

OXFORD HANDBOOK OF COMPARATIVE POLITICS

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EDITORS' INTRODUCTION TO THE HANDBOOK

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 our authors' contributions, with occasional critical commentary of our own or additional thoughts on the directions in which future research should go.

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 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 is 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. Field work 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 field work 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 farther 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’ account – 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 trade-off 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 Montesquieueian manner) or simply dodge (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 must 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 paper 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 I 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 apportions 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 data sets 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. Eleanor 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 round with this contribution: moving from cultural approaches under the aegis of modernization theory to the rationalist assumptions of institutionalist scholars and now back to a richer (perhaps looser but certainly closer to the way our classical thinkers thought about human nature) understanding human agency. This journey has not been useless. On the contrary, as we traveled from one point to the other we have learned that a good theory of politics must be based on solid microfoundations, that is, on a plausible characterization of interests, beliefs and actions of individuals.

STATES, STATE FORMATION, AND POLITICAL CONSENT

The institutional and ideological foundations of the modern national state are central concerns of comparative politics. Hendrik Spruyt considers the institutional dimension of state formation. Spruyt provides a bird's-eye overview of recent contributions to our understanding of state formation, an area of research that has grown exponentially in the last three decades. He reviews the ways in which the modern state, with its absolute claims of sovereignty over a particular territory and population, formed and displaced all other forms of governance. This change came in response to a shift in war technology, the growth of commercial capitalism, and new ideas about legitimate government. Spruyt also examines several influential and still-unsettled debates about what caused the emergence in the modern period of distinct types of constitutional and administrative regimes. Most studies of state-building have focused on Europe in the modern period; the recent emergence of independent states outside of Europe states in the last centuries is not adequately explained by these accounts. As Spruyt notes, state formation in the 20th century allows us to evaluate the extent to which the international system, the economy, and the colonial legacy affect how sovereignty and legitimacy have expanded across the globe.

Other chapters consider the ideological dimensions of state formation and of intra-state identity conflict. Russell Hardin's chapter lays bare the difficulties in positing legitimacy as the ideological foundation of national states. Hardin warns against the fallacy of assuming that the existence of a political arrangement means that those subject to it deem it to be "legitimate." Hardin's reflections on legitimacy as a positive and normative concept underscore the limitations of the concept, at least in the ways it has been deployed by comparativists. Social scientists and political theorists typically ascribe legitimacy to a regime, Hardin explains, based on "how it

came into existence, what it does for us, or our relationship to it both historically and now.” But none of these grounds for assessment is firm. In Hardin’s view, the dominant, Weberian definition amounts to equating legitimacy with a state’s capacity to stay in power. But this coding would have us attribute legitimacy to regimes which, from the vantage point of both those who live under them and those who examine them from a distance, fall far short of legitimacy.

The ideological underpinnings of the modern state are also the subject of Liah Greenfeld and Jonathan Eastwood’s contribution. They define national identity as a secular understanding of the self and its attachments, the vision of the world as partitioned into separate communities, and a notion of popular sovereignty. In contraposition to well-known arguments that stress either the perennialism of nationalism or its modern emergence, they claim that nationalism arose in modern times as a response to an upsetting of traditional hierarchies. Faced with the dissolution of the old concepts of status, individuals reinterpreted their position as one of belonging to a nation of equals. Within this shift in ideas, Greenfeld and Eastwood explore the distinct dimensions of nationalism: the criteria for membership in the nation and the images that a community creates about the relationship between the collectivity and each individual. The authors use these dimensions to develop a typology of nationalisms.

Nationalist states in the contemporary world are sometimes riven with conflict, and these conflicts have stimulated much theory-building and research in comparative politics. Assessing the literature of ethnic identity and conflict, Ashotosh Varshney describes how a very young field of research has grown and progressed by taking seriously both the need to look for causal mechanisms and the need to explain empirical variation. His chapter tracks the fruitful dialogue among scholars through several sequential theoretical layers: essentialism, which initially

dominated the field and now has been mostly superseded by the idea that nations are modern constructs; instrumentalism, which posits ethnic groups and nations as concepts that derive from material benefits and self-interest; constructivism; and institutionalism. Varshney's essay engagingly describes the advances as well as the limits of each school and offers ideas about how a blending of elements from each may help advanced our research agenda.\

POLITICAL REGIMES AND TRANSITIONS

Given the democratic revolution of the past quarter century, it is scarcely surprising that democracy has been a central concern – perhaps the central concern – of comparative politics. Christian Welzel and Ronald Inglehart aim to restore a role for mass beliefs in the process of democratization. In so doing, they offer an important methodological insight. They contend that certain kinds of mass beliefs make democratization (and authoritarianism) more likely, especially “societal-led” democratization. Evidence that this is true can be found in surveys applied to mass publics in countries which vary in their degrees of democratization. Yet because they fear committing an ecological fallacy, social scientists have been wary of drawing inferences from these data. The fear, Welzel and Inglehart suggest, is based on an equally debilitating “individualist fallacy.” Researchers commit an individualist fallacy when they (1) find that correlations that hold at the aggregate level do not also hold at the individual level, and then (2) infer that these correlations are therefore meaningless.

This, they insist, is a mistake: the discovery of a potential ecological fallacy may itself be theoretically illuminating. The disjuncture between correlations in democratic values among individuals and in societies is illuminating in just this way. Welzel and Inglehart contend that the presence of certain values in high levels in a population creates a pro-democracy climate in

the population as a whole, even though these values do not co-vary strongly at the individual level. The presence of these values in aggregate is predictive of effective democracy. Although some readers may remain skeptical about the last link, between mass beliefs and democratic institutions, the authors' methodological point, as well as their substantive claims, will be thought-provoking for many students of comparative democratization.

Mass attitudes or beliefs of an undifferentiated kind play little role in Barbara Geddes's theories of democratization, or in the theories she reviews. Instead, these theories focus on more narrowly defined actors: rich people and poor people, or regimes that seek to maximize their own political control versus regimes that act as perfect agents of the wealthy. Despite comparativists' near-obsession with democratization, Geddes argues, we have few firm and uncontested conclusions about democracy's causes. Our empirical results in this area, furthermore, are less robust than one would like, changing in theoretically important ways depending on the sample of countries studied, on the time frame considered, and on the nature of specifications (e.g., does the model include or exclude country fixed effects?). The problem is not an absence of theory; our theories of democratization have become increasingly sophisticated and explicit. Rather, Geddes suggests, the problem may lie in heterogeneity of the explanandum, democratization. Transitions from absolutist monarchy to constitutional monarchy or to republics may be fundamentally different than transitions from modern military dictatorship to mass democracy. Separating these distinct phenomena, analyzing them – and, more to the point, developing distinct theories of them – is the key, in her view, to gaining firmer knowledge of why countries democratize.

With the exception of Hobbes, the relationship between civic culture and political regimes has been one of the central preoccupations of all modern political theorists. Embracing

the new methods that characterized the new, self-consciously empirical political science that emerged after World War II, Almond and Verba in the 1960s tackled this secular concern in their highly influential book on civic culture. Yet, as Sabetti aptly explains, this attempt to put the study of the relationship on solid empirical grounds proved unsuccessful. The problem with this research agenda had less to do with the (still) very contentious notion of culture than with the ways in which researchers categorized democracy and political culture. They entertained too limited a conception of democracy, restricted to the institutional mechanisms that determine governance at the national level. They thus disregarded the vast number of democratic practices that operate at the local level and in intermediate social bodies. They defined political culture, in turn, as a set of beliefs and dispositions toward a certain political objects. But this notion proved to be unsatisfactory: the role that these beliefs and attitudes played in sustaining democratic life and practices was unclear; their origins remained unknown; and, from a purely empirical point, there was no clear proof that democratic stability was bolstered by a particular democratic culture. Yet, it was precisely at the time when the political culture approach had gone down “a degenerative path” that researchers rescued the concept of culture and hence the problem of its political effects by stressing its eminently relational nature. In the late 1980s, Gambetta put trust back into the research agenda. Several researchers emphasized the need to understand interpersonal networks to explain particular behaviour. Coleman drew on game theoretic concepts to develop the notion of social capital. And Putnam then transformed our way of understanding governance and culture in his famous study of Italian regional politics. This new approach is, as Sabetti insists, still in its infancy – we know little (both theoretically and empirically) about the mechanisms that go from social capital to good governance and next to nothing about the dynamics that create, sustain or deplete civic virtue. And some of us doubt

that trust, as opposed to an engaged scepticism, is the appropriate posture of citizens in democratic polities. But the new approach may well be putting us in the right path to “untangle the complex relationship between democracy and civic culture.”

More than 30 years ago, Juan Linz wrote a highly influential piece on dictatorships for the *Handbook of Political Science*, edited by Fred Greenstein and Nelson Polsby. Linz’s approach was mostly conceptual and sociological and drew on the literature on totalitarianism and authoritarianism that had been developing since World War II. Nondemocratic regimes, according to Linz, could be defined by their degree of internal pluralism, their ideology, and the level of political mobilization which they demanded of their subject populations. Preoccupied with the mechanisms that sustained dictatorships and the choices dictators and their subjects made, Ronald Wintrobe offers here a different account that starts from economic or rationalist assumptions. To rule, dictators have to combine some degree of repression with the construction of political loyalty. Given the two variables – repression and loyalty – and the objective functions dictators may have, Wintrobe distinguishes between tinpot dictators (who maximize consumption and minimize repression levels), totalitarian dictators (intent on maximizing power), tyrants (who repress without achieving much ‘loyalty’) and timocrats (who invest in creating loyalty and gaining their citizens’ love). Wintrobe presents evidence about the behavior of dictators that is supportive of this typology, and explores the ways in which democracies and dictatorships compare in terms of economic growth and economic policy-making.

POLITICAL INSTABILITY, POLITICAL CONFLICT

Revolutions, civil wars, and social movements are central objects of study in comparative politics. Blending his training as a historian with a keen interest in comparative analysis, Steven

Pincus examines the historical conditions that generate revolutionary episodes. He asks, why do revolutions occur and why do they have dramatically different outcomes? Scholars have argued that revolutions occur exclusively as a result of social and economic modernization (Skocpol, Huntington). More recently, an influential line of argument, brought forth by Goldstone, has framed revolutions as the outbreak that follows a Malthusian imbalance between a growing population and its environment. By contrast, according to Pincus, the necessary prerequisite for revolution was always state modernization. State modernization programs simultaneously bring new social groups and new regions into direct contact with the state, and legitimize ideologies of change. These two developments create a social basis and a language on which to build revolutionary movements. Revolutions lead to very different political outcomes. In part following in the steps of Barrington Moore, Jr., Pincus argues that revolutions lead to open, democratic regimes when the state relies on merchant communities and foreign trade. Absent the latter, however, revolutions typically result in the imposition of an authoritarian regime.

Where Przeworski alerts us to the omnipresence of endogeneity problems, Kalyvas alerts us to their centrality in a subject that reality has placed centrally on the agenda of comparativists: civil wars. Kalyvas reviews a plethora of studies of civil wars that offer a plethora of independent variables: features of the societies before the civil war broke out, or features of combatants in their pre-war incarnations. These pre-war-outbreak features of societies and combatants ostensibly explain the likelihood of civil wars' occurring, their duration once they occur, or the intensity of the violence they unleash. But such exogenous explanations, Kalyvas explains, may be wrong-headed: much changes as civil wars unfold, including the distribution of populations, the preferences of key actors, and the value of resources over which combatants seek control. These new, war-driven conditions are themselves likely to shape the outcomes of

interest. “Collective and individual preferences,” he writes, “strategies, values, and identities are continuously shaped and reshaped in the course of a war, while the war itself aggregates all kinds of cleavages from the most ideological to the most local.”

Sidney Tarrow and Charles Tilly examine contentious politics (episodic public collective action) and social movements (sustained challenge to holders of power). They analyze the ways in which these contentious politics and social movements happened in a dynamic sequence. The authors observe that modernization and the spread of democracy spawned the invention of social movements. Yet, at the same time, the time and location of social movements (that is, their interaction with political institutions, society and cultural practices) determined the form in which they emerged. Tarrow and Tilly conclude by reflecting on the impact that globalization may have on the processes of political and social mobilization as we know them. They ask whether globalization “more or less automatically connect potential activists across the world, present them with similar challenges, and thus move social movement collective action away from local and national concerns?” Their answer is, probably not: domestic political factors and involvement of national states in international organizations are the best predictors of participation in “transnational contention.”

Lichbach and deVries’s chapter complements that of Tarrow and Tilly by surveying theories of contentious politics in light of recent global protest movements. To fully understand the phenomenon of contentious politics, they remind us that we need to operate at three levels. At the macro level researchers have developed a vast array of explanations, that span from precise economic structural theories (such as the impact of trade on the welfare of populations) to cultural hypotheses (for example, the impact of modernization on the perception of elites in underdeveloped countries) to the emergence of a global civil society or global institutions that

permit generalized protest and act as focal points. These macro-level stories must be complemented with meso-level causes, in particular the insights of strategic political opportunity theory, that make protest feasible. Finally, understanding contentious politics involves comprehending the micro-level components of action: the motives that bring individuals to the fore, their resources, their prior commitments, and the networks that rear them in political action.

MASS POLITICAL MOBILIZATION

Why do party systems look the way they do? How do their origins help explain their contemporary dynamics? What explains dramatic differences in the strategies that parties deploy in their efforts to mobilize electoral support? These questions animate the contributors to this section of the volume.

Carles Boix presents a multi-stage yet compact account that helps explain how parties and party systems developed in Western Europe and North America from rather loose networks of politicians, catering to small and strictly delimited electorates, in the early 19th century to mass-based, well organized electoral machines in the 20th century. This chapter does not limit itself to explain, as in most analyses, how many parties effectively compete, but what kinds of parties espousing which ideologies. Boix traces the nature of parties and party systems back to the underlying structures of preferences, which could be either uni- or multidimensional. But, he then shows how these preferences or political dimensions were mobilized as a function of several additional key factors: the parties' beliefs about which electoral strategy would maximize their chances of winning, and the electoral institutions that mediate between voters' choices and the distribution of seats in national parliaments. (These electoral institutions, as Boix has shown in earlier work, were themselves the product of strategic action of parties.) In a way, the chapter

may be read as a response to two types of dominant approaches in the discipline: those institutionalist models that describe political outcomes as equilibria and that, somehow trapped in static applications of game theory, hardly reflect on the origins of the institutions they claim constrain political actors; and those narratives that stress the contingency and path-dependency of all political phenomena while refusing to impose any theoretical structure on them. By contrast, we think it should be possible to build historical accounts in which we reveal (1) how political actors make strategic choices according to a general set of assumptions about their beliefs and interests and (2) how their choices in turn shape the choice set of future political actors.

Where Boix develops an integrated model of the origins of distinctive party systems, Herbert Kitschelt offers of a broad review of the questions that scholars ask about party systems and the way they answer them. Why do democracies feature parties in the first place, as almost all do? Why do many parties compete in some democracies whereas in others competition is restricted to two major parties (or two major and one minor one)? Why do some parties compete with the currency of programs, others with valence issues, and still others with clientelism and patronage? Why are elections perennially close in some systems, lopsided in others? Kitschelt reviews the measures that scholar find helpful in answering these questions – party-system fractionalization, the effective number of parties, electoral volatility, and cleavages. The problems afflicting party politics are regionally specific: whereas scholars of advanced industrial systems worry, as Kitschelt notes, about the decay of party-voter linkages, scholars of new democracies worry about whether such linkages will ever take shape.

Several contributors to our section on mass political mobilization explore the question, under what conditions do political parties adopt distinct political strategies? Strategies may vary

from appeals to identity and nationalism, to personalistic and media-centric campaigns, to programmatic offers, to clientelistic linkages. Ann Wren and Kenneth M. McElwain identify a shift toward personalistic and media-heavy campaigns in Western Europe, a shift from the more organizationally grounded strategies of parties during the periods analyzed by Boix and by Kitschelt. One of the central insights of the comparative work done in the 1960s was that partisan attachments and party systems had remained frozen since the advent of democracy in the West. Yet, as this chapter explains, in the last forty years party-voter linkages have substantially thawed. Economic growth, the decline of class differences, and the emergence of postmaterialist values lie in part behind this transformation. In the wake of changes in the electorate and its preferences, it took party bureaucracies some time to adjust. Taking advantage of the slow rate of adjustment of the older parties, new parties sprang up to lure away dissatisfied voters.

Yet party dealignment and electoral volatility have not diminished, even after new parties that should have stabilized the electoral market have entered these party systems. Therefore, to explain continued volatility, we must look beyond changes in the structure of voter preferences. As Wren and McElwain stress, weakening party-voter ties must be put in the context of a shift in the educational level of the population and new technologies (radios and TV). As parties became less important as informational short-cuts, politics has grown more candidate-centered and party elites have been able to pursue electoral campaigns without relying on the old party machinery. If Wren and McElwain are right, our old models of, and intuitions about, party-centered democracy should give way to a more 'Americanized' notion of democracies, where personal candidacies and television campaigns determine how politicians are elected and policy made.

Chapters by Frances Hagopian and by Susan Stokes consider the origins and effects of clientelistic linkages between parties and voters. Hagopian addresses questions such as why do some parties build loose and heterogeneous coalitions of voters, or narrow constituencies that are linked by religious affiliations or programmatic preferences? And what effects do the parties' choices have? "Is there a relationship," she asks, "between who is mobilized, how they are mobilized, and how stable or successful the voter mobilization strategy is?" Her highly suggestive answers raise questions about the prospects for stabilization of party systems and electoral processes in developing democracies.

In the last two decades, democracy has become the dominant system of government across the world, both as a normative ideal and as a fact. But not all nominal democracies generate accountable, clean governments. Susan Stokes addresses one of the possible causes of malfunctioning democracies by looking at the practices, causes, and consequences of clientelism. Clientelism, or the "proffering of material goods [by the patron] in return for electoral support [by the client]," was a hot topic of research in the 1960s and 1970s, buoyed by the emergence of new nations. Shaped by a sociological approach, researchers at that time explained clientelism as a practice underpinned by a set of norms of reciprocity. Yet, as Stokes claims convincingly, clientelism must be rather seen as a game in which patrons and clients behave strategically and in which they understand that, given certain external conditions (such as a certain level of development and the organizational conditions that allow for the effective monitoring of the other side), they are better off sustaining a pattern of exchange over the long run. Such a theoretical account then allows us to make predictions, which are beginning to be tested empirically, about the institutions underpinning clientelistic practices, the electoral strategies pursued by patrons, and the potential economic and political effects of clientelism: whether it

depresses economic development and political competition.

Pippa Norris surveys the very large literature on political activism. She reviews the social and psychological model of participation developed by Verba and Nie, as well as the critiques generated from a rational-choice perspective. She then examines how key developments in the research community and the political world have affected the ways in which we evaluate this subfield. She notes a growing interest in the role of institutions in shaping participation in general and turnout in particular. Echoing Wren and McElwain, she draws our attention to changes in party membership, which was widespread and hence instrumental in many advanced democracies but has progressively shrunk, with consequences that are still widely debated among scholars. The constructs of trust and social capital, pioneered by Coleman and Putnam, are also relevant to our expectations about levels of participation. Norris also identifies cause-oriented forms of activism as a distinct type of participation, activism that includes demonstrations and protests, consumer politics, professional interest groups, and more diffuse “new” social movements and transnational advocacy networks. All of these, she notes, have expanded and in a way marginalized the more institutionalized, party- and union-based mechanisms of participation that dominated in the past.

PROCESSING POLITICAL DEMANDS

In the magisterial five-volume *Handbook of Political Science* mentioned earlier, published 30 years ago, the term *accountability* appears not once. The term *representation* appears sporadically and, outside of the volume on political theory, only a handful of times. Thirty years later, in our volume, *accountability* appears as an organizing concept in comparative politics, and *representation* is not far behind. The chapters in the current volume in the section

Processing Political Demands are deeply engaged with the concepts of accountability and representation.

In democracies, how do citizens' preferences get translated into demands for one public policy over another? This is the fundamental question that G. Bingham Powell takes up. If everyone in a society had the same preferences, the problem would not be a problem at all. But never is this the case. And scholarship on preference aggregation, as Powell notes, must come to grips with social-choice theory, which should lead us to doubt that citizens in any setting in which politics is multidimensional can evince any stable set of policy preferences. The dominant strains of research, some of which come to grips with the social-choice challenge and others of which ignore it, include examinations of the congruence of various sorts. One kind of congruence study looks at the fit between constituents' preferences and the issue positions of their representatives. Another looks at the fit between electoral outcomes and the allocation of elected offices, treating, as Powell notes, citizens' policy preferences as though they were fully expressed by their votes. Another sort of congruence study examines the coherence of issue positions among co-partisans, both political elites and citizens who identify with parties, and tend to find a good deal more coherence among the former than among the latter. Yet another deals with the congruence between electoral platforms and campaign promises, and government policy. Powell's overarching concern is about the potential for accountability and representation in democratic systems, and how this potential is best realized by certain institutional arrangements and political contexts.

Rein Taagapera goes at the question of the expression of citizen preferences through elections from a more institutional vantage point, focusing on electoral rules. After offering a typology of electoral systems, he reviews the "Duvergerian agenda" of electoral rules, that is, the

analysis of the ways (mechanical and psychological) in which electoral systems affect the voting behavior of electors and , as a result, the election of candidates, the structure of parties and party systems, and the politics of coalition building in democracies.

Shifting from voting behavior and elections to institutional politics, David Samuels reviews what we know about the impact of the separation of powers on accountability. The conventional view in the United States is that a separation of powers is so central to democratic accountability that this separation is nearly definitional of democracy. Samuels evaluates this proposition empirically. His own research and that of other authors which he reviews address questions of accountability and representation, as well as the effects of a separation of powers on the policy process and on regime stability. Among his central findings is that presidentialism has several deleterious effects; a separation of executive from legislative powers increases the chances for policy deadlock and for the breakdown of democracy.

The institutional design of judiciaries and of their relations with other branches of government is meant to produce horizontal, if not vertical, accountability (O'Donnell 1994). John Ferejohn, Frances Rosenbluth and Charles Shipan's contribution on judicial politics considers the institutional and political settings in which the courts attain independence, especially from executives but also from legislatures, independence which O'Donnell and others consider a necessary condition for vertical accountability. Ferejohn, Rosenbluth, and Shipan also explain other aspects of crossnational variation, such as why courts everywhere are not enabled to carry out judicial review and why courts are sometimes more active in the legislative process, other times less.

Assessing judicial independence, as these authors acknowledge, is not always straightforward. They advocate two measures: the frequency with which courts reverse

governments, and the frequency with which they reverse governments that nationalize parts of the economy (or attempt to do so). The authors note that a drawback of either approach is that courts, which seek (among other objectives) not to have their decisions reversed, may rule against governments only when they anticipate not being reversed, in which case these measures would tend to overestimate their independence. Another difficulty is that courts may rule in favor of governments when they find government's actions to be lawful or when they spontaneously agree with governments' actions. Hence, whereas rulings against governments probably indicate independence, rulings in their favor are less certain indications of dependence (see Helmke 2002, 2005).

The two final contributions in this section consider aspects of government structures that may have significant impacts on accountability, both vertical and horizontal. Pablo Beramendi provides an overview of the concept of federalism. He shows that federalism was first introduced to accommodate the interests of the periphery in the military and economic affairs of a union. Yet federalism is necessarily a complex, fluid institutional form. This insight then shapes the rest of the review. The relationship between democracy and federalism seems to be conditional, as far as we know, on the particular internal structure of federalism. The effects of having a federal structure on the economy, in turn, depend on how the federal institutions allocate power and responsibilities between the center and regional governments. Naturally, this opens up the question about the origins of federalism. Without a strong theory of how and when federal institutions are adopted, it is difficult to identify the independent effects of federalism.

Kare Strom and Benjamin Nyblade critically assess the literature on coalition making, particularly regarding the formation of governments in parliamentary democracies. Drawing on neoinstitutionalism and, more specifically, on the transaction-costs literature, they show how the

costs of negotiation and the demands of the electorate, interested in monitoring parties' performance, reduce cycles and push politicians to strike relatively stable pacts. They note that theories of coalition formation began with William Riker's application of the "size principle," which predicted that parties would try to minimize the number of actors in a coalition. Although influential theoretically, this approach proved to be rather unsatisfactory empirically. In response, Strom and Nyblade relax Riker's fundamental assumptions about payoffs, about the role of information, and about the effects of decision rules and institutions, to reach a much richer theory, and one that fits the data more closely.

GOVERNANCE IN COMPARATIVE PERSPECTIVE

The "discovery" of economic voting several decades ago transformed the fields of comparative voting behavior and party competition. It was thought to depict a simple rule-of-thumb that voters could – and did – apply when deciding whether to vote for incumbents: if the economy had performed well on their watch, retain them, if it hadn't, turn them out. Recent scholarly developments place economic voting in institutional contexts and present more nuanced stories about what voters need to know to carry off "simple" economic voting. Raymond Duch's chapter reflects and advances this new agenda. Duch develops a series of propositions about how varying institutional contexts, coalition governments, and informational settings will mediate between economic conditions and voters' appraisal of them. Factors that Duch suggests will influence economic voting include party-system size, the size of government, coalition governments, trade openness, and the relative strength of governing and opposition parties in the legislature. Duch offers empirical evidence that sheds light on these mediating factors.

Ever since a seminal paper published by Nordhaus in 1975 launched research into the political business cycles, the study of the effect of elections on policy-making has had to contend with substantial theoretical inconsistencies – why should voters accept policy manipulation and leave governments unpunished? – as well as considerable empirical disagreements. What scholars tend to agree about most is that the presence of politically-induced economic cycles is rather irregular. With these problems in mind, James Alt and Shanna Rose’s essay pursues dual objectives. They argue that political business cycles must be understood as a particular instance of the broader phenomenon of political accountability in democratic regimes. Political business cycles are not merely the result of a signaling game in which politicians try to build their reputation as competent policy-makers. Rather, the manipulation of economic policy and outcomes is an inevitable result of voters’ willingness to accept the transfer of some rents to politicians in exchange for the election of competent policy-makers. In their empirical analysis of American states, Alt and Rose implement a model that predicts that political manipulation of the economy will occur under certain institutional and social conditions: when elections are close, when voters are not very well informed, and in the absence of budgetary rules constraining policymakers’ room for maneuver.

Isabela Mares examines the evolution of the certainly very crowded field on the welfare state. Echoing the well-known essay Amenta and Skocpol wrote two decades ago, Mares masterfully reviews the different theoretical contributions in the area. After the first papers and books on the topic were written within the framework of modernization theory, welfare state scholars moved to assess the impact of power politics (through parties and unions) on the construction of different types of welfare states. That class-based orientation, however, had limited validity beyond some archetypical cases with high levels of union mobilization and

strong left-wing parties. Accordingly, researchers switched to explore the impact of cross-class coalitions – hence dwelling on the role of middle classes, agricultural producers and employers. In doing so, they have shifted our attention from the pure redistributive components of the welfare state, which were the keystone of pure class-based, power politics accounts, to social policies as insurance tools that address the problem of risk and volatility in the economy. Related to this change in perspective, welfare state scholars have progressively spend more time on mulling over the impact of the international economy on social policy. Two path-breaking pieces by Cameron and Katzenstein showing economic openness and the welfare states to be positively correlated have been followed by an exciting scholarly debate that has alternatively related the result to a governmental response to higher risk (due to more economic volatility in open economies), denied the correlation completely, or called for models that take openness and social policy as jointly determined. As Mares’ essay reveals, the welfare state literature has indeed traveled a long way from its inception. Yet it still has a very exciting research agenda ahead of it: first, it should become truly global and extend the insights (and problems) of a field built around Europe and North America to the whole world; second, it should offer analytical models that combine the different parameters of the successive generations of research in the area; third, it should take seriously the preferences and beliefs of voters across the world (and the cultural differences we observe about the proper role of the state); and, finally, it ought to integrate the consequences of welfare states (something about which we know much less than we should) with the forces that erect them.

Whether the transition to democracy in many developing countries in recent decades has meant a shift to accountable, effective government is a question that has concerned many scholars of comparative politics. Reviewing the burgeoning literature on development and

democracy, Philip Keefer notices that, although both the number of researchers and the theories on the topic have multiplied considerably, we still know little about the relationship between growth and political regimes. In particular, he points to the fact that policy and performance vary considerably across democracies. Poor democracies show lower growth rates and worse public policies than rich democracies. In a nutshell, in spite of having formal mechanisms that should have increased political accountability and the welfare of the population in poor democracies, the provision of public goods and economic performance remain thoroughly deficient in those countries. Since the key parameters of democracy and redistribution (inequality and the struggle for political control between elites and non-elites) cannot explain that outcome (since low development and democratization are cast as contradictory), Keefer turns to political market imperfections to explain the failure of governments to deliver in democracies. In young, poor democracies, politicians lack the credibility to run on campaigns that promise the delivery of universal benefits and public goods. Accordingly, they shift to building personal networks and delivering particular goods. This type of electoral connection, compounded by low levels of information among voters, who can scarcely monitor politicians, results in extreme levels of corruption and bad governance.

The promise of economic voting was that voters would be able to use economic conditions as a measure of the success or failure of governments; the anticipation of being thus measured would induce politicians to improve economic conditions on their watch. Economic voting would enforce accountability. Yet, as José María Maravall shows in his contribution to this volume, “in parliamentary democracies losses of office by prime ministers depend in one half of the cases on decisions by politicians, not by voters.” This fact would not be so dire if prime ministers were removed from office by colleagues who anticipated bad electoral outcomes

– if, as Maravall puts it, “voters and politicians . . . share the same criteria for punishing prime ministers.” But they do not. Whereas prime ministers are more likely to be turned out by voters when economic times are bad, they are more likely to be turned out by their colleagues when economic times are good. Hence politicians who hold their comrades to account seem to practice a reverse kind of “economic voting.” Maravall’s chapter cautions us against excessive optimism regarding democracy, accountability, and economic voting.

If (as economic voting implies) office holders who produce bad economic outcomes will face the wrath of voters, why would they ever risk a costly transition to liberalized economy? Whether asked in the context of post-Communist countries undertaking a “leap to the market” or in developing countries elsewhere in the world under pressure to move away from statist policies, the question has preoccupied comparative politics and political economy for more than a decade. Reviewing the literature on economic transitions in Eastern Europe, Timothy Frye identifies a number of factors, from the quality of domestic governance to membership in the European Union that make governments more likely to undertake reforms and then stick with them. Yet serious gaps remain in our understanding of the determinants of market reforms, including what role is played by institutional legacies from the past, and by contemporary social institutions – networks, business associations, reputational mechanisms – state institutions – courts, bureaucracies, legislatures – and the interaction of the two.

LOOKING AHEAD

By critically assessing the existing literature in their area of expertise, most, if not all, of the contributors to this volume already point toward the research questions and gaps that our discipline stills to address. Thus, we will refrain from paraphrasing and summarizing them again.

We just want to invite the reader to read them and mull over their suggestions carefully. That should be enough to push many a scholar to plunge into yet-to-be-discovered waters. Still, we wish to close this introduction by writing briefly about the broad issues raised to us by the rather long preparation and shepherding of this volume.

The scientific inquiry of comparative politics has certainly shifted in the last decades or, one may say, over the course of the last three generations of scholars devoted to this field, in at least two ways. First, the ways in which theory is built have changed markedly. Probably influenced by the then dominant approaches of structural sociology and Marxism, in the past comparatists relied on systemic, broad explanations, to explain political outcomes. Just think of the initial theories of political modernization, the first articles relating democracy to development or the work on party formation laid out in the 1960s. Today, theory-building very often proceeds (or, perhaps more modestly, claims to proceed) from ‘microfoundations’, that is, it starts from the individual, and her interests and beliefs, to then make predictions about aggregate outcomes. We found this to constitute a truly forward step in political science. Making us think hard about the final unit of analysis of the model, that is, about each individual (and his motives and actions), allows us to have theories that are more transparent (i.e. where one can truly probe the consistency and plausibility of assumptions) and easier to falsify.

At this point it is important, however, to pause to stress that embracing the principle of methodological individualism does not necessarily mean accepting a purely instrumental or rationalist model of human action. As is well known, our increasing reliance on microfoundations has been triggered to a considerable degree by an influx of mathematical and game-theoretic tools and by the influence of economic models in the discipline. But, as Moon already discussed in the Greenstein-Polsby handbook thirty years ago, models built on

propositions about how individual actors will behave under certain circumstances may well employ a variegated set of assumptions about the interests and beliefs of the actors themselves. In fact, his claim (and our hunch) is that the only way to show that rationalistic assumptions do not work is to build models that are populated by intentional actors (with goals that are not strictly instrumental) and that these models perform better than those developed by rational choice theorists. To sum up, building theories of intentional actors and constructing models of (strictly) rationalist individuals are two different enterprises. The latter needs the former but the reverse is not true. Realizing that difference should save to all of us what has been a considerable source of conflict and confusion.

Coming to appreciate the role of individuals and their motives has also had a very beneficial effect on comparative politics. It has moved it closer to our forefathers in the discipline. Each classical theory of politics, from Aristotle and Machiavelli to Hobbes, Locke, Rousseau and Nietzsche, starts from a particular conception of human nature. With different tools and with a different data set (for one, we have some information about how real democracies work in practice), all these different (micro) models are, at the end, grounded on specific assumptions about human behavior. These assumptions are still deeply contested in comparative politics: they span from a purely instrumental conception of political actors intent on securing survival and maximizing power to a notion of individuals that may consent to particular structures contingent on others cooperating to, finally, visions of politics that appeal to the inherent sociability of humans. This contestation is unavoidable. Our guess is that as we all move to build intentional models of politics, it should become easier to adjudicate between different points of departure.

The second way in which the discipline has changed has to do with the gradual

acceptance among most researchers about the need to develop broad, general propositions about politics and about the value of employing standard scientific practices to provisionally validate them (until they are disconfirmed). Interestingly, this growing consensus has come with an equally increasing and valuable skepticism about how much it can be accomplished by employing quasi-experimental methods of the kind comparatists usually employ. The problem is perhaps compounded by the fact that comparative politics cannot rely on something like microeconomic theory to keep building models (while empirically oriented researchers battle over what and how to test any of their propositions). (We say “perhaps” because not having something akin to a microeconomic theory makes our work less constrained and therefore less forgetful about all the traits of human behavior that violate the strict assumptions of rationality.)

For the provisional solutions to this question, which has mostly to do with endogeneity issues, we again refer the reader to the essays of the first part. Here we wish to present this question as an opportunity rather than as a problem. In recent years, the field of comparative politics has progressed substantially in modeling certain political outcomes, mostly as equilibria. Duverger’s law has become clarified and formalized through models of strategic coordination. Civic virtue has given way to models of trust sustained by repeated interaction. Patronage politics can be profitably thought of as a game in which patrons and clients are interlocked. But, we still know little about the ways in which political institutions, social practices, norms and arrays of political interests originate and collapse. History was important in the broad, sociological literature written a few decades ago. Yet, the way in which it was tackled was messy or unsystematic. Institutionalists altogether abandoned historical work. We think that, with the new tools we have in our hands, the right time has come to deal with that question again. To some extent, given the problems of endogenous causation we are confronted with, engaging in

this type of work is now becoming inevitable.