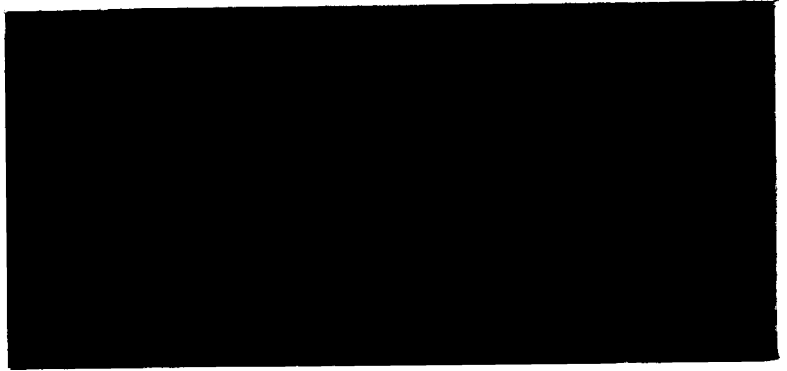




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**"What We Talk About When We Talk
About History: The Conversations of
History and Sociology"**

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What We Talk About When We Talk About History: The
Conversations of History and Sociology¹

Terrence J. McDonald

As the epigraph for his enormously influential 1949 book Social Theory and Social Structure Robert K. Merton selected the now well known opinion of Alfred North Whitehead that "a science which hesitates to forget its founders is lost." And both history and sociology have been struggling with the implications of that statement ever since. On the one hand, it was the belief that it was possible to forget one's "founders" that galvanized the social scientists (including historians) of Merton's generation to reinvent their disciplines. But on the other hand, it was the hubris of that view that ultimately undermined the disciplinary authority that they set out to construct, for in the end neither their propositions about epistemology or society could escape from "history."²

Social and ideological conflict in American society undermined the correspondence between theories of consensus and latent functions and the "reality" they sought to explain; the belief in a single, scientific, transhistorical road to cumulative knowledge was assaulted by theories of paradigms and incommensurability; marxist theory breached the walls of both idealism and the ideology of scientific neutrality only to be overrun, in its turn, by the hordes of the "posts": post-positivism, post-modernism, post-marxism, post-structuralism, and others too numerous to mention. In the deepest irony of all, we are here today, in part at least, to consider whether the oasis of epistemological peace shimmering on the horizon may be -- "history."

We work now in the twilight of the authorities of that generation of historians and sociologists; their inevitably

¹ The research assistance of Victoria Getis was indispensable to this essay and it was improved by many conversations with her. I have borrowed the title from the poignant short story of the late Raymond Carver, "What We Talk About When We Talk About Love," which has always seemed to say to me that we may do better just to talk, ^{some} intangibles, rather than attempting to define them. My colleagues in the "Historic Turn" group of CSST will see that I have tried to steal as many of their ideas as possible for this paper.

² Robert K. Merton, Social Theory and Social Structure: Toward the Codification of Theory and Research (Glencoe, 1949).

timebound and theoretically infused version of reality -- both "social" and epistemological -- is crumbling. The complaints of those who believe that we are losing our grip on "reality" are almost right: a certain version of reality containing propositions about society, history, epistemology, etc. is losing its grip on us. But the temptation presented to our era by the "paradigms" and the "posts" is to reproduce precisely the same mistake of the generation of Merton, to believe seriously that a "new (epistemological) order of the ages" is within our grasp; perhaps even simply a matter of will.

This new attempt to "forget one's founders" is particularly damaging for interdisciplinary discussion because it ignores the deep inter-relationships among all the disciplines reinvented in America after World War Two and the ability of those constitutive relationships to continually take this conversation in its most sterile directions. We are working today within, not beyond the disciplinary relationships constructed in that epoch. For example, our referents for "sociology" or "history" and the metaphors of our own conference -- turns, boundaries, the attempt to "talk across" disciplines, to avoid the "retreat behind disciplinary walls," etc. -- exist not in some transhistorical space but in the knowledge of disciplines and their interrelationships constructed in this era. It is only because of this shared knowledge of what these disciplines "are" that we can now talk about work that is "inter-disciplinary," "multidisciplinary," or "beyond disciplinary."

But this pre-existing relationship is doubled-edged; for while it permits us to talk it also tends to maintain the conversation within the safe boundaries of pre-existing discourse. Should historians use theory? Should sociologists do archival research? These questions--and others like them focusing on questions of practice -- have dominated the discourse between history and sociology because they protect and do not destabilize the currently operative relationship between these disciplines. Although these discussions are carried out with much heat at times--both within and across the disciplines--they serve only to prevent discussion of two more dangerous questions: Shall history become an object of theory? Shall sociology become an object of history.

My paper will argue that particular and not obvious kinds of atheoretical history and ahistorical sociology sprang from the same moment in American intellectual history. The constitution of history as a discipline that borrowed theory in fact saved it from the responsibility and potentially destabilizing effects of producing theory. Historians were permitted to borrow theory from sociology (and other disciplines) but not to test, generate, or even

discuss theory themselves. The constitution of sociology as a discipline with no sense of itself as an object in history prevented (and still prevents) it from recognizing the historical contingency of its own discourse. Sociologists are able to use history -- it is not correct to say borrow it -- as long as they do not allow sociology itself to become an object of history. The condition of transformation--assuming that such is desired--is that each discipline embrace its deepest fear; that history accept the potentially destabilizing threat of theory and sociology accept the potentially destabilizing threat of history. But it is the advocates of interdisciplinary work themselves who, in maintaining these relationships in the way that they have been structured, fail to recognize this and thus prevented such transformation.

To make this argument I will examine, in their turn, history's construction of "its" sociology and sociology's construction of "its" history. In the case of the former which I know better, my argument will be based on an analysis of statements about social science and history among historians as well as a case study of the sources of theory in one of the most important of the joint ventures between history and sociology, the so-called "new" urban history. In the case of the latter, which I describe from the "outside," I will similarly examine the historical context for and contents of statements about history in sociology, focusing in particular on a case study of the leading sources of historical theory in 48 articles by sociologists on historical sociology.

History's Sociology

The inability of history and sociology to imagine a new kind of relationship is embedded in the relationship between the two constructed in the years following World War Two. Like the other social sciences history in these years drank deeply of the elixir of the "new." With the help of the SSRC historians embarked on a highly successful transformation of their own discipline which would involve the demolition of the old "scientific" approach to history (with its view of an unmediated relationship with the "facts") and its replacement by a version of a new "scientific" approach to history (paralleling that of Merton and his counterparts in the social sciences), within which middle range theory mediated the relationship between the historian and the "facts" both as source of hypotheses and guarantor of "objectivity."

At the level of epistemology this transformation resulted in a permanent separation among the "actual" past, the "recorded" past and the "written" past. At the level of historiography this produced a spate of "new" histories -- e.g. the "new" urban, labor, political, family, etc. -- that

exploded into prominence in the sixties and seventies and that shared a theoretical orientation, some methodological sophistication, and a claim to be doing history "from the bottom up."

Most commentaries on this transformation have failed to understand the connection between the first change and the second, and have, therefore, misconstrued the second as essentially a methodological--not theoretical--change. In fact, the turn to the social sciences was never, either in theory or practice essentially methodological because it was necessitated by the destabilizing effects of the epistemological separation noted above. Once the unmediated search for "facts" was exploded as a myth both the danger of relativism and the prestige of science convinced many historians that it was time to look to the social sciences for a model of the disciplining role of theory. Advocates of this new relationship between history and social science rarely mentioned method. But they did not advocate a "full fledged" relationship with theory, either. In order for theory to do the job that was expected of it historians were to remain inferior to theory producing disciplines. They could, therefore, borrow theory but not speak of it.³

However, the necessity for a disciplining role for theory was far from the minds of Charles Beard and his allies as they wrote the text for the 1946 report of the SSRC's Committee on Historiography, Theory and Practice in Historical Study. Indeed, the report made only fleeting reference to the social sciences at all, noting that "significant advances in making the most comprehensive historical generalizations will require the close and constant cooperation of specialists in historical work with specialists in the social sciences and humanities," but also calling for coordination of the work of historians with the physical and biological sciences.⁴

Beard and his allies had other fish to fry. The first of these was to convince the historical profession of the tripartite distinction among history as actuality, record, and text, and the second to put forward a theory of change

³For examples of works that see this change as essentially methodological, see John Higham, History: Professional Scholarship in America (Baltimore, 1983), and Peter Novick, That Noble Dream: The "Objectivity Question" and the American Historical Profession (New York, 1988). For an argument on this point similar to mine, see Ian Tyrell, The Absent Marx: Class Analysis and Liberal History in Twentieth Century America (Westport, 1986).

⁴Social Science Research Council Committee on Historiography, Theory and Practice in Historical Study: A Report of the Committee on Historiography (New York, 1946), 139-140.

in the last. At the very opening of the report Beard laid down the definitions that would inform it. According to Beard, "history-as actuality" referred to "all that has been felt, thought, imagined, said, and done by human beings as such..." while "history as record" was "the documents and memorials pertaining to history-as-actuality on which written-history is or should be based." "History-as-written," therefore, became the systematic or fragmentary narration or account purporting to deal with all or part of this history-as-actuality..."⁵

These separations among "history" as actuality, record, and text were repeated in the chapter on "Controlling Assumptions in the Practice of American Historians," and further strengthened in the chapter on "Problems of Terminology" written by the philosopher Sidney Hook. Other approaches were summarily--sometimes humorously--dismissed as in this sentence that might bear reading by some historical sociologists today:

"When we have read John Stuart Mill's Logic and absorbed his baconian conception of the nature of science, we will eschew all guiding hypotheses and indefatigably collect "facts," hopefully trusting that somehow good, in the form of some "synthesis" that will make it all clear will be the final goal of all this ill. We will be strictly "critical" and "scientific" historians...."⁶

The point was, of course, that for Beard such a position was fantastic. Because of the separations at the outset, there was no history without what the report called a "frame of reference," which Hook defined "loosely" as "the set of principles which guides [the historian] in the selection of his problem, the organization of his materials, and the evaluation of his findings." The key question for historians, then, was how and why these frames of reference changed. The answer that the volume proposed through both its descriptive and prescriptive sections was a functional one; historians wrote with within a context "of a problem faced by men [in the present], of the causes of that problem, the means for its solution, and the course actually adopted." The frame of reference for history had been and always would be "functional" for the present. This argument was supported by an analysis purporting to demonstrate that changes in historiography in the twentieth century were brought about by the changing social and political agendas of American society with, for example, the history of Turner and Beard influenced by the progressive reform movement, that of the thirties by the "problems of capitalistic

⁵Ibid., 5.

⁶Ibid., 19.

development" and that of the postwar years to be affected by a changed international situation.⁷

The storm of protest that greeted this volume --amply treated by Peter Novick in his 1988 volume on American historiography, That Noble Dream --dealt with this essentially political functional and avowedly "presentist" theory of historiographical change, not the differences among history as actuality, record, and text. (Although because the report had both destabilized the fact/framework issue and offered this theory of change in the framework they were at times confused.) In fact from 1946 until now there has been no widely accepted work of historical theory that has not recognized these distinctions (if anything they have become even more firm and more complicated). The question that the volume raised among its critics was, given this separation, was there a more "disciplined" source of ideas for the frame of reference.

The 1954 report of the historiography committee of the SSRC (with mostly new membership) offered a solution that was to last for almost 30 years: social science theory would provide a stable--indeed "scientific"--source of ideas for these frames. This was the first of what would grow to be a large number of works by advocates of the social sciences in history, including the SSRC volumes, volumes of essays on the topic edited by Edward Saveth and Seymour Martin Lipset and Richard Hofstadter, and other essays and volumes by H. Stuart Hughes, Thomas Cochran, and Robert F. Berkhofer, Jr. These authors and volumes disagreed on many things, including, for example, how many and how much historians would use social science theory and methods, whether history should become a social science or merely use social science theory to clarify its generalizations, and whether the use of social science theory was simply to be an antidote to "Beardian relativism" or the route to a cumulative historical knowledge gathered according to scientific principles. But they also agreed on many things, including the rejection of the Baconian belief that synthesis would emerge from the "facts," on the priority of theory over method in the relationship between history and the social sciences, on the role of theory as a guarantor of objectivity, on the necessity and desirability of historians importing rather than generating theory, on the preference for middle range over "grand" theory, and of the irrelevance of Marxism to this entire enterprise. A brief review of these areas of agreement will reveal the way in which historians structured a essentially atheoretical relationship with the social sciences. 8

⁷Ibid., 51, 125.

⁸Social Science Research Council Committee on Historiography, Theory and Practice in Historical Study: A Report of the Committee on Historiography (New York, 1946);

The rejection of Baconianism was an assumption so deeply rooted in these works that it was seldom given much consideration. Just as the 1946 volume of the SSRC committee denounced the view that historians could indefatigably collect 'facts,' the section on "objectivity, certainty, and values" in the 1954 report declared that "no one now supposes that past history in its totality is recoverable, and few believe that 'the facts speak for themselves.'" Similarly, in 1969 Robert F. Berkhofer, Jr., admirably quoted Carl Becker's condemnation of the old scientific history as "...expecting to obtain final answers to life's riddles by absolutely refusing to ask questions--... the oddest attempt ever made to get something for nothing."⁹

It was in large part this belief that steered these commentators away from discussion of method and toward theory; obsession with method, after all, could become just another, more sophisticated kind of Baconianism. More important were the sources and types of theory that would help to carry history beyond Baconianism. Faithfully reflecting the pecking order of the philosophy of science of their time these authors agreed that history was not yet a science and, therefore, not yet capable of generating theory or even, for that matter, cumulative knowledge. For this reason, as H. Stuart Hughes put it, their works cast the historian "in a comparatively humble role--as a learner sitting at the feet of his colleagues in the social sciences."¹⁰

The 1954 report of the SSRC committee on historiography was the locus classicus of this approach. The report declared that history as social science "rests on the postulate that history can be more than entertainment and more than ideology" and that theory was the route beyond both. On the one hand, it was only via theory that history, too could become a cumulative science. "It is," the report

Social Science Research Council Committee on Historiography, The Social Sciences in Historical Study (New York, 1954); Social Science Research Council, Committee on Historical Analysis, Generalization in the Writing of History (Chicago, 1963; Edward N. Saveth, ed., American History and the Social Sciences (New York, 1964); Seymour Martin Lipset and Richard Hofstadter, eds., Sociology and History: Methods (New York, 1968); H. Stuart Hughes, "The Historian and the Social Scientist," American Historical Review, 66 (1960), 20-46; Thomas C. Cochran, The Inner Revolution: Essays on the Social Sciences in History (New York, 1964); Robert F. Berkhofer, Jr., A Behavioral Approach to Historical Analysis (New York, 1969).

9SSRC, Theory and Practice, 19; Berkhofer, Behavioral, 23.
 10H. Stuart Hughes, "The Historian and the Social Scientist," American Historical Review, 66 (1960), 44.

declared, "the use of theory that permits us to hope that in history, as in other sciences, the results of research may become increasingly cumulative." As importantly, on the other hand, it was theory that guaranteed objectivity. Contrary to what is sometimes believed by those "outside the scientific disciplines," the report declared "a set of interrelated hypotheses is the best check against unconscious bias." Indeed, without such an explicit scheme "the data are likely to be subconsciously selected, or catalogued on the basis of implicit or surreptitious assumptions not subject to a conscious process of analysis and rectification." Because of this, it was a high priority for this report--and later commentaries--that "principles of selection and interpretation...be rationally chosen and rationally established; by making the theories upon which it is based explicit and open to objective appraisal." 11

To facilitate this use of theory, the report's central section was a review of "concepts and viewpoints in the social sciences," that might be useful to historians. This was a chapter by historian Thomas Cochran on the state of the theoretical art in anthropology, sociology, demography, social psychology, political science, and economics that still repays reading. However the limits of Cochran's plan for historical social science that were implicit in this cook's tour of theory were explicit in his opening declaration that "A little knowledge may be a dangerous thing, but it is a necessary thing for the general historian." 12

Indeed it was. For a generation that believed, as Robert Berkhofer would write in 1969 that the "theoretical conceptions prevalent in his own society" define the "limits of the historian's objectivity" a "little knowledge" might have even been better than more. Because the goal was a stabilized, objective history, not a theorized history, a deeper encounter with theory might have been counter-productive for it would inevitably have revealed the disputes over theory that raged in some of the disciplines. Nowhere in the volume were historians encouraged to test theory or even attempt to understand it thoroughly. Cochran noted somewhat lamely that "if the reader is not quite sure that he understands the meaning of the terms and concepts mentioned in this chapter, he is in no worse situation than other social scientists. Even leading scholars in disciplines as closely related as sociology and social psychology find difficulty in precise communication." 13

In fact one of the most popular guides to this relationship between history and social science theory--the

11SSRC, The Social Sciences, 90.

12Ibid., 34-85.

13Ibid., 34,83; Berkhofer, Behavioral, 25.

essay on "The Historian and the Social Scientist," by H. Stuart Hughes that was published in the American Historical Review in 1960--suggested even less engagement. Hughes accepted wholeheartedly the view that history would take its theory from other disciplines: "since history has no generalizations of its own--since the only specifically historical category is that of time sequence--it must necessarily borrow its intellectual rationale from elsewhere." However, the way to apply this theory was quite different in Hughes' scheme. For Hughes even the word "application" of theory was "too immediate and too concrete" to describe accurately what the historian may most profitably do with the insights of his fellow workers in the social sciences. He continued:

In many cases, perhaps in a majority of cases, he does not really "apply" them at all. He lets them remain in the back of his mind, without bringing them explicitly into the foreground of his historical writing. He does not parade his knowledge of social science theory; he simple permits his thought to be informed by it. To the unpracticed eye, his prose may remain just as untheoretical as in the past. But the new type of knowledge he has absorbed will actually have worked subterranean alterations in his whole mode of thought and expression....14

There were those in this camp who disagreed with Hughes, most prominently David Potter in his 1963 contribution to the SSRC volume Generalization in the Writing of History on "Explicit Data and Implicit Assumptions in Historical Study." For Potter the approach recommended by Hughes was precisely the use of "implicit theory" that he criticized. But the more frequent references among historians to Hughes than to Potter tell a tale of retrenchment along this front that was broader than just the essay by Hughes. In fact the 1963 SSRC volume itself had pulled back considerably from the great expectations of that in 1954, admitting that there were only some historians--the report called them the "theoretical historians"--who would use social science theory anyway and concluding weakly that "historians borrow ready-made generalizations whether they know it or not. If they were to borrow them knowingly, they might be in a stronger intellectual position."15

One source from which historians were not to borrow, though, was Marx. The utility of Marxism was so thoroughly

14Hughes, "Historian," 34.

15David M. Potter, "Explicit Data and Implicit Assumptions in Historical Study," in SSRC, Generalization, 178-195; Ibid., 209.

discredited in the SSRC volumes that it was rarely even mentioned as a source for theoretical borrowing outside of them. In the 1954 report Marxism was not classified as social theory at all, but discussed along with a variety of conceptions and "misconceptions" of historical change, including evolutionary or theological theories and the works of Toynbee and Spengler (works already notorious among historians as merely speculative "philosophies of history."). According to this report the Marxist interpretation lacked validity because of its "limited purview of operative forces and the factual fallacy in the labor theory of value." The "complex windings of Marxian dogma" did not result in cumulative knowledge "because it is of first importance in Marxian dialectics that each new proposition asserted to be true must be logically consistent with the words of the master; it is a secondary consideration whether or not the words have any empirical validity."¹⁶

In the only essay in the 1963 volume that mentioned Marxism in general, Marx remained lodged among the "a priori" system builders of the type with whom theoretically inclined historians were loathe to be identified. In his essay on historical generalization in that volume William Aydelotte was at some pains to distinguish the generalizations he urged upon historians from those of Toynbee, Spengler, and Marx that some historians mistakenly confused as generalizations. While declaring that he did not advocate "ignoring the larger questions relating to the structure of society and politics that have always fascinated men," Aydelotte nonetheless argued that "the restriction of objectives, in history and the other social sciences, may be a sign not of degeneration, but of maturity. For him, as for so many of his generation, the preference was for those "middle range" theoretical procedures recommended by sociologist Robert Merton.¹⁷

This reluctance to engage with theory--or even to recognize a broader menu of theory--has often been ascribed to the essentially atheoretical and methodological relationship between history and sociology. In this argument theory was downplayed because historians--atheoretical to begin with--were really interested in the method of the social sciences. But this view is totally contradicted by analysis of the actual use of social science literature by the most popular and widespread of the "new" histories the "new" urban history. For another purpose I have undertaken an analysis of every citation outside of history in 140 works (113 articles and 27 books) of American urban history published from 1940 through 1985. This has

¹⁶SSRC, Social Sciences, 140.

¹⁷William O. Aydelotte, "Notes on the Problem of Historical Generalization," in SSRC, Generalization, 145-177.

netted 1543 such citations for analysis which reveal that this encounter between history and the social sciences was broad based, overwhelmingly theoretical, primarily sociological, mostly middle-range, and, unsurprisingly, only minimally marxist.¹⁸

Of the 113 essays analyzed for this project only 16 (14%) contained no references to works outside of history while 71 (63%) had three or more. Only 39 (2.5%) of the 1543 citations were explicitly methodological, and of these 39 the majority had to do with demographic calculations. Similarly, there were only 44 (2.9%) citations to the works of Marx or Marxists among all of these and the bulk of those came after 1980; 24 in a 1983 collection of essays alone. However, the lack of attention to Marx, himself, was symptomatic of a broader turn away from most classical social theory and toward the "middle range" theory under construction in the years after World War II. References to almost all works of classical social theory begin to die out in these citations after about 1963 and only about 20 percent of all these citations were to works of any author published before 1950, classical social theorist or otherwise. Table I, which lists the most frequently cited authors reveals an overwhelmingly sociological bias with a special attention to--not surprisingly--urban sociology. But it also reveals the popularity of some of the most important theorists of the middle range writing in the 1950s and 1960, for example, Merton, Lipset, and political scientist Robert Dahl.

[Table 1]

There was, however, almost no commentary on this theoretical development within the field. What was invoked instead of the authority of theory was the authority of the theory-producing discipline that produced it. In a famous article "introducing" urban "ecological" theory to the field of urban history Eric Lampard devoted exactly one paragraph to exposition of the theory, noting, instead, that it was developed in sociology and was, therefore, "already at hand." When in 1967 Charles Tilly lamented that no one [among urban historians] was listening to Lampard, he got it exactly backward, as Table 1 reveals. What Lampard--and others--were talking about was not "theory," but where to

¹⁸For details of this analysis see Terrence J. McDonald, "Faiths of our Fathers: Middle Range Social Theory and the Remaking of American Urban History," Paper prepared for presentation at the "Modes of Inquiry for American City History Conference," Chicago Historical Society, October, 1990.

look for theory, and almost everyone was "listening" to urban sociological theory, ecological or otherwise.¹⁹

The problem among historians was that no one was talking back to theory. Of more than 40 essays on the state of the field of urban history published between 1963 and 1985 less than half even mentioned theory and only two produced very searching analyses of any part of it. As the rate of two definitional essays per year reveals, historians attempted obsessively to define and redefine the field on the terrain of historiography but their labors had about as much effect as the handwashings of the obsessive-compulsive have on a neurosis. The problem for urban historians went deeper than historiography because, whether they admitted it or not, the field was, to a great extent, theory driven.

One result of the inability to confront theory was a failure even to begin to rethink the relationship between history and sociology. Even the advocates of this convergence on the history side offered little that was new. The 1954 report of the SSRC committee boldly declared that the relationship between history and the social sciences was not to be merely "one-way" because historians could "teach much as well." But there were no sections on the contribution of historians to social science. Stephan Thernstrom offered little more than this in one of the very few essays by an historian ever published in the American Sociological Review, his 1965 essay in "Yankee City Revisited: The Perils of Historical Naivete." Thernstrom pointed out rightly that communication between history and sociology had been "in the form of a monologue; with history on the receiving end." But his own article did little to challenge that relationship since the not unimportant brunt of his lengthy critique of W. Lloyd Warner's studies of Newburyport was that Warner didn't have his facts straight. Rather than proposing a new relationship between history and sociology Thernstrom reinforced the old wherein sociology dealt with the theory and history with the facts. In fact the choice that Thernstrom offered sociologists regarding history was identical to that offered in the 1964 report of the SSRC to historians regarding theory. According to Thernstrom the student of contemporary society was not "free to take his history or leave it alone. ... The real choice is between explicit history, based on careful examination of the sources, and implicit history, rooted in ideological

¹⁹Eric E. Lampard, "American Historians and the Study of Urbanization," American Historical Review, 67 (1961), 49-61; Charles Tilly, "The State of Urbanization," Comparative Studies in Society and History, 10 (1967), 103-104.

preconceptions and uncritical acceptance of local mythology."20

For their part, allies on the sociology side offered little more than backhanded encouragement. Leo Schnore's important 1975 essay on "Urban History and the Social Sciences: An Uneasy Marriage" in the Journal of Urban History offered one paragraph under the subheading of "Historians Can Help the Social Scientist" and eight and a half pages on how "Historians May Benefit from Exposure to the Social Scientist." The subject of the one paragraph was the infamous--and, as Schnore admitted, essentially vacuous -- notion of "historical perspective."21

This passive and ultimately untenable role for the historian vis a vis the social sciences brought with it two positive--if unintended--consequences, a sharpened sense of disciplinary self-consciousness and greatly lowered boundaries between history and the other disciplines. By continuously attempting to resolve the theoretical problems of history on the terrain of historiography historians never lost a sense of the construction of their discipline in history. History is today, therefore, one of the most historically self-conscious of the disciplines thanks, in large part, to a series of theory-generated transformations of the discipline. Similarly, while it was wrong to speak of theory, at the level of borrowing "everything was permitted." Therefore historians have continued to borrow massively from other disciplines and their sources of theory have spread far beyond sociology into anthropology, literary theory, feminist studies, cultural studies, ethnic studies, and elsewhere.

However, neither this heightened sense of history nor this broadened base of borrowing can overturn the longstanding--and by now quite comfortable--relationship of inferiority between history and the social sciences. As the recent uproar over the so-called "linguistic turn" has demonstrated, it is not at all clear that historians wish to have an "open" relationship with theory. Historians did not fail to develop a discourse about theory because they were ignorant of it, opposed to it, or dumfounded by it but because their relationship with theory was initiated in order to provide stability and prestige in a time of dangerous relativism. If to speak of theory means to change this relationship the silence may remain deafening.

20SSRC, Social Sciences, 16; Stephan Thernstrom, "'Yankee City' Revisited: The Perils of Historical Naivete," American Sociological Review 30 (1965), 234-242.

Sociology's History

In one of its first attempts to encourage interdisciplinary dialogue the Social Science Research Council selected six leading works of social science which it then submitted to interdisciplinary panels for evaluation at the end of the 1930s. To read the published transcripts of the discussions of these evaluation committees in the SSRC series Critiques of Research in the Social Sciences is to eavesdrop on the founding conversation about modern work across the disciplines, including history and sociology. The importance of this conversation for our purposes is that it set out a role for history in the social sciences generally and vis a vis sociology in particular that would remain prevalent, in spite of dramatic changes in historical practice, down to the present day. 22

From history the SSRC selected for analysis historian Walter Prescott Webb's book The Great Plains: A Study in Institutions and Environment which had been published in 1931 and the committee assigned another western historian, Fred Shannon, to prepare a critique that the committee would then discuss. Shannon produced 200 pages of empirical critique that included such gems as the complaint that Webb's description of jackrabbit hunting was inadequate and Webb was so enraged by the evaluation and discussion of his book that he denied at the end that it was a work of "history" at all. Inevitably, then, much of the discussion by the interdisciplinary panel which met in September of 1939 revolved around the proper relationship between facts and theory--or as it was referred to in the discussions, generalizations, hypotheses, or frames of reference. The evaluation committee included such giants of that epoch of social science as Louis Wirth from Sociology, Robert Redfield from Anthropology, and Arthur M. Schlesinger and Roy Nichols from History, both of whom were strong allies of interdisciplinary work in history.

The historians on the committee clearly worked to minimize the bitterness between Webb and Shannon--and thus improve the potential reception of the exercise among historians--by adopting the position that there was room in the profession for all "types" of historians. Historian John D. Hicks argued that historians fell into two categories "those who are interested in the woods as a whole and those who are interested in the trees, leaf by leaf."

21 Leo F. Schnore, "Urban History and the Social Sciences: An Uneasy Marriage," Journal of Urban History 1 (1975), 395-408.

22 Social Science Research Council, Critiques of Research in the Social Sciences III: An Appraisal of Walter Prescott Webb's The Great Plains: A Study in Institutions and Environment (New York, 1940).

While each group antagonized the other, there would always be such divisions among historians.²³

Wirth, however, held out for a more rigorous specification of the relationship between "facts" and "generalization" across all categories of history. What historians, whether of the forest or trees, often failed to realize, according to Wirth, was that "facts are made; they are not just found." "You cannot just look the world in the face and expect to discover a fact," Wirth argued, because facts "are always made in the light of some hypotheses." Failure to recognize this relationship between facts and hypotheses was what Wirth identified as the major problem among historians; they "are not explicitly aware of the theories upon which they proceed and therefore naively conclude that they have none."

Yet Wirth's own understanding of the relationship between facts and generalizations in general led him to send somewhat mixed messages to historians. At one point in the discussion he declared that "what we want, first of all, from the historians is authentic facts. Whether historians should generalize is a very complicated question. Their best service to social science, I think, is actually digging up the facts--bricks out of which some theoretic structure (generalization) may arise." Yet at another point he reminded them that historians, too, must work within a "frame of reference" to find the facts:

I hope historians will stick to the facts. Their concern should be to give us the fact--accurate, reliable facts. I realize, however, that nobody can get facts unless he has some frame of reference within which those facts appear.²⁴

Now as we have seen, historians themselves took up Wirth's call for the collection of "facts" within frames of reference beginning in 1946. Searching both for "science" and "objectivity" they borrowed massively from the social sciences--especially sociology--and transformed historical practice. Indeed the works of the "new" social history, while not producing a discourse on theory among historians, nonetheless effectively ended the epoch of purely "historical fact." Today there are few "historical" facts that have not already been infused by (mostly sociological) theory.

But there has been little recognition of this momentous transformation in historical practice among sociologists friendly to history. Thirty years after Wirth, during the

²³Ibid., 192.

²⁴Ibid., 189, 187, 193.

high tide of history's construction of its sociology one of the major bridge figures in this effort, Seymour Martin Lipset, would make almost the same point as Wirth:

"History must be concerned with the analysis of the particular set of events or processes. Where the sociologist looks for concepts which subsume a variety of particular descriptive categories, the historian must remain close to the actual happenings and avoid statements which, though linking behavior at one time or place to that elsewhere, lead to a distortion in the description of what occurred in the set of circumstances being analyzed."²⁵

Even critics of Lipset's division of "theory" for sociology and "facts" for history have tended to reproduce similar distinctions between history and sociology. In one of the most frequently cited --and most thoughtful-- discussions of the encounter between history and sociology, Charles Tilly's 1981 book, As Sociology Meets History, Tilly rejects Lipset's division in principle, but then reproduces it in his description of disciplinary practices. He notes, for example, that the authors analyzed by Arthur Stinchcombe in Theoretical Methods in Social History are not "archive-mongering professional historians," and contrasts the effort of sociologists to "bring data to bear on two conflicting hypotheses" with the historian's imitation of the procedure of the "literary critic: moving...from reinterperatation to reinterperatation" with each reinterperatation producing "a new understanding of the place, time phenomenon, and underlying question under study." More recently Theda Skocpol has similarly contrasted the "interpretive sociology" of a Geertz or Thompson with the "analytical" sociology that she practices. The former has the virtues of a "good Flaubert novel," while her brand of work analyzes the "facts" in search of "causal regularities."²⁶

According to Tilly "an analysis is historical to the extent that the place and time of the action enter into its explanations." But for almost 50 years now, the "place and time of the action" between history and sociology have seemed irrelevant to sociological commentators on it. Why is it that sociologists reproduce this "frozen" image of

²⁵ Seymour Martin Lipset, "History and Sociology: Some Methodological Considerations," in Lipset and Richard Hofstadter, eds., Sociology and History: Methods (New York, 1968).

²⁶ Charles Tilly, As Sociology Meets History. (NY: Academic Press, 1981); Arthur L. Stinchcombe, Theoretical Methods in Social History (NY: Academic Press, 1978); Theda Skocpol, ed. Vision and Method in Historical Sociology. (Cambridge: Cambridge University Press, 1984).

historical practice? The answer to this question requires analysis of sociology's experience with historical analysis, and the theoretical sources for its construction of "history." But it also requires consideration of an issue that few advocates of historical sociology have considered: sociology's deep fear of becoming, itself, an object of history. For just as historians feared and avoided the destabilizing effects of a discourse around theory sociologists have feared the threat from their own history, from the moment when their discourse of "science," "cause," "variable," and "analysis" is revealed, in its turn, to be historically contingent.

For all of the recent talk about sociology's "historical imagination," the discipline's encounter with history has, not surprisingly, been both relatively minimal and relatively recent. A survey of specialities of members of the American Sociological Association in 1959 (almost half of whom received their degrees before 1950) revealed only .2% who listed first a speciality in historical sociology or social history. And this lack of interest was reflected in articles published in the field's major journals. Analysis of the tables of contents for every year of the American Journal of Sociology (1895/96-1990/91) and the American Sociological Review (1936/37-1990/91) reveals a total of only 13 articles in the former and 11 in the latter on the relationship between history and sociology and only 95 articles in the former and 54 in the latter with historical content. In the AJS articles with historical content averaged about 5.5% of the total annually; in the ASR about 6.6%. In the ASR this annual average is consistently above the mean only after the 1975/76 volume; in the AJS only after 1978/79. In the American case, at least, it is incorrect to say that sociology is rediscovering its "past," unless that means its past 15 years.²⁷

Similarly, the sources upon which sociology is constructing its "history" are both relatively recent and overwhelmingly sociological. In Table 2 I have listed (in order of frequency of citation) the authors of the most frequently cited (i.e. 9 or more citations) sources of theory (of history and otherwise) in 48 programmatic essays by sociologists on historical sociology or history and sociology published since 1957. The recency of the references is explained by the recent appearance of most of the articles (listed in the appendix). The absence of historians among the most frequently cited is, of course, the striking thing about this list. In the top 25 authors cited there are only four historians: Stedman-Jones (7

²⁷Richard L. Simpson, "Expanding and Declining Fields in American Sociology," American Sociological Review, 26 (1961), 458-466. Data on articles is from my own survey.

citations), Thompson (7), Bloch (5) Carr (5). Both the theoretical legitimizers of historical sociology (e.g. Giddens and Stinchcombe) and the practicing commentators on it (e.g. Tilly and Skocpol) are sociologists, with Skocpol having more than twice and Tilly more than three times the number of citations of the leading historians.

[Table 2]

Is it safe for historical sociology to be constructed solely by representatives from a discipline which, as Giddens has noted, has "repressed" history? In fact it can be argued that at the core of sociology's "historical imagination" is a persistent attempt to imagine itself as exempt from history. To some extent, therefore, a battle on another front--between the forces of "presentism" and "historicism" in the history of sociology--helps to set the limits on history in sociology.

In the case of American sociology, at least, this ahistorical tendency in the history of sociology, too, springs from the fertile brain of Merton and his important distinction between the "history" and "systematics" of sociological theory. Merton's call for social science to "forget its founders" entailed not only the reinvention of disciplinary practice but also the end in any real sense of disciplinary history. His own words on this crucial issue in the 1949 edition of Social Theory and Social Structure bear repetition:

"The attractive but fatal confusion of utilizable sociological theory with the history of sociological theory...should long since have been dispelled by recognizing their very different functions. After all, schools of medicine do not confuse the history of medicine with current medical hypotheses, nor do departments of biology identify the history of biology with the viable theory now employed in guiding and interpreting biological research. Once said, this seems obvious enough to be embarrassing. Yet the extraordinary fact is that in sociology, this plain distinction between the history of theory and currently operating theory has in many places not caught hold...."

For Merton the analogy between natural and social science was the crucial legitimation for the separation between history and theory. Therefore, "systematic sociological theory" represented "the highly selective accumulation of those small parts of earlier theory which have thus far survived the tests of empirical research." The history of theory included "also the far greater mass of conceptions which fell to bits when confronted with

empirical test. Though "acquaintance" with "all this" (history) might be useful for sociologists, Merton believed that it was "no substitute for training in the actual use of theory in research." What he called the "prehistory of sociology" was "very far from cumulative," and, therefore, the contemporary sociologists were not--in the famous phrase of Newton that Merton was to make so much of-- "pigmies standing on the shoulders of giants." In fact, "the accumulative tradition is still so slight that the shoulders of the giants of sociological science do not provide a very solid base on which to stand."²⁸

This distinction--which Merton was to repeat and fortify--had two somewhat contradictory consequences within sociology. On the one hand sociologists would cohabit with the "founders" (e.g. classical social theorists) to the extent that they had produced ideas that not fallen to bits "when confronted with empirical test." On the other hand, they were implicitly forbidden to consider the historical context of transformations in sociological theory or practice, including their own. Invocations of the "founders" would be widespread both as inspiration and legitimation, "middle-range" operationalization and testing of their theories would be permitted, but analysis of the construction of sociology's modes of inquiry would be prohibited.

With the period before 1949 placed behind the "veil" of "prehistory" sociologists were spared the potentially unedifying sight of the triumph of recent tendencies in sociological theory and method and allowed to believe that the present development of sociological science was occurring "above" history. The historical development of such tools as "cause," "variable," or "general linear model," was ignored as were the contemporary appropriation of dichotomies from philosophy and elsewhere (e.g. idiographic, nomothetic; context of discovery, context of justification; analytic, interpretive) that justified particular kinds of sociological practice. What history of sociology there was came increasingly to resemble the regnant "history" of science before Kuhn, which, as Kuhn has noted, worked for both pedagogic and persuasive--but not historical--ends by chronicling the "successive increments" of cumulative knowledge and the "congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text." (As we have seen, this was Merton's own description of history.) Not surprisingly, sociology's self-image became not as a discipline constructed in a certain place and time,

²⁸Merton, Social Theory, 3-5.

but a "science" developing principles (of both theory and practice) relevant for all places and all times.²⁹

This problem of sociology's historical self-consciousness has only recently--but increasingly--been noted by historians of sociology as the problem of "historicism" versus "presentism" in the history of sociology. This debate, as carried out by Stephen Seidman and others revolves around whether in the history of sociology it is legitimate to "interpret texts in relation to the current theoretical context," (the presentist position) or whether a genuine history of sociology must be anchored in the historical context (the historicist position). Although this debate is recent--because the post-Kuhnian field of history of sociology is only about a decade old--its terms portend significant changes in the relationship between history and theory within sociology. For the "historicist" argument is that presentist approaches result in a narrative of scientific progress and enlightenment which tends toward an ideological reconstruction aimed at legitimating current theoretical or methodological positions. Just as a new empirical (but "historicist") history of science destabilized the philosophy of science (and social science) a new "historicist" history of sociology has the potential to relativize and destabilize current sociological practice.³⁰

Most of the (brief) histories of historical sociology presented by its advocates focus on the disjunction between that tendency and what Randall Collins has called "mid-century" sociology. But there are also powerful continuities between them in their constructions of and claim to "scientific" status. What I have called the "frozen" image of historical practice is a result of this continuity. This image, as we can now see, is doubly functional for mainstream sociology for by trivializing "history" as unscientific the discipline is spared both the relativizing dangers of its own history and the potential threat to the mainstream from historical sociology. But some historical sociologists, themselves, exhibit little historical self-consciousness and consciously or not over-emphasize the differences between their work and that of historians for the same reasons, so as to validate their own claim to the "scientific" status of the mainstream.³¹

29T. S. Kuhn, The Structure of Scientific Revolutions (Chicago, 1970), 1-2.

30For a useful introduction to these issues see Stephen Seidman et al., "The Historicist Controversy: Understanding the Sociological Past," Sociological Theory 3 (1985), 13-28.

31Randall Collins, Sociology Since Mid-Century: Essays in Theory Cumulation (New York, 1981).

The variety of these tendencies among historical sociologists is exhibited in the programmatic statements of the four most frequently cited influences on historical sociology: Tilly, Skocpol, Giddens, and Stinchcombe. While all of these authors have produced works of great importance for all practitioners of history there are, nonetheless, notable differences in their positions on the possibility of a unified "historical social science." On this point the two theorists of historical practice bracket the two practitioners. The more conventional empiricism of Stinchcombe links the historical sociology of the present more firmly to the "science" of the past and legitimates continuing separation between history and sociology, while the more radical "historicity" of Giddens leads to a critique of scientific "naturalism" in the past and thereby legitimates disciplinary convergence in the future.

Stinchcombe's somewhat idiosyncratic Theoretical Methods in Social History offers an unselfconscious empiricist plague on the houses both of "history" and "theory" that is delivered with little interest in (or apparent information about) the doings of professional historians. Stinchcombe's empiricism can be called conventional because it is anchored--like Merton's--in the analogy to the physical sciences: "It is not necessary to ignore the facts in order to have general concepts, as the example of the physical sciences shows. The argument of this book is that it is not only not necessary, it is also a bad idea." What he calls "epochal interpretations" (grand theories) ignore the facts by prematurely applying theory to them, "giving a specious sense that we understand the nature of the society we live in by providing a myth of how it came about--a myth illustrated with historical events." Narrative histories, on the other hand, "ignore the facts" by giving the false (linguistic) impression that the narrative is causal when, in fact, narratives must be broken down into "theoretically understandable bits" before causal analysis can proceed via his method of deep analogy. For Stinchcombe the "theoretical method" in social history requires both detaching the narrative from its "naive epistemological moorings," and "tossing out the epochal (theoretical) garbage."³²

At the other end of the spectrum stands Giddens whose characterization of the differences between the natural and social sciences and similarities between the latter and history distinguish him sharply from Stinchcombe. For Giddens there were three components of "mid-century" sociology (or what he calls the orthodox consensus); a set of statements about "industrial" society, a set of theoretical propositions loosely called "functionalism" and a set of statements about similarities in the historical

³²Stinchcombe, Theoretical, 1-25.

development and logical structure of the social and natural sciences that he calls "naturalism." While the rebellion against the first two is widespread, the predominance of the third remains less seriously challenged. It is, of course, the residue of this view that undergirds contrasts between (scientific) "analysis" and (discursive) "interpretation" and, therefore "history" and "sociology." By rejecting this naturalism Giddens is able to argue that because of their shared theoretical problematics and methodological challenges there is no "intellectually defensible" division between history and social science. Elaborated most completely in The Constitution of Society, Giddens' argument recognizes both the transformation in historical practice and the interpretive nature of much of sociological practice. Agreeing with Philip Abrams that the "acknowledged masterpieces of the discipline of history have become increasingly theoretically explicit," Giddens argues that the problems of social theory, of agency, structure and forms of explanation are "problems shared in general by all the social sciences." Furthermore, he contends that because most social science work is conducted "in and through texts" the methodological problems of making sense of texts are also shared by history and the social sciences.³³

It would be wrong to say that the most frequently cited practitioners--Tilly and Skocpol--are ranged between Stinchcombe and Giddens because both are more closely linked to the former, and therefore to mid-century naturalism, than to the latter. For example, both Tilly and Skocpol cite Stinchcombe, but in their programmatic work through 1984, neither cites anything by Giddens. While both are undoubtedly critics of the mid-century orthodoxies that Giddens labels "industrial society" and "functionalism," neither totally rejects the naturalism that Giddens criticizes and both, therefore, uphold the distinctions between "analysis" and "interpretation" that have replaced "theory" versus "facts" as the language of separation between "sociology" and "history."

As we have seen, even in his rejection of the longstanding division of labor of "theory" and "facts" between sociology and history Tilly has reproduced the separation between the two on another terrain. Although his portrayal of the distinguishing features of the historical profession are quite similar to those of Giddens, Tilly has not called for a "general rapprochement" between history and sociology, but "a highly selective shift of particular topics to historical analyses and historical materials." According to Tilly, historians group and gloss texts (the written residues of the past) in order to reconstruct past

³³Anthony Giddens, Central Problems in Social Theory (Berkeley, 1979), 230-233; 234-259; Giddens, The Constitution of Society (Berkeley, 1984), 355-363.

human behavior and they consider "where, and especially when an event occurred to be an integral part of its meaning, explanation and impact." But sociologists are not turning into historians because "they are not learning to do archival research...taking their questions from the prevailing historical agenda, or suppressing their inclinations to explicit modeling, careful measurement, and deliberate comparison." They are not, to use his earlier terms, either mongering archives or succumbing to interpretation.³⁴

Skocpol has maintained a similar division, but incorporated it into sociology in her essay on "Emerging Agendas and Recurrent Strategies in Historical Sociology," in Vision and Method in Historical Sociology. While professing a buoyant methodological pluralism--"Surely it would be a mistake to tie historical sociology down to any one epistemological, theoretical, or methodological development."--Skocpol leaves little doubt about her preference for "analytical" over what she calls "interpretive" historical sociology. In language deeply indebted to Stinchcombe she describes the former branch of sociology as involving the search for "causal regularities" without an effort to "analyze historical facts according to a preconceived general model." Because good analytic history requires that the "unities of time and place be broken for the purposes of drawing comparison and testing hypotheses," analytical sociologists must neither be fazed by the demand of some historians that they use "primary sources" nor let their findings be "dictated simply by historiographical fashions that vary from case to case or time to time." Interpretive sociology, on the other hand, (which for her includes--somewhat strangely--the work of E. P. Thompson and Clifford Geertz) displays "an insouciance about establishing valid explanatory models" that, from the standpoint of those "concerned with causal validity" in their analyses can be "misleading even when they are compelling." Interpretive works "seem extraordinarily vivid and full, like a good Flaubert novel," but their appeal is only to "others who share their sense of problems and their world views."³⁵

But, of course, the same thing could be said of Stinchcombe and Skocpol, themselves. For only those who share their sense of the untheorized historical "fact," the "garbage" of epochal theory, and the "naivete" of narrative will apparently be convinced by their analyses. Stinchcombe defends himself from the knowledge that historians produce only theorized facts by selecting for analysis only

³⁴Tilly, As Sociology Meets History, 43,14.

³⁵Theda Skocpol, "Emerging Agendas and Recurrent Strategies in Historical Sociology," in Skocpol, ed. Vision and Method in Historical Sociology, 356-391.

"historians"--Tocqueville, Trotsky, Bendix, and Smelser--who have not participated in the theoretical transformation of history. (And this, not their lack of "archive mongering" is, pace Tilly, the point.) By trivializing "historiographical fashions" Skocpol makes much the same mistake, ignoring the increasingly sophisticated discussions of historical practice in that literature over the last two decades and substituting in its place a resurrected J. S. Mill whose Logic was rejected with sarcasm by historians 50 years ago not because they couldn't talk about method but because, as Michael Burawoy has recently argued, Mill himself doubted the utility of the "method of induction" for the social sciences.³⁶

Skocpol's commitment to a "single world view" must also be detected in her refusal to apply her own "comparative historical method" to the differences between herself and Stinchcombe on the one hand and British sociologists like Abrams and Giddens on the other. This brief perusal of the works of all four reveals what she would call the "crucial difference": among the latter an almost complete absence of a rhetoric of "science" that justifies continuing differences between history and sociology. Now because this absence occurs in spite of "overall similarities" in the emergence and spread of professionalized sociology and history in the post World War Two years in Britain and the United States it makes a good case for her "method of difference." John Hall's 1989 article "They do things differently there, or, the contribution of British historical sociology," outlines briefly the "causal configuration" that explains the outcome of interest: a pre-existing body of sociological work on British social structure that led to a relative immunity to Parsonian functionalism, a favorable response in Britain to the earliest postwar American historical sociologists (e.g. Bendix, Lipset, Mills, Moore) and an early and close intellectual and political partnership between sociologists and practitioners of "history from below" (e.g. Thompson, Williams, Hoggart). Skocpol's "canons of induction" would seem to suggest that Stinchcombe and Skocpol speak not in the universal language of "science," but in the provincial, "scientific" argot of American academe.³⁷

It is the failure to recognize this crucial point that detracts from what is otherwise without doubt one of the most brilliant analyses of historical sociology to date, Michael Burawoy's 1989 article "Two Methods in Search of

³⁶Michael Burawoy, "Two Methods in Search of Science: Skocpol Versus Trotsky." Theory and Society v. 18, no. 6 (Nov. 1989): 759-806.

³⁷John A. Hall, "They Do Things Differently There, or, the Contribution of British Historical Sociology." British Journal of Sociology v. 40, no. 4 (Dec. 1989): 544-564.

Science: Skocpol versus Trotsky." This is a lengthy and devastating critique of Skocpol's methodological pronouncements that discusses, among other things, her contradictory attempt to produce historical sociology while standing "outside" of history, and that, therefore, challenges her positions on "facts," "historiography," and "theory." But because his discussion is conducted almost entirely in the language of a Popperian philosophy of science, Burawoy fails to realize that his own categories do not stand "outside" of history, either. The unasked question is whether the prominence of these categories or the metaphor of the "research program" are not themselves worthy of historical analysis. If they are then he has stopped his historical analysis just one step short of where it might have gone. In so doing, however, he has revealed the "hard core" of the research program for historical sociology that he had Skocpol share completely: that everything is an object of "history" except sociology itself.³⁸

Two Pasts; How Many Futures?

In a comparison of the methods and agendas of historical sociology and social history Skocpol has compared them to the proverbial "two trains passing in opposite directions in the night." This essay has argued somewhat differently and the metaphor it has sought to support is that of two trains hurtling down parallel tracks toward different kinds of derailment.³⁹

For historians the tracks are weak at the intersection of history and theory. In 1963 at the dawn of the relationship between history and social science that we have just considered the historian A. S. Eisenstadt wondered if, in a time of philosophical dismantling, "we do not take sides, we only take cover." From that day until this historians have tried to "take cover," by borrowing theory from other, allegedly more advanced disciplines with little or no commentary on the controversies over theory within those disciplines or the effects of theory within history. Both continued borrowing and increasing historical self-consciousness have eaten way at the ties at this point on this track.⁴⁰

For sociologists the tracks are weakest at the intersection of historical sociology and disciplinary

³⁸Burawoy, "Methods."

³⁹Skocpol, Theda. "Social History and Historical Sociology: Contrasts and Complementarities." Social Science History v. 11, no. 1 (Spring 1987): 17-30.

⁴⁰A.S. Eisenstadt, "American History and Social Science," in Eisenstadt, ed., The Craft of American History, II (New York, 1966, 110-125.

history. Charles Tilly has noted that a "hidden" piece of history "roots most sociology in the present." And that "piece of history" is the piece that sociologists are currently living. For almost forty years sociology has lived in an eternal present, free from both its own "history" and, until quite recently, from many practitioners of historical sociology. Because of this, unfortunately, its historical practitioners have little sense of the way that they have retained links with the traditions they think they have superseded. And for now, at least, they patrol the tracks zealously, apparently worried that interpretive sappers may blow them up.⁴¹

There is no logical reason to expect disciplinary convergence either pre- or post-derailment, nor is it clear that the human sciences would be improved if they did converge. But the flesh and blood evidence of a room full of track jumpers, switchpersons, and hoboes like those at this conference reminds us that anything is possible.

⁴¹Tilly, As Sociology Meets History, 214.

Table 1

Authors Cited 9 or More Times in Works of Urban History

Leo Schnore*	36
Robert Park*	28
Otis Duncan*	26
Louis Wirth	25
Lewis Mumford	18
Gideon Sjoberg	18
W. Lloyd Warner*	17
Adna Weber	17
Seymour Lipset*	16
Herbert Gans*	15
Robert Dahl	14
Robert and Helen Lynd	14
Brian Berry*	12
Amos Hawley	12
Max Weber	12
Robert Merton	11
Ernest Burgess	10
Stanley Lieberman	10
Homer Hoyt	9
C. Wright Mills	9

*Author or co-author

Table 2

Authors Cited 9 or More Times in Essays on Historical Sociology

Charles Tilly	23
Theda Skocpol	17
Anthony Giddens	15
Arthur Stinchcombe	14
Randall Collins	10
Neil J. Smelser	9

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