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978-1-107-67064-8 - The Sources of Social Power: Volume: 2: The Rise of Classes and Nation-States, 1760–1914

Michael Mann

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The sources of social power

VOLUME 2

The rise of classes and nation-states, 1760–1914

Distinguishing four sources of power in human societies – ideological, economic, military, and political – *The Sources of Social Power* traces their interrelations throughout history. This second volume of Michael Mann’s analytical history of social power deals with power relations between the Industrial Revolution and World War I, focusing on France, Great Britain, Hapsburg Austria, Prussia/Germany, and the United States. Based on considerable empirical research, it provides original theories of the rise of nations and nationalism, of class conflict, of the modern state, and of modern militarism. While not afraid to generalize, it also stresses social and historical complexity. Michael Mann sees human society as “a patterned mess” and attempts to provide a sociological theory appropriate to this. This theory culminates in the final chapter, an original explanation of the causes of World War I. First published in 1993, this new edition of Volume 2 includes a new preface by the author examining the impact and legacy of the work.

Michael Mann is Distinguished Professor of Sociology at the University of California, Los Angeles. He is the author of *Power in the 21st Century: Conversations with John Hall* (2011), *Incoherent Empire* (2003), and *Fascists* (Cambridge 2004). His book *The Dark Side of Democracy* (Cambridge 2005) was awarded the Barrington Moore Award of the American Sociological Association for the best book in comparative and historical sociology in 2006.

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MICHAEL MANN

University of California, Los Angeles



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Preface to the new edition

This book is bold and ambitious. It charts and explains the development of power relations in the advanced countries of the world over 150 years and interprets this with the aid of a general theory of power in human societies. Readers of my first volume will be familiar by now with my argument that the development of human societies can be explained in terms of the interrelations of four sources of social power – ideological, economic, military, and political (the IEMP model). These sources generate networks of interaction whose boundaries do not coincide. Instead, they overlap, intersect, entwine, and sometimes fuse, in ways that defy simple or unitary explanations of society given by social scientists. More importantly, they also defy the ability of social actors to fully understand their social situation, and it is that uncertainty which makes human action somewhat unpredictable and which perpetually develops social change.

And yet this book is not as big in scope as my other three volumes. Unlike them, it is not global. One enthusiastic reviewer did begin his review of this one with the word “Colossal!” and ended saying “this volume stands alone for its heroic scope, and the depth of its analysis attests to the author’s vision and determination” (Snyder, 1995: 167). Yet others were disappointed with what they saw as a narrowing of my scope compared to Volume 1. Here I am resolutely focused from beginning to end on Europe and America. I narrowed my focus firstly because in the “long nineteenth century” Europe and its white settler colonies constituted the “leading edge” of power in the world. This was the first period in world history in which one regional civilization came to dominate all four sources of social power across the world – ideological, economic, military, and political. This dominance was not to last long but it was still firmly in place in July 1914 at the end of the period covered by this volume. Yet this volume is even more tightly focused, for it largely ignores the global empires of these Powers. I have been criticized on both counts as being “Eurocentric,” but I feel that this is misplaced for this is avowedly a book about only a part, albeit the most important part, of the world at that time. It was never my intention to ignore the global empires or the globe as a whole, and they are the subject matter of Volumes 3 and 4.

However, in my decision to focus on the leading advanced countries, methodological issues also played a part. I am often asked about my method. I confess to being methodologically unconscious. I just do what

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I do without thinking much about my method. Joseph Bryant (2006) and Tim Jacoby (2004) give a much better explanation of my methodology and my ontology than I could ever provide. However, there are certain practical patterns to what I do. First, I cut down on the range of countries and regions by focusing on the leading edge of power, the most advanced civilizations at any one point in time. I have most obviously done that in Volume 2 where I only discuss the five leading countries in European civilization: Britain, France, Prussia/Germany, Austria-Hungary, and the United States (with Russia playing a more intermittent role).

Second, I then read everything I can on this edge within the limits of my linguistic abilities, but I stop reading when the result becomes simply to add detail or minor qualifications to my argument. I reached this point much sooner for earlier historical periods than later ones because in early history I could read almost everything published. But preparing Volume 2 was a learning experience for me. Even after deciding to focus on a few countries, my aspiration to read even half of what was available on them meant I was spending an inordinate amount of time and writing too much to be able to accomplish my original intent of including imperial history too, and of reaching the present day in my narrative. So I left the empires to Volume 3 (adding the American and Japanese empires), and I only reach up to today in Volume 4.

So with Volume 2 half-finished but already too long, I realized that if I was ever to reach the present day, I had not only to write more volumes but also to be much more selective in my reading. Luckily, technology then came to my aid. The development of online capabilities has added useful shortcuts to my reading task. In Volumes 3 and 4 I have been able to enter a period or problem by searching for relevant online university syllabi. The syllabi give me a sense of what every student is expected to read on the topic and the better ones also give me a preliminary sense of current debates. I then use recent book reviews and review articles in journals available to me online through UCLA's fine library resources to read further on current thinking. I soon learned to greatly prefer the type of book review that states clearly the book's arguments and data to the more self-indulgent review in which the author concentrates on giving his/her own opinions on the topic. Then I read the selected works. This method is probably the reason why I cite more books than journal articles, which I had not realized until Rogers Brubaker pointed it out to me. However, "read" is not always the most appropriate description for my treatment of books, because very often I "pillage" them, glancing through the table of contents and the index for sections that bear on the themes I am pursuing, neglecting the rest. This is a scholarly sin, of course, but it is absolutely necessary in any very general work, given the immensity of today's scholarly production.

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The third aspect of my method in all my volumes has been to continuously zig-zag between theory and data, developing a general idea, then testing and refining it on the historical evidence, then back to theory, then once again to data and so on, and so forth. In one respect here this volume differs from Volume 1. There I had noted that explanations of why Europe pioneered the way to modernity cannot employ the comparative method, because there are no other “pristine” cases of such a breakthrough (Japan’s remarkable breakthrough came through conscious adaptation of European institutions). All one could do was to compare Europe to the one case that might have broken through to industrial capitalism but did not do so – Imperial China. In Volume 2, however, I can deploy the comparative method, because Europe became divided into nation-states, which had enough boundedness and enough similarities and differences in their development to permit a comparative analysis of them. Some readers took my rejection of the comparative method in Volume 1 as being principled. But no, it was pragmatic, and in this volume reality allows me to do comparative research.

Once again, however, this volume expresses a distinctively sociological view of history, one that is more concerned with theoretical questions than is the case among historians, yet is more concerned with history than is the case among sociologists. This is true even in this volume, which does not have great geographical or historical breadth.

Let me state what I consider to be its strengths. I continue here my argument established in Volume 1 that “societies” are not unitary or systemic. Human societies are constituted by power networks – ideological, economic, military, and political power – which do not have the same boundaries. These networks are overlapping, intersecting, and entwining, forming much looser units than most sociologists have accepted. In the period covered in this volume, as I say on page 9, states harden into nation-states with a certain degree of boundedness. But they nonetheless entwine with a broader transnational “Western civilization” which was in a sense competing as a basic membership unit. Thus sociology’s master concept, “society” kept metamorphosing between the nation-state and the civilization. But the similarity and the distinctiveness of each national unit, and the fact that they were erecting what I call “cages” around part of the lives of their subjects/citizens, enabled me to do comparative analysis of them.

These comparisons centre on what I identify as the two main actors of modern times: classes and nation-states. I argue that the two cannot be seen, as is conventional, as utterly separate from each other. Nor are they opposites, the one undercutting the other. Instead, economic and political power relations have developed entwined with each other, influencing rather than undercutting each other’s development.

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Recent trends in the disciplines of sociology and history have served to obscure this. When I began writing this volume, class analysis dominated. What was called “social history” focused overwhelmingly on class relations, and especially on the working class. There was then a reaction against this overemphasis in the form of a general “cultural turn” in which culture took over from the economy as the main object of study. Insofar as classes were discussed at all, this was in terms of discourses, symbolic communication, and the like rather than concrete labour relations or the material means of production. This was one result of the decline of the traditional left in Western society, which was occurring from the 1980s onward. But a new left was also emerging, centred not on class but on “identity” rights, especially those of gender and ethnicity. Writers on gender relations then took much attention away from class analysis, though some were concerned to specify the relations between class and gender. But those focusing on ethnicity virtually ignored class relations, and that has been especially true of those working on nations and nationalism. Thus class and nation have been kept apart, in separate boxes, class predominating at first, then nation, thus obscuring the fact that class and nation have developed together, entwined. It is now conventional, for example, to say that World War I represented the triumph of nation over class. Yet we shall see in both this volume and Volume 3 that their interrelations were far more complex than this.

I believe that this book remains the best treatment available of the development of the modern state. Chapter 3 presents my own theory of the modern state. My notion that states “crystallize” in different forms as a result of both their different functions and the pressure of different constituencies on them is better able to cope with the real-world messiness of political life. Second, my treatment of the five states is rooted in a detailed statistical analysis of their finances and employment records, and on this quantitative basis I can launch into some grand historical generalizations. In the course of this period, the main functions of the state changed radically. At the beginning of the period, its main function was in the financing and the fighting of war. Charles Tilly famously remarked that “war made the state and the state made war” (1975: 42). But I find this was only so in Europe up to the mid-nineteenth century. Nor did I think it likely that either his model or mine would fully apply to other continents. In fact, Centeno (2002) found that it only applied to the history of Latin America in a negative sense. There states rarely made war and they remained puny, and Herbst (2000) says more or less the same thing about postcolonial Africa. So the question there turns to “why did they not make war?” By the end of the century, Western state civilian functions, like building infrastructures, education, public health, and the first stirrings of the welfare state, had emerged to rival warmaking. It

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was now a dual civil-military state, a character it retained during most of the twentieth century, although near the end of that century many states were predominantly pursuing civilian roles. They have lost their historic backbone. We can also see from my data that states developed greater infrastructural power over their territories, even though, surprisingly, their overall financial size was no greater as a proportion of the overall economy than it had been at the beginning of the period – because the growth of the economy was actually slightly greater than the growth of the state. It was not yet a Leviathan, nor was it as bureaucratic as is often assumed. On Sundays, U.S. President Harrison (in office from 1889 to 1893) would open the White House front door himself, because it was the butler's day off.

The third strength of my analysis of political power is the emphasis I place on the rise of the nation-state. This offers further justification of my oft-criticized, unconventional distinction between political and military power. The role of political power relations in this period is more in terms of collective power (power through people) than of distributive power (power over people). The rising costs of war followed by the growth of state infrastructures meant that people and their interaction networks were gradually mobilized into nations. The metaphor I use is that they were “caged” and “naturalized” within the nation-state. This was consequential because social relations – especially class relations – came to vary mainly according to the configuration of political power in each country. Although the economic power relations of capitalism varied across the advanced world, they were less important than national variations in political power in determining the various outcomes of labour conflict.

In the realm of classes, the period of this volume saw the phenomenal growth of a capitalism, which generated the first and the second industrial revolutions and massive economic growth. This led to the development of modern social classes like the capitalist, middle, working, and peasant classes. I focus for much of the time on the relations between workers and capitalists, although I discuss the middle class in Chapter 16 and the peasantry in Chapter 19. I show that the peasantry was capable of much more collective organization than Marx had argued, and that the middle class was very diverse, and not nearly as nationalistic as is often believed. In my book *Fascists* (2004), I show that they were not more susceptible to fascism than were other classes. All these classes were extremely important from the time of the French Revolution to World War I, because industrial capitalism became the fundamental economic power structure of society. Those sociologists who have criticized me for writing at length on class relations (on the grounds that class is passé) do not seem to grasp the realities in the long nineteenth century.

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Yet class relations between workers and their capitalist employers have been ambiguous, in two different senses. First, workers do feel exploited, yet they must cooperate on a daily basis with their employer in order to obtain their daily bread. Thus conflict versus cooperation is a perennial choice for both workers and their employers. Secondly, when workers do organize, three possible forms of solidarity emerge: class solidarity among the working class as a whole, sectional solidarity among workers in a particular trade, and segmental solidarity among workers in a particular enterprise. Here I argue that whether conflict or cooperation predominates and which combination of these three forms conflict takes are explained more by political than economic power relations. Most specifically, the more workers are excluded from sharing in political power, the more likely they are to form class-based organizations, to find plausible the claims of socialists or anarcho-syndicalists, and to be attracted by the prospect of revolution rather than reform. Thus, the ordering in terms of the emergence of class, socialist and revolutionary sentiments runs from Russia, through Austria-Hungary and Germany, to France and Britain, and finally to the United States.

I now turn to considering criticisms and misinterpretations of the volume. Some have interpreted my analysis in variations in class consciousness as my saying that political power relations are more important than economic ones and so they conclude that this book is “state-centric” (e.g., Tarrow, 1994; Mulhall, 1995). I reject this. In my conclusion on page 737, I identify two phases of what I call dual determination. In the first phase, lasting until 1815, economic and military power relations predominated in the structuring of societies. But in the course of the nineteenth century, power shifted and by the end of the century economic and political power relations (capitalism and nation-states) predominated. On the face of it this would seem to give economic power relations some priority, which is not surprising given that these two phases correspond to the onset of the first and second capitalist industrial revolutions. It also implies that the advanced world became *more* state-centric and that is one of my main arguments in this volume. But these dualities are heroic simplifications of a very complex reality, and I should admit that I have always been a little uneasy with them. And comparable heroic simplifications of other times and periods would look rather different.

As far as class relations are concerned, I should point out that it is principally the variations between countries that are more explicable in terms of political power relations. That there was everywhere in this period pronounced labour discontent is explicable in terms of the nature of the economic power relations intrinsic to capitalism, while I also acknowledge that to explain the emergence of sectional and segmental organization, we need to also pay attention to craft and corporate structure. The

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structure of capitalism is obviously also a *necessary* part of any explanation, and when we combine this with political power relations, we have a *sufficient* explanation of class outcomes. But I do not intend to elevate political over economic power in this period.

George Lawson (2006: 491) airs the possibility that my work as a whole contains an implicit hierarchy with military power at the top, followed by political power, then economic power, and finally ideological power. I think this would be a misinterpretation. Given that military power is neglected in most social science, I may mention it too much for most tastes. But my own view is that both military power and ideological power are rather more erratic in their effects than are the other two. They sometimes emerge powerfully in world-historical moments, militarism launching great transformative wars and ideological power turning occasionally transcendent and leading revolutionary changes in the way that people view the world. But otherwise military power stays on the sidelines in the form of a military caste minding its own business. Similarly, for the most part, ideological power largely reproduces dominant power relations (as Marxists argue). In this volume military power was important at the beginning and the very end of the period (except in the colonies, where it was continuously important), and it became more important again in the twentieth century, while ideological power never really matched the heights of the period of the much earlier emergence of the world religions or the heights of twentieth-century secular ideologies. I make more general comments on the interrelations and relative importance of the power sources at the end of Volume 4, but I reject the idea of any simple hierarchy among them.

Within Europe after 1815, this was largely a period of peace, so military power relations actually figure less in this volume than they did in Volume 1 or than they will in Volume 3. Their main entrances are at the beginning and the end. In the latter case we see evidence of the relative autonomy of militaries from civilian state control, and this was important in helping cause World War I. I discuss this in Chapter 21. In Volume 3, I briefly revisit these causes. And I should note that there I added to the explanation of the causes of this war greater emphasis on the thousand-year European tradition of militarism and imperialism. Europeans had long been from Mars. This chapter has received much praise and it is in many ways the clearest vindication of my overall model of human society. As I conclude, on page 796, the war “resulted from the unintended consequences of the interaction of overlapping, intersecting power networks.” No one could control the whole or could predict the reactions of other nations, classes, statesmen, and militaries. That was why in August 1914 a disastrous war began, one that was to ensure the demise of European power, whose rise I had charted in Volume 1. Military power relations

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were also to play a role in the denouement of class relations in the first half of the twentieth century. Only in countries that were effectively defeated in the two world wars were there serious attempts at revolution. This I show in Volume 3. These are examples of my most fundamental point: that we cannot explain major social developments in any period without considering the entwinings of more than a single source of social power. Ideological, economic, military, and political determinisms must all be rejected. However, in this period, having excluded colonies from my purview, military power and political power are closely entwined. In the advanced countries armies are no longer feudal, and paramilitaries and civil wars are rare. The wars discussed here are between states. It is really only the tendencies toward military castes, distinct from the civilian authorities, that maintain the autonomy of military from political power in this place, in this period.

Turning to ideological power, some criticize me for being too materialist, too instrumental, and too rationalist. In principle my model is none of these things, although my practice has sometimes faltered. I prefer the term “ideology” to “culture” or “discourse,” not because I view ideologies as false or a cover for interests, as materialists sometimes say. By ideology I mean only a broad-ranging meaning system that “surpasses experience.” “Culture” and “discourse” are too all-encompassing terms, covering the communication of all beliefs, values, and norms, even sometimes all “ideas” about anything. When used so generally, they presuppose a contrast between only two realms, the “ideal” and the “material,” leading to the traditional debate between idealism and materialism. The material might be conceived of as “nature” as opposed to “culture,” or as the “economic base” versus the “superstructure,” or as joint economic/military interests (as in international relations “realism”) as opposed to “constructivism” – or even as “structure” as opposed to “agency.”

These dualist debates are perennial. After a period dominated by materialist theories of everything, we now have cultural theories of everything. As noted earlier, “nation” and “ethnicity” have largely replaced “class” as objects of research; they are said to be “cultural,” whereas classes are said to be “material”; they are usually discussed without any reference to classes; and “cultural” and “ethno-symbolist” have largely replaced “materialist” theories of nations and ethnicities. Thirty years ago, fascism was explained in relation to capitalism and classes; now it is seen as a “political religion.” My books *Fascists* and *The Dark Side of Democracy: Explaining Ethnic Cleansing* suggest that this is not progress, but a shift among equally one-sided theories.

Nonetheless, I may have given the impression of being a materialist in four different ways.

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(1) I use the word “material” when, to avoid confusion, I should have written “concrete” or “real.” That is just an error of language, not of substance.

(2) I endorse John Hall’s and Perry Anderson’s description of my theory as “organizational materialism,” and this often involves emphasizing the “logistics” and “infrastructures” of ideological power, sometimes at the expense of the content of their doctrines. My originality here lies clearly with the organization of power, and I continue to emphasize that. I also find myself at least as drawn to Durkheim’s emphasis on religious rituals as to Weber’s emphasis on doctrine. Nonetheless, I should not neglect either.

(3) I declare here on page 35 (as I also had in Volume 1, pages 471–2) that ideological power declined through the eighteenth and nineteenth centuries. I still think this is broadly true within the most advanced countries, yet I did not discuss in this volume the major ideology of the period – racism. Lawson (2006: 492) goes further. He suggested to me that I neglect a whole series of nineteenth-century ideologies. He lists racism, Darwinism, colonialism, imperialism, nationalism, Marxism, and liberalism as the main ones. In one sense I do neglect the first four of them. But they form an interrelated group that was largely significant because of Europe and America’s overseas empires. For example, racism was only important in this period in colonies and not mother countries, except for the United States. I do exclude empires from this volume, but I deal extensively with them and with this cluster of ideologies in Volume 3. As for nationalism, Marxism, and liberalism, I think I do discuss them in this volume.

(4) I declare that the extensive power of religion has continued to decline since the nineteenth century in the face of rising secular ideologies like socialism and nationalism. Having subsequently researched twentieth- and twenty-first-century fascism, ethno-nationalism, and religious fundamentalism, I now disown half of this statement. My emphasis on rising secular ideologies is correct, but I accept Gorski’s (2006) criticism that religion has not generally declined in the world. I was generalizing only on the basis of traditional Christian faiths in Europe, which indeed still are declining, although much of the rest of the world differs. More specific criticisms with some force are that I have sometimes been too rationalistic about religions in earlier periods, and that I neglected the religious content of eighteenth-century politics (Bryant, 2006; Trentmann, 2006). Edgar Kiser (2006) is also right to see me as trying to lessen the rationalism and moving toward greater recognition of value- and emotion-driven behaviour in my later work on fascism (2004) and ethnic cleansing (2005).

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My model of power ultimately abandons the distinction between ideas and materiality in favour of one between “ideas and practices combined” (or “action and structure combined”) in each of four power networks. Nonetheless, ideological power is clearly more idea-heavy than the others. It comprises networks of persons bearing ideologies that cannot be proved true or false, couched at a sufficient level of generality to be able to give “meaning” to a range of human actions in the world – as religions, socialism, and nationalism all do, for example. They also contain norms, rules of interpersonal conduct that are “sacred,” strengthening conceptions of collective interest and cooperation, reinforced, as Durkheim said, by rituals binding people together in repeated affirmations of their commonality. So those offering plausible ideologies can mobilize social movements and wield a general power in human societies analogous to powers yielded by control over economic, military, and political power resources. This is when ideology is what I call “transcendent,” for it cuts right through institutionalized practices of economic, military, and political power.

The period discussed in this volume is not one of major ideologies. I hope that in this volume, ideological power autonomy comes through in my conception of an “ideological power elite” steering the direction of the French Revolution in Chapter 6. Elsewhere in this volume I stress that European states sometimes crystallize in terms of religious disputations, but if I do not deal extensively elsewhere in this volume with religion, it is because I believe that, with the exception of racism (which I discuss extensively in Volume 3), Europe did not see much ideological power in this period and place. Religion was declining and the great twentieth-century ideologies of nationalism, socialism, and fascism were just beginning to stir. Though people were beginning caged within the nation, nationalism was still a rather shallow emotion among the working and middle classes, becoming virulent (I argue in Chapter 16) largely among those deriving their employment from the state. I do not claim to discuss *all* ideas, values, norms, and rituals, only those mobilized in macro-power struggles. Schroeder (2006) gives my defence of this neglect: ideas cannot *do* anything unless they are organized. This is why the label “organizational materialism” still seems partly apposite, whatever the economic images it might set up in the reader’s mind, for ideas are not free-floating. Nor are economic acquisition, violence, or political regulation – they all need organizing. But maybe I should drop the word “materialism” and just say that I have an organizational model of power and society.

I must acknowledge one final omission: the absence of gender relations from this book. I admit on page 34 that I have omitted in this volume the more intimate aspects of human life. To a certain extent I repair this neglect in Volumes 3 and 4, although I doubt if this extent will satisfy my

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critics. In the end, my defence against this charge of neglect is only that I cannot do everything! But I think you will agree that I do a lot of things in this book.

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This is the second volume of what is intended as a four-volume study of the sources of social power. It delivers, however, only 63 percent of the coverage promised in Volume 1, ending in 1914, not in 1990, as I announced there. Volume 3 will cover the twentieth century (perhaps the whole century, by the time I finish). The theoretical conclusion to *The Sources of Social Power* will be Volume 4. I hope all who have expressed interest in my conclusions will still be around then.

I have worked on the research for this volume for more than a decade, beginning in the mid-1970s, when I believed *Sources* would be one normal-sized book. Over the years, I have benefited from the labors, advice, and criticism of many. Roland Axtmann and Mark Stephens helped me collect the comparative statistics in Chapter 11, and Mark also aided me with Chapter 5. Jill Stein helped to collect data on the French revolutionaries for Chapter 6. Ann Kane contributed substantially to Chapter 19, as well as elsewhere, especially Chapter 16. Marjolein 't Hart, John Hobson, and John B. Legler showed me unpublished data for Chapter 11. Joyce Appleby and Gary Nash set me almost straight about the American Revolution; Ed Berenson and Ted Margadant, about the French Revolution; James Cronin and Patrick Joyce, about British labor history; and Kenneth Barkin and Geoff Eley, about German history. Christopher Dandeker commented generously on Chapter 12; Ronen Palan, on Chapters 3, 8, and 20; and Anthony Smith, on Chapter 7. John Stephens was extraordinarily helpful for Chapters 18 and 19. Randall Collins and Bill Domhoff have been helpful in their responses to both volumes. I also thank an anonymous reviewer of the first draft of this book. His or her critique forced me to clarify some of my central ideas.

I thank the London School of Economics and Political Science and the University of California at Los Angeles for providing me with supportive working environments over the last decade. Both also provided seminar series whose excellent discussions helped me clarify many ideas. The LSE Patterns of History seminar flourished principally because of the excitement provided by Ernest Gellner and John A. Hall; the seminars of the UCLA Center for Social Theory and Comparative History have depended especially on Bob Brenner and Perry Anderson. My secretaries, Yvonne Brown in London and Ke-Sook Kim, Linda Kiang, and Alisa Rabin in Los Angeles, have treated me and my work perhaps better than we deserve.

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I owe the greatest intellectual debt to John A. Hall, who has continued for many years to provide me with perceptive criticisms entwined with warm friendship. To Nicky Hart and to our children, Louise, Gareth, and Laura, I owe love and perspective.