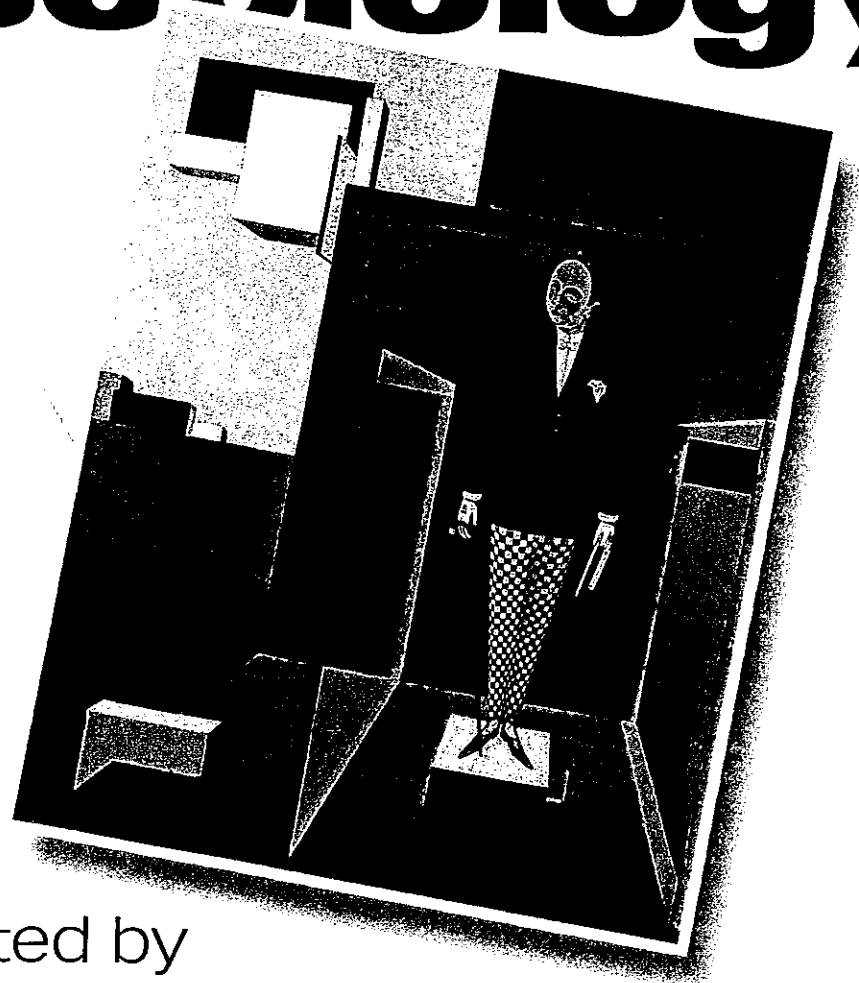


Handbook of
**historical
sociology**



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SAGE

Afterword

Why Historical Sociology?

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There are many reasons to study historical sociology – or to study history sociologically. For some scholars, engagement grows from wondering about a specific set of events, from doubts about a generalization or conclusion offered by an earlier author, or from the seeming analogy – or contrast – between present circumstances and an earlier era. For some sociologists, the prestige of association with the older discipline of history is an attraction, just as for some historians the converse prestige of association with the scientifically reputable ideas of theory and quantitative methods offers motivation. None of these reasons need be seen as disreputable, yet none fully explains the importance of bringing history and sociology together.

The most compelling reason for the existence of historical sociology is embarrassingly obvious (embarrassingly because so often ignored). This is the importance of studying social change. If it is remarkable that much sociology focuses on some combination of an illusory present and an even more illusory set of universal laws, it is still more remarkable that much history focuses not on crucial patterns, processes, trajectories and cases of social change but on aspects of the past divorced from their location in the course or context of social change. Historians study what life was like in the fifteenth century, what firms were like in the nineteenth century, the biographies of prime ministers, the texts of Chinese sages and the relics of Mesopotamia. These concerns are of interest

for an understanding of what life was like in other contexts, but also for thinking about social change. Curiously, though, it has often been the case that the 'discipline' of professional, academic history has urged a disengagement from the study of social change as somehow too broad a subject. As one Oxford don told me when I embarked on my D.Phil. thesis, a study originally conceived as addressing popular protest between the 1790s and 1830s, 'That's a ridiculous time span; you must focus, say on 1811–12.' As it happened, I did focus further, but my emphasis remained on social change during the Industrial Revolution and popular collective responses to it. This may seem a predictable 'sociological' choice. Yet very often sociologists doing historical research have manifested their own disengagement from history. They have brought contemporary sociological methods and theories to studies of past times, but approached these as simply further cases of phenomena they might see anywhere, or as 'snapshots' out of time. Though such studies can be interesting, they also invite criticisms like that of John Goldthorpe (1991), who suggests that in so far as sociologists seek to develop deeper knowledge of social processes, they would do better to conceptualize new studies of present phenomena about which they can gather more complete data.

Of course, not all historians have adopted an approach narrowed to brief time-spans or efforts simply to mine particular archives. The extent to which historians aim merely at the

discovery of new facts or at the reinterpretation of known ones, or even at the construction of causal models, is variable. There is oscillation and counterpoint between the intense study of specific moments and the study of how patterns of continuity and change are constituted – as, for example, in the recent French turn to *'microhistoire'* from *'histoire de la longue durée'*. Some subfields have been much more likely to take up the broader study of patterns in social change: economic history and intellectual history, for example, as in the magisterial and analytic studies of Joel Mokyr and Reinhart Koselleck. Art history has generally been more open to the long term and the study of social change, music history more heavily influenced by 'positivists' engaged in the recovery of scores or the preparation of authoritative texts of opera libretti. So too, national traditions vary, with the Anglo-American including perhaps the strongest current of 'positivism' (though German immigrants helped to bring this to America), and especially of positivism defined as a preference for factual discovery over theory or interpretation (Novick, 1988). And even within Britain and the United States, there are differences between historians of each of the 'home' countries and those focusing on European or non-Western histories.

A second compelling reason for historical sociology is closely related. It is a way of dispelling the illusions of false necessity. Along with comparison, attention to historical specificity is one of the crucial ways of demonstrating that what happens to be is not what must be. Sociology and history each have their own versions of empiricist false necessity. Sociologists too often try to build theory by generalizing from the results of empirical research, rather than organizing research to provide evidence or contradiction for propositions that might explain patterns in the distribution of empirical phenomena. They too often turn the relationships of causal implication in multivariate models into implications of necessity in the world – as though chance and human action and shifting contexts could not change the relationships among variables. This has the (often criticized) effect of exaggerating the extent to which present-day social arrangements are necessary (and thus beyond criticism) as distinct from being the results of power relations or failure to pursue alternatives. Seeing the present in relation to the past is an important way of recognizing its contingency, and pressing oneself to attend

not simply to surface phenomena but also to underlying causes and conditions that produce those phenomena. It is arguably his contribution to this analytic project that most legitimates treating Marx as the first great historical sociologist (see Duncan Kelly's chapter in this volume).

If sociologists get caught in illusions of the present as necessity, historians can be trapped by the limits of their own empirical data if not aided by theory, comparison and other means of transcending the mere record. This record can appear as one of a necessary succession of moments, an inexorable march towards the present (or for some Marxists, towards the future). If fears of Whig history lead many to avoid broad generalizations about the course of history, the opposite extreme is to extract specifics about the past from any concern for historical change. At the same time, the reality of life in another era can appear to be only those things which actually happened and chanced to be recorded, and not the aspirations that animated life and the struggles which might have gone otherwise. Historians are less likely to deny the centrality of interpretation to their work than sociologists are, but history has undergone its own transformations in pursuit of an ideal of empiricist science, from Ranke through more recent versions of professional specialization (see Novick, 1988).¹

Still a third compelling reason for historical sociology is the need to grasp analytic categories in the historical contexts of their production and application. There is no access to past or present or reality in general save through categories of thought which are themselves historical products and results of conscious or unconscious choice, of social as well as individual selection, of struggles both to understand and to dominate, and never more than relatively adequate to the pursuit of knowledge. Do we speak of nations? Of classes? Of crises? East and West? Colonies and post-colonies? Subjectivities? Individuals? Gender distinct from sex? Religion, when religions differ from as well as resemble each other – not just in particulars but also in fundamental understandings of what they are? There is no escape from such categories of thought into an ideal realm of pure facticity (perhaps the last idealist dream of many materialists). Some are better than others for particular explanatory projects, all go in and out of fashion, yet none are simply right or wrong. Each may be used casually, with little

attention to what it conveys, how it shapes understanding, or what the significance of alternatives foregone might be. Each may also be the object of inquiry, with not merely its lineage but also its implications at issue, and each understood in terms not just of scholarly choice of concepts but also of the manifold social factors that contribute to the production and reproduction of ways of understanding (a point made by several authors in the present book, including, notably, Peter Wagner and Johann Arnason).

Such categories of thought are not merely more or less adequate and accurate, they are also constitutive of social reality. The idea of nation, therefore, and with it the idea that the world is more or less naturally organized into nations (even if some are historical victors and others losers), has been constitutive for the very organization of history as a scholarly discipline. This has not been altogether divorced from the role of history as a public project – indeed often a national project, an education into citizenship and particular forms of identity. And the category of nation is part of the historical and sociological constitution of the object ‘nations’ not only in the attention of scholars but also in the consciousness of ordinary people and the actions of armies and ambassadors (see relevant discussions in these pages by Chakrabarty and Delanty, among others). The categories with which we think the world are part of a social imaginary that in some degree also makes that world real and makes it what it is (see Calhoun, 2002; Taylor, 2002). To use categories of thought in an appropriately self-aware and critical fashion requires attention to both theory and history, and to a sociological, not merely an individually intellectual, understanding of both the past and the present dynamics shaping the use and implications of such categories.

In short, history and sociology both need historical sociology. It is, among other things, an indispensable help to each in counteracting the baneful effects of the *Methodenstreit*. This German argument over method, science and the nature of knowledge flourished over a hundred years ago, but left an enduringly problematic heritage.² If anything, its effects are more profound in America than in Germany, and I think more substantial in German- and English-language scholarship than in most Latinate and other scholarly traditions (though I cannot pretend to know them all, and though Durkheim and others were engaged in partially similar struggles over

disciplinary identity and the relationship between theory and history). The protagonists of the *Methodenstreit* propagated a misleading dichotomy between particularizing and generalizing sciences. Not only was the opposition of particularizing to generalizing itself overdrawn, the idea that whole sciences – or divisions of the faculties of universities – should be characterized simply by one approach or the other is both empirically wrong and epistemologically specious. It is certainly true that disciplines, or lines of work within disciplines, may be characterized by greater attention to detailed description or specification of particular phenomena or general explanation of recurrent phenomena. The differences are more stylistic than fundamental, though, and the middle ground between the extremes is vast. This is especially the case if ‘generalization’ is elevated to the positing of universal laws.³

Very few historians are really interested simply in the radically particular, though they may often be interested in variations among cases that never match each other exactly, or in sequences of events grasped well by narrative interpretation partly because they involve so many different influences and dimensions that it is impossible to isolate each for more general study. Rather than identifying history with the ‘idiographic’, it may make more sense to identify it with the ideals of specificity and context. There is, in E.P. Thompson’s (1971) words, a ‘discipline of historical context’ that encourages seeing the ways in which events and understanding and actions and even structures are embedded both in other simultaneous phenomena and in time. The former aspect encourages connecting history to ethnography, as Thompson noted, though the latter is a limit to traditional ethnography and a reason for historical anthropology as well as historical sociology.⁴

Likewise, universal and invariant phenomena or relationships among variables occupy a relatively small place in sociological knowledge. Relationships of ‘weak implication’ (to borrow Boudon’s [1975] phrase) are more common in statistical research than strict causality. Sociology is concerned mostly with patterns of similarity and difference and with partially general accounts of these. Much of it consists in putting more or less general explanations to work in accounting for particular cases. But the general explanations are usually partial. That is, they explain aspects and dimensions of phenomena, not the whole of

interesting cases. Producing an adequate account of the state of social inequality in the United States, or the pattern of migration in Africa, or the implications of population policies in China (let alone changes in any of these) is a matter of bringing together multiple different more or less general theories, as well as interpreting facts specific to individual cases or analogies and disanalogies across cases. On the one hand, this means breaking down cases into narrower phenomena the generalizable aspects of which are more readily specified.⁵ On the other hand, it means also reassembling the partial explanations into an account of the larger whole which occasioned sociological interest in the first place.⁶ The partial explanations will usually vary greatly in their generality and robustness, and generalizability of the whole package will be rare. Indeed, part of the importance of historical and comparative sociology lies in overcoming faulty generalizations. It does this not by eschewing generalization, but by continually putting generalizations to the test in new contexts, not just to see whether they hold, but also to see whether the very terms in which they are framed continue to make sense. As Pavla Miller says in this volume, 'The very strength of historical sociology helps undermine the apparent universality of its conceptual tools.'

Historical sociology, then, stands between the idiographic and the nomothetic in both history and sociology. As it happens, the label is more commonly used among sociologists, though the practice clearly includes historians (as Peter Burke and others discuss in this volume). Indeed, one of the oddities of labelling is that ideas of 'social' and 'cultural' are often used to label opposing approaches in history, and social history is further divided between a 'social science' variant that is often quantitative and a more interpretative version. The label signals, moreover, a concern for distinguishing the realms of social relations and everyday life, especially of 'ordinary people', from high politics and elite culture. Witness the slogan of the *Journal of Social History*: 'history from the bottom up'. In the 1960s and early 1970s, this could join a quantitative approach to demography, urbanization, migration and other 'mass' phenomena – that is, phenomena involving large numbers of people considered in the aggregate rather than as individual actors – with recovery of working-class biographies, studies of family life and reconstruction of experiences like penal

transportation.⁷ Historians working on the latter sort of projects, however, were apt to take 'cultural' and 'linguistic' turns in the course of the last quarter of the twentieth century that divided them from advocates of 'social science history'. At the same, the cultural and linguistic turns often involved a new centrality for intellectual history, and in some cases a consequent shift away from the centrality of 'ordinary people' to an increasing emphasis on major thinkers and shifts in text-based 'discourses'.

Historical sociology has also had a curious existence as a subfield of sociology. As Holton notes in this volume, 'historical sociology has been seen by many as a subset rather than a core feature of the discipline, on a par with *industrial*, *political* and other such specialisms'. He rightly shows this to be at odds with Max Weber's approach to sociology as inherently historical in its very questions and in the overall intellectual approach it requires. Many historical sociologists share Weber's sense of the importance of historical (and comparative) inquiry for the discipline as a whole (whether or not the substantive theory they adopt is specifically Weberian). But after a brief period in which historical sociology seemed more central and more radical, its institutionalization has indeed made it appear mostly as one specialization among others. Most often this is described as a methodological approach, as though historical inquiry was a method analogous to survey research or ethnography. It is thus curious that organizationally – as in departmental hiring decisions, assessment exercises and the division of the American Sociological Association into sections – it should appear as one in a list of mostly topical specialisms. This reflects, however, a historical trajectory (and, of course, one can hope that books like this one are part of a reversal in the trend).

Historical sociology is in fact as old as any other sort of sociology. When Comte coined the word 'sociology' in the 1840s, and certainly when Durkheim launched his discipline-forming project in the 1890s, sociology was already in part a project of historical analysis. Vico, Montesquieu, Ferguson and Tocqueville all figure even before Marx in this dimension of sociology's history – and its history as historical sociology. Formative problems for the new discipline mandated historical inquiry: How was industrialization transforming society? Did revolution offer human beings control over their history? Was

community vanishing? Could all human societies be ordered in evolutionary sequence? Was there a distinctly Western trajectory or civilization? Was inequality declining or growing? Did population growth inevitably result in the 'corrections' of famine, warfare and pestilence?

Behind many of these questions lay sociology's struggle to understand European modernity in various of its aspects ranging from capitalism to a growing popular voice in politics, from bureaucracy to the apparent decline of community in favour of a more impersonal urban society. This involved looking at earlier periods and at processes of social change as well as at non-European sites. Historical sociology was inescapably central. It was marginalized and turned into a subfield mainly after the Second World War.

Two lingering effects of the *Methodenstreit* were influential. One was the subsequent rise to dominance of other research approaches that focused more on the pursuit of universal laws. Often now grouped together as quantification, these involved considerable advances in the ability to produce knowledge of certain kinds. This was interpreted in the terms of the *Methodenstreit* as bringing a more secure and potentially universal form of knowledge. This interpretation was not necessarily entailed by quantification as such, and, moreover, there are important epistemic differences masked by that term (or others such as 'behavioural').⁸ Second was a division of the faculties along the lines of the *Methodenstreit* opposition. This reflected a conflict not only over generality and particularity, but also over 'positive' knowledge and interpretation, and – importantly – over whether moral values were to be addressed directly or kept at arm's length (in ideally 'value-free' inquiry). No single international pattern of university organization resulted, though more or less similar distinctions were widespread. In the United States, where this organizational factor was perhaps most influential, the basic distinction was between the humanities and natural sciences, with the social sciences in between (but for the most part clearly opting for a self-understanding as science).⁹ In Germany and most of Northern Europe the *Geisteswissenschaften* occupied a similar contested space. This led to an eventual distinction of a tradition of cultural and usually hermeneutically oriented scholarship from increasingly positivist approaches emulating natural science. Within the former, cultural sociology long survived as a

separate branch of sociology with chairs located in a different division of the university.

An equally important marginalization of historical sociology came from a different quarter. This was the canonization of 'classical social theory' accomplished by Talcott Parsons and others, which fixed the historical concerns of the founding sociologists as theory rather than as themes for continuing research. Sociologists began to absorb their views of history – and especially of the key dimensions of modernity – from reading Weber and Durkheim rather than studying history directly, either in primary source research or in study of the writings of historians. History became a backdrop to sociology, invoked as part of theory more than the object of research. For decades (and perhaps still) there was more exegesis of Weber's discussion of the Protestant Reformation than sociological engagement with new research on the Reformation. The version of theory for which this was most true was that which aimed most to be all-encompassing and systematic.¹⁰ But it parsons betrayed Weber in sacrificing much of his historical side, the same could be said of the much less systematic tradition of symbolic interactionism with regard to its founder, George Herbert Mead. After the generation of Mead's own students, few symbolic interactionists read beyond his socio-psychological lectures (collected as *Mind, Self, and Society*, 1934) to his books on nineteenth-century thought or other themes. Mead's appropriation into a tradition centred on participant observation is itself curious, given his own interest in experiment, as well as historical scholarship. But the point is the way in which the theoretical canon both displaced historical inquiry for contemporary concept formation and systematization and substituted transmission of the historical analyses of the founders for new historical sociology.

One of the results of this was that historical sociology would re-emerge as a challenge to dominant orthodoxies in the discipline, commonly supported by Marxism or a rereading of Weber. It is worth recalling the extent to which the revitalization of historical sociology in the 1970s grew out of the politics of the 1960s, and the sense that grand theory as it then existed wasn't adequately answering the grand questions of the contemporary world. It was not that all historical inquiry had ceased. Weberians like Reinhard Bendix had kept a more historical version of that tradition alive. Some trained largely in the functionalist

tradition, like Neil Smelser, Philip Selznick, Robert Bellah and Seymour Martin Lipset, did important historical research. Robert Merton's functionalism did not simply replicate that of Parsons. Not least, Merton affirmed a greater space for historical inquiry (and more generally placed a greater emphasis on integrating theory and empirical research). Indeed at an early stage of his career, Merton (1938) did important historical research on early modern science that owed little to functionalism. From other sources, Barrington Moore wrote the enormously influential *Social Origins of Dictatorship and Democracy* (1966), while George Homans pursued a double life as historian of medieval England and behaviourist sociologist. Homans's student, Charles Tilly, in turn began producing important historical sociology before the more widespread revival, after which he would go on to become one of the most influential of all late twentieth-century historical sociologists.¹¹

In Europe too, important historical sociology was produced before the 'revival' of the field. Probably most influential was Norbert Elias, though the influence came three or four decades after his main work (1978 [1939], 1982 [1939]), which reflected really the intellectual orientations of the prewar period. Jürgen Habermas's early *Structural Transformation of the Public Sphere* (1989 [1962]) was a work of historical sociology, though most of his later work followed a less historical, more philosophically universalist path. Bryan Wilson's work on magic and religion (1973) was historical sociology without a formal declaration. Joseph and Olive Banks (1964) did distinguished work on themes like family planning. At the fringes of disciplinary identity, Lucien Goldmann produced a historical sociology of literature and culture.

From the history side of the interdisciplinary field, there was a substantial move to take up the mixtures of theory and research and conceptually if not always disciplinarily sociological agendas well before the 'revival' in sociology. This was especially strong where Marxist scholarship was influential. In Britain, E.P. Thompson, Christopher Hill, Eric Hobsbawm, George Rudé and others did work they (at least the first two) would cringe to hear called historical sociology but which none the less deserves the label in many ways. Keith Thomas (1971) acknowledged the disciplinary influence of anthropology rather than sociology, but joined this group. Also influential were the boundary-crossing

projects of the *Annales* school (see Peter Burke's chapter above). This influenced sociology directly through Immanuel Wallerstein, but also offered a counterpoint to the dominance of structuralism and inspired important studies (often anthropological rather than sociological) of the history of slavery, colonialism and peasant societies.

The founding of the Social Science History Association and of the journal *Comparative Studies in Society and History* both revealed stirrings of a sense of need to connect history and social science, specific research agendas and broader theoretical concerns. They were American-based, and the stirrings may have been strongest there partly because the earlier divisions also were. At the same time, though, a revitalization of evolutionary theory in the United States of the 1960s (which sometimes had connections to historical research) fell afoul of the rebellion against modernization theory and developmentalism but found greater reception in Europe.¹²

None the less, it is important to see not only the continuity of historical sociology but also the important sense in which it rose to prominence as a part of a sort of social movement in the 1970s and 1980s.¹³ Crucial aspects of this included rebellion against functionalist modernization theory and revitalization of Marxism. If the former was more of an issue in sociology, the latter was at least as important in history. Both were influenced by the political and social concerns of the 1960s. These figured biographically in attracting younger scholars interested in social change, but also observationally as they challenged the notions of a smooth and unilinear path of modernization and of a functionally integrated society in which power, domination and conflict were only aberrations. Some of the senior figures in historical sociology had long challenged the dominant approaches – perhaps most prominently Immanuel Wallerstein.¹⁴ Less an insurgent, but also challenging the dominant paradigm was Charles Tilly¹⁵ (who published Wallerstein's books on the modern world-system in his influential series with Academic Press).¹⁶ But what made historical sociology come to the forefront was not simply the work of a few distinguished individuals in more senior generations but the widespread engagement of a younger generation. With different specific theoretical orientations and coming from different disciplinary trajectories, a variety of scholars still in their twenties as the 1970s started led the broader change: Theda Skocpol, William Sewell and Lynn Hunt were

among the most prominent (see Hunt, 1984; Sewell, 1980; Skocpol, 1978). Perry Anderson and Michael Mann were (slightly older) English counterparts (though this was a disproportionately American movement – see Anderson, 1974a, 1974b; Mann, 1986). They were followed by many more historical sociologists half a generation younger (including me).

What drew these historical sociologists of the 1970s and 1980s together was the challenge of addressing several 'big issues' in social theory. The issues were not all uniquely historical, but historical research on each seemed crucial not just to set the record straight but also for the three reasons I enumerated at the outset. First, grasping the historical pattern of social change was important to clarifying a trajectory that included the present and understanding the options open, the paths closed and the forces at work. Second, the political significance of the renewal lay most basically in challenging the false necessity implied by dominant paradigms, not in any specific course of action or analysis of obstacles. And, third, the categories of analysis themselves needed to be rethought, not just in the abstract, but also in relation to their histories and the ways in which they had constituted dominant versions of history (and social reality). This meant looking anew at class, bringing the state back in and understanding the centrality of gender to the patterns of social organization and chance. This set of motivations, and this conversation, oriented the renewal of historical sociology most importantly to questions about modernity (even when that category was among those challenged). Studies of earlier historical epochs were secondary within it (though eventually these would become more significant).¹⁷

Because this renewal was centred on several big questions, it was inherently theoretical, often a matter of pursuing theoretical issues through historical research. This meant that it could readily engage (shaping and being shaped by) theoretical debates that were not exclusively about history or conducted by means of historical research. Some of the most important of these were organized in terms of Marxism and feminism. This also meant that the renewal of historical sociology could be integrated with new work in comparative sociology, which might have greater or lesser historical depth in itself. World-systems theory arose out of studies of dependency and development (as well as out of the *Annales* school), and these continued to be important.

A project like 'bringing the state back in' joined, among many, Theda Skocpol (whose version of historical sociology was shaped heavily by both Marxism and Barrington Moore), Peter Evans (whose work centred on dependency in Latin America) and Dietrich Rueschmeyer (a broadly Weberian researcher).¹⁸ Not least of all, this established a connection between, on the one hand, the resurgence of actual historical research among sociologists and attention to sociological theory among historians and, on the other hand, a redevelopment and reorientation of social theory itself.

During the 1960s and 1970s, a number of theorists successfully challenged the construction of the canon of classical social theory that Parsons had effected a generation or two earlier. Anthony Giddens was in this regard probably the most influential writer in English. The most obvious manifestation of this was recognition of Marx as one of the discipline's key founding theorists (part of a new trinity with Durkheim and Weber). Interest in Marxism (and in problems of culture) brought new readers to the Frankfurt school, whose theoretical work had always involved an attention to history, to Georg Lukács, and to Antonio Gramsci (whose work, though less directly historical, was readily appropriated into historical sociology because of his interest in definitions of historically specific institutional regimes – like Fordism – and his concern for historical variations in forms of class struggle). In addition, a new set of international theoretical connections and influences, some mediated by Marxist conversations (as in the role of the *New Left Review* and *New Left Books* in publishing translations), brought a new generation of continental European theorists into the forefront of English-language scholarship. Jürgen Habermas continued, but transformed, the Frankfurt school. The 'poststructuralist' generation from France became famous in English slightly later, but had perhaps even more influence. Michel Foucault and Pierre Bourdieu were most important for historical sociology. Even the earlier, more purely 'structuralist' work of Althusser had a significant influence, despite his own disinterest in history (and indeed, empirical research), because of its use by more comparatively and sometimes historically minded scholars, including Nicos Poulantzas, Samir Amin and others. Amin was also part of a larger, French-centred but also Latin American, Middle Eastern and South Asian rethinking of global economic

inequality, pre-capitalist economic formations and the articulation of modes of production (and, indeed, the role of the state). This overlapped and influenced the rise of subaltern studies, described by Dipesh Chakrabarty in this volume.

Like the rest of Anglo-American sociology, historical sociology was relatively slow to incorporate work from (and indeed work on) the rest of the world. This was never absent, of course, but the centring on the European historical experience has been strong. It is primarily as an adjunct to questions about modernity posed in terms of the European experience – including questions about its distinctiveness and about European domination – that Anglo-American historical sociology has taken up non-Western (and, indeed, non-modern, including ancient) history. Subaltern studies is among the most prominent of traditions to have flourished in neighbouring disciplines and regions without yet figuring as much as it might in historical sociology.

Michel Foucault has been the most important French influence on historical sociology after the core *Annales* school. His work is central to a variety of themes, including both substantive concerns about sexuality, medicine, knowledge, power and discipline and methodological-theoretical about continuity and discontinuity in historical change and the historical specificity and embeddedness of categories of knowledge (see Dean's chapter above for one dimension of this influence). While Foucault's most important work dates from the 1960s and 1970s, it was largely incorporated into English-language research in the 1980s and 1990s. It flourished more and earlier among historians than among sociologists; in both cases, it was central to a 'cultural turn'. Much other poststructuralist and postmodernist work also influenced this, of course, and I won't attempt to review it here. What is perhaps most important to grasp, though, is the extent to which the 'cultural turn' among historians drew many into new and different interdisciplinary relationships – with literature and anthropology, especially – and away from the older one with sociology (see Bonnell and Hunt, 1999). This reflected partly a clash of emphases: historians trying to grasp the cultural construction of sexuality or alternative knowledges while sociologists were bringing the state back in. While the political scientists leading the 'bringing the state back in' project were historically

sensitive, it is important to see the extent to which this could remind many historians of the older forms of political history (a history of elites, wars and institutions) against which the rise of the new social history had proposed 'history from the bottom up'. The new cultural history grew largely with the same 'bottom-up' concern, though this did not remain clearly dominant partly because of the large role of intellectual history, the importance of the critique of elite 'knowledges', and the very fact that sources on culture tended to come disproportionately from elites.

This disciplinary disjuncture was (like many) strongest in the United States (partly just because the scale of academia enables subfields more easily to achieve critical mass). Though there was overlap, comparative historical sociology and the sociology of culture grew largely as two distinct subfields within the American Sociological Association – to their detriment. The former enjoyed a major boom and discipline-wide prominence especially in the 1980s, but was slow to absorb the cultural turn (and by the same token did less to counterbalance it in interdisciplinary discourse than it might have). American sociology of culture flourished especially in the 1990s, but was (and remains) largely centred on the contemporary United States. Though British sociology is not free of ethnocentrism, work on culture has been significantly shaped by the problematics of both European integration and international migration and diaspora (both approached at least sometimes with historical depth and specificity). As central as migration has been to American sociology, and indeed to cultural studies in the US, it has not been a core concern for the American sociology of culture.

In both these regards, the situation in Britain is strikingly different. Historical sociology never took off equally as a subfield centred on new empirical research, but it flourished in much closer relationship to social theory and to cultural studies. The revitalization of interest in the work of Norbert Elias is an important example of this, but only one of many. Sociology was central to the birth of British cultural studies – for example, with the Birmingham school. Indeed, strong connections among literary, historical and sociological research were established early (for example, in the work of Raymond Williams, e.g., 1961) and maintained. These fields were more disjunct in the United States. In the US,

sociologists tended to stay on the margins of interdisciplinary cultural studies which developed with much more influence from literary scholars.

In Britain, historical sociology as written by sociologists appears largely in the form of theorization of large-scale historical phenomena: nationalism, wars, class relations, gender, power. If theory and research had to be opposed (which I hope they don't), much of the British writing would look like theory (or synthesis), not research, to Americans. It draws on historical sources, sometimes primary but largely secondary, to try to understand broad patterns of social life. These are approached in ways disciplined and systematized not by the logic of empirical inquiry or by the pursuit of detailed texture of description but rather by theoretical concepts and questions. In the United States, historical sociology appears more commonly as specific empirical studies intended (at least ideally) to bear on larger theoretical questions. The theoretical intentions were often stronger in earlier years, the questions often larger, and both more supported by directly theoretical work. The maturation of the subfield of the discipline has made it less of a challenge to the 'mainstream'. The divergences between British and American styles are unfortunate, not just for the sake of international understanding, but because a richer integration of theory and empirical research would produce stronger studies.

Given that English is a (more or less) common language, and, moreover, a language also in common use in other countries where the division between British and American styles of sociology may be less influential, one may hope for more work that bridges this gap. The same goes for more effective incorporation of attention to culture. And not least of all, the same goes for the integration of the work of historians and sociologists. Indeed, the recent revitalization of 'world history' and more generally of historical research that transcends conventional boundaries of place (usually nation-state) and period suggests that the time is ripe.

Certainly, the three compelling reasons for historical sociology have not been exhausted. Grasping social change continues to demand both empirical interpretation and theoretical explanation. Avoiding false necessity is still aided by both critical theory and comparative historical analysis. Understanding how basic concepts work not only in our theories and analyses but also in the social imaginary calls

for never-ending investigation into the histories of their production and transformation.

NOTES

1. The question of whether history is better understood as science or craft is an old one; see, for example, Carr (1967).
2. See my discussion, in relation to contemporary debates over historical sociology, in Calhoun (1998).
3. There was another approach to the contrast of universal and particular, exemplified by the neo-Kantian formalism of Georg Simmel's sociology. Simmel held that sociology was sharply distinct from history, as form was from content. History encompassed empirical phenomena in their specificity and multiplicity, while sociology developed purely formal concepts, devoid of empirical content and hence in principle universally applicable. But the actual production of knowledge, on this view, had to involve both content and form, empirical specifics and general concepts. This was a brief less for sociology as a separate science than for a clarification of the role of concept formation in knowledge.
4. Indeed, a good deal of interesting and important historical anthropology exists, and is too often ignored by historical sociologists.
5. The recent wave of enthusiasm for identifying and abstracting causal 'mechanisms' is an instance of this. See, for example, Bunge (1999), Elster (1989), Hedström and Swedberg (1998) and Tilly (2001). A key question is whether the emphasis on mechanisms is simply a restatement of Robert K. Merton's (1968) advice to stick to 'middle-range theories' between pure description and grand theoretical systems, or is itself part of a more dogmatic metatheory.
6. It is worth emphasizing, in this connection, that the distinctions universal/particular, whole/part, long duration/short duration and macro/micro are not entirely homologous. 'Macro' claims about large-scale phenomena – say, the French Revolution or European global dominance – may be relatively particular; this is the sense in which I speak of the explanatory 'whole' as a 'package' above. Very 'micro' phenomena may persist over long historical periods or obtain widely in the world.
7. An important question was whether, in the words of Eric Hobsbawm (1971), this sort of social history could be integrated into a 'history of society'. The latter would be not merely more theoretical, but also more attentive to the cumulative processes of social change and the capacities of social actors to shape this change – a theme Hobsbawm worried was being lost in the welter of studies of particulars of everyday life. Hobsbawm's discussion recalls not only Marx but also Adam Ferguson, who wrote on 'the history of civil society'. Both, however, also contributed to distinguishing society as a more or less self-contained system

- of relationships from the kinds of political phenomena – wars and other acts of kings – that had previously been the staple of history..
8. For example, survey research is at the centre of much quantification in sociology, and yet often denigrated by advocates of experimental design.
9. See Julie Reuben's (1996) excellent account of the way struggle over the place of religion and morality in the university figured in this.
10. There were certainly extremely large-scale attempts to be nearly all-encompassing and systematic from more historically minded sociologists – notably Pitirim Sorokin, who is remarkably absent from most accounts of the field (see Sorokin, 1957 – a revised but condensed version of the four volume original). Parsons displaced Sorokin not only at Harvard but also more generally, challenging his approach to a synthesis of historical patterns with his own, mostly ahistorical, approach. It should be said, though, that Parsons was not 'antihistorical'. It was not that he derided the discipline nor suggested that sociologists should eschew historical inquiry. Neil Smelser, perhaps Parsons's favourite student, wrote a major work of historical sociology as his PhD thesis (see Smelser, 1958). It was, rather, that Parsons's theory specifically reduced the place of historical variation in favour of abstraction to putatively universal processes. Smelser's book echoed this in this presentation of an 'empty-box' theory of social change which he filled in with an account of the Industrial Revolution in Britain. There was, though, always a tension between this abstract dimension of Smelser's analysis and his more directly historical research.
11. Among the most important: Hill (1964), Hobsbawm and Rudé (1969), Rudé (1964 – a book considerably influenced by Neil Smelser's sociology) and Thompson (1963).
12. It is Parsons's late turn to evolutionary theory that underwrites Holmwood and O'Malley's otherwise surprising characterization of him in this volume as a historical sociologist. Much more historically engaged evolutionary analysis came from Gerhard Lenski (1966). In Europe, Niklas Luhman carried forward the development of evolutionary theory, dramatically extending Parsons's interest in cybernetic systems theory. Luhman did historical analysis himself (for example, Luhmann, 1982), and influenced that of many others. It remains a curiosity that his work should be so prominent in much of the world and of little influence in the United States.
13. See my discussion in Calhoun (1997).
14. When Wallerstein debated with Alex Inkeles at the 1975 meeting of the American Sociological Association, the audience's overwhelming support for Wallerstein marked the final collapse of the dominant paradigm that had been under challenge for several years. Neither world-systems theory nor any other new paradigm achieved comparable dominance to that previously enjoyed by modernization and functionalism.
15. An anthology (Tilly, 1997) presents several examples of his wide-ranging work (and a valuable review essay by Arthur Stinchcombe).
16. Starting with Wallerstein (1974).
17. Other epochs, other parts of the world and other ways of distinguishing locations in the world all come to the fore with the recent revitalization of 'civilizational' analysis (see Mandaliou and Arnason in this volume). This comes to prominence in sociology especially through the work of Eisenstadt, building on Jaspers, but it has roots in many European analytic traditions. To what extent this is the return of a (reformed) modernization theory is subject to debate, though as Knöbl says in this volume, modernization theory has hardly passed conclusively away.
18. See Evans et al. (1985). This book was the work of a Social Science Research Council committee. An earlier committee on comparative politics had wrapped up its work with the publication, exactly a decade earlier, of Charles Tilly's *The Formation of National States in Western Europe* (1975). Though Tilly was a participant in the new project as well, looking at his work among others, one could wonder whether the state had been left quite so much out of the picture as the new project suggested. Part of the issue was, of course, the extent to which states had been left to political science and marginalized among sociologists. Another part was the shift in perspective reflected by the European (and often Marxian or Weberian) language of state by comparison to 'government' or 'politics' (cf. Poggi).

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