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Michael Mann
Frontmatter
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The sources of social power

VOLUME 1

A history of power from the beginning to AD 1760

Distinguishing four sources of power in human societies – ideological, economic, military, and political – *The Sources of Social Power* traces their interrelations throughout history. In this first volume, Michael Mann examines interrelations between these elements from neolithic times, through ancient Near Eastern civilizations, the classical Mediterranean age, and medieval Europe, up to just before the Industrial Revolution in England. It offers explanations of the emergence of the state and social stratification; of city-states, militaristic empires, and the persistent interaction between them; of the world salvation religions; and of the particular dynamism of medieval and early modern Europe. It ends by generalizing about the nature of overall social development, the varying forms of social cohesion, and the role of classes and class struggle in history. First published in 1986, this new edition of Volume 1 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 2004) 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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Contents

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More information

Preface to the new edition

This book presents a model for explaining the development of power relations in human societies and then applies it to human prehistory and most of history too. This was not an uncommon enterprise among nineteenth-century writers, but in today's academe it seems absurdly ambitious. It would have seemed absurd to me early on in my career. My early work gave little hint that I might later engage in such an enterprise. My doctoral dissertation at Oxford had been an empirical study of a corporation relocating its factory within England. It involved interviewing 300 employees, twice. I followed this (in collaboration with Robert Blackburn) with a study of a labour market, the town of Peterborough in England. This involved a larger interview survey of more than 900 workers, as well as the construction of job evaluation scores based on my observation of their jobs. Both projects were contemporary, highly empirical, and quantitative. I then broadened my scope by writing a short book on class consciousness, the product of what was intended to be a large empirical study on labour relations in four countries, together with teams from three other countries. But this was not accomplished since research funds were not forthcoming.

But it was teaching sociological theory at Essex University that radically shifted my trajectory. Reading Marx and Weber carefully, a week or two ahead of the students, gave me the idea of comparing and critiquing their "three-dimensional" models of social stratification – Weber's class, status, and party and Marx's economic, ideological, and political levels (as seen through the eyes of the structural Marxists of the time). At the same time my political engagement led me to reject the conventional leftist view of many of my friends that the nuclear arms race was somehow a product of a struggle between capitalism and communism. I thought instead that it had more parallels with other Great Power struggles of history. Thinking about this led me toward separating military power from political power, and so I arrived at the model of four underlying sources of social power – ideological, economic, military, and political – which has subsequently underpinned all my work. The book I initially hoped to write was intended to be mainly theoretical, albeit buttressed by three empirical case studies on the Roman Empire, feudal Europe, and contemporary societies. A typical chapter would have been the paper I published as "States Ancient and Modern" (1977), largely theoretical though backed up by a smattering of historical knowledge. How differently would my writings have developed, I wonder, if the book intended had been the book actually written?

Yet as I set to work on my case studies, they grew and grew, and they expanded across both time and space. I realized I was still a deeply empirical sociologist but one who loved reading history. Combined, these two qualities conspired to produce a manuscript that was becoming no less than a narrative of power through most of history. It became much too large for a single book and I split it into two volumes, with the division between them set at about the time of the Industrial Revolution. This I did, and soon after I finished the first of what proved to be four volumes of *The Sources of Social Power*. Volume 2, published in 1993, was subtitled *The Rise of Classes and Nation-States: 1760–1914*, Volume 3, published in 2012, is subtitled *Global Empires and Revolution: 1890–1945*, and Volume 4: *Globalizations, 1945–2012*, will be published in 2013. So this has been my life's work. Even my books *Fascists* (2004) and *The Dark Side of Democracy: Explaining Ethnic Cleansing* (2004) were expansions of chapters originally intended for *Sources*. I am quite content to be chained to *Sources*, although I do sometimes wonder what I might have achieved had it been otherwise.

The concrete method I developed for my four volumes was quite simple. First, I cut down on the range of countries and regions studied by focusing on what I called the "leading edge of power," the most advanced civilizations at any one point in time. Second, I then read everything I could on them, within the limits of my linguistic abilities, until the result of new reading was to simply add detail or minor qualifications to my narrative. This was reached much sooner for earlier than later periods, because in early history I could read almost everything published on my leading edges. I do not claim that I always did this, for there are sections in this book that are really just linking passages in the narrative between more fully researched sections. This is true of the chapter on Greece and Phoenicia, for example, although I hope this is compensated for by the strength of the main theoretical concept used there – "multi-power-actor civilizations." Third, I continuously zig-zagged between theory and data, developing a general idea, then refining it on the historical evidence, then back to theory, then once again to data, and so on, and so forth. This made for a distinctively sociological view of history, one that is more concerned with theoretical questions than is the case among historians, yet more concerned with history than is the case among sociologists.

My methods made me fear that when Volume 1 was published it might fall between disciplinary schools and no one would read it, so I contacted several colleagues in the United States and arranged to give a series of lectures given at various campuses. By the time I gave these lectures, there was in fact no longer a need for this purpose because the book was already receiving glowing reviews. But they came to serve another purpose. My American hosts wrongly assumed I was looking for a job, and two universities offered both my wife and myself attractive posts. It was February in London, and Los Angeles was an alluring alternative – warm, relaxed, and unexpectedly beautiful. We thought we might only stay a year, just enough to sample the delights of Southern California, but we have so far remained at UCLA for twenty-five years. Once again, *Sources* changed my life.

But my initial fears were partly justified because my volumes have received intermittent flak from both sides, from historians complaining that the theory gets in the way of a good narrative and from social science positivists complaining that I should be rigorously testing hypotheses derived from general theories and that my method prevents us from forming universal laws and causal universals. I accept neither criticism. The problem is, on the one hand, that empirical data do not make sense on their own. We need to import theories to give them meaning. Historians usually do this implicitly; I prefer to be explicit. On the other hand, positivists' theories always prove to be much simpler than social reality, a fact that not only my own historical research but also everyone else's reveals. There are no propositions that are valid across all societies other than utter banalities. Social reality is complex enough to defeat all human beings' attempts to fully comprehend their situation and so this also defeats the rational choice theory advocated by some positivists. That is why I offer more of a model than a hard theory – a way of looking at the world, an injunction to make sure we have considered all four sources of social power, that we recognize the dangers of holistic, totalized, and rational-choice theories, and a series of generalizations that apply to some times and places but not all (Bryant, 2006a, produces a good and full defence of my methodology).

I have often been labelled "Neo-Weberian," meaning that I derive my inspiration from Max Weber. The highest praise I have ever received is John Hall's (2011: 1) comment that I am "our generation's Max Weber." In one respect only I claim superiority to Weber: my style is easier to read! Perry Anderson, after a long and sometimes critical analysis of this book, concluded "No lesser than *Economy and Society* itself in analytical stature, it is superior as literature" (1992: 86). This is either high praise or a backhander (is my book fiction?). I accept that there are many points of similarity between Weber and myself. Weber tried to devise a methodology that could steer between nomothetic (lawlike) and idiographic (recognizing the uniqueness of all situations) aspects of social life, through concepts like the ideal-type, *verstehen* (interpretative understanding), and principled multicausality. He saw societies as generated by social interaction rather than by either individual agency or determining social structures. I also try to steer through the middle, if in a zig-zagging motion. Weber, like me, had reservations about the notion of "society." He rarely used the word, preferring the plural term "societal domains" (although I did not realize this until reading Kalberg, 1994). Weber clearly thought of multiple domains, even though he never listed them, and he would have probably regarded my four types of power as too limiting (his own three forms of power were devised only as useful ideal-types in particular contexts, not as universals). Weber also found that social complexity required him to be constantly inventing new concepts, and my critics say that I do this too. Jacoby (2004) has also noted that I add complexity by proliferating concepts that are dual, like transcendent and immanent ideologies, or the two organizational forms of military power (tight hierarchy and comradeship within the military, highly diffuse striking-range beyond). Overall, Weber offered us tools for dealing with societies that are always more complex than our theories, and I try to do the same.

William Sewell has a similar approach. He says sociological explanation must centre on what he calls "eventful temporality." "Social life," he says, "may be conceptualized as being composed of countless happenings or encounters in which persons and groups of persons engage in social action. Their actions are constrained and enabled by the constitutive structures of their societies. … Events may be defined as that relatively rare subclass of happenings that significantly transforms structures. An eventful conception of temporality, therefore, is one that takes into account the transformations of structures by events." He analyzes my "brave and powerful book" and declares it to be an exemplary case of "eventful temporality" (Sewell, 2005: 100, 114–23). Of course, the notion that we should merely "take into account" event-driven transformations is not very controversial, but I think Sewell means more than this. In an apparently similar vein, on page 3 of this book I call my account of social change "neo-episodic," meaning that change comes in intermittent bursts of major structural transformation. Like Sewell, I oppose structural determinism because I see "structures" as the outcome of collective actors, groups forming around the distribution of power resources. I see neo-episodic change as often emerging from the unintended consequence of action, from unexpected outside events, and sometimes indeed from accidents. Sewell is also right that I oppose teleological and evolutionary theories: there is no necessary development of human societies, no underlying evolution from lower to higher forms. Yet on the other hand, I accept that there has been an unsteady growth of human collective powers through history, not yet reversed, although different parts of world have provided the leading edge of development at different times. This is because, once invented and adopted, innovations that extend human collective powers, like literacy, coinage, or fossil fuel power, almost never disappear. And I now see "episodes" (Sewell's "eventful transformations") in a slightly different light. For what happens at major points of change is a series of conjunctions between causal chains, some of which are novel and "interstitial" (emerging between existing power structures), but others of which derive from deep-rooted institutions that are themselves changing, albeit at a much slower pace. A typical example of this would be capitalism, which is continually in a state of change. This resembles what most sociologists call "structure," and we cannot really abolish it from our theories. The label I gave for my model, "structural symbolic interactionism," remains appropriate for it indicates this combination of creative group action and institutional development. Thus while some conjunctions between interstitial emergence and existing institutions seem fairly accidental, others seem far more persistent and probabilistic, the consequence of many, many persons and actions over a long period of time. I explain this more when I later turn to a major example of change given in this volume – the "European Miracle." It is worth noting that economic and, to a lesser extent, political power relations are usually closer to being structural than are military and especially ideological power relations.

In this book I begin by being rather disloyal to sociological conventional wisdom because I immediately attack sociology's foundational notion of "society." I was not the only one to do this. Immanuel Wallerstein also rejected the conventional equation of "society" with the nation-state. He argued that in modern times, nation-states were embedded in broader networks of interaction constituted by the "world system," which he identified with capitalism. My alternative is more radical. I argue that social groups form around the social networks emanating from four power sources, yet these networks have rarely coincided with each other in any period of history. Thus human society is composed of multiple, overlapping, and intersecting networks of interaction. There is no such thing as a single, whole society, segregated from others. I oppose all systems theory, all holism, all attempts to reify "societies." There is no singular "French society" or "American society" (for these are only nation-states), nor is there an "industrial' or a "post-industrial society," no "world system," no singular process of globalization, no multi-state "system" dominated by a singular "realist" logic, no logic of patriarchy. History does not have a fundamental unity conferred by the history of class struggle or modes of production, or of "epistemes" or "discursive formations," cultural codes, or underlying structures of thought governing the language, values, science, and practices of an era, and all this is not underpinned by a singular process of power enveloping all human activity. All these offer networks with only a limited degree of boundedness. It is possible to identify a "logic" of capitalism or of patriarchy or of multi-state relations, provided these are recognized as being ideal types, for they are all in interaction with each other, and this interaction changes their natures in ways that are often unpredictable. Yet this model enables us to identify the root of social change, because power organizations can never be entirely institutionalized or insulated from influences coming "interstitially" from cracks within and between them. Social change results from interaction between the institutionalization of old and the interstitial emergence of new power networks.

Since I first developed my IEMP model of power in Chapter 1 of this volume, I have fairly consistently held onto it. At its simplest level the model implies that anyone dealing with macro-issues in the social sciences or history should explicitly consider the causal contributions made to overall outcomes by all four power sources: ideological, economic, political, and military relationships. None should be initially neglected, although one or two may often prove to be relatively unimportant in particular cases. In every historical period I have tried to consider the relative strength of each in causing important outcomes. Sometimes one power source will prove decisive, sometimes another, but most often it is configurations of more than one source that matter most. This obviously involves a multidisciplinary approach to social development such as was practiced by the classic theorists of the eighteenth and nineteenth centuries. But now, alas, I must fight against the extraordinary strength of disciplinary boundaries in academe – and also against the timidity of sociology and history, which should be ambitious and multidisciplinary but are usually not. Nonetheless, in the field of comparative-historical sociology my model and my overall generalizations have had considerable influence (Anderson, 1992: Chap. 4; Smith, 1991: 121–30; Crow, 1997: Chap. 1).

Economic power relations are not often neglected in either history or sociology. In our rather materialistic era, they have been done to death by vast numbers of scholars, while the "cultural turn" has in recent years brought ideological power to the fore, and we can always rely on political scientists to emphasize political power. Military power has been relegated to two small and neglected subgroups: military historians and sociologists of the military. So it has been an important part of my work to demonstrate just how important military organization and wars have been to the development of human society. We have just left a century that has seen perhaps more devastating wars over the world than any other (we should dismiss as absurd the "millions" of casualties sometimes given in the annals of early history). Yet such modern wars are still generally treated as exceptions, interludes, in the processes of globalization and capitalist development, with little impact on ideologies. How wrong that is! As Volume 3 will show, neither communism nor fascism would have become important in the world without World Wars I and II.

I have made a few amendments to my model. I have already mentioned my qualification of its "neo-episodic" character. The other important modification concerns military power. I have been sometimes criticized for separating military from political power, thus departing from sociological orthodoxy (e.g., Poggi, 2001; Anderson, 1992: 77). Although I reject this criticism, I have tried to make the separation clearer by slightly redefining military power. In this volume I defined military power as "the social organization of physical force in the form of concentrated coercion." I later realized that "coercion" was not strong enough. Webster's dictionary allows "coerce" to mean "compel to an act or choice" or "bring about by force or threat." This could refer to workers threatened with dismissal, or priests cowed into silence by their bishops, neither of which involve any military power. So I redefined military power as *the social organization of concentrated lethal violence*. "Concentrated" means mobilized and focused, "lethal" means deadly. Webster defines "violence" as "exertion of physical force so as to injure or abuse" or "intense, turbulent, or furious and often destructive action or force." These are the senses I wish to convey: military force is focused, physical, furious, lethal violence. This is why it evokes the psychological emotion and physiological symptoms of fear, as we confront the serious possibility of agonizing pain, dismemberment, or death. Military power holders say "If you resist, you die." Military power is not confined to armies. Organized, lethal violence also comes from gangs of terrorists, paramilitaries, or criminals.

This makes clearer the distinction I want to make between military and political power. I continue to define political power as centralized, territorial regulation of social life. Only the state has this centralized-territorial spatial form (here I clearly deviate from Weber, who located political power, or "parties," in any organization, not just states). Routine regulation and coordination exercised from the centre through territories, rather than either legitimacy (ideology) or violence (military), are the key functions of the state, exercised through law and rule-governed political deliberations in centralized courts, councils, assemblies, and ministries. So in some ways political power is the very opposite of military power. It is confined, not expansive; institutionalized, not arbitrary.

There are three possible ripostes to this. First, says Perry Anderson (1992: 77), the state has no distinctive form of power of its own: its power rests on a mixture of force and belief. Yet the same could be said of the power of landlords or capitalists over their peasants and workers. If the riposte to this is that landlords and capitalists control or own the means of production, then one can say that sovereignty buttressed by law (which Anderson rightly says I neglect in this volume) gives those who control the state "ownership" of social relations within its territories. Elsewhere, he added a third precondition of state power, saying that "political regulation is scarcely conceivable without the resources of armed coercion, fiscal revenue and ideal legitimation" (Anderson, 1990: 61). This is true. In this volume we see that states have not always been present in human society. They were created through particular configurations of ideological, economic, and military power. But the important point is that once they have been created, they have "emergent" properties of their own, subsequently constraining social life in significant ways. In this volume and also in Volume 2, their most significant power has been to "cage" much of social life within their sovereign territories. That is not reducible to ideological, economic, and military power relations. It is an emergent property of political power (cf. Bryant, 2006a: 77–8).

Second, it can be said that behind law and regulation lies physical force. Indeed, Poggi (2001: 30–1) identifies states, not militaries, with lethality, fear, and terror (which I find bizarre). Yet in most states physical force is rarely mobilized into lethal action, and when states do turn more violent, this is usually through graded escalations. Police may first employ nonlethal riot tactics, causing injuries but rarely deaths. Then mixed police, paramilitary, and army units may escalate into shows of force, shooting in the air and brandishing low-lethality weapons – clubs, tear gas, rubber bullets, the blunt edge of cavalry sabers, carbines rather than automatic weapons, and so on. If that fails to work, the armed forces may take over, exacting exemplary repression by killing as ruthlessly as they consider necessary. This sequence involves escalation from political through mixed to military power relations. However, the most violent states do leap right over any divide between political and military power. Nazis, Stalinists, Maoists, and Catholic Grand Inquisitors killed large numbers of people whose only crime was being defined as possessing an "enemy" identity (as Jew, kulak, landlord, heretic, etc.). Legal forms were phoney. These cases might seem to vindicate Poggi, and indeed these are cases where political and military power have become fused. But all the power sources are sometimes fused into each other. Economic and political fused and blurred in the Soviet state, for example. But these cases do not negate the utility of distinguishing between political and economic power. Nor does the existence of a few very violent states negate the division between political and military power.

The third riposte is that states themselves deploy armies and these are usually the most powerful armed forces. That has been true in many contexts. Nonetheless, even there civil and military administrations are normally separated, military castes and military coups reveal some power autonomy, and many armed forces are not organized by states. Most tribal militaries were stateless, while most feudal levies, knightly orders, private merchant armies (like the British East India Company), and most insurgent and guerilla forces have been substantially independent of states (Jacoby, 2004: 408). Most terrorists today are stateless, as are bandits and criminal and youth gangs. Such military formations are widespread across the world today, enjoying great success in challenging the armies of states. Only rarely since World War II have state armies defeated guerrillas. Indeed in this period, wars between states have declined almost to zero, and civil wars form the vast majority of wars and casualties. Finally, military power conquers new territories, whereas political power can only rule within. It is therefore useful to separate military power from political power.

Another amendment I have made is to make the placing of geopolitical power a little clearer. I have followed the conventional distinction made by political scientists between "hard" and "soft" geopolitics. Hard geopolitics concern matters of war, threatening diplomacy, and military alliances. They are primarily an extension of military power as wielded by states. Soft geopolitics concern peaceful diplomacy negotiating agreements over economic, judicial, educational, and other matters, and they are an extension of political power relations. Of course, as I emphasize in all my volumes, geopolitics are not the only form of power network that goes beyond the boundaries of states. In extra-state relations, alongside international relations lie transnational relations – especially ideological and economic, but sometimes also military – which diffuse right through the boundaries of states. It is worth stressing this because recent writers in the discipline of international relations misinterpret me when identifying me with traditional realism in their discipline. They assert that in going beyond national societies I have emphasized geopolitical relations, especially their "hard side," which is dominated by military power relations. This is not true, for the geopolitical is only one component of extra-state space. When John Hobson says that my theory contains the "potentiality" of avoiding this trap through my notion of ideological power, he seems to ignore the fact that I often do use ideological power in exactly this way. The most powerful "transcendent" ideologies diffuse right through political boundaries – as, of course, do many economic power relations, which he also ignores (Hobson, 2006; for parallel misplaced discussions in IR, see Lapointe & Dufour, 2011).

I remain proud of the scope of this volume. I like the insight that for more than 90 percent of their existence on earth, human groups sought to prevent the emergence of states. I like my argument that only rarely, and because of particular circumstances, did human groups "break through" to states and civilizations. The dialectic I identify between empires of domination and multi-power-actor civilizations has considerable explanatory power, as does my refining of Weber's dialectic of feudal versus patrimonial regimes. I remain proud of my logistical calculations concerning the military campaigns of early empires and of my fiscal calculations of English state expenditures over no fewer than seven centuries, even if these pioneering ventures can obviously be improved upon by further empirical studies. I remain attached to my notion of the "legionary economy" of Rome, whereby military power contributed to Roman economic development (which I consider a rare event). I continue to stress the existence of a basic level of normative consensus within medieval Europe provided by common membership in the Christian ecumene. I think that I provide a very good explanation of the "European Miracle," the burst of the capitalist agricultural and industrial revolutions, which brought Europe to economic riches and global dominance. I will defend this more in a moment.

Obviously, there are mistakes. I finished this volume almost thirty years ago and would now change various detailed arguments in the light of subsequent scholarship. I recognize my mistake in persistently using the rising productivity of land (rather than labour) as a fundamental measure of economic development, although we cannot get such data for most historical periods and where we can, the two measures seem to produce quite similar results. More generally, I do not always live up to the demands of my own model. This should compel me to always discuss all four sources of social power when dealing with all times and places. But juggling four balls at once throughout world history is very hard to do, and I drop one of them from time to time. Most of my critics say I am prone to fumble ideological power, which in this volume is mainly religious. They say that I minimize it or make it too rationalistic, neglecting the heavy emotional commitments it involves (Bryant, 2006a; Gorski, 2006). I think they have a point, and I correct this in Volume 3 when dealing with modern ideologies. Some also say that I do not consistently give ideology adequate treatment (Hobson, 2006). I accept the fact that I give it erratic treatment, but this is deliberate because I argue that ideological power plays a highly erratic role in human development. In this volume I emphasize its role in ancient Mesopotamia and then in Greece in giving a degree of civilizational unity to a region of multiple city-states. I emphasize it again in the fall of the Roman Empire, and then again in medieval Europe. In these contexts, religion had what I call a "transcendent role." In between times, however, religions tended to reproduce existing power structures and so had less autonomous power. So I treat ideological power erratically because it is exercised erratically.

It is also true that when I describe medieval Europe, I tend to reify "Christendom" as a civilization somewhat set apart from others. I demonstrate that Western Christendom was a real network of interaction, but I underestimate the extent of its links with Islam and Asia, to say nothing of Eastern Orthodox Christianity (which Anderson notes). Hobson (2004) has presented an impressive list of early modern European scientific and technological inventions that were imported from China or adapted from Chinese prototypes. He seeks to expose the Eurocentrism of most accounts of the European breakthrough to modernity, and here I think I show some culpability. I also plead guilty to downplaying Arab science, trade, and modes of warfare. This is the aspect of the book that I would most like to amend.

Apart from that error, however, I would defend my analysis of the rise of Europe against the accusation that it is too "Eurocentric." Blaut (2000) has called me one of eight "Eurocentric historians" – a doubly mistaken label! Clearly, having discussed much of the world in earlier periods, I do then home in on Europe. But that is because at the end of this volume the Europeans were conquering the Earth. That is one reason for being "Eurocentric" in this period. I locate the European dynamic deep in the social structure and history of the continent, which is a second reason for being "Eurocentric."

Yet a vigorous debate has erupted since I wrote this volume over whether the "European Miracle" was as deeply embedded as I and many others (ever since Weber) have asserted. Revisionist scholars have claimed that only in the nineteenth century did the European economy – more specifically, the British economy – overtake the Asian economy, specifically that of China's most advanced region, the lower Yangtze. The "great divergence" began in the nineteenth century, they say, for in the eighteenth century the two continents and regions were broadly level. Before then, Asia and China had been much more advanced, but in the eighteenth century both were similarly caught in the Adam Smith "high equilibrium" trap of agrarian economies. "Smithian" development could extend the division of labour and extend markets, but without major technological or institutional breakthroughs, no further development was possible. They say that only the technology and institutions of the Industrial Revolution, acquired first by England from 1800, enabled it and then Europe to surge forward into global dominance. They then explain this breakthrough in terms of two "happy accidents." First, Britain (unlike China) happened to have coal deposits located nearby its industry, thus reducing the costs of industrialization and enabling technological virtuous cycles to develop between its industries. Second, Britain forcibly acquired New World colonies that happened to provide sugar, timber, cotton, and silver, boosting its domestic economy and living standards and specifically enabling it to trade with Asia. So it was military violence, in which Europeans excelled, rather than economic/technological ingenuity, that enabled their eventual dominance of the world. The revisionists reject the view that Europe and Britain possessed a deep-rooted dynamic that more persistently led toward breakthrough (Pomerantz, 2000; Frank, 1998; Wong, 1998).

I will here briefly defend my "deep-rooted" argument (I do it at greater length in Mann, 2006). As Bryant (2006b) has observed, almost all sociologists (and historians) would regard the revisionist argument as implausible. Major social changes result from a whole complex of causes, not from just two accidents. Indeed, many of the revisionists' arguments are mistaken. I start with demography and the "moment" of overtaking. The revisionists say that the Chinese data indicate that China was at least level with England through the eighteenth and into the beginning of the nineteenth century. China had achieved over the previous few centuries a massive population growth with no apparent rise in mortality rates. China also practiced population controls such as infanticide, lesser and later marriage rates, and smaller family sizes. English data also show a massive population growth, a doubling of the population in a shorter space of time, between 1740 and 1820, but the crucial difference is that this was coupled with a complete absence of famines. Indeed, by 1700, the relationship between food prices and mortality rates, already weak, had disappeared. In contrast, Lee and Feng (1999: 45, 110–13) concede that there were famines and a strong relationship between grain prices and mortality rates in eighteenth-century China. Malthusian crises had been already banished in England but not in China. Kent Deng (2003) concludes that China but not England was still stuck within normal "Smithian" agrarian cycles. He dates the "great divergence" between Europe and China as occurring demographically before 1700.

The revisionists respond by saying that without subsequent industrialization, England would have reached the high point of a Smithian agrarian cycle and then slipped back again as overcropping and environmental degradation put a brake on living standards, nutrition, and fertility. But this is countered by Brenner and Isett (2002), who show that there was also a big increase in labour productivity in early eighteenth-century England, enabling the urban population to double without a decline in national health. This was unique, the first shift out of Smithian cycles, the fruits of a capitalist revolution in agriculture. Britain could expand agriculture yet also release labour from it. China could not.

Industry was also emerging to absorb the released labour. The conversion of coal into steam power was the energy core of the Industrial Revolution, and the revisionists say that coal was a happy accident, abundant near English emerging industries, whereas in China coal was abundant but far from the areas that might have industrialized. There is some debate over whether this contrast is accurate, and it is not clear who has won the argument. But even if it were true, England's good luck had come early. Even by 1700, England produced five times as much coal as the rest of the world put together, fifty times as much as China, and coal was fuelling all its industries. European capital markets were also more developed than their Chinese counterparts. Europeans could borrow more and at longer term and lower rates than the Chinese. Whereas Chinese interest rates were typically 8–10 percent, European rates were at this level as early as the fourteenth century, and they were down to 3–4 percent by the mid-eighteenth century (Epstein, 2000). This suggests that Europe had more secure fiscal arrangements and property rights before 1700.

The second "happy accident" relied on by the revisionists was the acquisition of colonies by the Europeans, for this gave them valuable resources, especially silver, timber, and food. Colonies did bring some economic benefit. The silver enabled Europe to trade with China, and new crops benefited diets and calorific intake. Yet O'Brien (2003) estimates that trade with the New World boosted British resources by only about 1 percent of GDP per annum, which is something but not a lot. These were causes but not the most major ones. And as we shall see, colonialism was far from being accidental.

There could have been no single "moment of overtaking," for the different sources of power had different rhythms. Science, Protestantism, and militarism came earlier than the breakthrough to industry, for example. I stress in this book the different rhythms yet long-run cumulation of ideological, economic, military, and political power development. But I disassociate myself from some of the notions of European/British "superiority" evinced by writers like David Landes (1998) and Eric Jones (2002). In this overtaking, efficiency was subordinated to power, and virtue played no part. Natives across much of the world would have been better off without the British Empire, as I shall demonstrate in Volume 3. I also agree with the revisionists that global dominance was acquired not by a broad-based superiority but by a decided edge in military power. Yet this was also deep-rooted, honed on centuries of warfare within Europe, which, following Bartlett (1993), I now see as a process of imperialism and colonialism in which bigger or better-organized militaries and states swallowed up the lesser fry. The victors developed an "intensive" form of warfare based on concentrated, lethal firepower, which they turned to good/bad account when they then expanded overseas. This happened first through the cannonry of naval vessels, but later on through land warfare in which armies were furnished with handguns and artillery batteries. Their concentrated firepower could defeat Asian armies many times their size. Europeans had become better at killing people and so at overcoming other civilizations. I explain this process fully in the second chapter of Volume 3.

The European victory then changed the parameters of economic efficiency, as militarism has done from ancient times. In this case militarism generated an international economy not of free trade but of trade and land monopolies won by lethal violence. Militarism helped bring global domination, and with it the power to restructure the international economy. Exterminating the natives in colonies in the temperate zones, and replacing them with white settlers, brought economic institutions that boosted per capita GDP there, so say modern economists (a very macabre calculation, given that "per capita" means by each surviving person's head – the heads measured did not include dead native ones). So Pomerantz, Frank, and Hobson are right to emphasize the importance of military power to European dominance, but this again means they must recognize that this militarism was neither accidental nor late, but deeply rooted in European social structure, repeatedly exercised, first against other Europeans and then across the world. Which European country would conquer which overseas territories was more contingent – sometimes it was accidental – but that some and eventually all of them would acquire empires eventually became more-or-less inevitable. It took centuries for European economic relations of production to become fully capitalist. It took centuries for European warfare to become so intensively superior. Both processes might have been stalled at various points in their development. But both economic and military institutions contained persistent dynamics whereby generation after generation of social actors made gradual refinements in their practices to generate the eventual outcome – in conjunction with "eventful moments," which more contingently helped on this process – like the passing of enclosure laws, the Portuguese navigational revolution, or the battle of Nancy in 1477.

But the institutions themselves were also entwined with each other in somewhat unpredictable ways. Each of the four sources of power contained distinctive surging rhythms, influencing the others. Somewhere between 1660 and 1760, these surges began to cumulatively take Britain beyond Smithian cycles of even a high-equilibrium agrarian society. It was not a sudden "takeoff" (as in the Rostow theory of the Industrial Revolution, now largely discredited), but a cumulative process of sustained slow growth of at first about 1 percent per annum, eventually rising to nearly 3 percent (and never higher) in the mid-nineteenth century. The period of overtaking came before global dominance. Not until after the mid-nineteenth century did the Western Powers begin to dominate East Asia – and of course Japan successfully resisted. Western dominance will probably have lasted less than two centuries when it comes to an end. But this was the only period in history in which any single region of the world has been globally dominant. Explaining this required starting early. No one has persuaded me I should have started any later, or that a proper explanation should ignore any of the four sources of social power.

Nonetheless, my explanation of the European Miracle is not perfect. I focused on the impact of militarism on the individual state, but rather neglected its role in wiping out many of them and enabling the expansion of Europe overseas. I neglected the contribution of Europe's scientific revolution to the Miracle, although this was not a major defect because it depended on boosts from capitalist market incentives, from states and militaries competing for technical advantage, and from religious strains of thought, which saw science as the discovery of God's laws (I explain this in my 2006 article). But above all, I now see that I actually gave two rather different overall explanations of the Miracle. As Anderson (1992: 83) points out, after I summarize the contributions made by all four sources of power, I say (on page 507): "I have singled out one, Christendom, as necessary for all that followed. The others also made a significant contribution to the resultant dynamic, but whether they were 'necessary' is another matter." Anderson comments with some irony that "the surprise hero of the tale is the Catholic Church." I qualify this slightly in the book, but not enough. This declaration of mine was wrong. It is at odds with the other explanation I give, which is the correct one. This is that the European Miracle was attributable to the greater role of competition in Europe than elsewhere – but I do not mean this in a purely economic sense. As I say, medieval Europe contained a plethora of competing collective actors – classes, of course, but also the village versus the manor plus monastic economic units, feudal lords versus urban bourgeoisies and guilds, and states challenging other states but also being challenged by barons and sometimes by the Church as well. But all this did not result in Hobbes's war of all against all, because the intense competition was principally regulated by the normative solidarity provided by Christendom (or more precisely Western Christendom). Solidarity was at a rather minimal level, true, but a Christendom with more teeth might have stifled the competition. All effective markets – all effective societies – need normative regulation, which sociologists have known ever since Durkheim. In modern economies and states this is mostly done through the law. Because of their origins, most European states and the Catholic Church had varying combinations of customary (Germanic) and statute (Roman) law that also played some regulatory role. But legal rights were contested, and it was the Church that predominated in establishing normative regulation up until the Protestant schism. As many have noted, on religion I have borrowed more from Durkheim than Weber, for I say that this regulation was more through ritual than doctrine.

Therefore, my underlying argument in the later chapters of this book is that there were two necessary, general causes of the European Miracle, not one: an intense competeiveness in European society involving all the sources of power, but this was regulated by the normative solidarity of Christendom. I ought to have made this clearer than I actually did. Nor should I have sometimes given the impression that "all was in place" by the medieval period. On page 377, for example, I say that all the essential preconditions for the Miracle were in place by 800 AD. As soon as reviewers noted and derided it, I knew that this was one of those places where an author's enthusiasm has overwhelmed his sense. I actually show that the development of the preconditions was actually a long-drawn-out and cumulative process that travelled erratically across Europe as power shifted gradually to the northwest of the continent. It might have been thrown off course by further conquest from the East or by economic and demographic crises. If the Armada had succeeded, England would probably not have been the leading edge of power, and who knows what form, if any, the Industrial Revolution might have taken. The Armada was defeated more by storms than by English seamanship, so this was a genuine accident. The institutions of capitalism, of intensive militarism, and of the modern state carried on developing unevenly but persistently. They might seem "structural" but they cannot be viewed merely as the static, institutionalized backdrop that episodic bursts of interstitial power were simply disrupting. Sometimes structural change resulted from a myriad smaller changes. Newcomen's first steam engine had come in 1713, while James Watt began tinkering with it in 1763 to great effect, but hundreds of designers added piecemeal improvements over 150 years from Newcomen. In my final chapter I indicate, that viewed from afar, the European dynamic seems systemic, and it was indeed persistent, but when we get up close to it, we see that many causal chains were coming together, sometimes rather accidentally.

I began my project by asking the "Engels question" – whether one of my four power sources was of decisive, final causal power in the structuring of social relations (he said economic power was ultimately decisive). My answer is probably the Weberian "no," although I did not start with this as a presupposition of this work, and at the end of this volume I reached only one-fourth of the way through my attempt to answer this question empirically. But the economy, the state, and so forth do not possess given structures, exercising steady, permanent influence on social development. They instead prove to have emergent properties, as new assemblages of bits and pieces of them emerge as unexpectedly relevant for more general social development and are appropriated as part of a new interstitial force. There seems to be no general, single patterning of these processes. All I have managed so far are period-specific generalizations and most of these are multilayered like the one just presented – tentative, controversial, and vulnerable to the empirical research of the next decade.

However, I make three general observations about causality. First, the causes of the development of one power source (other things being equal) mostly lie within its own antecedent condition, because its organization has some degree of autonomy. If we want to explain the Industrial Revolution, we look more at late agrarian economies than at religious or scientific discourse or at the practices of militaries or states, although all are necessary for a full explanation. If we want to explain the rise of the modern state, we must look first at antecedent politics, which derived more from struggles over fiscal-military exploitation than, say, from exploitation deriving directly from the mode of production. It is obvious that new military organizations and strategies arise primarily to counter prior ones, and that Luther developed his theology primarily in response to disputes within the Catholic Church – and he became of world-historical significance only when his doctrines became linked to capitalism (as Weber argued) and to shifts in geopolitical power (which I argue).

Second, the character of the power emanating from the four sources all differ. Economic power is the most embedded in everyday life and the one that exerts the most gradual, persistent causal pressure; ideology emerges powerfully, suddenly, erratically, and in its most powerful, transcendent guise only occasionally; military power is exercised suddenly, occasionally, and violently, but it also has a cumulative build-up of technique; and political power is distinctively territorial and institutionalized. I will explain all this at greater length in Volume 4.

Third, when we refine our explanation by including the influence of other power sources, we rarely stress their core qualities. More often we bring in peripheral aspects that come to have particular (usually unexpected) significance for the power source we are trying to explain. To explain the rise of the modern state, we must specify its economic preconditions, but most crucially those that were especially relevant to states, like taxes, and not perhaps the general level of economic development. Similarly, when we explain the military superiority of one method of warfare over another, we must specify its economic preconditions, but these might lie in the presence of ample grasslands (for chariots or cavalry) or in an iron industry making cannons before it was turned to other manufacturing industry. Conversely, to explain why twentieth-century capitalism is divided into nations as well as classes, we focus less on the major political struggles of the nineteenth century – which concerned class, religious, and regional movements – than on the unintended consequences of the pressure for them all to organize themselves at the level of the state in order to further their collective interests.

Such analysis seems to take us further away from the prospect of any simple theory of "ultimate primacy." Nonetheless, we can generalize about both the distinctive power capacities of each source of power and about primacy in particular spatial and historical settings. I have done the latter in all four of my volumes, but I reserve the former for Volume 4.

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Preface

In 1972, I wrote a paper called "Economic Determinism and Structural Change," which purported not only to refute Karl Marx and reorganize Max Weber but also to offer the outlines of a better general theory of social stratification and social change. The paper began to develop into a short book. It would contain a general theory supported by a few case studies, including historical ones. Later I decided that the book would set forth a sweeping theory of the world history of power.

But while developing these delusions, I rediscovered the pleasure of devouring history. A ten-year immersion in that subject reinforced the practical empiricism of my background to restore a little respect for the complexity and obduracy of facts. It did not entirely sober me. For I have written this large history of power in agrarian societies, and I will follow it shortly with Volume II, *A History of Power in Industrial Societies,* and Volume III, *A Theory of Power* – even if their central thrust is now modest. But it gave me a sense of the mutual disciplining that sociology and history can exercise on each other.

Sociological theory cannot develop without knowledge of history. Most of the key questions of sociology concern processes occurring through time; social structure is inherited from particular pasts; and a large proportion of our "sample" of complex societies is only available in history. But the study of history is also impoverished without sociology. If historians eschew theory of how societies operate, they imprison themselves in the commonsense notions of their own society. In this volume, I repeatedly question the application of essentially modern notions – such as nation, class, private property, and the centralized state – to earlier historical periods. In most cases, some scholars have anticipated my skepticism. But they could have generally done so earlier and more rigorously had they converted implicit contemporary common sense into explicit, testable theory. Sociological theory can also discipline historians in their selection of facts. We can never be "sufficiently scholarly": There are more social and historical data than we can digest. A strong sense of theory enables us to decide what might be the key facts, what might be central and what marginal to an understanding of how a particular society works. We select our data, see whether they confirm or reject our theoretical hunches, refine the latter, collect more data, and continue zig-zagging across between theory and data until we have established a plausible account of how this society, in this time and place, "works."

Comte was right in his claim that sociology is the queen of the social and human sciences. But no queen ever worked as hard as the sociologist with theoretical pretensions needs to! Nor is the creation of historically supported theory nearly as streamlined a process as Comte believed. Zig-zagging between theoretical and historical scholarship has unsettling effects. The real world (historical or contemporary) is messy and imperfectly documented; yet theory claims pattern and perfection. The match can never be exact. Too much scholarly attention to the facts makes one blind; too much listening to the rhythms of theory and world history makes one deaf.

So, to preserve my health during this venture, I have depended more than usually on the stimulus and encouragement of sympathetic specialists and fellow zig-zaggers. My greatest debt is to Ernest Gellner and John Hall. In our "Patterns of History" seminar, held since 1980 at the London School of Economics and Political Science (LSE), we have argued over much of the ground covered by this volume. My thanks go especially to John, who has read virtually all my drafts, commented copiously on them, argued with me all the way, and yet been invariably warm and supportive toward my enterprise. I have also shamelessly exploited the seminar's distinguished visiting speakers, in discussion turning their excellent papers toward my own obsessions, pumping them for ideas and specialist knowledge.

Many scholars commented generously on individual chapters, correcting my howlers, putting me in touch with up-to-date research and controversies in their field, demonstrating that I was wrong, even hoping that I would stay longer in their field and dig deeper. In rough order of their interests as organized by my sequence of chapters, I thank James Woodburn, Stephen Shennan, Colin Renfrew, Nicholas Postgate, Gary Runciman, Keith Hopkins, John Peel, John Parry, Peter Burke, Geoffrey Elton, and Gian Poggi. Anthony Giddens and William H. McNeill read the whole of my penultimate draft and made many sensible criticisms. Over the years, colleagues commented helpfully on my drafts, seminars, and arguments. I would like particularly to thank Keith Hart, David Lockwood, Nicos Mouzelis, Anthony Smith, and Sandy Stewart.

Essex University and LSE students were sympathetic audiences for trying out my general ideas in sociological theory courses. Both institutions were generous in giving me leave to research and lecture on the material in this book. Seminar series at Yale University, New York University, the Academy of Sciences at Warsaw, and Oslo University gave me extended opportunities to develop my arguments. The Social Science Research Council awarded me a personal research grant for the academic year 1980–1 and was most supportive toward me. In that year I was able to complete most of the historical research necessary for the earlier chapters, which I would not have been able to do easily while carrying a normal teaching load.

Library staff at Essex, the LSE, the British Museum, and the University Library, Cambridge, coped well with my eclectic demands. My secretaries at Essex and the LSE – Linda Peachey, Elizabeth O'Leary, and Yvonne Brown – were unfailingly efficient and helpful through all the drafts thrust at them.

Nicky Hart made the breakthrough that reorganized this work into three volumes. Her own work and her presence – together with Louise, Gareth, and Laura – prevented me from being blinded, deafened, or even too obsessed, by this project.

Obviously, the mistakes are mine.