approach is that knowing how we got to where we are will help to clarify where we are.⁷¹ This is not an attempt to "recover" the past, and certainly not a claim that the intentionality of past actors is the authentic means to doing so.⁷² Rather, if we can understand what puts ideas and knowledges in place and what brings them into being--not a teleology but an account of contingencies and "might have beens"--we can hopefully better grasp the meanings and the effects of those ideas, and their role in problem-formation. This, to be sure, is part of the context of discovery that logical positivism has dismissed as committing the "genetic fallacy." But since the approach is explicitly non-teleological, I think that the label "genetic fallacy" is insubstantial name-calling produced by an overly great admiration for a priori logic.⁷³

Indeed it is precisely this approach that has contributed so mightily to the challenges of the anti-historical view of hard science. The work of Bruno Latour, Andy Pickering, Simon Schaffer and Steven Shapin all demonstrate the power of genetic accounts of natural science logic and knowledge. These works conclude that while natural entities do exist, the classifications, categories, concepts of science, from Quarks to atoms, are not "found" in nature but in time, and constructed through historical activities.⁷⁴ To emphasize the point: I do not suggest that nature is "made up"; I do believe that the theories which categorize nature are made up, hence theoretical entities are made up. But the entities outlast the theories. (This may ultimately distinguish social from natural science, where the former may indeed be wholly a practice of social construction.) With a view to those processes and principles of historical construction, why certain problems persist becomes much clearer.⁷⁵

Let me now turn to a second possible model for an historical epistemology. One place to look is just that field to which we pay lip-service but to which we in fact pay very little intellectual respect--the field of history. One of the ironies of the historic turn in sociology is that we have turned to "history" as data, but not to the recent disciplinary practices and reflections of history itself. If we had in fact made the practice of history as much the subject of our intellectual interests as the scavenging of the data of history, we would have noticed that at just the

moment so many disciplines are making the "historic turn" with great hope and anticipation, our colleagues in history are throwing up their hands and saying-- "history! what's history?" And in response they are making their own turn--the "linguistic one."⁷⁶

It is unfortunate that the turn in history has been so labelled. For it has fostered the belief that our previously stolid colleagues the historians are now claiming that the world is all "text." That understandably scares sociologists, most of whom make their living trying to understand the "real" social world. It has made little impact that most historians are quick to point out that they don't really mean text in the literal sense as words, but texts as systems of meaning which embrace all the regular objects of sociological study--social organization, class, bureaucracy, and so on. This disclaimer makes the unlikely presumption that sociologists will overcome their distrust long enough to read this far into the literature.

A better way of characterizing this important development in history would be the "epistemological turn" since what is being argued is that we cannot understand the world without examining simultaneously how that understanding is mediated through conceptual frameworks.⁷⁷ They are suggesting quite simply that in between the "unthought" world and our thinking about the world are conceptual "filters." And most important, these are deeply historical. I don't take this to mean we should no longer care about the social world; indeed I take it to mean that our sociological concerns will be considerably more grounded if we also take those historical filters to be necessary subjects of inquiry. Scott, for example, has turned her attention to the deeply grounded empirical issues of class formation, politics, and gender precisely by analyzing the ways that the concepts of class, politics, and gender were central to the experience of her historical subjects.⁷⁸

Action and Order Historicized

Taking a look at the historicity of "presuppositional" categories of social thought also involves asking how the historical construction and transformations of a concept shaped and continues to shape its logical dimensions and its social meanings. Hacking calls this level of conceptual analysis looking at words in their "sites." It is another approach to historicizing by locating conceptual problematics not only in time, but in conceptual space. Sites include "sentences, uttered or transcribed, always in a larger site of neighborhood, institution, authority, language"⁷⁹ without which ideas would be just words, not concepts. Looking at the rise and fall of moral and social concepts as words in their sites, and in time, reveals their existence as historical--and thus contingent--entities.

Let me exemplify by returning to the ontological impasse of agency and structure and social science logic. We know that the conceptual framework of modern social science has a built-in aporia between agency and order, an aporia in part born of the revolutionary epistemology I described in the opening paragraph.⁸⁰ My own research has convinced me that the aporias of agency and order are not only a product of the logic of social science; perhaps even more significantly they rest on the core of an historical "narrative" of modernization theory, and the metatheoretical attempt to resolve the conundrum flounders on the unexplored historicity of its central categories.

Modern social theory was crafted out of epic moments in history. Plagues, wars, famines, and revolutions all play their parts; the Black Death, the English Civil War, the Reformation, the French Revolution, the Industrial Revolution all figure as shadows in the heart of the metatheoretical and theoretical framework. The classical founders constructed a social theory based upon an appropriation of the historical and empirical world. (Indeed the very power and durability of sociological thought can only be explained by the substantive and empirical content of its core principles

and the concepts to which it gave rise.) Its principles thus emerged from--and so encoded--the macro-sociological obsession that possessed them all, namely, how to explain the emergence and the nature of the modern world and its epochal break from "traditional society." The aporias of modern sociology can be traced to more than an abstract explanatory logic and revolutionary naturalism. These were categories generated by a fusion of history and epistemology.

What were the consequences of this inextricable entanglement of the new social scientific naturalism of the historical transformations of the nineteenth century? Classical modernization theory--the macro-theoretical schema aimed at describing and explaining the making of the modern Western world, its structural and its social dynamics--was the outcome, indeed the great and lasting invention, of this complex fusion of history and theory. The new theorists of society deployed the social scientific tools of Townsend's naturalism to carve out their views of the macroscopic transformation of the social world and abstracted historical processes to flesh out an historically victorious epistemological vision. They aligned themselves with the willful optimism of the revolutionary epistemology by propounding a most unique idea: If the nature of modern society could be conceived as organized according to the systemic laws of nature, the emergence of modernity could be explained by an endogenous, rational, and progressive logic shed of the constraints of ethics and law, political authority, religion, and kinship. New concepts were unleashed, now guided by a set of "presuppositions": the social world was developmentally bifurcated between 'tradition' and 'modernity', was driven by the relentless motor of technical rationality which had the power to remake society, institutions, social life, even the drama of human intentionality itself, in its own image.

Modern sociological logic and the narratives of classical modernization theory--an enmeshing of history and epistemology--are ineluctably entangled. That these historical constellations have so long passed as universal truth is a major source of the continuing dualities and antinomies of sociological thought. Ours is a social theory comprised of both historical concepts and a conceptual history. What we know today as social theory and metatheory are the legacy of those historical fragments distilled into abstract ontological and scientific presuppositions. These essential categories of our "frozen observation language"--whether defined as agency and structure, subject and object--are presuppositional only in the degree to which they are universally defined as social scientific givens.

We may have arrived at one of the reasons for the enduring presence of the ontological impasse over agency and structure in historical sociology. Agency and structure are presuppositional and ontological concepts which were a creation of a particular historical narrative. It is their unexamined and deeply problematic historicity that is central to the ontological dilemma of social science. If sociology's impasses are in the original fusion of macro-historical analysis and epistemology, solving the ontological impasse cannot be accomplished by juggling with the problems of order and action until a "multidimensional dialectic" emerges. If the concepts we use to describe the world are historicized and limited it follows that we must deconstruct the historicity of the concepts we use by means of an historical epistemology. Agency and structure in social theory must be re-examined in their origins and in relationship to classical modernization theory. We must indeed "take a look." Here are brief speculative conclusions from having done so elsewhere.⁸¹

Recall the epistemological template for the problem of action as expressed in Townsend's fantasia of the goats and the dogs on the isle of Juan Fernandez. This naturalism was one moment of the general revolutionary progressivism of eighteenth and nineteenth-century science and politics. Faced with the dilemma that naturalism inevitably threatened to annihilate the subject, sociological theory did not bury a

theory of action. Rather, it made an unlikely conjoining of naturalism with the philosophical counterpart to social structural progressivism--what I have called the "revolutionary idiom of action." What made this view of the subject revolutionary was the conceptual transformation of social agency into an abstract individual driven by the motivation of "freedom from..." anything associated with a world caricatured as *Gemeinschaft* or traditional society. This was a modernized ontology of progressive autonomy. Although Marx, Weber, and Durkheim each had different notions of the autonomous actor, all three nonetheless appropriated the striving for autonomy. Consequently--and inevitably--they each built their theories on the duality of subject and object, the individual versus society. In this schema, the identity of the subject was abstracted from history; social relations and institutional practices--even collective memory--would exist as external objects of power and constraint.

As a result of the revolutionary idiom, much of the data of social action has been the subject of social science problematizing, and usually explained by recourse to some version of social determinism. Why? Because the dispossessed modern agent is "less liberated than disempowered"⁸², indeed cannot--even heuristically or "analytically"--exist. To protect this idiom of agency, action that does not conform to its postulates must be explained by the external power of order or institutional constraint--be it norms and social laws, bureaucratic power, or economic forces. The idiom of the modern agent thus contains a built-in and paradoxical mechanism of self-extinction: it strives to assert moral agency against the determinism of the naturalistic logic of society, but its criteria for authentic agency forces structure/order/society into an oppositional and external system of domination. Aspiring to essentialize and freeze a universally autonomous modern subject has consistently problematized forms of moral agency that link autonomy to complex relationality and thus makes identity both non-essentialized and variable. The consequences of this problematization in sociology are usually dizzying tautologies about "internalized social norms." When action is in the final analysis defined as internalized social norms, the meaning of action becomes "over-sociologized"⁸³--a mere reflection of a stage in a developing social order. In this, agency is detached from history, time, and space while remade by the restless momentum of changing social conditions. The autonomy of action cannot survive this ontology of the subject which can only explain agency by recourse to the external social order; deprived of substance, action can only be a response to collective constraint or internalizations.

This is not the place to spell out alternative conceptions of ontologies. Instead I would just point to the tremendous revitalization of the culture concept.⁸⁴ In my own research, I developed the concept of a "narrative identity." I found it to be capacious and historically sensitive enough to capture the presence of contested meanings of the self and agency. Admittedly a non-sociological expression, it reflects nonetheless my effort to capture the non-fixity of agency. The "narrative" suggest identities were not formed by interests imputed from a stage of societal development (be it pre-industrial or modern), or by "experience" imputed from a social category (such as traditional artisan, factory labourer, or working-class wife), but by a person's "place" in cultural settings comprised of (breakable) rules, (variable) practices and discourses, binding (and unbinding) institutions. The concept of narrative identity is intended to make action and agency not an event but an episode--one that is shaped by both memory and anticipation. This makes identities both malleable and contested, but nonetheless only intelligible through the cultural narratives of their past. Narrativity also eliminates the notion that certain actions are rational while others are irrational or "backwards-looking"; the contexts which give meaning, contingency, and historicity to identity have no teleology. If identities are indeed narrative and historically constituted, it would

seem impossible to define absolutely the nature of the self. It is time to turn to the more challenging question of how identities are formed in varying contexts.⁸⁵

Ways of Explaining: Historicity and the Context of Justification

In this last section I will be using the phrase "narrative causality" to denote a form of explanation that incorporates sequence, temporal order and contingency. This detaches cause from its alliance with prediction and general a priori logic, and restoring to it its core constituents--narrativity, sequence, temporality, and contingency. Let me return to Skocpol's and Somers' three modes of inquiry and strategies of justification discussed above. In the absence of an alternative model of explanation, attempts to incorporate historicity remain vulnerable to the charges of "naive inductivist empiricism."⁸⁶ This is the charge issued most vehemently by Edgar Kiser and Michael Hechter in a recent attack on historical sociologists. Their charge is directed especially at what Skocpol has called "analytic inductivism"--the method of building arguments from historical research using analytic concepts to organize evidence. They lambast historical sociology for its lack of universality, claiming it is not broad enough in scope and "bold" enough in its propositions.

Where are we to look for adequate responses to these reaffirmations of anti-historical definitions of explanation? The macroanalytic alternative has itself been able to make temporality a constitutive part of its causal strategy--reverting instead to an interactive model of converging variables rather than a contingent process of narrative sequence. The hermeneutic alternative, accepting uncritically the covering law's narrow definition of cause, not only rightly refuses a method which excludes a place for meaning, but also dismisses as a goal altogether any form of causal explanation. As I have tried to show earlier, however, the explicit refusal of causality cannot contain the implicit "smuggling in" of ad hoc--an thus unexamined--cause. Indeed it is arguable that none of the three modes are capable of accounting for sequence, temporality, narrative ordering, and contingency--and still adhere to an explanatory goal. In light of this apparent stalemate, let us once again turn to science's historic turn in search of a model for the challenge to construct an historical epistemology of narrative causality.

Given the centrality of history for Kuhn, it is surprising to realize that his focus on history concerned not the scientific method itself but rather the historical development of science as an intellectual endeavor. His work was thus an historical sociology of those conditions enabling scientific achievement to be so defined. This sociological aspect of his work was the source of the charges of irrationality. But for Kuhn the rationality of scientific method was never at issue. To be sure, questioning the discreteness of fact and theory had significant implications for methodology. But these were issues over realism, not over the epistemology of justification per se, and certainly not over the historicity--or lack of it--in the explanatory process. Indeed in a later lecture, "Objectivity, value judgment, and theory choice," Kuhn affirmed the traditional understanding of scientific method--theories should be "accurate, consistent, broad in scope, simple, and fruitful in new research findings."⁸⁷

The historic turn in science thus shared this same limitation with that of historical sociology. They both stopped short of taking on the most intractable aspect of both scientific and social scientific assumptions--how to build the contingency of temporality into the attempt to make sense of the world's regularities. Without this step, in both science and social science we are left with the ideal of judging what counts as an explanation on the basis of a priori standards common across the fields--generality and parsimony of conditional propositions, the reduction of cause to a subset of predictive law, or the antihistoricism of deductivist logic.

A new historic turn

What was left untouched by Kuhn remains both the most outstanding challenge to and the most neglected concern of philosophers and scientists. But in the last few years, a glimmer of a new historic turn in the epistemology of scientific method itself has begun to emerge from a corner of philosophy and science. It is glimmer that has much to offer both the social sciences and the humanities. Most prominently, Nancy Cartwright, Richard Miller, and Stephen Gould have all to greater or lesser degrees of explicitness have rejected the covering law definition of causality and taken up the challenge to defend the historicity of the explanatory process. The keystone of their argument is a renewed--one could say reclaimed--definition of cause. They insist on the analytic detachment of cause from prediction and law, and in so doing, they have provided a model for historicizing social explanation.⁸⁸

In order to highlight the novelty of their arguments, let's look briefly at our existing models of cause. Take any historical narrative about which we want to explain a pattern of events across cases--say, revolutions. The models we have now are 1) setting up a general law of revolutions that analyzes the necessary and sufficient conditions for a revolution to happen. That means given c, then when x happens, y will follow. The cause of the French revolution is deduced from this, meaning that it had to happen. Within this there are 2 ways at arriving at this law: a) making it from a deduction/parallel; b) inducing it. What is wrong with these approaches? The first problem is that they can't accomodate what is different about trying to understand historical events as opposed to experimental phenomenon. The revolutions occurred in a particular temporal historical sequence--they followed or were built on an accumulation of past events. The interaction of the conditional variables, however, says nothing about this sequence; the narrativity of the phenomenon to be explained becomes residual to the superior importance of the simple presence or absence of crucial variables. In Gould's phrase, it assumes that if we "ran the tape backwards," as long as the same variables eventually came together, a revolution would happen.⁸⁹ The reason why these uses of causality can not cope with this crucial aspect of narrativity and sequence has to do with the fundamental defining feature of the method--the necessity for generality and for cause to have a uniform logical structure outside of any particular circumstance which would limit its applicability or testability.⁹⁰

Narrativity is excluded precisely because of its necessary contingency. Andrew Abbott has shown this in an analysis of "what do cases do" in standard sociological analysis using positivist methodology.⁹¹ Taking apart a few studies, he demonstrates that the typical sociologist's case begins with "mere existence" and then is constructed out of the relevant properties assigned to it by the investigator--this is true whether the case is a biological individual or a state, for example. He finds that in order for the causal variable relations to hold across cases, all the cases must follow only one narrative sequence (even if they in fact don't). Variable analysis, in this model, thus means sacrificing the narrative order on the assumption that sequence and order would not affect the causal explanation. In Abbott's words, "cases are not complex entities whose character is simplified in this model, but characterless entities 'complexified' (!?) by the variable that assign properties to them."⁹² In one historical study, the sociologist takes the stories of how workmen's compensation developed in 48 states in the US, and transforms these 48 narratives into 960 independent, one-step narratives by analyzing each state's process of developing the policy at 20 different time periods. This means that what for an historical case is the state of Massachusetts in the years 1912, 1913, 1914 for example is treated by this historical sociologist as three different cases--Massachusetts in 1912 is one case, Massachusetts in 1913 another, and so on. Contrast this with an historian's discourse in which a case is defined not just by geography but equally by temporality. For the sociologist, however, that any given

event was constitutively connected to the past events on which its very existence depended or was contingent is extraneous to the scientific method.

An additional feature of the way that positivist analysis deals with an historical case is to force it into stasis; social transformation of some sort may be the object of explanation, but the explanatory process consists in assigning enduring--and not changing--variables to the case. Contrast this with an historical case in which, as Abbott points out, a case which began as one category may end up being an instance of another: a state becomes a nation, a craft becomes a profession, and so on.⁹³ Refusing transformation within the case itself--as opposed to being the object of explanation--is another aspect of the incapability of the positivist approach accounting for contingency.

The problem we are facing is how to bring historicity into the method itself. What would we want from a causal explanation after the historic turn? To get at what we would want and need from an adequate historical method entails inverting many of the problems we have identified with the non-historical one. We would want a method that can identify cause and at the same time account for sequence, narrative, and contingency of history. It would be a method would step out of the dichotomy between cause and meaning--insisting that meaning can be a cause and that cause can be meaningful. And it would have to be a method which also refused to participate in the either/or of general covering law versus "just random history." For the issue isn't degrees of generality or story-telling, but whether a meaningful explanation can be found. With these criteria so distinct from those of the covering law position, we might also assume that the means of achieving them would also be quite the opposite from those of the traditional model. This is indeed the case. We would want to separate cause from law; it is the conflation of the two that has made cause so antithetical to those who would pursue historicity and meaning. Making this separation will allow us to see that from a different angle causality is inherently narrative and historical; it can account for patterns that nonetheless exist within frames of contingency and indetermination.

Causality after the historic turn would first and foremost gain autonomy from any a priori logical structure, and from conditional propositions and prediction. In the absence of this a priori logic, Richard Miller specifically suggests the notion of a "core conception" which contains some varieties of classes that count as causes, but which, like a work of art, can be extended through intermediate cases.⁹⁴ An adequate causal description is not a priori but empirical and varies depending on context and subject matter, as well as the actual state of knowledge about that subject. That alternative can be found in Gould's notion of contingency as the essence of history; in Miller's notion of causality as historical, comparative, and anti-general; in Abbott's notion of generalized narrative; or Cartwright's notion of causal stories. Each of these represents different angles to get outside the dichotomous bind of determinism, a priori general logic, cause as a subset of prediction on end of the dichotomy; with randomness, particularism, non-causal chance events on the other. By contrast, they each represent the argument that meaningful cause can be detected, and meaningful patterns can be explained, but that outcomes cannot be predicted from explanations. Each step of the pattern "proceeds for cause", but no finale is pre-patterned into that cause; a change in an early event will produce a very different pattern of sequence.

Consider Gould's evolutionary theory as an exemplar. He establishes a theory, not just a story, of evolution, by being able to show that if not for his account of causal repertoire, sequence, life as we know it would have turned out differently. As he demonstrates poetically in his gripping story of the Burgess Shale, this "third way" between determinism and randomness is really what historical causality is all about--contingency. Contingency, he insists "is a thing unto itself, not the

titration of determinism by randomness."⁹⁵ Contingency means the accumulation of the past is a precondition for any moment in the present, and thus the ability to make sense analytically of the result, in comparative terms. But it also means accepting that if the results are truly contingent on the past, then an alteration in one of those events will change things. This is not anti-structural; he is not suggesting they have to change things. The classic example is the American Civil War: change the military strategy and the North still would have won because of superior population and resources. Structures are part of the contingent story.

But let me look a bit more closely at how evolutionary theory, Marxist theory, and Freudian theory could be seen from this perspective. Evolutionary theory has long been excluded from the "hard sciences" by virtue of its narrative structure. To the extent that it was included at all, it was due to the long-accepted attempt to locate evolutionary explanation within the pattern of direct predictability, determinism, and invariant laws of nature.⁹⁶ This produced the well-known teleologic ladder of evolution with which we are all familiar. Gould turns this story on its head. He tells the story of the Burgess shale which evidenced hundreds of different species, only two of which are with us today. Through comparative historical analysis, Gould is able to rule out the standard causal explanation about the survival of the fittest and to instead identify "decimation" of many of the very fittest species, leaving only the puny and weak ones. Through a long story of cause and effect, he weaves together a new theory of evolution that turns the traditional ladders and cones of our evolutionary iconography on their heads, only to reveal an entirely different pattern.

Gould's explanation is rooted in contingency. It is not guaranteed by basic laws of natural selection, mechanical superiority in anatomical design, or even by lower-level generalities of ecology or evolutionary theory. The absence of law, however, does not preclude the recoverability of cause through the method of comparison--or what Gould calls "replaying the tape." "Testability" is still his requirement for causal attribution, and he still believes in the possibility of discovering whether hypotheses are definitely wrong or probably correct. Using the notion of "consilience"--the "conspiring" of many independent sources to indicate a particular historical pattern--Gould advocates comparative narrativity (my term) as the method for determining historical causality. Historical explanations must take the form of narratives: Y happened because X happened before it preceded by A, B, and C. The key step is the following comparative test: if any of these earlier stages had not occurred, or had developed different, then Y would not either have happened at all or would be so different in form that a different explanation would be necessary. 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 outcome Y; and any variant of Y emerging from differently configured antecedents would "have been equally explicable, though massively different in form and effect."⁹⁷

This causal explanation thus includes our crucial criteria: narrativity and attention to order and sequence, contingency, and causal intelligibility. The centerpiece is the notion that historical explanation is contingent upon an unpredictable sequence of antecedents states where changes in the sequence would alter the results. The result is thus dependent--that is contingent--UPON everything that came before, not a deducible consequence from a law of nature or even predictable from a general property of the larger system.⁹⁸

A different example that nonetheless illustrates this alternative notion of causality is taken from psychoanalytic theory. From the classic deductivist definition of cause, Freud's theory of character does not in fact qualify as a theory since the theory provides no premises for deducing the course of a person's emotional

life: whether pathology develops in an individual is a result of a convergence of factors, and their narrative sequence, including pathological tendencies and ego strength, for which there is no psychoanalytic formula. What there is in Freud's theory, by contrast, is what Miller calls "a repertoire of causal mechanisms" for use in the explanation of adult character. Two of these would be the capacity to cope and the be emotionally open are shaped by unconscious desires and thoughts which develop in childhood; and that these childhood desires are the most important source of psychic pain and impulsive or inflexible behavior. It is precisely the range of possible sequels that excludes the theory from the status of "theory" in the deductivist mode. Indeed while some deductive principles could be extracted from the theory, they are the least important aspects of any version of psychanalytic theory. The requirement that premises for deductive explanation are necessary to attribute theoretical status has had poor effects on the theory, leading advocates to insist that a variety of deductive premises must be accepted to accept the theory at all.99

A similar exercise could be performed on Marxism. It also would not qualify as a proper theory given its absence of appropriate deductive premises. Although the theory provides a testable causal repertoire--the causes of basic internal change lie in the old mode of material production and the interests and resources it creates--these are not testable premises for deductions. Meanwhile, there is a premise that has been awarded the deductivist attribution of theory--that of technological determinism. One of the more interesting aspects of Marxology is that the philosophers in search of a proper theory, have tended to define Marxism in terms of either such technological determinism or, more recently, rational choice. By contrast, these debates over Marx's general theory of history are fairly useless to even Marxist historians who employ historical explanations virtually everyone would agree to be Marxist, and whose arguments virtually everyone would see as instances of Marxist theory, but the character of which excludes them of interest to those who would define theory.100

What all of these theories share is a concept of cause and a concomitant concept of theory that is shaped by an historical epistemology. The moral of this story is that in science as well as sociology and history an explanation which actually depicts causal mechanisms is always told in narrative form. It is a set of sentences with transitive verbs. Cause implies narrative. The historical and temporal dimension of comparative history is thus as important as its comparative component for it entails explanatory narrative. It is narrative because the explanation is embedded in time, and moves through time. Indeed the success of any explanation resides in its accounting for temporality and sequence.

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