

According to Gerring, “each case may provide a single observation or multiple (within-case) observations (19) . . . For those familiar with the rectangular form of a dataset [i.e., a data matrix], it may be helpful to conceptualize observations as rows, variables as columns, and cases as either groups of observations or individual observations” (22). Gerring then advises the use of experimental or quasi-experimental methods to analyze variation across within-case observations. Yet for practitioners of case studies, the advice seems problematic; as more and more observations are compared, and the inferential leverage afforded by experimental or quasi-experimental methods is brought to bear, it seems less likely that one is really conducting a case study—that is, an intensive analysis of one or several instances of a phenomenon (19–20).

The concluding chapter on process tracing, co-authored with Craig Thomas, seems to come closest to shedding light on the distinctive contributions of case-study research to causal inference. In process tracing, not-strictly comparable pieces of information are combined in a way that adds up to a convincing causal account, by rendering alternative explanations less plausible while showing that microevidence is consistent with theoretical claims. Process tracing may be akin to detective work; bits of evidence about the maid, the butler, and the suspect are combined to formulate or investigate a central hypothesis about who committed a crime. In a different but related account, Collier, Brady, and Seawright (in *Rethinking Social Inquiry*, 2004) describe how what they term causal-process observations can provide a smoking gun that demonstrates—or rules out—a particular causal hypothesis. Unfortunately, the discussion of process tracing is a relatively short addendum to the core concerns of Gerring’s book.

Definitional slippages are one distracting feature of this book, despite the inclusion of a glossary and careful attention to defining terms. At one point, for instance, Gerring notes parenthetically, “I use the terms proposition, hypothesis, inference, and argument interchangeably” (22). Nevertheless, articulating the distinctive contributions to social science of case studies has been a core challenge for qualitative methodologists, and together with other recent contributions in this area (Brady and Collier, *Rethinking Social Inquiry*, 2004; George and Bennett, *Case Studies and Theory Development*, 2005) Gerring’s volume takes on this challenge in a spirited fashion. It will provide useful and engaging reading for substantive researchers of all methodological stripes.

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*Politics in Time: History, Institutions, and Social Analysis*. By Paul Pierson. (Princeton University Press, 2004.)

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In *Politics in Time*, Paul Pierson has written an important book that is engaging, ambitious, and provocative. Its purpose is essentially threefold: to advocate that political scientists situate arguments in temporal perspective, to illustrate a number of ways in which they might do so, and to argue that much of the discipline does not presently take time seriously enough. The book is oriented not merely toward qualitative scholars or historical institutionalists, but rather “those interested in the attempt to develop claims about the social world that can potentially reach across space and time” (7)—effectively all social scientists who seek to advance generalizable explanations. In his effort to reach such a broad audience, Pierson engages widely with the discipline and beyond, drawing upon theoretical insights from Kenneth Arrow to Arthur Stinchcombe, exploring causal mechanisms from positive feedback to absorbing Markov chains, and offering substantive examples from U.S. congressional committees to state building in early modern Europe.

The publication of Pierson’s book in 2004 occurred in the midst of a still-ongoing resurgence of interest in qualitative methods and temporal arguments—stimulated in no small part by Pierson’s *American Political Science Review* article on path dependence published in 2000. Following an initial series of articles on the sources of institutional lock-in, a second wave of scholarship by authors such as Kathleen Thelen and Jacob Hacker shifted the focus to ways in which institutions change rather than remain stable over time. *Politics in Time*, which presents revised versions of four previously published articles as well as an entirely new introduction, fifth chapter, and conclusion, plays the very useful role of encapsulating this evolution of the literature and also offering an attempt at synthesis. Ultimately, as Pierson argues in Chapter 5, change and continuity must be seen as two sides of the same coin.

*Politics in Time* begins with a focus on institutional continuity via path dependence and positive feedback. Examining the mechanisms that sustain stability over time in economic history, Pierson argues that such processes should be at least as common in the political realm. Reasons include the prevalence of collective action in politics; the potentially self-reinforcing accumulation of power asymmetries; the absence of a price mechanism to clearly indicate

optimal behavior; and the centrality of formal institutions, which have large set-up costs and generate learning effects, coordination effects, and adaptive expectations. When political actors begin to travel down a self-reinforcing path—even one that is eventually considered inferior to the road not taken—reversal is unlikely because of the short time horizons of many political actors and the obstacles to change built into many political institutions.

Having established the importance of positive feedback mechanisms, the second chapter of Pierson's book examines the closely related issue of timing and sequence. Here, he advocates an approach that combines the insights and precision of rational choice models of legislative cycling with the attention to large-scale social changes that have been the mainstay of historical institutionalism and American political development. Sequencing arguments, he maintains, need not be "Dr. Seuss-style," ad hoc explanations in which it "just so happened" that one event followed another. Rather, the best of these arguments illustrate, often via comparison, how the order of particular events matters crucially for the eventual outcome. Examples include the self-reinforcing incumbency advantages enjoyed by a party that initially "fills up" a political space, such as by organizing the working class. Future competitors for this constituency will have greater difficulty than if they had been the first ones to occupy the same political niche.

The third chapter of Pierson's book shifts the focus from arguments emphasizing continuity to those that focus on long-term change. Many topics in political science, Pierson argues, are those in which cause immediately precedes effect and both occur relatively quickly. The tendency to limit time horizons of research topics in this fashion, however, ignores a number of important processes, such as cumulative causes, threshold effects, and causal chains, in which the cause or the outcome unfolds over a significant period of time or there is substantial temporal separation between the two. Pierson argues that the trend toward research involving only short-term independent and dependent variables risks excluding many of the classic subjects in political science, such as party-system change and state building. An exclusively short-term focus may even contribute to errors of causal inference if scholars mistakenly treat slow-moving causal variables as mere background conditions.

An excessively static approach to political science can occur not only in the selection of research topics, but also in the explanation of institutional origins—an

argument Pierson develops in the fourth chapter. Here, *Politics in Time* takes aim at "actor-centered functionalism"—the rational-choice claim that institutions exist because farsighted, purposive, and instrumental actors benefit from them. Drawing upon numerous empirical examples, Pierson lays out six limitations to this perspective. Institutions may have multiple or unanticipated effects, such that their existence cannot be explained by a simple reading of their creators' preferences. Designers also may adopt institutional forms for noninstrumental reasons, such as diffusion or cultural specificity. Actors may have short time horizons, creating institutions with undesirable long-term effects, or their preferences may change over time as institutions remain stable. Finally, political actors themselves may change; a new generation may inherit institutions that reflect previous actors' preferences rather than their own.

In recognition of these limitations to the rational choice perspective, many historical and sociological institutionalists have begun to articulate mechanisms of how institutions change over time. Yet in Chapter 5, Pierson argues that shifting from an assumption of static institutions to an exclusive focus on fluidity involves too much of a swing of the pendulum. Research on institutional change, which has identified mechanisms such as critical junctures, layering, conversion, and diffusion, places undue emphasis on the malleability of institutions at the hands of entrepreneurs or the losers from previous rounds of political competition, and it does not predict when we should expect one type of institutional change versus another. Most importantly, arguments about institutional change often lose sight of the sources of institutional stability, such as coordination problems, veto points, asset specificity, and positive feedback. Thus, Pierson concludes by proposing five distinct research agendas on processes of institutional development that seek a synthesis between arguments about change and continuity.

As mentioned at the outset, the objective of Pierson's book is not only to illustrate ways in which political scientists can incorporate the temporal dimension into their research, but also to argue that much of the discipline does not presently take time seriously enough. While acknowledging that rational choice scholars and quantitative methodologists often *can* model long-term historical processes, Pierson insists that theoretical possibility does not translate into typical practice. The main evidence to support this claim is presented in Chapter 3, where he shows that in several top political science journals, articles investigating short-term causes and outcomes vastly

outnumber those in which change in an independent or dependent variable occurs over a long period of time. Yet in attributing this outcome to the rise of rational choice analysis and quantitative methods, Pierson moves into the realm of assertions that—while plausible—he does not have data to support.

Similarly, Pierson argues that statistical tools for estimating complex temporal relationships “frequently go unexploited” (168), and that while it is possible to incorporate long-term sociological processes into rational choice explanations of comparative politics, “they are not the kinds of hypotheses that these analysts typically go looking for” (100). Yet it is questionable whether typical practice is so myopic at a time when Beck and Katz’s 1995 article on time-series cross-sectional methods is the ninth most cited article in the history of the *American Political Science Review*, and Acemoglu and Robinson’s (*Economic Origins of Dictatorship and Democracy*, 2005) rational choice analysis of long-term processes of democratization has generated so much excitement among comparativist formal modelers. Quantitative and rational choice scholars certainly approach the temporal dimension in a different manner than does Pierson, but his point is that it is rare for these scholars to take time seriously *at all*. His critiques of tendencies in the discipline may ultimately be accurate, yet they would be much stronger if they were accompanied by a more convincing empirical demonstration of actual practice.

Despite overstepping the bounds of its own evidence when critiquing current practice on the quantitative and formal side of the discipline, Pierson’s book nonetheless offers an excellent demonstration of best practice on the qualitative side as well as a convincing claim that all political scientists should take time seriously. Some will certainly dispute the book’s arguments, but one suspects that that is precisely the author’s intention. Indeed, the greatest value of *Politics in Time* lies in the fact that is certain to stimulate (and indeed, has already begun to stimulate) an important process of debate about temporal processes in politics and the ways in which different research traditions can better address them in the future.

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*Progress in International Relations Theory: Appraising the Field*. Edited by Colin Elman and Miriam Fendius Elman. (MIT Press, 2003.)

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Colin Elman and Miriam Fendius Elman have produced a refreshingly coherent, thought-provoking, and engaging volume dedicated to the use of Imre Lakatos’s methodology of scientific research programs (MSRP) for appraising progress in the field of International Relations (IR). After the editors’ detailed overview of MSRP and the controversies it has generated in the philosophy of science community and other disciplines, Part I of the book moves into a series of applications by leading figures in IR. Robert Keohane and Lisa Martin reconstruct institutional theory according to MSRP, followed by Jonathan DiCicco and Jack Levy on power transition theory, Andrew Moravcsik on liberal IR theory, James Ray on the democratic peace, Stephen Walker on operational code analysis, Robert Jervis on the debate between realist and neoliberal IR theories, Randall Schweller on neoclassical realism, and Jack Snyder on the empirical aspects of normative research in areas like ethnic conflict. Part II contains a mixture of commentary on the aforementioned applications, critiques of MSRP, and arguments for alternative rationalist models of scientific development provided by David Dessler, Roslyn Simowitz, John Vasquez, and Andrew Bennett. The coherence of the book derives in part from strong editorial guidance in the overview of MSRP, but also a conference held in Arizona in 1999, where the ideas that shaped this book were debated in spirited fashion.

This book is a fine example of methodological work that bridges the quantitative-qualitative divide. The model derived from Lakatos’s dense prose is clearly operationalized by Elman and Elman in Chapter 2 to guide subsequent applications (see also the handy brief guide on pages 19–20). The following chapters describe key elements of theories or successions of theories contained in well-known IR research programs. The contributors charged with applying MSRP reconstructed their research programs adductively by comparing observations with the model.

In keeping with the adductive method, many of the contributors questioned the operational definitions created by the editors as they were piecing together evidence and theory in their respective research programs. There were disagreements over the editors’ preferences for heuristic novelty as the operational definition of novel facts, with Moravcsik and Bennett making strong cases for the use of background theory novelty. Most contributors noted the difficulty of identifying all of the elements of a scientific research program in a nonarbitrary manner. Determining what counts as the hard core of a research program is complicated by Lakatos’s