



THE  
OXFORD  
HANDBOOKS  
OF  
POLITICAL  
SCIENCE

GENERAL EDITOR  
ROBERT E. GOODIN

EDITED BY  
CARLES  
**BOIX**  
SUSAN C.  
**STOKES**

≡ The Oxford Handbook of  
**COMPARATIVE  
POLITICS**

THE OXFORD HANDBOOK OF

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POLITICS**

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**COMPARATIVE  
POLITICS**

*Edited by*

CARLES BOIX

*and*

SUSAN C. STOKES

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**PART I**

**INTRODUCTION**



## CHAPTER 1

# INTRODUCTION

CARLES BOIX  
SUSAN C. STOKES

WHY do authoritarian states democratize? What accounts for the contours, dynamics, and ideologies of the nation-state? Under what conditions do civil wars and revolutions erupt? Why is political representation channeled through political parties in contemporary democracies? Why do some parties run on policy programs, others on patronage? Can citizens use elections and courts to hold governments accountable?

These are some of the crucial questions that comparative political scientists address. And they are the questions, among others, around which this volume is organized. We asked a set of top scholars in the field of comparative politics to write critical surveys of areas of scholarship in which they are expert. We assembled the volume with two guiding principles. First, we are committed to the possibility (and desirability) of generating a systematic body of theoretical knowledge about politics. The discipline advances, we believe, through theoretical discovery and innovation. Second, we embrace a catholic approach to comparative methodology. In the following paragraphs we offer an overview of our authors' contributions, with occasional critical commentary of our own or additional thoughts on the directions in which future research should go.

## 1 THEORY AND METHODS

The questions posed above and others that our contributors raise are too complex, and too important, to restrict ourselves to one or another methodology in our attempts to answer them. It is not that, methodologically speaking, "anything



goes;" some research designs and methods for gathering and analyzing evidence are not fruitful. But our contributors explain the advantages and pitfalls of a wide range of techniques deployed by comparativists, from econometric analysis of cross-national datasets and observational data to extended stints of fieldwork. They employ a variegated toolkit to make sense of political processes and outcomes.

Among the starkest shifts in comparative politics over the past two decades is the rise of statistical studies of large numbers of countries. Most graduate students in comparative politics who studied in leading departments in the 1960s through the 1980s were trained to conduct research in a single region or country. Indeed, the very term *comparative* was in most cases misleading; comparative politics frequently entailed not making comparisons but studying the politics of a foreign country. With slight exaggeration one could think of this as the State Department approach to comparative politics, where one scholar staffs the "Japan desk," another the "Chile desk," etc. Of course there were important exceptions. One was Almond and Verba's *The Civic Culture*, which compared citizens' attitudes in five countries. Still, it would have been hard to predict circa the 1970s or even 1980 the degree to which comparative politics would come to prominently feature large-N cross-national studies.

Our volume, significantly, includes two studies that take stock of what we would lose should the traditional comparative enterprise, with its emphasis on close knowledge of the language, history, and culture of a country or region, be abandoned altogether, and should the activity supporting that approach, the extended period of work in the field, be lost along with it. John Gerring contends that neither case studies nor large-N comparisons are an unalloyed good; rather, both entail tradeoffs, and we are therefore well advised as a discipline to retain both approaches in our collective repertoire. Where case studies are good for building theory and developing insights, Gerring argues, large-N research is good for confirming or refuting theory. Where case studies offer internal validity, large-N studies offer external validity. Where case studies allow scholars to explore causal mechanisms, large-N comparisons allow them to identify causal effects.

Elisabeth Wood's chapter alerts us to what we are in danger of losing should we as a profession give up on field research. To the rhetorical question "Why ever leave one's office?" she gives several answers. Interacting personally with subjects in their own setting may be the only way to get a handle on many crucial research questions, such as which of many potential political identities subjects embrace and what their self-defined interests are. Fieldwork is not without perils, Wood explains, both intellectual and personal. Interview subjects may be evasive and even strategically dissimulating; field researchers may have strong personal reactions, positive or negative, to their subjects, reactions that may then color their conclusions; and fieldwork is a lonely endeavor, with predictable highs and lows. Wood suggests strategies for dealing with these pitfalls.

James Mahoney and Celso Villegas discuss another variant of qualitative research: comparative historical studies. The aims of this research differ from

those of cross-national studies, they contend. Comparative historical scholars "ask questions about the causes of major outcomes in particular cases," and hence seek to explain "each and every case that falls within their argument's scope." By contrast, large-N researchers "are concerned with generalizing about average causal effects for large populations and... do not ordinarily seek to explain specific outcomes in particular cases." Mahoney and Villegas discuss recent methodological developments in comparative historical research, such as the identification of necessary and sufficient conditions, the use of Boolean algebra to uncover interactive causal effects, and fuzzy-set logic. They also address some of the criticisms of comparative historical research, such as the reliability and generalizability of the historical record. They tout both secondary and primary source research.

One might press Mahoney and Villegas to go a step further in their definition of primary historical research. They cite as primary sources "government documents, newspapers, diaries, and bulletins that describe past events at roughly the time they were occurring." Yet, with the exception of diaries, these printed documents fail to meet the historian's criterion of a manuscript source. Unpublished manuscript or archival sources—internal memos, individuals' notes on organizational debates, correspondence among political actors, spies' accounts—are the functional equivalent for historians of personal interviews for field researchers, which (as Wood explains) can be the best window into an actor's identity, strategic calculations, and interests. Government documents, newspapers, and published bulletins, while useful, represent a version of "events as they were occurring" that has been produced for public consumption. This particular critique raises broader questions about the adequacy of training of many social scientists who undertake historical research.

Robert Franzese's chapter defends large-N, quantitative techniques against some of the critiques that other contributors level against them. Comparative political scientists, like empirically oriented sociologists and economists, are bedeviled by four problems: a tradeoff between quantity and quality in the collection of data; multicausality; context-conditionality, that is, the fact all the effects of our variables are conditional on other variables; and endogeneity. Yet, as Franzese argues, these obstacles, which are in fact inherent in our trade, should not lead us to dodge quantitative strategies of research. On the contrary, a simple, back-of-the-envelope calculation shows that the plausible loss of precision involved in measuring large numbers of observations does not justify retreating to qualitative studies of a few cases—even if we attain very precise knowledge about small samples, they fail to yield robust inferences. Similarly, the presence of multiple and conditional causality cannot be solved easily by case studies (although good process tracing may alleviate these problems). Finally, qualitative case study research does not necessarily escape from problems of endogeneity. To move from correlational analysis to causal propositions, Franzese contends, we need to employ more sophisticated techniques, such as variable instrumentation, matching, or vector autoregression. But even these techniques are not sufficient. Here we would like to add that, influenced by a few macroeconomists and political economists, part of the discipline seems on the verge of uncritically embracing the use of

instrumentation to deflect all the critiques that are leveraged against any work on the grounds that the latter suffer from endogeneity. It turns out that there are very few, if any, instruments that are truly exogenous—basically, geography. Their use has extraordinary theoretical implications that researchers have either hardly thought about (for example, that weather determines regime, in a sort of Montesquieuian manner) or just avoid (when they posit that the instrument is simply a statistical artifact with no theoretical value on its own and then insist that it is the right one to substitute for the variable of interest). Thus, we want to stress with Franzese that only theory building can truly help us in reducing the problem of endogenous causation.

Adam Przeworski offers a less optimistic perspective on observational research, large-N or otherwise. Observational studies, ones that do not (and cannot fully) ensure that the cases we compare are matched in all respects other than the "treatment," cannot deal adequately with problems of endogeneity. "We need to study the causes of effects," he writes, "as well as the effects of causes." Some covariates (traits of a unit that it has prior to the application of a treatment) are unobserved. These unobserved covariates may determine both the likelihood of a unit's being subjected to the treatment and the likelihood of its evincing the effect. Because these covariates are unobserved, we cannot test the proposition that they, rather than the treatment or putative cause, are actually responsible for the effect.

Przeworski discusses traditional as well as more novel approaches to dealing with endogeneity, but his chapter leans toward pessimism. "To identify causal effects we need assumptions and some of these assumptions are untestable." His chapter will be essential reading for comparativists as they assess the promise and limitations of observational versus experimental or quasi-experimental designs.

But perhaps the mood of the chapter is more pessimistic than it need be. Theory should help us distinguish cases in which endogeneity is plausibly present from ones in which it is not. One way of reading Przeworski's chapter is that a crucial research task is to shift key covariates from the unobserved to the observed category. This task is implied by a hypothetical example that Przeworski offers. A researcher wishes to assess the impact of governing regime on economic growth. Future leaders of some countries study at universities where they become pro-democratic and learn how to manage economies, whereas others study at universities that make them pro-dictatorial and teach them nothing about economic management. Both kinds return home to become leaders and govern their societies and economies in the manner consistent with their training. It therefore appears that democracy produces economic growth. The training of leaders is a variable that we cannot observe systematically, in Przeworski's view. But there is a difference between unobserved and unobservable. It is not obvious to us why this variable could never be systematically observed, should our theory—and, perhaps, our close, case study-informed knowledge—tell us that we should worry about it.

Whether one studies a large or small number of cases, and whether one employs econometrics or other techniques, Robert Bates argues that one should do theoretically sophisticated work informed by game theory. Indeed, the use of game-theoretical models, of varying degrees of formalization, is a strong recent

trend in comparative politics. Illustrating his methodological claims with his recent research on the politics of coffee production and commercialization, Bates offers a strategy of comparative research that, in a way, revisits all of the chapters that constitute Part II of the volume. The first step of research is apprehension: a detailed study and understanding of a particular time and place. *Verstehen* is then followed by explanation: the researcher apporions the things she knows "between causes or consequences" and attempts to develop "lines of logic to link them." In Bates's view, the explanatory drive should begin with the assumption (or principle) of rationality and use game theory to impose a structure on the phenomena we observe. The structure of the game allows us to push from the particular to the construction of broader theories, themselves susceptible of validation. The construction of theoretical explanations must be then subject to the test of confirmation: this implies progressively moving from small-N comparisons to much larger datasets in which researchers can evaluate their theories against a broad set of alternatives and controls.

The final contributor to our theory and methodology section also explores the role of rationalist assumptions in comparative research. Elinor Ostrom takes as her point of departure the proposition that "the theory of collective action is *the* central subject of political science" and that the problem of collective action is rooted in a social dilemma (or, in game theory terms, a prisoner's dilemma) in which, as is well known, rational individuals in pursuit of their optimal outcome may end up not cooperating even if it was in their interest to do so. Ostrom assesses the first generation of studies of collective action, which stress the structural conditions (number of players, type of benefits, heterogeneity of players, the degree of communication among them, and the iteration of games) that may increase the likelihood of achieving cooperation. She finds these studies wanting. Ostrom recognizes that the rationalist model only explains part of human behavior. Hence she calls for a shift towards a theory of boundedly rational, norm-based human behavior. Instead of positing a rationalistic individual, we should consider agents who are inherently living in a situation of informational uncertainty and who structure their actions, adopt their norms of behavior, and acquire knowledge from the social and institutional context in which they live. In this broader theory of human behavior, humans are "adaptive creatures who attempt to do as well as they can given the constraints of the situations in which they find themselves (or the ones that they seek out)." They "learn norms, heuristics, and full analytical strategies from one another, from feedback from the world, and from their own capacity to engage in self-reflection. They are capable of designing new tools—including institutions—that can change the structure of the worlds they face for good or evil purposes. They adopt both short-term and long-term perspectives dependent on the structure of opportunities they face." All in all, her approach encompasses a broader range of types of human action, from instances in which individuals exhibit "complete rationality" (normally in those environments in which they live in repetitive, highly competitive situations) to more "sociological agents" for which their rules of action are derived from shared norms. To some extent, the discipline seems to come full