Emerging Agendas and Recurrent Strategies in Historical Sociology

THEDA SKOCPOL

Master agendas for historical sociology were first set back when Tocqueville, Marx, Durkheim, and Weber asked important questions and offered such fruitful, if varying, answers about the social origins and effects of the European industrial and democratic revolutions. During the twentieth century, the major scholars discussed in the essays collected here have been at the forefront of those carrying forward the traditions of historical sociology launched by the founders. At moments, to be sure, these men may have seemed rather isolated bearers of modes of scholarship that most sociologists considered part of the honored past rather than the vital present and future of the discipline. By now, however, it is clear enough that the stream of historical sociology has deepened into a river and spread out into eddies running through all parts of the sociological enterprise.

Until the 1970s, “historical sociology” was not a phrase one often, if ever, heard in conversations among sociologists in the United States. Of course, major works of comparative history by the likes of Bendix, Eisenstadt, and Moore were widely known and respected. But these works were thought to be peculiar accomplishments. Only unusually cosmopolitan older men, operating in relative isolation from the mainstreams of empirical research in the discipline, were considered capable of producing such major historical works, while ordinary sociologists used quantitative or field-work techniques to study specialized aspects of present-day societies.

Then, from the mid-1970s onward, remarkable changes occurred. Partly these changes were due to the efforts of prominent institution builders like Charles Tilly and Immanuel Wallerstein. As I suggested in the introduction, they were also connected to shifting sensibilities about meaningful scholarship within and beyond academia, sensibilities that revived long-standing historical orientations in sociology. Younger scholars increasingly posed historical questions and used historical evidence and modes of reasoning in their doctoral dissertations. Yearly sessions at the annual meetings of the American Sociological Association were given over to Historical Sociology or to Historical Methods. Many topical sessions, especially those on such macroscopic issues as the sociology of development or labor markets or the growth of the welfare states, began regularly to include historical papers. Graduate and undergraduate courses with historical labels or contents proliferated, and departments across the United States sought faculty members in comparative and historical sociology. Finally, even the major journals in the discipline opened their pages to historical articles by sociologists. By the mid-1980s, in short, historical sociology is no longer exclusively the province of the odd, if honored, grand older men of the discipline. Students and rising young sociologists, even women and middle-Americans, can and do make modest or major contributions to sociology through historical genres of research. Nowadays, historical questions or methods are the stuff of which conferences, courses, and sessions are made, and they orient the efforts of organized research groups as well as those of lone scholars in library studies.

Perhaps the surest sign that historical sociology is in a period not only of growth but of renewal lies in the changes one can see when the research agendas and methods of contemporary historical sociologists are compared to those of the founders of sociology. Where the traditional questions about the roots and consequences of the European Industrial Revolution, the rise of the working class, and the bureaucratization of states and democratization of politics are still being investigated, they are being pursued with more telling evidence and methods of analysis than those deployed by the founders. Excellent examples that come to mind are Jere Cohen’s reexamination of Weber’s thesis about rational capitalism through a close look at economic practices in Renaissance Italy; Mark Traugott’s reexamination of Marx and Engels’s assertions about class and political conflicts in the French Revolution of 1848; Jack Goldstone’s careful probing of the demographic and institutional preconditions for the English Revolution considered in comparative perspective; Victoria Bonnell’s meticulous study of the roots of rebellion among Russian workers in the early twentieth century; Mary Fulbrook’s comparative historical analysis of the contributions made by Puritan and Pietist religious movements to struggles for and against absolutist monarchies in Prussia, Wurttemberg, and
Emerging Agendas and Recurrent Strategies

The United States in historical or comparative-historical perspective. They thus provide the kind of full contextual basis for the better understanding of current American social relations and political events that C. Wright Mills advocated in *The Sociological Imagination* and attempted to provide in his own historical studies of class and power in the United States. Were Mills still alive today, he would have much more reason for optimism about American sociology's historical imagination than he did in 1959. At that point, only a few sociologists, including Lipset and Bendix along with Mills himself, were placing American patterns in truly historical and comparative frameworks.

Of course, historical sociologists today are only part of a growing interdisciplinary community of historically oriented social scientists. The rise of historical work in sociology has happened in tandem with complementary developments in political science and anthropology, and it has come in a period when many scholars in the venerable and slowly changing discipline of history are unusually open to methods and theories from the various social sciences. Historical orientations in the discipline of sociology have their own logic and contents, not always parallel to developments in other disciplines. That is why historical sociology deserves attention in its own right. Yet historical sociology certainly blends at its edges into economic and social history, and completely melds in one of its prime areas, political sociology, with the endeavors of scholars who happen to be political scientists by (original or adopted) disciplinary affiliation. Understood as an ongoing tradition of research into the nature and effects of large-scale structures and long-term processes of change, historical sociology becomes, in fact, a trans-disciplinary set of endeavors that simply has always had one important center of gravity within the academic discipline of sociology.

Is Historical Sociology a Subfield?

Within sociology itself, historical sociology is not-and in my view, should not become-a subfield or self-contained specialty. To the glory as well as the despair of the discipline, sociologists have always been remarkably eclectic in the problems they choose to investigate, the research methods they use, and the styles of argument they develop. Today, historical orientations are on the rise in all of these aspects of the sociological enterprise, but in no instance are they necessarily exclusive orientations. Sociological research on historical problems, for example, may be research about past times and places, or it may be research on processes of change over time leading into and flowing
through the present. In the realm of actual research practice, moreover, sociologists may borrow archival methods from historians, or they may use historians’ works as “secondary sources” of evidence. Yet such historical techniques and evidence can readily be combined with other methods of gathering and analyzing evidence about the social world.

In fact, quantitative techniques traditionally identified with nonhistorical sociological research have been reworked to become relevant for the analysis of temporal, processes. Even more than has occurred so far, quantitative and qualitative approaches could be creatively combined in research. Through quantitative and qualitative modes of analysis alike, sociological theorizing can become more sensitive to sequences over time and to alternative historical paths, without giving up long-standing concerns to explain the patterns and effects of social structures and group action in potentially generalizable terms.

Whenever a set of scholarly activities expands as remarkably as historical sociology has done since the 1970s, a sudden premium is placed on characterizing and classifying the phenomenon so it can be taught and properly pigeonholed in various institutional settings. Those historical sociologists who are currently attempting to link contemporary historical sociology closely to the epistemological, theoretical, and methodological legacies of Max Weber may be attempting a narrow and defensive approach toward carving out a secure place for historical sociology.

A recent article by Charles Ragin and David Zaret, “Theory and Method in Comparative Research: Two Strategies,” cogently exemplifies this strategy. In effect, Ragin and Zaret concede most of what has traditionally been understood as the sociological enterprise—the search for general explanatory variables, in large part through quantitative analysis—of a Durkheimian approach that they present as inherently antihistorical. In sharp opposition to this Durkheimian perspective, they define a Weberian approach dedicated to exploring the particular features of historical cases with the aid of ideal-type concepts. Ragin and Zaret downplay the ways in which quantitative methods can be adapted to the analysis of processes over time, or to the analysis of complex configurations of causes that may account in generalizable ways for particular cases. They drive a wedge between historical methods and others. They end up trying to force all practitioners of comparative history, from Reinhard Bendix and Perry Anderson to Barrington Moore and myself, into a single Weberian camp, ignoring the important differences between those who use comparisons essentially to sharpen particularistic descriptions and those who use them to explore or establish causal generalizations.

Surely it is a mistake to tie historical sociology down to any one epistemological, theoretical, or methodological orientation. Such an attempt fails to do justice to the variety of approaches used by the nine major scholars discussed in this book. And it certainly fails to capture the current variety of historically oriented research proceeding in (and around the edges of) sociology. Both Charles Tilly and Immanuel Wallerstein have taken a different approach from Ragin and Zaret. In effect, both have refused to dally with defining historical sociology and have, instead, simply set wide-ranging agendas for research and theorizing about important substantive concerns. Using qualitative and quantitative approaches alike, Tilly and his students and co-workers have focused especially on describing and explaining historically changing forms of collective action in modern European history. Wallerstein and his adherents have taken a more theoretically oriented approach—positing a capitalist world system with certain structures and dynamics—and then doing many kinds of studies on a huge variety of times, places, and problems to demonstrate the cogency of the new perspective.

Beyond the research agendas set by Tilly and Wallerstein, we have also seen indications of the rich array of topics currently being addressed by historically oriented sociologists. Anyone who looks into the methods and ideas used by these many scholars will immediately perceive variety and fruitful eclecticism. Obviously, when substantive problems and perspectives, rather than preconceived epistemologies or methodologies, define the substance of historical sociology, research and arguments are free to develop in a variety of styles. Research strategies in historical sociology quite properly reflect all of the diversities, disagreements, and dilemmas that have always marked sociology and the social sciences as a whole. At the same time, historical questions and answers are left free to challenge nonhistorical approaches wherever they may be found in sociology. Intellectual competition can remain open, and historically oriented sociologists can gain ground wherever their ideas and research can do a better job than alternatives in accounting for the patterns and dynamics of social life.

Do we conclude, therefore, that nothing useful can be said about research strategies in historical sociology, broadly conceived? In fact, particular choices about research designs and techniques must always be made by individual scholars or groups of researchers who are addressing given problems in the light of specific concepts, theories, or hypotheses. There are no mechanical recipes for proper methods of historical sociology. Nevertheless, surveying the entire range of histori-
Theda Skocpol

cally oriented sociological work, one can devise a "map" of alternative strategies for research and writing that have been, and are likely to continue to be, chosen. Such a map cannot provide methodological dicta for any given investigation. But it can sensitize both practitioners and audiences of historical sociology to the purposes, the advantages, and the disadvantages of alternative approaches.

I take for granted that sociologists always do historically oriented research with some sort of explicit theoretical or conceptual interests in mind." Given this relatively neutral premise, one can readily identify three major strategies for bringing history and theoretical ideas to bear on one another. Some historical sociologists apply a single theoretical model to one or more of many possible instances covered by the model. Other historical sociologists want to discover causal regularities that account for specifically defined historical processes or outcomes, and explore alternative hypotheses to achieve that end. Still other historical sociologists, who tend to be skeptical of the value of general models or causal hypotheses, use concepts to develop what might best be called meaningful historical interpretations. Each of these strategies may be applied to a single historical case or to two or more cases through comparative historical investigations.**

The three major strategies are not hermetically sealed from one another; creative combinations are and always have been practiced. Still, many studies regularly cluster around each major strategy, and the strategies recur despite variations in the kinds of problems historical sociologists address, the precise ways in which they gather and analyze evidence, and the content of the theoretical ideas they bring to bear upon these problems. With the aid of the examples of published scholarship arrayed on Figure 11.1, let me now flesh out these assertions and explore some of the strengths and weaknesses of each of these major practical strategies within the full range of historically oriented sociological scholarship.

Applying a General Model to History

Back in the 1950s and 1960s, when sociology was comfortably- and imperiallyistically! - assumed to be a discipline capable of formulating a universally applicable general theory of society, and history was condescendingly assumed by sociologists to be a collection of archival researchers devoted to gathering "the facts" about particular times and places in the past, the application of a general model to one or more historical instances was the kind of historical sociology most likely to

Emerging Agendas and Recurrent Strategies

Figure 11.1. Research Strategies in Historical Sociology: The Uses of Theory, Concepts, and Comparisons.

<table>
<thead>
<tr>
<th>Aply a General Model to Explain Historical Instances</th>
<th>Use Concepts to Develop a Meaningful Historical Interpretation</th>
<th>Analyze Causal Regularities in History</th>
</tr>
</thead>
<tbody>
<tr>
<td>Single Case</td>
<td>Erikson</td>
<td>Thompson Starr</td>
</tr>
<tr>
<td>Smelser</td>
<td>Bendix</td>
<td>Geertz</td>
</tr>
<tr>
<td>Skocpol book</td>
<td>Moore</td>
<td>Skocpol article</td>
</tr>
</tbody>
</table>

be recognized as empirically rigorous and theoretically relevant in mainstream disciplinary circles. A leading example of this approach is Neil Smelser's *Social Change in the Industrial Revolution*, a major structural functionalist work of historical sociology published in 1959 and appropriately subtitled *An Application of Theory to the British Cotton Industry."

The theory applied in Smelser's book is a supposedly universally relevant model of the logical sequences through which any and all evolutionary changes involving societal differentiation could be expected to proceed. This model is elaborated by Smelser in the form of elaborate sets of "empty theoretical boxes," which he then proceeds to "fill" and "refill" with two sets of facts from nineteenth-century British history: first, facts about changes in the structure of the cotton industry as a set of economic enterprises, and then facts about changes in the lives and activities of workers in the cotton industry. Precisely speaking, therefore, *Social Change in the Industrial Revolution* is a work of comparative history, in the sense that the same general model is successively applied to two analytically distinct (albeit empirically interconnected) cases of social differentiation. Smelser, however, is not interested in comparing his two sequences of change directly to one another. Nor does he present his application of theory to British history as anything more than incidental to his overall theoretical purpose. His structural functionalist theory of evolutionary differentiation could, in principle, be applied equally well to an infinite array of other instances across times and places.

362

363
Another example, this one published in the mid-1960s, further illuminates the intentions characteristic of historical sociologists who apply general models to history. Kai Erikson's appealingly written book, Wayward Puritans: A Study in the Sociology of Deviance, begins by elaborating a Durkheimian model of how any community might define and regulate deviant behavior. Then it uses the Puritan community of Massachusetts Bay in the 1600s as a setting in which to examine several major ideas about deviant behavior derived from the Durkheimian model. Erikson acknowledges that he personally had an intrinsic interest in the historical case he chose to explore, and indeed he (like Smelser with the British Industrial Revolution) investigates his historical case by drawing on primary records much as a social historian might do. Nevertheless, Erikson stresses that his study "should be viewed as sociological rather than historical," and he offers a perfect statement of the logic of our first genre of historical sociology in support of this characterization:

The data gathered here have not been gathered in order to throw new light on the Puritan community in New England but to add something to our understanding of deviant behavior in general, and thus the Puritan experience in America has been treated in these pages as an example of human life everywhere. Whether or not the approach taken here is plausible... will eventually depend on the extent to which it helps explain the behavior of peoples at other moments in time, and not just the particular subjects of this study...  

While Durkheimian and, especially, Parsonian structural functionalist ideas lend themselves with special force to this genre of historical sociology, very different kinds of theoretical ideas can also form the basis for general models to be applied to cases treated simply as one or more among many possible historical instances to which the model could be applied. No more determined critic of Smelser's views could be found, for example, than Michael Schwartz, who draws his arguments about subordinate classes and their experiences and behavior from Karl Marx, Nikolai Lenin, Mao Tse-tung, and Robert Michels. Yet, following a strategy of analysis that closely resembles Smelser's approach, Schwartz's book Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy, 1880-1890 elaborates a general model of the processes by which radical protest movements develop, and either succeed or fail in overturning an established power structure. Then Schwartz applies the model to the historical example of the Southern Farmers' Alliance, which arose at the end of the nineteenth century in the United States to challenge oligarchies of cotton planters and merchants.
issue, how are we to know that any two investigators would concretize them in the same way? Could some arbitrarily selected historical facts perhaps always be found to illustrate any conceivable general model? How do we know that the sociologist applying his or her favored model is not leaving out important facts that might tell against the model? Such questions are especially likely to arise when books or articles in this genre of historical sociology have a very high ratio of general theoretical elaboration to analytic presentation of concrete sequences of historical events. Especially to historians, the entire exercise can seem like a highly uneaesthetic imposition of sociological jargon onto arbitrarily selected and arranged historical facts. Complaints along this line have certainly been directed against the Smelser book.29 In contrast, books like those of Schwartz and especially Erikson, which spend much more space on description and reconstruction of historical events tied down to particular places and conjunctures, may arouse less criticism of this sort. Nevertheless, they could in principle be just as subject to charges of tailoring historical presentations to fit a preconceived theory.

Working within the confines of their genre, historically oriented sociologists who apply general models have moved in two diametrically opposite directions to break out of the trap of appearing to apply a theory to arbitrarily selected cases and facts one solution used by the evolutionary theorist Gerhard Lenski is, in his own words, to "apply a general model to the universe of all known historical (including-graphic) instances."30 This approach has the advantage of avoiding the charge that cases are selected to fit the theory while others are ignored. The disadvantage, however, is that the investigator is driven so far away from intrinsic interest in any particular cases that the label "historical sociology" hardly seems appropriate for this kind of scholarship.3

A contrasting approach to modeling history is nicely exemplified by David Willer's attempt to use elementary formal models of social relationships and social conflicts to explore the adequacy of existing historical interpretations of the processes that led to the fall of the Roman Empire in the West. Willer does not try to capture the entire historical case, in all of its complexity, in one pregiven model. Instead, he probes existing historical arguments about the case at selected, strategic points. His aim is simply to see if the processes being posited hold up in terms of his formal models, which have themselves been tested in controlled experimental situations. The results are merely suggestive and, as Willer himself emphasizes, do not substitute for more comprehensive arguments about the Roman case. Still, Willer's study does suggest useful tactics to sociologists who want to apply general models to historical instances.

Yet if problems of perceived arbitrariness can plague works in this first genre of historical sociology, would-be appliers of general models do not often back off to the more selective and partial tactics exemplified by Willer's study. More often, they combine the application of a general model with one of the other two major strategies of historical sociology to be discussed here. In their book The Rebellious Century, for example, Charles, Louise, and Richard Tilly apply a general "political conflict" model to account for the patterns of violent collective conflict in France, Italy, and Germany between 1830 and 1930.33 They make their application of that model much more convincing by systematically confronting the historical patterns of each national history not only with causal hypotheses derived from their own preferred model, but also with causal hypotheses derived from a rival Durkheimian model that has often been used by laypeople and sociological theorists alike to account for collective violence. Interestingly enough, in a late chapter of Social Change in the Industrial Revolution, Smelser briefly engages in a similar strategy. He contrasts his approach to explaining working-class unrest in mid nineteenth-century Britain with hypotheses derived from Marxist or classical-economic premises. Not incidentally, perhaps, these passages provide some of the most vivid and convincing reading in Smelser's otherwise cumbersome book.34

An alternative strategy for shoring up the plausibility of a general model-applied to history is best exemplified by the work of one of the major historical sociologists analyzed in the body of this book. Immanuel Wallerstein's Modern World-System, as we have seen, applies a model of world capitalism to the last five hundred years of world history. Complementary models -- "world empire" and "mini-system" and "world socialism" -- are also presented to cover all other, previous or subsequent, possibilities in world history. Wallerstein's enterprise, however, cannot be considered simply one of applying a general theory to history. He also offers a meaningful world view imbued with the political perspectives of Third World and American radical critics of the world capitalist system. As Ragn and Chirot stress, the plausible appeal of Wallerstein's approach depends very much on its resonance with the political sensibilities of many younger social scientists.35

In these concluding remarks about alternative ways in which the application of a general model to history can be made more plausible, I
have assumed that the next two major genres of historical sociology to be discussed—using concepts to develop meaningful historical interpretations and exploring alternative hypotheses about causal regularities in history—typically deploy stronger rhetorical tactics than this first genre for convincing audiences that a plausible set of arguments is being presented. The reasons why this may be true should become more apparent as we now explore each of these other approaches in its own right.

Using Concepts to Interpret History

A second major strategy regularly employed by historical sociologists is one that uses concepts to develop meaningful interpretations of broad historical patterns. In some ways, this strategy can be considered a self-conscious critical response to the efforts made by structural functionalists, Marxists, and many others to apply putatively very general theoretical models to history. Works by Reinhard Bendix and E. P. Thompson exemplify our second strategy, and we have already seen how thoroughly the methods and contents of their scholarship have been shaped in reactions against the overgeneralizing and determinist tendencies they perceive in structural functionalism and in economic readings of Marxism. Beyond its possible genesis in such critical responses, however, the strategy of using concepts to develop meaningful interpretations of historical patterns is a positive approach in its own right. As shown by Paul Starr’s The Social Transformation of American Medicine, this is a strategy of research and rhetorical presentation that may be used straightforwardly, and not primarily in polemical opposition to arguments offered by general model builders.36

Interpretive historical sociologists—the label I want to give practitioners of this second strategy—are skeptical of the usefulness of either applying theoretical models to history or using a hypothesis-testing approach to establish causal generalizations about large-scale structures and patterns of change. Instead, these scholars seek meaningful interpretations of history, in two intertwined senses of the word meaningful. First, careful attention is paid to the culturally embedded intentions of individual or group actors in the given historical settings under investigation. Second, both the topic chosen for historical study and the kinds of arguments developed about it should be culturally or politically “significant” in the present; that is, significant to the audiences, always larger than specialized academic audiences, addressed by the published works of interpretive historical sociologists.

Although interpretive historical sociologists are implicitly or explicitly skeptical of what passes for theory among scientifically oriented students of society and history, they certainly are not themselves antitheoretical. On the contrary, they pay careful attention to matters of conceptual reorientation and conceptual clarification, and they always use explicit concepts of some generality to define their topical concerns and to guide the selection and presentation of historical patterns from one or more case studies. For example, E. P. Thompson’s The Making of the English Working Class puts forward (in polemical opposition to economic determinist views) a concept of class as “an historical phenomenon,” an active process which owes as much as to agency as to conditioning.”37 and then uses this concept to order selected narratives of events in early nineteenth-century British history. And Paul Starr’s The Social Transformation of American Medicine reworks Weberian notions of authority and specifically dramatizes a conception of “cultural authority” to set the stage for a lengthy account of the rise of the American medical profession to a position of great prestige, power, and wealth.38

Similarly, Reinhard Bendix’s major books in comparative political history, Nation-Building and Citizenship and Kings or People, do not simply plunge into historical narratives of each national case. First, Bendix draws themes and specifies concepts from the works of Max Weber, Otto Hintze, and Alexis de Tocqueville to direct his readers’ attention to the questions about political authority and the varying patterns of political institutions that he chooses to discuss in the various cases he covers. Because Bendix’s books are all comparative rather than single-case studies, he deploys his orienting concepts in two ways. First, like Thompson and Starr, he uses some of them—especially those posed as themes basic to organized political life in sets of polities of a certain type—to orient the narratives of events and patterns in each of his case studies. In addition, though, Bendix uses some concepts as benchmarks to establish the particular features of each case, either through contrasts of case patterns to a general concept or through contrasts of that case to others in terms of how it handles a certain basic issue (such as legitimating the authority of a king).

Indeed, whenever interpretive historical sociologists do comparative historical studies, rather than simply conceptually structured presentations of single histories, they use comparisons for the specific purpose of highlighting the particular features of each individual case. Comparative studies, according to Reinhard Bendix, increase the visibility of one structure by contrasting it with another. Thus European feudalism can be more sharply defined
by comparison, say, with Japanese feudalism, [and] the significance of the Church in Western civilization can be seen more clearly by contrast with civilizations in which a comparable clerical orientation did not develop.40

Elsewhere, Bendix further elaborates this way of using historical comparisons:

By means of comparative analysis I want to preserve a sense of historical particularity as far as I can, while still comparing different countries. Rather than aim at broader generalizations and lose that sense, I ask the same or at least similar questions of divergent materials and so leave room for divergent answers. I want to make more transparent the divergence among structures of authority and among the ways in which societies have responded to the challenges implicit in the civilizational accomplishments of other countries.41

Because interpretive historical sociologists use comparisons to highlight the particular features of each case, they are likely to choose cases for inclusion in their studies that will maximize the possibilities drawing dramatic contrasts. If, like Bendix, they cover a range of cases, they will most frequently invoke the extremes, such as England versus Russia, in their comparative arguments. If, as often happens, they discuss only a pair of cases, they will select them according to the logic succinctly illustrated by Clifford Geertz’s little book, Islam Observed. In a first chapter tellingly entitled “Two Countries, Two Cultures,” Geertz tells us why, among the many possibilities, he selected Indonesia and Morocco for his study of religious development in modernizing Islamic countries:

Their most obvious likeness is . . . their religious affiliation; but it is also, culturally speaking at least, their most obvious unlikeness. They stand at the eastern and western extremities of the narrow band of classical Islamic civilization which, rising in Arabia, reached out along the midline of the Old World to connect them, and, so located, they have participated in the history of that civilization in quite different ways, to quite different degrees, and with quite different results. They both incline toward Mecca, but, the antipodes of the Muslim world, they bow in different directions.”42

For Geertz Indonesia and Morocco are so promising to compare precisely because, through the sharp contrast they offer within Islam, “they form a kind of commentary on one another’s character.”43 His choice of cases, along with his rationale for the choice, perfectly reflects the distinctive purpose for which interpretive historical sociologists use comparative history. The aim is to clarify particularities through contrasts, not to show the repeated applicability of a theoretical model as in the first genre of history sociology just discussed, and not to test or develop causal generalizations, as in the third strategy, to be presented subsequently.

If done well, interpretive works can be the most compelling contributions of any genre in historical sociology—certainly the most compelling for broad audiences that stretch beyond academia. The reasons why are simple. First, graceful writing can be deployed to fullest advantage in this genre. Orienting concepts can be presented briefly, and much of the argument can proceed through the common-sense device of narrative storytelling. There is no need to move from a highly abstract model to historical specifications that may appear arbitrary or artificially ripped out of context; nor do flows of description need to be repeatedly broken to examine alternative causal hypotheses. Second, works in this genre necessarily tap into vivid contemporary sensibilities, intellectual trends, and assumptions about how the world works. Interpretive works deliberately stress relevance to the meaningful world views of their intended audiences, whether they be establishment audiences (as for the Bendix and Starr books) or politically oppositional audiences (as for E. P. Thompson’s book).

Finally, too, both single-case studies and comparative studies in this genre stress the portrayal of given times and places in much of their rich complexity, and they pay attention to the orientations of the actors as well as to the institutional and cultural contexts in which they operate. Consequently, interpretive works can seem extraordinarily vivid and full, like a good Flaubert novel. Of course, the whole story can never be told in any work of history or historical sociology. But interpretive works can convey the impression of fullness much more readily than works of historical sociology that aim to apply models or establish causal connections of relevance to more than one case.

From certain philosophical points of view, the kinds of understanding of social history that interpretive works seek to convey represent the most desirable, and perhaps the only really feasible, kind of knowledge available through historical sociology.44 It follows that interpretive works can only be judged to be more or less successful at meeting the challenge they set for themselves: finding the most compelling conceptual lenses through which to mediate between meaningful happenings in the past and the concerns of present day audiences. From the perspectives of social scientists concerned with (any degree of) general theoretical knowledge about regularities in social structures...
and processes, however, interpretive historical sociologists can almost always be faulted for their insouciance about establishing valid explanatory-arguments. Both the concepts deployed by interpretive historical sociologists and the descriptive narratives on which they rely so heavily assert or imply all sorts of causal connections. Yet these histori- cal sociologists are not concerned to establish explanations that hold good across more than single cases. From the perspectives of those concerned with causal validity, therefore, interpretive works can be misleading even when they are compelling.

The danger is probably greatest for single-case studies in the inter- pretive genre. Comparative histories, especially wide-ranging works such as those of Reinhard Bendix, are likely to display inconsistent causal assertions and missed opportunities for exploring causal regularities in ways visible to any astute reader (as Dietrich Rueschemeyer’s reflections on Bendix demonstrate). For single-case explorations such as those of Thompson and Starr, however, a critic needs to call to mind potential new comparative cases to begin to perceive such causal inade- quacies or missed opportunities.

Interestingly, for each of these examples one can wonder how the arguments presented for England or the United States might have held up if either author had extended his tentative causal assertions to the other nation. Recent work by Ira Katznelson suggests that E. P. Thompson might have developed a less cultural and more political argument about the structures, conjunctures, and activities that “made” the English working class distinctive, if only he had been willing to make careful comparisons to the United States and Western Europe. Like those of the other two major genres of historical sociology presented here, interpretive historical investigations can certainly be synthesized with elements of the alternative strategies. I have already argued that Wallerstein’s world-system perspective combines the application of a general theoretical model to history with the development of a politically meaningful historical interpretation. Appliers of general models, I suggested, may find it helpful to make the kinds of appeals to current audience sensibilities that interpretive studies routinely embody. From the interpretive side, Perry Anderson’s Passages from Antiquity to Feudalism and Lineages of the Absolutist State supplement comparative historical arguments devoted to highlighting particular historical trajectories with the application of a Marxist theory about the logic of long-run sociopolitical change to one historical lineage deemed more dynamic, progressive, and globally relevant than all other historical lineages. Anderson’s pair of books could thus be considered an attempt to fuse the application of a general model with a primarily interpretive and particularizing study. But this kind of effort is unusual. Interpretive historical sociologists find it more congenial to move toward cautiously testing alternative hypotheses, since the application of truly general theoretical models violates their sense of historical particularity and variety.

Alvin Gouldner’s “Stalinism: A Study of Internal Colonialism” is an excellent example of an interpretive case study that also moves in the direction of turning its favored interpretation into a cross-nationally testable causal hypothesis. Most of Gouldner’s essay is taken up with discussion of how Stalinism can be most meaningfully conceptualized, followed by a narrative presentation of the drama of Soviet history from the 1920s through the 1930s in terms of the “internal colonialism” conception that Gouldner favors. Briefly, however, at the very end of his essay, Gouldner considers whether this interpretation can also ac-
Analyzing Causal Regularities in History

Practitioners of a third major strategy of historical sociology proceed differently from either interpretive historical sociologists or those who apply a general model to one or more historical cases. Here, as exemplified by some of the most important works of NM and Barrington Moore, the focus is on developing an adequate explanation for a well-defined outcome or pattern in history. Neither the logic of a single overarching model nor the meaningful exploration of the complex particularities of each singular time and place takes priority. Instead, the investigator assumes that causal regularities—of limited scope—may be found in history. He or she moves back and forth between aspects of historical cases and alternative hypotheses that may help to account for those regularities.

Emerging Agendas and Recurrent Strategies

Ideas about causal regularities may come from two or more preexisting theories that are brought into confrontation with the historical evidence. Or they may be generated more inductively from the discovery of what Arthur Stinchcombe calls “causally significant analogies between instances” during the course of a historical investigation. The crucial point is that no effort is made to analyze historical facts according to a preconceived general model. Alternative hypotheses are always explored or generated. Ideas from apparently-opposed theoretical paradigms may be combined, if that seems the most fruitful way to address the historical problem at hand. Or old theories may be entirely set aside, and a new explanation tentatively generated from the historical materials. The investigator’s commitment is not to any existing theory or theories, but to the discovery of concrete causal configurations adequate to account for important historical patterns.

Indeed, in this analytic genre of historical sociology, research always addresses a clearly posed historical question. Where, how, and why did peasant-based revolts against the French Revolution occur, and what light can the answers shed on the general issue of collective protests in modernizing contexts?–as Charles Tilly asks in The Vendée. Why did some commercializing agrarian monarchies end up as democracies and others as fascist or communist dictatorships?–as Barrington Moore asks in Social Origins of Dictatorship and Democracy. What accounts for the similar causes and outcomes of the French, Russian, and Chinese Revolutions, and why did episodes of political crisis and conflict in other modernizing agrarian states not proceed in the same way?–as I ask in States and Social Revolutions. Why did some regions of Europe experience the decline of serfdom and the emergence of capitalist agriculture, while others did not?–as Robert Brenner asks in Agrarian Class Structure and Economic Development in Pre-Industrial Europe. Why were the nineteenth-century Chinese unusually resistant to buying foreign commodities?–as Gary Hamilton asks in The Chinese Consumption of Foreign Commodities: A Comparative Analysis.
Comparative studies have a very different purpose for analytic historical sociologists than for interpretive historical sociologists. The latter, as we have seen, use comparisons to make contrasts among cases to highlight the features particular to each individual historical context. For analytic historical sociologists, differences among cases are also interesting—no less so than similarities. Yet these scholars examine the variations of history with the intention of establishing causal regularities, quite a different aim than that of their interpretive counterparts. To understand this difference, listen first to interpretive historical sociologist Reinhard Bendix, and then to analytic historical sociologist Barrington Moore, on the purposes of comparative history. According to Bendix, macroscopic comparisons have no role in establishing causal inferences, for such comparisons should be used only to contrast socio-historical contexts to one another:

"Comparative analysis should sharpen our understanding of the contexts in which more detailed causal inferences can be drawn. Without a knowledge of contexts, causal inference may pretend to a level of generality to which it is not entitled. On the other hand, comparative studies should not attempt to replace causal analysis, because they can only deal with a few cases and cannot easily isolate the variables (as causal analysis must)."

Barrington Moore offers a very different perspective:

Comparisons can serve as a rough negative check on accepted historical explanations. And a comparative approach may lead to new-historical generalizations. In practice these features constitute a single intellectual process and make such a study more than a disparate collection of interesting cases. For example, after noticing that Indian peasants have suffered in a material way just about as much as Chinese peasants during the nineteenth and twentieth centuries without generating a massive revolutionary movement, one begins to wonder about traditional explanations of what took place in both societies and becomes alert to factors affecting peasant outbreaks in other countries, in the hope of discerning general causes. Or after learning about the disastrous consequences for democracy of a coalition between agrarian and industrial elites in nineteenth- and early twentieth-century Germany—the much discussed marriage of iron and rye—one wonders why a similar marriage between iron and cotton did not prevent the Civil War in the United States; and so one has taken a step toward specifying configurations favorable and unfavorable to the establishment of modern Western democracy.

In this excerpt from Moore's Preface to Social Origins of Dictatorship and Democracy, one notices much the same suspicion of overly generalized theories as that which suffuses the scholarship of Reinhard Ben-
As Moore puts it, “too strong a devotion to theory always carries the danger that one may overemphasize the facts that fit a theory beyond their importance in the history of individual countries.” Yet Moore obviously cares more than Bendix about establishing causal generalizations, and unlike Bendix he believes that historical comparisons can be used both to test the validity of existing theoretical hypotheses and to develop new causal generalizations to replace invalidated ones.

The flavor of the intellectual operation is effectively conveyed in the excerpted passage. Rather than contrasting whole histories in terms of pregiven concepts or themes, as interpretive historical sociologists do in their comparative studies, analytic historical sociologists like Moore think in terms of alternative hypotheses and comparisons across relevant aspects of the historical cases being compared. They thus try to specify in somewhat generalizable terms the “configurations favorable and unfavorable” to the kinds of outcomes they are trying to explain in their cases.

Research designs used in such comparative historical analyses share with other methodological approaches in the social sciences the aim of establishing controls over variation to distinguish valid from invalid causes. In contrast to the probabilistic techniques of statistical analysis—techniques that are used when there are very large numbers of cases and continuously quantified variables to analyze—comparative historical analyses proceed through logical juxtapositions of aspects of small numbers of cases. They attempt to identify invariant causal configurations that necessarily (rather than probably) combine to account for outcomes of interest. As originally outlined by John Stuart Mill in *A System of Logic*, comparative historical analyses can be done according to either of two basic research designs diagramed in Figure 11.2, or through a combination of them.

Using the approach Mill labeled the “method of agreement,” a comparative historical analysis can try to establish that several cases sharing the phenomenon to be explained also have in common the hypothesized causal factors, even though they vary in other ways that might seem causally relevant according to alternative hypotheses. Or, using the approach Mill called the “method of difference,” a comparative historical analysis can contrast cases in which the phenomenon to be explained and the hypothesized causes are present to other (“negative”) cases, in which the phenomenon and the causes are absent, even though those negative cases are as similar as possible to the “positive” cases in other respects. Taken alone, this second approach is more powerful for establishing valid causal associations than the method of agreement used alone. Sometimes, however, it is possible to combine the two methods by using several positive cases along with suitable negative cases as contrasts.

A monumental work of comparative historical analysis, Barrington Moore’s *Social Origins of Dictatorship and Democracy* primarily uses the method of agreement, yet also argues at times along the lines of the method of difference. With the aid of causal configurations referring to the strength of commercial bourgeoisies in relation to landlords, to modes of agricultural commercialization, and to the rebellious potential of different types of peasant communities and peasant/landlord relationships, Moore seeks to explain why the seven major agrarian states he compares traveled one or another of three alternative routes leading to democracy, Fascist dictatorship, or Communist dictatorship. Within each of his routes, Moore primarily argues along the lines of the method of agreement; each route has two or three nations about whose development Moore makes a similar causal argument, at times using the individual features or the differences of the cases to eliminate possible alternative arguments about the roots of democracy, fascism, or communism. Simultaneously, Moore makes some use of the method...
of difference at the level of comparisons across his three major routes. As he discusses countries within each route, Moore occasionally refers to relevant aspects of the histories of countries in one or both of the other routes, using their contrasting directions of development at similar junctures to help validate the causal argument he is currently making. Not only in terms of its substantive scope, therefore, but also in terms of the complexity of its explanatory design, Social Origins of Dictatorship and Democracy is a work of virtually unparalleled ambition.

My own book, States and Social Revolutions, is much less ambitious than Moore’s masterpiece. Yet, especially in its first part, “The Causes of Social Revolutions in France, Russia, and China,” it also employs a combination of Mill’s basic analytic approaches. I argue that, despite differences along many dimensions that certain theorists of revolution would consider decisive, Bourbon France in the late eighteenth century, Imperial China after 1911, and Czarist Russia from March 1917 all experienced social revolutionary crises because similar sets of causes came together. By thus stressing causal similarities in the face of other important differences, I reason according to the method of agreement. I also use the logic of the method of difference by introducing analytically focused contrasts between France, Russia, and China, on the one hand, and relevant moments and aspects of the histories of England, Prussia/Germany, and Japan, on the other. These other countries are suitable controls because, even at moments of revolutionary crises, they did not undergo successful social-revolutionary transformations, despite important structural and historical similarities to France, Russia, and China.

Contrasts to different sets of countries at relevant moments in their histories help to validate each specific part of the overall argument about France, Russia, and China. For causal arguments about crises in the relationships of states to landed upper classes or the agrarian economy as one configuration favoring social-revolutionary crises, I draw contrasts to the Japanese Meiji Restoration and the Prussian Reform Movement. For arguments about the contributions of certain kinds of agrarian structures and peasant revolts to social revolutions, I make contrasts to the English Parliamentary Revolution and the (failed) German revolutions of 1848-50. In States and Social Revolutions, the control cases are discussed much more briefly than Russia, China, and Japan. They are introduced not for the purpose of fully explaining their own patterns of political conflict and development, but instead for the particular purpose of strengthening the main line of argument about social revolutions in the three major cases.

Comparative historical analyses presented in article-length pieces, as opposed to books, can often range with greater flexibility across cases, especially when it comes to using relevant comparisons to call competing causal arguments into question. Two examples, both of which emphasize the method of difference in their research designs, nicely illustrate this point.

Robert Brenner’s article “Agrarian Class Structure and Economic Development in Pre-Industrial Europe” seeks to explain long-term economic change in late medieval and early modern Europe, in particular “the intensification of serfdom in Eastern Europe in relation to its process of decline in the West” and “the rise of agrarian capitalism and the growth of agricultural productivity in England in relation to their failure in France.” Determined to debunk explanations of European economic growth that attribute it to market expansion or demographic trends, Brenner undermines such arguments by showing that similar market and demographic processes were associated with markedly different outcomes of economic development between Eastern and Western Europe, and also among regions within each of these broad zones. Then Brenner proceeds to argue that variables referring to class relations and the strength of peasant communities versus landlords can better account for the variations in economic development he wants to explain.

In his article “Chinese Consumption of Foreign Commodities,” Gary Hamilton is concerned to sort out the factors influencing the use of Western commodities by people in non-Western civilizations. The unwillingness of the nineteenth-century Chinese to buy many Western textile products provides an especially intriguing concrete problem through which to address this broad issue. Why the Chinese reluctance? Hamilton suggests at the outset three alternative lines of explanation: faulty marketing and merchandising arguments; cultural explanations; and a Weberian “status-competition” hypothesis. Proceeding methodically, Hamilton makes ingenious use of comparisons across time and space to dispose of the first two explanations: Economic arguments cannot explain why China differed from certain other non-Western countries in the nineteenth century; and references to Confucian cultural values cannot explain why Chinese in earlier historical periods were willing to consume foreign products. Finally, Hamilton demonstrates that his preferred status-competition explanation can account for the temporal and cross-national variations that its competitors could not. All in all, therefore, Hamilton is able to make optimal use of comparative history as a tool of causal analysis, above all because he
ranges freely across countries and epochs to find the logically necessary comparisons to develop his explanatory argument.

Because wide-ranging comparisons are so often crucial for analytic historical sociologists, they are more likely to use secondary sources of evidence than those who apply models to, or develop interpretations of, single cases. Secondary sources are simply published books and articles by historians or by scholars specializing in the study of one geocultural area of the world. Some people believe that such publications are automatically inferior to primary sources, the original residues of the past that most historians use as their basic sources of evidence about given times, places, and issues. From the point of view of historical sociology, however, a dogmatic insistence on redoing primary research for every investigation would be disastrous; it would rule out most comparative-historical research. If a topic is too big for purely primary research-and if excellent studies by specialists are already available in some profusion-secondary sources are appropriate as the basic source of evidence for a given study. Using them is not different from survey analysts reworking the results of previous surveys rather than asking all questions anew, or students of comparative ethnography synthesizing results from many different published field studies.

This said, however, it remains true that comparative historical sociologists have not so far worked out clear, consensual rules and procedures for the valid use of secondary sources as evidence. Certain principles are likely to emerge as such rules are developed. Comparative historical sociologists who use secondary sources must, for example, pay careful attention to varying historiographical interpretations, both among contemporary historians and across scholarly generations of historians. The questions that the comparative historical sociologist needs to ask about every one of the cases included in his or her study may not correspond to the currently fashionable questions historians are asking about any given case. Thus, the comparativist must be very systematic in searching through historical literatures to find evidence for and against the hypotheses being explored. Perhaps the evidence will be embedded in minor corners of publications, or in the work of an “odd” historian out of tune with dominant historiographical trends. Above all, the historical sociologist cannot let his or her findings be dictated simply by historiographical fashions that vary from case to case or time to time.

Secondary research can also be strategically supplemented by carefully selected primary investigations or reinvestigations, and I suspect that comparative historical sociologists will increasingly converge on the practice of starting with secondary analyses, but not stopping there. Targeted primary investigations can be especially useful for answering questions relevant from a comparative perspective that historical specialists have simply not pursued to date. In addition, comparative historical sociologists are well advised to familiarize themselves with at least some of the primary evidence on which the secondary sources have built conclusions. Such a practice may reconfirm confidence in the findings of the specialists. Alternatively, it may call particular secondary sources into question or open up the possibility for the comparative historical sociologist to build new findings out of primary sources previously inadequately analyzed.

Good comparative historical sociologists nevertheless must resist the temptation to disappear forever into the primary evidence about each case. Marc Bloch once made a statement that could be taken as a maxim for comparative history when done by analytic historical sociologists: “The unity of place is merely disorderly;” Bloch declared. “Only a unified problem constitutes a central focus.” Analytic historical sociologists take this point very seriously, especially when they do comparative history. The temptation to narrate unbroken sequences of events, or to cover everything about a given time and place, is resisted. Instead, aspects of cases are highlighted according to the causal configurations currently under discussion. From the point of view of interpretive historical sociologists (and traditional historians), good analytic comparative history may seem rather unaesthetic. The unities of time and place must be broken for the purposes of drawing comparisons and testing hypotheses.

When analytic comparative historians sit down to write their books or articles, they face special challenges of integrating descriptive accounts for various cases with discussions of alternative hypotheses and with the coherent pursuit of the overall argument. Historical trajectories cannot simply be juxtaposed and contrasted, as in interpretive works of comparative history. Instead, the best approximations to controlled comparisons must be explicitly presented to carry off the logic of the analysis. Thus, effectively organized writings in this genre of historical sociology are difficult to prepare. When they are produced, however, they can rival interpretive works in rhetorical persuasiveness, not for sheer aesthetic reasons but through the force of an explanatory argument put forward as more able than plausible competitors to answer a dramatically posed historical question.

Because my own work in historical sociology falls within analytic historical sociology, it will come as no surprise to readers that I con-
sider it the most promising strategy of the three I have discussed. Analytic historical sociology, I believe, can effectively combine the concern to address significant historically embedded problems—a concern that most of its practitioners share with interpretive historical sociologists—without ongoing efforts to build better general social theories, a concern shared with those who have applied general models to history. Analytic sociology can avoid the extremes of particularizing versus universalizing that limit the usefulness and appeal of the other two approaches.

Nevertheless, there are pitfalls and limits to the effectiveness of analytic historical sociology, especially in its strongest guise of comparative historical analysis. The search for appropriate controls to meet the logical requisites of comparative designs can become a dry and mechanical business, especially since the historical record does not always oblige in providing relevant comparative instances. Perhaps more serious, the assumption that independent units can be found for use in comparative assessments of causal regularities may be unfounded. This is especially likely to be the case if meaningful cultural wholes, or single systemic entities like a “world capitalist division of labor,” are at issue. Immanuel Wallerstein, readers will recall, resists using comparative historical analysis precisely because he does not consider its logic applicable to partial and variously situated units (such as nations) within a capitalist world economy.

Even when they are more or less successfully accomplished, comparative historical analyses aimed at validating causal regularities in history cannot ever substitute for theoretical models or conceptual lenses in offering a meaningful portrait of how the world works. Obviously, some theoretical ideas always need to be used to set up the terms of a comparative historical investigation, even if an honestly even-handed effort is made to examine alternative hypotheses in the course of the investigation. In addition, when comparative historical analyses are completed and written up, they are often introduced and concluded with arguments that partake of the flavor of general model building or the provision of a meaningful view of the world. Charles Tilly’s works invoke the tantalizing promise of general model building to convince readers that hypothesis-testing studies of French (and Western European) patterns of collective action offer a window toward a possibly much more widely applicable sociological theory. As Dennis Smith has argued, Barrington Moore’s Social Origins of Dictatorship and Democracy relies on the taken-for-granted significance of “democracy” versus “dictatorship” in its sorting of the world’s major polities into alternative, teleologically defined routes of long-term social and political development. Much of the power of the book’s causal arguments comes from the reader’s willingness to accept the alternative political routes of democracy, fascism, and communism at face value.

More than most social researchers, major historical sociologists end up with a hankering to develop grand maps of history. With considerable admiration, Charles Tilly has recently labeled these grand maps “encompassing comparisons.” Much less approvingly Arthur Stinchcombe calls them “epochal interpretations.” Analytic historical sociology as I have presented it does not in itself provide the wherewithal for creating such grand maps. So perhaps it should not be surprising that the most ambitious of comparative historical analysts end up borrowing emphases from our first two strategies of historical sociology to help them frame their questions and results in more encompassing or epochal ways.

In the final analysis, the theoretical skepticism that I have presented as intrinsically characteristic of good analytic historical sociology is simply a practical strategy for research and the presentation of arguments. Yet, for both the individual scholar and the community of historical sociologists, it is a practical strategy of immense value. This strategy of research cannot ultimately displace basic epistemological and substantive choices or render grand theories and meaningful world views superfluous. But using this research strategy makes possible lively debates about the regularities to be found in history and about the specific usefulness—or lack of it—of alternative theories and concepts for formulating valid causal arguments about those regularities.

The practice of analytic historical sociology forces a more intimate dialogue with historical evidence than either interpretive historical sociology or the application of a model to a historical case. However untenably in some strict philosophical sense, analytic historical sociology holds forth the possibility of constructing better social theories in a manner Arthur Stinchcombe has captured in a compelling metaphor: The analytic historical sociologist builds “as a carpenter builds, adjusting the measurements as he [or she] goes along, rather than as an architect builds, drawing first and building later.”

Ours is an era when no existing macrosociological theory seems adequate, yet when the need for valid knowledge of social structures and transformations has never been greater. Analytic historical sociology allows sociologists to move toward better theories through a full and detailed confrontation with the dynamic variety of history. Important questions about social structures and change can be continually raised