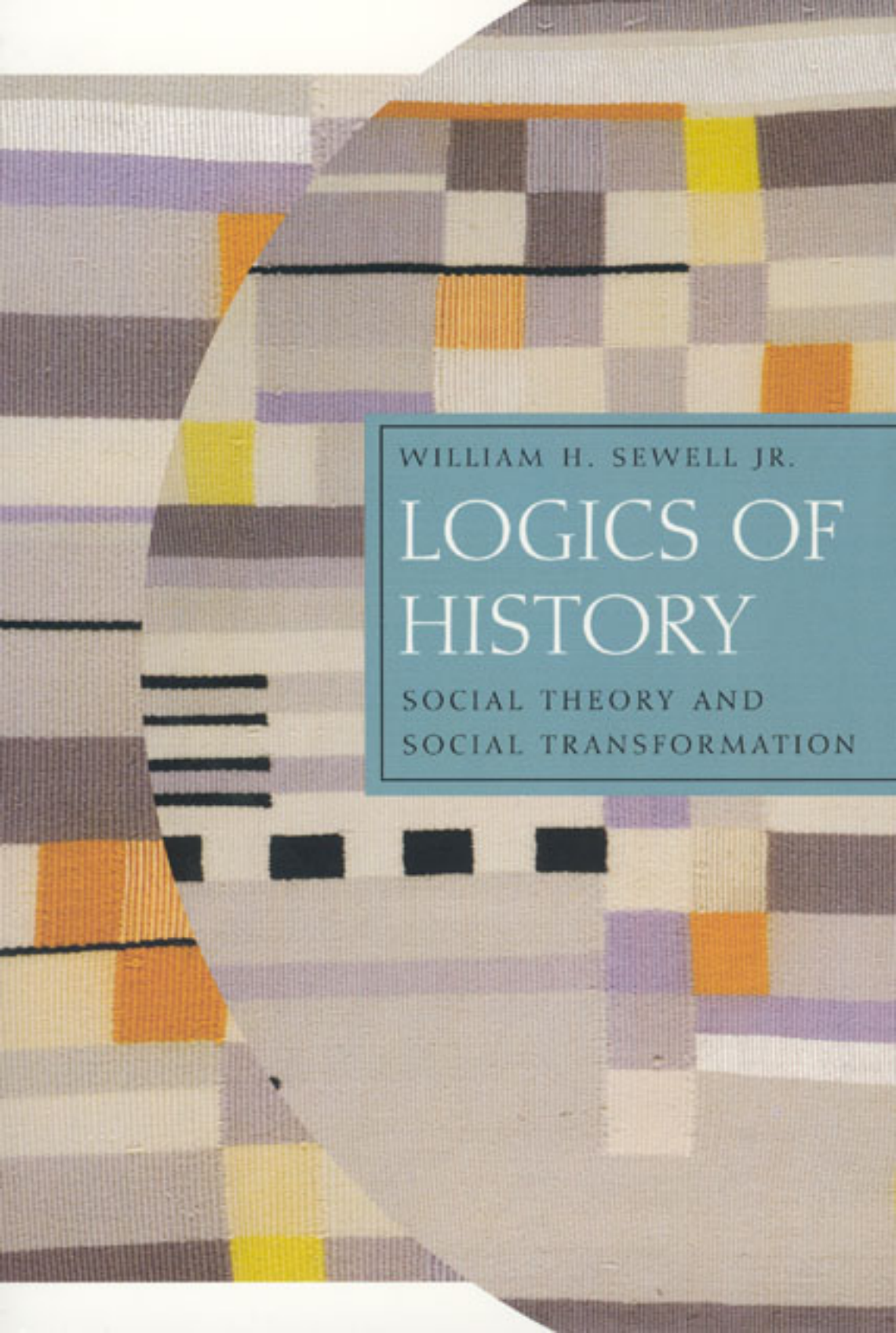
WILLIAM H. SEWELL JR.
LOGICS OF HISTORY
SOCIAL THEORY AND SOCIAL TRANSFORMATION

LOGICS OF HISTORY

CHICAGO STUDIES IN PRACTICES OF MEANING

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SOCIAL THEORY AND SOCIAL TRANSFORMATION

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To my colleagues in

the SOCIAL HISTORY WORKSHOP,

the IAS SOCIAL SCIENCE SEMINAR,

the SEMINAR ON SYMBOLIC ANTHROPOLOGY,

the SEMINAR ON SYMBOLISM AND SOCIAL CHANGE,

the DAVIS CENTER SEMINAR,

the NEH SUMMER SEMINAR ON LABOR HISTORY,

the ARIZONA SOCIOLOGY BROWNBAG,

the MSG-TUCSON,

the MSG-ANN ARBOR,

CRSO,

CSST,

the SEMINAR ON SOCIAL MOVEMENTS AND STATE BUILDING,

the CASBS SEMINAR,

CCP,

the WORKSHOP ON COMPARATIVE POLITICS AND HISTORICAL SOCIOLOGY,

the WORKSHOP ON SOCIAL THEORY,

the MODERN FRANCE WORKSHOP,

the SOCIAL THEORY GROUP,

the MELLON SEMINAR ON CONTENTIOUS POLITICS,

the WILDER HOUSE FACULTY SEMINAR,

the CIAR SUCCESSFUL SOCIETIES PROGRAM,

and CCCT,

whose friendship, conversation, and boundless capacity for critical thought have made this book possible.

CONTENTS

PREFACE

The origins of this book go back to the late 1980s, when I was serving as the first director of the Center for the Study of Social Transformations (CSST) at the University of Michigan. CSST was a university-funded experiment in interdisciplinarity, a collection of historians, anthropologists, and sociologists—as brilliant as they were argumentative—who met regularly to thrash out questions of theory and method. Our discussions were intense and absorbing; they spilled over into lunches, parties, and countless ad hoc seminars in the corridors. The discussions certainly vindicated the founding group's assumption that scholars in these three disciplines had plenty of interests in common and much to learn from each other. They also made it clear that disciplinary divides were very real: disagreements between those hailing from different fields were often sharp and sometimes heated.

It was in the midst of these rather dramatic interdisciplinary debates that the idea of writing this book took shape. At the time, I had a joint appointment in history and sociology and had been reading anthropology and hanging out with anthropologists for years. I was convinced then, and remain convinced today, that a social science combining historians' nuanced sense of social temporalities, anthropologists' recognition of the power and complexity of culture, and sociologists' commitment to explanatory rigor is both possible and necessary. My own particular obsession in our debates was to insist that we recognize the fundamental historicity of all social forms. Understanding the operations of social temporality was no less important, I was convinced, for anthropologists or sociologists studying "the present" than for historians studying "the past." I argued that cen-

tral theoretical categories—like "culture" and "structure"—needed to be reconceptualized to make them capable of confronting the unavoidable fact of historical change. This book, which has taken many years to write, is dedicated to building the problematics of historical transformation into the conceptual vocabulary of social theory.

In 1991, I took up a position in political science and history at the University of Chicago, thus extending my experience as a participant observer to a fourth social science discipline. By then, I had written three of the essays that were to find their way into this book and had several others in mind. But in the helter-skelter of contemporary academic life, I never found the time to sit down and write the book through end-to-end. Instead, I took advantage of invitations to give a paper here or attend a conference there and composed them one at a time. The essays have, therefore, gradually been scattered about in various journals and edited collections. But in spite of their varied histories, all the essays in this book—with the exception of chapter 9, which I have revised extensively to make it fit the book's themes—were written with this volume in mind. I hope readers who know previous versions of these essays will find that they gain in depth from each others' company.

Scholarship, which may seem a lonely occupation to those who do not pursue it, is in fact profoundly social. Our ideas are produced within the socially constructed network of puzzles, problems, and obsessions that are the stuff of intellectual communities, and they are advanced by endless discussion and argument. This general observation is particularly pertinent for a book like this, which attempts to bridge the conversations of several more or less distinct academic specialties. My ability to formulate the questions asked in this book and my confidence that the answers I propose may prove useful to others depends utterly on a long string of overlapping interdisciplinary conversations I have carried on with other scholars in several different cities over the past four decades. These conversations have been sustained above all in a succession of more-or-less organized discussion groups. These have varied enormously in form, focus, and institutional location, but they have had one thing in common. They have made it possible to explore a range of topics and ideas that would have been far beyond my ken as an individual scholar—and to do so in a framework of unfettered discussion, critical probing, and mutual respect. It is in these discussion groups that my thinking about history and the social sciences has been stimulated, tried out, criticized, reformulated, and, I hope, improved. I feel that the arguments I have made in this book are a joint

product, co-authored with the many friends and colleagues who have collaborated in my reflections over the years. It is to them, and to the discussion groups that sustained our conversations, that I dedicate this book.

Scholarship also requires material support. During the years I worked on this book I was awarded a fellowship from the John Simon Guggenheim Foundation and spent glorious year-long research leaves at the Center for Advanced Study in the Behavioral Sciences in Stanford, California and at the Institute for Advanced Study in Princeton, New Jersey—the first supported by the National Science Foundation (grant number BNS-870064) and the second by the National Endowment for the Humanities. I also received generous support from the Universities of Michigan and Chicago and from the Successful Societies Program of the Canadian Institute for Advanced Research.

I would like to thank David Brent, my editor at the University of Chicago Press, for skillfully guiding this book to publication. I received valuable and generous readings of the entire manuscript from the Press's two referees, Michèle Lamont and Keith Baker. During the final two years of writing and rewriting this text, I benefited from the loving companionship, the constant intellectual stimulation, and the fine critical eye of Jan Goldstein, who was always willing to take time out from the book she was finishing to hear my latest thoughts or to read a chapter. I hope we'll finish many more books in each other's company.

1

THEORY, HISTORY, AND SOCIAL SCIENCE

The goal of this book is to initiate a serious dialogue about social theory between history and the social sciences. The groundwork for such a theoretical discussion already exists. Social scientists and historians have been talking to each other and reading each other's work for a long time, and both groups of scholars discuss social theory and make use of it in their research. Yet, for reasons I sketch out below, an adequate dialogue about social theory has not developed. In particular, historians' complex and many-sided understanding of the temporalities of social life has scarcely found its way into social theoretical debate. Having spent the better part of my career as a simultaneous participant in the everyday academic life of history and various social science disciplines, I am convinced that a deeper theoretical engagement between historians and social scientists could be mutually enlightening. In this book I indicate what shape such an engagement might take, some of the topics it could illuminate, and how it might affect thinking on both sides of the disciplinary divide. If my arguments are not found convincing, I hope they will provoke counterarguments. I firmly believe that social theory, history, and the social sciences—and more importantly, our common project of gaining knowledge about the social world—can advance only by means of free, open, and spirited intellectual exchange.

HISTORY AND THE SOCIAL SCIENCES

It is hardly novel, at the beginning of the twenty-first century, to call for dialogue between historians and social scientists. Such a dialogue was, for example, one of the stated ideals of Marc Bloch and Lucien Febvre as long

ago as 1929, when they founded the famous and influential French historical journal *Annales*. In the decades following World War II, and particularly in the 1960s and 1970s, the discipline of history all over the world was profoundly affected by methods and theoretical perspectives borrowed from the social sciences. Since the 1970s, this borrowing has increasingly been reciprocated by social scientists: sociologists and anthropologists, in particular, have become increasingly interested in questions of historical change. Moreover, at least in the abstract, social scientists and historians have always been interested in the same fundamental problems: the functioning, reproduction, and transformation of social relations. Finally, history and the social sciences crystalized out of a single field of discourse. It was only between the 1880s and World War I that sociology, history, political science, anthropology, geography, and economics emerged as the distinct and professionalized academic disciplines we know today. Prior to that time, intellectual discussion flowed easily across the indistinct boundaries between different genres of scholarship. It is difficult to assign major thinkers of this era to a single field, as we understand these fields today. Was Adam Smith or John Stuart Mill a philosopher, an economist, or a political scientist? Was Marx an economist, a sociologist, a philosopher, or a historian? Tocqueville a historian, a sociologist, an ethnographer, or a political scientist? It was only in the early twentieth century that the academic disciplines, replete with their distinct departments, chairs, curricula, and doctoral programs, became genuinely—if artificially—distinct intellectual universes (Abbot 1999; Clark 1973; Keylor 1975; Lepenies 1988; Novick 1988; Ross 1991). Only then were history and the various social sciences sufficiently clearly bounded that one could think about the necessity of initiating a dialogue. The cross-disciplinary poaching that currently prevails in history and the social sciences might therefore be seen as betokening a return to the golden age of our predisciplinary past.

The academic disciplines, however, have utterly transformed the Edenic intellectual landscape of the late eighteenth or early nineteenth centuries. The disciplines, true to their name, wield powerful disciplinary mechanisms of control and constraint. With their monopoly on certification and their control over curriculum, hiring, tenure, and allocation of research funding, the disciplines have entrenched themselves within clearly drawn borders. The untiring efforts of several generations of academic social scientists and historians have succeeded in forming distinct communities of discourse, with distinct methods, vocabularies, and standards of evaluation. It is because the disciplines have so successfully divided up the intel-

lectual terrain—not of course, without smuggling and recurrent boundary disputes—that conversation between historians and social scientists must take the form of dialogues between disciplines.

It is certainly true that in the past forty years or so the borders have become ever more porous. It has become de rigueur in academic circles to praise interdisciplinarity in scholarship and in graduate training—although it should be said that the actual practice of interdisciplinarity often falls short of articulated ideals, especially when it comes to hiring decisions. It is common for current work in historical studies to transcend disciplinary boundaries in multiple ways, with historical research being carried out by scholars with various formal disciplinary affiliations, using methods and theories of the most assorted provenance. In this sense, dialogues between history and the social sciences are carried on every day. Both historians and social scientists have also widened this discourse to include theoretical borrowing from the humanities. The "linguistic turn" or "cultural turn" that has swept over the social sciences and history alike in the past quarter century has brought theories about meaning and representation, many developed by literary critics and philosophers, into the interdisciplinary mix. Nevertheless, the nature of these dialogues has been strongly shaped by the disciplinary cultures of the fields from which the scholars come. It is for this reason that I feel comfortable speaking of the need to initiate a dialogue about social theory between historians and social scientists, even in the current landscape of widespread interdisciplinarity.

Theory has a strikingly less central place in history than in the social science disciplines. From the beginnings of the systematic differentiation of disciplines in the late nineteenth century, historians and social scientists alike have contrasted the "ideographic" or "descriptive" research of historians—which attempts to capture the uniqueness and particularity of its object—with the "nomothetic" or "explanatory" research of social scientists—which aims to establish general laws or at least valid generalizations. Social science fields might be said to be defined by their theories and formal methodologies; history is more informally (but no less effectively) defined by its careful use of archival or "primary" sources, its insistence on meticulously accurate chronology, and its mastery of narrative. This difference is clearly marked in the fields' graduate programs. Graduate students in the social sciences are usually required to take courses in the discipline's theory and/or methods at the beginning of their careers. By contrast, courses in historical theory or methods usually are purely elective, if they are offered at all. The most important site of disciplinary training in his-

tory graduate programs is not the required theory or methods course, but the research seminar, where students learn to be historians by doing research in primary sources and writing historical narratives, rather than by mastering the theories and explicit methodologies of their fields. At the point when they have finished their formal coursework, historians typically are already quite sophisticated researchers and writers but lack systematic theoretical training. Social scientists, at this moment in their careers, often have little sense of how to carry out a research project, but can argue about fine points of theory indefinitely.

This difference in the place of theory in the disciplines remains true at all stages and in all aspects of the career. Articles in social science journals nearly always begin with a discussion of the relevant theory, whereas articles in historical journals are more likely to begin with something that evokes the particularity of their subject matter—for example, an anecdote or a salient quotation from contemporary sources—than with discussions of theory or historical interpretation.[1] The same difference shows up in definitions of jobs: it is common for positions to be advertised as sociological theory, economic theory, or political theory, but I have never seen a job advertised in historical theory. Countless famous social scientists have made their reputations on the basis of work in theory, but it is hard to think of more than a handful of historians whose eminence arose from theoretical writing, and the few cases that come to mind generally do not have exclusively history appointments. Hayden White, the only historian I can think of whose fame rests almost entirely on theoretical works, spent most of his career teaching in the interdisciplinary History of Consciousness humanities program at the University of California, Santa Cruz (1973, 1978, 1987, 1999). Others—such as Joan Scott, a historian who is an eminent feminist theorist, or Dominick LaCapra, an intellectual historian who is also an important figure in literary theory, or William Reddy, who has done path-breaking theoretical work on the social and historical significance of emotions; or Dipesh Chakarabarty, who is a major "postcolonial" theorist—initially rose to prominence on the basis of more em-

1. I have checked this perception by looking at the opening paragraphs of articles in a handful of recent history and social science journals and coding them as either theoretical or descriptive. In both the *American Journal of Sociology* in 2002 and the *American Journal of Political Science* in 2002, articles that begin by invoking theoretical issues vastly outnumbered those that begin by invoking some temporally and spatially delimited trend, situation, or event (27 out of 30 and 51 out of 56, respectively). In both the *American Historical Review* in 2001 and 2002 and the *Journal of Modern History* in 2002, articles were about twice as likely to begin with invocations of trends or events as with theoretical or interpretive issues (21 out of 31 and 8 out of 12, respectively).

pirical works and continue to publish in the empirical genre. Moreover, it is surely not coincidental that they all have some kind of extra-history-department appointment.[2] That there are a handful of historians whose theoretical writings have received considerable attention even outside the history profession certainly indicates that theory is no longer off-limits to historians, but the fact that their numbers are so few and that their institutional location is commonly at once inside and outside of history shows that theory remains peripheral to the historical enterprise.

At present, it is common for historians to read social theory and to cite theorists in their written work. Indeed, such figures as Clifford Geertz, Antonio Gramsci, Michel Foucault, and Pierre Bourdieu—not to mention Karl Marx, Max Weber, and Emile Durkheim—have become something like household names in contemporary historical discourse. Yet even this growing use of theory does not constitute a genuine *dialogue*. There is no dialogue because the historians rarely speak back. They use social theory to orient their thinking, or borrow its vocabulary in their interrogation of historical sources or in formulating their arguments. What remains exceptional is for historians to intervene actively in social-theoretical debates. When historians borrow social-theoretical concepts we often find that the concepts don't quite fit, that they need to be adjusted, nuanced, or combined with concepts from other, apparently incompatible, theoretical discourses in order to be useful in historical research. In this sense, our use of theory is, practically speaking, critical. But we almost never reflect in print about these critical adjustments, nuancings, or recombinations. We tend not to ask whether something is systematically amiss in the theories we are borrowing that makes them ungainly for use in historical research, or to propose new vocabularies or conceptual schemas that might improve upon or supersede the existing concepts.[3] As a consequence, our use of social theory, while often implicitly critical in practice, has little impact on

2. Scott is a professor in the interdisciplinary School of Social Science at the Institute for Advanced Study, LaCapra holds a professorship in Humanistic Studies at Cornell; Reddy holds a joint appointment in History and Anthropology at Duke, and Chakrabarty has appointments in both History and South Asian Languages and Civilizations at Chicago. (For what it is worth, my own appointment is joint in Political Science and History at Chicago.) These historians' best-known theoretical works are Scott (1988), LaCapra (1983, 1985, 1989, 1991), Reddy (2001), Chakrabarty (2000). But see also Scott (1974, 1996), LaCapra (1972, 1982), Reddy (1984, 1997), and Chakrabarty (1989).

3. An instructive exception to this rule is Jan Goldstein's "Afterword," in the reedition of her *Console and Classify* (2001). Published fourteen years after the appearance of the original edition, Goldstein's afterword is precisely a reflection back upon the categories of social theory, in this case sociological theories about professionalization and Michel Foucault's opposition between disciplines and the law.

the store of social theory available to historians or to other scholars. Thanks to our training and to the prevailing culture of professional history, we have lacked the confidence to become active participants in theoretical debates.

In this book, I hope to show that this lack of confidence is mistaken: that historians have important and illuminating things to say about central issues in social theory. As I see it, social theory badly needs a serious infusion of historical habits of mind. But we can't expect sociologists, geographers, political scientists, and anthropologists to do the job for us. Only if historians enter the fray and develop systematic critiques and reformulations of the theories we borrow from social scientists can we expect to build social theories adequate for grasping the ever-changing world that is our common object.

WHAT HISTORIANS KNOW

What historians generally think of themselves as knowing about are their topics of research—the Russian Revolution, the Italian city-state, the Indian Ocean trade, the New Deal, the Ming Dynasty bureaucracy, the Boer War, Brazilian popular culture. This includes, of course, knowledge about how to use and interpret the relevant published and archival sources. But historians, whatever their particular topic, also know something else: how to think about the temporalities of social life. The common topic of historians is the unfolding of human action through time. Our thinking about time tends to be implicit rather than explicit, to be embodied in specific narrative accounts of particular series of events or particular transformations of communities, states, or fields of discourse. We don't think of ourselves as having a *theory* of social temporality. Yet I am convinced that most historians actually share a set of assumptions about how time is implicated in the organization and transformation of social relations and that these assumptions can be stated abstractly. In other words, historians have implicit or working theories about social temporality. Moreover, these theories are of considerable subtlety and sophistication, far superior, in my opinion, to the rather clumsy temporal assumptions that plague most theorizing in the social sciences. It is precisely as theoreticians of temporality that historians can most usefully participate in social theoretical debates.

How, then, do historians think about social temporality? First, and most fundamentally, I think we believe that time is *fateful*. Time is irreversible, in the sense that an action, once taken, or an event, once experienced, cannot be obliterated. It is lodged in the memory of those whom it

affects and therefore irrevocably alters the situation in which it occurs. Although I might make a promise and then retract it, the fact of my having made the promise is not obliterated by the retraction. I become, both to myself and to others who know about the incident, a different person, one who has made and retracted a promise. Most of our actions, of course, do not transform the situation in which they are undertaken. By nodding to my co-worker when we pass in the hall, I merely reaffirm our common employment status. Yet this simple gesture is itself significant for the ongoing history of social relations in my work unit (in my case, a department of political science). That this is so becomes immediately obvious if I fail to produce the acknowledging nod. If I simply walk by with no acknowledgement, this might be read as an ominous act, as a sign that I have entered a hostile faction of the department or that I have decided to vote no on his upcoming promotion. Especially if repeated, failing to give the expected nod will result in a chilling of social relations between me and the snubbed colleague. Either act, the nod or the lack of a nod, leaves a historical residue; it inflects the social relations between me and my colleague and potentially those of my department as a whole. And it goes without saying that more dramatic actions—denouncing a colleague at a faculty meeting, arguing vociferously against making new hires in one of the department's rival subfields, or inviting a previously nodding acquaintance to co-teach a course or collaborate on a paper—will generally have considerably more powerful effects on the course of social relations in the department.

Although individual actions can be shown to have fateful social effects, it is also true that every act is part of a *sequence* of actions and that its effects are profoundly dependent upon its place in the sequence. My relations with my nodding-acquaintance colleague will be more profoundly ruptured by my non-nod if I have failed to nod to her the past three times we've passed in the hall, or if she has recently been snubbed by one of my known friends and allies, or if I have recently said disparaging things in a department meeting about the kind of research she does. By contrast, the effects of my non-nod will be decreased if we have recently been on the same side in a struggle to reform the department's voting procedures or if she has just gotten an article accepted in one of the field's leading journals. Historians believe that we cannot understand why things happened as they did without figuring out the sequence in which things happened. As this implies, historians assume that the outcome of any action, event, or trend is likely to be *contingent*, that its effects will depend upon the particular complex temporal sequence of which it is a part. The effects of a given happening

may be nullified, magnified, deflected, compounded, channeled, or broadcast by previous, subsequent, or simultaneous happenings. The fact that the outcome will be contingent upon not only a wide range of other actions, trends, or events, but also upon the precise temporal sequence in which these occur, means that historical happenings are extremely unpredictable.

It is of course true that social scientists also recognize the fatefulness of time in their personal lives. Sociologists or economists are just as aware as historians that having a baby, deciding to take a new job, being left by one's spouse, making friends with a colleague whose ideas transform one's own, or learning that one's child has a life-threatening disease have major and unpredictable consequences for one's life. The difference, as I see it, is that while social scientists recognize temporal fatefulness as a truth of everyday existence, most of them bracket this truth out of their scientific consciousness. Although they see these everyday or personal experiences as fateful and existentially wrenching, they view them as essentially random, as noise, from the point of view of the whole. As social scientists, they see their task as rising above the contingency and messiness of everyday life to find the lawful regularities that actually govern the whole. Historians' practices imply a rejection of this partitioning of everyday life from the social totality and claim instead that temporal fatefulness we experience in our personal lives is replicated at every level of social life.

The conceptual vehicle by means of which historians construct or analyze the contingency and temporal fatefulness of social life is the *event*. Historians see the flow of social life as being punctuated by significant happenings, by complexes of social action that somehow change the course of history. Historians constantly talk about "turning points" or "watersheds" in history and spend much of their conceptual energy dividing the flow of history into distinct eras that events—the establishment of the Han Dynasty, the Crusades, the rise of printing, the Reformation, the Industrial Revolution, the Russo-Japanese War, the Nazi seizure of power, the Cuban Missile Crisis, the rise of electronic media, the fall of the Berlin Wall—mark off from each other. Historians see events like these, which transform the histories of entire human collectivities, as having the same sort of fatefulness and contingency as the smaller events—divorces, new jobs, angry breaches in department meetings—that inflect the course of our personal lives. As usual, historians haven't engaged in much abstract theoretical reflection about how events have such dramatic, historically transformative effects. They have, rather, given countless narrative ac-

counts of how particular sequences of happenings have indeed changed the course of history of some collectivity or other—Oxford dons, Shanghai workers, New Yorkers, Russians, Roman Catholics, or the world as a whole. As against the implicit assumption of most social scientists, that social change takes place according to smooth, gradual, predictable, and linear processes, historians assume that historical temporality is lumpy, uneven, unpredictable, and discontinuous.

Thinking about historical events makes clear another fundamental assumption about temporality that is probably less obvious (although no less true) at the level of personal experience: that social temporality is extremely *complex*. One significant characteristic of historical events is that they always combine social processes with very different temporalities—relatively gradual or long-run social trends, more volatile swings of public opinion, punctual accidental happenings, medium-run political strategies, sudden individual decisions, oscillating economic or climatic rhythms—which are brought together in specific ways, at specific places and times, in a particular sequence. That there are a diversity of temporalities operating in any present raises difficult analytical challenges. How do we handle the problem of sequence when we are dealing not with a chain of discrete and precisely timeable decisions, but the intertwining of long-term with punctual processes? Which social processes, with which temporalities, will emerge as dominant in an event that mixes them together? How, and when, do short-term processes override, deflect, or transform long-term processes? How do long-term trends reassert themselves in situations where they seem to have been eclipsed by more pressing political processes? Writing convincing historical narratives often hinges on the ability to resolve such complex temporal conundrums.

The historians' "eventful" conception of temporality certainly posits that different historical times have, effectively, different rates of change—that history may be "accelerated" by events. But it also posits that events transform or reconfigure social relations. The consequence is that they see distinct historical eras as having varying forms of life and different social dynamics. Historians, to put it differently, assume that time is *heterogeneous*. We assume that what entities exist in the social world, how they operate, and what they mean change fundamentally over time. This is not to say that the world is in constant flux and chaos; the social temporality posited by historians is always a mix of continuity and change. But our working assumption is that every important form of social relations is potentially subject to change: not only ideas, institutions, and identities, but

tools, forms of shelter, sex, gods, climate, diseases, cultivated plants, and languages. Another way of putting this is to say that historians implicitly assume that social life is fundamentally constituted by culture, but by culture in the widest possible sense—that is, by humanly constructed practices, conventions, and beliefs that shape all aspects of social life, from agriculture and procreation to poetry and religion. We assume that because these practices are humanly constructed, humans are also capable of destroying, altering, neglecting, forgetting, or radically reconstructing them, either purposely or unintentionally.

Temporal heterogeneity implies *causal heterogeneity*. It implies that the consequences of a given act are not intrinsic in the act but rather will depend on the nature of the social world within which it takes place. This assumption is quite contrary to the practices of mainstream social scientists, whose entire mode of operation is to discover and apply *general* causal laws, laws implicitly or explicitly assumed to be independent of time and place. The model case would be economists, who assume that all social actors everywhere are utility maximizers and that the laws of supply and demand are universal. Historians of course admit the existence of causal regularities of considerable duration. But rather than assuming that the world of the past must have been governed by the same logics as the world of the present, historians assume that the social logics governing past social worlds varied fundamentally, and therefore that their logics must be discovered and puzzled out by the researcher.

Temporal heterogeneity also implies that understanding or explaining social practices requires *historical contextualization*. We cannot know what an act or an utterance means and what its consequences might be without knowing the semantics, the technologies, the conventions—in brief, the logics—that characterize the world in which the action takes place. Historians tend to explain things not by subsuming them under a general or "covering" law, but by relating them to their context.

Finally, if the world in which actions take place is temporally heterogeneous, it makes sense for historians to insist on the importance of *chronology*. Indeed, chronology—the precise placement of a happening or a fact in time—is important for two reasons. First, as I have already pointed out, historians insist that we cannot know why something happens or what its significance might be with-out knowing where it fits in a sequence of happenings. Meticulous attention to chronology is the only way to be sure that we have the sequence straight. But chronology is also important because the meaning of an action or an event depends on the temporal con-

text in which it occurs. In order to understand the relation of one social fact to another, one needs to know whether the temporal boundaries of the social facts placed them within the same "historical era"—that is to say, within a period during which some particular historical logic obtained. Chronology is crucial because it tells us within what historical context we must place the actions, texts, or material artifacts we are attempting to interpret or explain.

The historian's implicit theorization of social temporality—as fateful, contingent, complex, eventful, and heterogeneous—is, I hope to have indicated, reasonably coherent. And its methodological corollaries—a concern with chronology, sequence, and contextualization—seem to me logically consistent with the theory. I think that the vast majority of working historians would concur with at least the general outlines of what I have claimed on their behalf, although I am sure many would contest one or more of my specific formulations. This theorization is intentionally very abstract, and even historians who accept my abstract outline might disagree violently over how its various points might be specified: What counts as an adequate contextualization? What social causalities vary from one period to another? What is it about events that enables them to "change the course of history"? What is contingent and what is necessary in a given course of change?

Historians, at least implicitly, conceptualize social temporality with considerable care and finesse. But with rare exceptions, they do so only implicitly. They don't regard their understanding of the temporality of social life as being a matter of theory at all, but simply as how the world works, as the mere factuality of things. They learn their conception of temporality by a kind of scholarly osmosis, by reading other historians and internalizing the ways they narrate accounts of historical change and continuity. They know a lot about social temporality, but they know it as a kind of professional common sense, all the more so because it is roughly consistent, as I have tried to indicate above, with a more everyday common sense about the temporality of our personal experience. Moreover, historians, in my experience, suffer from a kind of narrative overconfidence. When they reach tight spots in their arguments, they tend to try to narrate their way out of trouble, going back to the sources for yet more detail, laying on more and more examples, instances, and anecdotes. This often means that important conceptual questions—about temporal dynamics, about causation, about the nature of the relations between events or entities—get lost in a welter of narrative detail, rather than being addressed

at the appropriate conceptual level. Historians may be virtuosos of social temporality, but their theoretical consciousness is often so underdeveloped that they are not conceptually aware of what they know.

It also must be said that there are plenty of sociologists, political scientists, anthropologists, or geographers whose working assumptions about temporality more closely resemble the historians' model I have sketched above than the assumptions of mainstream social science. Indeed, most of the examples of adequately historical conceptualizations of temporality that I discuss in this book were assembled by sociologists or anthropologists, not historians.[4] I have chosen these examples in part because it is important to show that sophistication about temporality is not fated to be a unique possession of professional historians and in part because sociologists and anthropologists tend to be more self-consciously aware than historians of the theoretical problems posed by their historical arguments. Yet it is symptomatic of the primitive state of theoretical dialogue between history and the social sciences that even these social scientists, whose general theoretical and methodological instincts are finely honed, are often insufficiently explicit themselves about their temporal assumptions. This is another reason to believe that a more robust theoretical dialogue between social scientists and historians would be beneficial to both.

WHAT SOCIAL SCIENTISTS KNOW

Social scientists tend to be much more self-conscious about theory than historians. That is why it makes sense for historians to enter into theoretical dialogue with them. But in initiating a dialogue, it is important to note the internal diversity of the social sciences, both within and between disciplines. The same professionalization of the academic fields that divided history from the social sciences also made each of the social science disciplines distinct from the others. The predominant epistemological culture of most social science fields in the United States is positivist, but anthropology has developed in a very different and much more interpretivist direction. The theoretical assumptions of most social or cultural anthropologists are probably closer to those of historians than to those of other social scientists. Indeed, many anthropologists pay more attention to the work of humanities scholars than to that of sociologists, political scientists, economists or—for that matter—historians. Economics, by contrast, is the most fully mathematical and quantitative of the social sciences, while

4. See, especially, chapters 3 and 7, below.

sociology, political science, and geography are deeply rent by theoretical and methodological disputes. There is, for example, almost no epistemological overlap between the views of symbolic interactionists and demographers within sociology. This diversity in theoretical outlook among social scientists means that the notion of a dialogue between history and social science is something of an oversimplification. In fact, a dialogue with anthropologists will be different in form and content from one with sociologists or economists, and one with symbolic interactionists different from one with demographers. Nor are all social scientists likely to be equally good or willing interlocutors for historians. Indeed, most of the social scientists who practice "mainstream" quantitative social science begin with theoretical premises so incompatible with the working assumptions of historians that we might expect conversation with them to be largely a dialogue of the deaf.[5]

Practically speaking, the most valuable dialogic partners for history are social scientists who are interested in historical and cultural questions, and who either use or are at least willing to countenance what are called "interpretive" or "qualitative" methods. Nearly all social scientists who write what would normally be called "social theory" fit into this category, as would most social or cultural anthropologists. But in predominantly positivistic and quantitative fields like sociology, economics, political science, and geography, such scholars are a dissident minority. If my experience is any guide, one reason that interpretively inclined social scientists might be interested in theoretical exchanges with historians is their sense of embattlement within their own fields. What I am actually advocating and attempting to carry out in this book is not and cannot be a general and diffuse dialogue between "history" and "social science," but a more specific theoretical dialogue with those social scientists who might be ready to join in a common enterprise. At the same time, it is important to realize that historians' theoretical insights of are valuable not only to social scientists who work on specifically historical topics. Because the social world is in fact ever-changing, because it is structured by complex and contingent temporalities, it is as crucial for someone who studies the contemporary

5. See Andrew Abbott's (1988) classic critique of the standard assumptions of quantitative sociology. I hasten to add that I certainly do not oppose quantitative research and have indeed done a fair share of it myself. In the final chapter of this book I make a specific argument for the value, even the necessity, of quantitative methods—but from a theoretical perspective very different from that which currently dominates the social-scientific mainstream in the United States.

social world to understand the logics of history as it is for someone who studies the past.

What, then, do social scientists know that they can bring to a dialogue with historians—other than their generally higher level of theoretical consciousness? Social scientists' most theoretically valuable habit of mind, in my opinion, is their strong penchant for *structural* thinking, a penchant that interpretivists generally share to a greater or lesser degree with positivists. By contrast with historians, who tend to opt for multiple causality and detailed circumstantial narrative, social scientists tend to look for explanations in terms of a relatively limited set of enduring, entrenched, and causally powerful features of the social world—such features as class relations, dominant ideologies, enduring occupational or demographic patterns, powerful economic interests, stubborn cultural beliefs, or built-in characteristics of organizations. Where historians tend to be satisfied with multi-stranded but ultimately causally diffuse accounts, social scientists tend to single out what they take to be the most causally important features of the world and to elaborate their dynamics systematically. This insistence on explaining phenomena by means of well-defined structural features tends to push researchers to greater theoretical and methodological clarity. In my opinion, structural thinking is a social-scientific virtue that historians could profitably emulate. Indeed, I think one reason for historians' widespread borrowing of concepts from social theory in the past few decades has been precisely an attempt to introduce more structural forms of thought into historical research.

However, social scientists' structural thinking is commonly flawed by inadequate temporal assumptions. "Structure," in most social-scientific usage, is at least implicitly conceived of as *given*, as the solid social facts or underlying and fundamentally invariable logics that determine the phenomenal shapes of social action. Social conduct, the specific features of institutions, or particular beliefs and opinions may vary widely, but these variations are seen as effectively shaped or regulated by underlying structures. In the rhetoric of social-scientific discourse, the buck tends to stop at structure. Once social or cultural variations have been attributed to structures—to institutional design, occupational distributions, gender stereotypes, binary systems of classification, consumer preferences, and so on—the explanation is regarded as complete. The problem, of course, is that the underlying causal structures themselves undergo mutations or transformations over the course of historical time. They, too, have histories. Many social scientists are aware of the ultimately historical character

of structures, but they often don't know quite how to think about the problem. This seems to me one of the most obvious places where deeper theoretical dialogue between social scientists and historians could yield substantial benefits. What both social scientists and historians need is forms of structural thinking that are compatible with historical conceptions of temporality.

A second valuable habit of mind that historians might pick up from social scientists—this time from historical sociologists, themselves only a minority subfield within sociology—is a willingness to confront the biggest historical questions. Historians, with their emphasis on archival research, exact chronology, and detailed narration, tend to be very diffident about questions that take them beyond the limits of their scholarly mastery. Historical sociologists, by contrast, have traditionally been willing to address the biggest questions: the rise of capitalism, the nation-state, or modernity; the dynamics of revolutions; the governance of empires; the rise and fall of civilizations.[6] According to the scholarly standards of the history profession, scholars should possess a full command of the relevant historical literature and must have the ability to read documents in the original language; sociologists, by contrast, lack these admirable but also crippling scruples. This has freed historical sociologists to look for larger historical patterns that professional historians care about in principle but feel paralyzed about pursuing in practice. By reminding historians of the big questions and challenging us to examine, confirm, or refute their arguments about them, historical sociologists consistently enlarge historians' horizons.[7]

The social sciences, thus, have important virtues that historians should emulate. But they have their vices as well. Most fundamentally, mainstream social scientists are hampered by an uncritical, or at least insufficiently critical, embrace of a certain natural science model—by what in-

6. Examples include Eisenstadt (1963), Moore (1966), Wallerstein (1974a), Bendix (1978), Skocpol (1979), Mann (1986, 1993), Tilly (1990), and Arrighi (1994).

7. It is by no means the case, it should be said, that social scientists are in general more wide-ranging than historians. It is, for example, rare for even the most mundane historical works to approach the empiricist triviality that threatens to become the norm in the political science subfield of American politics, where article after article contributes to our knowing more and more about less and less—detailed statistical estimations of the effect of ballot initiatives on voter turnout, or of the effect of party influence or campaign contributions on congressional roll-call votes; or mathematically elaborate game-theoretic models of signaling in legislative-judicial interaction, of tactical maneuvering on omnibus bills in congress, or of the effects of separation of powers on congressional decision-making.

terpretivist wags have aptly dubbed "physics envy." The ideal of social science, at least since Auguste Comte announced the foundation of "sociology" in the early nineteenth century, has been to attain the degree of exactness that had already been achieved by physics at the time of Isaac Newton. Comte's schema has been repeated ever since: physical science, which deals with the simplest phenomena, was the first to reach the scientific stage; biology, which deals with more complex phenomena, attained scientific status later; and sociology (or, as we would say, social science), which deals with the most complex phenomena, will be the last to become fully scientific. The social sciences, so the litany goes, are still young sciences, and to become mature they must emulate their elders, adopting strict "scientific methods" and using mathematics and quantitative data whenever possible. This program has been carried out most fully in economics, which in recent decades has attained a status second only to physics among advocates of the natural science model.

One might regard it as strange that a science could still remain "immature" some century and a half after its foundation, or that it could still be under the thrall of a long-surpassed Newtonian form of physics. The repeated failures to make the social sciences more efficacious by making them mathematical should have long since rendered the neo-Comtean schema implausible, but they have not. We are faced here with an extremely powerful historical structure indeed, a stubborn intellectual founding myth whose abandonment seems equivalent, in the eyes of most American social scientists, to giving up the project of seeking exact social knowledge altogether. Indeed, as I argue in chapter 3 below, a version of this myth is powerful even among historical sociologists, who might seem the obvious allies of historians within the social sciences. The prestige of the natural sciences, the wish to emulate their "scientific methods," to seek legitimation in quasi-experimental rigor—this continues to haunt even the most historically inclined social science fields. One of the most difficult obstacles facing a dialogue between history and the social sciences is this entrenched belief that some form of natural science model is the royal road to truth in the study of social life.

This overvaluation of the natural science model reinforces a deep resistance among social scientists to the notion that society is culturally constructed—which, as we have seen, is central to contemporary historical thinking. If societies are indeed culturally distinct at some deep level, this implies that any putative "social laws" can only be valid locally, that truly general social laws are an impossibility. This means that social physics, in-

cluding the form of social physics already invented by the economists and widely copied by quantitative political scientists and sociologists, must be illusory. Admitting that social relations are culturally constituted would imply that the Newtonian grid of uniform space and time posited by the quantitative social sciences is in fact crumpled and rent—that the world is too messy a place to be understood by a Newtonian social science. And so it is. This book is based on the premise that our messy and mutable world needs the conceptual tools that only a collaboration between interpretive social scientists and historians is likely to provide.

Social and cultural anthropology, which has for the most part eschewed the natural science model, is in many ways a natural ally for theoretically minded historians. Anthropologists generally assume that the social world is culturally constructed all the way down. In the 1970s, when historians initially became enamored of the works of anthropologists, anthropological conceptions of culture tended to be entirely synchronic. They could explain how seemingly strange or exotic forms of life were coherently structured by cultural systems, but they were at a loss when it came to explaining historical change in cultural patterns. Over the past twenty-five years or so, historical questions have become much more central to anthropology, and anthropologists have developed much more sophisticated conceptions of historical temporality. But it is the encounter with poststructuralist philosophy and literary criticism that has had the biggest theoretical impact on anthropology since the mid-1980s. Critical anthropologists have effectively deconstructed the older conception according to which culture was a coherent system of meanings that could account for the orderliness of social life. This philosophical critique has been mixed with much soul-searching about the moral standing of the field. Anthropology, as the critics properly point out, has from the beginning been associated with European and American colonialism. Its attempts to speak for the people whom it studies is consequently fraught with moral and political ambiguities. Such moral and political critiques are certainly well taken. And the development of a more fragmented and contradictory concept of cultural forms is certainly laudable, as is the attempt to introduce multiple voices into ethnographies.

However, I believe that the hypercritical thrust of much contemporary anthropological thinking has resulted in an effective abandonment of some of the field's most powerful insights and useful conceptual tools. I think that the more classical or structural conception of culture remains extremely valuable—indeed, indispensable—for constructing a properly

historical form of social theory. Anthropology's classical notions of culture, no less than sociology's classical notions of social structure, need to be infused with historical temporalities. We need to develop conceptions of culture that make change as normal as reproduction and that build in continuing practical struggles over cultural meanings. But in doing so, I think we must begin from the strong conceptions of culture developed before anthropology's deconstructive turn. A more systematic focus on logics of historical change might help to restore a certain rigor to a field that sometimes seems more intent on critique of past errors than on constructing a viable way forward.

HOW THE ARGUMENT PROCEEDS

The essays that make up this book were written over the course of fifteen years, for a variety of occasions, and many of them have been published previously. But most were written with this book in mind. My intention is to carry out and exemplify in the following chapters the sort of theoretical dialogue between history and the social sciences that I have called for above. The chapters do this in different ways.

Chapter 2 is a partly autobiographical reflection on the history of historical scholarship during the four decades since I began graduate studies. The outstanding developments of this period were the rise of social history in the 1960s and 1970s and its surprisingly rapid displacement by cultural history in the ensuing years. I was an enthusiastic participant in both phases of this historiographical development, but I have recently come to feel that something important has been lost in the turn from social to cultural history. In particular, I worry that the currently dominant forms of cultural history are not capable of grasping the historical transformation of world capitalism that is powerfully altering social relations in our own era. I attempt, in this chapter, both a critical diagnosis and a historical explanation of the current state of historical thought, pointing out the political as well as intellectual stakes of the encounters between history and social theory.

Chapter 3 is a critical evaluation of sociology's encounter with history. It argues that the potentially radicalizing effects of historical sociology on the discipline's epistemological assumptions has been blunted by the historical sociologists' own conventional and limiting conceptualizations of temporality. On the basis of critical readings of representative texts, I distinguish three different conceptions of social temporality employed by historical sociologists: the conventional teleological and experimental concep-

tions and a more historical (and more interesting) eventful conception. I try to show that certain classic works employing teleological and experimental temporalities owe much of their intellectual success to untheorized but nevertheless crucial eventful historical analyses. Historical sociology, and sociology as a whole, I argue, needs to recognize, and to integrate into its theoretical arsenal, the complex social temporalities that historians take for granted.

Chapter 4, like chapter 3, is an effort to introduce a more historical temporality into sociological thinking. Chapter 4 takes on the key sociological concept of structure. I attempt to develop a theory of structure that bridges the very different sociological and anthropological implications of the term; that internalizes a concept of agency; and that can account for the transformation of structures over historical time. The argument is developed largely by means of a critique and appreciation of the works of the British sociologist Anthony Giddens and the French sociologist Pierre Bourdieu.

If chapters 3 and 4 attempt historical critiques and reformulations of sociological theory, the following three chapters are primarily confrontations between history and anthropology. Chapter 5 examines the notion of culture, long anthropology's central concept. Over the past two or three decades, the concept has escaped from anthropology and has been taken up enthusiastically by scholars in many other disciplines—for example, history, sociology, political science, geography, and literary studies. At the same time, anthropologists have become increasingly ambivalent about the culture concept, which they increasingly see as tainted by imperialism and orientalism. I argue that culture is, as anthropologists have traditionally claimed, the inescapable ground of the human sciences, and that it needs to be conceptualized more carefully rather than abandoned or avoided.

In chapter 6, I look at the work of Clifford Geertz, the anthropologist whose work has done more than any other's to spread an interest in culture beyond the discipline of anthropology. Historians, in particular, have been profoundly influenced by Geertz. The adoption of Geertz by historians is paradoxical, on the face of it, because his work is insistently synchronic and therefore seemingly antihistorical. I argue, however, that it is precisely his brilliant deployment of synchronic argument that makes his work so interesting to historians, whose conceptions of temporality in fact combine both synchronic and diachronic elements. Finally, I show that Geertz's famous definition of culture as at once a model of and a model for social life, which underwrites his synchronic cultural analyses, can actually

be mobilized diachronically, as a way of explaining cultural and historical change.

Chapter 7 examines the work of another anthropologist, Marshall Sahlins, who has developed a particularly sophisticated anthropological theorization of historical change, via the category of the event. Sahlins, whose anthropological theory is strongly influenced by that of the famous French "structuralist" Claude Lévi-Strauss, uses a striking historical event—the first arrival of Europeans in Hawaii—as a means of developing a potentially general theory of cultural transformation. Sahlins argues that the Hawaiians took Captain Cook to be an incarnation of one of their deities, whose seasonal arrival was being celebrated just as Cook's ships came ashore. Sahlins shows how the Hawaiians' actions, which began with Cook's veneration and ended with his murder, simultaneously reproduced Hawaiian culture (by assimilating things European to Hawaiian cultural categories) and transformed it (because the assignment of such novel beings to conventional categories changed the meanings of the categories and therefore altered the entire categorical system). I argue that Sahlins's theorization is of very general value, in spite of the unique and seemingly exotic character of his exemplary case. But I also argue that his theory of historical change requires certain modifications—largely the building of a more variegated conception of historical duration into the argument.

Chapter 8 continues the theme of the event, but moves from the historical anthropology of Oceania to what is generally acknowledged as one of the central events of European history, the taking of the Bastille on July 14, 1789. The chapter attempts to work on two levels. First, it elaborates a historical argument, claiming that the initial formulation of the modern conception of revolution arose from this particular event. But it also uses the example of the taking of the Bastille as a kind of analytical template for developing a more general theorization of the event. In this chapter, I attempt to take historians' conventional assumptions about the nature of social temporality, raise them to the level of explicit theory, and use this theory to illuminate an important historical case.

Chapter 9, like chapters 7 and 8, is a theoretically informed case study. It examines what might be thought of as the opposite of a transformative event: a case of the endurance, against what would seem to have been insuperable odds, of a highly anomalous institution. It looks at the history of the dockworkers of nineteenth-century Marseille, whose trade society maintained extraordinary privileges for its members during a period when such societies were officially outlawed. I also use the case as a means of

thinking through the relationship between a large-scale global process—capitalist development—and the strikingly variable economic and political experiences of different categories of workers. Using a combination of quantitative and interpretive methods, the chapter stresses the unevenness of the social temporalities operating in nineteenth-century Marseille and shows how human agency, contingency, and inexorable social processes were twisted together in a surprising and dramatic historical sequence.

Chapter 10, the final essay in this book, attempts to cover the whole territory laid out in the previous nine, but from a different and more philosophical angle of vision. It attempts to set forth the ontological assumptions that underlie the historically inflected social theory laid out in the previous chapters. It does so by attempting to define the "social" in social science. "Social," I note, is an exceptionally vague but also extremely capacious term—one that signifies the complex totality of human interrelatedness. I suggest that the social, in this sense, is constituted by overlapping and interconnected streams of semiotic practices. To flesh out this notion, I start with an assumption shared either explicitly or implicitly by most cultural historians and cultural anthropologists: that social life should be understood as constituted by language, that is to say as a kind of complex and open-ended text. Beginning from this point, I attempt to complicate and broaden this linguistic conception of the social: by including forms of semiotic practice that are not strictly speaking linguistic; by investigating the problem of articulations between different kinds of semiotic practices; by showing how such varied semiotic practices can generate patterns of human actions whose deciphering may require recourse to quantitative or mechanistic forms of reasoning. Thus, I recreate what is in some ways a familiar picture of social science: one that includes both cultural/semiotic and quantitative/mechanical methods. But there is an important difference. Whereas semiotic and quantitative research in the existing social sciences are based on completely incompatible ontological assumptions, I try to found both on a single ontological basis, and one in harmony with interpretive methods. This common ontology, I argue, can generate the entire complex gamut of historical logics explored in this book.

2

THE POLITICAL UNCONSCIOUS OF SOCIAL AND CULTURAL HISTORY, OR, CONFESSIONS OF A FORMER QUANTITATIVE HISTORIAN

This chapter straddles the boundaries between scholarly essay, personal reflection, and political critique. My topic is the history of social history over the past forty years or so, that is, since I began graduate school, at Berkeley, in 1962. In the American context, these four decades correspond rather neatly to an entire developmental parabola of the research program that was commonly called the "new social history"—from its meteoric rise in the 1960s and 1970s to its surprisingly rapid displacement by a "new cultural history" in the 1980s and 1990s. In this essay, I shall focus above all on the American case, which I know from the inside as a participant, but I shall also glance repeatedly at both Britain and France, where developments were roughly parallel but by no means identical, in intellectual substance, politics, or temporality. I shall not attempt to survey the research accomplishments of social or cultural history but will concentrate instead on the epistemological and methodological presuppositions that underlie these two types of historical research. I shall pay especially close attention to the relationship between epistemology and politics, a relationship that, in the case of social and cultural history, has been complicated,

This is a considerably extended version of an essay by the same title that appeared in *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, ed. George Steinmetz. Chapter 2 also incorporates some passages from "Whatever Happened to the 'Social' in Social History," which appeared in *Schools of Thought: Twenty-five Years of Interpretive Social Science*, ed. Joan W. Scott and Deborah Keates. I would like to thank Laura Downs, Geoff Eley, Jan Goldstein, Dagmar Herzog, Lynn Hunt, Jacques Revel, Joan Scott, and George Steinmetz for comments on earlier versions of this chapter.

paradoxical, and at times even perverse. This chapter is at once a somewhat disillusioned reflection on what I and my fellow cultural historians have wrought over the past quarter century and an argument that we need to revive some of the lost virtues of social history without abandoning the tremendous intellectual gains attendant upon history's linguistic turn. I wish to make it clear from the outset that I write as a fully engaged participant in the history I am chronicling and that my critiques should be read, at least in the first instance, as a form of self-criticism. My mode of political interpretation will be a form of Marxist criticism. I shall try to indicate how changes in the social and political forms of world capitalist development have—for the most part unconsciously—affected the politics of social and cultural history. Although my title invokes Fredric Jameson (1981), my approach to the recent intellectual history of social and cultural history actually has more in common with the work of Raymond Williams (1973, 1977). Like Williams, I am attempting to trace the emergence and expression in discourse—but in historical writing rather than in literature—of "structures of feeling" that arise from the writers' experiences of fundamental transformations in the social relations of capitalism.

My reflections are inevitably influenced by the peculiarities of my own experience. Three of these should probably be noted immediately. The first is that I work in French history. American historians of France have naturally been influenced by the example of the powerful and prestigious "Annales school," which led the way internationally both in social history from the 1930s through the 1960s, and in the turn to cultural history in the 1970s. This tended to give us the sense of being in the historiographical avant garde and meant that the gravitational pull of political history, which outside France often remained the specialty of the vast numerical majority of historians, was particularly weak. The history of the past forty years might look quite different to an American historian of Germany or the United States. The second peculiarity is that I was actually raised a positivist. My father was an eminent sociologist whose life-project was to make his discipline more fully "scientific." He was instrumental in building the University of Wisconsin's powerful and notoriously positivist sociology department and in obtaining a place for sociology at the federal feeding trough, especially at the National Institutes of Mental Health and the National Science Foundation (Sewell 1988). I began my career as a historian fully equipped with a positivist vision of science that I had learned at my father's knee. My first published paper, which dates from my graduate student days, was an attempt to explicate Marc Bloch's use of comparative his-

tory according to a positivist notion of hypothesis testing (Sewell 1967), and I undertook a dissertation that involved a massive effort of quantitative research (Sewell 1971). It seems clear that I was more deeply imbued with positivist views than were most of my social historian contemporaries.

A third peculiarity of my experience is that it has been far less bounded by history departments than is the norm. Even in graduate school, my training was cross-disciplinary: my major concentration was in the interdisciplinary field of economic history, and I completed significant course work in economics and did a minor field in sociological theory. Moreover, in only ten of the thirty-five years since I gained my first academic post has my appointment been exclusively in a history department. In addition to unalloyed history department appointments (at the University of Chicago from 1968 to 1975 and the University of Arizona from 1980 to 1983) I had a five-year interdisciplinary social science appointment (in the School of Social Science at the Institute for Advanced Study from 1975 to 1980), and have had joint appointments in sociology and history for seven years (the University of Arizona from 1983 to 1985 and the University of Michigan from 1985 to 1990) and in political science and history for fourteen (at the University of Chicago, from 1990 to the present). This unusually interdisciplinary professional experience means that I have, in effect, engaged in a good deal of participant observation of the theoretical, methodological, and rhetorical practices of several social science fields.[1] This has certainly made me far more aware than most historians of the wide range and the "culturally constructed" character of disciplinary epistemic practices; and it surely is at least partly responsible for the emergence in my work of a much stronger interest in theory than is generally characteristic of historians. (Of course, this last point could also be read the other way around; surely it was in part my penchant for theory that led to my unusually interdisciplinary career.)

Despite these idiosyncrasies, I do not think that my methodological views or my styles of historical research have been radically different from those of the mainstream of my generation of social historians. I began as a committed new social historian and made considerable use of quantitative data in my early work; subsequently I moved increasingly toward work with a cultural bent. This trajectory, as I will argue later, was actually quite

1. Another important disciplinary influence that does not show up in this enumeration of academic appointments is anthropology, which—as will become apparent later in this essay—affected me profoundly in the course of the 1970s.

common in my age cohort and has by no means been limited to historians working in the United States. Meanwhile, my political views and experiences have been almost embarrassingly typical of historians of my generation. Like many of my contemporaries, I was involved in a whole range of 1960s political and cultural movements: the Civil Rights movement, the movement against the Vietnam War, the university revolts (in my case, the Berkeley Free Speech Movement), and the counterculture. My active participation in politics slackened in the 1970s, as a result of both the declining vitality of the various movements[2] and changes in my personal and professional life—the demands of increased family responsibilities, of holding down a teaching job, and of producing publications so as to attain tenure. But I remained politically on the left and eventually became one of the "tenured radicals" who were so vehemently bemoaned by right-wing commentators during the Reagan and George H. W. Bush presidencies (Kimball 1990) and who are, indeed, currently very plentiful on the faculties of major American universities, in history departments as elsewhere.

SOCIAL HISTORY

In the years following World War II, social history was very much an international project. Its leading early centers were France, where the school of historical studies associated with the journal *Annales* gained intellectual and institutional ascendancy under the leadership of Lucien Febvre and Fernand Braudel; Britain, where the lead was taken by an extraordinary cohort of Marxist historians; and the United States, where a school commonly known as "the new social history" rose to prominence in the 1960s.[3] I was a member of the younger generation of "new social historians," those who completed dissertations in the late 1960s and early 1970s.

Eric Hobsbawm, writing about social history in 1971, noted "the remarkably flourishing state of the field," concluding "it is a good moment to be a social historian" (Hobsbawm 1971, 43). It certainly was a good moment for the large cohort of historians who, like me, had entered graduate school in the 1960s, chosen to write dissertations on social historical topics, and

2. Of course, the great exception to the decline of such movements was the feminist movement, which became more prominent and more militant in the 1970s than it had been in the 1960s. But in this case I could be only a supporter and sympathizer, not a direct participant.

3. As far as I know, there is no proper history of "the new social history" in the United States, although there are pertinent sections in Novick (1988). On the Annales school, see Burke (1990) and Dosse (1994). On the British Marxists, see Kaye (1984, 1988) and Kaye and McClelland (1990).

secured good academic jobs in the rapidly expanding American university system with an ease that now seems almost obscene. Most of this rising generation of social historians were largely self-taught. We sought out dissertation advisors who were sympathetic to the kind of work we wanted to do, but found few who could give us detailed methodological guidance. We educated ourselves in method and theory largely by taking courses in sociology, political science, or economics. There were a handful of older scholars, in American universities and abroad, whose work served as crucial models for our research, but most of us knew them by their books rather than as teachers. I was most influenced by three books published in 1963 and 1964, precisely when I was developing a dissertation topic—E. P. Thompson's *The Making of the English Working Class* (1963), Charles Tilly's *The Vendée* (1964), and Stephan Thernstrom's *Poverty and Progress* (1964). But even most of the "elder statesmen" of the new social history were still quite early in their careers when the 1960s ended. It was the entry into the profession of my cohort of social historians, and the outpouring of our articles and monographs over the course of the 1970s, that secured the ascent of social history in the United States. By the mid-1970s, the new social history had achieved a strong institutional presence and was quickly moving toward hegemony in the profession. One clear sign of the rise of social history was the proliferation of social historical journals. *Comparative Studies in Society and History*, founded in 1958, was the only American journal devoted to social history before 1960. *The Journal of Social History* and *Historical Methods Newsletter* (later *Historical Methods*) appeared in 1967, and in the early 1970s there was a new social history journal virtually every year. These included *The Journal of Interdisciplinary History* in 1970, *International Labor and Working Class History* in 1971, *Peasant Studies* in 1972, *The Journal of Urban History* in 1974, *Social Science History* in 1976, and *The Journal of Family History* in 1976. Moreover, articles on social history also became common in such general journals as the *American Historical Review* and the *Journal of Modern History*, dozens of social history monographs were flowing from the university presses,[4] and social historians were getting tenure and moving rapidly up the ranks in all the major departments.

The rise of social history in the United States not only represented the arrival of a new generation of historians; it also effected a profound and

4. The flood of works in social history can be illustrated by my own field of nineteenth-century French history, which was transformed by the appearance of such books as Bezucha (1974), Johnson (1974), Scott (1974), Moss (1976), Margadant (1979), Judt (1979), Hanagan (1980), Lehning (1980), Smith (1980), Sewell (1980), Aminzade (1981), Moch (1983), and Reddy (1984).

lasting intellectual transformation — something like a paradigm shift — in the field of history. Social history represented a change in subject matter, in methods, and in intellectual style. One of its most significant and lasting achievements was a vast enlargement of the scope of historical study. This enlargement was twofold. First, social history studied categories of people who had previously been ignored by historical scholarship. Rather than political leaders and great thinkers, who had previously been the prime subjects of history, social historians tended to work on the obscure and downtrodden: servants, workers, criminals, women, slaves, shopkeepers, peasants, or children. This interest in the forgotten millions of ordinary people was, clearly, consonant with the populist tendencies of 1960s political activism. Second, rather than concentrating on politics narrowly defined, social history attempted to capture the whole range of ordinary people's life experiences: work, child-rearing, disease, recreation, deviant behavior, kinship, popular religion, sociability, procreation, consumption. Social history thus not only studied new categories of people but asked new questions about them. And in order to answer new questions about new categories of people, it used new forms of evidence. All sorts of records previously not thought to contain information relevant to historical research suddenly became gold mines of documentation. Old census manuscripts, tax registers, wills, advice books, inventories of estates, popular songs, city directories, statutes of mutual aid societies, building permits, records of marriages, baptisms, and deaths: all these and many other kinds of documents yielded evidence about the social structures, institutions, and life experiences of millions of ordinary people.

These new forms of documentation were also subjected to new methods of analysis. A characteristic mark of the new social history was the systematic use of quantitative methods. The kinds of people social historians studied were often illiterate, and even those who could read and write rarely left papers that revealed much about their lives. But such people came into contact with public authorities when they paid taxes or tithes; when they were drafted; when they had contracts notarized; when they registered births, marriages, and deaths; when they got counted by the census or were arrested by the police. It was largely by aggregating the rather thin and stereotypic information contained in the records of such encounters between ordinary people and public authorities that social historians were able to reconstruct the patterns of these otherwise anonymous lives. Quantification as a method of analysis was thus intimately linked to social history's radical expansion in subject matter.

It was from the social sciences that historians borrowed the quantitative methods they applied to these novel data sources. But the borrowing involved far more than a simple transportation of a set of methods: along with the methods came a distinctive theoretical and epistemological outlook. The borrowing of methods was but one aspect of a self-conscious modeling of ourselves and our work on the social sciences. Because we tended to regard what we called "traditional narrative history" as atheoretical and intellectually bankrupt, the neighboring social science fields of sociology, political science, economics, demography, and geography looked very attractive. In the course of the 1950s and 1960s, these fields had become overwhelmingly positivist and quantitative, encouraged by massive federal spending on social science research (Kleinman 1995; Ross 1991; Steinmetz forthcoming; Turner and Turner 1990). By the 1960s, these quantitatively inclined social sciences had high prestige within the academy and seemed far more methodologically rigorous and theoretically sophisticated than history. Not surprisingly, their positivist and objectivist stance was carried over into the new social history.

The various changes introduced by social history were mutually reinforcing—they made up a fairly coherent package, constructing a distinct epistemic object for social history. The new social historians' "social" was above all of what we (following our social scientist friends) called "social structure." Social structures were objective and transpersonal patterns or forces of which actors were at best incompletely aware and that tightly constrained their actions and thoughts. These social structures—occupational distributions, business cycles, demographic patterns, inheritance systems, hierarchies of wealth, urban settlement patterns, systems of land tenure, and the like—left palpable traces in historical records, especially in the quantifiable records that supplied what we called "hard data." We thought of social structures as essentially autonomous from political or intellectual history. Indeed, we often argued that they formed the underlying conditions for, even the determinants of, the political or intellectual developments that historians had previously taken as primary. The distinction between the "hard data" of quantitative history and the "soft" or "impressionistic" data of political and intellectual history subtly implied an underlying ontological distinction between a determining social structure and a determined politics and culture. In short, the rise of social history entailed a redefinition of the primary object of historical knowledge—from politics and ideas to anonymous social structures—as well as the discovery of new means of gaining knowledge about this object.

The new social history paradigm I have just outlined is of course an ideal type. Not every social historian adhered equally to all aspects of this epistemic package. Those strongly influenced by demography or economics, for instance, tended to be particularly enthusiastic about quantitative methods and "hard" data, while those who worked on rebellions and social movements tended to combine quantitative data with verbal accounts culled from archives, memoirs, or newspapers. But for all the internal differences among social historians, we tended to adopt a common front in our struggles for recognition within the field, arguing for the necessity of interdisciplinary borrowing, for the recognition of quantitative methods as part of the historian's toolkit, for the expansion of history's subject matter beyond politics and great ideas, and for recognizing the historical importance of ordinary people's experiences. These arguments were, it seems to me, largely successful; social history did succeed in significantly redefining the object of historical knowledge. Social history, I would even say, briefly became hegemonic in the field in the United States. Although social historians never accounted for a numerical majority, they were hegemonic in the sense that they managed to define the terms of historiographical debate—so that, for example, political and intellectual historians themselves began to ask more social-historical questions and to experiment with the new methods.[5] By the mid-1970s, social history was generally recognized, even by those skeptical of its claims or methods, as the "cutting edge" of historical research.

The relationship of this research program to the political commitments and sentiments of the rising generation of American social historians was complicated, in part because the social historians' political commitments were themselves far from simple. As I have already intimated, quantitative techniques were quite consonant with the strong populist impulses of 1960s radicalism—because they made it possible to carry out detailed studies of classes or categories of the population who were poorly represented in the sources used by more traditional historians. Quantification, in other words, was one important way of pursuing the populist goal of "history from the bottom up." But social historians' politics could also make their embrace of positivist social science significantly ambivalent. It is important to remember that the politics of the 1960s was by no means limited to an upsurge of populism. Sixties politics also featured a powerful

5. For two influential early articles about what was often called "the social history of ideas," see Darnton (1971a, 1971b).

revulsion against the bureaucratic conformity that student radicals saw as characteristic of contemporary American society. Sixties radicalism, especially its "countercultural" moment, must be seen as a rejection of the corporate political and cultural synthesis of "big government, big business, big labor" that became dominant in the 1950s and 1960s—what has since come to be called "Fordism."[6] The term Fordism designates the mode of macrosocial and macroeconomic regulation that underwrote the long postwar economic boom, which stretched from the late 1940s to the early 1970s. The Fordist package combined mass production technologies, relatively high wage levels, stable systems of collective bargaining, Keynsian management of aggregate demand, full employment strategies, welfare state institutions, and highly bureaucratized forms of both public and private management (Aglietta 1979; Amin 1994; Gramsci 1971b; Harvey 1989; Jessop 1992; Lepietz 1987).

From the perspective of the hypercompetitive, predatory, and extraordinarily inegalitarian American capitalism of the early twenty-first century, the Fordist mode of regulation may seem remarkably humane, a kind of quasi social-democratic "world we have lost." But from the point of view of young critics of the system in the 1960s, its benefits (for example, economic stability and steady productivity gains) were hardly noticed. They seemed givens of modernity itself, permanent and unproblematic acquisitions of an irreversible social progress. Meanwhile the defects of Fordist capitalism—especially corporate conformity, bureaucratic monotony, repressive morality, and stultifying forms of mass culture—were highly visible and repugnant, at least to the youthful political intelligentsia who made up the student movement. The countercultural style of the 1960s movements—psychedelic music, consciousness-altering drugs, infatuation with "Eastern" meditative practices, outlandish clothing styles, sexual experimentation—was largely a revolt against the standardization associated with the Fordist mode of socio-economic regulation. And while the leftist political movements and the counterculture were by no means one and the same, it was difficult to participate in the political movements of the era without also exploring and embracing some of the new possibilities offered by the counterculture.

I am convinced that this anti-Fordist strain in the politics of the 1960s endowed many social historians with at least a latent ambivalence about

6. For similar arguments dealing primarily with European social movements of this era, see Hirsch (1983) and Steinmetz (1994).