Introduction

PLACING POLITICS IN TIME

> For an economic historian, time has always been something that is fundamentally disturbing, because there is *no time* in neoclassical theory. The neoclassical model is a model of an instant of time, and it does not therefore take into account what time does. . . . I will be blunt: Without a deep understanding of time, you will be lousy political scientists, because time is the dimension in which ideas and institutions and beliefs evolve.
> —*Douglass North (1999, p. 316)*

We can begin with an analogy. Imagine that your friend invites you to the trendiest new restaurant in town, charmingly named "The Modern Social Scientist." As an added bonus, he informs you that he knows the chef well, and that you will have a chance to tour the kitchen. When you arrive, the chef explains that the kitchen is divided into two parts. On the left, she has all the ingredients (which to your puzzlement she refers to as "variables"). These ingredients, she insists, are the freshest available and carefully selected. On the right is an extraordinary profusion of measuring devices. You express astonishment at their complexity and detailed ornamentation, and the chef explains that each requires years to learn how to operate properly.

The chef proceeds to elaborate her culinary approach: good cooking, she says, amounts to having the perfect ingredients, perfectly measured. Traditional cooks have stressed how important the cooking process itself is, including the sequence, pace, and specific manner in which the ingredients are to be combined. Not so, says the proprietor of The Modern Social Scientist. As long as you have the correct ingredients and they are properly measured, she insists, how, in what order, and for how long they are combined *makes no difference.*

Few would want to patronize a restaurant with such a philosophy of cooking, but most social scientists are working in that kind of a kitchen. Disputes among competing theories center on which "variables" in the current environment generate important political outcomes. How does the distribution of public opinion affect policy outcomes? How do individual social characteristics influence propensities to vote? How do electoral rules affect the structure of party systems? Yet the significance of such "variables" is frequently distorted when they are ripped from their temporal context. Contemporary social scientists typically

take a "snapshot" view of political life, but there is often a strong case to be made for shifting from snapshots to moving pictures. This means *systematically* situating particular moments (including the present) in a temporal sequence of events and processes stretching over extended periods. Placing politics in time can greatly enrich our understanding of complex social dynamics.

This book explores a range of temporal processes that are common in political life. It seeks to distinguish various processes that unfold over substantial stretches of time, to identify the circumstances under which such different processes are likely to occur, and to highlight the significance of these temporal dimensions of social life for our understanding of important political outcomes. In doing so, I seek to demonstrate the very high price that social science often pays when it ignores the profound temporal dimensions of real social processes. The ambition, in short, is to flesh out the often-invoked but rarely examined declaration that history matters.

It is no accident that so many of the giant figures in the formative period of the social sciences—from Marx, Tocqueville, and Weber to Polanyi and Schumpeter—adopted deeply historical approaches to social explanation. This stance was not simply a quaint feature of a transitional stage to modern social analysis. It was a key source of their profound insights about the nature of the social world. Attentiveness to issues of temporality highlights aspects of social life that are essentially invisible from an ahistorical vantage point. Placing politics in time can greatly enrich both the explanations we offer for social outcomes of interest, and the very outcomes that we identify as worth explaining. The systematic examination of processes unfolding over time warrants a central position in the social sciences.

Two Illustrations

Because I am eager to contribute to the efforts of social scientists working on a wide range of matters, I initially discuss many of the issues addressed in this book at a fairly high level of abstraction. It may be of some help at the outset to briefly outline two examples of recent scholarship that offer compelling examples of the insights to be gained by shifting from a "snapshot" to a moving picture of important social processes. Daniel Carpenter's *The Forging of Bureaucratic Autonomy* (Carpenter 2001) presents a striking demonstration of how attention to a long-term sequence of causes (what I will call a "causal chain" in Chapter Three) can turn our understandings of social phenomena on their heads. He criticizes the large and influential literature on relations between legislatures and bureaucrats grounded in Principal-Agent theory (McCubbins, Noll, and Weingast 1987; McCubbins and Schwartz 1984). The literature argues that congressional "principals" have substantial political resources to assure that their bureaucratic "agents" largely comply with their preferences. Yet Carpenter persuasively demon-

strates how these analyses substantially underestimate the potential for bureaucratic autonomy because they adopt a cross-sectional approach to studying what should be understood as a long-term causal chain. Under the right conditions, ambitious and entrepreneurial bureaucrats were able over extended periods of time to enhance their reputations for innovativeness and competence, and develop strong networks of support among a range of social actors. These achievements created a context in which Congress, facing pressure from below and deferring to the expertise of leading bureaucrats, essentially asked for what the bureaucrats wanted. Viewed as a moment in time, one sees what looks like congressional dominance; viewed as a process unfolding over time, *the same cross-sectional evidence* provides indications of substantial bureaucratic autonomy.

Thomas Ertman's *The Birth of Leviathan* (Ertman 1996) offers a compelling explanation of different patterns of state-building in early modern Europe that emphasizes how the results of critical junctures, and cross-national differences in the sequencing of key historical processes, generated durable variations in the structures of nation-states. Ertman emphasizes the lasting repercussions of the financing methods adopted in European states that faced military competition before the rise of modern bureaucracies. Following predecessors such as Hintze and Tilly, Ertman argues that the onset of military competition had a critical effect on patterns of European state development. Crucially, however, Ertman stresses that different states experienced intense military conflict *at different times.* He maintains (p. 26) that "differences in the timing of the onset of sustained geopolitical competition go a long way towards explaining the character of state infrastructures found across the continent at the end of the 18th century."

States confronting military competition faced the life-or-death challenge of generating sufficient revenues to wage prolonged warfare. However, the available repertoires of administrative response were conditional on the point in historical development when this challenge appeared. According to Ertman, "timing mattered because the range of 'technical resources' available to statebuilders did not remain invariant across this period." In the twelfth century, literacy was a very scarce resource and sophisticated bureaucracy an unknown organizational technology. In this historical context, monarchs were forced to rely on systems of proprietary office holding and tax farming "which were much more beneficial to [these officeholders] than to their royal employers." By contrast, countries that faced intensive military competition at a later time "found themselves in a quite different world" (p. 28). Literacy was much more common, and knowledge of more modern bureaucratic organizational forms (as well as evidence of tax farming's considerable drawbacks) was widely available. The result was that state builders in these countries could work to construct "proto-modern bureaucracies based upon the separation of office from the person of the officeholder."

Why, then, did not all states adopt the superior bureaucratic structures? Because, Ertman argues, initial outcomes were strongly self-reinforcing. Once a dense network of institutions and interests developed around tax farming, especially in

a context where monarchs often had immediate needs for revenues, it became virtually impossible to switch over to more modern forms of financing. In the terms I will introduce shortly, the experiences of different countries were highly path dependent. And these different paths mattered a great deal, Ertman maintains, because the bureaucratic alternative was both more effective in waging war and more conducive to the development of parliamentary institutions.

Ertman's powerful analysis, like Carpenter's, is built on an understanding that social processes, such as state building and the construction of bureaucratic autonomy, unfold over time. Just as Carpenter illuminates temporally extended but crucial causal chains, Ertman employs elements of temporality explored systematically in the chapters to follow: path dependence (Chapter One), sequencing (Chapter Two), and slow-moving processes (Chapter Three). For both authors, the turn to history is not primarily a matter of employing narrative or compiling historical evidence; it is the grounding for theoretical claims about how things happen in the social world.

A "Historic Turn" in the Social Sciences?

As these illustrations attest, the social sciences have had a rich tradition of historical research. Scholarly communities devoted to extending such traditions flourish in parts of the social sciences. Indeed, some (McDonald 1996; Bates et al. 1998) claim to witness a "historic turn" in the human sciences as a whole. Yet in spite of this activity there has actually been surprisingly limited attentiveness to the specifically temporal dimensions of social processes. In contemporary social science, the past serves primarily as a source of empirical material, rather than as a spur to serious investigations of how politics happens over time.

At least within political science, the adoption of a historical orientation has generally failed to exploit its greatest potential contribution to the more systematic understanding of social processes. One can in fact distinguish three prominent "historic turns" in the social sciences. Each of these has added significantly to our store of knowledge, but each has serious limitations as well. The first, especially prominent in the field of American political development, might be termed "history as the study of the past." Here analysts study particular historical events or processes, with a focus on offering convincing causal accounts of specific outcomes of interest. Such investigations often greatly increase what we know about particular facets of the political histories of specific countries.

What is less clear, however, is how particular studies fit into some broader research program. Little effort is made to suggest what, if anything, might "travel" from one investigation to another. Indeed, many historically oriented analysts are uninterested in this question, assuming the stance of most historians: that the rich particularities of each event or process render it unique. Alternatively, these analysts seem to assume implicitly that a discussion of, say, social movements in

the 1920s generates clear implications for our understanding of contemporary social movements. Such an assumption is highly problematic. This first historical turn is often admired. Yet it is fair to say that the great majority of social scientists, whose principal interests lie in understanding contemporary society, sees "history as the study of the past" as largely irrelevant to their own inquiries.

A second "turn" could be termed "history as the hunt for illustrative material." Here the researcher's perspective could uncharitably be summarized as follows: "I have a model of some aspect of politics. Let me search the past for a good illustration of the model." This type of exercise is especially common among those aspiring to generate very general propositions about politics. Thus, rational choice theorists—contemporary social science's most ambitious pursuers of general propositions—comb the past for examples of credible commitment mechanisms or for solutions to a particular class of collective action problems. Given a wealth of available historical material and the often highly stylized accounts generated in these exercises, the past may well offer up the desired illustrations. Here again, however, the analyses say little or nothing about the temporal dimensions of social processes. The motivation for going back in time is simply to get at examples that may not be available in the present.

This is also the case of the third turn, which could be termed "history as a site for generating more cases." History becomes a source of data, especially for phenomena that are relatively uncommon in the contemporary environment (Bartolini 1993, p. 144). There are deep controversies about this methodological move, whether it takes the form of large-n studies (e.g., using pooled time series) or small-n studies (e.g., using some variant of Mill's methods of agreement and disagreement).[1] I share some of these critics' misgivings. My main point, however, is a different one, and it applies to all three of the turns to history that I have discussed. The best case for connecting history to the social sciences is neither empirical (turn #1) nor methodological (turns #2 and 3), but theoretical. We turn to an examination of history because social life unfolds over time. Real social processes have distinctly temporal dimensions.

Yet an exploration of these temporal dimensions of social processes is precisely the weakest link in social science's historical turn. We largely lack a clear outline of why the intensive investigation of issues of temporality is critical to an understanding of social processes. The declaration that "history matters" is often invoked, but rarely unpacked.[2] Many of the key concepts needed to underpin analyses of temporal processes, such as path dependence, critical junctures,

[1]On the former see Shalev 1999. For different views on the latter see Lieberson 1985 and Mahoney 1999.

[2]There are notable exceptions, to which the current analysis is deeply indebted. Among these I would single out Collier and Collier 1991; Katznelson 1997; Mahoney 2000; Mahoney and Rueschemeyer 2003; Orren and Skowronek 1994; Skocpol 1992; Stinchcombe 1965, 1968; Thelen 1999, 2003. The excellent essays collected in Abbott 2001 cover some of the territory I take up here, albeit in very different fashion.

sequencing, events, duration, timing, and unintended consequences, have received only very fragmented and limited discussion. To assert that "history matters" is insufficient; social scientists want to know why, where, how, and for what. As the sociologist Ronald Aminzade has put it (1992, p. 458), we seek "the construction of theories of continuity and change that are attentive to order and sequence and that acknowledge the causal power of temporal connections among events." This "requires concepts that recognize the diversity of patterns of temporal connections among events."

What is at stake in this effort to refine our theoretical understandings of the different ways in which "history matters" in explaining social phenomena? Most important, examining temporal processes allows us to identify and explicate some fundamental social mechanisms. By mechanisms I mean what Jon Elster has termed "plausible, frequently observed ways in which things happen." A number of scholars have recently emphasized that where possible—and it will not always be possible—it is extremely helpful to identify frequently recurring causal mechanisms. Jon Elster has put it most strongly in arguing that "the basic concept in the social sciences should be that of a mechanism rather than of a theory. . . . [T]he social sciences are light years away from the stage at which it will be possible to formulate general-law-like regularities about human behavior. Instead, we should concentrate on specifying small and medium-sized mechanisms for human action and interaction" (Elster 1989, p. viii).[3]

The main focus of this book is on social mechanisms that have a strong temporal dimension. Although historically oriented scholars are (rightly) skeptical about the prospects for generating anything like a general theory of politics, most social scientists remain interested in developing at least limited generalizations—arguments that can "travel" in some form beyond a specific time and place.[4] The current study seeks to address this gap by outlining mechanisms that have a strong temporal dimension. Exploring the character of these mechanisms, and the features of social contexts that generate them, can thus simultaneously address the desire of analysts to move beyond a single case and the desire of historically oriented scholars to capture how history matters. The identification and clarification of such mechanisms can enhance our ability to develop arguments about temporal processes that are both convincing and have at least limited portability.

Exploring these mechanisms, I will suggest, can lead us to reassess prominent areas of social science inquiry and conventional practices in new and fertile ways. It will often suggest new hypotheses regarding important subjects and open exciting possibilities for extending existing theoretical work in new direc-

[3]For additional discussions see Scharpf 1997, chap. 1, and Stinchcombe 1991.

[4]Indeed, one of the striking features of much of the qualitative historical literature in the field of American political development, for example, is the limited capacity of studies to actually build on their predecessors, rather than simply piling up one after another. For a discussion of the possibilities for enhancing the quality of research programs in comparative historical analysis, see the essays in Part One of Mahoney and Rueschemeyer 2003.

tions. A focus on these temporally oriented mechanisms suggests new questions and reveals new outcomes of interest—questions and outcomes that are linked to, but distinct from, existing lines of inquiry. Finally, the intensive exploration of the temporal dimensions of social processes forces us to rethink our strategies of social investigation. I cannot emphasize enough that this is *not* a book about methods. Yet how you choose to look for things depends heavily on what you think you are looking for. Hence, many of the arguments explored here have substantial methodological implications (Hall 2003).

The principal audience for this book consists of those interested in the attempt to develop claims about the social world that can potentially reach across time and space. Yet even for scholars who are dubious about the prospects for generalization or uninterested in its pursuit, theoretical explorations of historical causation remain important. It is easy to underestimate the extent to which theoretical discussions underpin, if only implicitly and by way of diffusion, all empirical research. As Fritz Scharpf (1997, p. 29) puts it,

> in a world that is exceedingly complex and in which we will often be studying unique cases, we must have a good idea of what to look for if we wish to discover anything worthwhile. Since a single data point can be "explained" by any number of regression lines, post hoc explanations are too easy to invent and usually (unless invented with the trained skill of the master historian) totally useless. The implication is that our search for explanations must be disciplined by strong *prior* expectations and that we must take the disconfirmation of such expectations as a welcome pointer to the development of more valid explanations.

In general, we will be in a much better position to carry out convincing research if we think through these prior expectations—and our justifications for holding them—explicitly and carefully. Clarifying how various temporally grounded mechanisms operate and suggesting where we should expect such processes to be at work can provide stronger theoretical underpinnings for the analyses of even those seeking to explain a single case.

Achieving greater clarity about how history imparts its effects on the present will open up possibilities for more constructive intellectual dialogue. It will help historically oriented scholars be more effective in communicating the import of their research to each other as well as to often skeptical colleagues. The social sciences are highly balkanized and tribal. In this context, there is much to be said for efforts that seek to articulate the aspirations and achievements of a body of research in a language that makes sense to those outside the tribe.[5] This is not

[5]Even within "tribes," communication is often hindered by the lack of a common conceptual language that allows scholars to recognize common interests—as, for instance, when scholars studying different areas of the globe lack a shared vocabulary to recognize their overlapping concerns. On this see Mahoney and Rueschemeyer 2003.

just about making oneself understood or about clearing away false disagreements—although these are important benefits. At least as significant, greater clarity and precision can reveal significant points of overlap and identify opportunities for useful exchange as well as highlight genuinely contentious issues.

In this context it makes sense to briefly situate the current discussion vis-à-vis two prominent schools of thought in contemporary political science: "rational choice theory" and "historical institutionalism." Such an effort is hazardous, since these are loose camps—"theoretical imageries," as Ronald Jepperson (1996) has usefully put it—rather than coherent theories. Yet given the tendency of many scholars to orient their work in allegiance to one camp (and often in opposition to others) it may be useful to make my own position clear: a focus on the temporal dimensions of social processes largely cuts across this divide. The systematic examination of temporal processes can usefully draw on rich contributions from each tradition, while also highlighting certain limitations. It can help to distinguish points of overlap, instances of genuine disagreement, and substantial areas in which the two traditions simply speak to different (although perhaps complementary) questions. My claim is that a focus on temporal processes can point to fruitful lines of theoretical, methodological, and substantive inquiry for those working within each of these "imageries"—although these lines will and should remain distinctive in important respects.

One would think that the issues at hand would be the natural terrain for "historical institutionalism," with its long-standing insistence that social science research should be historically grounded (Skocpol and Sommers 1980; Thelen and Steinmo 1992; Katznelson 1997). Yet those associated with historical institutionalism have generally been more explicit in discussing the "institutionalist" dimensions of their frameworks than the "historical" ones. They have concentrated on examining the impact of relatively fixed institutional features of the political landscape (such as constitutional arrangements and major policy structures). The specifically historical component of historical institutionalism has, at least until recently (Hacker 2002; Thelen 2004) generally been left unclear. As already noted, if a justification for a turn to history is given at all, it has more often been methodological rather than theoretical in nature.

Thinking more explicitly about the role of time in politics will, however, justify some of the key concerns and offer support for some key propositions advanced by historical institutionalist scholarship. It will provide stronger theoretical grounds for emphasizing the "stickiness" of inherited social arrangements, for questioning functional explanations (that is, claims that social arrangements exist *because* they meet certain needs of societies or particular powerful actors), for concentrating on issues of timing and sequence, and for investigating long-term processes of social change. More broadly, this investigation will vindicate historical institutionalism's interest in macrolevel social phenomena, as well as its cautious stance regarding social science's capacity to

develop broad generalizations about social processes that apply across sweeping stretches of time and space.

At the same time, I stress that rational choice analysis, broadly defined, offers essential analytical tools for investigating temporal processes. In this book I develop theoretical arguments organized around four major themes: path dependence, issues of timing and sequence, the significance and distinctiveness of "slow-moving" processes that require attentiveness to extended periods of time, and problems of institutional origins and change. On each of these points, work drawn from economic theory, focusing on problems of strategic interaction among calculating, rational individuals, has much to offer.

Yet a focus on the temporal dimensions of political processes highlights the limits of choice-theoretic approaches as well as their strengths. Most important is the way in which micromodeling exercises that are centered on strategic interaction among individuals encourage a highly restricted field of vision, both in space and time. Among the things that tend to drop out of such exercises are issues of macro structure, the role of temporal ordering or sequence, and a whole host of social processes that play out only over extended periods of time and cannot be reduced to the strategic "moves" of "actors." Thus, a restricted field of vision adversely affects both the kinds of questions rational choice theorists tend to generate and the kinds of answers they typically provide. The critique, it should be emphasized, is not the common (in my view often misguided) one about rational choice theory's assumptions about human behavior, but about its restricted range of application. The implication is thus not that rational choice theory should be rejected, but that its scope should be placed in proper perspective. Analysts should focus on establishing how insights from rational choice can be linked to other approaches, or where other approaches are simply more appropriate for addressing particular kinds of questions (Jepperson 1996).

Clearer recognition of some of rational choice's blind spots may also facilitate efforts within that tradition to address challenging problems in convincing ways. In the pages that follow I will often criticize strong *tendencies* associated with particular techniques or theoretical approaches, while accepting—indeed emphasizing—that there is nothing about these modes of inquiry that renders these tendencies logically *necessary*. Analysts will sometimes seize on this point to suggest that the critique has little import.[6] This reaction is a mistake. The question, after all, is not just what a particular technique or theory is capable of doing in principle, but how and to what extent it is actually used in practice—a distinction that is almost always glossed over in general discussions of method and theory. To paraphrase a recent observation by Ronald Jepperson, when faced with a causal account employing a long-term temporal structure, quantitative scholars (as well

[6]This response has been common, although far from universal, in the reactions of rational choice scholars to some of the arguments presented in this book.

as some rational-choice theorists) might well respond by exclaiming, "I can model that!" This riposte loses much of its bite, however, if the theoretical imageries and methods these analysts employ rarely lead them to consider the outcomes or hypotheses in question in the first place (Jepperson 1996).

The thrust of the arguments in *Politics in Time* is to reconsider a wide range of theoretical approaches and methodological techniques from the vantage point of issues related to temporality. In doing so, I hope to challenge some of the tribal, polarized character of much contemporary social science. A focus on the temporal dimensions of social processes provides exciting possibilities for shaping some common—or at least overlapping—intellectual terrain for scholars working out of highly diverse research traditions. In the remainder of this introduction I briefly summarize my efforts to map out that terrain.

Analytical Foundations

The five chapters that form the core of this book explore some key temporal dimensions of social and political processes. They explicate different ways in which things happen over time in social life, drawing attention to processes that are unlikely to be visible without specifically addressing questions of temporality. In each case, I seek to demonstrate why such processes are likely to be prevalent, the circumstances under which they should be expected, and the major implications for our efforts to understand social outcomes.

Chapter One focuses on *path dependence*. Path dependence has become a faddish term, often lacking a clear meaning. Yet there are exciting new possibilities for applying the notion in a more rigorous way to the analysis of social processes. I argue that this means focusing on the dynamics of self-reinforcing or positive feedback processes in a political system. Such processes have very interesting characteristics. They can be highly influenced by relatively modest perturbations at early stages. Thus, such processes can produce more than one outcome. Once a particular path gets established, however, self-reinforcing processes make reversals very difficult. In economics, such ideas have become increasingly popular in the investigation of new technologies (Microsoft vs. Macintosh), trade, economic geography, and economic growth.

Drawing on the research of Brian Arthur and Douglas North, I show how these ideas can be extended and modified to address issues of central importance to political scientists. There are strong grounds for believing that self-reinforcing processes will be prevalent in political life—arguably more pervasive and intense than they are in the economic sphere. Once established, patterns of political mobilization, the institutional "rules of the game," and even citizens' basic ways of thinking about the political world will often generate self-reinforcing dynamics. Once actors have ventured far down a particular path, they may find

it very difficult to reverse course. Political alternatives that were once quite plausible may become irretrievably lost.

Claims about path dependence typically suggest that beginnings are extremely important. So one might ask: Why begin this discussion with path dependence? The answer is that an understanding of self-reinforcing processes is extremely helpful for exploring a wide range of issues related to temporality. Exploring the sources and consequences of path dependence helps us to understand the powerful inertia or "stickiness" that characterizes many aspects of political development—for instance, the enduring consequences that often stem from the emergence of particular institutional arrangements. These arguments can also reinvigorate the analysis of power in social relations by showing how inequalities of power, perhaps modest initially, can be reinforced over time and often come to be deeply embedded in organizations and dominant modes of political action and understanding, as well as in institutional arrangements. Path-dependence arguments also provide a useful and powerful corrective against tendencies to assume functionalist explanations for important social and political outcomes—the supposition that the existence of current social arrangements is to be explained through reference to the needs they address for the currently powerful. Moreover, an appreciation of the prevalence of path dependence forces attentiveness to the causal significance of temporally remote events or processes. Path-dependent processes exemplify what Arthur Stinchcombe has termed "historical causation," in which dynamics triggered by an event or process at one point in time reproduce themselves, even in the absence of the recurrence of the original event or process. Finally, an appreciation of positive feedback also justifies attentiveness to issues of temporal ordering. In path-dependent processes, the order of events may make a fundamental difference. In all these respects, path dependence underscores the distinctly temporal dimensions of social processes, laying a foundation for the chapters that follow.

Chapter Two explores issues of *timing and sequence* in greater detail. Social scientists tracing broad patterns of political development across a number of countries often argue that the timing and sequence of particular events or processes can matter a great deal. Settings where event A precedes event B will generate different outcomes than ones where that ordering is reversed. In Ertman's account, for example, it is the relative timing of the arrival of mass literacy and the onset of intense military competition that is crucial. The concepts of timing and sequencing are, however, more often invoked than clearly thought through. Reviewing some prominent work in comparative historical analysis, I show that sequencing arguments have been both prevalent and, often, analytically muddled. There are actually a number of distinct ways to make such claims. Different types of sequencing arguments are likely to be relevant in different settings. Clarifying these differences and specifying the mechanisms that link claims about sequences to particular outcomes are the central tasks of this chapter.

I focus on two broad classes of arguments. One class of arguments about timing and sequence focuses on *conjunctures*—interaction effects between distinct causal sequences that become joined at particular points in time. For instance, it arguably mattered a good deal for the trajectory of domestic politics whether left wing or right wing parties happened to be in power at the time when a cataclysmic event, the Great Depression, hit a particular country. Just as a falling brick has distinct consequences when it arrives at the same time as an unfortunate pedestrian, the simultaneity of two processes that in other cases occur at different times produces critical consequences. Bendix's famous analysis of what happens when the twin processes of industrialization and democratization occur at the same time is a good example.

The first part of Chapter Two explores the potential and pitfalls of such conjunctural arguments for social scientists. Historically oriented scholars rightly point out that explanations for important social outcomes often rest at least in part on such conjunctures. At the same time, however, there appear to be real limits to our capacity to use conjunctural claims to search for patterns across cases. With modest exceptions, such claims would seem to be most useful for understanding, after the fact, specific outcomes of interest. They seem less likely, however, to yield an understanding of mechanisms that could be applied in multiple settings.

This is not the case for the second class of arguments about sequencing that I consider. Many—probably most—arguments about sequencing turn out on closer investigation to be grounded in claims about positive feedback. This makes them amenable to the forms of analysis developed in my discussion of path dependence. Here, timing and sequence matter because self-reinforcing processes affecting a particular aspect of political and social life can transform the consequences of later stages in a sequence.

Linking arguments about path dependence to a focus on sequencing produces powerful theoretical synergies. Path-dependent arguments about self-reinforcement explain why and when sequencing can matter. Positive feedback processes occurring at particular times essentially remove certain options from the menu of political possibilities. By doing so, they can greatly alter the consequences of events or processes occurring at a later stage. At the same time, the specific focus on sequencing generates a rich new set of hypotheses about path-dependent processes. It can draw attention to contests over "political space" in which potential competitors seek first-mover advantages, while clarifying the likely long-term impact of initial defeats on the opportunities and constraints facing initial "losers" or groups that arrive at a later point in time. It draws attention to the significance of large-scale social changes and the importance of the timing of these changes relative to each other. Furthermore, a focus on historical sequences suggests how arguments about path dependence can address claims about political change as well as political inertia. For instance, path-dependent processes may operate to institutionalize political arrangements that prove to be

particularly vulnerable to some event or process emerging at a later stage in political development.

These arguments about path-dependent sequences can both draw on and enhance arguments that rational choice theorists have developed about the temporal ordering of choices in highly institutionalized settings. Working from Arrow's paradox of voting, which suggests the likelihood of endless cycling in many collective choice situations, rational choice theorists have argued persuasively that institutional arrangements governing agenda control and decision-making procedures can produce stable outcomes. These institutional arrangements are crucial because in contexts vulnerable to cycling it can be demonstrated that the sequencing among alternative choices will determine the outcome. This whole line of argument rests on institutional mechanisms that generate path dependence: steps in a sequence are irreversible because losing alternatives are dropped from the range of possible options. By showing how such "irreversibilities" can be generated in a wide range of social contexts, however, it is possible to extend this crucial insight to a far broader set of social phenomena than those covered in the literature derived from Arrow's work. Sequencing can matter not only for collective choices within legislatures, but for *any* social process where self-reinforcement means that forsaken alternatives become increasingly unreachable with the passage of time. As I demonstrate, in comparative historical analyses powerful arguments about sequencing are often applied not to the "moves" of actors on a micro scale, but to examine the impact of large-scale social changes such as democratization, industrialization, or state building.

A striking feature of many of the arguments discussed in Chapter Two is that they draw attention to lengthy, large-scale historical processes such as democratization or state building. Chapter Three turns to a more systematic discussion of big, slow-moving aspects of the social world. If the preoccupation of Chapters One and Two is questions of temporal ordering, especially the significance of beginnings, the central preoccupation here is the long durée. The chapter examines a wide range of processes that cannot be understood unless analysts remain attentive to the unfolding of both causal processes and important political outcomes over extended periods of time. Contemporary social scientists are strongly predisposed to focus on aspects of causal processes and outcomes that unfold very rapidly. Yet many things in the social world take a long time to happen—such as the spread of literacy in Ertman's analysis, or the efforts of bureaucrats to build the foundations for autonomous action in Carpenter's study. The fact that something happens slowly does not make it unimportant.

Chapter Three also explores a range of different causal processes and outcomes that may unfold over substantial stretches of time. Some causal processes and outcomes occur slowly because they are incremental—it simply takes a long time for them to add up to anything. In others, the critical factor is the presence of threshold effects. Some social processes may have little significance until they attain a critical mass, which may then trigger major change. Other social

processes involve considerable time lags between the appearance of a key causal factor and the occurrence of the outcome of interest. This may be true because the outcome depends on a "causal chain" that takes some time to work itself out (*a* causes *b*, which causes *c* . . .). Alternatively, causal processes may turn on "structural" features that involve transformations that are probabilistic during any particular period, which means that several periods may be necessary before the transformation occurs. Under conditions such as these, the social outcome of interest may not actually take place until well after the appearance of key causal factors.

Analysts who fail to be attentive to these slow-moving dimensions of social life are prone to a number of serious mistakes. They may ignore potentially powerful hypotheses. They are particularly likely to miss the role of many "sociological" variables, like demography, literacy, or technology. Their explanations may focus on triggering or precipitating factors rather than more fundamental structural causes. Indeed, by truncating an analysis of processes unfolding over an extended period of time they may end up inverting causal relationships—as Carpenter suggests in his critique of Principal-Agent theory. Perhaps most fundamental of all, they may fail to even identify some important questions about politics because the relevant outcomes happen too slowly and are therefore simply off their radar screens.

Chapters Four and Five integrate and extend the arguments of the first three chapters through a focus on issues of institutional origins and change. Questions about the effects of institutions have become central to theoretical discussions throughout the social sciences. More recently, social scientists have become interested in explaining institutional arrangements rather than simply analyzing their effects. My analysis is designed to show how systematic attention to the arguments of the first three chapters can illuminate this major topic in contemporary social theory. At the same time, in arguing for a shift in focus from the problem of institutional *selection* to the problem of institutional *development*, I further extend my critique of the ahistorical proclivities of modern social science.

As social scientists have sought to explain institutional outcomes, there has been a strong tendency to employ "functional" interpretations in which institutional arrangements are explained by their consequences. In particular, what I term "actor-based functionalism" typically rests on the claim that institutions take the form they do because powerful actors engaged in rational, strategic behavior are seeking to produce the outcomes observed. Functional explanations of institutional arrangements are often plausible, but Chapter Four demonstrates how the adoption of an extended time frame reveals numerous problems for such accounts. Functional interpretations of politics are often suspect because of the sizable time lag between actors' actions and the long-term consequences of those actions. Political actors, facing the pressures of the immediate or skeptical about their capacity to engineer long-term effects, may pay limited attention to the long term. Thus the long-term effects of institutional choices,

which are frequently the most profound and interesting ones, should often be seen as the *by-products* of social processes rather than embodying the goals of social actors. A second issue related to temporal gaps between actions and outcomes concerns unintended consequences. Even where actors may be greatly concerned about the future in their efforts to design institutions, they operate in settings of great complexity and high uncertainty. As a consequence, they will often make mistakes. For these and other reasons developed in my discussion, we should generally exercise considerable skepticism about assertions that institutional arrangements will reflect the skilled design choices of rational actors.[7] Instead, we should anticipate that there will often be sizable gaps between the *ex ante* goals of powerful political actors and the actual functioning of prominent institutions.

Such gaps would not matter much for functional accounts if institutions were easily adapted in response to current needs or improved understandings of institutional shortcomings. This suggests that a central issue for institutional theory is the role of adaptation, which may play out through two important social mechanisms—learning and competition—operating over time. Although these are two very significant social processes, I argue that neither is likely to prove adequate to rescue functionalist accounts in many political contexts. This is in part because these mechanisms will often be weak in practice, but it is also because these mechanisms must also overcome substantial barriers to institutional change. This is one of the most significant implications of the earlier discussion of path dependence, although there are additional reasons why institutional redesign is often difficult in politics.

Indeed, I argue in Chapter Five that understanding how institutional arrangements can become deeply embedded over time suggests the need to reframe the topic as one of institutional *development* rather than institutional *choice*. We need to think not just about moments of institutional selection and moments of institutional change, but of processes of institutional development unfolding over significant periods of time. These processes profoundly shape the circumstances under which modifications to institutions are likely to occur, and the kinds of changes that are likely. A focus on the dynamics of institutional development, I argue, can generate quite distinctive research agendas for those interested in explaining patterns of institutional outcomes in the social world.

These five chapters provide the core of my answer to why "history matters" for social scientists. Indeed, we are now in a position to reframe the question in a more helpful way. Why do social scientists need to focus on how processes unfold over significant stretches of time? First, because many social processes are path dependent, in which case key causes are temporally removed from

[7]Although such claims can be, and often will be, supported by careful inquiry. The point is that such connections are often assumed or asserted without justification, and that exploring the limits of such "rational design" accounts is theoretically fruitful.

their continuing effects and a central focus of analysis is on "lost" alternatives resulting from the accumulation of self-reinforcing processes. Second, because sequencing—the temporal order of events or processes—can be a crucial determinant of important social outcomes. Third, because many important social causes and outcomes are slow-moving—they take place over quite extended periods of time and are only likely to be adequately explained (or in some cases even observed in the first place) if analysts are specifically attending to that possibility. Finally, because the task of explaining institutional outcomes is better framed as an issue of institutional development rather than one of institutional selection. Institutional development, in turn, cannot be adequately treated without attending to issues incorporating an extended time frame, including the role of time horizons, unintended consequences, learning and competitive selection processes, and path dependence.