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DOES SOCIAL THEORY NEED HISTORY?
REFLECTIONS ON EPISTEMOLOGICAL ENCOUNTERS
IN THE SOCIAL SCIENCES

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*Thanks to Elizabeth Anderson for discussing with me some of the contemporary problems in the philosophy of science.

Introduction to Social Science History Roundtable:

"Does Social Theory Need History?"

by Margaret R. Somers

With the founding of the Social Science History Association, a future of interdisciplinary encounters between history and the social sciences was clearly signalled. The last two decades have seen these encounters established in both scholarly and institutional developments. The wide impact and the perduring influence of the work of, among others, Barrington Moore, Lawrence Stone, Charles Tilly, and Theda Skocpol indicates just how intellectually significant these encounters have been in the academic community. In the past few years major institutions such as the University of Michigan, the University of Minnesota, and New York University have established and funded interdisciplinary graduate programs with titles such as Michigan's "Comparative Studies in Social Transformation", a program coalescing faculty and students from history, sociology, and anthropology.

But following in the great tradition of Barrington Moore, we must begin to ask -- so what? What difference has the interdisciplinary encounter made in the approach to knowledge that we bring to our research topics? Has there been an epistemological transformation in our thinking about the social world as scholars have more boldly begun to transgress the boundaries of the traditional disciplines? And perhaps one of the most important questions: How significant has historical work been to the recent resurgence of social theory?

In considering these questions, we are lucky in that one of the more interesting aspects of the interdisciplinary encounter is that the writings of its leading scholars have not been confined to their research findings but have also included numerous reflections and programmatic statements on the methodological and theoretical foundations of their work. One need only think of the recent debate carried on in the pages of the SSHA journal between Theda Skocpol and Oliver Zunz concerning each of their recent books, Lawrence Stone's *The Revival of Narrative*, or Charles Tilly's *Big Structures, Large Processes, Huge Comparisons*.

In the spirit of continuing in this tradition of epistemological self-consciousness this roundtable will address the current state of the encounter between history, sociology, and social theory. Although each participant will of course be free to focus on their own area of interests, some of the general questions the organizers will put to the participants include: Has any consensus been achieved over whether there are central problems unifying the concerns of those who work at the boundaries of our social science disciplines? What has been the impact of such books as Theda Skocpol's *Vision and Method in Historical Sociology*, Oliver Zunz's *Reliving the Past*, Charles Tilly's *Sociology Meet History*, or Gertrude Himmelfarb's *The New History and the Old*? Are the problems of each discipline simply exported onto new territory in the interdisciplinary encounter, or does the merger of disciplines provide explanatory tools previously unavailable to the single disciplinary approach? We will especially ask the participants to offer not only theoretical reflections but also to consider the contributions of specific exemplary works, using them as points of reference for these epistemological reflections.

The participants who have consented to sit at this roundtable are each uniquely equipped to take on these issues. Craig Calhoun is a founder of the Comparative History section of the American Sociological Association and a leading historical sociologist who writes extensively in the area of history, anthropology, and social theory. Nick Dirks, a specialist in Indian history, has recently published a history of Indian state and civil society formation, *The Hollow Crown*, and is currently completing a book for Cambridge University Press on the relationship of history and anthropology. He is also on the steering committee of Michigan's program in the Comparative Study of Social Transformation. Barbara Laslett, another leading historical sociologist, is known for her work in the historical sociology of gender and the social reproduction of families. Margaret Somers has written on the relationship of history to social theory and on methods of comparative history. She is also a council member of the American Sociological Association's section on comparative history and on the Steering Committee of Michigan's program in the Comparative Study of Social Transformations. Arthur Stinchcombe is of course one of the country's leading sociologists who is author of *Theoretical Methods in Social History* as well as a number of other pieces on the relationship of history and sociology.

Does Social Theory Need History?

by

Margaret R. Somers

What does it mean to ask "Does social theory need history?" What it definitely does not mean is "Does sociology need history?" The answer to that should be self-evident. Social theory must be the subject for the terrain of theory and epistemology dissolves the disciplinary boundaries of history and sociology. Nor does the question ask whether historians use or need theory; that question also should be long settled. The question instead aims to explore the historicity, or what I will call the "historical epistemology," of that level of theory common to both historians and sociologists -- indeed to most social scientists -- and asks what are the implications of this epistemology? The term "theory" is conceived in many ways. My use of the term is not propositional, but rather limited to the theory embodied in the conceptual vocabulary of social and historical discourse -- social class, culture, agency tradition, gender, community, social actors, state, and so on. There is a dual sense in which this vocabulary is theoretical. Not only are each of these concepts theoretical constructions in that they are constructed as ideal types; they also contain implicit relational statements. The term "community," for example, implies both an ideal typical definition of a community as well as assumptions about the relationship between a ([an] ideal typical) community and one or more acting subjects.

In the last decades a dramatic "historic turn" has flourished across the social sciences. As valuable as it has been for scholarship, this turn generally has not been epistemologically reflective, tending instead to study "the past" and concentrate on methodologies rather than to explore the historicity of the concepts of our trade. But if the historic turn is to mean anything more than social science applied to the study of the past through the extension of data points, epistemology and the historic turn need each other. The task of historicizing our conceptual vocabulary should be the priority of the historic turn, and debates over discourse and structure, agency and society, class and state, must address the very question of how we have constructed and how we continue to use the concepts themselves. These constructs of social theory do indeed need history, or more accurately, historical epistemology. We must learn to think historically about our theoretical categories as we have begun to do with our empirical problems.

The "encounter" of the title of the roundtable ("Reflections on the epistemological encounters in the social sciences") is thus only marginally about the encounter between the disciplines. More fundamentally, it is about epistemology and theoretical categories -- or how we construct knowledge. And that is a matter that displays a reckless disregard for disciplinary boundaries.

I

These remarks are organized to address two current trends in the social sciences: 1.) The wide ranging interest in theories of action on the one hand, and 2.) the recent backlash among certain sociologists (such as Hechler and Kiser) against the work of many leading comparative historical sociologists (such as Mann, Moore, Skocpol, Stinchcombe, and Tilly). I will briefly address each of these two trends.

the entirety of academic ire among social social scientists is exercised over questions of validity, verification, and justification--lie answers.

But it is the context of discovery--that 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 evidence appropriate to formulating that question--which is less recognized and less discussed among sociologists and historians.

My argument today is that social theory needs better questions, and it is toward this task that we most need history.

Why do questions matter so much? Again we learn from the philosophy of science that questions are epistemologically prior to answers. There is no such thing as an explanation that doesn't contain within it a prior question, a problematic; 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. Consider the following historical example:

Charles II once invited the members of the Royal Society to explain to him why a dead fish weighs more than the same fish alive; a number of subtle explanations were offered to him. He then pointed out that it doesn't. (MacIntyre, 1983, p. 88.)

And of course we all remember that when Gertrude Stein was asked on her death bed "What is the answer?" she replied, "What is the question?"

Our research agendas--like those of Charles II's Royal Society--are not shaped by method and data but by the formulation of the problem to be explained.

Where do questions come from and what is their relationship to history? Questions come from paradigms. I hope it insults no one to remind you that a paradigm is not just a body of methodological principles and strategies, but

(in Zeldich's gloss on Kuhn) "a set of concepts and theoretical categories, of definitions of problems to be solved, and the criteria for their acceptable solution." (p. 277). Because contexts of discovery are "paradigm-laden," it is the character of a paradigm that specifies our questions. Michael Polanyi, from whom Thomas Kuhn draws much of his inspiration, simplified the notion of paradigm by calling it the "tacit knowledge" of social science--that which defines the boundaries of normalcy within which the problematics of social research operate.

This is where history comes in. The dangers of normalcy are first that it protects and insulates our questions from adequate scrutiny and so allows the repeated use of what may be flawed questions. Historical scrutiny is one of the scrutinies from which paradigms are protected; paradigms, after all, are rooted in "heuristics" (in Lakatos' terminology) and cannot be invalidated by history. Social theory is in large part comprised of theorizing about contingency and variation. Because only historically embedded concepts and questions can lead to that kind of theorizing, historically embedded concepts and questions must be at the heart of social theory. But because they are heuristically based, many social science paradigms and their theoretical constructs are ideal types. Historians and sociologists who employ these constructs willy-nilly fall into the trap of the infamous "fallacy of misplaced concreteness" in which abstractions are used as if they are historically concrete. Since history inevitably deviates from the ideal types, these abstractions produce questions often equally ahistorical and, paradoxically, framed as problems of deviance from the abstraction.

This is the second danger of the normalcy of paradigms: Normalcy produces "anomalies"--inconvenient empirical counter-examples that appear deviant and puzzling when compared to the terms and predictions of paradigms. In the land of fairy tales, anomalies junk theories. Were this true, we could

have faith in the power of history. But if there's anything the last thirty years in the philosophy of science has done it is to demolish the idea that science or social science discards theories on the basis of facts. (It was no less than Popper himself who admitted that "all theories are born refuted.") The real story behind anomalies is that they are the meat and potatoes of normalcy; far from refuting theories, inconvenient counter-examples are turned into deviant cases and are 1) explained away by problems of measurement or auxiliary hypotheses and 2) converted into questions formulated not to ask about historical variation but about why history seems to recurrently produce cases that deviate from theoretical predictions. Such questions then become self-reinforcing for the paradigms and their social categories: When a variation (and of course that is all that an anomaly really is) is redefined as anomalous it cannot be used to challenge a theory.

To suggest that the paradigms and theoretical constructs driving many of our problematics are abstractions is an assertion controversial enough to deserve a bit of qualification. Very briefly: The founders of classical social science (Marx, Weber, Durkheim) crafted the central postulates of modern social theory from their historical rendering of a particular macro-historical moment. Built into the heart of social theory is that of the putative rupture between "tradition" and "modernity" as it unfolded in Europe in the epoch of the industrial revolution. Many of the theoretical concepts regarding political institutions and social agency were fashioned from their assessment of the central human drama of the modernization process, namely the cause and consequences of class formations in the same epoch of industrialization. These abstracted historical readings were then abstracted into a social theory and endowed with generality.

At the same time, this reading of history was itself the product of the newly inherited revolutionary epistemology of the eighteenth century, that of

"naturalism"--the optimistic paradigm that politics, philosophy, and moral action had been surpassed by the laws of nature and society. The epistemology on which the upstart new "science of society" based itself was constructed upon two essential rejections of the past: The rejection of a concept of human nature that could be construed outside of its individualist construction; and the rejection of a concept of society that did not declare the subordination of political relations to society's own immanent "laws".

Modern social theory fused together the postulates of the revolutionary epistemology with this historical reading of the modernization of the west. The outcome inevitably came to reflect the available analytic discourse of the new epistemology. The revolution in social science conceptually reinvented the social and political vision of the workings of society. To say that our theoretical constructs and paradigms are ahistorical is therefore not to say they are not a product of history. Rather it is to say that deconstructing those constructs leads to the discovery of a particular rendering of history embedded within their ideal typologies.

If these large propositions make any sense, it follows that the much needed project of theoretical renewal is at once an historical project--neither can be accomplished in isolation. To accomplish the theoretical task requires a reconsideration of many of the same historical processes that are frozen into modern social theory, just as the historical task requires a new conceptual frame of reference that can only be developed by reflecting on the problematic nature of the assumptions underlying classical social theory.

Historians and sociologists alike therefore need to subject our questions, and the social categories which constitute their vocabulary, to historical inquiry. This can be done by subjecting the questions to 1) the discipline of comparative concreteness--by which I mean the conversion of questions of anomalousness or deviance into questions of historical variation;

and 2) the discipline of historical specificity--by which I mean grounding our questions and categories in time and space. Both disciplines are designed to combat the fallacy of misplaced concreteness.

Let me give an example of how the first has been done in a few cases dealing with social agency. The first comes from gender studies. For years scholars of moral development pondered the seemingly unanswerable question of why women do not achieve the highest stages of development allegedly achieved by men. Why are women deviant or marginal, social scientists and social historians alike have asked? More specifically, they ask why is it that while men develop according to the correct theoretical norm, becoming increasingly autonomous, individuated, and oriented to rules of abstract justice, women get stuck at a regressed stage where they are tied primarily to their social relationships and make political and moral decisions based on principles of equity and relationality rather than autonomy. As we know, social scientists and historians have invented innumerable explanations for why women do not live up to the male paradigm.

Carol Gilligan did something different. She did not ask yet once again what is wrong with women; instead she asked what is wrong with a paradigm whose central problematic generates questions primarily about women's deviant and marginal agency. She did so by subjecting the abstraction of agency to comparative concreteness and thus turned the question into one of concrete variation. Male and female actions were compared not against the ideal type of individuation, but against each other's own practices. She then produced a new question: How do we explain gender-varied processes of development? and so turned theories of moral development on their heads.

Because Gilligan's argument is both ahistorical and essentialist, I would opt instead for the more post-structural concept of identity developed by Joan Scott (1988.) Nonetheless, it is still important to note that the

gendered concept of agency as it was used before Gilligan was an abstraction used as if it referred to an historically specific agent. Counter examples only produced questions about deviance, rather than about variation, and so precluded the historicization of the concept of agency.

One could take the point even further and argue that the entire problematic of agency and structure is premised on the idea that people are constituted first as abstract individuals and then enter into relationships with their social world. The abstraction is well represented in the prevailing problematic of agency and structure: If agents are constrained by structure, how do they come to act? And if they can indeed act, how are they constrained? The question presumes an antinomy between the actors and the structure of social relationships in which they are acting. But if identity was in fact defined by those historically situated relations, the entire problematic would be a false one--not unlike Charles' dead fish. This is only something that can be discovered by calling the construct of agency out of the realm of tacit knowledge and into the realm of historical inquiry.

One way to do so is to look at the concept of agency which emerged from the making of modern social theory described above--a concept shorn of social connectedness and moral agency in favour of autonomy and interest. As each of the conceptual principles of the new epistemology declared an irrevocable break between tradition and the modern world, continuities and connections could not be theoretically accommodated as other than cases of failed progress. The conceptual liberation from the past relegated any signs of tradition to the oppression of the dead. People were detached from their historicity and made and remade by the endless momentum of changing social conditions. In this "revolutionary idiom" of the subject, only the negative actions of "freedom from..."--from others, from politics, from constraint--fitted the modern actor. The modern view of community solidarity followed accordingly.

Communities were the stuff of romantic--and dangerously reactionary--visions of Gemeinschaft where the newly liberated self potentially would be subjected to the tyranny of the group. Small wonder that these theoretical principles central to modern social theory cannot explain those dimensions of social life that were excluded by the classical interpretation, namely continuities and connectiveness in social agency.

It is the prior acceptance of these assumptions in social theory that explains why that which is considered problematic for sociological inquiry is the "irrational", "conservative", and "anachronistic" behavior of those people--usually women and the working classes--who fail to conform to the proper standards of the revolutionary idiom's rejection of ontological connectedness in favour of an abstract autonomy and "freedom from..."

A second example of how history could better inform question-formation comes from theories of social class. Rather than seeking to explain events and processes that are or have been, more commonly have sought to explain what is not or has not been the problematics of class formation, namely class consciousness among the Western working class. At the heart of the problem is Marx's "class in itself-for itself" problematic--an ideal typical and thus abstract formulation which predicts the emergence of a working class revolutionary consciousness from capitalism's class structure. Numerous problems--conceptual, methodological, and explanatory--have followed. By class do we mean only objective class position, or should the definition include social action? If a unitary capitalist development is the causal a priori of class development, how are we to explain variation among working classes?

But of all the problems generated by this prediction, none has been greater than the "failure" of the western working class to behave correctly. The results of this failure are striking: Rather than a rich literature

explaining variations among working class histories, countless explanations can be found for why any given working class "deviated" from the prediction. Surely it is time to ask some new and critical questions: Why is it that the standard problem to be explained in class analysis is how to explain not what is or has been empirically present, but rather the failure of a people to behave correctly according to a theoretical prediction? Why are studies of class formation rooted in an epistemology of absence?

The answer lies in the agent of class theory. It presupposes that agents are constituted first in relationship to "making" and social production, and then (in the heuristic sense of then) enter into social relationships. From this abstract presupposition it follows logically that the central political problematic in analyses of class formation is that of the transition from the first "objective" positionality of class to the second "relational" or collective moment of class action. From a different premise, it is the same problematic as that posed by rational choice theory, namely how to account for counter-examples to the free rider option in social action.

One recent approach in historical sociology replaces the abstract category of class with a relational or network approach to political identity (Bearman, forthcoming). This approach reframes class as a set of concrete relationships grounded in time and space, and shaped by the relationships and networks among the actors themselves. The advantage of this for a theory of class formation is notable: Because patterns of relationality are already incorporated into the constitution of the agent of social class, the age-old problematic of how classes move from class structure to social solidarity is bypassed.

Another approach, exemplified by Katznelson's and Zolberg's recent *Working-Class Formation* (1986), is to challenge the ahistoricism of the ideal typical prediction--the teleology of development from class structure to

political action. By instead subjecting the problem to the discipline of comparative concreteness--namely, asking why did each pattern of working class formation vary from each other, and not from an abstract prediction--they transformed the prevailing problematic of exceptionalism and deviance in class formation into a different question. This made possible an entirely different answer.

And of course, the same points could and should be made about the questions we ask about structure using the concepts of bureaucracy, states, communities, etc. For years, for example, political sociologists have asked why England's development produced a political bureaucracy so deviant from the Weberian ideal-typical model we've all come to accept as the standard against which to judge "strong" and "weak" states. But when we compare the actual workings of England's bureaucracy against the French--not just as ideal types but as historically embedded structures--we discover not that England deviates from a "true" bureaucracy (a deviancy then necessary to problematize) but that the states worked differently in each case. And it is those differences that become interesting only once we have discarded the fallacy of the misplaced abstraction.

An example of the second discipline I mentioned above--subjecting our questions to the discipline of historical specificity (their grounding in time and space)--is drawn from my own research (Somers, 1986). I began my research into English working-class formation with an inherited question: What was it about the industrial revolution that led nineteenth-century English workers to carry out the kinds of political practices and protests that they did? The literature is rich with answers to this question, all of which presuppose the industrial revolution to be the causal element in the explanation for the social practices of working people. For background I read widely in the literature of the seventeenth and eighteenth centuries. Much to my surprise I

found evidence for the presence of much of the same social discourse of protest and many of the same political practices. Discovering also the regional distribution of an early industrial labour market, I devised a new question asking what was it about the intrusion of early rural-industrial markets that could explain these earlier working-class activities. Again interested in background, I turned to the fourteenth century. Most striking of all, I found that peasant communities (in the same limited regions as in the later periods) articulated their social and political practices in language and action remarkably similar to those of their nineteenth-century descendants. In the fourteenth century, however, there was no industrial labour market. By now it had become clear that an entirely new question was in order, one which firmly detached the problem to be explained from the presuppositions of economic and market influence and reframed it in terms of actual (rather than paradigmatic) temporalities and spatial distributions. Thus my new question: How are we to understand such continuities and regional contrasts in popular practices for over five centuries--despite the radical economic changes and the transition from feudalism to capitalism to the industrial revolution? The original question--drawn from the abstractions of modernization theory with its fixation on the industrial revolution--contained misleading assumptions about time and space; subjecting the question to the discipline of historical specificity produced a surprisingly different, but hopefully more historically grounded, problem.

II

Let me briefly move to the second trend I mentioned in the beginning: The backlash against history from some sociologists who claim that social theory is suffering from what they call the empiricist and historicist methods of many of our leading historical sociologists. The attack, led by Michael Hechter and Edgar Kiser, among others, is directed at what Skocpol has called

"analytic inductivism," the method of building arguments from historical research using only loosely organized concepts, rather than conducting research through the hypothetical-deductive (h/d) method of testing theories. The attack is similarly aimed at Stinchcombe's use of historical analogy and M. Mann's interpretive approach. Hechter argues that only the theory-driven h/d method can produce the kind of social theory needed in historical sociology, namely general theory which can equally predict the future and explain the past--that which in science is called the covering law model of theory.

Presumably it is unnecessary in this audience to defend the use of history. Instead I'd like to play the devil's advocate and address Hechter's anti-history argument about social theory. Suppose we all agree that sociology and history are both sciences; what kind of models does science actually provide for theory-building?

If we begin with physics, the "queen of the sciences," we discover that in recent years analyses of its workings have changed considerably. For one, the age-old deductivist bias in the philosophy of science (from Popper to Lakatos) is no longer a consensus. This bias had it that scientific work is only theory-driven and that the purpose of for observation is to test pre-existing theories and hypotheses. This hypothetical-deductive method alone, the argument continues, is the source of the production of new theory through the falsification of theory by data. "People propose, data dispose" is the fairy-tale version of the story.

Hechter and Kiser should have looked at the history--not the philosophy--of actual scientific practice instead of merely repeating the h/d bias. In last ten years the purely deductive version of science has come unravelled from new perspectives on scientific practice. These demonstrate that the relative import of theory-driven data testing has been radically exaggerated.

Science reveals many avenues to pathbreaking knowledge: testing theories by observing only those phenomena designated by a pre-existing hypothesis is only one of them. Observation, very much like the role of analytic induction in historical sociology, has a life of its own independent of the h/d model of conducting research. This is not to say that those who observe are engaged in mindless empiricism. They bring to their observations the same kinds of controlled theoretical constructs and limited theoretical concepts Hechter and Kiser condemn comparative historical sociologists for employing. Analytic induction is theory-building driven by history and observation, not by pure deduction.

Consider the classic case of the big bang theory. In one laboratory Penzios and Wilson, two radio astronomers, experimented on what they were afraid might be a meaningless phenomenon but which held intrinsic interest to them--the static found in transatlantic radio. After considerable experimentation, they determined that there was a uniform amount of energy in space. Meanwhile, physicists at Bell Labs were theorizing about what came to be called the big bang theory. Only the discovery of the experimental tests--conducted utterly independently of any hypotheses about the origins of the earth--could confirm the theorists' speculation of a uniform temperature throughout space. They found it in the work of Penzios and Wilson. If science had proceeded the way Hechter and Kiser advocate we would have many less theories about the physical world. Should we have lesser aims for historical work?

The second revision in physics is equally important and addresses the nature of theory and historical narrative. Hechter and Kiser attack historical sociologists for not being concerned enough with the production of "general theory" which can explain the nature of the causal mechanisms by which empirical phenomena are generated. In the name of true science, they

lambast historical sociologists for not producing theory that is universal enough in scope and "bold" enough in its propositions to be able to predict the future and explain the present in the same way the past is causally explained.

But scientific practice once again poses a challenge. In physics the assumption that the fundamental covering laws of general theory can both predict and offer causal explanation has recently begun to unravel. It is now questioned whether a single theory can combine covering laws with explanations of actual causal mechanisms. Explanation in physics involves two different kinds of activities and two different tasks. First, when scientists explain a phenomenon or class of phenomena, they state its causes. They provide detailed accounts of the causal mechanisms by which the phenomena are produced. Second, they try to fit these phenomena into a broad theoretical framework which brings together, under one set of fundamental equations, a wide array of different kinds of phenomena. Both kinds of explanations use what philosophers have called laws of nature, but the two aspects of theory do not look at all alike.

The causal story is just that--a story that uses highly specific accounts which tell what happens in concrete situations. In contrast, covering laws are thoroughly abstract formulae which describe no particular circumstances at all. This is evident in the respective rules for the two types of accounts. Competing theoretical treatments--treatments which offer different general laws for the same phenomena--are encouraged in physics. By contrast, only a single causal story is allowed. Causal stories do not tell first one causal story then another according to their convenience. Causal stories are treated as if they are true or false, but which fundamental theories govern the phenomena is a matter of convenience (e.g. there are dozens of fundamental theorems for laser operations and scientists openly

choose one or another depending on other factors. And not surprisingly, although philosophers generally believe in laws and deny causes, because of the specificity of causal stories actual practice in physics works in just the reverse.

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

The conventional n/d account claims that we have explained a phenomenon when we have shown how it follows from a more fundamental law. But a real explanation in fact cannot fit under a fundamental law for its necessary detail and complexity contradicts the n/d method's aims of extending the scope and universality of theory. Fundamental laws do not govern objects in reality; they govern only objects in models. What they govern has only the appearance of reality and the appearance is far tidier and more readily regimented than reality itself. Reality demands causal stories. Generality and causality cannot be contained in a single theoretical utterance. The historicity of causality precludes their co-existence.

Since causality, unlike generality, involves "stories", it is also the historicity of the causal explanation that is equally relevant for the defense of history against the attacks of the general theorists. Take an example with which we are all familiar: It is a general covering law that to lose weight, you have to eat less. This suggests causal mechanisms--eating less causes the effect of losing weight--and it may also be a powerful predictor, but it doesn't in fact explain the mechanism of how that actually happens. The statement doesn't exhibit a cause. How do we exhibit a cause? We tell a story, a narrative about how a class of events is actually affected by something else. Since food has calories, and calories are energy, when we reduce our intake then the body has less energy to draw from external sources so it has to turn to internal sources of energy, which are stores of fat and it uses up fat when it draws that energy, etc. etc. Along the way we may use general laws but they aren't in themselves explanations for why, when we eat less, we lose weight.

The moral of this story is that in science as well as sociology, an explanation which actually depicts causal mechanisms is always told in narrative form. It is a set of sentences with transitive verbs. "The reduction of energy caused the body to draw on other sources..." "A actually caused B to happen by means of the following mechanisms and processes..." Cause implies narrative. The historical and temporal dimension of comparative history is thus as important as its comparative component for it entails 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, sequence and contingency. The explanatory narrative, if effectively argued, should produce an explanation through the historical trajectory itself.

Conclusion

First the obvious: The "historic turn" in science is making it impossible for anti-historians to play the science card any more. If sociology and history are sciences (or perhaps what my my colleague Mayer Zald (1988) has recently called "quasi-sciences") and must live up to scientific standards, historical sociologists are using scientific methodology no less than are those advocates of the h/d model of social theory. Historical sociologists by and large are interested in explaining patterns over time and space. Just as in physics, once too great a degree of generality has been introduced, we are no longer producing actual causal stories but abstract propositions. How, for example, a revolution occurred in one place and not another, cannot be explained by covering laws. Historical sociologists have set themselves a different task than general theorists and philosophers are now aware that the same division of labour exists in scientific practice. The same theories in physics cannot not do both: they cannot give a general covering law and a causal statement in the same statement. We can no longer simply subsume one under the other and claim that same task is being performed.

Since historical sociologists are largely interested in generating social theory by explaining patterns and variations over time, we have an interest in avoiding exactly the kind of general theory that Hechter and the anti-historians advocate. Indeed the lesson from physics is that a high degree of generality and actual causal stories about the past cannot be combined in a single theory. Theory, it appears, is either historically grounded or abstractly general. It cannot be both.

But no less important is the lesson that physics provides on the role of historical narrative in the process of generating theory. If the grand

theorists of physics now recognize the sine qua non of historical narrative, I ask the same question as above, should we ask anything less for social theory?

So when we ask, does social theory need history? The answer must be a resounding yes--because our work is rich with unexamined and ahistorical constructs which are organizing the questions of our social research. Many of the social categories we all use--historians and sociologists alike--are free-floating pieces and fragments of abstractions and paradigms. If we continue to use these to formulate questions, we will continue to produce not explanations for historical variation but increasingly desperate theories of deviance from the abstractions that pass as theory. History can no longer be taken as the "context" and the "background chapter" for social research and social theory. Attention to the context of discovery makes it clear that it is no longer adequate to say that history enhances our social theories. It is time to recognize that without an historical epistemology, we actually get our questions, and thus our answers, wrong.

To solve the problems of theory we need more historical deconstruction of social categories, of the theoretical problematics that are embedded in historical ones, and of the contexts in which they are discovered. This is not a plea for empiricism; on the contrary, the high ground on which we all meet is that of theory and ultimately epistemology, and the highest grandeur one can endow to epistemology is historical specificity.

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