



ROUTLEDGE
HANDBOOKS



Routledge Handbook of Comparative Political Institutions

Edited by Jennifer Gandhi and Rubén Ruiz-Rufino

ROUTLEDGE HANDBOOK OF COMPARATIVE POLITICAL INSTITUTIONS

The *Routledge Handbook of Comparative Political Institutions* (HCPI) is designed to serve as a comprehensive reference guide to our accumulated knowledge and the cutting edge of scholarship about political institutions in the comparative context. It differs from existing handbooks in that it focuses squarely on institutions but also discusses how they intersect with the study of mass behavior and explain important outcomes, drawing on the perspective of comparative politics. The Handbook is organized into three sections:

- The first section, consisting of six chapters, is organized around broad theoretical and empirical challenges affecting the study of institutions. It highlights the major issues that emerge among scholars defining, measuring, and analyzing institutions.
- The second section includes fifteen chapters, each of which handles a different substantive institution of importance in comparative politics. This section covers traditional topics, such as electoral rules and federalism, as well as less conventional but equally important areas, including authoritarian institutions, labor market institutions, and the military.
- The final section, encompassing seven chapters, examines the relationship between institutions and a variety of important outcomes, such as political violence, economic performance, and voting behavior. The idea is to consider what features of the political, sociological, and economic world we understand better because of the scholarly attention to institutions.

Featuring contributions from leading researchers in the field from the US, UK, Europe and elsewhere, this Handbook will be of great interest to all students and scholars of political institutions, political behavior, and comparative politics.

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With a stellar line-up of brilliant authors and a wide range of chapter topics, the *Handbook of Comparative Political Institutions* should be indispensable to all scholars of comparative politics and political institutions.

Anna Gryzmala-Busse,
University of Michigan

From the birth of modern political science in the early 20th century to contemporary rational choice institutionalism, political institutions have been a defining subject of our discipline. This Handbook covers the full terrain of analytical ideas and empirical regularities discovered by this great tradition and also points the way forward for future scholarship. This is a “must have” for any serious political science collection.

Simon Hix,
London School of Economics

Gandhi and Ruiz-Rufino have brought together an outstanding set of scholars to provide a comprehensive treatment of political institutions and their impact. This volume will be essential reading for those interested in exploring the variety of ways in which the formal (and sometimes informal) rules of the game affect political, economic and social outcomes. A major contribution.

Daniel Posner,
University of California

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INTRODUCTION

Jennifer Gandhi and Rubén Ruiz-Rufino

Introduction

“Institutions matter” has served as a mantra for the social sciences for almost thirty years. In political science, scholars in the subfield of comparative politics embraced this intellectual agenda by first “bringing the state back in” (Evans *et al.* 1985). Since then, comparative politics has focused much attention on institutions, and at this point, it seems appropriate to ask: What is our accumulated knowledge about political institutions in the comparative context, and how have we acquired it? Where have debates in the study of institutions emerged and how have scholars resolved them? What remains to be done in the comparative study of institutions? In this volume, we address these questions by having leading scholars in the field discuss their areas of substantive and methodological expertise.

The volume is organized into three parts. [Part I](#) focuses on broad theoretical and empirical challenges affecting the study of institutions. The six chapters of this part highlight the major issues that emerge among scholars defining and analyzing institutions. The chapters discuss how these challenges have been handled since the “institutionalist revolution” in political science and highlight the open questions that still bedevil the study of institutions. [Part II](#) of the volume consists of fifteen chapters, each of which handles a different substantive institution of importance in comparative politics. In this part, we cover institutions, such as electoral rules and federalism, whose study is well established in the field. But we also expand our focus to include areas that traditionally either have attracted less attention or have not been aggregated as a separate area within comparative politics. These include authoritarian institutions, the military, and electoral management bodies. The final part, [Part III](#), encompassing seven chapters, examines the relationship between institutions and a variety of important outcomes, such as political violence, economic performance, and voting behavior. The idea is to consider what features of the political, sociological, and economic world we understand better because of the scholarly attention to institutions.

Because our contributors discuss the state of our knowledge and what constitutes the research frontier in their specific areas, each chapter offers its own answers to the questions posed above. But on the basis of these chapters, we also can offer a more synthetic account of what we broadly know about institutions, how we know it, and what remains to be studied. We begin by discussing how comparative scholars have developed institutionalist theories: how they have borrowed from other subfields, confronted the tension between generalist and more time- or place-specific

claims, and used a variety of tools to craft their arguments. We then go on to discuss two recurring themes in the volume that highlight how many comparative scholars view institutions: first, the use of institutions to address strategic dilemmas and, second, how institutions both incentivize behavior and are created by purposive action. Understanding institutions both as rules and as equilibria opens up a host of theoretical and empirical challenges. We review these challenges that confront the general study of institutions before discussing the open debates surrounding some of the specific substantive institutions that have been highlighted by authors in the volume. These last two sections clearly show that while we have accumulated substantial knowledge about comparative institutions, there is still much work to be done.

The sources and scope of our institutional theories

In developing theoretical arguments about the emergence and effects of institutions, it is clear that many areas of comparative institutional analysis have borrowed and built upon the frameworks first developed in the study of American politics. In his chapter on legislative organization and outcomes ([Chapter 11](#)), Eduardo Alemán discusses the rationale for legislative rules as well as their impact in distributing gatekeeping and agenda-setting powers which, in turn, influence passage rates and the substantive content of legislation. Many of the theories—explaining both why a majority of legislators would adopt organizational arrangements that occasionally make them worse off and how such organizational rules can pull policy outcomes away from the median legislator’s preferences—have been developed in the context of studying behavior in the U.S. House of Representatives. As Alemán notes, scholars in comparative politics are using these theoretical frameworks to make sense of legislative behavior and outcomes in other places around the world, and in the process they are able to comment on the extent to which these theories are generalizable beyond the American context.

Similarly, in his chapter on fiscal institutions ([Chapter 20](#)), Joachim Wehner points out that much of the early empirical work on budget institutions focused on the American states. These works focused on the effects of institutions such as formal budget rules and legislative committee structures on fiscal adjustment and policy outcomes. Due to the variation in institutions and outcomes across a large number of comparable units, the American states serve as a good laboratory to investigate the effects of fiscal institutions. In recent years, as comparative scholars have increasingly accumulated cross-national data on budget institutions, policies, and outcomes, they have been able to test theories that were developed within the American context as well as others that take into account comparative institutional differences.

In some contexts, this crossover continues to be influential in the development of comparative studies. In other cases, scholars are questioning to what extent institutional theories or theories of institutions developed within the context of established democracies (e.g. the US, Western Europe) are helpful in understanding these same types of institutions in other areas of the world. In his chapter on party politics ([Chapter 10](#)), Noam Lupu tackles this issue by noting that the variation in how parties emerge, how they persist or change over time, and how they differ in their strategies for appealing to voters in many parts of the world cannot be fully accounted for by extant theories of parties that were developed in the context of American and Western European parties. The discrepancy between received theories and empirical realities can be handled in two different ways. *Splitters* argue that theories derived from the experiences of the advanced democracies fail to apply to other places, and as a result alternative explanations must be developed. *Lumpers*, in contrast, attempt to generate broad theoretical frameworks that incorporate prior theories of party politics but still enable us to make sense of diverging empirical patterns.

The challenge, of course, is in building theories that are general but that still account for historically and contextually distinct paths. The difficulty of the endeavor should not be underestimated, as noted by Gabriel Negretto in his chapter on electoral systems ([Chapter 9](#)). Most explanations of the origins and reform of electoral systems draw from the experience of Western European states at the beginning of the twentieth century that underwent a shift from majoritarian to proportional systems. But, as Negretto argues, it is clear that outside of Western Europe, proportional representation (PR) was adopted under historical conditions that were quite different: greater uncertainty over which systems would benefit established parties, political elites without full control over the reform process, and other goals of reform besides short-term partisan interests. As a result, while the adoption of PR in places such as Latin America can be considered within the general framework of actors behaving strategically to achieve their interests, historical specificities may render impossible any attempt to generate a general theory of electoral reform.

Of course, there are sometimes very good contextual reasons for limiting the scope of our research questions and theories. In their chapter on the military ([Chapter 18](#)), Aurel Croissant and David Kuehn observe that the research questions surrounding the study of the military vary by regime type. In authoritarian regimes, the focus has been on understanding military coups and military regimes. In new democracies, the most significant issue is how to establish civilian control over the military, while in established democracies scholars examine how civil–military relations play out when norms and institutions already have established civilian primacy over the military. That the research agenda is segmented by regime type is, in part, a reflection of real-world concerns. Establishing civilian control, for example, is a critical question for many new democracies, but less so for established democracies. But since these research foci are interrelated and countries sometimes transition across regimes, scholars studying the military under different regimes still have much to learn from each other. In investigating how contemporary new democracies might establish civilian control, for example, the historical experience of established democracies in tackling this issue may be instructive.

Moreover, that contextual differences exist is not a reason to cease thinking and conversing broadly about institutions. For one, the substantive context studied by scholars in one subfield may have explicit relevance for phenomena under investigation by scholars in another subfield. James Raymond Vreeland ([Chapter 22](#)) directly addresses this type of symbiosis between comparative politics and international relations scholars. Governments must be incentivized to join international organizations (IOs). Interstate relations may strongly structure their incentives, but so do domestic factors. So in order to understand why states join IOs, how IOs are structured and operate, and what are their effects, it is critical to know something about the domestic politics of these states. Comparative scholars, in turn, will have a better understanding of some features of domestic institutions, policies, and outcomes if they factor in the effects of IOs.

More broadly, as the examples in this chapter and across the chapters in the Handbook illustrate, the structure of the problem confronting actors in one substantive context often resembles the one that emerges in a very different environment. The challenge for scholars, then, is to consider how specifics of a place or time require modification of extant institutional theories to account for the specific phenomena of interest. In order to do so, they must know about those existing theories which may require knowledge about an institution in another time and place. For this reason, in their chapter on the study of institutions ([Chapter 3](#)), Tom Clark and Jennifer Gandhi call for more collaboration not only across subfields, but also across substantive areas. To take one example: if the object of study is the agency problem among parties within a coalition government, it is useful not only to know how coalition governments operate in many parts of the world, but also to learn about how the agency problem functions in other substantive contexts (e.g. bureaucrats and legislators, elected officials and voters).

Formal theory, as Milan Svoblik argues in his chapter ([Chapter 6](#)), is well placed to enable scholars to engage in this type of inquiry across substantive areas. Institutional rules usually delineate the participants, their available actions and information, and the procedures by which their actions translate into outcomes—all elements required in a game theoretic model. Furthermore, the participants often have conflicting interests, requiring them to behave strategically to get what they want. Whether this means making strategic decisions within the constraints established by institutional rules or making them about the choice of institutions, the rationality assumptions underpinning formal theory are reasonable even if models necessarily require some abstraction from reality.

Institutions as solutions to strategic dilemmas

Because institutions are explicitly rules, incentives, and constraints on individual actors, scholars studying institutions have become more attuned to the microfoundations of their explanatory accounts. As Diermeier and Krehbiel (2003: 126) point out: “The crucial link between *institutions* (as contextual constraints) and *outcomes* (as consequences of collective choice) is *behavior*.” In order to determine that institutions affect outcomes and how they do so, it is critical that we draw as sharp a line as possible between institutions and behavior, and specify and show how these incentives and constraints influence actions. Theoretically, scholars have been thinking carefully about the strategic dilemmas that confront political actors and how institutions address (or do not address) them. Throughout the work on a variety of different substantive institutions, the problems of coordination, commitment, and agency appear, and institutions serve as solutions to these dilemmas.

Coordination

Actors may be better off coordinating their actions, but individual rationality frequently does not lead to collective rationality. In such situations, institutions may act as a coordinating device. It is possible, for example, to organize the structure of government in a variety of ways, but constant renegotiation over it is detrimental to political life. Institutional instability inhibits the formation of plans and expectations and prevents actors from solving “lower-order” problems. Constitutions, as Tom Ginsburg points out in his chapter ([Chapter 8](#)), may address this problem by laying out a structure of government that serves as a focal point for actors. By offering a blueprint for how government works, a constitution enables individuals to coordinate their beliefs, expectations, and actions. Besides enabling action, however, constitutions can serve to constrain government within limits. By generating an intersubjective understanding of what constitutes appropriate state action, constitutions may help coordinate citizens so that they can credibly threaten punishment of a government that exceeds its limits.

In his chapter on judicial institutions ([Chapter 14](#)), Julio Ríos-Figueroa observes that courts may serve a similar function. Courts may convey relevant information about government misconduct and signal when rulers have overstepped constitutional bounds. Thus, judicial rulings serve an important informational role that aids citizens in determining when government has committed a transgression, which is the first step in developing a coordinated response to restrain it.

Commitment

When actors’ preferences are time inconsistent, institutions may enable them to make credible commitments. As Ríos-Figueroa points out, judicial institutions play this role as well. Courts can

serve as a commitment device for an executive or legislature interested in signaling its intention to protect property rights. He points out that courts can serve as such a commitment device, however, only in so far as their rulings command compliance—not a trivial detail given that courts lack enforcement power.

The problem of enforcement also plagues institutions as a means for resolving conflict. The problem is that peace agreements—like constitutions and laws—are just pieces of paper. So any promises to disarm or refrain from violent actions made now may not appear credible in the future, especially if the actors involved retain their capacity to use force. In her chapter on the relationship between institutions and political violence ([Chapter 26](#)), Laia Balcells notes that there are domestic and international institutions that can alleviate this commitment problem among civil war combatants, influencing the likelihood of war termination and the duration of peace. But there is no “one size fits all” solution, and as a result, “there are a lot of context-dependent factors that affect their likelihood of success” (p. 381).

Agency

When actors delegate authority to an agent, problems of hidden information or hidden action frequently emerge. Institutional structure often determines the extent of agency loss. In their chapter on federalism ([Chapter 15](#)), Pablo Beramendi and Sandra León point out that the degree to which the regions are able to shirk or the center “overawes” them depends on the precise nature of federal institutions, including their fiscal authority as well as the interdependence between the national and regional electoral arenas. As a result, the variance in outcomes among federal countries is often as large as what exists between federal and unitary states.

In some cases, institutions are able to directly minimize the degree of agency loss. Lanny Martin and Georg Vanberg in their chapter on parliamentary government ([Chapter 13](#)), for example, discuss the agency problem among parties within a coalition government. Parties must figure out how to insure that their coalition partners will enact policy that is acceptable to the coalition as a whole for the ministries that they control. Martin and Vanberg highlight the importance of strong legislative institutions in enabling coalition partners to monitor and enforce policy agreements and thus mitigate the moral hazard problem.

Institutions-as-rules and institutions-as-equilibria

Institutions and their effects

If actors use institutions to address particular strategic dilemmas, it must be true that they believe that institutions have important effects on behavior and outcomes. Otherwise, there would be little point in employing such institutions as solutions. So it seems appropriate that much of the literature on institutions examines how they serve as rules or constraints on behavior and, as a result, have important effects on a variety of outcomes.

Institutions often shape the opportunities available for political mobilization. In her chapter ([Chapter 25](#)), Erica Chenoweth points out that one of the most noteworthy findings of the literature on mobilization is that significant extra-institutional action takes place in precisely those countries with many formal, legal, and viable institutional channels through which people can express their preferences. In support of her point, Chenoweth reviews several hypotheses and the scholarly work behind them, examining the effects of institutions, such as regimes, electoral rules, and political parties, on protest activities.

Similarly, Ian McAllister ([Chapter 23](#)) observes that voting is “not simply a personal choice, but one that is shaped by the context within which the choice is made” (p. 333). Institutions play an important role in determining whether individuals vote, whether they vote strategically or sincerely, whether they develop partisan identification, and for whom they vote. And the range of institutions that play a role in structuring this choice is large, including voter registration laws, ballot structure, electoral systems, and legislative structure. The challenge is in uncovering the interaction between system-level and individual-level characteristics in determining these decisions.

Institutions shape the choices not only of individuals, but also of governments. In her chapter on institutions and economic development ([Chapter 29](#)), Luz Marina Arias focuses on the ability of governments to credibly commit to refrain from opportunistic behavior; to protect private property rights and enforce contracts; and to provide public goods and infrastructure. She argues that these three features of state behavior are critical in accounting for the substantial variation in economic performance across countries and also are heavily influenced by institutions.

Institutions as outcomes

If institutions address strategic dilemmas, however, it also means that they are human constructs, chosen under constraints and incentives. They cannot exist—at least, not with any stability—unless critical actors agree to their existence. As such, institutions are endogenous and can constitute equilibria.

International organizations, for example, are composed of member states, and there often are domestic reasons why states do (or do not) join these organizations and covenants. As Vreeland discusses ([Chapter 22](#)), governments may join international organizations or agreements to solve domestic problems, marginalize opposition, or deflect blame for unpopular policies. As such, the focus is on understanding the domestic factors that contribute to making international organizations stable institutional equilibria.

For new democracies and states recovering from domestic conflict, transitional justice institutions offer a way for their societies to deal with their authoritarian and violent legacies. There are normative reasons for establishing these types of institutions, but, as Monika Nalepa discusses in her chapter ([Chapter 27](#)), the process by which these institutions emerge is purely political. Consequently, in order to understand why some countries adopt transitional justice institutions and the different forms they take, it is necessary to focus on the electoral incentives that mold parties’ preferences for these institutions and the partisan and legislative features that influence their abilities to establish them. In Nalepa’s account, institutions play an important role in shaping the emergence of other institutions.

Challenges and opportunities in studying institutions

Numerous chapters in the volume make clear that the study of particular substantive institutions has included both lines of inquiry—to varying degrees. What is less clear is how to think about the joint implications of the fact that institutions influence outcomes and that they are endogenous. Thinking about institutions-as-rules and institutions-as-equilibria generates significant theoretical and empirical challenges for the study of institutions. The fundamental problem lies in establishing that institutions exhibit a true causal effect in the face of critical issues such as epiphenomenality, selection effects, and reverse or simultaneous effects.

Institutions may be epiphenomenal, or, at the very least, have a more modest role to play in influencing behavior and determining outcomes. In their chapter on authoritarian institutions

(Chapter 19), Clara Boulianne Lagacé and Jennifer Gandhi discuss the many reasons why autocrats govern with institutions that are familiar to students of democracy: legislatures, parties, elections, courts. One claim in the literature is that these institutions can constrain autocrats so that they can credibly commit to share power with elites or desist from predatory behavior. It is unclear, however, to what extent the institutions—or the balance of power behind them—act as the real constraint on autocratic behavior since such institutions appear to be malleable. The difficulty in sorting out the issue exists also at the empirical level due to the difficulty of measuring the balance of power between leaders and their supporting coalitions in autocracies.

Selection effects also may render the relationship between an institution and an outcome spurious. Susan Hyde and Kevin Pallister (Chapter 17) discuss two institutions that *potentially* have a significant influence on election quality: electoral management bodies (EMBs) and election observation missions (EOMs). EMBs are the institutions that run elections: they carry out essential tasks such as registering voters, regulating campaigns, conducting the polling and counting of votes, and resolving electoral disputes. By performing these tasks in ways that do not advantage one side over the other, EMBs can increase the quality of elections. But in describing the effect of these institutions on election quality, the use of the word “potentially” is intentional. As Hyde and Pallister point out, scholars have not yet uncovered why some countries adopt “better” EMB models than others. As a result, it may be the case that countries that are more likely to adopt “good” (e.g. non-partisan) EMB structures are the same ones that are likely to hold high-quality elections. Because EMBs are a choice, the factors driving that choice may be more determinative of electoral outcomes than the EMBs themselves.

Finally, reverse or simultaneous causation may characterize the relationship between an institution and an outcome. In his chapter (Chapter 16), Victor Lapuente reviews the factors that contribute to the rise of a merit-based bureaucracy. In doing so, he tackles the difficult relationship between economic development and a well-functioning state apparatus. Does development lead to the rise of a meritocratic bureaucracy? Or does such a bureaucracy encourage the flourishing of the economy? The answer to both questions is most likely “yes,” complicating efforts to determine the independent impact of bureaucracy on important economic outcomes.

But the fact that institutions are, at the same time, the product of choices and act as incentives for other choices opens up other large promising areas for research: institutional change and institutional bundles. We discuss each topic in turn.

First, institutions may have effects which sometimes provide incentives for actors to maintain or change these very same institutions. In his chapter (Chapter 4), Adam Przeworski examines this type of endogenous institutional change in a notoriously difficult context—that of economic development. In some situations, endogenous change may occur through a decentralized process. Przeworski claims that Marx was one of the first to provide such a theory in explaining how the actions of individual capitalists would result in the downfall of their economic system. In other cases, elites who control resources and political power make conscious choices—in response to economic conditions—that result in a more centralized process of change. But complications may emerge because new institutions do not always “take.” As Przeworski notes: “often reforms are futile: institutions change and life goes on as before” (p. 52). This may occur because, as Przeworski observes, institutions may not map onto outcomes one to one. But it raises the question of why some institutional arrangements are respected while others are not.

One answer may lie in acknowledging that formal institutions exist beside informal ones. In his chapter (Chapter 5), Hans-Joachim Lauth discusses the nature of informal institutions and points out that the relationship between formal and informal institutions is particularly thorny when political change is involved. When change occurs—for example, in the case of regime transitions—it may consist of changes in the formal rules of the game, but also in informal

institutions. When informal institutions undermine new democracies, Lauth argues, it is relatively straightforward to observe. But it is much more difficult to assess their influence when they support formal democratic institutions. In this case, scholars may end up attributing greater causal impact to formal institutions than they deserve.

Second, while scholars often examine individual institutions, it is important to keep in mind that institutions exist in bundles. Depending upon the procedures governing institutional choice, existing institutions may play an important role in determining the origins of other institutions. In her chapter on transitional justice ([Chapter 27](#)), Nalepa explores this route, discussing how legislative and partisan institutions in new democracies influence the choice of transitional justice institutions.

Indeed, it may be the case that we cannot understand how any one institution functions without considering its place within the larger institutional setting. Beramendi and León ([Chapter 15](#)) argue that this is the case with federalism. The degree to which central and regional governments cooperate, for example, may depend on the degree to which their electoral fates are tied, making the interaction between federalism and party systems critical for determining the types of outcomes that are produced. Similarly, Ginsburg ([Chapter 8](#)) observes that in order to understand the endurance and change of constitutions, it is important to consider not only its content (e.g. amendment procedures), but also institutions of constitutional review. To the extent that courts and other institutions can interpret a constitution's meaning and application, they play a decisive role in determining that institution's effect on behavior and outcomes.

Not only do institutions influence the choice and operation of other institutions, but there is clearly more room to understand how institutions work together as substitutes or complements in addressing various strategic dilemmas facing political actors. Martin and Vanberg ([Chapter 13](#)), for example, suggest that there are many institutional ways in which parties within a government coalition can police their policy bargains. Similarly, in their chapter on authoritarian institutions ([Chapter 19](#)), Boulianne Lagacé and Gandhi discuss how autocrats use legislatures, parties, and courts to solve problems of commitment while they use some of these same institutions along with elections and decentralization to improve their efforts at information-gathering. These cases (along with others) raise several questions: Why do some countries use one type of institutional solution while others use different ones? Why do some governments address a problem with multiple institutional solutions while others use one? Are various institutions—that may work together or at cross-purposes in addressing a problem—chosen together intentionally? Most scholars study a particular substantive institution, imparting a rudimentary understanding of how bundles of institutions operate.

The challenges and opportunities of institutional analysis lie not only in our theories and explanations, but also in our empirical strategies. As is evident from several of the chapters in the Handbook, the bulk of scholars' empirical efforts have centered on assessing the effects of institutions. The focus on institutions-as-rules is due, in part, to the difficulty of explaining institutional origins and change, but perhaps in equal measure to the scarcity of comparative historical data.

Recent efforts to improve contemporary cross-national data have enabled scholars to focus more closely on the effects of institutions on a variety of outcomes. Wehner ([Chapter 20](#)), for example, notes that for many years the major impediment to comparative work on fiscal institutions was the paucity of cross-national data. But in the early twenty-first century, scholars have compiled detailed and consistent information on budget institutions in fifteen European Union countries as well as increasingly better data on these institutions in Latin America. Similarly, the study of institutions and voting behavior received a significant boost from the Comparative Study of Electoral Systems (CSES) project, which coordinates the operation of more than fifty national election studies across the world. McAllister ([Chapter 23](#)) notes that this data collection

effort insures that information about citizens' behavior and attitudes gathered in each country is comparable and can be matched with information about institutional arrangements.

With observational data, there also has been a move towards examining variation within countries, relying upon sub-national data. In their chapter on institutions and political corruption ([Chapter 28](#)), Miriam Golden and Paasha Mahdavi discuss many of the reasons for this trend. At the sub-national level, scholars may be able to collect fine-grained measures to more precisely test theoretical arguments. In addition, within a single country there may be more variation in the relevant variables (e.g. institutional structure) while still enabling scholars to better “control” for important contextual factors. In the study of political corruption, the fact that studies based on cross-national and sub-national data produce conflicting results is impetus enough for demanding more within-country analysis.

Observational data—whether at the national or sub-national level—carries its own challenges in assessing the effects of institutions. Because we know that actors purposively choose or change institutions due to the outcomes they generate, disentangling the effects of institutions and the conditions under which they exist is difficult (Przeworski 2004). It is also then likely that the universe of institutions that we observe does not contain all of the counterfactuals that we need to adequately assess causality (King and Zeng 2007). The end result is that the use of observational data creates a variety of inferential problems.

In response—following the trend more generally in political science—there is a small but growing literature that uses experimental methods to understand the effect of institutions. In their chapter on the use of field experiments to study institutions ([Chapter 7](#)), Guy Grossman and Laura Paler discuss the introduction of participatory institutions or the provision of information to improve the functioning of existing electoral mechanisms at the local level. The experiments enable scholars to empirically assess whether institutions influence individual-level behavior. The experimental design avoids the messiness of observational data, and the zeroing-in on microfoundations allows for close testing of particular theoretical linkages. But Grossman and Paler observe that, as with all things, there are trade-offs: field experiments may not allow us to get at questions associated with “big” institutions, such as regimes or executive–legislative relations.

Do institutions matter?

Careful theoretical and empirical work has furthered our understanding of institutions, but it also has led to continued debate on many substantive questions. What may have stood as conventional wisdom is often revisited, with more precise theorizing to account for more empirical patterns. This volume highlights a number of these debates, ranging from the sources of instability in presidential democracies to the class origins of economic and social policies. Some of these substantive debates also highlight that “institutions matter” may still be considered a question rather than a statement.

In his chapter on executive–legislative relations ([Chapter 12](#)), Sebastian Saiegh revisits the Linzian argument about presidentialism and democratic stability. Saiegh argues that there are good theoretical reasons to think that presidential regimes are no more dysfunctional than parliamentary ones, and he shows empirically that presidents form government coalitions and, more significantly, are successful at passing their policy agendas through the legislature at a frequency approaching that of parliamentary governments. His findings demonstrate that presidential regimes are not inherently prone to deadlock and instability. But the fact that presidential democracies die at a higher frequency than parliamentary ones still remains, leaving scholars with an open puzzle.

Similarly, Rubén Ruiz-Rufino ([Chapter 24](#)) discusses the complex relationship between ethnicity and elections. There is a large, still unsettled, debate over whether elections serve as a peaceful mechanism through which groups can process their conflicts or whether they serve to incite mobilization and/or conflict along ethnic lines. The answer depends, in part, on the way the election is structured. Certain types of electoral rules may encourage party formation or post-electoral behavior that reflects ethnic peace or conflict. And as scholars are learning more about how candidates and parties use fraud and manipulation, Ruiz-Rufino argues that we should expect these aspects of electoral behavior to affect whether elections quell or foment ethnic conflict.

Finally, in their chapter on labor market institutions ([Chapter 21](#)), Irene Menendez and David Rueda trace the evolution of institutionalist arguments designed to account for variation in macroeconomic outcomes such as unemployment and inflation. Scholars emphasized the role of corporatist and partisan institutions as a corrective to an earlier consensus on the role of labor market rigidities in influencing such outcomes. Yet concertation bargaining along class lines and left partisan control of government go only so far in accounting for historical patterns because the emphasis on these institutions assumes that workers and employers engage in zero-sum conflict while the interests of labor and Social Democratic parties are static and homogenous. Because cross-class alliances do sometimes emerge and workers are often divided between “insiders” and “outsiders,” Menendez and Rueda call for a focus on these dynamics as well as different institutions to explain labor market policies and outcomes.

The above examples are just some of the substantive areas, highlighted in the Handbook’s chapters, where scholarly debate continues and research frontiers remain wide open. More broad is the debate over whether institutions are even as important as we think they should be in accounting for various outcomes. There are some areas in which it is clear that the “institutional revolution” has made a measurable impact on our understanding of some political outcomes. Martin and Vanberg ([Chapter 13](#)), for example, observe that the study of coalition government used to be “institution-free” in that scholars focused on how non-institutional factors such as party ideology influenced coalition formation. Due to the turn towards institutions, we have learned that institutions, such as investiture requirements and the continuation rule, influence the government formation process, and we are just now beginning to understand how institutions affect governance under coalitions.

Similarly, Balcells ([Chapter 26](#)) argues that institutions are critical in understanding political violence for a number of reasons. Institutions themselves are often the target of political violence; they also shape the opportunities and capacities for using violence. The institutional means by which actors can advance their goals, for example, may influence the decision to use violence. Institutions also may be critical for the resolution of commitment problems among combatants, influencing the extent of inefficient conflict.

In reviewing institutionalist approaches, Margaret Levi and Victor Menaldo ([Chapter 2](#)) generally observe that “we have established that institutional variation explains a big share of the observed variation in economic growth, regime types, and public policies” (p. 16). This understanding of institutions has been hard won. As Levi and Menaldo explain, it has required not only the careful development of theoretical mechanisms and empirical strategies to assess them, but also the creativity and imagination to consider rare or non-existent institutional forms. Given the importance of institutions for explaining such big, important outcomes, it becomes all the more imperative to understand why most of the world still lives under “bad” institutions.

But there are other areas in which the importance of institutions is still unclear—both in theoretically disentangling their effects from non-institutional factors and in empirically uncovering substantively important effects. In examining party systems, for example, Negretto ([Chapter 9](#))

makes clear that electoral rules matter, but he also points out that so does social heterogeneity. While much of the literature on electoral and party systems treats cleavages as a priori fixed within a society, we know that social cleavages must become political cleavages in order to be relevant. But this happens only if political parties take action, and, being strategic actors, when parties try to activate cleavages they probably also consider electoral rules. As a result, the interaction between electoral rules and social heterogeneity in determining party systems is probably more complicated than currently acknowledged.

In examining political corruption, Golden and Mahdavi ([Chapter 28](#)) adopt a significantly more pessimistic view. They review the recent scholarship that has examined the effect of regimes, electoral rules, executive–legislative relations, the judiciary, and federalism on corruption, and conclude that “[I]nstitutionalist approaches are surprisingly ineffective in understanding corruption” (p. 416). One problem is that cross-national empirical studies have produced a tangle of conflicting results. The other issue is that any institutional effects are dwarfed by those generated by economic development. Poor countries—whatever their institutional makeup—tend to be more corrupt than wealthy ones. If institutions matter, they do so only at the margins. For other chapters in which the authors are more reticent in stating an opinion on whether institutions truly matter, readers will have to be the judge. But it should be clear from the work in this volume that there is still ample room for further research.

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Part I

Approaches to studying institutions

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2

THE NEW ECONOMIC INSTITUTIONALISM IN HISTORICAL PERSPECTIVE

*Margaret Levi and Victor Menaldo*¹

The new institutionalism arose in the 1980s, more than thirty years ago, in reaction to narrowly individualistic approaches in economics, political science, and sociology. Behavioralists and conventional neo-classical economics “interpreted collective political and economic behavior as the aggregate consequence of individual choice...[and] viewed institutions as epiphenomenal, merely the sum of individual level properties” (Powell and Dimaggio 1991: 2). The new institutionalists, building on a long tradition, claim that rules, norms, and customs—often arbitrary and artificial—structure human interaction, and they use institutionalist approaches to explain major macro-outcomes and long-term secular change.

The new *economic* institutionalists are methodological individualists who recognize the constraints and incentives encapsulated in institutional arrangements. They are both micro and macro. By combining the analytic tools of rational choice with serious consideration of institutional arrangements, both formal and informal, the new economic institutionalists build on microfoundations to reveal how institutions intervene between preferred choices and actions to meaningfully affect outcomes of interest to economists, political scientists, and sociologists. In contrast, the *historical* institutionalists, who came to prominence in the mid-1980s (Evans *et al.* 1985), have a determined emphasis on structural features, i.e. the type of economy or regime. At least until the 1990s, the historical institutionalists paid little attention to microfoundations (Thelen 1999). On the other hand, they did emphasize how institutions shape preferences and behavior (Steinmo *et al.* 1992), an insight embodied in the work of older economic institutionalists but initially lost (if subsequently found) by their successors.

Here we focus on the new economic institutionalism, but hasten to note that all variants of contemporary institutionalists have their roots in the older versions of institutionalism that date back to at least the seventeenth century. Consideration of how institutions influence individual behavior and collective action has a distinguished history. Major thinkers have long sought to address big questions with important policy implications; and each subsequent generation built (and builds) on the questions, tools, and insights bequeathed by the prior ones. While earlier institutionalists, comprising the authors of classical works in political economy, flirted with the origins of order, secularism, and markets, the new economic institutionalists fashioned a rigorous analytic approach to explain real world variation in the quality of institutions, state capacity, democracy, economic development, and the welfare state. The most recent economic institutionalists use state-of-the art models and methods to explain why underdevelopment remains pervasive—despite the

now widespread knowledge that institutions account for a large share of the variation in the outcomes that constitute development.

Earlier generations of institutionalists, including such luminaries as Thomas Hobbes, Adam Smith, and Karl Marx were not necessarily aware that they were making inroads into the study of institutions. They did not often use the language of rules and beliefs to describe and explain social life. What they did was stretch the limits of their imagination to conjure worlds that were not their own but *could be*. In the process, they came up with the first justifications and organizational blueprints for centralized order, liberalism, and capitalism. Because many of these institutions had not previously existed, or were vehemently opposed by reactionary thinkers and forces, their contributions were groundbreaking.

A later generation, which includes Douglass North, Barry Weingast, and Robert Bates, were self-consciously institutionalist. They imported the logic and implications of neo-classical economics while taking up the torch from the classical political economists, who had put politics at the center of their analyses of social life. In their models rules and beliefs, but also power, trust, and distributive politics, play a central role.

More recently, institutionalists try to make sense of the fact that government predation, repression, politicized legal systems, corruption, citizen-on-citizen predation, and inveterate mistrust persist (e.g. Besley and Persson 2011; Acemoglu and Robinson 2012); this despite the fact that their forbearers, the first economic institutionalists, adduced that strong and competent states, the rule of law, and democracy are the key to promoting welfare-enhancing policies and development.

Indeed, thanks to robust empirical evaluations of the theories expounded by the new economic institutionalists, we have established that institutional variation explains a big share of the observed variation in economic growth, regime types, and public policies. We now have compelling evidence concerning how the rule of law and concomitant institutions contribute to development: by reducing the transaction costs of impersonal exchange (Wallis and North 1986; Knack and Keefer 1995; Hall and Jones 1999), facilitating the generation of greater revenues (Levi 1988; North and Weingast 1989; Olson 1993; Kiser 1994; Barzel and Kiser 2002), and enabling democratization and pluralism (Clague *et al.* 1996; Weingast 1997; Acemoglu and Robinson 2006). And we can document how democracy endows states with greater fiscal and administrative capacity, if not legitimacy (Levi 1997; Schultz and Weingast 2003; Stasavage 2011), fosters the provision of public goods (Lake and Baum 2001; Bueno de Mesquita *et al.* 2003; Albertus and Menaldo forthcoming-b), and promotes greater global integration (Eichengreen and Leblang 2008).

Meanwhile, institutional variation among democracies matters for the scope and quality of governance. Proportional representation is associated with the provision of social insurance and greater economic equality, better environmental stewardship, and lower incarceration rates (Fredriksson and Millimet 2004; Iversen and Soskice 2006; Lijphart 2012). Majoritarianism deters corruption and excessive government debt (Persson and Tabellini 2005). Federalism and political decentralization may improve the provision of public services at the local level and promote more responsible spending (Weingast 1995; Rodden and Wibbels 2002; Wibbels 2005; Diaz-Cayeros 2006; Treisman 2007).

In arguing that institutions have a profound impact on the organizations, policies, and even the identities that constitute people's everyday reality, the new economic institutionalism forces us to grapple with an uncomfortable conclusion, however: Although "bad" institutions are potentially subject to change, they nonetheless continue to condemn a huge swath of the world's population to lives that are short, nasty, and brutish—through no fault of their own (Fukuyama 2011; Acemoglu and Robinson 2012). These institutions include unchecked autocratic authority

and weak bureaucracies that promote predatory behavior, corruption, and civil strife. They therefore breed insecurity and underdevelopment, if not the serial violation of human rights and tragedies such as famines (Sen 1981). Paradoxically, it is Hobbesian absolutism that has often replicated the state of nature depicted in the *Leviathan* across today's developing world, not transcended it.

This realization ushers in the critical puzzle that contemporary institutionalists are grappling with. If "good" institutions are the key to promoting the means by which humans achieve greater welfare and societies flourish, such as economic growth or egalitarianism, if not ends with intrinsic value, such as civil liberties, then why don't all societies simply adopt these institutions? A fledgling theoretical literature stands on the shoulders of giants in economics, economic history, political sociology, and comparative political science and confronts this paradox. Scholars have arrived at four explanations for why there is often a paucity of reforms that might bring about more optimal equilibria despite the now common knowledge that certain institutions should engender outcomes preferred by a greater swath of society: commitment problems; information asymmetries; collective action difficulties; and scope conditions.

This chapter continues as follows. We will first examine how earlier generations of institutionalists, the classical political economists, provided the scaffolding necessary to address the puzzle of underdevelopment. We will explore how the ancestors of the new economic institutionalism imagined new worlds where none had existed before, and outlined foundations for secular authority, liberalism, and capitalism, despite the fact that religiously justified rule, disorder, absolutism, and illiberalism were the only game in town. We will argue that they fashioned a more primitive, yet visionary, articulation of the question of where development comes from; in doing so, they bestowed on the next generation of institutionalists a fruitful research agenda. We then turn to the new economic institutionalists and explore how they marry the study of economics to the study of politics to generate clear, falsifiable predictions about where order, liberalism, and development come from, and how this equilibrium is sustained. We conclude with a question: How do we make sense of a world in which many of the practical problems first diagnosed by the classical political economists endure, even though knowledge of what institutions contribute to development has become widely available?

From Hobbes to North

Some of the earliest theorists of the state, Bodin (1530–1596) and Hobbes (1588–1679), deserve the label of the first institutionalists. Having lived through horrific periods of civil strife, they argued for the establishment of government institutions capable of creating constraints on human action through laws and the arrangements devised to uphold law-abidingness. Their primary goals were order and stability. However, the autocratic institutions they promoted, while certainly enabling economic growth by ensuring relative peace, had the conjoined effect of blocking innovation by stabilizing power in some hands and thus reducing competition, enhancing the repressive capacity of rulers, and denying voice to the vast majority of the population.

Given these normatively suspect outcomes, we can only speculate about why these "proto-institutionalists" proffered such a stark and draconian solution to the problem of anarchy and disorder. One hypothesis that comes to mind is that it was the only response to the stranglehold that religious apologists continued to have on the legitimation of monarchy in Western Europe well after Westphalia. The product of consummately secular assumptions that fed directly into deductions inspired by geometry instead of the Bible, the Hobbesian social contract and its attendant political system did not require a metaphysical justification. Such a heterodox approach to the study of political institutions did require, however, some respect for convention: arriving

at the modal normative conclusion, the superiority of absolute monarchy, albeit through different means. It was almost impossible to imagine any other prescription.

What were the limits of these theorists' imagination? Not only was it perhaps difficult to predict how the consequences of absolutism might ultimately play out in the real world, but Bodin and Hobbes were constrained by their historical and cultural context. In the face of a paucity of successful experiments in self-rule that could operate over large territories and prove sufficiently flexible and enduring, it might have been a step too far to hypostasize a social contract that put rulers and ruled on a more equal playing field. For example, the direct democracy practiced in Ancient Athens was an idiosyncratic reflection of its relatively small size and a relatively equal distribution of property, knowledge, and sophistication among its (male) citizens, a social structure that would only be recreated again, millennia later, among white colonists in the thirteen American colonies.

This suggests that one way to think about the progress that has been made by subsequent generations of institutionalists since Hobbes is succeeding theorists' ability to stretch beyond their predecessors' creative limitations. It fell upon them to imagine widespread liberty when none existed, to imagine a vibrant civic life when most of the population was still composed of illiterate peasants, to imagine how political institutions might be specifically crafted to produce such outcomes even though institutions had, hitherto, not been engineered to produce improvements in social welfare.

It is almost a century later when a new set of theorists prepared to do just that emerged. Locke (1632–1704), Montesquieu (1689–1755), and Rousseau (1712–1778) all emphasize an interaction between the governors and the governed. They, too, want to promote law and order, but they also seek means to ensure institutional change in order to break up ossified power relationships that fail to advance the interests of the general population. They did not put it in these modern terms, but these Enlightenment thinkers inferred that the absolutist solution to disorder only served to “raise the stakes” of politics and promote further disorder down the road. Its ultimate empirical implication, which would soon be attested to during both the American and French revolutions, is that there may be strong, perverse incentives for the marginalized to illegally contest power, lest a despot with few constraints on his authority and paranoid about threats to his rule act first and deprive the former of their “life, liberty, and property.”

Of course, although thinkers such as Locke and Montesquieu attempt to be scientific in their claims, their work on government and governance is largely normative. Nonetheless, their introduction of a new, more egalitarian basis for the social contract stretched the limits of imagination beyond the political status quo of their age: they sought to provide a way to think about how to impose systematic limits on executive authority to align the incentives of the rulers and the ruled. In doing so, these institutionalists introduced a way to reduce the stakes of politics while continuing to undertake a largely “scientific” and secular approach to the study of institutions. The idea of a more reciprocal social contract had arrived.

These ideas were far from academic. They were soon instantiated in the limited, secular, and deliberately engineered government of the first modern republic, the United States. This political experiment would ultimately prove that previously contentious and seemingly irreconcilable conflicts over political authority, property, and religion could be defused and contained; and over a huge territorial expanse, no less. This was achieved through the practice of constitutionalism, federalism, and individual rights.

As institutions evolved to support a more democratic polity, they did so in harness to the concurrent growth of capitalism. The result was theoretical emphasis on individual actors working in their own self-interest, but under the right institutional conditions able to produce a collective good. Bernard Mandeville (1670–1733) is among the first to both trumpet and raise

concerns about a highly individualized society. Best known for his *Fable of the Bees* (Mandeville and Kaye 1924 [1714]),² that pioneering text was an elaboration of his earlier popular poem, “The Grumbling Hive” (Mandeville 1705). Mandeville’s private vices became transformed into public virtues only if selfishness reigned and corruption was tolerated; virtuous individuals, from his perspective, created a far smaller economy and duller society.³

But it is also noteworthy that during the long seventeenth century, fairly sophisticated notions of self-interest were promulgated; notions that accord well with the institutionalist approach to political economy that now predominates. When their life is on the line, it is hard for individuals to exercise moral restraints that allow them to behave as the better angels of their nature. Thinkers both then and now understand that this does not imply that individuals are, by nature, ruthlessly egotistical. Rather, in some contexts, the price of morality, the ability to give others the benefit of the doubt, and to trust them fully, is prohibitive. People can be individualistic and selfish, yes; and under some circumstances narrowly focused on economic well-being. But, even those most closely associated with the concept never fully believed it. Hobbes argued that people prefer to act according to the golden rule, although most of the time their circumstances prevent it. Absent the centralized provision of the rule of law, in a world of theft and predation, people must resort to defensive selfishness.

It is during the Scottish Enlightenment that David Hume and Adam Smith well and truly establish the basis for contemporary institutional theorizing in which the combination of self-interest within rules is linked to producing economic well-being. Indeed, they went farther than that, arguing that appropriate institutions actually bring the best out of people living in a society. For Mandeville, the key is the transformation of private vice into public virtue. For Smith, it is private interest embodied in good husbandry governed by protection of property rights that produces public virtue. The combination of the division of labor, the invisible hand, and—lest it be forgot—appropriate laws enables individuals with wide-ranging capacities to generate economic growth for the country in which they reside. Recall that Smith authored not only *The Wealth of Nations* (1993 [1776]) but also *The Theory of Moral Sentiments* (1982 [1759]). In his second opus the emphasis is on human behavior and norms; his complex theory goes beyond the narrow characterization of self-interest for which he is currently caricatured.

Smith did have an implicit theory of politics, of course; one that is taken up by the classical liberals and more contemporary conservatives who perceive government regulation as a threat to efficient markets and to various forms of personal freedom. Smith’s contemporary David Hume brought political institutions even more directly into his theorizing. He harked back to the pre-Hobbesian concern with historically informed classical statecraft, and he shared with Locke some skepticism about the nature of the capacity of the poor and humble to withdraw their consent (Ryan 2012: 418, 486). Hume, however, unlike Smith, recognized and assumed economic scarcity and the conflicts of interest it creates (Commons 1934: 6, 71, 74). Equally important, he had a developed set of arguments concerning the incentives and psychology of the individuals whose choices, when aggregated, produced macrobehavior. He brought to bear his variant of empiricism, which incorporates passions, ideas, and imagination, as well as interests, into a model that allows us to better understand the complexities of our world. That allowed Hume to almost envision modern democracy and the tensions it would face in the increasingly complex economy that accompanied the Industrial Revolution.

Neo-classical economics built on *homo economicus*, the division of labor, and the mechanisms for aggregation of interests, ideas inherited from Smith, Hume, and their successors. More recently, some new economic institutionalists and all behavioral economists have come to recognize the importance of passions and beliefs, long neglected in conventional economics,⁴ albeit present in their bloodlines. What these late eighteenth and early nineteenth century political

economists neglected or under-theorized were the vast variations among what states do and how well they serve their publics. Making sense of this requires models of power, legitimacy, and institutional change.⁵

Weber more directly considers power, legitimacy, and sources of change. He takes seriously the rights and treatment of the governed, the constraints on the governed by means of law and popular support, and, if not institutional change per se, at least the comparative statics that help account for why change might (or might not) occur. Perhaps Weber was the first to alert us to the importance of institutional complementarities, an issue taken up today in the “Varieties of Capitalism” literature (see, e.g., Hall and Soskice 2001; Hall and Gingerich 2009), as well as in other approaches (e.g. Acemoglu *et al.* 2001, 2002; Fukuyama 2011; Acemoglu and Robinson 2012). Ancient Rome and feudal China had large bureaucratic structures, but not the economic institutions to support a modern state. Weber was sensitive to the nexus between political and economic institutions.

Huntington (1968) took Weber’s analysis of political authority to its logical conclusion. He speculated about disparaged bases of legitimate and stable political authority in countries experiencing the growing pains of modernization. He zoomed in on the transition from traditional sources of legitimate political authority to legal-rational authority rooted in impersonal rules. Whereas Weber largely neglected to foresee and therefore address how turbulent and discomfiting the social and political changes unleashed by economic development would be, Huntington deftly identified the blind spots that characterized modernization theories such as Weber’s, and later Lipset’s and Rustow’s.

Huntington did not believe that the diffusion of new organizational, communications, and transportation technologies, alongside the steady march of industrialization, presaged a smooth, let alone teleological, progression of social and political liberalization. Instead, these changes might very well lead to chaos and disorder; that is, if they were not interdicted with new, deliberately constructed, ways of imposing social order and organizing politics that could mimic the basis of traditional authority forsaken by Weber: cooptation and patronage. Huntington’s preferred solution to the rampant authority vacuums, anomie, and resistance unleashed by modernization was the cultivation of inclusive political parties that could fulfill the neglected functions of social control on the bumpy road to legal-rational legitimacy. In this way, Huntington brought institutions back in to the study of modernization and social change.⁶

Huntington’s 1968 book is located near the end of a long chain of scholars identified now with the “old institutionalism.” In the United States,⁷ Thorsten Veblen (1912 [1953], 1914) and others initiated a variant of institutional theory in the early part of the twentieth century. Drawing on the “instinct habit” psychology of William James, the evolutionary theory of Darwin, and his own concern with social position, Veblen rejected fully rational decision-making and brought to bear a nuanced view of uneven influence and power (Hodgson 2007). John Commons (1924, 1934) soon identified himself with this group, and it is Commons—far more than Veblen—to whom several leaders in the new economic institutionalism, including both Douglass North and Oliver Williamson, claim a lineage.

Commons, however, has thus far proved more important for his emphasis on institutions than for his actual theories and models. His books are mostly out of print, and his name is all but forgotten by today’s students, unless they have an interest in labor economics (Commons 1926), where his work still breathes. Even so, he belongs in this list of those who exercised a major influence on contemporary economic institutionalism—albeit less for his writings than his example and preoccupations. He brought to institutionalism a life rich in practical economic experiences combined with a deep knowledge of political economic theory.⁸ In his perspective, institutions shape individual preferences and beliefs, but their ultimate source is in transactions,

which generate conflict of interests. Institutions, that is, laws, rules, and regulations, are the means by which to solve social order problems by embodying collective action. He was fundamentally concerned with the non-contractual basis of contracts:

I make conflict of interests predominant in transactions. But I conclude that this cannot be allowed to be the only principle, because there are also mutual dependence and the maintenance of order by collective action. I start, like economists, with scarcity, as universal for all economic theory. Then I proceed, as did Hume and Malthus, to show that out of scarcity derives not only conflict, but also the collective action that sets up order on account of mutual dependence.

(Commons 1934: 6)

While predicting a revival of interest in Commons (a revival yet, if ever, to come), Kenneth Boulding summarized nicely the problem with this body of work: “Commons’ theoretical structure remains today exactly where he left it: a tangled jungle of profound insights, culled by an essentially nontheoretical mind from a life rich with experience of economic realities” (Boulding 1957: 8). Commons’ insights on transaction costs were refined and made more theoretical only a few years later by Ronald Coase (1937). One of his most notable insights was that institutions provide the basis of legitimacy for competition and for innovations that threaten the existing order. But it lay with Joseph Schumpeter (1942) to develop this critical insight into the full-blown theory of “creative destruction.”

Even so, rereading Commons today—despite the literal dustiness of the pages and the denseness of the prose—alerts us to questions and issues that still require research and thinking if we are to understand the prevalence and persistence of so many “bad” institutions: how institutions provide legitimacy to various practices, how institutions shape preferences and beliefs, the steps needed to build the rule of law, the role of custom, and the fragility of social order.

More sociological authors, such as Philip Selznick (1957, 1966), Seymour Martin Lipset (Lipset 1950; Lipset *et al.* 1956), Reinhard Bendix (1978), and Edward Banfield (1958), shared with Commons a focus on how institutional arrangements solve certain problems of public goods and social order, while creating others. But their way of approaching these questions, also critical to the “new” institutionalists, was distinctly different than the methods used by the first generation of rational choice and sociological/organizational institutionalists, as Stinchcombe nicely summarizes:

Institutions...shaped the creation and functions of units in markets and the relations between them. But unlike the institutions of modern institutionalism, people ran these institutions by organizing activities on their behalf. Institutions were, in the first instance, created by purposive people in legislatures and international unions, and in pamphlets of business ideologists in Northern England. Modern institutionalism, to create a caricature, is Durkheimian in the sense that collective representations manufacture themselves by opaque processes, are implemented by diffusion, are exterior and constraining without exterior people doing the creation or the constraining.

(Stinchcombe 1997: 2)

Government, especially laws, regulations, and organizations, played an important role in these accounts, but what was lacking was both a theory of the state and an explicit account of how institutions create preferences and shape behavior. Historical institutionalism⁹ arose as a corrective to the “old institutionalists,” the behavioralists who ignored institutions altogether, and to the

economic and rational choice institutionalism which had arisen in the early 1980s with the publication of Douglass North's *Structure and Change in Economic History* (1981).

Building not only on Smith (and Coase), but also on Marx, in his effort to understand long-term secular change, North looks for the underlying matrix of incentives, constraints, and risks that structure individual choice. His emphasis on transaction costs, property rights, and commitment problems was pioneering in the sense that he was able to meld insights from neo-classical economics with the primary concern of political scientists and sociologists: how power, at least bargaining power,¹⁰ matters for the construction, governance, and legitimacy of both political and economic markets. North argues that the self-interest of rational rulers bent on holding on to power and maximizing their own stream of revenues could usher in, under the right conditions, more optimal and stable equilibria. This could be accomplished through the self-enforcing provision of private property rights and public goods with similar transaction cost reducing properties: the standardization of weights and measures, time, and, most importantly, civil law and contracts. However, such rules and the institutions they created to sustain their wealth and power could also, over time, undermine the general welfare and prove destabilizing. As North noted (1981: 20), "The existence of the state is essential for economic growth; the state, however, is the source of man-made economic decline."

In this sense, institutions mattered and mattered absolutely. While the rules of the game and the attendant expectations about the future were themselves the byproduct of political strategies adopted by rulers who aimed to consolidate authority and maximize rents, their independent effects were pronounced and far reaching. If rulers were constrained to provide property rights to a relatively large number of citizens and similar transaction cost reducing public goods to remain in power, the result was usually representative government. Only this could adequately guarantee a self-enforcing commitment to these Pareto improving policies. The ultimate economic upshot would be economic development, because citizens would be incentivized to work, save, and invest in productivity enhancing endeavors instead of theft or rent-seeking. The ultimate political upshot would be a strong state flush with tax receipts and access to cheap and consistent credit. If rulers could somehow fulfill these objectives without the support of a broad coalition that included the merchant class, and without the consent of the taxed, then they would fail to provide property rights or other transaction reducing public goods. The logical result would be underdevelopment—due to predation of the ruler against the ruled and the ruled against each other, or at least the fear of predation—and weak states bereft of consent and revenues (see also Levi 1981, 1988, 2006).

While his emphasis on transaction costs, property rights, and rents is consistent with neo-classical arguments, his concern with how to acquire and process information and norms aligns him with those pushing the boundaries of what standard models can achieve. Institutions create the rules of the game for North and organizations facilitate collective action, but equally important are the cognitive limitations that configure the choices and beliefs of individuals and their reference groups. In his later work North attempted to better theorize about the role of ideology through the conception of "mental models" (Denzau and North 1994) and the incorporation of how cognitive limits impinge upon and shape rational decision-making (North 2005).

The new economic institutionalism that North spawned encouraged a generation of scholars to develop new theories of and empirical approaches for studying democracy, state building, economic growth, and solutions to a whole range of collective action and commitment problems. Barry Weingast and Kenneth Shepsle, initially influenced more by William Riker than North, brought to institutionalism the insights of game theory, deriving important and testable propositions about institutional equilibria and disequilibria in Congress and other voting bodies (Shepsle and Weingast 1981, 1987). Weingast, on his own, and in collaboration with North and

others, also used game theory to consider the relationship between rule of law and democracy (Weingast 1997), the establishment of credible commitments that make cooperative relationships possible (North and Weingast 1989; Milgrom *et al.* 1990; Greif *et al.* 1994), and most recently how beliefs are established and affect collective behavior (Rakove *et al.* 2000; Weingast 2005).

Robert Bates (1981, 1983) was one of the first researchers to bring the modern study of institutions to the developing world and to try to make sense of why seemingly irrational and pathological policies were so ubiquitous there. Taking North's assumptions about rational and self-interested rulers to heart, Bates speculated that in the post-colonial nation states of Sub-Saharan Africa, in particular, rulers responded to perverse incentives inherited from their colonial masters to retain power by pandering to nascent urban constituencies that could provide them with political support and rents. This called on adopting industrialization policies that ran against these countries' comparative advantage and were subsidized by indirect taxes on the countryside imposed via marketing boards that allowed the state to monopolize the pricing and distribution of cash crops. This allowed weak states such as Kenya and Ghana to simultaneously erect protectionist barriers for infant industries rooted in overvalued exchange rates, raise revenues, and ration credit and agricultural inputs via rural clientelism.

Besides the consolidation of power by rulers among a narrow, urban proletariat and a mass of unorganized farmers, the upshot was the distortion of behavior in the marketplace. Farmers eventually stopped producing enough food, nascent manufacturers allocated dear capital to wasteful investments and supplied low quality goods at exorbitant prices, and urban labor grew reliant on subsidized food. This pathological equilibrium was built on an erstwhile Russian and Prussian trick: squeezing the peasantry. Like those examples before it, it ultimately proved unsustainable: balance of payment problems and attendant macroeconomic and political crises undermined the revenues and political coalitions at the core of this strategy. Haber *et al.* (2003) would later identify a similar vein of crony capitalism in Mexico and illustrate how mechanisms of credible commitment to property rights first identified by North worked to offer selective protection to a few oligarchs at the expense of entrepreneurs and the majority of the population, with similar results to those exposed by Bates.

Bates helps bring the new economic institutionalism into political science and in the process takes up a preoccupation of political science that economics largely neglects: a conceptualization of power and the recognition of its importance for the analysis of institutional arrangements. In her work on revenue production and on conscription, Levi (1988, 1990, 1997) not only makes key to the analysis the influence of those with resources on which rulers and government actors are dependent, she broadens the notion of resources to include the withdrawal of compliance and consent. Even the seemingly powerless may indeed possess "weapons of the weak" (Scott 1985).

It is Jack Knight (1992), however, who brings issues of conflict, distribution, and ultimately power onto center stage. Knight applies his model to the transformation of norms of bride-wealth, common property, and clan exogamy (Ensminger and Knight 1997). As the forms of power change, so do the norms.¹¹

Meanwhile, it is Elinor Ostrom who develops and applies models of institutions that solve a variety of common pool resource problems. Her work determinedly emphasizes the cognitive limits of individuals and the influence of norms and contexts on their decision-making, monitoring, and collective actions (Ostrom 1990, 1998, 1999, 2005).

Other scholars also theorize the sources and sustenance of norms. Less concerned about the role of power than are Bates, Levi, or Knight, what they emphasize are the informal institutionalization of relationships that solve difficult problems, such as in trade (Greif 1994, 2006), and interethnic cooperation (Fearon and Laitin 1996). Relying on game theory, Greif offers an alternative to North's definition of institutions. For Greif, "An institution is a system of rules, beliefs,

norms, and organizations that together generate a regularity of (social) behavior” (2006: 30). The inclusion of both organizations and beliefs has significant implications for how to model institutional change (Greif and Laitin 2004).

Where North and Greif converge, however, is on the importance of beliefs in understanding not only why institutions arise but also why problematic institutions are retained. Marx, Schumpeter, and most political economists who followed recognize how powerful actors keep in place institutions in their own interest that are not necessarily in the general interest. But it is not solely a story of power, at least not power in the form of observable material interest. It is also a story of what the population believes is in their interest or what they believe it is possible to do. A snowballing of protest can be one precipitant of the recognition that others share one’s perspective or that it is safe to act (Granovetter 1978; Kuran 1989, 1995). Often it requires information and leadership to change what people think the world is like and what they can do about it (Ahlquist and Levi 2013).

Acemoglu and Robinson are, in many ways, the culmination of the different ideas introduced by the old economic institutionalists and formalized and honed by the new ones. They situate their work at the intersection of the primacy of power and how formal institutions shape individual behavior through the provision of economic policies that incentivize either consistent productivity and egalitarianism or inefficiency and oligarchy. Across very influential articles and books (e.g. Acemoglu *et al.* 2001, 2002; Acemoglu and Robinson 2006, 2012), they have outlined dynamic theories of institutions in which democratization or the rule of law institutions that sustain property rights are introduced or undermined by changes in the balance of power in society. In their view, all institutions are next-best solutions to commitment problems. They reflect the preferences over economic policy of those who wield power; but they would not exist if those with *de facto* power could somehow perpetuate their threat of coercion into the indefinite future. Indeed, *de jure* institutions are selected because *de facto* political power is transitory. Different groups in society have a fundamental conflict of interest over the distribution of social gains and those with a temporary edge impose the rules of the game upon society to guarantee a favorable distribution of resources into the future, even after their raw power fades.

Not all commitment problems have institutional solutions, however. The “best” institutions theoretically presage increases in absolute wealth across social classes. Unfortunately, these cannot always be crafted to simultaneously guarantee a favorable economic distribution for those who wield power and the most Pareto Optimal equilibrium that grows the biggest possible pie. This is a difficult hat trick and historically rare. This is why, and, perhaps in an effort to chastise economists who believe examples of systematic inefficiency bespeak irrationality, Acemoglu and Robinson are wont to remind us that it is foolhardy to think about politics through the prism of the Coase Theorem. Relative gains matter, and gains from trade may therefore be forsaken. Increased welfare can be used to bolster *de facto* power of those who are shut out of formal office. Development threatens the institutional equilibrium that benefits whoever controls economic policy. The losers from institutional reform may cling to suboptimal institutions if they fear that a transition to an *ostensibly* Pareto superior outcome will only serve to threaten their political power and economic rents (Sutter 1995; Acemoglu *et al.* 2005; Albertus and Menaldo forthcoming-b).

The problem is about time-inconsistency. New winners from institutional change have incentives to renege on any agreement to compensate the losers after the reforms have come to pass. If these potential losers hold power today, they will block said reforms because guarantees of side payments or other inducements are not credible tomorrow. “Rather, the natural tendency is for the gainers to husband their winnings and to stop their ears to the cries of the afflicted” (Rogowski 1989: 17). This insight helps explain the puzzle that free trade is relatively rare

historically, and an arena of pitched battles between social classes and sectors, despite the fact that it is Pareto improving in the textbook sense: gains from trade at the macro-level are axiomatic if nations specialize along the lines of comparative advantage (Rogowski 1989). This can also explain why many long-lived dictators run their economy into the ground and otherwise forgo policies that may strengthen their opponents. It may even explain why they choose to fight to the death rather than relinquish political power (Albertus and Menaldo forthcoming-a).

Enduring underdevelopment is therefore explained by the fact that powerful actors often opt, strategically, for the largest possible slice of pie from a smaller overall pie instead of a smaller slice from a larger pie. Yet, under moderate levels of inequality, a temporary burst in *de facto* power by a coalition of the middle classes and the poor can introduce democracy. To avoid revolution, the rich will step down and allow democracy to become a credible commitment to redistribution; and, yet, this should not pose an existential threat to capitalists after transition. The result is liberal democracy coupled with secure property rights and public investments that usher in enduring economic growth.

However, North, Wallis, and Weingast (2009) have pointed out that, even if credible commitments to protect the privileges and rents earned by elites who block democracy and progress can be extended, this is not sufficient for ensuring that a transition to a more liberal order will occur. The fabric of developing societies—what the authors call “natural states”—is supremely delicate, and liberalizing reforms that aim to shrink hard-earned privileges and rents cannot merely be compensated for, on an ad hoc basis, with side payments or other compensatory schemes. Institutions in much of the developing world have been molded and fine-tuned over long periods of time to deter powerful actors from deploying violence to upset the political order or enlarge their piece of the pie. Indeed, the state or other political actors are wont to dispense rents in proportion to the *de facto* power that each group possesses. Therefore, rents are often “the glue that holds society together” in underdeveloped countries in which power and concomitantly the potential for violence are widely distributed, and threatening to withdraw them through neoliberal reforms may unleash a violent backlash.

North, Wallis, and Weingast remind us that the groups’ potential for violence interact in complex and nonlinear ways, and the uncertainty about how each group might respond to reforms that threaten their rents often crows states into the most rational strategy; that is, a risk averse conformity to the status quo based on the realization that the opportunity costs of reform in developing societies are steep. Every reform that is attempted is a forgone opportunity to deploy rents to foster peace, itself a still quite scarce and highly valued commodity throughout the world.

Next steps

There are other sources of underdevelopment still. Even if the losers from reform can actually believe promises that their welfare will not be reduced in the wake of changes to the rules of the game, the competence or integrity of the policymakers in charge of institutional reform may be questioned. In turn, imperfect information of this sort may create perverse incentives by policymakers to signal reliability through suboptimal behaviors (Besley 2006; Acemoglu *et al.* 2010; Albertus and Menaldo 2012). These may weaken the foundation of the rule of law or democracy, offsetting the benefits of institutional reform. How citizens and other stakeholders curtail these information deficiencies is key to understanding when and how “better” institutions can be selected and whether new technologies such as social media can help move a critical mass of actors closer to the Pareto frontier: compel them to adopt and abide by more efficient, fairer, and more stable institutions.

Yet the obstacles to this type of institutional engineering should not be underestimated. Collective action on the large scales implied by modern nation states is very difficult. Once you have stable coordination around one focal point, coordination on a superior equilibrium is a victim of the tyranny of self-fulfilling prophecies. Switching to a Pareto improving scenario calls for finding a way to convince individuals stuck in their ways that a critical mass of others are going to change their behavior (Laitin 1992; Mackie 1996; Ahlquist and Levi 2013). Sometimes, what is needed is not Twitter, perhaps, but an old-fashioned, and perhaps traumatic, exogenous shock (Acemoglu and Robinson 2006; Miller 2012). What those shocks might look like in a future with fewer overtly repressive regimes and with an accelerated news cycle, and why exactly they might induce changes in beliefs and the motivation to coordinate different strategies with collectively beneficial consequences, are open questions.

Scope conditions may also mitigate the effectiveness of putatively good institutions. The local context in which formal institutions are implemented matters (Ostrom 1990). So does the interaction of these formal institutions and informal institutions. Indeed, informal institutions at the local level may be more critical to local governance and development than national-level policies (Tsai 2007). This means that veritable changes in policy and human welfare may lie at the sub-national level and be a product of changes in informal, rather than formal, institutions. This is a door that has been pushed wide open by entrepreneurial researchers, who are increasingly using big datasets and innovative survey research to ask seemingly smaller questions at smaller and smaller scopes.

Conclusion

This brief review of the history of the “new” economic institutionalism reveals enduring questions but some methodological advance in the efforts of social scientists to facilitate “good” institutions and eliminate the “bad.” The review also reveals the importance of having a theory not only of the institutions themselves but of the human actors who build and are shaped by those institutions. The move into behavioral economics is just the most recent in a long collaboration of political economists with psychologists, sociologists, and political theorists who reveal how decision-making is not fully “rational”; the ways power matters; and the sources of legitimacy. This collaboration was lost to conventional economics for nearly a century as neo-classical economics came to dominate the field. With the rise of the “new” economic institutionalists, we are once again bringing interdisciplinary knowledge to bear on the puzzle of how to improve the quality of our societies and of the lives lived in them.

Notes

- 1 We wish to thank the editors of this volume as well as Ira Katznelson for their helpful comments on an earlier version.
- 2 Steven N. S. Cheung brings the “fable of the bees” into contemporary transaction cost economics with his discussion of the externalities of beekeeping Cheung (1973).
- 3 See Alan Ryan (2012) for how Mandeville was reacting to what he perceived as the problematic conception of republican virtue.
- 4 But not, of course, in all economics. See, e.g., Hirschman (1977).
- 5 In addition, there are issues of the relationship between government and citizen obligations regarding welfare, taxation, and the like. Considerations of public goods exist in Smith but are far too limited.
- 6 Migdal (1988) would later explore how similar reconstitutions of political authority at the local level would allow states to consolidate their hold on power after colonialism in ways unforeseen by modernization theorists—by working through intermediaries, so-called strongmen such as Afghani warlords and Senegalese chiefs. These local brokers retained a modicum of traditional authority, whether legitimate

- or coercive, and exercised power in strong societies able to adapt to the destructive changes wrought by capitalism and globalization.
- 7 There is also a European counterpart that included Sombart in Germany and the Webbs in Britain (Hodgson 2003: 548).
 - 8 For interesting discussions of Commons' contributions, see Boulding (1957), Hodgson (2003), and Stinchcombe (1997).
 - 9 This approach begins with the seminal work of the Social Science Research Council (SSCR) Committee on States and Social Structure, culminating in *Bringing the State Back In* by Evans, Rueschemeyer, and Skocpol (1985). This line of research blossomed, marked by the publication of *Structuring Politics*, edited by Steinmo, Thelen, and Longstreth (1992), *Politics in Time* by Pierson (2004), and the edited volumes by Mahoney and Rueschemeyer (2003) and by Streeck and Thelen (2005). Debates and arguments raged about the relative virtues of the competing historical and economic variants of institutionalism. The publication of *Varieties of Capitalism* (Hall and Soskice 2001), however, is one of many indicators of the increasing merger of the two approaches.
 - 10 See Moe (2005) for a critique of this narrow version of power that cannot adequately account for the role of or consequences for those without any significant bargaining clout.
 - 11 Power is also key to the variant of new economic institutionalism adopted by international relations scholars, those largely identified as neo-realists (Fearon 1998; Keohane 1986). However, given that the questions we are raising lie more in the domain of comparative politics than in international relations, we leave this important body of research to others to discuss.

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3

STUDYING INSTITUTIONS

Tom Clark and Jennifer Gandhi

Introduction

Virtually everything we study in political science involves thinking about how individual preferences and/or actions reconcile with those of the group in which the individual is situated. Peace is not possible without both the government and the rebel group laying down their arms. A legislator needs the votes of other colleagues in order for his bill to pass. The budget cannot be passed without the support of the executive and legislature. A coup d'état cannot successfully displace the government unless various factions of the military coordinate their actions. As Pierson (2000: 258) observes: "In politics, the consequences of my actions are highly dependent upon the actions of others. What I get depends not just on what I do, but (mostly) on what others do."

In this chapter, we begin by discussing the ways in which this dependence takes different forms. We provide a variety of examples from across the subfields that, in turn, highlight the fact that the salient problems confronting political actors in different substantive contexts are generic in nature. We then discuss how institutions play a role in these strategic dilemmas by either creating or addressing them (intentionally or not) and clarify the implications of thinking about institutions in this way. Understanding how institutions function leads us to consider their origins. We highlight three particular challenges that emerge in trying to understand institutional origins and that represent some of the areas in which our knowledge of institutions is thin. We conclude by emphasizing the generality of the issues faced in trying to understand the functioning and emergence of institutions, providing good reasons for scholars across the subfields to engage more with each other if they want to have a better understanding of institutions.

The problem of collective decision-making

One thing political actors—whether they are members of a legislature, a court, a junta, or a party—must do is make collective decisions. However, collective choice is wrought with many problems that can lead to undesirable outcomes. For example, Arrow's Theorem teaches us that there exists no preference aggregation rule—such as simple majority—that simultaneously satisfies a small number of fundamental desiderata, except dictatorship. At the same time, the various chaos theora, especially those from McKelvey (1976) and Schofield (1977), show that collective

choice in a multi-dimensional space suffers from perverse problems—especially cycling and intransitivity—even when actors' preferences are individually rational. Scholars have pointed to political institutions as a way of understanding solutions to those troubling results.

Implicit in much of this analysis is that cycling is normatively undesirable. It results in policy instability, leading to inefficiencies in governing and unpredictability for citizens conditioning their behavior on government actions. In such situations, institutions—such as parties or legislative committees—can enable actors to escape this trap and arrive at stable collective decisions.

Another way of arriving at this point is to assume directly that political actors prefer most of all to cooperate (rather than not cooperate) with others. In the classic coordination problem, the critical difficulty lies in finding a focal point. The illustrative example that captures this dynamic is that of Bach vs. Stravinsky: I like Bach more than Stravinsky; you like Stravinsky more than Bach; but we both prefer to go to either concert together. Hardin (1989) argues that individuals within a society considering the organization of a polity may find themselves in a similar situation. They may have preferences for different kinds of constitutive rules, but they most prefer to come to some agreement on *any* set of rules. Constitutions provide these rules and serve as a focal point that enables us to avoid living in chaos or in a world in which foundational rules are constantly amended. Settling on some set of constitutive rules enables us to make other decisions and laws that lead to more efficient governance (Holmes 1995).

When actors are confronted with the task of preference aggregation or coordinating on outcomes, institutions can help them settle on policies or outcomes, which presumably is better than living with policy instability or chaos. But the efficiency-enhancing properties of institutions do not absolve them of their distributive implications (Knight 1992). Long coalitions in the form of political parties may reduce transaction costs in passing legislation, but they also enable majority parties to act as gate-keepers. Legislative committees organized by policy jurisdiction may help reduce cycling but also bestow agenda-setting power on select members of the assembly. Gate-keeping and agenda control exerted by select actors within a group often result in policy outcomes that deviate significantly from the median group member's preference (e.g. Romer and Rosenthal 1978). In short, there generally exists a frontier of institutional efficiency. There are usually no perfect solutions to collective dilemmas, only different ways of balancing alternative tensions. What institutions do in this situation is create a mechanism to trade off one inefficiency against another. If one worries about policy instability and undue transactions costs, there exist institutional solutions to alleviate those problems, but they may entail accepting potentially undesirable outcomes, such as non-median policies.

The problem of interdependent action

Political actors do more than make collective decisions; they also take actions on behalf of or in conjunction with others. A variety of strategic dilemmas fit this characterization—collective action, delegation, commitment—with their canonical structures having been imported from economics. But as the substantive examples illustrate, the political contexts that we study usually require us to combine and complicate these frameworks.

The prisoners' dilemma, the collective action problem, and the common pool resource problem share a specific feature: they are situations in which individual rationality is not sufficient for collective rationality. A cooperative outcome could make all participants better off, but individual incentives lead them to an inferior position (Sandler 1992).

The problem of enforcing the appropriate limits on state power provides such a dilemma for citizens (Weingast 1997). The state has the power to transgress against the rights of some citizens, and citizens must decide whether to acquiesce or challenge such transgressions. The twist

is that the state can expropriate from some individuals and give a portion of those assets to others. The result is that there are non-targeted citizens who support the state in violating the rights of others even if citizens collectively would be better off if they were to resist all state transgressions. Because individuals have incentives to deviate from any collective challenge of such violations, the state cannot commit to not carrying them out.

Political parties in a coalition government face a problem with a similar structure, but in a very different context. Budget-making under coalition governments is a common pool resource problem. The coalition would be better off if it could adhere to limits on the aggregate budget, but each party, via its control over ministries, has an incentive to overspend for its own projects since it can claim credit with its own constituency for such spending and blame the other parties for “overspending.” Indeed, coalition governments typically engage in higher fiscal expenditures than single-party governments, and the amount of spending increases as more parties join the coalition (Bawn and Rosenbluth 2006). Some types of fiscal institutions may resolve this problem. When the formulation of the budget is centralized within the finance ministry or if floor amendments on budget bills are prohibited, for example, parties may not be able to “free ride.” In the presence of such restrictive budgetary institutions, government expenditures do not increase with each additional party in the coalition (Martin and Vanberg 2013).

Introducing the element of time creates its own set of problems for political actors. Their preferences may be time inconsistent because what might maximize their utility in an individual period may not be equivalent to what would maximize it if the whole time path were considered (Shepsle 1991). When there is a tension between consistency and optimality, two key issues emerge: first, verifiability—the one to whom the commitment is made must be able to verify the other actor’s upholding of the commitment; second, enforcement—there must be some mechanism by which the actor who made the commitment can be punished if he/she reneges.

The end of a civil or interstate conflict is often marked by belligerents signing a peace agreement that may entail promises to disarm, to cede territory, or to share power—all actions that must be taken in the future. The problem is that a bargain that is satisfactory for the parties now may not be in the future due to changes in the balance of power among actors (Fearon 1998) or the emergence of spoilers (Heger and Jung 2014). This time inconsistency incentivizes actors to renegotiate or renege on the agreement, making peace unlikely in the future and at the current time since parties may be unwilling to settle if these issues remain unresolved. One potential solution comes in the form of third party mediators that can verify whether actors are following through on the agreement and exact costly consequences for cheating (Walter 2002). By increasing the costs of renewed conflict, mediators may increase the likelihood of peace, but their effects in the long term are less clear (Beardsley 2008).

Similarly, in developing democracies, vote brokers may purchase votes for their candidates and parties. Before the election, they offer cash and in-kind benefits in exchange for a promise from the voter that she will support the candidate or party that provided these goods at the polls. Since the advent of the secret ballot, however, it can be difficult to insure that the voter actually honors his or her promise on election day. Brokers may target communities with strong social ties in order to verify individual voter behavior and use social sanctioning or the withholding of benefits as punishment (Stokes 2005; Magaloni 2006). The voters’ commitment problem, however, appears in tandem with a collective action problem. As Rueda (2013) observes, voters who want to receive bribes but also to vote for their preferred candidates have incentives to free ride on other voters’ compliance. Such free-riding is easier to get away with the larger the number of voters casting ballots at the same polling station. This may account for the inverse relationship between levels of aggregation of electoral results and vote buying (Chandra 2004; Schaffer and Schedler 2007; Birch 2011).

For actors seeking to make their commitments more credible, delegation is another solution. Rather than manipulate the consequences of actions to discourage renegeing, actors seeking to make a credible commitment can simply place authority in the hands of others. For the King of England after the Glorious Revolution, the temptation to expropriate was avoided by delegating to parliament the power of the purse (North and Weingast 1989). Similarly, a government can desist from manipulating the economy by handing control over monetary policy to a central bank.

What is critical, of course, is that the actor to whom power has been delegated is, in fact, independent from the actor seeking to make his commitment credible. Yet delegation creates its own set of problems (Sánchez-Cuenca 1998; Miller 2005). The principal–agent framework highlights two broad classes of problems—those of hidden actions (moral hazard) and of hidden information (adverse selection). Whether the agent has superior information about his actions or his type, the principal experiences some degree of agency loss. If delegation occurs in an effort to tie hands, such agency loss may be evidence of the credibility of the commitment. If the principal delegates authority as a remedy for his own lack of time, expertise, or information, then he trades off this solution for some loss of control over the agent. The principal–agent framework is flexible enough to capture the various types of trade-offs political actors must consider and the conditions under which they occur (Gailmard forthcoming). As the following examples illustrate, this flexibility is critical since political actors frequently encounter situations in which agents possess more than just superior information about actions or types. Compliance by the agent cannot be taken for granted, and agents sometimes are in the position to force principals to comply with their decisions—both significant deviations from the standard principal–agent model.

Courts, for example, can use their rulings to resolve core problems related to policymaking and institutional prestige (Staton and Vanberg 2008). Justices want to achieve policy outcomes as close as possible to their ideal points. Yet they may have limited policymaking abilities in an uncertain world, providing them with incentives to issue vague opinions so that others with more expertise can implement policy outcomes. The vagueness or specificity of rulings, however, has implications for the court as an institution. On the one hand, vague decisions leave room for other actors to avoid compliance with the court’s ruling—a problem especially vexing for an institution that does not have enforcement powers. On the other hand, a vague decision can mask open defiance; so if it is unlikely that the other branches of government will abide by the court’s ruling anyway, the court may use vague language in an attempt to protect its institutional prestige.

Representative democracy is centered around a critical act of delegation: citizens charge their representatives to make decisions either as they would or on their behalf (Manin 1997). As such, citizens must deal with both problems of adverse selection and moral hazard: they must choose among agents who possess superior information about their own competence, integrity, etc...., and they must control these agents in the absence of full knowledge about their actions. That citizens have one instrument—elections—to solve both problems may make things worse: in using elections to distinguish among candidates by type, citizens may undercut their ability to optimally motivate whoever is elected once in office (Fearon 1999). The manner in which citizens select and control their elected officials is not a trivial concern, especially given the fact that in this instance of delegation agents actually have the authority to make decisions that bind their principals (Moe 1990).

The role of institutions

Given that actors face many different types of strategic dilemmas when they are making collective choices or taking interdependent actions, institutions fit into this picture in a variety of ways.

In a somewhat trivial sense, institutional structures sometimes create the strategic dilemma in the first place or serve as a forum in which these dilemmas arise. The problem of cycling emerges, for example, in a setting of group choice (e.g. a legislature) with specific proposal and decision rules (i.e. pairwise alternatives under majority rule). The very nature of representative government creates the agency problem between citizens and elected officials. Similarly, if we did not have elections with a secret ballot, then the commitment problem for voters vis-à-vis vote brokers would not exist.

Analysis of political institutions also has a long tradition in which scholars evaluate how institutional arrangements resolve problems of coordinated action and strategic interdependence. One of the most well-known and insightful examples of this tradition is Milgrom, North and Weingast's (1990) study of the law merchant. Mercantile law, they argue, evolved to facilitate economic activity in the context of changing world conditions that exacerbated commitment dilemmas that plague economic transactions. Specifically, they show that whereas in a small community repeated interaction among traders creates an incentive for honest behavior, in larger communities resembling modern society, relative anonymity among traders due to the ability to find new trading partners undermines that incentive. Just as traders in large communities face a relatively low risk of encountering the same trading partner again, they also recognize the limited capacity any cheated partner would have to spread word of their misdeed. As a consequence, a trader does not fear the consequences of dishonest behavior. To compensate for the adverse consequences of larger societies on economic activity, the law merchant developed. This institution created an information clearinghouse of which potential traders could make use to learn about each others' trading records and permit to render judgments in the event of alleged dishonesty. Milgrom, North and Weingast show that it is possible to design an information clearinghouse in such a way that traders would make use of the institution and would comply with its adjudicative decisions.

As our examples from the previous section illustrate, others in this tradition have tackled nearly every imaginable problem of social and political interaction. Among other things, scholars have evaluated how political parties can prevent cycling in complex policy spaces. They have investigated how a system of separation of powers can enable more credible commitments by leaders (North and Weingast 1989), how committees resolve informational (e.g. Krehbiel 1991) and distributive (e.g. Weingast and Marshall 1988; Shepsle and Weingast 1995) problems inherent in legislative settings, and how investigation and litigation can resolve agency loss in the bureaucracy, the judiciary, and federalism. In other settings, scholars have studied how legislative institutions allow governments to manage the interests of their constituent factions. In places with coalition governments, for example, the different parties in the government have potentially conflicting pressures from their coalition members and their electoral constituents. Some types of institutional arrangements enable the government to allow member parties to serve their electoral constituencies without undermining the coalition itself (e.g. Martin and Vanberg 2011).

In other cases, institutions constrain the possible solutions to the problem. Whereas in the previous examples institutions create incentives for people to work together and overcome their collective social dilemmas, in other examples institutions simply foreclose behavioral patterns that are counter-productive or otherwise problematic. For example, in much of the law there are clearly specified institutional rules for what types of disputes courts resolve. For disputes that do not qualify for judicial resolution, those rules foreclose using the judiciary as a means for resolving a conflict. Similarly, the U.S. Constitution specifies a division of powers among the branches of government and a set of authorities specifically reserved to some political entities. Those rules proscribe, for example, the House of Representatives from confirming presidential

nominations. Of course, these examples are of rules that could themselves be changed, but the crucial point is that the institution serves to constrain behavior, rather than induce desired choices.

Whether one views institutions as solutions to social dilemmas or mechanisms to facilitate the choice of solution—e.g. by constraining the options available to political actors—the crucial point is that institutions allow actors to manage trade-offs. They structure the options available to actors, provide a framework for evaluating competing interests, and allow for a mechanism to choose among competing solutions to social dilemmas. Of course, this does not mean all solutions to strategic dilemmas are institutional. However, the tradition of institutional analysis provides rich, extensive insight into how institutions shape social behavior.

Institutional origins

These lines of inquiry are primarily about the effects of institutions on social coordination problems. However, the social science model for making inferences about causal effects relies on the construction of counter-factuals. What would have happened if the institution were not adopted? The strongest model for making such an inference relies on the assumption that the institutions are randomly created—some places have independent courts and property rights, whereas others do not, and which ones do and which ones do not is random. Unfortunately, that institutions are randomly created is implausible. As a consequence, we must face an *a priori* question about the origins of institutional arrangements.

Specifically, we must ask ourselves *why* an institution exists. Are the institutions we observe in the world created specifically to optimally resolve the dilemma we are studying? In some cases, the answer is probably yes. To the extent that political actors want to avoid the inefficiencies resulting from uncertainty or renegotiation over the basic structures of government, for example, they may purposefully settle on a set of constitutional rules to serve as a focal point (e.g. Ginsburg, [Chapter 8](#)). Yet the specific provisions of constitutions are usually a product of political bargains, designed by actors who are keenly aware of their distributive implications and only secondarily (if it all) considered about their implications for collective welfare (Elster 1993). Similarly, the rules a society selects for legislative policy making can aid in resolving problems of collective choice, but also have important consequences for which groups in society tend to fare better in the legislative process. We know that institutional arrangements have important distributive implications (Knight 1992: [ch. 2](#)). The implications are twofold. First, even an institution that maximizes collective welfare is not free of distributive consequences. A variety of different possible institutional solutions lie on the Pareto frontier, and each one of them implies different distributional consequences. Second, and perhaps more commonly, distributive concerns may lead to a set of institutions that is not optimally designed for other roles the institutions play.

As an example, consider the U.S. Congressional committee system. There exists a deep debate about whether legislative committees are primarily concerned with resolving distributive or informational limitations in the legislature (e.g. Shepsle and Weingast 1987; Krehbiel 1991). Under the distributive account, giving committees jurisdiction over specific substantive domains enables a form of logrolling by which the farmers' representatives can control agricultural policy and Silicon Valley's representatives can control technology and patent law policy. The two groups can sustain a bargain in which they each get what they want on the dimensions about which their constituents care most. Related, the committee system, by breaking policy into discrete dimensions debated and acted on separately, can help resolve the social choice pathologies that exist—such as cycling—when a group collectively votes on multi-dimensional policy domains.

Under the informational account, an entire legislature lacks expertise in every single policy domain and so would prefer to delegate crafting agricultural policy to the farmers' representatives and crafting patent policy to Silicon Valley's representatives. Because those representatives have strong interests in the policy domains, they will have an incentive, lacking for others, to collect information and develop expertise in each of their respective policy domains.

What is critical about this example is that the inferences we draw about the committee system's ability to resolve informational problems, for example, may be different if they were intended to resolve distributional battles rather than informational limitations. Drawing an inference about whether the committee system is more about informational challenges or more about distributive concerns cannot be accomplished simply by examining the committee system and seeing whether it is stacked with preference outliers who push for extremist policy. One must begin with a theory of the problems facing a legislature before a committee system existed, what the goals were for legislators, and what choices were available to them. Once we consider the *a priori* choice among institutional arrangements we can begin to consider what consequences institutions have on incentives and behavior.

Indeed, this observation is the crucial point Diermeyer and Krehbiel (2003) advance. Drawing an inference about the effects of an institutional structure on political behavior requires scholars to make assumptions about actors' preferences and then evaluate how they behave under alternative institutional arrangements. Understanding why those institutions exist, in turn, requires backing up that logic one step, making an assumption about political actors' preferences and evaluating which institutions they adopt under varying conditions. This latter analytic goal is clearly more complicated than the former, but it offers the potential for important insight and raises a host of challenging lines of inquiry, to which we turn shortly.

First, however, we highlight a second, related, challenge in studying institutional origins. Specifically, institutions are usually not established by fiat by one actor. So part of the process to understand is how such institutions (that solve the collective problem) actually emerge. For example, a regime party may be useful for sharing spoils and avoiding internecine conflicts among authoritarian elites. But it also requires each of them to give up some part of their fiefdoms. So how does the party emerge? Do they realize that it is collectively beneficial for them in the long run? Or must it be imposed, perhaps on some but not others? Further, if any particular institutional arrangement will have long-run distributive consequences for those subject to the institution—for example by resulting in policies that tend to favor special interests over median voters—then might not the institutional founders anticipate those outcomes and have a harder time agreeing on institutions in the first place?

The upshot is that because groups must agree to institutions in the first place, forward-thinking political actors are likely to build in to their initial negotiations an anticipation of what will follow, perhaps not being able to agree to institutional solutions to short-run problems because of long-run concerns, or vice versa. When studying institutional origins, then, it is important to consider the full range of concerns that might be in play—concerns about social efficiency but also concerns about distributive politics and the political consequences of resolving the social dilemma. The rub may be that sometimes the solution is worse than the problem, at least from some people's perspectives.

Complications of studying institutional origins

Once one opens the lines of inquiry to the origins of institutions and the extent to which they are endogenous to political preferences, distributive concerns, and contextual factors, a host of challenging opportunities emerge. Among these intellectual puzzles are: (1) unintended

consequences; (2) institutional change; and (3) the effect of institutions on preferences themselves. In our view, these topics are among the most important, yet elusive, issues in current research on political institutions.

Unintended consequences

One issue that confronts scholars concerned with institutional origins is that the most significant effects of an institutional arrangement may be distinct from the social dilemma the institution was designed to address.

When the military seizes power, for example, it often dissembles about if and when it plans to allow the return of civilian rule. Initially, the military government that came to power in Chile in 1973 was no different. But within the junta there was disagreement about the restoration of a civilian regime, with Pinochet encouraging indefinite military rule (Barros 2002). But because other members supported a return to the barracks, the junta drafted the 1980 constitution both to regularize its own rule-making, but, more importantly, to circumscribe the degree to which future governments could enact policy outcomes far from the military's preferences. In order to secure the validity of the constitution before a transition, the junta decided to put into operation as much of the constitution as possible.

What it could not foresee, however, was the dilemma that would emerge should other institutional actors challenge the junta's authority under the rules of the new constitution. The constitution stipulated that a plebiscite should be held to approve or reject the junta's nominee for president. The junta began organizing such an event for October 1988 when voters would vote "yes" or "no" for an eight-year term for Pinochet as president. But before the plebiscite, the constitutional court both struck down a transitory article leaving oversight of the contest to an ad hoc electoral court and required that the electoral system, as fully specified in the constitution, be put in place beforehand. With that ruling, the junta found itself between a rock and a hard place: either comply with the court's decision, resulting in a fairer election and a higher likelihood of losing, or defy the court by manipulating the plebiscite for Pinochet's victory but at the cost of undermining the constitution. In the end, the other members of the junta convinced Pinochet to accept the court's ruling, which ultimately led to his electoral defeat. As Barros (2002) observes, an institution created with the intention of binding others ultimately ended up constraining the junta and ushered it out of power.

What this and other examples of unintended consequences highlight is the importance of ex ante specifying actors' preferences. Members of the junta—other than Pinochet—wanted an eventual return to civilian rule, but they did not write the 1980 constitution with the intention of being constrained by it while they were in power. If we had inferred their goals and objectives from their actions—that complying with the court's decision was an indication of their desire to be constitutionalist—we would not be able to conclude that the institution produced consequences that were unintended from their perspective.

Institutional change

Closely related to the study of unintended consequences is the study of institutional change. As noted, institutions can be understood as equilibria in social settings; a feature of equilibria is that they are stable. Nevertheless, we often observe change in political institutions, from changes to legislative rules to the re-writing of constitutions. If institutions are stable equilibria, what prompts change? In the literature, scholars often distinguish between endogenous institutional change and exogenous institutional change, though, as we will see, the distinction is not always

so clear. Further, scholars often distinguish between types of institutional evolution, with one particular area of focus being the path dependence of institutional development.

If an institutional arrangement works to resolve a social dilemma, then changes to that institution must come from one of two sources. On the one hand, there could be an exogenous shock to the strategic setting or the preferences of the individuals subject to that institution. The discovery of immense oil and mineral resources, for example, may enable a country's rulers to buy off or suppress opposition, resulting in greater authoritarianism. As this example illustrates, a shock to the environment may prompt a revision of the institutional arrangement—the regime—because actors have new preferences or new strategies available to them. Similarly, one might imagine the broader contextual features in which an institution operates change—for example because of a dramatic electoral outcome or a financial crisis—which could create the same kind of disruption to equilibrium outcomes and precipitate institutional reform. In some sense, exogenous shocks provide succinct explanations for some institutional change. Yet even the seemingly most “exogenous” sources of change often have roots within the existing institutional structure. The discovery of resources may be exogenous, but the decision to develop this sector of the economy, become a resource exporter, and use resource rents for political purposes is probably a function of the country's current institutions, including its regime (Haber and Menaldo 2011; Schrank 2004). On the other hand, many instances of institutional change seem to follow from processes that *ex post* seem predictable.

In these situations, institutional change is said to be endogenous to the institutional framework itself. The incentives created by an institutional framework induce individuals to take actions that themselves undermine the institution and bring about change. One notable example of such endogenous institutional change is Carrubba's (2009) model of endogenous emergence of judicial independence in a federal setting. In Carrubba's model, courts are faced with a task of resolving disputes in a federal setting—i.e. between sovereign entities with their own political bases of support—but have no power to compel compliance with their decisions. Because courts dislike making decisions that are ignored, they only resolve disputes in such a way that both parties will be willing to voluntarily comply. At the same time, the sovereign entities have political constituencies characterized by uncertainty about whether they prefer the state to comply with the court's decisions. Over time, as the constituencies observe compliance and (generally) beneficial outcomes, they begin to expect that the court operates to their benefit. That changing perception creates a political pressure for the sovereign litigants which in turn enables the court to make increasingly aggressive decisions that attain compliance. In short, Carrubba's model is one in which the court's equilibrium strategy changes beliefs over time in such a way that equilibrium behavior itself changes.

Studying institutional change, though, raises additional questions about the way in which institutional innovations connect and relate to previous institutional arrangements. One complex and engaging area of research is on the path-dependent nature of institutional evolution. In this view, the particular choices made over institutional rules at one point in time affect the options and incentives facing institutional designers in the next period. On one account, path dependence seems to pose the risk of undermining the systematic study of institutional change—if unique, idiosyncratic factors are critical for understanding institutional development, then a predictive, general theory of institutional change seems out of reach. A more promising view of path dependence, though, might make predictions about the kinds of factors that are influential in institutional choice, the choices and outcomes most likely under predictable circumstances, and the long-run consequences of initial conditions or choices.

A closely related idea is that as institutions develop and evolve, rather than new rules or arrangements supplanting old ones they “layer” on top of each other. Institutional innovations

generally modify past rules or exist alongside them. The consequence is that any particular institutional development serves not to create a new set of rules or incentives that exists in a vacuum but instead interacts with past arrangements. This perspective again complicates the goal of studying institutional effects insofar as individual institutional rules can only be understood in the context of the existing institutions on which they are layered. However, it also provides a mechanism for understanding the reason we see seemingly abrupt changes to institutions that appear to be the consequence of incremental changes over time.

As our brief discussion implies, there are myriad challenges facing the analyst of institutional change. This remains an important area of research and promises to influence institutional analysis in fundamental ways. However, because this is another area of inquiry at the cutting edge of the discipline, it is one where we have more questions than answers, and we hope to see much research on institutional change in the future.

The effect of institutions on preferences

A final challenge is one that brings us full circle in the study of institutions. As noted, in order to assess the consequences of political institutions, one must hold something constant—and that something is usually preferences. The model of institutional analysis described in Diermeyer and Krehbiel (2003) is one in which we make assumptions about individuals' motivations—i.e. preferences—and then theoretically or empirically (or both) evaluate how individuals' choices and behavior vary under alternative institutional settings. However, under some circumstances the effect of an institutional arrangement is to change preferences themselves, thereby complicating the model of inference that guides the inquiry in the first place.

Gailmard and Patty (2007) study this type of phenomenon in the context of bureaucratic expertise. In their study, a legislature is faced with an agency to implement policy, and the bureaucrats in that agency may or may not have an interest in developing policy expertise (i.e. they may be “zealots” or “slackers”). In their model, which repeats over time, the legislature must decide how much policy discretion to give the agency. Bureaucrats then decide how much to invest in policy expertise development and whether to remain in government or exit for private employment. Those grants of discretion create an incentive for zealots to develop expertise and remain in government; however, they do not create an incentive for slackers to develop expertise or to remain in government. The consequence is a selection process whereby only policy-motivated bureaucrats remain in office. So, the preferences of the bureaucracy change endogenously as the game plays over time and the legislature and bureaucrats make rational choices about discretion, expertise, and employment options.

Indeed, it is not hard to see how this and similar dynamics are at work in many political settings. The institutions we develop—in Gailmard and Patty, civil service and delegation—create incentives for actors to care about some goals over others. In light of the Diermeyer and Krehbiel model of institutional analysis, however, this creates a significant problem. We cannot fix *a priori* a set of bureaucratic preferences that underlie agency action. As the agency does its job of developing expertise and implementing policy, the legislature responds rationally by making choices (i.e. discretion) that can affect *who* works in the bureaucracy in a non-random way—focusing bureaucratic preferences. If institutions, as they operate, affect what actors' underlying goals are, then, as Diermeyer and Krehbiel point out, it is not clear how one can evaluate the effect of institutions on behavior.

Developing models of endogenous preferences, in the spirit of Gailmard and Patty, is one way forward, though these theoretical exercises are challenging and remain at the cutting edge of the literature. We view the potential for endogenous preference change as one of the major obstacles

to institutional analysis in contemporary literature and expect much research in this area moving forward.

Conclusion

Scholars traditionally study their particular substantive institution of interest within their respective subfields. This segmentation emerges for understandable reasons related to professional specialization. But if we think about how institutions emerge and function as responses to a variety of strategic dilemmas facing political actors, there are good reasons for scholars to pay attention to institutions across a range of different substantive contexts. For example, scholars interested in commitment problems—whether they emerge within the context of vote-buying, peace agreements, or constraints on executive power—can and should learn more from each other. Similarly, scholars who examine principal–agent relations within a particular context can learn more about related areas *and their own* subject of study by engaging with work on delegation across different substantive contexts. It is only with this kind of intra-disciplinary work that we can make headway in understanding more complex questions related to institutional origins and change.

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4

ENDOGENOUS CHANGE OF INSTITUTIONS

Adam Przeworski

Introduction

The central claim of “new institutionalism” is that institutions affect outcomes, they “matter.” Yet the new institutionalism also recognizes that institutions are endogenous. Suppose both these claims are true. What are their joint implications? What historical paths should we observe if they are true? When should we expect institutions to change?

These are the questions examined here. The chapter offers a rudimentary analytical framework for understanding endogenous institutional change, with some distinctions and examples. It is not intended as a summary of the empirical literature nor as a review of alternative approaches (see Greif and Laitin 2004; Greif 2006; Caporaso 2007;¹ and Tang 2011). Its purpose is purely methodological. Moreover, I consider only one form of endogeneity, namely, the class of situations in which institutional change is induced by some developments generated by the existing institution, not by some conditions that arise independently.

As a heuristic example consider the filibuster rule used with regard to confirmations of judicial appointments by the U.S. Senate. Let the current number of accumulated judicial vacancies be $y(t)$ and the number of new vacancies that appear at each time $x(t)$. Using the filibuster (60 percent majority) rule, the Senate converts at each time the proportion α of the backlog, $y(t-1)$, into the current number of vacancies, thus generating a process $y(t) = (1 - \alpha)y(t-1) + x(t)$. The current number of vacancies is evaluated at each time by each of the senators according to their preferences. Each senator, $i \in 100$, has some threshold value y_i , such that when $y > y_i$ this senator prefers to change the rule for confirming appointments to simple majority. If and when a majority of senators finds that there are too many vacancies, $\sum_i : \{y(t) > y_i\} > 50$, the filibuster rule is replaced by a simple majority rule. Hence, institutional change occurs when the number of vacancies generated by the current institution exceeds some value.

This is all there is to it, and the standard tools of dynamic games suffice to analyze it.² There is some institution, this institution affects some outcomes, these outcomes are evaluated by individual agents, and when those who can change the institution do not like its outcomes, preferring some feasible alternative, they change it. Note again that a particular institution may change independently of the outcomes it generates, when some other conditions change. Filibuster rule, to continue with the example, may be abolished simply because the partisan composition of the Senate changes independently of the number of vacancies it generates. Such a change is

also endogenous in the broader sense in which particular institutions can exist only under specific conditions and change with these conditions, even when these conditions are exogenous with regard to the particular institution.³ In what follows I focus exclusively on endogeneity in the narrower sense, namely, institutional change that results from the dynamic of some states of the world that are generated by the institution subject to change.

To build intuitions, I first present a sketch of the first theory of endogenous change, Marx's theory of the dynamic of capitalism, and a more complete theory of history as a succession of stages by Lagerlöf (2002). Then I offer a rudimentary analytical framework, followed by a series of distinctions and examples that highlight specific issues. A brief summary closes the chapter.

Stage theories of history

Stage theories of history, beginning with Auguste Comte's distinction between "theological, metaphysical, and positive" mental states, saw development of societies as proceeding lawfully from some less to some more advanced forms of social organization. Thus, for Herbert Spencer societies were to evolve from "military" to "industrial," for Lewis H. Morgan from "savagery" to "barbarism" to "civilization," for Émile Durkheim from "*solidarité mécanique*" to "*solidarité organique*," for Ferdinand Tönnies from "*Gemeinschaft*" to "*Gesellschaft*," and for political science of the 1960s from "traditional" to "modern." None of these theories, however, specified the micro-mechanisms by which each stage would evolve to reach the next one. The first theory that did was Karl Marx's theory of capitalism, which is why I use it to exemplify endogenous institutional change. Yet among the stages distinguished by Marx (and Frederick Engels)—primitive society, slavery, feudalism, capitalism, communism—he analyzed the dynamics of only one. To illustrate what a complete stage theory of history would look like, I therefore use a recent paper by Lagerlöf (2002), where the stages are hunting and gathering, slavery, and wage labor.

Marx: capitalism

Summarizing his views as of 1859, Marx wrote:

At a certain stage of development, the material production forces of society come into conflict with the existing relations of production.... From form of development of the productive forces these relations turn into their fetters. Then begins an era of social revolution.... No social order is ever destroyed before all the productive forces for which it is sufficient have been developed, and new superior relations of production never replace older ones before the material conditions for their existence have matured within the framework of the old society.

This is history working like a clock: an institutional system operates successfully as long as one hand rests behind the other and changes when the two hands coincide, not a minute sooner nor later. But societal clocks can only work if someone pushes the hands: Even if the theorems of social science concern macro-level changes, their proofs must explain how such changes are generated.

Marx's solution was to think of actors as occupants of places in the relations of production—"here individuals are treated only as personifications of economic categories, embodiments of class relations and class interests" (*Capital*, Vol. I, *Preface to the 1867 edition*)—and to consider only the actions they *must* undertake as such. As individuals, capitalists can be good fathers, correctly

understand how capitalism functions, even be revolutionaries (Engels), but they must maximize profits; otherwise they will not remain capitalists.

Why, then, must capitalism be replaced by another form of social organization? To answer this question, the reader must forbear by entering into Marx's peculiar accounting scheme. In Marx's terminology, capital has two parts: constant (fixed) + variable (labor). Then comes the crucial assumption: only labor generates surplus. Hence surplus value = variable capital, and total output = constant capital + variable capital + surplus. Constant and variable are inputs that are reproduced in each cycle of production, surplus is output above the costs of reproducing them. And now see what happens in this accounting scheme to the rate of profit = surplus/total capital. When constant capital is 200 and variable capital is 100, the rate of profit is $100/(200 + 100) = 0.33$. When constant capital is 500 and variable capital remains the same, the rate of profit is $100/(500 + 100) = 0.166$. *The larger the constant capital, given the variable, the lower the rate of profit.*

Thus, technological progress consists of increased labor productivity. And as more fixed capital per worker is used, the volume of production increases but the rate of profit falls: "the gradual growth of constant capital in relation to variable capital necessarily leads to a gradual fall of the general rate of profit." (*Capital*, III: XIII). Hence, as time progresses, the capitalist system must arrive at a state in which the rate of profit is zero, no one wants to invest, no one wants to produce. The system must die: there is an optimal time to change institutions, and the agents of change, capitalists, want to change them at that time. Capitalism contains a "contradiction": its development necessarily leads to its death. (Note that Keynes and Schumpeter thought the same, but for different reasons.)

Does this outcome transpire because capitalists are myopic, not seeing that by investing in fixed capital they will bring the system down? Marx's answer is that capitalists must compete with one another:

A capitalist with more capital will obtain a larger revenue than a small capitalist who appears to make higher profits . . . When the larger capitalist wants to make space for himself in the market, he uses it [the revenue] in a practical manner, that is to say, he deliberately lowers his rate of profit to push the little one against the wall. . . . A capitalist operating improved methods of production that are not yet generally adopted sells below the market price but above his individual cost . . .

(Capital, III: XIII)

Hence, capitalists are caught in a prisoners' dilemma: if one does not invest, he will be pushed out of the market by those who do; if all invest, the average rate of profit will go down:

The competitive struggle decides which part of capital will be particularly affected. The class, as such, inevitably has to lose. . . . The antagonism between the interests of each individual capitalist and those of the capitalist class as a whole comes out to the surface.

(Capital, III: XIII)

It is remarkable that workers play no role in the development or the fall of capitalism. They, too, are caught in a prisoners' dilemma of their own, with competition in the labor market reducing wages to subsistence (costs of reproduction of variable capital). They can accelerate the downfall by organizing but cannot affect the tendency. (See Vol. II, Chapter X.)

I introduce this example because the theory is thoroughly modern. The assumptions, specifically that technical progress is always labor saving, are wrong and they lead to wrong conclusions.

But look at the structure of the theory: The relevant actors are those whose decisions affect the equilibrium of the institutional system, the actors are strategic, but caught in a prisoners' dilemma. The dynamic effect of their decentralized actions is to make the particular form of social organization, capitalism, obsolete. When it becomes obsolete, no one has incentives to maintain it, so it must change.

Lagerlöf: hunting-gathering, slavery, and wage labor

Marx developed a theory of the dynamics of capitalism but not of the systems that according to him preceded and would follow it. To see a theory of institutional change that specifies the dynamics at each stage and the trajectories that lead from one stage to another, consider a simplified version of Lagerlöf (2002). In his story, there are three possible institutions: hunting-gathering (HG), slavery, and wage labor (which he calls “freedom” and Marx would have called “wage slavery”). Under HG everyone gets an equal share of the collective product; under slavery, the slaves and their guards (slaves must be supervised) receive subsistence incomes and slave owners get the rest; under the wage system workers get the marginal product and capitalists get the rest.⁴ A dictator, who is a member of the elite under each system, decides which institution to adopt.

Because the story concerns agriculture, the state of each system at each time is given by agricultural productivity (output per unit of land), A_t , and the size of the population, P_t . Output is produced according to $Y_t = F(A_t, P_t)$. In turn, the dynamics of agricultural productivity and of population are specific to each institutional system, $A_t = I(P_t)$ and $P_t = I(A_t)$, $I \in \{HG, slavery, wage\}$. Given the production possibilities and the dictator's income under each system, we can determine which institutional system is best for the dictator given each state $\{A, P\}$. Because starting from any initial conditions we can determine the trajectory of the system in the $\{A, P\}$ space, we can also determine the evolution of institutions. A simplified version of the dynamics is reproduced in Figure 4.1.

HG is optimal for the dictator when population is small relative to agricultural productivity, slavery when agricultural productivity is high and the population is of middle size, wage system when population is large relative to agricultural productivity. The thick line is the trajectory of population and agricultural productivity that results from the laws of motion under each



Figure 4.1 Stage dynamics

institution. Because both the population and productivity increase under each system (but at different rates), the trajectory leads from HG to slavery to the wage system.⁵

A framework

Let the set of institutions that are feasible given exogenous conditions be $I: \{A, B, \dots\}$. Institutions determine (perhaps many-to-one) the manner in which some available resources, x , are transformed into some outcomes, y , which are positively or negatively valued by individuals, who are economic agents and political actors. Outcomes are generated according to $y = I(x)$,⁶ so that y_t is some process, for example $y_t = (1 - \alpha)y_{t-1} + x_t$, as in the example of the filibuster rule. An agent i prefers institution A to institution B if the present value of his or her payoffs, $V_i(I)$, is such that $V_i(A) > V_i(B)$.⁷

Optimal vs. equilibrium change

To see what is entailed, consider a simple example generalized from Acemoglu, Aghion, and Zilibotti (2006). Suppose y_t stands for average per capita income, which is positively valued by everyone. Assume that an institution A generates a faster rate of growth of incomes when y is low and institution B results in faster growth when y is high. Figure 4.2 plots next year's income as a function of the current one, so that the growth rate is zero along the 45 degree line.

When $y < \hat{y}$, the rate of growth is higher under A , while for $y > \hat{y}$ it is higher under B . Hence, the optimal time to change institutions is when $y = \hat{y}$. If the change occurs when $y < \hat{y}$, it is suboptimal because it is too early; if it occurs at $\hat{y} < y < y'$, it is suboptimal because it is too late. But suppose that it does not happen until y reaches y' , at which $y(t + 1) = y(t)$, so that the economy does not grow any more, stuck at this income level. Then the change never happens.

This much is general, so that the question to be studied in each case concerns the value of y at which those who have the power to change institutions want to change them. Institutions can be altered in a centralized way, meaning that someone's decision to change them applies uniformly to everyone, or in a decentralized way, resulting from decisions of individual agents.

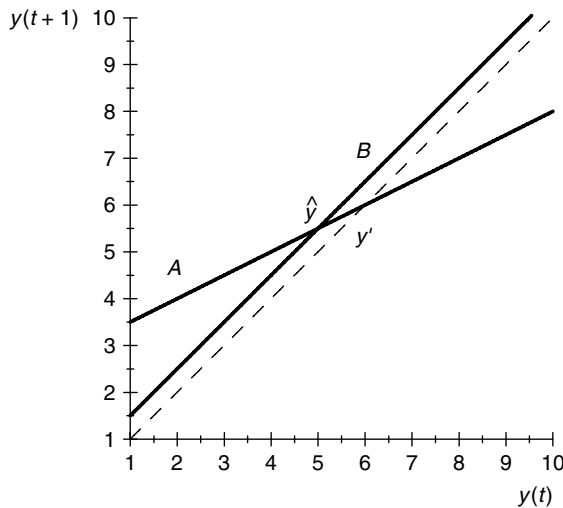


Figure 4.2 Optimal institutional change

Changing an electoral system requires a centralized decision but the institution of marriage may evolve as a result of decentralized decisions of individuals. Political institutions entail rules enforced by the threat of coercion, so the rest of the analysis is limited to centralized decisions. These decisions can be made only by those who have the power to change institutions: a dictator, a homogeneous elite, some group within a heterogeneous elite, or a broader collectivity, including the entire electorate. These are the potential decisive actors, D .

Endogenous institutional change may occur because (1) the existing institutions become inferior to some feasible alternative for those who have the power to change them,⁸ (2) those who have the power to alter institutions change, including their removal by force, as a result of the functioning of the existing institutions, or (3) those who control the existing institutions fear that unless they change them they would be removed and they prefer new institutions to the eventual outcome of being removed.

Consider an example that perhaps has been studied most extensively: extensions of suffrage along the lines of class. According to Justman and Gradstein (1999), these extensions occurred because excluding people from suffrage was costly in terms of labor productivity and, when output became more dependent on productivity, the elite's gains from political incorporation outweighed the potential losses from redistribution of income that would result from extending voting rights. The argument of Lizzeri and Persico (2004) is similar, replacing productivity gains with demand for public goods: the newly enfranchised would vote for a more extensive provision of public goods, so that when the value of public goods increased among the elite, a majority of it opted for an extension. In turn, Llavador and Oxoby (2005) think that a party of industrialists would extend suffrage to workers in order to obtain a mandate for pursuing industrialization policies, while a party of landowners would want to block such policies by enfranchising peasants in addition to workers. Hence, what matters in their story is which elite occupies office under the existing institutions and if, as industry grows under restricted suffrage, industrialists win an election and suffrage is extended to workers. Finally, there are several arguments to the effect that extensions occurred because of revolutionary threats by the lower classes which gained in strength as a consequence of economic development under restricted suffrage (Bendix and Rokkan 1962; Przeworski and Cortés 1971; Freeman and Snidal 1982; Jack and Lagunoff 2003; Xi 2014).⁹ The reasoning here is that even though because of its redistributive consequences the elite would be made worse off by extending suffrage, it would have been even worse off if it were overthrown by force as a result of not conceding to the demands of the excluded.

The main point of these distinctions is that the equilibrium change need not be optimal. Depending on who holds power under the extant institution and on how the power holders compare their interests under the extant institution and some feasible alternative, change may be premature, delayed, or not occur at all.

Conflicts over institutions

Now, it is banal that if everyone is affected in the same way by each institution, the choice of institutions will be optimal; moreover, it will be optimal regardless who decides and how the decision is made. Hence, to understand institutional change that is by some criterion socially suboptimal it is necessary to introduce conflicting interests. Suppose that under A the decisive actor extracts some rents from others, while under B it does not. Let the total income of D under A be $A(x) + R$ and under B , $B(x)$. If the rate of growth follows the dynamic portrayed in Figure 4.3, then the institutional change will either occur later than optimal by the criterion of maximizing the average income or not at all. The optimal change occurs when $A(x) = B(x)$,

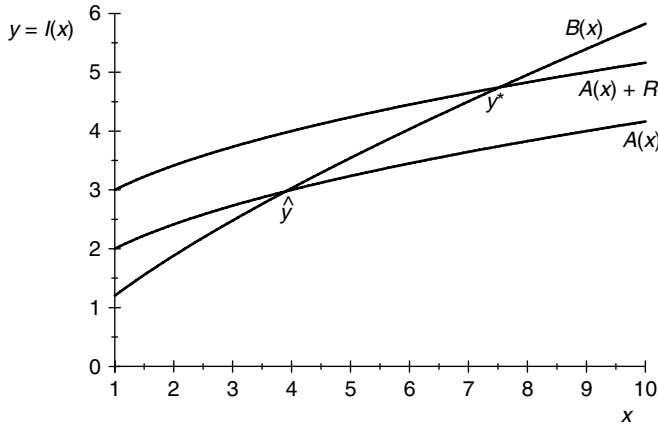


Figure 4.3 Optimal and equilibrium change when dictators extract rents

at $y = \hat{y}$, but D wants to change institutions only when $B(x) - A(x) = R > 0$, which occurs when $y = y^* > \hat{y}$.

Conversely, in many studies of democratization (Acemoglu and Robinson 2000; Rosendorff 2001), the elite fears that its incomes would be taxed if poorer people have a political voice, so that the incomes of the elite under A are $A(x)$ but under B they would be $(1 - \tau)B(x)$, where τ is the tax rate. Institutional change will be again late by the criterion of maximizing growth, occurring when $B(x) - A(x) = \tau B(x) > 0$.

One more twist. Suppose that the elite is divided in terms of its views with regard to B . Assume that one part of the elite would lose by a change to B and another part would gain. In the Lizzeri and Persico model, for example, members of the elite differ in their evaluations of suffrage extension because they would receive different values of transfers and public goods if suffrage were to be extended. As another example, consider an authoritarian regime in which some part of the elite fears retribution for the acts of repression it committed while another part thinks it will escape unscathed if the regime falls. Such situations cannot be summarized schematically because we need to specify how such conflicts might be resolved: in the Lizzeri and Persico model, for

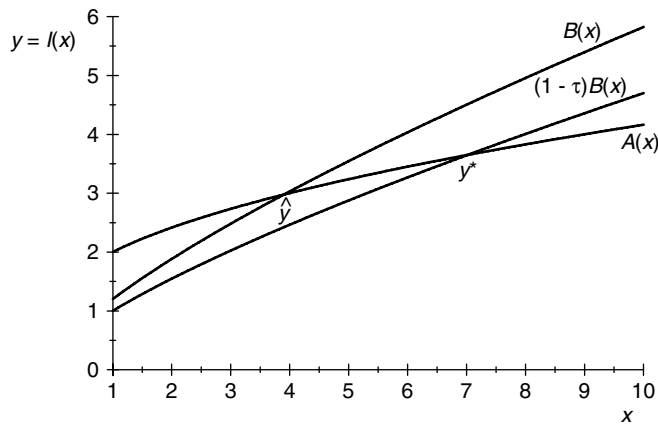


Figure 4.4 Optimal institutional change when the elite is taxed under democracy

example, elections generate a different outcome than a referendum. Yet many, if not most, institutional changes occur when the elite divides with regard to the consequences of change.

Push and pull

Once interests are in conflict, the defenders of the existing institution may face the specter of losing power if they persist to maintain it. Suppose now that the elite under A can decide to change institutions to B , where its incomes would be taxed, or face a threat of a revolution, R , that would reduce its incomes even more drastically. Now the elite under A faces a lottery with the expected value of $E_p A(x) = (1 - p)A(x) + pR(x)$, p being the probability of a successful revolution. Hence, the elite switches to B when $(1 - p)A(x) + pR(x) = (1 - \tau)B(x)$, or $A(x) - B(x) = p(A(x) - R(x)) - \tau B(x)$. Hence, whether and when a change occurs depends on the relation $p \geq \tau B(x)/(A(x) - R(x))$, that is, whether a successful revolution is a real threat relative to being taxed. The threat of revolution induces the entrenched elite to shift earlier to a more productive institution. This shift may occur too early but at least prevents permanent stagnation. Revolutions are the locomotive of history (as in a poem by Mayakovsky, even if they (almost) never occur).

Any theory that assumes that political actors are forward looking must consider the eventual consequences of institutional change for those who have the power to make this decision, but push factors may be present or absent. Push factors are absent in Marx's theory outlined earlier: He could never reconcile his theory of capitalism which self-destructs with his calls for a revolution. But they are present in many theories in which the rulers under some institution face a threat of revolution.

This threat may or may not be endogenous. For example, in Acemoglu and Robinson's (2000) model of suffrage extensions, the threat of revolution arises randomly, independently of the state of the economy. Hence, the presence of push factors is not sufficient to render the change endogenous. Yet in several other analyses of suffrage extensions the threat of revolution arises because economic development under restricted franchise leads to the emergence and political organization of the working class, so that this threat is endogenous.

Some examples

Self-institutionalization (Przeworski 2015)

Consider the viability of the institution of contested elections, specifically the probability that a series of contested elections would be interrupted by a constitutional breakdown in the form of a coup, autocoup, civil war, or some other major constitutional violation. The process works as follows: At each time there is a ruler, elected or not, who decides whether to hold a competitive election, in which his probability of winning depends on the true distribution of support, or hold an election in which he assures himself of victory, or not hold one at all. Rulers who do not expose themselves to the possibility of losing an election may be overthrown by force; rulers who run competitive elections may lose them. What each incumbent does not know is what will happen to him if he loses an election: he may be treated by the winner as just a future electoral opponent or he may experience a hard landing in the form of violent death, imprisonment, or exile. Each incumbent attaches some probability, θ_t , to soft landing. If no alternation in office by elections had yet occurred, this probability is θ_0 , but with each successive alternation this probability is updated upward until it converges to $\theta = 1$. Under these assumptions, a ruler holds competitive elections if he believes that his probability of winning is high relative to his military prowess, that his landing is likely to be soft, and that the value of being in electoral opposition is not much lower than the value of being in office. If the incumbent loses the election, the winner

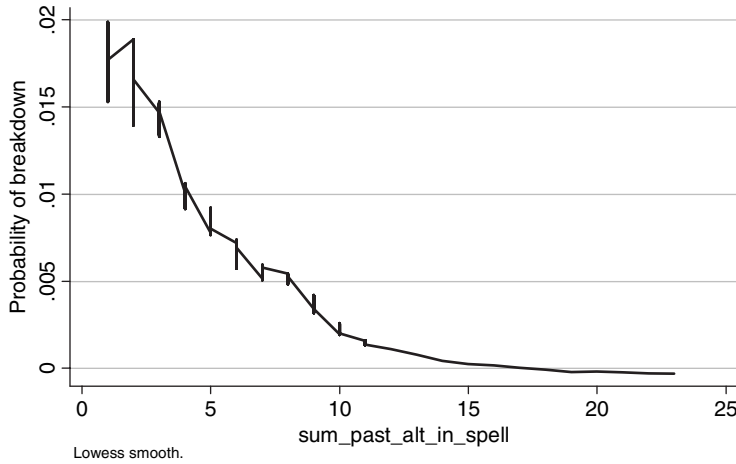


Figure 4.5 Probability of institutional breakdown given the number of past alternations

may still not reciprocate, even though he knows that the defeated incumbent is of a type who exposes himself to defeat and peacefully yields office, so that $\theta_1 > \theta_0$, if the probability that he would lose in turn is very high and the value of soft landing is relatively low. If the winner does hold competitive elections and loses in turn, θ_t is again updated upward, etc. Hence, each peaceful alternation in office resulting from elections increases the probability that the electoral mechanism would survive, until this probability converges to 1, so that the institution of processing conflicts through competitive elections locks in.

Empirical patterns show that this is what in fact happens. Hence, the institution of contested elections is more likely to survive if it has generated more partisan alternations in office. An institutional breakdown occurs either early or not at all. In this sense, this institution is “self-institutionalizing.”

Institutional cycles (O'Donnell 1977)

Argentina is the world record holder in having experienced the largest number of changes between democracy and dictatorship since the end of World War II. Here is an explanation relying on but greatly simplifying O'Donnell's (1977; see also Gerchunoff and Fajgelbaum 2006) analysis. The particular feature of the Argentine economy is that its main exports have been wage goods: cereals and meat. The consequence is that the exchange rate is a crucial variable in determining the state of the entire economy: When the Argentine currency (the name of which changed over time but let us call it by its current name of *peso*) has a high value in relation to the dollar, agricultural output is directed to the internal market, real wages are high, inflation is low, aggregate demand is high, but the balance of payments is negative; when the *peso* is low, wage goods are exported, real wages are low, aggregate demand is low, and the balance of payments is positive. Negative balance of payments accumulates to a foreign account crisis; low real wages lead to insufficient domestic demand and slow growth. There are three political actors—industrial bourgeoisie, agricultural bourgeoisie, and industrial workers—and two possible coalitions: bourgeois and industrial. The industrial coalition wants the value of the *peso* to be high, so that agricultural output is directed to the domestic market, real wages are high, and so is domestic demand for industrial output. The bourgeois coalition wants the value of the *peso* to be

low, so that the agricultural output is exported, real wages are low, and the trade balance is positive. The industrial coalition supports democracy; the bourgeois coalition can exist only under dictatorship, as it has to repress real wages.

The result are institutional cycles: When the balance of payments becomes too negative, the industrial bourgeoisie, which is the pivotal actor, breaks its alliance with the working class and collaborates with the agricultural bourgeoisie in devaluating the currency and repressing real wages by force. When aggregate demand falls too much, the industrial bourgeoisie abandons its agricultural ally and supports democracy to increase real wages. Whether or not this theory is true is not the issue here (and the effects of exchange rates are much more complicated than assumed here). The point is that institutional change need not be irreversible and there are some structural conditions, in this case economic, which may induce back and forth shifts of institutions.

The problem with this argument is that political actors are myopic. The optimal institution here is one that would support stable exchange rates and avoid the cycling. Without parametrizing the model it is impossible to say which regime could sustain this equilibrium, but it would obviously have to entail some power sharing among all the three political forces. The present Argentine government has been attempting to play this balancing act, thus far with some success. But the situation remains brittle.

A caveat: ineffective institutional reforms

The central assumption of “new institutionalism” is that institutions are a “deeper,” “primary” cause of economic development than the availability of factors of production and technological change. The theoretical program has been laid out by North (1997: 224): “To make sense out of historical and contemporary evidence, we must rethink the whole process of economic growth.... The primary source of economic growth is the institutional/organizational structure of a political economy.” Specifically, we learn that “Third World countries are poor because the institutional constraints define a set of payoffs to political/economic activity that do not encourage productive activity” (North 1990: 110).

This assumption has practical, policy, consequences: it licenses institutional engineering. Indeed, all the examples above are based on the assumption that a change of institutions results in a change of the manner outcomes are generated out of inputs, $y = I(x)$. If this were true, institutional reforms would be always effective: changing institutions would change the outputs given exogenous conditions. Yet often reforms are futile: institutions change and life goes on as before. One reason may be that the mapping of institutions to outcomes need not be one-to-one. As Rodrik (2008) emphasizes, different institutions may generate the same outcomes, good or bad. And if this is true, those who want to change institutions because they are dissatisfied with the way they function may be disappointed.

Perhaps the most vivid illustration are the Soviet-style communist systems. As distinct from contemporary China, where most economic and political decisions are decentralized to regions and subregional units (Xu 2011), the Soviet Union adopted a U-form of organization (Maskin, Qian, and Xu 2000), in which decisions were centralized in functional ministries. Without effective yardstick competition, principals could not effectively evaluate agents, and because the interests of principals and agents often diverged, the principals were repeatedly dissatisfied with the performance of agents. To remedy the situation, the Soviets incessantly introduced institutional reforms that involved the relations between the central planners and the firms as well as between the one-party and the state apparatus. Yet the agents always found some ways to get around the new rules and the status quo stagnated. As Khrushchev lamented in a 1962 conversation with Fidel Castro, “You’d think that I, as first secretary, could change anything in this

country. Like hell I can. No matter what changes I propose and carry out, everything stays the same” (quoted in Taubman 2003: 598).

More generally, there is evidence that political regimes dichotomized as democracies and non-democracies affect the rate of growth of population but not of aggregate output (Przeworski *et al.* 2000).¹⁰ Neither do they affect the distribution of incomes in market economies (Przeworski 2012). Even if institutions cause outcomes, institutional reforms may be futile when different institutions generate the same outcomes.

Conclusions

What historical patterns should be observed if institutions matter but are endogenous in the strong sense that they change as a function of the conditions they generate? As the examples detailed indicate, these assumptions do not restrict much the variety of feasible historical trajectories. Institutions may or may not change endogenously. Importantly, suboptimal institutions may lock in if they do not change when the conditions they generate still allow it: institutions are subject to traps, as in the example of economic growth. In turn, good institutions may also lock in once they survive some initial period; they may be self-institutionalizing, as in the example of competitive elections. Institutions may experience cycles if the elites which dominate each of them are not sufficiently far-sighted, as in the example of Argentina. When different institutions generate the same outcomes given the endogenously generated conditions, institutional reforms may be ineffective, as in the example of communist reforms. Yet one may also expect that some historical patterns do follow stages: the dynamic of outputs generated by one institution generates endogenous transitions to some other institutions, with different dynamics, and so on, as in the examples of Marx and Lagerlöf. Because all institutions have distributive consequences, institutions are subject to conflicts, and a revolutionary threat by the social forces generated by a particular institution may propel their change by the elite vulnerable to this threat, as in the example of suffrage.

The purpose of this chapter is merely to introduce an analytical framework for understanding endogenous change of institutions and to make some distinctions that may be relevant in analyzing particular cases. It is best summarized as a recipe, a list of questions that needs to be posed when studying institutional change:

1. What are the states that affect the evaluation of alternative institutions by different political actors?
2. How do these states change as a consequence of the functioning of the institution in place?
3. What are the feasible alternatives to the institution in place?
4. Are those in power under the institution in place threatened with being removed unless they change institutions?
5. If optimal change can be defined, when is it to be expected that the change will be optimal, premature, or belated, and when will a society be stuck with suboptimal institutions?

Notes

- 1 This is a different approach, developed narrowly to explain the institutional evolution of the European community. The mechanism of change here is deviations from agreements.
- 2 Greif and Laitin (2004) invent a “puzzle,” namely, that if the institutional equilibrium is self-enforcing then all change must be exogenous, and solve it by introducing concepts of “quasi-parameters” and “institutional reinforcement.” As is obvious from what follows here, I see no such puzzle. Already Marx knew how to cope with this issue.

- 3 Montesquieu as well as Rousseau, the latter in his folkloric description of Poland, claimed that particular institutions can function only if they correspond to cultures, mores, religions, or geographic conditions. J. S. Mill considered the issue of endogeneity in the [first chapter](#) of *Considerations*, entitled “To What Extent Forms of Government Are a Matter of Choice.”
- 4 Both under slavery and wage labor, there are subcases in which not everyone is employed. I ignore them here.
- 5 There are also conditions, not portrayed here, under which slavery becomes a trap.
- 6 Note that x and y may be the same, as in “corn” models of growth, where the same commodity can be consumed or invested to be used as an input.
- 7 The present value is $\sum_t \beta^t U(I_t)$ or $\int_t e^{-\rho t} U(I_t) dt$, where $U(I_t)$ is the instantaneous utility an agent receives at each time from the institution I .
- 8 Jennifer Gandhi (personal communication) suggested an interesting example of an institution becoming simply obsolete: electoral quotas for women may become unnecessary when voters learn not to discriminate as the result of more women occupying prominent political positions as an effect of the quota.
- 9 I do not include Acemoglu and Robinson (2000) because in their formulation revolutionary threat is exogenous and random.
- 10 Note that several researchers, most recently Acemoglu *et al.* (2014), found that regimes affect the rate of growth of per capita incomes. But this effect is due exclusively to different rates of population growth.

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5

FORMAL AND INFORMAL INSTITUTIONS

Hans-Joachim Lauth

Introduction

“Generally, the idea that there is a dividing line between institutions that are entirely ‘formal’ on the one hand and entirely ‘informal’ institutions on the other is false” (Hodgson 2006: 18). Hodgson’s surprising statement is a call for scholars to engage a set of issues. First, there is a need for definitions of formal and informal institutions that allow them to be clearly distinguished from one another (Hayoz 2013). Second, it is important to debate the relationship between formal and informal institutions. In doing so, we should distinguish between different types of informal institutions and their varying complexities.¹ Third, it is important to understand the implications of formal and informal institutions for regime type (autocracy vs. democracy) and vice versa. In this context, the question about the generation, creation, and survival of informal institutions is also addressed. Finally, the chapter discusses methodological approaches towards formal and informal institutions. For this purpose, the chapter reflects on the use of typologies that cover formal institutions and which are widely used in the study of governance systems. The starting point of the following considerations is the finding by Helmke and Levitsky that “informal structures shape the performance of formal institutions in important and often unexpected ways” (2004: 726).

Institutions

Since Aristotle, comparative analyses of institutions have focused on the structures outlined in the constitutions of the countries being studied. Scholars have not explicitly distinguished between organizations (such as parliament or government) and systems of law (such as the electoral system or the constitution itself). The classic studies of institutional research focus on the interests that gave rise to legal structures and rules. Even while scholars were attempting to demonstrate how constitutions worked in reality, institutionalist approaches fell short of incorporating informal rules and institutions in a coherent theoretical manner. This only began to change in the 1980s and was part of the wider debate about the discussion of neo-institutionalism (Peters 2005: 3–21). Critiques of the classical institutional approach offered numerous possibilities for developing institutional research further. Scholars began to regard institutions as dependent and not just as independent variables. Scholars also began to engage in comparative analyses

to a much greater extent. Finally, scholars also started to examine those institutions that did not have a legal and formal background and incorporated these institutions into their analysis.

Definition

The starting point of the definition of institutions is similar in all neo-institutionalist approaches. Like Douglass North (1990: 3), I regard an “institution as a norm or set of norms that have a significant impact on the behavior of individuals” (concerned by or included in the institution). Institutions thus constrain the actions of individuals. Although North did not emphasize the role of sanctions, in the neo-institutional debate one can find different interpretations of constraints that are linked with them.

General agreement exists that institutions restrict individual behavior to some extent (Peters 1999: 18). The extent and the mechanisms through which this occurs vary. Some authors (March and Olson 1984) highlight the internalization of norms during processes of primary or secondary socialization (family, kinship—school, military, companies). In this case, not following the rules causes a guilty conscience, and deviations are sanctioned by an internal mechanism. External sanctioning mechanisms also exist (social discrimination or exclusion, loss of status, arrest, etc.). Rational choice perspectives include the latter, as rational choice approaches have included the possibility of suffering from disadvantages when rules are not followed. In this case, actors violating the institutions will not benefit from incentives linked to the institution.

Common to all types of enforcement mechanisms is that defecting from the rules set by informal institutions implies losses for rule-breaking individuals. To avoid a catch-all category, which includes all sorts of inconveniences (caused by a particular sanction mechanism), it seems convincing to consider rules as institutions only when they maintain (their own) external sanction mechanisms. This obviously applies to formal institutions.²

Even if sanctions are a defining feature of institutions, they are not the only reason why actors follow institutions. Actors follow institutions because they regard them as given or “natural.” Actors also follow institutions because they display a legal character or because they regard them as legitimate. In accordance with North, these reflections on sanctions and the reasons why actors follow rules relate to the main purpose of institutions: “Within an institutional perspective, a core assumption is that institutions create elements of order and predictability” (March and Olson 2006: 4). Summarizing all of the above, institutions are defined as follows: Institutions constitute a set of rules. The set of rules implies rights and responsibilities. The set of rules also creates and shapes a social order in such a way that the behavior of all actors involved in that social order is predictable. Institutions affect performance by voluntarily following the rules or being motivated by the threat of sanctions.

Formal vs. informal institutions

To differentiate between formal and informal institutions, the following serves as a useful point of departure: *Informal institutions* are institutions that are *not formally codified* in official documents (in constitutions or laws). *Formal institutions* are officially codified in written documents. Thus, regulations are included which have the status of constitutional clauses and laws, but also standing orders and norms that have legal consequences. According to this line of thought, all private contracts or rules of associations which are protected by the state are formal institutions.

Formal institutions are guaranteed by state agencies and deviations from these institutions are sanctioned by the state. Informal institutions also have sanctions in place. These sanctions include either mechanisms of social exclusion or mechanisms that restrict access to much-needed goods

and services. Informal institutions are known and recognized publicly; however, they are often codified. Their authority stems from various sources. First, informal institutions are socially accepted, which provides them with a basic degree of legitimacy. The fact that these informal institutions are socially acceptable also serves as a major source of motivation for actors when they follow the patterns of social conduct prescribed by these informal institutions. Actors pursue different purposes when they enter these patterns of conduct; purposes can be defined either narrowly or broadly. These purposes can be linked to outputs as well as to certain patterns of behavior. Institutions facilitate interaction between individuals and groups. They foster stability by creating known and accepted behavioral structures that cannot be changed by individual people. Even if actors disagree with these structures, they obey them because, in accordance with rational calculation, the costs involved in rejecting them can only be offset when behavioral alternatives are available.

In contrast to formal institutions, which receive legitimacy from the state and—at least in democratic regimes—from the sovereignty of the people, most informal institutions emerge through social actions without guidance (“invisible hand”); another intentional way of emergence is discussed later. While the nature of formal institutions can be shaped and changed by actors with rule-making authority (Mayntz and Scharpf 1995), this is unusual in the case of informal institutions, as these depend on deeply rooted social practices. They (mostly) do not possess a central body that directs and coordinates their actions. If informal institutions are no longer recognized as such, they cease to exist. By contrast, ineffectual formal institutions continue to be in demand and, formally at least, continue to remain in place. These considerations correspond with the proposed definition by Helmke and Levitsky (2004: 727): “We define informal institutions as *socially shared rules, usually unwritten, that are created, communicated, and enforced outside of officially sanctioned channels.*” The most significant characteristics of formal and informal institutions are presented in [Table 5.1](#).

Although institutions constitute social phenomena at the macro level, they cannot be properly understood without an understanding of what happens at the micro level. Informal norms exist only if they are rooted in the beliefs and/or attitudes of individuals. Their acceptance is not necessarily based on a positive normative assessment. The existence of an informal institution merely indicates that the institution is expected to function. Whether or not the institution and its output are regarded as good is irrelevant. We find informal institutions in the beliefs and attitudes of individuals. If not found there, they do not exist. The individual foundations of informal institutions help explain their slow transformation. While formal institutions can be changed solely by state authorities, the process of change within such socially-based institutions is extremely lengthy, as informal institutions are internalized by the participating actors and reproduce themselves by shaping future behavioral expectations. Despite their unofficial nature,

[Table 5.1](#) Formal and informal institutions

<i>Categories</i>	<i>Formal institutions</i>	<i>Informal institutions</i>
<i>Formally codified</i>	Yes	No
<i>State sanctions</i>	Yes	No (social sanctions)
<i>Legitimation</i>	Sovereignty of the people (democracy) or state authority	Self-creation (social acceptance)
<i>Modus of change</i>	Action of political decision makers	Change of incentives / public discussions / by force
<i>Timing of change</i>	Short-term	Long-term (mostly)
<i>Coordinating center</i>	Yes	No (or seldom)

informal institutions can be understood and described precisely at the analytical level, as they display specific functioning logic and rules of identity that distinguish them from others.³ As stated by Zintl (1993: 89): “We are dealing with systems which have a clear difference between the internal and external perception of actors as well as in terms of the type and density of interactions, not simply with statistical aggregates.”

Differences between informal institutions and related concepts

This short discussion has emphasized the differences between formal and informal institutions. Regarding their ideal types (on the methodological level), the difference is not only in degree, as noted by North (1990: 46). The reflections also demonstrate that informal institutions are more than a residual category, including all non-formally codified patterns of behavior. To highlight this further, informal institutions are distinguished from other concepts in the following discussion.

Informal institutions are more than *regularities* (in the sense of habits or routines, like the specific sequence of the United States primaries), or structures (like patterns of social stratification), or fashions and trends (if most people wear the same style of clothes during a particular season, we do not consider that to be an informal institution). Regularities and structures are important aspects of informal institutions, but these do not constitute sufficient characteristics.

In many neo-institutional approaches, *cultural patterns* and routines are treated as informal institutions. There seems to be no difference between informal institutions and culture. Equating informal institutions with culture, however, would mean ignoring important particularities. Informal institutions and culture are never identical. Although it is very difficult to find a generally accepted definition of culture, all existing definitions share two features. First, culture indicates a broad concept including rules, values, traditions, and customs. Together, these indicate patterns of a collective identity, which in turn provide a specific interpretation (or construction) of the world and motivate the adoption of common lifestyles (or frame the horizon of possible actions) to a certain degree. Culture as a whole, however, does not determine individual actions in the same (concrete) way that informal institutions do. Second (and closely linked to this first point), while informal institutions can be enforced through sanctions, cultural patterns lack sanctioning mechanisms. Even when actors do not act in accordance with their respective cultural patterns, they are not sanctioned for their deviations by the cultural norms in place. A third aspect must be included: the distinction between values and expectations. In the words of Helmke and Levitsky (2002: 38): “Informal institutions reflect shared expectations, but not necessarily shared values.” In this sense, informal institutions denote the cognitive side of culture.

Culture and informal institutions are overlapping concepts. Informal institutions can be understood as an integral part of cultural patterns. Some social phenomena, such as customs, include both. Not all types of customs should be understood as institutions, however. In some cases, customs relate to the routine behavior of individuals or small groups. They have no specific meaning for others. Customs can also include the routines of very large groups. This does not mean that these customs constitute informal institutions (for example “many people are used to participating in elections” or “many people are used to going on vacation”). Such customs can be changed on the individual level easily because the transaction costs of doing so are rather low. On the other hand, customary law definitely indicates a type of informal institution.

Informal institutions are also not the same as *informal politics*, although some authors use the concepts interchangeably (Eisenstadt 2003). Informal politics include different sets of actions, which must not necessarily be linked to institutions. Informal actions can be based on rules, but they can also have a spontaneous or erratic character. Examples of informal politics are coalition

discussions, corporatist meetings, or private talks behind closed doors. Similar to cultural patterns, informal politics have no noteworthy sanctioning mechanisms in place.

Informal institutions are only one part of the world of informality. As the considerations about other expressions of informality have shown, the differences between informal institutions and these other expressions are significant, but not substantial. In this regard, it is possible that regular practices, informal practices, and politics can be transformed into powerful informal institutions.

Relationship between formal and informal institutions

The relationship between formal and informal institutions varies significantly. Informal institutions and formal institutions may: (1) compete with each other; (2) reinforce each other; or (3) maintain a neutral relationship. Only the first two cases are scholarly relevant. Relationship type (2) indicates a case common in many established democracies, where formal and informal rules reinforce each other. Informal institutions provide guidance in situations where the existing framework of formal institutions does not apply. Informal institutions can also mitigate the rigidity of formal institutions. In cases where formal and informal institutions compete with each other (relationship type (1)), formal institutions are weakened. Informal institutions change, weaken, or undermine the rules laid out by formal institutions. Whenever this occurs to a considerable degree, formal institutions no longer have the capacity to influence the behavior of actors significantly. Thus, formal institutions can no longer guarantee the desired behavior of actors.

Competition between formal and informal institutions can even lead to displacement or the elimination of formal institutions. It can also cause a precarious coexistence between the two. In such situations, informal institutions depend upon the existence of formal institutions. Informal institutions exist at the expense of formal institutions and exploit formal institutions for their own purposes. Informal institutions either partially occupy formal institutions or penetrate them completely. Thus informal institutions are parasitic institutions. Corruption is an example of such an informal institution. Informal institutions should be understood as “penetrating” environments (Powell and DiMaggio 1991: 13). They evade any quantitatively oriented empirical analysis to a considerable degree, as they do not change the letter of the law of formal institutions. In this way, informal institutions are also often invisible.

Helmke and Levitsky offer an alternative distinction between formal and informal institutions (2004: 728). They differentiate between four types of informal institutions: (1) complementary; (2) substitutive; (3) accommodating; and (4) competing. It is not entirely convincing to distinguish types of informal institutions on the basis of their relationship with formal ones, as this makes any classification dependent on a particular context (a point to which I subsequently return). Two of the four types conceptualized by Helmke and Levitsky correspond to the relationships outlined above. When formal and informal institutions are mutually reinforcing, they are complementary. When informal and formal institutions compete with one another, they are in a competing relationship. Helmke and Levitsky’s substitutive type corresponds to a situation where formal and informal institutions complement each other. In that case, it is not clear whether formal institutions are merely ineffective or whether they are missing.

Helmke and Levitsky’s accommodating type is intriguing. Here, informal rules do not impair formal rules directly, but alter the outcome of the formal rule in place. As stated by Helmke and Levitsky (2004: 729): “they contradict the spirit, but not the letter, of the formal rules.” However, the examples chosen by the authors to illustrate the accommodating type indicate that this type can be classified as the complementary type when we differentiate between two levels of

abstraction (governance vs. regime type). In the concrete example, coalition presidentialism, which is based on informal coalition agreements, contradicts the spirit of presidentialism (as a form of governance), but, as a democratic institution, not the democratic spirit. So, the accommodating type is equal to the complementary type when the intention of the informal institution aligns with the normative intention of the formal institution. Coalition presidentialism can also be linked with other forms of informal institutions, like “perverse elite agreements” (Thiery 2011: 17) and “clientelism.” These may not always constitute illegal acts, yet they clearly are not in accordance with the democratic spirit.⁴ In cases where the intention of these institutions does not align with democratic ideals, such informal institutions must be classified as competing and not as accommodating ones. A deeper examination of Helmke and Levitsky thus reveals that the normative character of informal institutions should not be ignored. A typology of informal institutions should also be based on the internal features of institutions and not on the relationship between formal and informal institutions.

Types of informal institutions

The previous considerations have mentioned various expressions and forms of informal institutions, which are only one part of the world of political informal institutions. Categories are necessary to structure the complexity we are faced with in the formal world. The social science literature provides different examples that classify informal institutions. Some are characterized by a high *level of abstraction*, such as trust or distrust (Putnam 1993). The existence of such basic norms (dispositions of conduct) has a significant effect on actor behavior in various areas.⁵ Others are linked to special circumstances (*low level of abstraction*), or express a very specific meaning in certain situations. An example would be the specific manner in which ordinary citizens had to greet each other during the Third Reich in Germany.⁶ Vendetta (a blood feud) is another example of this type of informal institution, which determines a specific reaction of a family or clan to a capital crime. All of these behavioral traits are embedded in a particular context. These informal institutions have no relevance beyond a specific context. A third type of informal institution (located at the meso level of abstraction) constitutes informal rights systems, systems of corruption or clientelism. These institutions are more specific than the first type, but cover a broader spectrum of action than the second type.

So, informal institutions such as clientelism and corruption incorporate different subtypes of informal institutions, as illustrated in the following. Each of these specific subtypes has a specific meaning. This is illustrated in the case of clientelism, where we can distinguish between (1) kinship, (2) mafia, (3) autocratic cliques and clientelistic parties, and (4) nepotism. In the case of corruption, we can distinguish between two subtypes. The subtypes are based on the different characteristics corruption can assume. In one instance, societal actors try to influence political decisions by offering a material contribution (*bribe*). In another instance, state officials ask citizens to provide financial resources, and, in return, the state official fulfills an administrative task he would otherwise be required to fulfill anyway (*extortion*). In addition to subtypes, it is possible that aggregated institutions—such as clientelism and corruption—form common patterns at a higher level of abstraction. Such informal institutional combinations can extend up to the level of a hidden constitution; they can structure a system of rent-seeking or cronyism, or a neo-patrimonial system.⁷ The degree of abstraction of an institution is determined by the variety of actions the institution enables. The higher the level of abstraction, the greater is the range of actions covered. The scope of different actions shrinks with the declining degree of abstraction. The use of the category of abstraction mirrors the idea of Sartori, who classifies formal institutions along the ladder of abstraction.

Besides their level of abstraction, and similar to the differentiation between democracy and dictatorship, informal institutions can be differentiated along a second normative-based category: by their connection to *civic traditions*. The latter find their expression in the defense of universal rights that are assigned to all human beings. By contrast, non-civic traditions maintain the idea of difference and, with it, particularistic patterns, as O'Donnell (1996) highlights. In general, an ongoing dispute exists between universalism, on the one hand, and particularism, on the other. Trust, solidarity, tolerance, and fairness are basic codes ingrained in long-held civic traditions. The particularistic view is marked by distrust, egoism,⁸ intolerance, or unfairness (at least vis-à-vis unfamiliar and foreign groups). The former is called "civic pool" (CP), the latter "anti-civic pool" (ACP). More specifically defined institutions at lower levels of abstractions can be linked with either one of them.

Beyond the already mentioned informal institutions, one can include additional examples of both the *civic pool* and the *anti-civic pool*. Inside the CP, we find elements such as unwritten constitutional conventions that can be observed in Great Britain. Although this example is widely used in the literature and is viewed as an exceptional example, informal rules (as expressions of an informal constitution) can be observed in other political systems as well. The consociational systems of Austria and Switzerland or coalition presidentialism in Brazil and in Chile come to mind.

At the same time, it is possible to observe a larger number of specific informal rules that affect the legislation and administration of a country. These specific and informal rules coexist productively with formal institutions. They adjust the content of the formal rules that are designed to structure the administrative procedure. The actual legislative power of the American president and a coalition agreement of governmental parties (as in Germany, for example) are two examples among many. Another informal institution in the civic tradition is civil disobedience. This institution is a special case: Although its legitimacy is strongly based on civic values and norms, it is by definition illegal.

A further example is customary law, which is compatible with the rule of law. Certain customs, however, can be incompatible with the principle of the rule of law. Customary law includes all non-codified rules and modes of behavior that the state or private tribunals can sanction. This includes traditional, secular, and religious systems of law, such as Islamic law (Sharia). This encompasses a very broad field with hundreds of various (sub)types. Several of the informal institutions associated with these systems of law are compatible with the rule of law; several are in conflict with the rule of law or contradict their central principles. Folk traditions continue to belong to customary law, the adherence to which can even be enforced in non-state or even partially state-controlled tribunals, as can be observed in various West African countries.⁹

An important new area has developed in the field of business relations, especially at the international level, where conflict arbitration is conducted by private entities. These arbitration mechanisms have created business law, well known in the self-regulation of the banking system (Basel Accords). Informal systems of law can also be observed in neighborhood networks, where these systems regulate specific aspects of social security (self-help networks, saver and migrant clubs, or burial societies). Such networks exist in many developing countries. Self-help networks constitute informal institutions that, in terms of their understanding of the law, correspond to arrangements made under the rule of law (or that imitate this understanding).

Violence as a means of influencing the political process is another tool the ACP offers political actors. For violence to qualify as an informal institution, it need not be applied on a permanent basis. It is sufficient if actors threaten to use violence whenever political decisions are about to be made. As is the case with other institutions, different variants of violence can be identified. They differ in terms of their aims, their *modus operandi*, and their effect. The following situations

Table 5.2 Map of informal institutions

Level of abstraction	Civic pool universalism	Anti-civic pool particularism
High	Trust / solidarity	Distrust / egoism
	Tolerance / fairness	Intolerance / unfairness
Middle	Conventions	Hidden constitution (delegative code)
	Informal parts of constitutions	Customary law in tension with the rule of law
	Customary law compatible with the rule of law	Corruption, clientelism, power threat / threat perception (organized crime)
	Civil disobedience	
Low	Administration rules, representation quotas	Vendetta

can be defined as “identifiable forms of violence-based participation”: the threat of a *coup d’état*—which Valenzuela (1992) refers to in his brilliant discussion of “perverse institutions”—riots and organized crime.

The contrast between CP and ACP institutions in Table 5.2 is somewhat exaggerated. As customary law indicates, certain institutions can be found in both categories. Or, at the very least, some subtypes straddle the zone between both (for example clientelism in the middle or the headscarf at the lower level). It is also debatable whether the basic codes (i.e. institutions with a high level of abstraction) should be understood as institutions. They have external enforcements in place, but these sanctions do not always apply. For this reason, the term “basic codes” seems more appropriate than “institutions” (although neo-institutional approaches refer to them as institutions). To distinguish the world of informal institutions alongside the two dimensions outlined in Table 5.2 is one way in which to classify informal institutions.

Regimes types and informal institutions, developments, and performance

Regime types—like democracies and autocracies—constitute special institutional arrangements, which regulate the access, distribution, and organization of political power. The respective regulations constitute formal institutions, which clearly have a strong impact on citizens (Fishman 1990: 428). In all regime types, formal and informal institutions can have different relationships with each other, as indicated previously.

Scholars examining the relationship between formal and informal institutions should note that these relationships differ regarding the regime type. In democracies, informal institutions such as clientelism and corruption constitute a conflictive relationship. In autocracies they can be classified as complementary, as the study of neo-patrimonial and sultanistic regimes demonstrates. In totalitarian regimes, however, clientelism and corruption oppose formal institutions, as they challenge the control of the ruling class over the political system. The same reasoning applies once we create subtypes of clientelism and corruption, and we relate them to different subtypes of democracy and autocracy. As a result, the relationship between formal and informal institutions is not always the same, but depends on the regime type in place. The same institution can lead to very different outcomes in different government systems.

This finding is particularly relevant for the study of transition processes. Whenever political transitions occur, formal institutions change. As a result, the relationship between formal and informal institutions changes accordingly. Informal institutions, such as clientelism, are central pillars of authoritarian rule. During and after the transition process, the same informal institutions may hamper democratization. Other informal institutions—such as violent intimidation by

organized crime—have the same effects as prior to the transition. The transition phase will also give rise to new informal practices and institutions whose implications are ambivalent.

To know whether informal rules support or hinder democracy requires a nuanced analysis. The impact of informal institutions on democratic institutions—institutions that ensure and regulate the three defining features of democracy, namely freedom, equality, and political and judicial control—is key to the analysis. All of the informal institutions inside the ACP affect democracy in a negative manner. Other informal institutions (such as coalition presidentialism or civil disobedience) can improve the quality of democracy; they can serve as functional equivalents of (distorted) formal participation channels (Lauth 2000). Thus informal institutions do not always diminish the quality of democracy. The impact of informal institutions on democracy varies to a certain degree. As the example of customary law has shown, informal institutions can actually support democratic institutions. Clientelistic participation can express a positive defensive reaction by allowing more popular political participation than in formal democratic institutions controlled by elites.

Thus, the struggle for democracy is not located on the grounds of distinction between formal and informal institutions, but rather inside the informal arena itself. Due to the many ways in which informal political institutions function and the inability of a democracy to escape their influence, it is necessary to emphasize that without the inclusion of informal institutions an analysis of the functioning of a democracy remains incomplete. The same reasoning applies to autocratic regimes.

While the situation is more straightforward when informal institutions undermine democracy,¹⁰ informal institutions that belong to the CP undoubtedly support formal democratic institutions or serve as a safeguard against the rigidity of formal institutions. To understand democratic consolidation processes and to understand how defective democracies work in practice, it is necessary to study informal institutions. The study of fledgling democracies requires an equally thorough analysis of all informal institutions. Empirical research needs to start by identifying the complete set of institutions of any given political system. This task sometimes seems inexhaustible given the complex dynamics of the institutional network that constitutes the regime (for an example, in the case of Russia, see Gelman 2003; Ledeneva 2006). Structuring and analyzing the coexistence of formal and informal institutions is the next research step for which the above presented categories should be helpful; this analysis should provide a better understanding of the dynamics of young, as well as established, democracies.

Are informal institutions more important in autocracies than in democracies, as Köllner (2012) indicates? There is some evidence of this, as most definitions of authoritarian rule refer explicitly to informal rules. Yet we cannot generalize from these definitions, as we still know very little about the role of informal institutions in dictatorships: Empirical findings on informal institutions are rare (Radnitz 2011: 354). However, we can say for sure that analyses of political transformation processes should not be confined only to formal institutions. Successful democratic transformations always also require the transformation of informal institutions. Hybrid regimes and defective democracies constitute additional and important fields of research.

Do political regimes account for the creation and survival of informal institutions? There are various reasons for the rise of informal institutions. Most arguments about their emergence have already been discussed in the neo-institutional debate, which draws attention to differing explanations (Jancsics 2014) that are applicable to informal institutions (examples include reduction of transaction costs, fixed expectations, order and stability, power, and interests). By taking this debate seriously, two principal ways of generation can be distinguished. One refers to a more or less intended process (“invisible hand”) where informal institutions fulfill the realization of common needs (Giordano 2013). Such institutions can arise from informal practices and can also

indicate weak formal institutions. Another way arises intentionally due to specific interests (“visible hand”). Again, there is a broad spectrum of different forms. They can be compatible or even necessary for the democratic process (such as coalition agreements, representation quotas) or can undermine it (such as rules of organized crime). One specific creation of informal institutions is noted by Köllner (2012: 7). They offer attractive alternatives for political elites because they can expand the capacities of the elites to act and react. Informal institutions frequently mean little accountability and transparency. Thus, informal institutions constitute mechanisms through which political elites can sustain their own power and domination. Indeed, elites are interested in the survival of these institutions as well. Besides the interests of actors, mechanisms of path dependency are responsible for the survival of informal institutions. Nevertheless, they change and can be changed. Whether they can successfully be guided depends on the appropriateness of the given mean regarding the specific situation.

It is certain that informal institutions influence the outcomes in political, social, and economic areas. Following the logic of the previous considerations, it is obvious that the outcome differs depending on the type of informal institution, as well as on the constellation and relations with formal institutions. As these change equally in the process of transformations, the outcome does, too, as indicated previously. Likewise, the effects of informal institutions can vary along the different areas (political, social, economic) as—for example—the discussion about corruption can underline (Debiel and Gawrich 2013). For the complex situation of research, the application of clear concepts, types, and classifications is necessary for the stringent analysis. An interesting finding of many studies on informal institutions is that, independent of the variance of the types, all show a common principle: the rule of reciprocity. One could argue on the basis of this finding that informal institutions would promote or even be the basis for interpersonal trust. This is certainly true in many situations, but one should not forget that informal institutions can also be enforced and that many interactions are exclusive. In that sense, interpersonal trust can be supported inside closed groups, but not as an overlapping universal trust, which is appropriate for social capital.

Methodological reflections

Although political science research has increasingly acknowledged the relevance of informal institutions, empirical comparative research still focuses predominantly on formal institutions. A major reason for this is the availability of data. Data on formal institutions is readily available, which makes even qualitative research possible. This is not always the case with informal institutions, where empirical data remains difficult to obtain. Moreover, the study of informal institutions necessitates laborious case studies that require sociological and ethnological research methods. Numerous empirical studies highlight the importance of informal institutions (Ganev 2007; Grzymala-Busse 2010; Thiery 2011). These studies can be combined with studies on informal governance (Christiansen *et al.* 2003; Christiansen and Neuhold 2012).

With a few notable exceptions (Helmke and Levitsky 2006), the study of informal institutions has not yet led to the emergence of theoretical constructs. A typology of informal institutions can provide a point of departure for a new research program on informal institutions. Systematizing empirical findings could also lead to an improved capacity of political actors to control or even steer informal institutions. Scholars engaging in this kind of research should not forget to identify the respective window of opportunity, i.e. the moment when informal institutions could be changed by political actors. It is also well known that political change often requires a long time to manifest itself. Therefore, political strategies should be analyzed over much longer periods of time than is currently the case.

This is not the appropriate time to debate the fundamental causes of stability and change. However, one should note that such a debate must uncover a complex interdependent relationship between the two types of institutions. Formal and informal institutions should be examined as dependent and independent variables. A change in one part of the institutional design has direct implications for the legitimacy, efficiency, and effectiveness of the political system. Formal institutions can experience change through informal institutions. Scholars have long noted that these changes differ according to the particular political context. But scholars must still identify the extent to which change—in the formal set-up of a political regime—affects informal institutions, and how informal institutions are altered by changes within the world of informal institutions that constitute themselves.

Finally, I would like to draw attention to several other methodological and theoretical implications. As outlined, one of the major accomplishments of political science has been the systematic analysis of formal institutions. These analyses have given rise to several classification typologies: presidential vs. parliamentary and consensus vs. majoritarian systems. None of these schemes accounts for informal institutions. Instead, they assume that formal institutions are capable of governing the political system on their own. This assumption is not very plausible. As noted, formal and informal institutions are connected. Informal institutions have a variety of means at their disposal to affect the logic of formal institutions. Hence scholars must examine the empirical validity of conventional political science assumptions to avoid conceptual stretching (Lauth 2013). The study of (formal and informal) institutions requires additional alternative typologies with subtypes (as types of defective democracies). It further requires an awareness of the context-specific effects resulting from the interplay between formal and informal institutions.

Conclusion

Institutions are relevant, because they shape our social order. Political actors follow the content of the rules institutions establish, be they formal or informal rules. Informal institutions are part of every political system; their relationship with formal institutions is difficult to decipher. Predicting the effects of informal institutions becomes easier by classifying informal institutions with the help of typologies and by systematically thinking through their relationships with formal institutions. But this is only a first step. As empirical studies show, the interactions between formal and informal institutions differ from situation to situation. This is true regarding the number of institutions involved but also regarding the extent to which the institutions are capable of influencing the actors involved. Formal and informal institutions filter political action. However, they are only partially able to determine the outcome of a political process (Köllner 2012: 15). To account for these outcomes, we need to consider the preferences, interests, and ideas of the actors shaped by these institutions (and how the actors in turn shape these institutions).

No political system can survive without informal activities. Hodgson (2006: 18) has highlighted this: “To put it differently, legal or ‘formal’ institutions that do not have strong ‘informal’ support are unsupported legislative declarations rather than real institutions. This does not mean that legal rules are unimportant but that they become important by becoming incorporated in custom and habit.” However, this statement does not mean—as his first quotation at the beginning of the chapter suggests—that it is impossible to separate both worlds analytically. A separation between formal and informal institutions is urgently required to examine empirically how both interact. Informal institutions constitute only one part of the informal world, yet a decisive part. It is impossible to understand political processes without including existing informal institutions in the analysis. In some cases, the impact of informal institutions could be small;

in other cases, they can “dominate” the game. Accordingly, an identification of strong and weak institutions should also be part of any institutional analysis (Levitsky and Murillo 2009). The empirical findings further emphasize that informal institutions are not endangered species. Traditional institutions are still part of modern political environments, and new informal institutions continue to emerge. This calls for additional future research into the role of informal institutions.

Notes

- 1 For example, corruption constitutes a simple set of rules. Clientelistic networks are already more complex. Neo-patrimonial states are even more complex and require sophisticated analysis. These types of states constitute a very specific combination of formal and informal rules. To examine neo-patrimonial states, an entire set of formal institutions has to be analyzed—outlined in the constitution of a state—in conjunction with a set of informal institutions.
- 2 This does not mean that internal sanctions have to be absent. They can also exist in the case of formal institutions (not obeying the rule of law can create such internal mechanisms). The meaning here is simple: internal sanctions build no defining characteristic of an informal institution.
- 3 The understanding of “functioning logic” or “rule of identity” is based on the idea that the following criteria are present in a specific form and inner connection. First, they must be recognized by certain symbols or elements that indicate their existence. Second, there are specific forms of interconnection or interaction. Third, each institution is linked with a special purpose, which allows its functional description. These criteria mark clearly identifiable and classifiable action patterns.
- 4 “Perverse elite agreements”—i.e. elite agreements on political rules that do not accord with or even transgress the constitution—supplement the role of other informal institutions. Such agreements allow for elite conduct that leaves transgression of basic constitutional rules unchecked (Thiery 2011: 17).
- 5 “Basic norms” or “basic codes” are effective in the sense of general norms or general dispositions of perception and behavior. They structure the system of interaction of the entire society and its subsystems.
- 6 The example of the Islamic headscarf as a specific institution is problematic. It can not only serve as an expression of fundamentalist conviction, but also as a cultural custom or even a fashion accessory. The numerous meanings attached to these symbols make interpreting them precisely very difficult.
- 7 “Neopatrimonialism is a mixture of two co-existing, partly interwoven, types of domination: namely, patrimonial and legal-rational bureaucratic domination.... Formal structures and rules do exist, although in practice the separation of the private and public sphere is not always observed. In other words, two role systems or logics exist next to each other, the patrimonial of the personal relations, and the legal-rational of the bureaucracy.... The patrimonial penetrates the legal-rational system and twists its logic, functions, and output, but does not take exclusive control over the legal-rational logic. That is, informal politics invades formal institutions. Informality and formality are intimately linked to each other in various ways and by varying degrees; and this mix becomes institutionalized” (Erdmann and Engel 2007: 105).
- 8 To regard “egoism” as part of the anti-civic pool seems surprising because keeping one’s own interests in mind is one of the basic beliefs of the liberal tradition. Free markets rely on many of the views Adam Smith outlined. According to the liberal tradition, however, “egoism” is limited by sympathy for other human beings, while from the perspective of non-civic traditions egoism is unlimited and serves as a synonym for greed.
- 9 The archaic custom of the blood feud (vendetta) can only be included under customary law to a limited extent, as the roles of plaintiff and judge are normally embodied in one person. What can be included more easily, however, are “kangaroo courts” or “mafia courts,” whose basic concern is the violation of an unwritten “code of honor.” Such institutions are to be understood as political ones, not because they can be classified as part of the political system, but rather because through their violation of the exclusive authority of state they combat the central feature of the political system, thus forming at least partial opposition to the state.
- 10 The negative impact of cooperation among various forms of clientelism and corruption on democracy has been explained by O’Donnell (1993: 1359f) in the “brown areas” theorem and through the concept of “delegative code” (1996), which is similar to Croissant and Merkel’s (2004) “illiberal code.” The mafia is another example of a combination of different types of informal institutions (corruption, clientelism, and threat of violence).

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6

EQUILIBRIUM ANALYSIS OF POLITICAL INSTITUTIONS

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Introduction

At their most political, institutions determine who holds power and how power is exercised. Political institutions thus shape the influence and behavior of their participants and, for that very reason, their adoption and design are the subject of political struggle. Formal political theory—the application of game-theoretic methods to political phenomena—develops this basic insight.² Game-theoretically inspired arguments explain the emergence and persistence of political institutions as the equilibrium outcome of strategic interaction among actors who strive to anticipate the implications of the institutional status quo and its conceivable alternatives for both their own and others' welfare.

Formal political theory provides an attractive and general analytical framework for the study of political institutions for several reasons. First, the analysis of political institutions is particularly amenable to formal modeling as institutional rules frequently delineate precisely those elements that are required for a well-defined game-theoretic model: the participants, their available actions and information, and the procedures by which their actions translate into outcomes—like the majority vote, for instance. Depending on whether our central question concerns the consequences of institutions, their emergence and persistence, or their role in equilibrium selection, formal-theoretic analyses of institutions typically proceed in three conceptually distinct ways.³

In its most direct approach to the study of institutions, formal political theory takes institutions as given and examines their implications for the behavior of those governed by them. A key feature of this approach is the recognition that political actors understand that their welfare depends on not only their own actions but also the actions of others, who hold potentially conflicting interests. The analysis of the equilibrium consequences of institutions therefore often leads to predictions that are not immediately obvious from the institutions' formal structure, like the sharp prediction of candidate platform convergence and the entrenchment of two parties in majoritarian electoral systems that I discuss in the next section.

By contrast, when we analyze institutions as equilibria, we are interested in not only the political consequences of institutions but also why, given their consequences, key actors have an incentive to establish and maintain those institutions in the first place. In this more analytically complete view of institutions, both the behavior and the institutions that govern it must jointly constitute an equilibrium—they must be self-enforcing. This approach is especially appropriate

for the analysis of foundational institutions and weakly institutionalized settings. When we study the emergence and persistence of democracy, electoral systems, or the rule of law, key conceptual puzzles concern not only these institutions' consequences but also why, in light of their consequences, political actors abide by them in the absence of a higher authority that could enforce their compliance. This approach to the analysis of institutions thus yields insights into not only their potential benefits but also their limitations and the obstacles to institutional reform. In the section on 'institutions as strategic equilibria' (pp. 73–74), I illustrate these points by contrasting two perspectives on the emergence and survival of democracy, which I respectively refer to as the redistributive and the accountability models of self-enforcing democracy.

The third distinct approach to the study of institutions emphasizes their role in focal coordination and equilibrium selection. In many settings, a game-theoretic analysis predicts the existence of multiple equilibria; that is, more than one pattern of behavior is consistent with strategic rationality in a given setting. Schelling's (1960) focal point effect helps us understand how institutionally embedded cultural expectations can play a decisive role in coordinating every actor on one among several equilibria. Hence, rather than an undesirable indeterminacy, this approach to the study of institutions recognizes the multiplicity of equilibria as a fundamental feature of politics and an opportunity to incorporate historically and culturally shaped expectations into the equilibrium analysis of institutions. I illustrate this approach by discussing the focal role of supreme courts and the act of inauguration in the efficient arbitration of political disputes and transfers of power.

The second and conceptually distinct set of benefits that formal political theory brings to the study of institutions stems from the productive discipline that the construction and analysis of transparent political models bring to theory-building. This discipline takes at least two forms. The first is required by the equilibrium concepts that we employ in the analysis of our models. The Nash equilibrium and its refinements posit that in order for a political scenario to be in equilibrium, at a minimum, no actor can have a unilateral incentive to deviate from it. This steady-state requirement is an appealing "negative" criterion for predicting political outcomes.⁴ That is, while this requirement may admit multiple outcomes as equilibria, each of which may vary in its plausibility, any behavior that it excludes is definitely implausible: A non-equilibrium prediction effectively expects that at least one actor will act against her own interests. Crucially, the Nash equilibrium and its most frequently employed refinements exist for any well-specified game (Nash 1951). That is, we can employ game-theoretic methods to study essentially *any* political setting. Thus we can think of the analytical discipline required by the Nash equilibrium and related equilibrium concepts as a general methodology for theory-building in political science.⁵

The second form of the productive discipline that formal political theory brings to our study of institutions emerges in the process of model-building. Any game-theoretic analysis begins with an explicit description of the relevant political setting: the players, their preferences, and the actions and information available to them. The conceptual process by which these elements are specified entails abstraction and simplification, and, as a result, a trade-off of descriptive realism in favor of analytical tractability and transparency. Rather than a drawback, formal theory views the analysis of transparent, tractable, even if simple models as a key step toward our understanding of more complex political settings. Its by-product is analytical clarity about assumptions, logic, and empirical implications that allows us to draw connections between conceptually related mechanisms in substantively different contexts. A number of such mechanisms, including costly signaling, cheap talk, ultimatum bargaining, as well as commitment, collective action, and principal-agent problems, has become a part of political scientists' conceptual vocabulary. Thus even those political scientists who do not find particular game-theoretic equilibrium concepts

or mathematical modeling appealing nevertheless benefit from the heuristic transparency that formal political analysis brings to our understanding of politics.

I conclude this chapter by discussing how formal political theory contributes to empirical research and the challenges it encounters therein. The interaction between formal modeling and empirical research is essential for the construction of models that both are tractable and capture key aspects of real-world politics. At the same time, game-theoretically inspired modeling contributes to empirical research by clarifying the strategic origins of frequently encountered problems in causal inference, like endogeneity and selection effects. By providing the microfoundations for these inference problems, formal theoretic research helps guide the search for plausible identification strategies and helps us evaluate their external validity.

Equilibrium consequences of institutions

When we study the equilibrium consequences of institutions, we take a particular institution as given and examine how the incentives created by that institution shape the behavior of its participants.⁶ This approach to the analysis of institutions is one of the earliest in historical terms—as in Condorcet’s eighteenth century investigation of voting cycles under majority rule. It is also conceptually antecedent, as we think that the primary driver of institutional reforms (or the lack thereof) is their anticipated consequences. As an illustration of this approach, consider the game-theoretic analysis of two distinctive consequences of majoritarian electoral systems: platform convergence and Duverger’s law.

A canonical model conceives of democracy as a political system in which two candidates compete for the votes of a large number of voters whose policy preferences vary along a single dimension. First, each of the two candidates proposes a policy that he will later implement; then each voter votes for the candidate whose proposed policy is closest to his or her preferred policy. Unlike the voters, the candidates do not care about policy but only about winning office. The candidate who obtains the larger share of the vote wins the election (a tie is decided by the flip of a coin) and implements the policy that he proposed.

This simple model, frequently labeled Hotelling’s or the Downsian model of electoral competition after its early proponents (Hotelling 1929; Downs 1957), yields sharp predictions about the nature of electoral politics. In the unique equilibrium, both candidates propose the policy favored by the median voter and therefore tie. The intuition behind this result builds on the prominent role that the median voter plays in majoritarian decision-making.⁷ Because the median voter evenly divides the electorate, any candidate who adopts a platform other than the one favored by the median voter could be defeated by a competitor who positions himself closer to the median. Only a candidate who proposes the median voter’s favorite policy cannot be defeated in this way: The best that his or her competitor can do is to propose the same median policy and therefore tie the election.⁸

While the Hotelling–Downs model of electoral competition takes the number of candidates as given, the formal analysis of coordination problems faced by voters in majoritarian electoral systems derives Maurice Duverger’s (1959) conjecture that these electoral rules result in the competition of only two candidates or parties as an equilibrium (Riker 1982; Cox 1997). In an electoral system with single-member districts and a single vote per voter (SMD), voters who favor candidates whose perceived popularity ranks at the third or worse place have an incentive to desert these candidates in favor of one of the top two contenders out of fear that their vote will be wasted. In turn, we should observe only two serious contenders in any district. This intuition extends to electoral systems with more than two candidates per district: Cox’s (1997) $M + 1$ law posits that there will be only $M + 1$ serious contenders in a district that elects M candidates (see also Shepsle 1991).

The prediction that SMD electoral rules will result in a contest between only two competitive candidates can thus be viewed as a justification for the assumption of two-candidate competition in the Hotelling–Downs model and as a statement about the relatively high barriers to entry in SMD systems (Myerson 1999b). Meanwhile, the Hotelling–Downs model of electoral competition clarifies why two-candidate majoritarian elections result in the convergence of candidate platforms to the median voter’s preferred policy, and in turn, close election outcomes. Crucially, observe that SMD rules do not expressly prohibit more than two candidates from competing for office, nor do they prevent voters from supporting candidates ranked third or worse. Similarly, the intuition behind platform convergence in the Hotelling–Downs model does not depend on any specific distribution of voters; in particular, we are not assuming that most voters are located close to the median. Rather, it is the anticipation of how SMD rules shape the strategic interaction among voters and candidates—the analysis of the equilibrium consequences of this institution—that accounts for the sharp predictions about the number of candidates and the nature of their platforms.

Institutions as strategic equilibria

According to Douglas North’s oft-cited definition, institutions are the “humanly devised constraints that structure political, economic and social interactions” (North 1991: 97). Thus unlike geographical or resource constraints, institutions can be devised and changed by the very actors whose behavior they are supposed to bind. Our explanations approach this feature of institutions with political realism when we conceive of the emergence and persistence of political institutions as the equilibrium outcome of strategic interaction. Analytically, this amounts to a more coherent view of institutions than the analysis of the equilibrium consequences of institutions alone as it presupposes that the agents who decide to adopt or maintain an institution understand the equilibrium consequences of the status quo and its conceivable alternatives.⁹

As an illustration of this approach, consider two competing perspectives on the emergence and survival of democracy, which I respectively refer to as the redistributive and accountability models of democratization. Building on the implications of the Hotelling–Downs model of electoral competition for redistributive politics (Meltzer and Richards 1981), the democratization models of Acemoglu and Robinson (2001, 2005) and Boix (2003) emphasize that the key political consequence of a transition from dictatorship to democracy is the redistribution of income from the rich to the poor. That is, while redistribution is limited under dictatorship because power is held by the rich, electoral competition under democracy must heed the preferences of the (much poorer) median voter and thus results in significant income redistribution. A majority of the population thus prefers democracy to dictatorship and may threaten a revolution with more radical redistributive consequences if its demands are not met. According to this perspective, stable democracy emerges at moderate levels of economic inequality when a significant fraction of the population demands it but the rich do not fear it so much as to find repressing the redistributive demands of the poor under a dictatorship more attractive.¹⁰

Contrast this reasoning to the accountability view of democracy. According to this perspective, the major difference between dictatorship and democracy is that, in the latter, electoral competition affords the public the opportunity to hold politicians accountable for their performance in office. In turn, democratic elections generate incentives for politicians to promote the general welfare (Barro 1973; Ferejohn 1986), whereas authoritarian elites cater primarily to a much narrower “selectorate” (Bueno de Mesquita *et al.* 2003; Besley and Kudamatsu 2007; Boix and Svolik 2013). Accordingly, in Lizzeri and Persico’s (2004) model of franchise extension, elites

favor democracy when the status quo favors only a small fraction of the elite—the landed classes—and the extension of the franchise promises public-good oriented policies that are key for the welfare of the growing commercial and urban classes. Meanwhile, democracy survives according to the accountability perspective when it indeed delivers policies that are better than those under a dictatorship because only then can voters be realistically expected to defend democracy against politicians or groups with authoritarian ambitions (Weingast 1997; Fearon 2011). In the models of Myerson (2006), Bidner and Francois (2013), and Svolik (2013), elections may fail to deliver accountability when voters come to believe that most politicians are self-serving and any attempt to discriminate among them based on their performance is therefore a waste of time. When espoused by a sizeable fraction of the electorate, such expectations are self-fulfilling: They fuel a mutually reinforcing cycle of voter apathy and poor government performance. Hence, according to this perspective, the key threat to democratic stability is not the redistributive conflict between the rich and the poor but rather the failure of elections as an instrument of accountability.

The analysis of institutions as equilibria is especially valuable for our understanding of the emergence and persistence of foundational institutions, like democracy, electoral systems, or the rule of law, and our understanding of weakly institutionalized settings, including international, post-conflict, or authoritarian politics.¹¹ In these settings, a key conceptual puzzle behind the emergence or persistence of institutions stems from the absence of a higher, independent authority with the power to enforce the compliance of key actors with institutional rules when doing so is against their interests.¹² Thus in their analysis of the emergence and persistence of democracy, both the redistributive and accountability perspectives start by outlining the redistributive and accountability consequences of dictatorship and democracy for key actors: the rich versus the poor and the elites versus the public, respectively. Democracy is an equilibrium—it is self-enforcing (Przeworski 1991)—when no key political actor has an incentive to undermine it in light of its anticipated consequences, whether it be the rich by coopting the military in order to stage a coup or an incumbent by refusing to step down after losing an election.

But in other contexts, it may be more productive to (at least initially) bracket the question of the origins of institutions and focus exclusively on their equilibrium consequences. For instance, while the origins of single-member district electoral systems may be traced back to the proto-democratic institutions of early modern Europe and their colonial dissemination, contemporary persistence of SMD electoral systems may be rooted in the entrenched two-party systems that SMD rules foster (Cox 1997). A shift to an alternative electoral formula may therefore be feasible only when new parties that emerge due to the extension of the franchise or an economic transformation threaten to fragment established parties (Boix 1999). Thus when Cox (1997) studies the coordination dilemmas that electoral systems create for voters and parties, he can reasonably bracket the question of the origin of electoral systems on analytical grounds: An initial analysis may be most tractable if it focuses on the consequences of electoral systems alone. Such a “partial equilibrium” analysis is also warranted on substantive grounds: Electoral rules change only at critical junctures and can therefore be considered both binding and fixed, at least in the short run.

Institutions as focal points

In many political settings, the Nash equilibrium and related solution concepts predict that more than one pattern of behavior is consistent with rational, strategic reasoning—that there are multiple equilibria. In some cases, the indeterminacy associated with a multiplicity of equilibria may be the consequence of an underspecified model and thus only a technical artefact.¹³ But in many

instances, the multiplicity of equilibria is a fundamental feature of politics. In these cases, a major function of institutions can be to focally coordinate their participants on one among the multiple equilibria (Schelling 1960).

As an illustration, consider the focal properties of two institutions, the supreme court and the act of inauguration. The scenario in which the supreme court acts as an arbiter of last resort is only one equilibrium in a broader game in which a legal dispute could also be decided by a costly contest whose outcome depends on the contending parties' brute force. When a supreme court arbitrates a disputed election, for instance, its ruling might be seen as just one among the many opinions typically given by various actors, from the candidates to media pundits to foreign observers. But in a functioning constitutional order, the court's opinion becomes a self-fulfilling prophecy by virtue of its prominent status in the constitutional hierarchy.¹⁴ The court's verdict is self-enforcing because it coordinates the expectations of the multitude of actors on whose consent the proper functioning of any constitutional order depends. Hence a losing candidate who would contemplate ignoring the court's ruling must anticipate opposition from not only the declared winner but also those actors. Its focalness endows the court with the power to preclude costly disputes in spite of the fact that, in strictly material terms, the court's opinion was just that—an opinion.

Similarly, the symbolic act of inauguration marks the transfer of power from one leader to the next by focally coordinating expectations about who heads the executive among the large number of actors that constitutes the hierarchy of any government. One metric of the focal power of this symbolic act is the difference between a departing leader's order on the day before and on the day after the inauguration of his successor. By contrast, in political systems that lack the institution of inauguration or where its relevance is in doubt, genuine transfers of power must be accompanied by a demonstration of force or a costly political conflict. Thus in contemporary Russia, for instance, it has become apparent that the effective head of the Russian government is neither the president of the Russian Federation nor its prime minister. Rather it is Vladimir Putin, regardless of the official post that he holds. As a consequence, Putin's potential successor cannot expect to genuinely assume power by the mere symbolic act of inauguration. He will have to wrestle power away from Putin publicly and personally—by exiling, imprisoning, or killing him.¹⁵

Schelling's (1960) focal point effect thus helps us understand how institutions can become the tools of efficient arbitration of political disputes and transfers of power. These two examples illustrate a broader Humean view of foundational political institutions (Hume 1748 [1987]): Institutions are conventions (rather than contracts) whose key role is to establish shared expectations that coordinate the citizenry on one among multiple (and not equally efficient) self-enforcing ways to organize political interactions.¹⁶ The central role of many constitutional provisions, in this view, is to focally coordinate everyone's expectations on clearly defined limits on the government's authority (Weingast 1997).¹⁷

Because focalness refers to environmental factors that may be conspicuous but do not directly affect the participants' welfare, focal coordination allows for the incorporation of cultural, psychological, and historically contingent factors into the rational choice analysis of institutions.¹⁸ According to this approach to the analysis of institutions, many of the symbols and ceremonial acts that accompany real-world institutions are seen as cultural artifacts that serve to reproduce a shared understanding of the focal role that specific institutions play in equilibrium selection (Chwe 2001). But this does not imply that cultural or symbolic factors override strategic concerns. Only when multiple patterns of behavior are consistent with strategic reasoning can institutionally embedded cultural expectations play a decisive, focal role in equilibrium selection.

The heuristic value of formal models

The first step of a game-theoretic analysis of politics is to explicitly describe the relevant political setting: the players, their preferences, and each player's available actions and information. Political institutions are especially amenable to this initial step since their infrastructure often mirrors many of these elements. Nonetheless, the make-up and operation of most real-world institutions are too complex to be modeled in their entirety. In turn, the conceptual process by which these elements are specified entails abstraction and simplification, steps that require a trade-off of descriptive realism in favor of analytical tractability and transparency.

Rather than a drawback, game-theoretically inspired theory-building views the analysis of simple, tractable models as a necessary and productive first step toward the understanding of more complex environments. The abstraction and simplification involved in formal theory-building foster a *productive discipline* that is essential to any kind of theory-building, both mathematical and less formal.¹⁹ The specification of the elements of a well-defined game amounts to a statement of the key forces that the analyst believes operate in the political setting under study. Meanwhile, the resulting analytical tractability and transparency facilitate the reproducibility of theoretical arguments (just like data and code sharing facilitate replication in empirical political science) and make it easier to draw connections between analytically related mechanisms in substantively different fields. Thus even those political scientists who are skeptical about the plausibility of game-theoretic equilibrium concepts will benefit from the productive discipline required by the methodology of model-based theorizing and the ensuing heuristic transparency.

As an example of a productive departure from descriptive accuracy, consider the ultimatum bargaining model. According to this model, one of two players first proposes how to divide a positive quantity (money in the simplest application) and then the other player either accepts or rejects the proposal. If the proposal is accepted, each player gets his or her share of the proposed division; if it is rejected, both players get nothing. This simplest model of sequential bargaining results in a sharp equilibrium prediction: Because the second player is effectively choosing between accepting the first player's proposal or getting nothing, the first player optimally demands and obtains the entire quantity divided; no rejections occur in equilibrium.²⁰ Hence the alternate name for this model: "take-it-or-leave-it" bargaining.

This seemingly unrealistic model of bargaining has become one of the key building blocks for the formal analysis of a range of political institutions.²¹ In their analysis of budgetary agenda-setting, Romer and Rosenthal (1978) reformulated the ultimatum game by conceiving of the first player as a committee that proposes a policy (e.g. a school board) and of the second player as a collective actor that decides by a majority vote (e.g. a referendum) whether to accept the committee's proposal or whether to retain the status quo. This extension of the ultimatum game became the workhorse model in the study of legislative organization (Denzau and Mackay 1983) and the separation of powers, especially the study of executive-legislative bargaining (Cameron 2000). Meanwhile, an extension of the ultimatum game according to which a rejection of the first player's proposal results in a costly conflict became the leading approach to the study of war (Fearon 1995; Powell 1999). In political applications of the ultimatum game, the initial trade-off of descriptive realism in favor of analytical tractability proved to be essential in facilitating the development of more realistic models in which conflict may occur in equilibrium (typically as a result of private information) and in which the power of key actors is shaped either by the political environment (e.g. the location of the status quo) or by the details of the institutional setting (e.g. open versus closed committee rules, the requirements for a veto override.)

Political applications of the ultimatum game also illustrate the heuristic value of our thorough understanding of this mechanism: The tractability and transparency of these models help us

discern analytical connections between areas as disparate as the committee system in the U.S. House of Representatives, the separation of powers, and international crisis bargaining. A highly incomplete sample of other mechanisms frequently encountered in the formal analysis of institutions includes the classic dilemmas (i.e. the Prisoner's Dilemma, the Stag Hunt, the Game of Chicken, the Battle of the Sexes, the Matching Pennies), the problems of commitment and non-credible threats, collective action problems, principal-agent problems, costly signaling, and cheap talk.²² As in the case of the ultimatum game, these mechanisms and the intuitions behind them have been productively applied in issue areas well beyond those in which they initially emerged.

Conclusion: the symbiosis of theoretical modeling and empirical testing

I conclude this chapter by discussing how formal analysis of institutions benefits from and contributes to empirical research. While formal political modeling is primarily a theoretical enterprise, its interaction with empirical research is essential for the construction of models that capture key aspects of real-world politics.²³ At the same time, the empirical evaluation of formal models presents unique challenges: Models are at best approximations of reality and good models are deliberate about the many aspects of reality that they ignore and the few that they focus on. In turn, it is easy to dismiss any formal model as failing to capture some aspect of real-world politics. But that would miss the key purpose of modeling: to offer a tractable analysis of some aspect of politics. To paraphrase a famous quotation, since all models are approximations at best, all models are wrong—but some are useful (Box and Draper 1987: 424).

Any kind of theory-building, whether mathematical or less formal, entails abstraction and simplification and hence a departure from descriptive realism. Formal theorists view these aspects of modeling as a productive step toward the construction of models that serve a specific purpose, which is as often analytical or heuristic as it is to yield testable empirical predictions. This approach to modeling as a multi-purpose enterprise is more productive than attempts to construct a single, complex model that would be at the same time analytically tractable, theoretically revealing, and yield a wealth of concrete empirical predictions (Myerson 1992: 64–66). The value of specific models should therefore not be judged by their descriptive details or even by the richness or precision of their empirical predictions, but rather by whether they accomplish their intended purpose.²⁴

In turn, when assessing the predictions of formal models, we should focus on the key forces hypothesized to shape equilibrium behavior, test a model's comparative statistics rather than point predictions, and evaluate its goodness of fit by the standard of alternative, competing models. Meanwhile, when evaluating the appropriateness of modeling assumptions, we need to differentiate between models whose emphasis is analytical and therefore have only crude empirical implications and models whose emphasis is to highlight their predictions for a specific type of data or identification strategy. Thus while Schelling's (1960, 1966) and Fearon's (1995) models of commitment and bargaining problems in international crises are primarily analytical and heuristic, Signorino's (1999) and Lewis and Schultz's (2003) models highlight the implications of these mechanisms for statistical inference from international conflict data.

The evolution of models of electoral competition illustrates the productive interaction of theoretical modeling and empirical analysis. When viewed in strictly empirical terms, the predictions of the Hotelling–Downs model discussed in the section on “Equilibrium consequences of institutions” (pp. 72–73) are obviously “wrong”: Candidates never propose perfectly identical platforms and there are no perfectly tied large-turnout elections on record. Attempts to bring the model's prediction closer to empirical observations led to extensions that capture richer political

settings and, in turn, more realistic predictions. Some of the key extensions include models in which candidates care about policy (Wittman 1973; Calvert 1985), cannot commit to platforms (Osborne and Slivinski 1996; Besley and Coate 1997), compete over multiple dimensions (Roemer 2001), face the threat of entry (Palfrey 1984), differ in platform-specific skills (Krasa and Polborn 2010) or valence characteristics (Ansolabehere and James M. Snyder Jr., 2000; Groseclose 2001; Aragonese and Palfrey 2002); and models in which voters experience a random shock to their preferences before they vote (Lindbeck and Weibull 1987; Coughlin 1992; Persson and Tabellini 2000), form parties (Roemer 2001), become candidates themselves (Osborne and Slivinski 1996; Besley and Coate 1997), and may choose to abstain (Myerson 1998, 2000).²⁵

These extensions help clarify what kind of departures from the initial Hotelling–Downs setting result in equilibrium platform divergence and election outcomes that are not (ex-post) perfectly tied. Yet, at the same time, these extensions also confirm that competitive pressures toward platform convergence are a general feature of majoritarian electoral systems. Thus, once confronted with data, the Hotelling–Downs model proved to be a theoretically productive starting point.

Game-theoretically inspired modeling not only benefits from interaction with empirical research, it also contributes to it by highlighting the challenges to statistical inference and external validity. By emphasizing that most political interactions involve actors with potentially conflicting interests, formal analysis provides the microfoundations for recurring problems in empirical inference, like endogeneity and selection effects.

Consider one major obstacle in the estimation of the causal effects of institutions—their endogeneity. Endogeneity refers to the concern that the presumed consequences of institutions are simultaneously the cause of their emergence and persistence. According to the most skeptical view, institutions are epiphenomenal—their presumed consequences are entirely due to the material conditions that are responsible for their emergence and persistence.²⁶ This concern about the endogeneity of presumed causes to their effects mirrors the conceptual distinction between the partial analysis of the equilibrium consequences of institutions and the full analysis of institutions as equilibria. In fact, a frequently employed nomenclature distinguishes these as the exogenous and endogenous approaches to the study of institutions (Weingast 1998; Diermeier and Krehbiel 2003).

In turn, the challenge in identifying the consequences of institutions is to isolate the causal effect of an institution from the indirect effects of factors that contribute to its persistence. As our earlier discussion of Duverger’s law highlights, by favoring two-candidate competition majoritarian electoral systems generate vested interests in the perpetuation of such electoral rules, and, in turn, further the entrenchment of both the electoral system and two major parties. As a result, the causes of these institutions can be plausibly separated from their consequences only at particularly favorable historical moments. Electoral systems, for instance, may be plausibly exogenous only at the time of their colonial dissemination or when new parties threaten established ones due to unexpected demographic or economic shocks.²⁷ Models of Duverger’s law thus provide the analytical microfoundations for institutional path-dependence and highlight the challenges to empirical inference about the consequences of electoral systems.

Finally, explicit—but not necessarily formal—theory-building can help empirical researchers evaluate the external validity of specific cases and research designs. Most empirical research aspires to claim that its findings are relevant beyond the specific case, context, or data from which it originates. But claims of such external validity then amount to a conceptual justification for why some political mechanism operates similarly across different contexts. Explicit modeling of the interaction between institutions and their participants helps us evaluate which features of an empirical case or data are relevant to the mechanism at work and thus generalizable beyond the specifics of any context.

Notes

- 1 I would like to thank Jose Cheibub, Bonnie Weir, Matt Winters, and the editors for insightful comments and conversations, and Abdullah Aydogan, Tyler Pack, Hollan Peterson, Ashlea Rundlett, Kelly Senters, and Wei Zhong for editorial and research assistance.
- 2 For an introduction to formal political theory, see McCarty and Meirowitz (2007), Morrow (1994), and Osborne (2004).
- 3 For complementary perspectives on formal analysis of institutions, see Weingast (1998) and Shepsle (2006).
- 4 Myerson (1991: 107–108) refers to this “negative” predictive plausibility of the Nash equilibrium as an “upper solution concept.”
- 5 For a discussion of the value and limits of game-theoretic analysis in political science and related social sciences, see Aumann (1985), Geddes (2003: [Ch. 5](#)), Kreps (1990), Morton (1999), Myerson (1992, 1999a, 2009), Powell (1999: [Ch. 1](#)), Rubinstein (1991), and Tsebelis (1990: [Ch. 2](#)).
- 6 In an influential paper, Shepsle (1979) refers to this type of institutional analysis as “structure-induced equilibrium.”
- 7 The median voter has exactly one half of the electorate to both his or her left and right when the electorate is ordered along a single policy dimension. Black’s Median Voter Theorem (Black 1958) states that the ideal point of the median voter is a Condorcet winner—it is preferred by a majority to any other platform—if preferences are single-peaked. See Myerson (2013) for a recent review of social choice theory.
- 8 For an introductory formal exposition, see Osborne (2004: 73), Gehlbach (2013: 2–3), and McCarty and Meirowitz (2007: 101–103).
- 9 For early appeals to treat institutions as equilibria, see Riker (1980), Shepsle (1986), and Calvert (1995a).
- 10 This prediction is based on the models in Acemoglu and Robinson (2001, 2005); Boix (2003) predicts that democracy and inequality should be negatively correlated.
- 11 On electoral systems, see Boix (1999); on the rule of law, see Weingast (1997) and Hafer (2006); on international institutions, see Koremenos, Lipson, and Snidal (2004); on authoritarian institutions, see Gandhi (2008) and Boix and Svolik (2013).
- 12 The relevance of this conceptual point to foundational political questions is eloquently captured in James Madison’s “In framing a government which is to be administered by men over men, the great difficulty lies in this: You must first enable the government to control the governed; and in the next place, oblige it to control itself” (Madison 1788 [2010]).
- 13 The global game reformulation of the Stag Hunt game, for instance, yields a unique equilibrium, whereas the original Stag Hunt has multiple; on global games, see Carlsson and van Damme (1993) and Morris and Shin (2003).
- 14 On the role of law in focal coordination, see McAdams (2000).
- 15 Similarly, the last Mexican caudillo Plutarco Elías Calles officially stepped down from the Mexican presidency in 1928 but nevertheless overshadowed his three successors, inspiring the expression “the president lives in the presidential palace, but the man who gives orders lives across the street” (Krauze 1997: 430). The genuineness of his political demise during Lázaro Cárdenas’s presidency had to be marked by a forced exile to the United States.
- 16 See especially the discussions by Calvert (1995b), Hardin (1989), and Myerson (2004, 2009).
- 17 For related arguments, see de Figueiredo and Weingast (2005) and Tucker (2007).
- 18 Greif (2006: [Ch. 9](#)) discusses the focal role of cultural beliefs in medieval institutional development; Elster (2004: 176–177) suggests that focalness played a substantial role in the forming of a consensus about what “just” compensation was for forced laborers during World War II; Elster, Offe, and Preuss (1998: 62) propose that past constitutional choices become natural focal points in constitutional design during transitions to democracy.
- 19 Put differently, non-formal theories implicitly assume most of the elements required for a well-defined game model but they are typically less transparent about them; see Epstein (2008).
- 20 For an introductory exposition of this model, see McCarty and Meirowitz (2007: 176–177) and Osborne (2004: 181–182).
- 21 In fact, when ordinary people play this game, they are rarely as self-regarding as the basic ultimatum bargaining model assumes (Camerer and Thaler 1995).
- 22 On the role of commitment problems in the analysis of institutions, see North and Weingast (1989), Greif, Milgrom, and Weingast (1994) and Acemoglu and Robinson (2005); on the role of institutions in resolving collective action problems, see Olson (1965), Hardin (1982), Ostrom (1990), and Aldrich

- (1995); on principal–agent treatments of electoral competition and bureaucracy, see Ferejohn (1986) and Epstein and O’Halloran (1999); for institutional applications of costly signaling and cheap talk, see Banks (1991), Gilligan and Krehbiel (1987), and Krishna and Morgan (2001). For a rigorous introduction and more comprehensive review of these topics, see Austen-Smith and Banks (1999, 2005), Gehlbach (2013), Mueller (2003), and Persson and Tabellini (2000).
- 23 Myerson (1992: 64–66) refers to the iterated interaction between theoretical modeling and empirical evidence as the “modeling dialogue.”
- 24 On empirical evaluation of formal models, see especially Myerson (1992), Morton (1999), Granato, Lo, and Wong (2010), Clarke and Primo (2012), and Lorentzen, Fravel, and Paine (2013). For skeptical perspectives on whether game-theoretic models should be empirically evaluated in the first place, see Aumann (1985) and Rubinstein (2006).
- 25 For excellent exposition and review of these developments, see Osborne (1995), Persson and Tabellini (2000), Grofman (2004), and Austen-Smith and Banks (2005: Chs. 7–9).
- 26 For a discussion of this skeptical view, see Przeworski (2009) and Pepinsky (forthcoming).
- 27 For an influential attempt to overcome institutional endogeneity via an instrumental variable approach, see Acemoglu, Johnson, and Robinson (2001).

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USING FIELD EXPERIMENTS TO STUDY POLITICAL INSTITUTIONS

Guy Grossman and Laura Paler¹

Political scientists are increasingly using experiments to study the relationship between institutions and political and economic outcomes. Institutions are the “rules and procedures that structure social interaction by constraining and enabling actors’ behavior” (Helmke and Levitsky 2006: 5). During more than two decades of renewed interest in institutions in political science, researchers have sought answers to broad questions, like: How do institutions affect outcomes such as growth and development, participation, accountability, and policy selection? Which institutions, and what elements of institutional design, matter for these outcomes? How do formal institutions interact with informal institutions? How can weak political institutions be strengthened? And what are the causes of institutional change?

The interest in using experiments to address such questions reflects an enduring concern with causal inference in the institutions literature (Frye 2012). Early scholarly work encountered limited success in dealing with the identification issues that arise because the causes and effects of institutions are highly endogenous. This motivated a large empirical literature that used instruments to exploit exogenous variation and isolate the causal effects of institutions (Acemoglu, Johnson and Robinson 2001). Yet, it is notoriously difficult to find instruments that meet the requirements for unbiased inference (Harrison and List 2004). It is also far from clear whether instrumental variables approaches identify a quantity or population of theoretical interest (Deaton 2010). Moreover, many of these studies employed cross-national data and composite indices of institutional quality (such as Polity IV and Freedom House), which induce measurement problems and compromise the ability to identify the effects of any single institution (Pande and Udry 2006).

Randomized experiments offer one of the most promising approaches available for addressing the causal inference problem in research on institutions. One defining feature of experiments—whether field, lab, or survey—is that the researcher randomly assigns units in the target population to a “treatment” group, which receives a particular intervention, and a “control” group that does not, or that receives a different version of the intervention.² With enough units, random assignment achieves, in expectation, balance across the groups on all pre-treatment covariates, making the control group a suitable counterfactual for the treatment group. Thus, differences in outcomes across the groups can be attributed solely to the effect of the treatment itself—eliminating concerns about reverse causality and spurious causation (Gerber and Green 2012).

A second defining feature of a randomized experiment is that the researcher has some degree of control over the treatment itself. This makes it possible to study the effects of introducing new institutions or altering specific aspects of institutional design at the micro level. Precisely because they offer a high degree of control, lab experiments have proven a valuable means for investigating how institutions affect political behavior and social preferences (Palfrey 2009; Levitt and List 2007). Only recently have researchers started to experiment with institutions in field settings, inducing random variation into the nature of democratic institutions, decision-making rules, whether marginalized groups get reserved seats, and whether and how office holders are monitored.

The main goal of this chapter is to assess how field experiments contribute to the study of political institutions in comparative politics, with special emphasis on experiments conducted in developing countries.³ In this chapter we primarily consider experiments related to those political institutions that determine the extent to which citizens can control their leaders and the political process.⁴ We focus on field experiments as an increasingly popular research approach that has exciting potential yet notable limitations. While much has already been written about the strengths and weaknesses of field experiments in general (Harrison and List 2004; Humphreys and Weinstein 2009), we begin by discussing how these apply to research on institutions in particular. We then review a number of experiments loosely grouped into three categories: experiments that study the effects of introducing new democratic institutions; that investigate the design of democratic institutions; and that aim to make weak democratic institutions work better.

We show that field experiments are becoming increasingly central to research on institutions, contributing meaningfully to both theory-testing and theory-building. They are particularly promising for knowledge accumulation because they make it possible to vary a number of different features of institutional design in different real-world contexts. Yet, it is also true that many field experiments on institutions to date have taken place at the local level. This raises important concerns about the extent to which they can answer questions about the national institutions—such as democracy, decentralization, the rule-of-law—that political scientists often care about. The fact remains that, practically speaking, it is often more challenging to use field experiments to study institutions than to study individual political behavior such as turnout and vote choice (Gerber and Green 2000). Obtaining a subject pool (Harrison and List 2004). Lab-in-the-field experiments respond to this critique by selecting subjects who better represent the population of interest and bringing the lab to their natural environment. For example, Grossman and Baldassarri (2012) conducted a lab-in-the-field experiment among villagers in Uganda to shed further light on how electoral institutions mediate cooperation. The authors used a series of public goods games in which subjects were randomly assigned to conditions in which a central authority capable of punishing defection was either elected by subjects or imposed exogenously. The results show that, holding incentives constant, subjects contributed more when they elected their authority. Like Dal Bo, Foster and Putterman (2010), Grossman and Baldassarri (2012) helped demonstrate that the political process by which institutions are put in place has an independent effect on behavior. As such, these experiments offer an important insight for the “new institutionalist” literature, which has primarily focused on the effects of the incentives and constraints that institutions generate (Levitsky and Murillo 2009).

Yet, lab-in-the-field experiments are subject to much of the same criticism that accompanies lab experiments. Lab-in-the-field experiments can also unnaturally influence behavior, for instance by introducing lab-specific ethical considerations or a high level of scrutiny of one’s actions (Levitt and List 2009). This points to naturalism as the main appeal of field experiments.

Yet, field experiments also have a number of limitations that raise questions about the extent to which they can answer important questions about institutions.

One limitation of field vis-à-vis lab experiments is that the researcher cannot generally achieve the same degree of control over the treatment, not to mention underlying parameters. This, in turn, can affect the extent to which the experiment addresses questions of theoretical import as well as the ability to adjudicate between similar experiments conducted in different locations that produce contrasting results. In implementing a field experiment, the researcher typically collaborates with a governmental or nongovernmental partner. While such collaborations are essential and beneficial—and also mean that the experimental results have a ready policy audience—it is not always feasible or desirable to test specific institutional variations.⁵ In a field experiment, the researcher also often has less control over implementation, which can compromise implementation (Chong *et al.* 2011) or lead to unintended spillover between treatment and control units (Ichino and Schündeln 2012).

Because randomizing institutions is complex and cumbersome, and because the need for statistical power makes it difficult to randomize institutions at high levels of government, many field experiments on institutions have taken place at the local (e.g. village) level. This raises important external validity questions about the extent to which results generalize to the big, high-level institutions (e.g. democracy, decentralization, rule-of-law) that political scientists care about. On the one hand, field experiments can play an important role in uncovering the micro-foundations of more macro theories. Moreover, as Banerjee and Duflo (2011) the necessary statistical power and implementation capacity to randomize institutions often requires overcoming substantial hurdles to obtain resources and build durable relations with donor or governmental partners. We argue that expanding the use of field experiments to study institutions in a way that can address challenges of external validity and scale will likely require developing new models for organizing and conducting research.

Why use field experiments to study institutions?

Field experiments address many of the limitations in observational research with their focus on identifying the causal effects of specific institutions, or comparing different institutions or aspects of institutional design. A field experiment involves the implementation of a randomized intervention in a naturalistic way, meaning: in a natural environment, with naturally affected subjects, and where outcomes in the study resemble those in the real world. As such, field experiments strive not only for a high degree of internal validity (unbiased causal inference within the context of the experiment itself) but also ecological validity (the experiment closely approximates a real-world causal process (Morton and Williams 2010)).

In emphasizing ecological validity, field experiments differ from lab experiments, which have also been used widely to study the causes and effects of institutions. Lab experiments—in which subjects (oftentimes students) are recruited and brought to a central location—are appealing because they give researchers a high degree of control over the treatment and underlying parameters. They have been used to test how the incentives created by institutions affect outcomes like turnout (Levine and Palfrey 2007), voting (Duffy and Tavits 2008), and cooperative behavior (Ostrom, Walker and Gardner 1992). As Morton and Williams (2010) note, lab experiments are particularly useful for testing sharp theoretical predictions from formal theories or the effects of new or rare institutions like innovative voting rules (for example Casella, Gelman and Palfrey (2006)).

Dal Bo, Foster and Putterman (2010) provide one important example of the contribution lab experiments can make to knowledge on institutions. The authors tested in the lab whether the

effect of a policy on cooperation varies depending on whether that policy is selected democratically or imposed externally. Subjects participated in several rounds of a prisoners' dilemma game in which they elected whether to enact a fine for defection. In a random set of cases, the researchers overrode the majority vote of subjects and imposed the fine. The authors found that selecting the fine democratically caused a 40 percent increase in cooperation, demonstrating that the effect of the fine on behavior depended on how that policy was instituted. Moreover, the experimental design enabled the authors to parse out the impact of how the policy was instituted from selection effects (the probability that those who support a policy will be differentially affected by it).

While lab experiments have generated valuable insights, they are often criticized for not predicting behavior in the "real world" because they lack naturalism in the setting, stakes, argue, high-level institutions (what they call "INSTITUTIONS") consist of myriad lower-level institutions; identifying how to improve institutional design or performance on the margins is thus highly relevant. Critics, on the other hand, argue that more work is needed to explicate and test the conditions under which the results from these studies will hold.

Overall, issues of control and external validity are important to determining the extent to which field experiments are answering important questions in the institutions literature. In the next section we review several field experiments on democratic institutions to evaluate their contribution in light of the strengths and weaknesses described here.

The growing experimental literature on political institutions

There is oftentimes a gap between how institutions work on paper and in practice (Helmke and Levitsky 2006). This is particularly obvious in the functioning of democratic institutions in developing countries, where the quality of governance—according to metrics such as corruption and public service provision—is often low. Bad governance is widely attributed to weak accountability, or citizens' lack of will or ability to select good leaders or exercise control over bad ones (Besley 2006). Political failure can arise from a number of causes, including ingrained power structures, information asymmetries between citizens and representatives, clientelistic voting, or outright electoral malfeasance. Others have argued that institutions are difficult to change in the short term and through outside intervention (Easterly 2006). In light of these obstacles to good governance, researchers are using experiments to grapple with questions like: Do new (externally imposed) democratic institutions affect governance? How does the design of democratic institutions affect such outcomes? And how can weak democratic institutions be strengthened? This section reviews recent field experiments that address each of these broad questions.

Introducing new democratic institutions

To what extent does introducing new democratic institutions facilitate development, political engagement, and accountability? A growing body of research has aimed to shed light on this question through field experiments conducted in the context of community-driven-development/reconstruction (CDD/R) programs. While specifics vary across projects, CDD/R interventions typically involve a donor giving block aid grants to villages while requiring recipient communities to establish participatory and transparent institutions that enable all community members to have a voice in the decision over how the funds will be allocated for development. By coupling development aid with democratic institutions, CDD/R programs typically aim to improve economic welfare, governance, and social cohesion at the community level. In the past decade,

development organizations have increasingly adopted CDD/R projects as a model for delivering assistance at the local level.⁶

The CDD/R model is based on two key assumptions. The first is that new participatory institutions can increase leader responsiveness to community members and reduce elite capture (Platteau and Abraham 2002). A corollary assumption is that introducing new institutions is preferable to working through existing institutions if there are entrenched local power structures (Mansuri and Rao 2013). Second, it is assumed that introducing new institutions in one development project will have an impact on village governance more broadly by demonstrating the benefits to community members of more inclusive decision-making. Yet, the political science literature mentioned challenges these assumptions in arguing that it might be ineffectual to impose institutions externally, particularly in contexts with strong informal institutions or uneven power structures. The widespread adoption of the CDD/R model thus offers a unique opportunity to contribute to the study of political institutions since the model is predicated on core debates in the institutions literature.

Two recent field experiments involving CDD/R programs in Sierra Leone (Casey, Glennerster and Miguel 2012) and the Democratic Republic of Congo (DRC) (Humphreys, de la Sierra and van der Windt 2013) shed light on this debate.⁷ In the GoBifo project in Sierra Leone, researchers worked with the Ministry of Internal Affairs to assign randomly 236 communities to a treatment group that participated in a standard CDD/R program and a control group that received no aid. In the Tuungane program in the DRC, 1,250 conflict-affected villages were similarly grouped into 280 treatment and control communities.

Interestingly, neither study finds that the CDD/R intervention caused higher levels of participation, accountability, or social cohesion in treatment communities. In Sierra Leone, the authors observe that the CDD/R program improved welfare by some metrics but had no effect on governance and social cohesion. For instance, women were no more likely to attend or voice an opinion at community meetings, despite their involvement in various CDD activities. Similarly, in the DRC both survey data and behavioral measures reveal little effect of the Tuungane program. Overall, both of these experiments were very well done but neither finds evidence that the program reshaped village institutions, empowered marginalized populations, or improved collective action capacity.

These results not only have important implications for the design of future development programs but also contribute to the academic literature on institutions. Both studies suggest that exogenously introducing new participatory institutions has little effect on local governance and social cohesion in the short run. This lends support to the idea that local governance institutions are resilient to attempts at alteration by external actors. Interestingly, however, the evidence also suggests that these results might reflect the fact that levels of general participation, public information, and equity in decision-making within communities were relatively high to begin with.

Future CDD/R field experiments could advance the institutions literature through a number of promising research directions. These include studying the effects on development of institutions at different levels of government, which could be addressed by varying the level at which the CDD/R intervention is implemented (e.g. village or district). Another option is to investigate the effects of paying taxes (or making financial contributions) on engagement and accountability by varying whether recipient communities are required to make financial or in-kind contributions in order to receive aid. Alternatively, future research could isolate the effect of introducing new institutions by implementing the aid and institutional components of the treatment separately, as Blimpo and Evans (2011) did in a study on education administration in Gambia.

The design of democratic institutions

How do differences in the design of democratic institutions affect policy outcomes as well as satisfaction with the overall political process? This section reviews experiments that help to answer this question by testing how direct versus representative democracy affects the allocation of resources and the impact of reserving seats for marginalized groups on various attitudinal outcomes.

Direct versus representative democracy

The political science literature has long been concerned with the effects of direct versus representative democracy on policy outcomes and satisfaction with the political process. As Olken (2010: 243) summarizes, there are two possible advantages of direct democracy. First, it may allow voters to circumvent unresponsive institutions. Second, inclusionary and participatory decision-making rules enhance the legitimacy of political decisions, regardless of the decisions themselves (Grossman and Baldassarri 2012). In observational settings, the rules that govern policy or project selection are chosen endogenously, complicating the testing of these predictions. Field experiments that randomly assign decision rules are thus uniquely positioned to evaluate the impact of such political institutions.

Recent field experiments in Afghanistan and Indonesia were designed explicitly to test these propositions, again in the context of CDD/R interventions. Beath, Christia and Enikolopov (2013a) conducted a field experiment in 250 Afghan villages participating in the National Solidarity Programme. Half of the study villages were randomly assigned to select development projects at village meetings convened by councils, mirroring traditional decision-making. The remaining villages were randomly assigned to select projects by secret-ballot referenda, giving villagers the opportunity to vote directly for their preferred project. The authors used surveys to elicit *ex-ante* preferences to isolate the effects of direct democracy on allocation and, specifically, the ability of elites to realize their preferences. The authors find that elites' preferences only matter when selection occurs through the village council. This indicates that direct democracy limited elite influence over resource allocation.

Similarly, Olken (2010) conducted a field experiment in 49 Indonesian villages to study how different political institutions govern the selection of local public goods in the context of a national CDD program. Each village followed a political process that resulted in two infrastructure proposals, one "general project" proposed by the village at large and one "women's project" proposed exclusively by women in the village. As in Beath, Christia and Enikolopov (2013a), half of the study villages were randomly assigned to select projects through local assemblies while the remaining villages selected projects via plebiscites. Olken (2010) finds relatively little impact of the plebiscite treatment on the general project. Yet it had a notable impact on the women's project, resulting in the location of projects to poorer areas of the village.

Broadening representation for marginalized groups

Another recent set of experiments focuses on the effects of ensuring that marginalized groups, such as women and minorities, are represented in decision-making bodies by introducing institutional reform in the form of quotas.

Importantly, legal restrictions are usually not the cause of wide gender disparities in political representation. Instead, in both rich and poor countries, women's access to public office at local

and national levels is restricted by voter bias in favor of male politicians (Inglehart and Norris 2003). On the one hand, scholars argue that the bias against electing women for public office is difficult to change in the short term (Inglehart and Norris 2003) and that mandated quotas are likely to spark a backlash if they are seen as violating voters' sense of identity and restricting their vote choice (Rudman and Fairchild 2004). On the other hand, supporters argue that quotas can improve the perceived effectiveness of leaders of marginalized groups through demonstration effects. For instance, electing women to power can weaken stereotypes about gender roles in the public and domestic spheres and eliminate the negative bias in the perceived effectiveness of female leaders among men and women alike.

The field experiment in Afghanistan conducted by Beath, Christia and Enikolopov (2013b) also sought to identify the causal effects of reforming the rules of representation by introducing gender quotas at the community level. In half of 500 villages, the research team introduced institutional rules that promoted gender equality by establishing a gender-balanced village development council; requiring equal participation of men and women in elections and project selection; and mandating that at least one selected project was prioritized by women. Using survey data from 13,000 respondents, the study found that these new rules had little effect on intra-family decision-making. Gender quotas, however, increased female participation in village governance and community life while also increasing support for female participation in village decision-making. These results mirror evidence from a recent policy experiment that exploits the random assignment of gender quotas across Indian village councils. Here Beaman *et al.* (2012) find that the likelihood that a woman speaks in a village meeting increases by 25 percent when the local political leader position has been reserved for a woman. Also examining the Indian case, Bhavani (2009) finds that even after quotas have been removed female candidates are more likely to run and win elected local political positions.

All in all, experimental research on gender quotas suggests that, though social norms are resilient, institutional reforms can induce meaningful attitudinal and behavioral change (see also Paluck and Shepherd (2012)). Specifically, formal (quota) institutions that increase women's representation are able to get women *constituents* to participate more meaningfully in local governance. This is particularly interesting in light of the null results reported by CDD/R experiments testing the effect of introducing new democratic institutions at the village level. Existing evidence thus suggests that giving women a formal designated role in village decision-making as in Beath, Christia and Enikolopov (2013b) produces a stronger effect than encouraging broader participation more generally as in Casey, Glennerster and Miguel (2012) and Fearon, Humphreys and Weinstein (2011). We suspect that this may be because gender quotas, which are designed to affect prior perceptions of women as leaders, are less threatening to local elites than democratic reforms that more directly challenge deep-rooted informal power structures.⁸ Finally, gender quota experiments also contribute to the literature on informal institutions by showing that, at least in the case of informal institutions of female discrimination in leadership positions, significant change can occur in short order and does not have to be slow or incremental (Lauth 2000; Knight 1992).

Making weak democratic institutions work better

Whereas the studies reviewed in the previous section assess the impact of introducing new democratic institutions or altering institutional design, a number of field experiments focused on information and monitoring have examined ways to make weak democratic institutions work better without directly altering the institutions themselves.

Information campaigns

There is good reason to believe that electoral institutions fail in part because voters lack the information they need to hold government accountable. A number of recent experiments have therefore investigated whether providing citizens with more information will have a significant effect on turnout and vote choice. One broad set of experiments explores the effect of civic education on political behavior. These experiments derive from the core insight in Almond and Verba (1963) that democratic institutions work better when the political culture is imbued with knowledge of the rights and duties of both citizens and government. Yet, such a democratic culture is rarely present in new democracies where citizens are unfamiliar with challenging authority, tend to vote along ethnic or religious lines, or are susceptible to vote-buying and clientelistic exchange.

One example of such an experiment is a voter mobilization campaign aimed at informing women in Pakistan of the benefits of voting and of the secret ballot (Gine and Mansuri 2012). Geographic clusters within villages were assigned to treatment and control groups; a further subset of households within treatment villages were assigned to receive the door-to-door campaign. The authors found that treated women, compared to controls, were 12 percent more likely to vote in the 2008 national elections. They also found similar turnout rates for *untreated* women within treatment clusters, demonstrating the potentially powerful spillover effects from civic education campaigns. Importantly, the authors show that the increase in female turnout caused a decrease in support for the winning party, which suggests that information could have a significant effect on the policy agenda.

A number of other studies have demonstrated that civic education campaigns can also have an effect on undesirable political behavior. For instance, Banerjee *et al.* (2013) show that messages encouraging citizens not to vote along ethnic lines caused a 10 percent reduction in ethnic voting and reduced support for criminal politicians in 2007 elections in India. Collier and Vicente (2014) demonstrate that an anti-violence pre-election campaign in Nigeria reduced the intensity of violence and increased voter turnout. In another study, Vicente (2014) finds that an education campaign criticizing vote-buying in Sao Tome and Principe reduced the influence of money in elections. Interestingly, the campaign also decreased electoral participation and increased the vote-share of the incumbent. This experiment thus provides the insight that vote-buying can increase turnout and that challengers might be more likely to use vote-buying as a strategy for counteracting incumbency advantage. Overall, these experiments provide growing evidence that giving voters more information that aims to instill a “civic culture” can indeed have a positive effect on political behavior.

Another set of experiments arises from the notion that citizens lack information on politician performance and therefore are ill positioned to hold incumbents accountable by rewarding or sanctioning them at re-election time. Several recent experiments aimed to provide voters with better information on incumbent performance to assess the extent to which this affects voter behavior and, ultimately, accountability.

In an experiment implemented prior to state legislature elections in Delhi, India, Banerjee *et al.* (2011) examine whether the exogenous provision of political information through the media in 200 treatment villages influences voter turnout, incumbent vote-share, and the use of vote-buying as an electoral strategy. Treatment villages received newspapers containing information on the performance of the incumbent legislator along a number of dimensions, on legislator responsibilities, and on the qualifications of major candidates. The authors find that the information caused higher turnout, reduced vote-buying, and higher vote-share for better performing and more qualified incumbents. Chong *et al.* (2011) conducted an

experiment in Mexico in which treatment households received different information on corruption and public spending in the lead-up to 2009 mayoral elections in Mexico. They found, in contrast to Banerjee *et al.* (2011), that information on corruption *decreased* voter turnout and increased apathy. Similarly, de Figueiredo, Hidalgo and Kasahara (2013) found in a field experiment in Brazil that, under certain conditions, corruption information can suppress voter turnout.

Two recent information field experiments directly involve politicians rather than simply providing unmediated information to voters. To study the effects of information on the performance of Members of Parliament (MPs) in Uganda, Humphreys and Weinstein (2012) informed a random sample of MPs that their constituents would be given detailed information on their performance in the lead-up to 2011 elections. While survey data shows that Ugandan voters wanted information about their MP's performance, the dissemination campaigns had no impact on re-election probabilities. Finally, Bidwell, Casey and Glennerster (2013) focus on an intervention that provides voters with information on campaign platforms through videos of candidate debates and find moderate to strong evidence for shifts in voting behavior.

All in all, the information experiments help to demonstrate that providing voters with better information affects political behavior and accountability. They provide some support for the notion that institutions—in this case, electoral institutions—can be improved without making changes to the rules by which those institutions operate. Yet, they also suggest that information is not always welfare enhancing and, under certain conditions, it may have an adverse effect on turnout. Importantly, the mixed results are motivating a second generation of field studies that aim to explain how voting behavior varies depending on voters' priors (Paler 2013); perceived importance of candidate identity (Conroy-Krutz 2013); or expectations about what government should be doing in the first place (Gottlieb 2012).

Independent monitoring

Electoral institutions are unlikely to induce political accountability if informal power structures shape how those institutions operate. Recent research suggests that introducing independent monitoring institutions can compel democratic institutions to work better (Olken 2007). Monitoring institutions constrain the behavior of politicians insofar as they come with a credible punishment for wrongdoing. A number of recent experiments have aimed to test the hypothesis that election-monitoring institutions will undermine politicians' ability to engage in electoral malfeasance. This is an important hypothesis given the concern that monitoring might simply cause candidates or parties to shift illegal activities to unmonitored locations or administration levels.

To test these hypotheses, Ichino and Schündeln (2012) implemented a field experiment examining the causal effect of *domestic* election observation on irregularities in voter registration in the lead-up to 2007 elections in Ghana. Using a two-level design, the authors find evidence that observers displace a substantial portion of irregularities to nearby unobserved registration efforts following a pattern of communication among political party agents.

This sort of coordination among agents is also the focus of the recent paper by Callen and Long (forthcoming), who evaluate an innovative election-monitoring technology that was introduced during the 2010 parliamentary elections in Afghanistan. The technology works by photographically recording differences between post-election polling-center-level counts and the corresponding numbers in the certified national aggregate. The authors found that such monitoring reduced both the incidence of theft or damage to election materials at polling centers and the number of votes cast for powerful candidates. The authors also found evidence

of elite coordination in that election officials are more willing to alter results for candidates that could shield them from possible retribution. All in all, these studies contribute to knowledge of how monitoring affects institutional performance and sheds light on the strategic response of politicians to efforts to detect and deter misconduct.

Conclusions and future directions

This chapter has assessed the extent to which field experiments are contributing to the study of political institutions, particularly democratic institutions. This review has aimed to show that recent experiments designed to investigate the causal effects of introducing new democratic institutions, varying institutional design, and strengthening weak institutions are contributing both empirically and theoretically to the institutions literature.

With respect to research on the effects of introducing new democratic institutions, the recent set of experiments conducted in the context of CDD/R programs does not find evidence that introducing new democratic institutions at the local level results in significant improvements to local governance or collective action capacity. In contrast, experiments on different aspects of institutional design are showing that direct (versus representative) democracy and mandated gender quotas can ameliorate ingrained norms and have meaningful effects on policy outcomes. Another set of experiments is providing robust evidence that information and monitoring have an effect on citizen political behavior and the ways in which weak democratic institutions work.

While these studies are shedding light on long-standing debates in the institutions literature that cannot easily be resolved using observational data, it is important to take stock of the experiments literature. The number of experiments on institutions to date is relatively small, making it inappropriate to draw definitive conclusions from them. For instance, the fact that recent CDD/R experiments provide little evidence that introducing new institutions has an effect on governance and cohesion cannot be taken as conclusive proof that they will never have an effect. Instead, these “first-generation” studies are primarily highlighting the need for more empirical and theoretical work on the conditions under which introducing new institutions might be effective. This speaks to the potential for future research to address concerns of external validity, namely by intentionally designing experiments to investigate how results vary in different contexts or if aspects of the treatment are altered. The idea is to replicate experiments in different locations, but only after explicitly theorizing how specific contextual variables are likely to affect the relationship between the manipulated institution and the outcome of interest. We believe that better theorizing and testing of the relationship between distinct experimental manipulations and the disparate social and political contexts into which they are introduced is necessary for extracting generalizable knowledge that researchers and policy makers can put to use in varied settings.

There is a growing need for a large number of experiments (in various contexts using different permutations of treatment design) on a single topic if we are to use experimental methods to achieve meaningful knowledge accumulation. Such a research agenda goes beyond the important call for more replication studies to address external validity concerns (Humphreys and Weinstein 2009). Instead, achieving knowledge accumulation likely entails rethinking the way that experimental research is organized in the first place. In addition to the current decentralized, entrepreneurial approach to research in the social sciences, a new model that fosters greater coordination among experimental research teams around topics of theoretical importance is needed. Such coordination would explicitly use site selection to increase generalizability as well as strive to achieve standardization around such things as research design,

outcomes, sample selection, measurement, replication materials, and implementation protocols. Such standardization is necessarily for conducting meta-analysis, the workhorse of knowledge accumulation.⁹

Future experimental work on political institutions must also address the thorny issue of level of analysis. As this chapter demonstrates, the vast majority of field experiments in developing countries take place at the community (village or town) level. For some research questions (for example election monitoring), the community or neighborhood is the natural unit of analysis. In such cases field experiments at the local level can help uncover general or universal micro-foundational behavior. This seems to be the case, for example, with experiments testing the impact of mandated gender quotas where discrimination against women in leadership positions is more a matter of prejudice than about voter coordination.

However, for other outcomes—for example those testing the resilience of informal institutions in the face of new formal institutions—it may be difficult to draw conclusions at higher levels from studies that take place at the village level. On one hand, village leaders perform several roles in a community (e.g. access point to higher levels of government and providers of services such as loans), which may reduce the willingness of community members to challenge their authority. On the other hand, the dense networks characterizing villages allow tools that are not available to constituents in large geographical units, such as naming and shaming, to be used to constrain elite behavior. The experimental literature needs to explicate more clearly the conditions under which findings at the local level might be generalizable to other levels of government and other contexts. Moreover, a greater awareness of the limitations of research on institutions at the local level is motivating researchers to conduct field experiments on political institutions at higher levels (Humphreys and Weinstein 2012; Grossman, Humphreys and Sacramone-Lutz forthcoming; Banerjee *et al.* 2012). This is an important development if field experiments are to continue making meaningful contributions to the study of the causes and effects of political institutions.

Notes

- 1 We are grateful to Alex Garlick for his excellent research assistance.
- 2 Randomized experiments differ from natural experiments, in which social and political processes are randomly, or as good as randomly, assigned by something outside of the researchers' control.
- 3 The focus on developing countries reflects both the authors' expertise and the fact that field experiments are typically conducted in developing countries for both theoretical and practical reasons.
- 4 We do not address several interesting experiments on institutions in the political economy literature for lack of space. These include experiments that alter the incentives of bureaucrats to improve government performance, including in the areas of tax collection (Khan, Kwaja and Olken n.d.), police performance (Banerjee *et al.* 2012), and the public sector more generally (Dal Bo, Finan and Rossi 2013). Other notable experiments have studied the effects of strengthening informal (Blattman, Hartman and Blair 2014) and formal (Sandefur and Siddiqi 2013) institutions of dispute resolution.
- 5 This would be the case, for instance, if a treatment were considered too politically sensitive or if the partner specifically wanted to test a bundle of activities together, which is often the case in policy evaluations.
- 6 The World Bank alone currently spends about \$2 billion per year on various CDD/R programs (Mansuri and Rao 2013).
- 7 A similar CDD/R experiment has been conducted in Liberia by Fearon, Humphreys and Weinstein (2011).
- 8 Existing evidence suggests that across the developing world women who win seats via quotas are usually related to the male local leaders.
- 9 See, for example, the new granting initiative by the Experiments in Governance and Politics (www.e-gap.org), which prioritizes a high degree of coordination across funded research teams.

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Part II

Comparative political institutions

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CONSTITUTIONS AS POLITICAL INSTITUTIONS

*Tom Ginsburg*¹

Constitutions are the primary institutions for organizing and regulating political systems, and therefore crucial subjects for comparative political analysis. Indeed, because constitutions provide the rules about the making of rules (Buchanan and Tullock 1962), they might be productively characterized as meta-institutions, and have been of interest to scholars since Aristotle. In recent decades, institutionalist scholars have gone a long way toward articulating functional theories of constitutions and have also paid attention to the determinants of form through studies of diffusion and borrowing. But there is still much work to be done in testing theory, and in delving into complex questions of institutional complementarity. Although there are significant methodological and data challenges for the research program in comparative constitutional studies, there is great promise for the future.

At the outset, it is worth distinguishing between constitutions, which are institutions, and constitutionalism, which is a normative ideal that government should be limited by law. Not all functions or features of constitutions necessarily advance the ideals of constitutionalism. This ties into another central distinction in the study of constitutions, between “large-C” Constitutions and “small-c” constitutional rules. The former are formal documents that have become part of the script of the nation-state (Meyer *et al.* 1997), while the latter consist of other formal and informal institutions and rules that play a constitutional function, however defined. These might include judicial decisions interpreting a constitutional text, statutes that are so entrenched that they are unlikely to ever be overturned (sometimes called “super-statutes”), and informal norms and conventions that define and constrain the exercise of power. The primary focus in this essay is on large-C constitutions, as the small-c variant might be argued to encompass virtually every institution discussed in this volume.

There are many ways to study constitutions from either perspective. Lawyers tend to focus exclusively on those rules that are legally binding, and to trace various modes of constitutional interpretation by courts. Some political scientists and comparative law scholars prefer to emphasize the role of constitutions as defining and producing identity (Jacobsohn 2010; Rosenfeld 2009) or their “poetic” role in appealing to emotion (Sajo 2010). The institutionalist tradition in political science and economics, in contrast, tends to draw on rational choice methodologies to understand the functions of constitutions for the political and economic systems.

The institutionalist tradition draws from older work in public choice and constitutional political economy, which focuses on the higher order rules that shape behavior. Political constitutions of nation-states are only one example of “constitutional” rules and institutions, but are particularly important because of their structure as the highest set of legal norms that structure political systems. The basic assumption is that different constitutional schemes can have different incentive effects on political life, the production of public goods, and the function of the economy. This research program has incorporated contributions from many different disciplines, and has both a positive component as well as a normative one in which the concern is optimal institutional design (see, e.g., Cooter 2000; Voigt 1997; Riker 1964; Persson and Tabellini 2003; Van den Hauwe 2000).

This chapter begins by reviewing the functions of national constitutions from an institutional perspective. It then goes on to very briefly survey some of what we know about constitutional drafting, constitutional endurance and change, and the efficacy of constitutions. The last part of the chapter identifies challenges and provides suggestions for future research directions.

Constitutions as coordination

All regimes need institutions, and need to coordinate which institutions will play what roles in different situations that arise downstream. Laying out the structures of government facilitates the operation of institutions because it prevents continuous renegotiation. A written constitutional text can minimize conflict over basic institutions for any regime (Carey 2000), including authoritarian ones (Barros 2002). Furthermore, constitutions can set up certain institutions, such as constitutional courts, to play a role in resolving *future* disputes about power. In this way, constitutions are important tools for the functioning of governments.

Constitutions are also useful for constraining the very government that they empower. Constitutionalism is the idea that government ought to be limited, and so citizens are protected in terms of their core interests by a “social contract.” While citizens may have strong shared intuitions about the limits on government power, they may not have precise intersubjective agreement on the precise definitions of what counts as an abuse, or on procedures for identifying violations. A formal constitution can help to generate this intersubjective understanding (Weingast 1997). In this model, citizen demands and expectations do the bulk of the work in limiting government behavior; but the addition of a written constitution increases the probability that this deep force will indeed be brought to bear by the citizens when government violates the terms of the social contract.

A related constitutional function is that of intertemporal commitment (North and Weingast 1989; Sunstein 2000). Rulers can make promises, but why should their subjects believe them? Embedding a promise into a constitution, which is typically more difficult to change than ordinary law, can raise the costs of reversal, in turn making regime promises more credible.

Several scholars have argued that successful constitutions ought to reduce the stakes of politics (Przeworski 1991; Weingast 1997; Sunstein 2000). By providing for rights, constitutions maximize consent over issues of great importance to individuals. This limits the harm that can be imposed through political processes, which has the added virtue of directing political energy from insoluble problems (for example, which is the best religion?) to solvable ones (how much money should we spend on the military vs. building roads?) (Sunstein 2000). Among other benefits, politics involving lower stakes will result in reduced expenditures by groups to capture government processes.

Constitutions may also facilitate control of lower-level agents. All regimes need to gather information on the activities of their own employees, and occasionally to punish them as well.

Constitutions can provide incentive structures for principals (monarchs, the general public, or heads of government) to be able to monitor their agents. One device is to encourage others to complain about agent misbehavior, as in the “fire alarm” model of legal controls (McNollGast 1987). Constitutions have long featured such institutional design. As Jean Bodin (1576) noted, the French king granted immunity to members of parliament in order to encourage them to bring complaints against provincial agents of the king. This helped the king to learn about agent misbehavior, making monarchical governance more effective.

The history of constitutionalism reflects this combination of credible commitments and agency control. In his study of the origins of the state, Roger Myerson (2008) provides a model in which an autocrat creates a court of notables with the ability to remove him from power. “Without such an institutionalized check on the leader,” Myerson writes, “he could not credibly raise the support he needs to compete for power” (2008: 130). To ensure that his agents do not cheat or rebel against him, the prince must credibly assure all agents that he will act fairly. A promise to have a public trial of an agent before any punishment, observed by the other agents, ensures that the prince will act fairly; otherwise the other agents will be able to coordinate and overthrow him. Applying this perspective to the early history of the Court of the Exchequer in England, Myerson shows how common knowledge and the constitutional commitment by the king to punish only those agents whose malfeasance has been publicly verified help to make government more effective. This simple constitutional setup solves both the agency problem and the commitment problem on the part of the monarch.

Problems of coordination, commitment, agency control, and the generation of information for governance are general problems of all government institutions. Effective constitutions can help solve each of these institutional problems. What, however, ensures that the constitutions will actually be effectively enforced, even if in the interests of all? After all, a powerful ruler can always overturn the rules or rewrite them to his advantage. Entrenchment plays a crucial role in ensuring efficacy, but entrenchment is not foolproof. Rather, it merely raises the price that a dominant coalition must incur before changing the rules.

The theory of self-enforcing institutions, which has been very influential in recent years, helps to resolve this puzzle (Ordeshook 1992; Weingast 1997). The central problem to which self-enforcement theory is addressed is that, as the highest institution in a political system, there is no external enforcer of the constitutional bargain. Any enforcer is itself part of the governance structure. For example, while constitutional courts can help parties coordinate their understandings of constitutional norms in cases of ambiguity, they lack the proverbial power of the purse or sword, and so must depend on other actors to enforce their rulings.

The answer to the puzzle is that sustainable institutions must be self-enforcing, in which powerful players exercise mutual vetoes on constitutional change. They are equilibrium phenomena. Weingast (2006) provides an extended analysis of the first century of the American constitution, in which the formal constitutional bargain between North and South was supplemented with a series of pacts ultimately embodied in the Missouri compromise, in which each new free state was accompanied by a slave state to preserve sectional balance. As the population of the country moved westward to regions in which slavery was not as feasible, the bargain became obsolescent, leading eventually to the civil war. The constitutional bargain was preserved for a century, however, by coordinating understandings and expectations among political antagonists.

Institutional theory has provided profound insights into the political functions of constitutionalism, and helps us understand positive and normative acquiescence to some constitutional provisions. However, these functional accounts are not particularly useful for generating predictions about the circumstances under which actual constitutions will be formed and endure.

After all, many countries are unable to produce effective constitutions, even if doing so would deliver profound benefits. A successful coordinating device is simply recognizable by the presence of a constitution in force that is generally accepted; but we do not have anything within game theory that provides insight into what the equilibrium will be. To understand that, we must turn to the actual circumstances of constitutional drafting, on which there is relatively little known in a systematic way.

Drafting

Studies of constitutional drafting tend to consist of descriptive case studies, but the method of the analytic narrative (Bates *et al.* 1998) can be helpful in helping to uncover the institutional logic. Such narratives can allow us to specify interests, trace negotiations, and understand the possible Pareto-superior outcomes. Theories of contracting drawn from law and economics are also helpful in this context. While social contract theories have come under attack from various quarters as a normative account of constitutionalism, contractarian theories help us understand how constitutions are formed and operate—as a subject of positivist enquiry.

A key variable in shaping the constitutional negotiation is the organization of the production process of the constitution itself (Tushnet 2013; Widner 2008). This in turn may be determined by a prior constitution or set of norms, by a peace treaty among warring factions, or simply by pragmatic accommodation among powerful players. Institutional features of the process are likely to have some impact on the ability to reach an agreement and on the nature of the resulting process. For example, Elster (1995) claims that more transparent processes can lead to more arguing than bargaining, in which parties spend time and energy on posturing before their respective principles. Many constitutions are produced as elite pacts by a small number of groups or factions. Others are more open, and there is a strong normative trend in recent years toward encouraging broad participation of the public in constitution-making (Arato 2012), though the empirical literature is mixed on whether participation is necessary or sufficient for effective constitutional design (Horowitz 2013; Ginsburg *et al.* 2009). Elster argues for an “hourglass” approach to transparency, with wide participation at the beginning of the process and to approve the final product, but a relatively closed middle, in which interests can be worked out. But there is still relatively little empirical examination of these conjectures (for a summary, see Ginsburg *et al.* 2009).

Time is another variable. Some processes are rushed, while others are more leisurely, depending on the external circumstances in effect. Time constraints might pressure parties to leave more things unspecified if they run out of time to write things down; at the same time, they can reduce the possibility of strategic behavior in the negotiation process. If time for drafting were unlimited, parties might never sit down to sort out the hard issues. Another crucial factor is the diversity of parties to the constitutional negotiation process.

In an important recent study of constitutional change in Latin America from 1900 to 2008, Negretto (2013) differentiates between the common interests of constitution-makers in coming to an agreement to capture collective benefits from the distributive conflict within a particular constitutional scheme. He argues that general guidelines for reform are determined by responses to actual governance problems, but power comes into play in the choice of specific institutions at moments of reform.

One area for further systematic investigation is the role of outsiders. Notwithstanding the assumptions of self-enforcement in institutional theory, in many design situations there are external actors heavily involved in the drafting and negotiation process. An extreme example is the Constitution of Bosnia-Herzegovina, attached to the Dayton Accords ending the Bosnian

war. This document forms a classic example of a consociational bargain (McCrudden and O'Leary 2013), but it is one in which external monitors are embedded in the constitutional order. For example, three members of the constitutional court are appointed by the President of the European Court of Human Rights and a High Representative of the international contact group helps to resolve the frequent impasses in Bosnia's collective presidency. Kenya's Constitution of 2009 was produced by a drafting commission that included three members from other African countries. And the historical example of Japan's Constitution, produced in first draft by the American occupation authorities, suggests that constitutions can indeed survive and prosper despite foreign influence.

As an aside, and in contrast with the theory of self-enforcing institutions, external involvement in the production of constitutions suggests that external actors may also have an interest in *enforcing* the constitution. Elkins *et al.* (2008) discuss the role of external actors in bounding the terms of constitutional debate in Japan, and the more general role of external enforcement in constitutions drafted under occupation. More broadly, one might think of external actors as being actual parties to constitutional production processes, with their own interests and constituencies—the United Nations has played this role in several post-conflict societies.

Contents

Constitutions are drafted by experts, government officials, and lawyers acting on behalf of the negotiating groups. In many cases, rather than draft the document from scratch, they will look for a model, or several models, to see how the document is organized and what subjects it covers (Choudhry 2007). I call these boilerplate provisions of constitutions (Ginsburg 2010; see also Law 2005 describing “generic” constitutional law). Empirical work has identified a good deal of constitutional copying. Law and Versteeg (2011) have identified the contents of global templates of rights provisions in constitutions. Elkins (2010) traces relative similarity among constitutional documents in Europe. Elkins *et al.* (2013) have shown the influence of international treaties on constitutional rights provisions. In short, the content of constitutions may, like some contracts, have a form-like quality. Provisions migrate from document to document, sometimes with only minor amounts of local tailoring.

Boilerplate provisions have the advantage of saving on transaction costs of negotiation. Furthermore, in the constitutional context, the usual objections to “boilerplate” in contracts between buyers and sellers—namely that they involve a power imbalance in favor of drafters—are less salient (Ben-Shahar 2007). On the other hand, in the constitutional context there are few of the mechanisms of market discipline that some believe restrain the use of “inefficient” boilerplates in the contractual setting (Ben-Shahar and White 2007). We cannot be confident that the phrases that are being borrowed are in fact the best provisions.

If one believes that constitutional provisions have been adopted by other countries based on an independent assessment of their benefits, borrowing can represent a form of social learning, by which states learn from others' experience (Posner and Sunstein 2006). Further, some provisions of a constitution may be directed externally, such as rights provisions that are designed to act as signals to international audiences (Farber 2002). It might make sense for drafters to use conventional forms of rights language to achieve this signaling purpose. On the other hand, boilerplate can lead to adoption of provisions without much local meaning. If there is less local understanding of what the provisions entail, there is less likelihood of effective enforcement in practice (Weingast 1997).

One consequence of the boilerplate quality of constitutions is that the subjects included in them are partly a result of conventions or trends. Many of the things commonly included in

constitutional texts, such as provisions about flags or national mottos, are hardly of such significance that they need to be enshrined in the highest law of the land. On the other hand, there may be certain things that are not included in most constitutions which perhaps ought to be. For example, few constitutions mention the administrative state, even though it is the central feature of modern government. The details of the electoral system are often left out, even though this means there is a danger of self-dealing by those in control of the legislative process.

In our work on the Comparative Constitutions Project, Elkins, Melton, and I identify certain core provisions to written constitutions, defined as those found in a vast majority of constitutional texts. For example, nearly every constitution provides for its own amendment, includes some rights, and establishes a mechanism to select the head of government. Conversely, other provisions are more peripheral, and might be thought of as options. Constitutions are also subject to trends over time. In the nineteenth century, for example, few constitutions mentioned political parties, while most written in the twentieth century do so. Conversely, bankruptcy was a popular topic in the nineteenth century but is now less likely to be contained in constitutions.

Our general view is that time and space are important determinants of content. But we do not know much empirically about the relationship between the production process of constitutions and their contents, or their effective enforcement. Normative theory has preceded positive theory, which in turn has preceded systematic empirical testing. (We will return to this issue in the section on “Future challenges,” pp. 108–110.)

Endurance and change

Constitutions, while intended to endure, do not typically do so (Elkins *et al.* 2009). Enduring constitutions require enforcement, either by self-enforcing mechanisms or by international actors, and will endure so long as relevant parties believe they are better off within the bargain than in risking new constitutional negotiations.

The equilibrium concept of constitution is not a normative theory. It says nothing about the quality of the document as being public-regarding or dominated by private interest. But it suggests a quality that will be as necessary for either type of constitution to endure, namely that a coalition of interest groups must support the constitution’s continuing efficacy.

The initial constitutional bargain will of necessity be “incomplete,” in that the parties will be unable to specify every future contingency. Because of transaction costs to negotiation, parties that seek to specify every contingency will never conclude a deal. This means that the bargain will be subject to unanticipated “shocks” that occur downstream. Just as parties to a contract may find, for example, that prices of supplies increase, forcing a renegotiation of the deal, constitutional bargainers may need to adjust their bargain down the road if anticipated benefits do not materialize. This might involve constitutional amendment, or even a replacement of the constitution with a new one.

The contract theory notion of efficient breach suggests that parties in some circumstances can and should violate their prior agreements when it is efficient to do so. By analogy, for every period after the initial negotiation the parties to a constitution have to make a decision as to whether to remain in the current bargain or to renegotiate. Sometimes pressure for renegotiation may result from the revelation of information that was strategically hidden during initial negotiations. Sometimes pressure can result from an external shock that changes costs and benefits over time, leading parties to want to renegotiate the deal. And sometimes the costs and benefits may simply shift because of internal changes, reflecting the rise of a minority group or newly emerging social force seeking to enhance its position.

Amendment procedures are thus crucial for constitutional survival, and there has been interesting work on the thorny problem of measuring amendment difficulty cross-nationally (Lutz 1995; Lorenz 2005; Rasch and Congleton 2006; Ferejohn 1997). The question presents complex methodological problems of incommensurability and endogeneity. To illustrate, do we really know whether an amendment scheme that requires, say, a two-thirds majority of the legislature and a signature of the president is more rigid than one that requires only a legislative majority in two successive legislatures? The variety of different schemes is great, and it is difficult to compare institutional setups directly.

Most approaches that try to solve this problem draw on the observed rate of amendment difficulty to evaluate the relative rigidity of different institutional setups. But here endogeneity raises its ugly head. The observed rate of amendment difficulty may be a product of the same factors that shape the design of the amendment rule. Because we do not know what leads some drafters to adopt a low amendment threshold and others to adopt a high threshold, it is difficult to draw conclusions about the effects of different rules on the rate of constitutional amendment.

A further difficulty is that many constitutions have different decision rules across different issues. Just as constitutional rules are typically entrenched relative to normal legislation, so too some constitutional rules may be more entrenched than others. For example, in the United States Constitution, Article V provides that no state may be deprived of equal representation in the Senate without its agreement. This provision sets the decision threshold as unanimity, entrenching the representative scheme in the Senate far more strongly than the representative scheme in the House. India's Constitution is another example of a complex scheme, as some issues require an absolute majority and others higher majorities. Frequent amendments themselves modify the parts of the constitution subject to the different amendment formulae.

Besides formal amendment, constitutional review is another mechanism for constitutional change, and one that has received a good deal of scholarly attention. Scholars have theorized the institutional rationale for the adoption of constitutional review and its timing (Hirschl 2004; Ginsburg 2003). And an increasingly rich empirical literature is enhancing our awareness of the various functions of constitutional review. It is a particularly important constitutional institution, but there are, of course, many others that have been subjected to institutional analysis.

In terms of the effect of content on constitutional endurance, Elkins *et al.* (2009) find that a relatively flexible amendment scheme is important (notwithstanding the challenges of measurement identified above). We also show that constitutions that are more specific tend to last longer. But there is much more work to be done in this area.

Efficacy and variation

The question of when and how constitutions “work” is a very important one, and first requires nuanced specification of the target of inquiry (Breslin 2009). Constitutional efficacy probably depends on the specific rule in question. For example, one might distinguish rights provisions from those rules that set up government institutions. Institutional provisions, in theory, seem to be better candidates for self-enforcement than are rights provisions, which tend to distribute moral claims.

While major research programs in comparative politics have addressed issues like federalism, the relative merits of presidentialism and parliamentarism, and the role of small-c constitutional design in managing divided societies, little of this work has actually focused on the inclusion of the relevant rules in the formal large-C constitution. Evaluating the consequences of political institutions is a slightly different research question than is evaluating the efficacy of formal constitutionalization in producing those consequences.

With regard to rights, on the other hand, a small cottage industry has grown up trying to examine the role of formal constitutions in human rights protection (Camp Keith 2002; Cross 1999; Davenport 1996). Some of these efforts study multiple rights; others focus on a single one (Cross 1999). The basic theory is that including rights in a constitutional text facilitates enforcement activity by private actors and other government officials. The evidence here seems to be mixed. Linda Camp Keith (2002) compares the provision of rights, both substantive and procedural, and actual human rights protection, and finds that provisions for judicial independence improve human rights protection, as do due process provisions such as guarantees of public and fair trials. These process rights seem to be more important than the presence or absence of the substantive provisions themselves. Frank Cross (1999) finds that judicial independence is correlated with the presence of political rights and protection against unreasonable searches and seizures. Christian Davenport's (1996) more comprehensive analysis covers 39 countries over a thirty-year period and distinguishes *de jure* provisions on rights, limitations on rights, state of emergency clauses, and limitations thereof. Davenport finds that countries with a state of emergency clause have lower levels of repression, and that the freedom of the press is a crucial variable for reducing repression. Recently, Camp Keith, Tate, and Poe (2009) confirmed many of the findings from earlier studies using a longer time period and larger sample of countries, and Law and Versteeg (2013) have developed a test of constitutional rights.

An important recent line of work on constitutional efficacy follows the trend in comparative politics toward studying authoritarian regimes (Levitsky and Way 2010; Svoboda 2012; Schedler 2006; Gandhi 2010). While these have been traditionally viewed as "shams" (Murphy 2006; Law and Versteeg 2013), we are now learning more about their functions. Ginsburg and Simpser (2014) identify a series of functions that can and are played by constitutions in authoritarian regimes, drawing on both institutional analysis and other work. They argue that many of these functions are similar to those played by constitutions in democracies. For example, some authoritarians may use constitutions as precommitment devices; others may seek to use them to control lower-level bureaucratic agents. Authoritarians also need to coordinate among themselves by establishing political institutions, and elicit cooperation from subjects. For all these functions, texts can play helpful roles.

Ginsburg and Simpser summarize these roles by characterizing constitutional provisions as *operating manuals*, *billboards*, *blueprints*, and *window dressing*. An operating manual simply describes government institutions, allowing for coordination. A billboard is an advertisement, designed to signal the intentions or policies of a regime. Blueprints are plans for the future. For example, military authoritarians may produce a constitution in the aftermath of a coup to set a timeline for a return to civilian rule, and the terms under which such a return may take place. Announcing such a project will raise the costs of violating the text of the constitution, and may also facilitate a period of "legitimate" authoritarian rule. And sometimes constitutions are window dressing, designed to obfuscate. This is akin to the traditional view of constitutions as shams.

Conclusion: future challenges and directions

The institutional research program on constitutions has gone a long way toward enhancing our understanding of why constitutions should matter for comparative politics. The general thrust of this literature has been to explain how the constraints provided by constitutions are empowering for rulers and ruled alike. Yet, how these functions map onto real-world constitutional design is not always completely clear. There is no doubt that the institutionalist functions do not exhaust those that are attributed to constitutions, nor do they go far in explaining form, which varies quite dramatically across time and space. We tend to assume comparability in the topic of

constitutions across different concepts, but this is not always appropriate (see Elkins *et al.* 2009 for a discussion). Furthermore, institutional theories tend to be of a one-size-fits-all character, and we do not have predictive accounts of the conditions under which particular institutions will be adopted. Here, diffusion studies focusing on emulation have been useful, at least for establishing the broad variation.

The research program evaluating the actual effects of constitutions is in its infancy, and faces some of the methodological challenges common to other fields of comparative politics. Individual case studies have been very important but large-N work has been somewhat hindered. General problems of causal inference are severe in a context in which multiple factors interact with environmental conditions in a relatively small number of real-world settings. Endogeneity is unavoidable. As Law (2010: 388) puts it, “Large-C constitutions may be intended to structure political, social and economic arrangements, but they are also the products of the very arrangements they are supposed to shape.” Another problem with ordinary statistical models is heterogeneity in the effectiveness of institutions depending on particular conditions, and standard cross-sectional time series approaches may not pick up the appropriate variance.

However, some new data sources may facilitate further testing. The Comparative Constitutions Project tracks the formal features of constitutions of independent states since 1789. Versteeg has a dataset on rights provisions in formal constitutions of states after World War II (Versteeg and Goderis 2008). The CompLaw database (Carrubba *et al.* 2012) captures information on constitutional adjudication by the highest courts in 43 countries, with more being added. And there is an increasing array of international survey data that may prove helpful in terms of testing hypotheses (World Justice Project 2014).

Still, empirical problems remain. The new data sources sometimes confront interesting problems of measurement, as our discussion of amendment difficulty suggested. Furthermore, there is not yet comprehensive data on the drafting process of constitutions, and this has somewhat hindered efforts to try to relate the process of constitutional production with the contents or efficacy of the resulting documents. This is an important area in which normative speculations have far outpaced rigorous empirical work.

A major challenge to the institutionalist research program is that it has not gone far in understanding the interaction among different institutions that co-occur. Provisions may be designed and analyzed in isolation; but they operate as part of institutional systems in which the whole is more than the sum of its parts (Vermeule 2011). For example, in examining the effectiveness of particular constitutional rules, for example on emergencies or term limits, scholars have not explored whether the presence of a constitutional court makes a difference in performance. How do the co-presence of multiple institutions interact to produce efficacy? This research program is important, particularly since it has been noted since the beginning of modern constitutional theory. James Madison noted in Federalist 51 that “different governments will control each other, at the same time that each will be controlled by itself.” But the particular configurations of different institutions have not received much attention.

New methods may help us understand the interaction of different institutions and context that produces effects. Law (2010) speculates on use of agent-based modeling in computer simulations, and also notes that experimental design may help to advance the research program. And more complicated tools for analyzing texts, such as topic models, might be helpful for us to learn about channels of institutional borrowing.

Constitutions are at the core of the institutionalist research program in political science, and also are institutions for which the real-world stakes could not be higher. An increasingly dense array of international and domestic actors is looking for answers to questions about optimal

institutional design, yet the literature has provided precious few answers. Yet there is hope that the new tools available to scholars and expanded data will help to address these concerns.

Note

- 1 This work draws on various pieces of joint work with Alberto Simpser, James Melton, and Zachary Elkins. All mistakes are my own.

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9

FROM DUVERGER TO ROKKAN AND BACK

Progress and challenges in the study of electoral systems

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Traditional electoral studies have primarily been concerned with the effect of the rules of election on party systems and with the consequences of the rules of candidate selection on party organization and behavior. More recently, a considerable number of scholars have addressed the potential endogeneity of electoral rules; in particular, how the conditions of party competition may themselves trigger processes of electoral change. In this chapter, I discuss some central topics in the literature on the party system effects of electoral rules and on the determinants of electoral reform, two areas of electoral research that reveal the progress made and the challenges ahead in the more general study of political institutions.

In relation to the effect of electoral rules on party formats, I argue that progress in specifying and testing the interaction of electoral structures both with broader institutional configurations and with non-institutional factors stands in contrast with the still limited theoretical understanding of how politicized dimensions of conflict emerge in a polity and how they affect the number of parties. As regards electoral change, I propose that although the strategic model has made important contributions to understanding the origins of electoral rules, it needs to be revised to incorporate the existence of diverse historical paths and varying conditions of electoral reform. In particular, it must relax the assumption that reformers have complete information, that they maximize short-term partisan interests under every condition, and that the political elite have perfect control over the process of reform. The chapter concludes by discussing the need to incorporate the mutual causal effects between politicized dimensions of conflict, party systems, and electoral reform into a single, dynamic theoretical framework.

The party system effects of electoral systems

Most research on electoral systems has been devoted to the analysis of Duverger's propositions about the party system effects of plurality, two-ballot majority, and proportional formulas. As this research agenda has evolved, political scientists have accumulated a great deal of knowledge about the way electoral systems work and how their effects are conditional on a variety of institutional and non-institutional factors. Yet advances in the empirical analysis of the mechanics of electoral systems and conditional relationships have not been accompanied by similar progress

in the theoretical understanding of the exact causal role of electoral rules in a world plagued by mutual causality.

Electoral rules as independent causal factors: the mechanical effect

In 1951 Duverger proposed that while plurality rule tends to party dualism, two-ballot majority and proportional representation systems tend toward multipartism (Duverger 1957: 232–233). The analysis of these propositions led to a vast literature. I will consider only some key contributions about the party system effects of single-member district plurality (SMDP) and proportional representation (PR) systems, the most advanced areas of research in both theory building and empirical testing.

All electoral systems reduce the number of electoral parties to a smaller number of legislative parties.² However, this effect, which Duverger labeled “mechanical,” is stronger in SMDP than in PR systems. For this reason, SMDP has a discernible “psychological” effect: Voters tend to cast a vote for their second-order preferences to avoid wasting their votes and political leaders tend not to launch an independent party into the competition to avoid losing an election against stronger competitors (Duverger 1957: 252; Cox 1997). As a result of both effects, SMDP tends not only to preserve an established two-party competition but also to restore party dualism when it has been disrupted by the emergence of a third party (Duverger 1957: 254).

Most of the initial research on Duverger’s propositions was aimed at exploring the hypothesis that electoral institutions have an independent causal effect on party systems. Evidence about the mechanical effects of electoral formulas on the number of legislative parties seemed to support this hypothesis. It was firmly established that levels of disproportionality between votes and seats vary significantly across electoral systems (Rae 1971). Specifically, scholars found ample evidence that disproportionality is higher in SMDP than in PR systems (Lijphart 1994: 95–117; Katz 1997: 137). When looking at the number of legislative parties, scholars also found a strong association between discrete electoral formulas and levels of party system fragmentation (Norris 1997: 307; Lijphart 1994). These findings do not, however, validate Duverger’s theory as it was proposed.

Plurality rule is supposed to have a built-in disincentive for small parties to compete alone or for voters to support them. For this reason, it favors the creation of a two-party system and contributes to its maintenance over time. PR formulas, in contrast, are associated with more than two parties because they lack the disincentives that plurality systems have for small parties to compete or for new parties to form. As a consequence, PR increases the probability of multiple parties but does not determine it; multipartism would only occur if in a given country voters really demanded more parties and political leaders were able to form them. This is why Riker proposed (1986: 26–30) that whereas the reductive effect of SMDP should be considered a “law,” the multiplying effect of PR is simply a “hypothesis.”

Duverger’s “law,” however, needed some refinements to account for deviant cases. In Canada and India, plurality systems have coexisted with more than two significant parliamentary parties at the national level. In the case of Canada, the explanation for the anomaly was that one of the third parties nationally was continually one of the two main parties locally (Sartori 1968; Rae 1971). In India, it was argued that more than two parties existed at both local and national levels because of the presence of a centrist party in most districts (the Congress Party) that prevented the formation of coalitions of small parties located to its left and to its right (Riker 1986). The revision of these cases thus led to a reformulation of the law to the claim that a plurality system will produce and maintain a nationwide two-party system *except* when strong minority parties are concentrated in certain constituencies or geographical pockets and when one party

among several is almost always the Condorcet winner in elections (Sartori 1994: 40–41; Riker 1986: 32).

Even if stated in probabilistic terms, Duverger's hypothesis also needed reconsideration for the purposes of comparative analysis. PR systems can differ widely and it is not obvious how these variations are associated with different numbers of parties. Moreover, there are several electoral systems that mix proportional and plurality formulas and give different weight to one or the other (Gallagher 2005: 549). Obviously, either variations within the formula or some other component of the electoral system should be used if we want to assess the number of parties associated with PR.

Sartori (1968) proposed that within PR systems the larger the magnitude of (seats contested in) the district, the greater the number of parties winning votes and seats in the district should be. This view has been generalized and most scholars now consider district magnitude as the key variable that determines the number of parties (Taagepera and Shugart 1989).³ The great advantage of this variable is that it can be used to make comparisons not only within PR formulas but also across different electoral systems. Since the outcome of interest is usually the national party system, a single measure is needed to capture the aggregate effect of electoral districts when they vary in magnitude. The national average district magnitude is the most commonly used measure, but there are alternative formulas to estimate the nationwide district magnitude in mixed-member electoral systems and PR systems with multiple tiers (see Gallagher and Mitchell 2005; Teorell and Lindstedt 2010).

However, district magnitude as a single determinant of party systems also presents problems. There is evidence that higher district magnitudes are negatively associated with disproportionality. But the expected association between district magnitude and party system formats is not always supported. In qualitative analyses using relatively few cases, the association between district magnitude and the number of parties (both electoral and legislative) appears to be weak (Gallagher 2005: 549; Morgenstern and D'Elia 2007). Large-N statistical analyses detect a stronger correlation, but they sometimes overestimate the mechanical effect by not taking into account that the observed effect of district magnitude on the number of legislative parties has already been pre-filtered by the impact that the same variable had on the number of parties winning votes (Benoit 2002: 39). Moreover, even when the indirect impact of district magnitude on the number of electoral parties is considered, results may vary. Teorell and Lindstedt (2010: 443) show that the impact of larger districts on the proportionality of votes and seats may be statistically significant or not depending on the way in which average district magnitude is measured in multitier systems and on whether the analysis includes new democracies.

Electoral rules as intervening variables: the psychological effect

The most interesting investigation into the political consequences of electoral rules is on the influence of the latter on the number of parties competing and winning votes in an election. This analysis offers a better understanding of the macro-level phenomena that affect the process of party formation in a polity and the micro-level mechanisms (Duverger's "psychological" effect) that influence the strategic incentives of party leaders to launch new parties to compete and of voters to vote for them (Blais and Carty 1991; Coppedge 1997; Cox 1997). Since these processes both affect and are affected by the existing electoral system, they provide a closer look at the exact causal role of electoral systems on the shape of the party system. Over time, the attempt to link macro structures with micro-level mechanisms began to cast doubt on whether electoral rules really have an independent causal effect.

One of the first questions about the determinants of electoral parties is whether institutional factors other than district magnitude intervene in the calculations of party strategists and voters.⁴ Duverger only examined parliamentary elections; his predictions may not hold in presidential systems. Presidential elections may “contaminate” legislative elections depending on what formula is in place to elect presidents and on the electoral cycle. The multiparty effect of PR may be neutralized if plurality is used to elect presidents and presidential and legislative elections are held concurrently. By contrast, the tendency toward multipartism may be reinforced when runoff formulas are used to elect presidents and/or when presidents and legislators are elected in non-concurrent cycles (Shugart and Carey 1992; Jones 1995; Shugart 1995). The impact of presidential electoral formulas on electoral parties is, however, indirect, acting through the number of presidential candidates.⁵ Using this approach, various empirical tests have confirmed that temporally proximate presidential elections stop having a reductive effect on the number of electoral parties when the number of candidates competing in presidential elections is sufficiently high (Amorim Neto and Cox 1997; Cox 1997; Golder 2006).

Another important addition to the analysis of the number of electoral parties is the impact of non-institutional variables such as the number of cleavages that divide citizens in a given society. This deserves special attention because it goes beyond the simple incorporation of a new variable into the estimation of the party system effects of electoral rules.

Lipset and Rokkan (1967; Rokkan 2009 [1970]) argued that the shape of national party systems emerged from the particular timing, sequence, and structure of basic sociocultural cleavages triggered by the processes of nation-building, industrialization, and democratization. This literature analyzed electoral structures as interacting with parties and party systems, but mostly as dependent rather than as independent variables.⁶ Duverger did not theorize about the conversion of sociocultural cleavages into national party systems and he famously argued for the causal role of electoral systems in determining the number of parties. Yet at various stages in his analysis, Duverger clearly suggests that the number and structure of political divisions in a polity (often but not always derived from sociocultural factors), along with electoral structures, affect the number of parties in a polity (Duverger 1957: 259–261).

Two different strands of the literature have explored the role of non-institutional determinants of party systems. The first is theory oriented and has attempted to develop an analytic framework to understand the relationship between sociopolitical heterogeneity and the number of parties (Lijphart 1984, 1999; Taagepera and Grofman 1985; Taagepera 1999). A second body of literature, with a more empirical approach, seeks to find the correct statistical specification for the influence of both social and electoral structures on the number of parties (Powell 1982; Ordeshook and Schvestova 1994; Amorim Neto and Cox 1997; Clark and Golder 2006). Since the statistically oriented literature has set the current standard of research in this area, I will summarize its perspective and findings.

The new literature sees the number of parties as a multiplicative rather than an additive function of electoral permissiveness and social heterogeneity.⁷ The interactive model is based on the idea that electoral rules (measured by district magnitude) merely exert a constraining or permissive effect on the influence that social heterogeneity has on the supply of parties for which citizens cast their votes (Ordeshook and Schvestova 1994: 101). Accordingly, a polity will have many electoral parties only if it has *both* many cleavages *and* a permissive enough electoral system (Amorim Neto and Cox 1997: 155). A second feature of this literature is the way in which social heterogeneity is conceptualized and measured. Whereas in the theoretical literature social heterogeneity is analyzed through the number of issue dimensions, understood as the *politicized* dimensions of conflict in a society, the statistical literature mostly restricts it to the effective number of ethnic groups.⁸

Scholars have confirmed the interactive effect of district magnitude and ethnic heterogeneity on the number of electoral parties. Amorim Neto and Cox (1997) demonstrate that multi-party competition in legislative elections and multi-candidate competition in presidential elections are the joint product of many social cleavages *and* a permissive electoral system. Clark and Golder (2006: 700) provide a more specific test that shows that the marginal effect of social heterogeneity is positive and significant once the district magnitude becomes sufficiently permissive. In a parallel analysis on presidential elections, Golder (2006: 45) also shows that whereas social heterogeneity significantly increases the number of candidates competing in runoff (that is, permissive) presidential elections, it does not have any effect in plurality rule (that is, restrictive) elections. In spite of its seemingly robust findings, however, this research agenda raises a number of important substantive and methodological questions about the impact of electoral rules on party competition.

Conceptualization of cleavages, reverse effects, and equilibrium outcomes

Using ethnic fragmentation as a proxy for social heterogeneity has several advantages for the purpose of statistical testing. First, there are several available indexes of ethno-linguistic fragmentation that cover most countries of the world. Moreover, this measurement surely captures divisions that are exogenous to the electoral structure, thus avoiding the potential endogenous effect of the latter on the actual political divisions in a country.⁹ Even so, reducing the sociological determinants of electoral parties to the number of ethno-linguistic cleavages is problematic.

As Lipset and Rokkan (1967: 6–7) and Duverger (1957: 260–261) argued, ethnicity is just one among many other sociocultural factors such as region, class, and religion that can lead to political divisions. Some cleavages, such as those that relate to regime support or foreign policy, are purely political and unrelated to the social structure (Duverger 1957: 260; Lijphart 1999: 85). More importantly, even if we agree that political divisions are primarily determined by preexisting social structures and that ethnic fragmentation is the most relevant division, it is conceptually implausible to think that social cleavages automatically translate into political cleavages. Social cleavages need to be transformed into explicit political conflicts to affect party formation and competition (see Coppedge 1997; Taagepera 1999).

The problem is that it is not clear what determines the politicization of underlying social divisions. Several countries in Latin America, for instance, have had ethnically divided societies during democratic periods before and after the expansion of electoral democracy in the region in the 1980s. Yet it was not until the 1990s, when ethnicity became a politically salient issue, that new ethnic-based parties were formed and managed to win significant portions of the vote. Interestingly, there is scant evidence that variations in district magnitude worked as an intervening variable to account for changes in the supply of ethnic-based parties (Van Cott 2003: 16). It seems obvious that both the opportunity set and strategic skills of political leaders were crucial for explaining when new ethnic parties emerged and when they became successful in exploiting a preexisting social division.

Interactive models apparently assume that social cleavages determine both the “demand” of voters for parties and the incentives of political elites to “supply” them. Yet a reverse causality is possible (see Colomer and Puglisi 2005). Political elites may themselves activate the demand of voters by launching parties that turn a dormant dimension of conflict into an active one. Moreover, political entrepreneurs might introduce new dimensions of conflict unrelated to the social structure. Lipset and Rokkan (1967: 3) acknowledged (although they did not explore) this possibility when they hypothesized that “parties themselves might establish themselves as significant poles of attraction and produce their own alignments independently of the geographical, the social, and the cultural underpinnings of the movements.”¹⁰

If strategic politicians have an influence in the politicization of latent cleavages, it means that the number of relevant dimensions of conflict in a country may change over time. However, social cleavages in general and ethnic cleavages in particular are relatively constant, so they cannot account for variations in the number of parties over time within a single country. The problem is apparent when the electoral system remains unchanged. The United Kingdom had an average of 2.3 electoral parties from 1950 to 1970, but between 1974 and 1997 this average increased to 3. Obviously, neither ethnic cleavages nor the electoral system can explain the shift because both remained constant across these periods. Within the same structure of social cleavages, there are also cases in which both the electoral system and the number of electoral parties have changed over time, but not in the expected direction. For instance, in Peru—a country with a relatively high degree of ethnic fragmentation—the number of electoral parties has decreased and increased over time. However, this country experienced a decrease in the number of electoral parties just when the electoral system became more permissive (from 1993 to 2000) and an increase when it became more restrictive (from 2000 to 2006).¹¹

The nature of the interaction between social heterogeneity and electoral structures is unclear in recent reformulations of Duverger's theory. Most works attempt to retain the deterministic nature of the effects of plurality systems by proposing that they should discourage the formation of more than two parties, *regardless* of the level of heterogeneity of the social structure (Ordeshook and Schvestova 1994; Clark and Golder 2006). Cross-national statistical tests do show that social heterogeneity (as measured by ethnic heterogeneity) has no significant effect when district magnitude equals one (Clark and Golder 2006: 700). But does it mean that we should expect the same number of parties whatever the number of dimensions of political conflict in the country? This expectation would be inconsistent with the social choice literature.

As Taagepera and Grofman (1985: 346) point out, Duverger's Law cannot apply to a multi-dimensional space because in such scenario there is no median voter ideal point to which parties will converge—leading, in equilibrium, to a two-party competition. For this reason, these authors propose and provide evidence that, even assuming a reductive effect, a plurality system with more than one issue dimension should have between 2 and 3 parties (Taagepera and Grofman 1985: 344).¹² In other words, the impact of the number of dimensions of political conflict on the number of parties should lead to the conclusion that in no case can electoral rules (whether restrictive or permissive) be considered independent causal factors.

This perspective suggests a different view of anomalies in the comparative analysis of plurality systems. India may have more than two parties simply because it has multiple dimensions of political conflict, not because of the existence of a median party in most districts, as Riker would have it. As I pointed out earlier, it has been argued that the reason why Canada has more than two parties is that geographically local parties are the main parties in some provinces, while they are third or lower at the national level. Yet it is also true that the larger number of parties in Canada correlates with the fact that it has more than one dimension of political conflict (see Lijphart 1999) and a relatively high level of ethnic heterogeneity. This may explain why New Zealand, with only one relevant dimension of conflict and a lower level of ethnic heterogeneity, had an average of 2.5 electoral parties from 1946 to 1993 (the year it shifted to a mixed system), whereas Canada had 3.1 electoral parties from 1949 to 2000. Moreover, it may also be the case that the geographic concentration of the vote in Canada is itself determined by the structure of social cleavages at the provincial level, as some tests have shown (see Tanaka 2005: 73–76).

If the impact of sociopolitical heterogeneity clarifies differences among plurality systems, it makes the comparative analysis of permissive electoral systems with PR or mixed formulas more uncertain. Since sociopolitical heterogeneity (however measured) and the nationwide district magnitude may vary in different and not necessarily consistent ways, it is not clear what expectations

we should have when comparing countries that have different scores in both sociopolitical heterogeneity and district magnitude.

A final note is appropriate about equilibrium outcomes under different electoral rules. Evidence in support of the interactive model is weaker among new democracies than among established democracies (Clark and Golder 2006: 702). Since both voters and party elites need time to learn and internalize the effects of electoral rules, it may be that many early elections in new democracies are off-equilibrium. The underlying problem, however, transcends the dichotomy between new and old democracies. No matter how many years a democratic regime has lasted since its inauguration, we have no theory to predict exactly how many elections are necessary for an electoral system to produce an equilibrium number of parties. And even when an electoral system has been in force for several years, off-equilibrium elections may occur, sometimes triggering a process of electoral reform.¹³ This puts into question how units of observation are selected in statistical analyses when comparing electoral systems of widely different duration and elections (in new or old democracies) that take place at various times during the life of these systems.¹⁴ It also suggests that electoral outcomes and electoral system change may be closely interrelated.

The origins and reform of electoral systems

One of the earliest criticisms of Duverger's theory was that the French jurist got the causality wrong: It is not electoral systems that cause party systems but the other way around (see Grumm 1958). In fact, Duverger himself acknowledged (1957: 232, 270) that two-party or dominant party systems are likely to choose SMDP, whereas multiparty systems are likely to opt for PR formulas. This endogeneity is not fatal, however, for the study of electoral system effects.

The relationship between electoral and party systems is one of mutual causation (Taagepera 2007: 7). Parties are temporally prior to electoral systems; they are the agents that create them. At the same time, parties choose a particular electoral system over other alternatives because they expect to benefit from it. The effects of electoral systems can nevertheless be studied in isolation from the intentions of strategic politicians. Electoral outcomes result from the interaction between electoral rules and environmental parameters (number of voters, number of parties competing, etc.) whose features cannot be fully anticipated by institutional designers at the time of choice (Shvetsova 2003).

All these reasons justify the separate analysis of electoral system effects. The relevant question is whether electoral rules should be analyzed *only* as independent variables. This perspective would be acceptable if electoral systems were always stable; but they are not. It is for this reason that the recent development of studies on electoral reform is a welcome event. This literature makes a valuable contribution to understanding not only electoral system change but also institutional change more generally. I will address three main issues: the frequency of electoral system change, the need to expand strategic models to account for the diverse conditions of electoral reform, and the multiple causal links that should be observed in the explanation of electoral system choice.

The frequency of electoral reform

It is part of the conventional wisdom of electoral studies that electoral reform is a rare event. Given the costs of information and learning implied in institutional change, it is argued that electoral reform tends to be infrequent and, when it occurs, incremental. As Shugart and Taagepera (1989: 218) summarize, "familiarity breeds stability."¹⁵ Yet the stability of electoral systems is not a constant; it varies significantly across countries and regions.

Whether we see electoral systems as stable or unstable depends on our prior assessment of what events qualify as electoral change. Perceptions of stability usually restrict the notion of electoral change to a shift from one of the main electoral formulas to the other. Under this definition, Lijphart finds evidence of electoral stability in that no country changed from plurality to PR or vice versa in his study of 27 stable electoral democracies from 1945 to 1990 (Lijphart 1994: 52). In a similar vein, but including presidential elections, Katz (2005: 58) observes that only 14 major changes in the electoral formula to elect legislators and executives have occurred among all countries that have had uninterrupted democratic regimes between 1950 and 2001. These observations of electoral stability do not take into account the relatively more frequent changes that occur at the level of district magnitude, assembly size, or legal thresholds. The problem is that these reforms may have equally (or more) significant effects on party competition to a change in the electoral formula.

It is also important to note that the prediction of stability and inertia in the basic components of electoral systems is almost always restricted to analyses of established democracies. The picture changes dramatically if we include new democracies. According to Negretto (2013: 25–29), merely between 1978 and 2008, new democracies in Latin America underwent 45 major electoral changes in their formulas to elect presidents and systems to elect deputies, which includes shifts in the electoral formula and changes of more than 25 percent in the average magnitude of the districts and the size of the assembly. Other authors (Birch *et al.* 2002) have found a similar pattern of electoral system instability in Eastern Europe, particularly during the 1990s.

The perception that electoral rules change little and rarely is probably responsible for the scant attention that electoral reform has received until recently. Except for the analysis of the historical origins of the shift to PR in Western Europe, few scholars have focused on electoral reform and change as a distinct aspect in the analysis of electoral systems. The electoral reforms in France in the 1980s, in New Zealand, Italy, and Japan in the 1990s, and the proliferation of electoral changes in the new democracies of Latin America and Eastern Europe have dispelled the perception that institutional stability is the norm across countries and regions. However, how to assess the frequency of reforms and the conditions that affect the stability of electoral systems has yet to be specified in a more systematic way.¹⁶

The strategic model of electoral reform

Rational choice theories, particularly those that emphasize the distributive consequences of institutional arrangements, are well equipped to explain electoral reform. Naturally, if the creation and maintenance of institutions reproduce the existing distribution of power resources among self-interested actors, institutions should not remain stable if the interests or resources of these actors change. It follows from this perspective that governing parties would tend to replace existing electoral rules when these rules no longer serve their interests or when parties that lost under the existing rules gain sufficient influence to induce a reform. This framework of analysis has been developed by several authors, among them Boix (1999), Benoit (2004), and Colomer (2004, 2005).

In its more general, comparative formulation, the power-maximization model postulates that whereas large or ascending parties support restrictive electoral rules, small or declining parties tend to favor the adoption of inclusive electoral rules, such as PR for legislative elections (Geddes 1996; Colomer 2004, 2005) and more-than-plurality rules for presidential elections (Negretto 2006). The strategic model has also been specified in more substantive terms to explain the historical origins of major electoral reforms, such as the adoption of PR rules in Europe at the beginning of the twentieth century (Rokkan 2009 [1970]; Boix 1999).

Scholars using different formulations of the power-maximization model have shown convincing qualitative and quantitative evidence that support its main propositions. Yet there is a lively and growing debate about its applications and explanatory reach. One version of this debate questions whether there is a unique set of conditions that induces parties to reform the electoral system for their own advantage. Another version questions some of the basic assumptions of strategic models in relation to the level of information of reformers, the scope of partisan self-interest, and the level of control of the political elite over the process of electoral change.

An important part of the theoretical debate about electoral reform has taken place in the context of historical explanations for the shift from majoritarian to proportional systems of election in Western Europe at the dawn of the twentieth century.¹⁷ The standard explanation, initially proposed by Lipset and Rokkan (1967) and Rokkan (2009 [1970]) and later generalized by Boix (1999), is that the shift occurred in a changing electoral arena as a result of the threat that emerging socialist parties posed to established parties when the latter were divided. But Rokkan also argued (2009 [1970]: 157) that in the case of early reformers, such as Denmark, Switzerland, and Belgium, the drive to reform was not the socialist threat but the desire to protect minorities in heterogeneous societies. Most studies on the adoption of PR have neglected the analysis of this alternative route.

According to Calvo (2009), Rokkan was right in identifying a different road to PR but wrong in proposing that it was the desire of established parties to protect minorities that led to reform in these cases. Rather, it was the established parties' strategic interest in reducing the vote-seat distortions that affected them when new parties entered the electoral arena due to the expansion of suffrage and when the territorial distribution of the vote was asymmetric (Calvo 2009: 256). Calvo finds evidence to support his argument in historical cases such as Belgium and Denmark but more generally claims that the alternative path may be fruitfully explored in all those countries where socialist parties never posed a serious threat to established elites.

One can extend the idea of alternative routes to strategic reform even further. It is apparent that outside Western Europe PR was adopted under historical conditions different from both the "socialist threat" and the partisan biases created by three-way competitions in majoritarian systems with an asymmetric distribution of the vote across districts. In many countries of Latin America PR was often adopted independently of the expansion of suffrage and in the absence of a rising new party, let alone one with a socialist ideology. For instance, in countries like Uruguay and Colombia, no significant third party existed before the adoption of PR. The reform occurred at a time when factional divisions within the dominant party enabled the opposition party to force a negotiation or pass the reform in alliance with the challenger faction.¹⁸ Moreover, in some instances PR was adopted by dominant parties to fulfill pre-election commitments or by non-elected actors, such as the military, to crack the power of majoritarian populist parties.

Within the discussion about the origins of PR, some authors have argued that while politicians act strategically, they do not always make decisions under conditions of complete information. The "socialist threat" theory assumes that old parties had sufficient information to act preemptively, and shifted to PR to minimize their seat losses. But, as Andrews and Jackman (2005: 71) observe, this explanation discounts the role of uncertainty, which should have been significant in an electoral arena whose stability was affected by the expansion of suffrage. These authors claim that if we take uncertainty seriously, strategic politicians should have adopted PR only when the seats-to-votes ratio of the largest party decreased under a majoritarian system, regardless of whether the party was new or old, conservative or socialist. And they find historical evidence to support this claim (Andrews and Jackman 2005: 80–81).

Contemporary debates about electoral reform have also shown that while political elites may want to reform only if they expect to be better off under alternative rules, they are not always

able to do so. In certain situations, political elites may be forced to initiate an electoral reform in response to systemic failures induced by the existing electoral system. According to Shugart and Watterberg (Shugart 2001; Shugart and Watterberg 2001: 571–577), electoral systems that promote extreme forms of concentration of institutional power in the plurality party, party system fragmentation, personalization, or party centralization tend to create pathologies that result in systemic failures that force politicians to reform. This framework has been used to persuasively explain recent cases of the adoption of mixed systems in the 1990s in New Zealand, Italy, Japan, and Venezuela. In a similar vein, Sakamoto (1999), Negretto (2008), and Renwick (2011) emphasize that, contrary to the assumption of most strategic models, political elites do not always have full control over reform. This lack of control may occur because in a democratic context elites are subject to citizen demands and fear electoral retribution if they do not respond with reforms that satisfy the people or simply because reforms are sometimes pushed forward by citizen-initiated referendums, as was the case with the 1993 electoral reform in Italy.¹⁹

Electoral rules are distributive institutions par excellence. They determine how many actors can compete with some probability of success and who may win or lose given the expected popular vote in an election. From this perspective, it makes perfect sense to assume that professional politicians would attempt to reform the existing electoral system when it affects their partisan interests and, if they have sufficient power, choose those rules that they think would benefit them most. Proof that this basic analytic framework is realistic is that it has worked better than others to explain electoral reform in a wide variety of settings. The strategic model needs several adaptations, however. Comparative historical evidence suggests that various conditions may lead to the same outcome. In addition, some of the model's assumptions must be relaxed to account for electoral reform in different contexts. It is obvious that uncertainty often restricts information about what alternative system would benefit established parties, that politicians must sometimes respond to a performance crisis of the existing electoral system, and that the political elite do not always have full control over reform. The challenge for future research in this area is, of course, to show the existence of different routes to electoral reform while accommodating the explanation within a single analytical framework.

Causal links: triggering events, reform proposals, and electoral choice

A comprehensive theorization of electoral reform must specify the causal mechanisms and links that should be observed in qualitative case studies if all the assumptions and propositions of a theory are correct. Some of the existing explanations provide theory-derived propositions that are then tested in a statistical model without showing how qualitative evidence coming from cases included in the sample might deviate from the empirical implications of the theory. Different explanations propose specific causal chains of events. In general, however, almost all theories imply certain observations about (1) the nature of the event that triggers the reform, (2) the identity and interests of the actors proposing and adopting new rules, and (3) the approval process.

If it is true that the adoption of PR at the turn of the twentieth century was a preemptive strategy by established parties to avoid losing seats as new parties emerged, we should observe that new parties were indeed increasing their electoral support before the reform. Yet the Belgian Socialist Party was never in a position to win an election before the 1899 reform (Calvo 2009: 268). We should also observe that the reform proposal originates with the established elites. In Germany, however, it was a government of the left that adopted PR in 1919 (Andrews and Jackman 2005: 77). Moreover, it is implicit in the “socialist threat” theory that established parties had control over the process of reform. Yet in Switzerland PR was adopted by a 1918

referendum on a citizen-initiated proposal of constitutional reform sponsored by the Social Democrats (Lutz 2004).

Statistical evidence about the average or marginal effects of certain variables cannot substitute for a careful analysis of the implied causal links and mechanisms. Quantitative evidence should be complemented with qualitative historical evidence (Kreuser 2010). In-depth historical analysis, however, can also be ambiguous in specifying causal mechanisms. For instance, Lipset and Rokkan present the mechanisms leading to the adoption of PR in the “anti-socialist” route in two different ways. One version postulates a convergence of interests between incumbents and challengers: “the rising working class wanted to lower the threshold of representation to gain access to the legislature, and the most threatened of the old-established parties demanded PR to protect their positions against the new waves of mobilized voters under universal suffrage” (Lipset and Rokkan 1967: 32). But in a different rendering of the same cases, they emphasize the predominant interest of incumbents: “the decisive moves to lower the threshold of representation reflected divisions among the established *regime censitaire* parties rather than pressures from the new mass movements” (Lipset and Rokkan 1967: 34). Note that if the interests of established and new parties converged, we should observe a consensual process of reform in which both supported the adoption of PR. If, however, PR primarily served the interest of the threatened elite, we should expect old parties to be enthusiastic supporters of PR and challengers (who may expect to become the future majority) to be reluctant about the reform or against it. To be sure, it may be that some cases fit one description more than others.

The general point is that any theory on electoral reform should be as detailed as possible about its observational implications. If different paths to reform exist, it could be a very demanding task to accommodate them in a single analytical framework that at the same time provides for clear causal links and individual mechanisms. But there is no other way to make progress on a research agenda about institutional change that could make sense of both general trends and relevant historical outcomes.

Conclusions

Electoral rules are important political institutions not only because they may have a direct effect on party formats but also because of their potential impact on other relevant outcomes, such as electoral participation, coalition formation, congruence between the preferences of citizens and policy makers, government stability, and corruption, among others. The electoral rules that matter are obviously not just the electoral formula, district magnitude, or the assembly size. Rules of candidate selection and details of ballot design may affect crucial aspects of party organization, the behavior of party candidates in elections, and the decisions that party representatives in congress make. There is an important and growing literature on all these areas. Yet it is still the research agenda on the party system effects of electoral rules that contains the core contributions of electoral studies to the comparative analysis of political institutions.

There is no doubt that progress has been made by electoral studies in understanding how electoral systems work. Electoral scholars are also more aware than they were in the past of the conditional and interactive effects of electoral rules in relation to other institutions and non-institutional factors. However, as regards the old question about how electoral systems affect party systems, the theoretical development of key causal mechanisms is less well developed than the statistical techniques used to test precise but simplistic explanatory models. Many key conceptual problems remain obscure or unexplored, such as the influence of strategic politicians on the political activation of latent social cleavages, the process of party formation in response to both preexisting cleavages and the existing electoral rules, possible variation in the number of

dimensions of political conflict over time, and mutual causation processes between party system effects and electoral system change.

Studies on electoral reform have advanced our knowledge about the conditions under which electoral systems remain stable and the conditions under which they might change, thus putting into question the universal assumption of electoral stability. Yet a comprehensive analytic framework for understanding the frequency of electoral change in a variety of contexts has yet to be developed. Strategic models of electoral reform have provided valuable knowledge about the historical origins of electoral rules and the processes that lead to their replacement. These models must, however, account for the existence of diverse historical paths leading to similar reforms, and accommodate the fact that political elites face various levels of uncertainty over which rules are most beneficial to them, that under certain conditions they must address systemic failures produced by the electoral system, and that they do not always have perfect control over the process of choice.

The most important task ahead is to build a unified theoretical framework linking sociopolitical heterogeneity and electoral structures to the number of parties and the number of parties to the politicization of cleavages and electoral reform. This is the kind of theory suggested by a joint reading of Duverger and Rokkan, the two founders of electoral studies in contemporary political science. The task is demanding but not unlike the challenge facing institutional studies in general, where until now the analysis of the effects and maintenance of institutions has remained separated from the study of origins, in spite of the obvious interaction between the two processes.

Notes

- 1 I would like to thank Octavio Amorim Neto, Ernesto Calvo, Josep Colomer, Marcelo Escolar, and the editors of this Handbook for their comments on a previous version of this chapter.
- 2 Throughout this chapter I will use the term electoral parties to refer to the parties winning votes in an election and legislative parties to refer to the parties winning seats in parliament or congress. By number of parties I mean the “effective” number of parties calculated according to the Laakso–Taagepera formula (1979) unless otherwise indicated.
- 3 The effect need not be perfectly linear, however.
- 4 I concentrate here on presidential elections, the most obvious institution that might influence legislative elections. But other institutions have been proposed, such as universal suffrage, federalism, and assembly size (see Blais and Carty 1991).
- 5 As in the case of parties, the number of presidential candidates is usually calculated using the Laakso–Taagepera (1979) index.
- 6 As Lipset and Rokkan plainly state (1967: 30), “in most cases, it makes little sense to treat electoral systems as independent variables and party systems as dependent.”
- 7 Only Powell (1982) specified this relation as additive.
- 8 On the different alternative measures of ethnic heterogeneity used in this literature, see Stoll (2008).
- 9 In fact, the number of politicized dimensions of conflict could be endogenous to both electoral structures and political competition. However, as Taagepera and Grofman point out (1985: 349), for a given number of political divisions in a country, widely different numbers of parties remain possible, so they do not stand in a circular relation to each other.
- 10 Note that this hypothesis may put into question a central proposition of Lipset and Rokkan’s work, the “freezing” hypothesis or the idea that party systems reproduce for long periods of time the fundamental cleavage structures that initially shaped them.
- 11 In particular, the average effective number of electoral parties in Peru went from 4.07 when the average district magnitude was 6.92, to 3.64 when the average district magnitude increased to 120. Then in 2001 and 2006, the average effective number of electoral parties increased to 6.94, in spite of the fact that the average district magnitude decreased to 5.
- 12 Although Taagepera and Grofman focus on legislative parties, their argument applies even more strongly to electoral parties.

- 13 This is what happened in some cases when at the turn of the twentieth century a third party emerged in restrictive electoral systems. Either a re-equilibration occurred or political elites shifted to PR.
- 14 The cross-sectional database of Cox (1997) includes the 1985 election in Sweden, which took place under an electoral system created in 1970, along with the 1985 election in Honduras, which was the first election regulated by a new system. On the other hand, the longitudinal database of Golder (2005) includes elections in France from 1946 to 1997. Yet France had five different electoral systems during that period, some of which regulated only one (1946) or two (1951 and 1956) elections.
- 15 A similar argument is found in Arend Lijphart (1994: 52).
- 16 Understanding the conditions that make some electoral systems more unstable than others would also shed light on the problem of why some proposed reforms are adopted while others fail.
- 17 I will consider here only political explanations that while grounded in a historical context have the potential to be transformed into a general comparative framework of electoral reform. I leave aside non-political explanations, such as that of Cusack, Iversen, and Soskice (2007), which is based on specific economic conditions in the early development of labor markets.
- 18 See Buquet and Castellano Christy (1995: 114–119) on how factional divisions within the Colorado Party created the political situation that forced this party to negotiate the adoption of PR with the Blanco Party in 1917 in Uruguay, and Mazzuca and Robinson (2008: 313) for an analysis of the alliance between the challenger faction of the Conservative Party and the Liberals that made possible the introduction of PR in 1929 in Colombia.
- 19 The 1993 electoral reform in New Zealand was also approved in a popular referendum. However, different from Italy, where the referendum is available on popular demand, in New Zealand only the government initiates the referendum. See Sakamoto (1999: 424–425).

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10

PARTIES AND PARTY SYSTEMS

*Noam Lupu*¹

Although political parties are rarely mentioned in constitutions, there is not a single modern democracy that does not have them. Indeed, scholars have long noted their indispensability to mass, representative democracy (Key 1958; Rosenblum 2008; Schattschneider 2003 [1942]; Stokes 1999).² Parties resolve collective action and social choice problems in legislative decision-making (Aldrich 1995). They serve as heuristic devices and bundle policy dimensions for voters who lack the time or inclination to learn about every issue and every candidate (Downs 1957; Popkin 1991). They structure electoral competition with a stable menu of options that makes uncertain elections more predictable and extends the time-horizon of politicians (Mainwaring and Scully 1995). And they discipline politicians and hold them to account even when voters cannot (Alesina and Spear 1988).

This has long focused scholarly attention on parties and party systems, generating a voluminous body of studies. But the task of theorizing party politics is a daunting one: parties are at once institutions that persist over time and dynamic organizations that change constantly. It is not surprising, then, that fundamental questions about this important topic of political research remain far from settled. For instance, scholars continue to debate why and how parties emerge, why some new parties survive, and why established ones collapse. Moreover, early studies of political parties extrapolated from the unique experiences of advanced democracies in Western Europe and the United States. In some cases, their insights have helped us understand subsequent developments in newer democracies, but in many cases their expectations have clashed with empirical reality. Even within the advanced democracies, parties and party systems changed in ways unanticipated by earlier theories. Booming cross-national survey data also made it possible to subject long-accepted theoretical assumptions to empirical testing, with (at best) mixed results.

Students of party politics have approached the discrepancy between received theories and changing empirical realities in two ways. The *splitters* argue that theories derived from the experiences of advanced democracies at particular moments fail to apply to other places or times. In these times and contexts, alternative explanations performed better at explaining party formation, competition, or strategy. Studies of this type have vastly enriched our understanding of party politics and expanded the kinds of factors we know *can* affect parties and party systems. Whereas early studies of political parties focused on exogenous cleavages or institutions to explain stasis and change, more recent scholars have noted how parties and party systems persist

or transform as a result of endogenous factors. The very strategies parties employ in response to particular threats can affect their survival and the stability of the party system. Similarly, scholars have found that parties employ different types of appeals to mobilize voters, depending on various contextual and temporal factors.

Scholars who are *lumpers* have taken a different approach to grapple with puzzling empirical developments: they try to generate broad theoretical frameworks that incorporate prior theories of party politics and also help us understand diverging empirical patterns.³ These kinds of frameworks specify the circumstances under which we would expect prior theories to hold and those under which parties and party systems may develop and behave quite differently. This approach thus seeks to simultaneously understand both why received theories explain a certain subset of cases and also why and how other cases diverge. The timing of regime change and party formation may make some parties and party systems differ from others, or they may conform to received theories in contexts of low political uncertainty but diverge from those expectations dramatically in highly uncertain political settings.

This chapter traces these parallel scholarly developments by focusing on three unsettled questions in the study of parties of party systems: (1) why parties and party systems persist or change over time; (2) how parties appeal to voters; and (3) why parties and party systems in developing democracies differ from those in older ones. In each case, recent studies have highlighted new empirical puzzles or brought new data to bear on untested claims. Offering new explanations and findings, they have expanded our theoretical scope by suggesting new kinds of factors that shape parties and party systems. And they have offered broader theoretical frameworks that help us make sense of both prior contributions and puzzling new developments.

Origins, stasis, and change

How we understand why parties and party systems change or perpetuate over time depends in part on where we think they come from. Early studies of political parties tended to fall into one of two perspectives. One set of scholars focused their efforts on explaining party systems they viewed as largely static over time. In their seminal study, Lipset and Rokkan (1990 [1967]) famously posited that rare critical junctures—the Reformation, democratization, or the Industrial Revolution—froze European party systems in place around a particular social cleavage that came to dominate others (see also Bartolini and Mair 1990).⁴ “The party systems of the 1960s,” they noted, “reflect, with few but significant exceptions, the cleavage structures of the 1920s” (Lipset and Rokkan 1990 [1967]: 134). Although later scholars suggested a further, post-materialist critical juncture might again have reshaped European party systems (e.g. Inglehart 1977), the notion that party systems coalesce at key junctures and thereafter remain fixed seemed to characterize developed democracies fairly well.

A similar perspective prevailed within the study of U.S. politics. Observers of the U.S. party system noticed its long periods of stasis, punctuated by major shifts at critical elections or realignments (e.g. Beck 1974; Burnham 1970; Key 1955, 1959; Sundquist 1983). Despite blistering critiques of this characterization of U.S. party politics (Mayhew 2004), scholars continue to defend the perspective that party systems remain stable for long stretches between critical junctures (e.g. Merrill *et al.* 2008; Norpoth and Rusk 2007).

Other scholars, beginning with Duverger (1951), explained the origins and long-term stability of these party systems in institutional terms (e.g. Aldrich 1995; Calvo and Hellwig 2011; Ezrow 2010). As long as institutions like electoral rules remained in place, parties and party systems that had adapted to those institutions would prevail. Voters would not waste their votes on new, uncompetitive options, and elites would coordinate around and join the existing

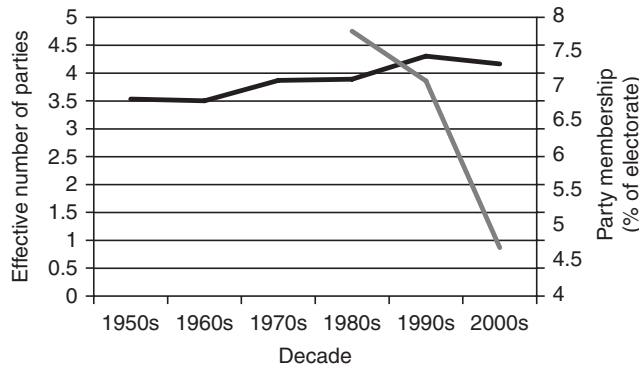


Figure 10.1 Electoral fragmentation in advanced democracies, 1950–2010

Notes: Values on the black line represent the average effective number of electoral parties across advanced industrial democracies in each decade. The effective number of electoral parties is calculated using the index developed by Laakso and Taagepera (1979) with the correction for an “other” category (Taagepera 1997). Values on the gray line represent the average across Western European countries of party members as a proportion of the electorate.

Source: Bormann and Golder (2013); Van Biezen *et al.* (2012)

competitive parties (Cox 1997). Stable institutions would thus generate a stable configuration of parties (Lijphart 1994; Powell 2000; Sartori 1976). Only major institutional changes would upend the frozen party system.

An alternative perspective, building on the seminal contributions of Downs (1957), viewed party systems instead as fundamentally dynamic. Spatial models of party competition assume that strategic parties immediately internalize changing electoral incentives, whether they take the form of new electoral rules, shifting public opinion, or increased competition (e.g. Hinich and Munger 1994; Roemer 2001; Schofield 2008). These exogenous factors change the optimal strategies of parties, and spatial models assume that parties quickly adapt to these new circumstances. But while these simplifying assumptions allowed spatial models to fare well in explaining the equilibrium positioning of parties, they often over-predicted change. In advanced democracies, major parties seemed slow to change even when they knew they were uncompetitive (Przeworski and Sprague 1986; but see Sánchez-Cuenca 2004).

Contradicting both the stasis predicted by cleavage-based accounts and the dynamism of the spatial model, parties and party systems in the advanced democracies did slowly change during the 1980s and 1990s (Kitschelt 1994). Scholars also increasingly observed new parties emerge (Hug 2001): Niche parties took up narrow non-economic issues (Wagner 2012), and radical right parties espoused anti-immigrant positions (Kitschelt 1996; Mudde 2007). At the same time, old parties seemed to transform their organizations and shift their mobilization strategies, detaching from voters (Dalton and Wattenberg 2000; Katz and Mair 1995, 2009).⁵ Some parties and party systems even collapsed unexpectedly (e.g. Cox *et al.* 1999; Golden 2004; Vowles 1995; Wellhofer 2001). Figure 10.1 illustrates both the rising number of competitive parties in advanced democracies since 1950 and declining party memberships.

In the newly democratizing countries of the developing world, similar changes occurred much more rapidly. In some new democracies—particularly those where most parties were also new—it took some time for voters to learn about the parties and for politicians to form strategic coalitions.⁶ Fairly quickly, voters and elites coalesced on a subset of parties that seemed particularly competitive, and irrelevant parties disappeared (Tavits 2005; Tavits and Annus 2006).

But in others, party systems did not seem to coalesce. When Mainwaring and Scully (1995) astutely classified some party systems as institutionalized and others as inchoate (see also Mainwaring and Torcal 2006), they held out hope that repeated democratic competition would reinforce the institutionalized ones and strengthen the inchoate ones (see also Dix 1992). “Democratic stability,” they posited, “encourages institutionalization” (Mainwaring and Scully 1995: 28). The process of “shaking out” the initial party system (Bernhard and Karakoç 2011: 3) would eventually yield a stable system akin to the “frozen” party systems of Western Europe and the United States.

Decades later, though, many party systems in the developing world continue to be weak and inchoate. It seems that systems can also freeze into an equilibrium of fluidity and incoherence, not just an institutionalized and stable one. Even so, some parties and party systems subsequently underwent tremendous changes, veering from their expected paths. In Latin America, for instance, indigenous parties emerged and sometimes succeeded electorally (Madrid 2012; Van Cott 2005). As in some advanced democracies, some previously institutionalized parties and party systems in the developing world weakened or completely collapsed (Lupu 2014a; Morgan 2011; Seawright 2012). In some cases, these developments responded to the kinds of variables that similarly concerned early party scholars: institutional reforms like electoral rule changes (Hicken 2009; Remmer 2008; Roberts and Wibbels 1999; Van Cott 2005), shifts in social cleavages (Evans 2006; Stoll 2013; Tavits and Letki 2014), or specific critical junctures (Roberts 2013; Slater 2010). Yet, in others, neither the underlying social cleavage nor the electoral rules can account for the transformations of parties and party systems.

Change in these cases responded instead to the capacities, choices, and strategies of the parties themselves. Recent studies of these cases highlight that parties and party systems sometimes change *endogenously*. Without changing the electoral rules or the basic societal cleavages, party systems sometimes transform from within.

One set of these studies examines the role of historical legacies in guiding the capacities and choices of parties. In post-Soviet transitions to democracy, it was the capacity of the communist successor parties that helped determine whether party competition would stabilize or fluidity would prevail (Grzymala-Busse 2002). Where communist successor parties inherited coherent and credible organizations, they stabilized both party competition and the democratic regime. In Sub-Saharan Africa, authoritarian rulers played a similar key role. Authoritarians who had secured their power by incorporating—rather than supplanting—local power brokers were better able to control the transition process, win founding elections, and force opposition leaders to coalesce into stable party organizations, institutionalizing a competitive party system (Riedl 2014; see also Hicken and Kuhonta 2011, forthcoming). These explanations have gone a long way toward helping us understand why some new democracies quickly institutionalize stable party competition whereas others fluidly cycle through weak and ephemeral parties.

Whereas these studies focus on the inherited capacities of parties, other studies of endogenous change focus on party strategies. In Western Europe, how the major mainstream parties responded to electoral threats from emerging niche parties determined whether these new entrants would succeed (Meguid 2008).⁷ When mainstream parties chose to adopt the new issues raised by niche parties, they successfully stymied the competition; but when they chose to dismiss or attach the new issues, niche parties grew. In Latin America, the types of appeals employed by new indigenous parties appear to explain their electoral success (Madrid 2012). Those that combined ethnic and populist appeals succeeded in building a winning electoral coalition, whereas those that appealed to voters only along ethnic lines made little inroad.

How parties adopted market-oriented economic reforms in Latin America also helps to explain their electoral survival or devastating failure (Lupu 2014a). During the 1980s and 1990s,

parties across the region bowed to international pressure to implement economic policies inconsistent with their traditional positions, provoking internal conflicts and forming strange-bedfellow alliances with historic rivals. This diluted their brands and eroded voters' attachments to these parties. Without the assured support of a partisan base, these parties become more susceptible to collapse, upending even the most stable party systems in the region. The policy choices of individual parties can thus have enormous impacts on their survival and the nature of the party system. Major changes in parties and party systems can emerge endogenously from the choices and capacities of the parties themselves.

Whether to explain stasis or change in parties and party systems, scholars have long been building upon the insights of early studies that focused their explanations on influences outside the party system—institutions, social cleavages, and their interaction. But recent scholars have highlighted that endogenous factors affect parties and party systems across countries and over time.⁸ In new democracies, party capacities inherited from authoritarian rule have played key roles in stabilizing party competition. In Europe and Latin America, party systems have also endured or changed because of how parties themselves chose to confront new challenges ranging from niche parties to international pressure for economic reform. Parties and party systems may for long stretches of time appear frozen at an institutional equilibrium or around a particular historic cleavage. But scholars are increasingly recognizing that such stasis is also maintained by ongoing endogenous policy choices. Even without electoral reforms or major social transformation, parties' policies themselves can transform parties and party systems.

Voters, parties, and party systems

If parties and party systems are indeed dynamic—either reproducing static equilibria or changing over time—then how are those dynamics perceived by voters? After all, parties are intermediaries: they mediate the relationship between voters and government institutions (e.g. Caillaud and Tirole 2002; Rosenblum 2008). By influencing the electoral success of the parties, citizens can shape the policy outcomes of legislative bodies. Parties may play a more important intermediary role in some systems than in others (Samuels and Shugart 2010); where elections are candidate rather than party centered, or where parties are weakly institutionalized, they may be incapable of successfully mediating voter–government interactions. But this is one of the key roles of parties that makes them indispensable for modern mass democracies.

The empirical question is whether in fact citizens and parties behave this way, even under the most hospitable institutional and structural arrangements. If parties indeed act as political intermediaries, they should act on the preferences of voters. Indeed, spatial models assume that party competition incentivizes parties to change in tandem with public opinion. And cleavage-based accounts expect that parties will represent discrete segments of a salient social cleavage. But do parties in fact behave this way? And do voters punish those that do not?

Empirical studies in advanced democracies suggest that parties do sometimes change their positions in response to public opinion. In Western Europe, mainstream parties generally shifted the ideological tone of their manifestos in response to changes in citizens' preferences (Adams *et al.* 2004, 2006, 2009; McDonald and Budge 2005).⁹ And niche parties seem to have changed their policy positions when the preferences of their current supporters changed (Ezrow *et al.* 2011). On the other hand, in the United States the two major parties seem to have become more extreme than the electorate.¹⁰ Indeed, when parties veer away from voters' preferences, they appear to pay an electoral price (Adams and Somer-Topcu 2009; Bawn and Somer-Topcu 2012; Spoon 2011). Whether similar party responsiveness occurs in developing democracies remains an open question. In Latin America during the 1980s and 1990s, parties regularly

implemented neoliberal economic reforms in response to international pressure, with little public support (Campello forthcoming; Remmer 1998; Stallings 1992). Even when doing so required breaking their own campaign promises, these parties nevertheless paid few electoral costs in the immediate term (Johnson and Ryu 2010; Stokes 2001). At least in this context, voters do not appear to punish parties that shift away from their preferences.

One possible reason for this is that voters simply fail to notice when parties change their stripes. At least in the United States, most voters pay little attention to politics (e.g. Delli Carpini and Keeter 1996), so parties may be able to shift without voters noticing. But studies of cross-national survey data are inconclusive on this score, with some finding that voters do not register party shifts (Adams *et al.* 2011) and others finding that they do (Lupu 2013). Certainly, in the dramatic cases of Latin American policy switches voters were well aware that parties had radically shifted their platforms (Lupu 2014a). Another possibility is that parties cue voters, rather than voter preferences driving party positions. In the United States, studies find some evidence that voters change their preferences based on their party's position (Goren 2005; Carsey and Layman 2006), although similar cuing does not seem to take place elsewhere (Adams *et al.* 2011).

A final explanation for incongruence between voters and parties is that voters may simply support parties for non-programmatic reasons. If that is the case, then citizens would have little reason to follow parties' ideological shifts, let alone use those changes to inform their choice at the ballot box. Some scholars note that valence evaluations may trump position issues for some voters (Stokes 1963, 1992). Valence issues are those on which all voters hold the same position. All voters agree that they prefer economic prosperity and disfavor political corruption. Since voters agree about valence issues, all parties take the consensus position. Every party is for economic prosperity and against corruption; the difference among them is the degree to which they are seen as more competent on a particular valence issue than others. Some parties are viewed as weaker on fighting corruption, others as more competent stewards of the economy, because of either the particular qualities of their leaders or the successes and failures of their tenure in government (e.g. Clark 2014). Retrospective economic voting is thus one form of valence-based evaluation (Fiorina 1981). Voters may be giving parties free rein in terms of policy, as long as the policies they choose generate good outcomes (Bleck and van de Walle 2013; Stokes 2001).¹¹

Other non-programmatic considerations could include partisanship, ethnicity, and clientelism. Voters around the world form lasting attachments to political parties. Following the classical perspective on these attachments (Campbell *et al.* 1960), some scholars view them as orthogonal to the party's ideological position (Adams *et al.* 2005). If that is the case, then widespread partisanship would allow parties to take ideological positions less in line with voter preferences. Similarly, if voters choose parties because of ethnic affinities unrelated to policy (Horowitz 1985), or if voters sell their votes to party machines in return for particularistic goods (Bratton and van de Walle 1997; Kitschelt and Wilkinson 2007; Wantchekon 2003), then parties will be unconstrained by public opinion in choosing their policies. Thus, if considerations of partisanship, ethnicity, or clientelism are orthogonal to parties' policy positions, and if they weigh heavily in determining how most citizens vote in certain contexts, this could explain why party policies appear unrelated to voter preferences. Parties would be failing to fulfill their essential function to obtain "popular consent to the course of public policy" (Key 1958: 12).

In certain contexts, these factors may help to explain why parties fail to respond to voters' policy preferences. But they are unlikely to be the whole story. Recent research on partisanship shows that it is often closely related to policy (Achen 2002; Fiorina 1981), making it unlikely to be a main source of party-voter divergence. Indeed, voters' partisan attachments respond to the policy shifts of parties (Lupu 2014a, 2014b), if sometimes slowly (Green *et al.* 2005). Similarly, ethnic considerations are often correlated with policy preferences, and the extent of ethnic

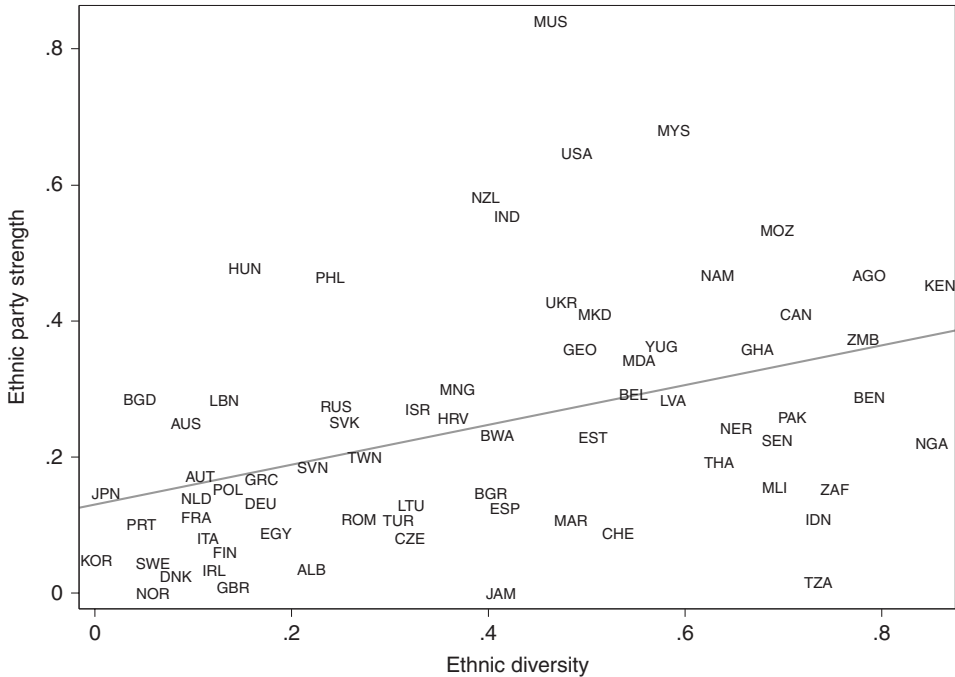


Figure 10.2 Ethnic diversity and ethnic party strength

Notes: Ethnic diversity is measured using the index of ethnic fractionalization developed by Alesina *et al.* (2003). The index uses survey samples to measure the probability that two randomly selected individuals have the same ethnicity. The strength of ethnic parties in each country is calculated as the weighted average of party scores on a measure of ethnic-based support derived from the Democratic Accountability and Linkages Project. The DALP asked experts in 2008–2009, “Do the following parties have strong linkages to ethnic/linguistic organizations?” Responses to this question are averaged across experts by party. The weighted average across parties in each country uses the party’s average vote share in the two national legislative elections prior to 2009. The gray line represents the best-fit line.

Sources: Alesina *et al.* (2003); Kitschelt (2013)

voting even in contexts where ethnicity is very salient appears to be exaggerated (e.g. Hoffman and Long 2013; but see Ichino and Nathan 2013).¹² As Figure 10.2 demonstrates, ethnic diversity explains only a fraction of the cross-national variation in the strength of ethnic parties.¹³ Finally, we still know little about the extent of clientelism around the world (e.g. Gonzalez-Ocantos *et al.* 2012), and there are good reasons to think that there are limits on the extent to which parties can rely on clientelism (Weitz-Shapiro 2013). We also know even less about the extent to which clientelism trumps other voter considerations. In fact, recent studies suggest that parties target clientelistic goods, and pork barrel projects, more generally, at their core supporters—voters whose policy preferences are already in line with the party (e.g. Cox and McCubbins 1986; Stokes *et al.* 2013). If that is the case, then clientelistic links between voters and parties can hardly be seen as orthogonal to programmatic ones.

Scholars are still struggling to understand the complex relationship between voters and parties. Theories about parties and party systems typically focus on structural or elite-level factors and make (often implicit) assumptions that voters notice party behavior, that they punish unaccountable parties, and that they determine which party to support on policy grounds. But events in the real world, as well as empirical studies, suggest otherwise: voters sometimes seem unaware of party shifts, they regularly fail to punish parties that take unpopular positions, and

they consider factors other than party program when casting their ballots. Students of party politics are increasingly coming to terms with these inconvenient facts from political behavior. Bridging the gap between these historically distinct fields of research will substantially improve our understanding of parties and party systems.

Old and new democracies

Historically, theories of party politics were gleaned from the experiences of advanced democracies in Western Europe and the United States. To a certain extent, that regional focus limited the kinds of puzzles that scholars undertook to study. For instance, important questions on single-party dominance (e.g. Greene 2007; Magaloni 2006; Smith 2005) or party politics in non-democracies (e.g. Brownlee 2007; Levitsky and Way 2010) did not make it on to the agenda of research on parties and party systems until recently. As democracy emerged (or reemerged) in the developing world during the Third Wave, scholars looked to existing theories of party politics in an effort to explain parties and party systems in these newly competitive systems.

Often, the behaviors and structures of parties in these developing democracies remained puzzling. Party systems in developing democracies appeared more volatile (e.g. Kreuzer and Pettai 2003; Mozaffar and Scarrit 2005; Roberts and Wibbels 1999; Tavits 2005) and less institutionalized (Dix 1992; Hicken and Kuhonta forthcoming; Kuenzi and Lambright 2001, 2005; Mainwaring and Scully 1995; Stockton 2001), voter attachments with parties seemed weaker (Dalton and Weldon 2007; Hale 2006; Mainwaring and Zoco, 2007; Manning 2005), parties appeared less reliant on programmatic appeals (Bratton and van de Walle 1997; Keefer 2007; Kitschelt *et al.* 1999, 2010), and party systems were not always organized along salient social cleavages (e.g. Dix 1989; Kitschelt 1992). Figure 10.3 illustrates one manifestation of this difference: Elections in older democracies exhibit significantly less volatility than those in newer democracies. Moreover, decades of continued party competition have rarely diminished—and sometimes even exacerbated—these divergences from theoretical expectations and experiences in advanced democracies (e.g. Reich 2001, 2004; Roberts and Wibbels 1999).

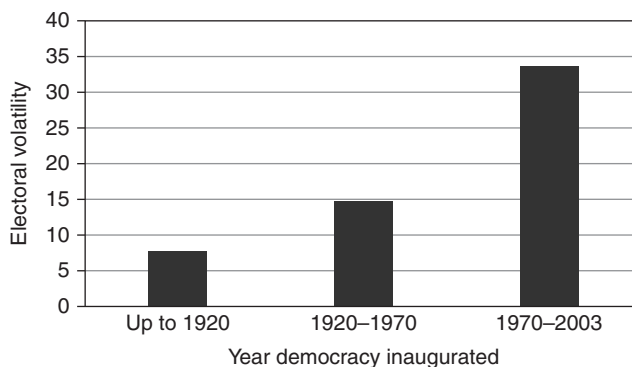


Figure 10.3 Electoral volatility in old and new democracies

Notes: Values represent the average electoral volatility since the inauguration of democracy (or 1945, for democracies born before 1902) across countries that transitioned to democracy during each time period. Data are for 47 democracies included in the first module of the Comparative Study of Electoral Systems.

Source: Mainwaring and Zoco (2007)

One reason might be that the timing of democratization matters for party politics. An obvious difference between advanced and developing democracies is that the latter emerged later in world-historical time. The global context was vastly different in the nineteenth century, when most advanced democracies appeared, than it was in the 1980s and 1990s, when many developing democracies emerged. Parties in older democracies had been the vehicles by which societal groups pressured for suffrage extensions. By the time new democracies emerged, universal suffrage had become the international norm (Schmitter 2001). And whereas political entrepreneurs had used parties to mobilize supporters in the old democracies, by the 1980s they could communicate directly with voters through mass media instead. As a result, politicians no longer needed to nurture partisan attachments in order to successfully compete in elections (Mainwaring and Torcal 2006; Mainwaring and Zoco 2007). By the time new democracies emerged, the age of mass parties was over. That different historical context plays a crucial role in setting up the incentives around which parties form and compete. So old democracies had embarked on one path of party politics in the nineteenth century, and new democracies would embark on a very different one.

More commonly, scholars have focused on contingent shocks or institutional arrangements to explain why parties and party systems in new democracies diverge from the theoretical expectations derived from old ones. Economic transitions or crises struck new democracies particularly severely in the 1980s and 1990s, transforming or upending parties and party systems (e.g. Roberts and Wibbels 1999; Roberts 2013). Partly as a result, politicians felt compelled to use bait-and-switch tactics to implement market-oriented economic reforms, tactics that undermined parties, fragmented party systems, and weakened programmatic competition (Kitschelt *et al.* 2010; Lupu 2014a; Stokes 2001). In other cases, new democracies adopted peculiar institutional arrangements that differed from those in advanced democracies and informed party development (e.g. Hicken 2009). Or else new democracies inherited distinctive legacies or leaders that shaped parties and party systems in distinctive ways (Grzymala-Busse 2002, 2013; Hale 2006; Kitschelt *et al.* 1999; Riedl 2014; Tavits 2013; Weghorst and Bernhard 2014). Since factors like colonial or authoritarian institutions were specific to developing democracies, they could well have caused their parties and party systems to diverge from expectations based on the experiences of advanced democracies.

Both of these approaches have strengths and weaknesses. The fact that developing democracies emerged through a different sequence of events and at a very different moment in world history doubtless affected party development. But the strong, deterministic expectation that mass parties will not emerge in these contexts belies the varied trajectories of parties and party systems in these new democracies. Some strong mass parties have indeed emerged in these contexts, and others have successfully adapted to new environments (e.g. Hunter 2010; Levitsky 2003; Lupu and Stokes 2010). At the same time, historical legacies and contingencies surely have influenced parties and party systems in these contexts. But these explanations yield scattered case- or region-specific theories that are difficult to extend coherently into a general framework.

Building on aspects of both perspectives, Lupu and Riedl (2013) offer such a theoretical framework at a broader level of generality. They argue that political uncertainty is significantly higher in developing democracies than in advanced ones, and this affects the choices of political elites. In these contexts, political actors ascribe some nontrivial probability to the possibility of authoritarian reversals or major institutional changes, making it difficult to predict how long the current rules of the game will persist. Developing democracies tend also to have emerging economies that are particularly vulnerable to market swings and susceptible to economic shocks amid financial globalization. As a result, the possibility of exogenous shocks that transform the entire structure of political interaction is much higher in new democracies than in older ones.

This heightened unpredictability means that political elites may prioritize short-term gains and hedge their political strategies. They may, therefore, invest less in programmatic appeals, consistent party brands, and institutionalized party organizations.

Developing democracies followed different historical trajectories and confronted different domestic and international opportunities and constraints than the older, advanced democracies. These differences have had lasting effects, making parties and party systems in developing democracies less institutionalized, more unstable, and less programmatic. But how we think about the underlying causal relationship affects not only the generality of our theories but also our expectations about the future of political parties in new democracies. The path-dependent approach, that the age of mass parties is over, implies a particularly pessimistic outlook that overlooks variation among these systems. The contextual approach does better at explaining regional or case-specific variation, but can be difficult to generalize. One way forward is to combine aspects of both approaches into a more general framework. For instance, recognizing that political uncertainty tends to be higher in developing democracies than in advanced ones may help us explain both the general divergence in outcomes between them and also variation within them. Similarly general frameworks may not only help us to explain why parties and party systems look so different in new democracies, but also teach us something about variations among and changes within old democracies.

Political parties and democracy

A combination of splitting and lumping approaches have contributed enormously to our understanding of parties and party systems in recent years. Splitters have highlighted additional factors—beyond those emphasized by early scholarship on Western Europe—that help explain how parties emerge, how they persist or change over time, and how they differ across contexts in their behavior or strategies for appealing to voters. Early scholars emphasized the role of social cleavages and electoral institutions in determining these outcomes. More recent studies suggest both more nuanced mechanisms underlying those relationships and additional factors—including institutional legacies and parties' strategic choices—that help us better understand how parties and party systems form, persist, and change. Whereas early theories of party competition made simplifying assumptions about the link between parties and voter, newer efforts to link voter behavior and party politics yield a richer understanding. Under some circumstances—that scholars are just beginning to identify—parties appeal to voters through valence, partisanship, ethnicity, or clientelism, each with different implications for party politics. These contributions have vastly expanded the set of variables scholars should consider in trying to understand parties and party systems around the world.

But if this expanding set of variables seems disparate or specific to particular cases, lumpers are starting to offer more general frameworks that encompass many of them. These efforts look for more general dimensions that make one set of variables more determinative than another. So a continuum of political uncertainty might explain why cleavages and institutions seem to explain a great deal about party politics in advanced democracies and far less in developing ones. And thinking about these broader dimensions could help us better explain variation even among cases that, while broadly consistent with received theories, exhibit nuanced differences. For instance, differences in economic uncertainty among advanced democracies may help to explain why some parties and party systems emerged unscathed from the Great Recession while others transformed dramatically.

Both approaches have taught us a great deal about party politics, but they also leave many questions unanswered. We know that salient social cleavages matter, but how do latent

cleavages come to be salient, and why do some take precedence over others in shaping the party system? We have also learned that parties employ a variety of strategies to mobilize voters—from fostering partisan attachments to priming ethnic identity to buying votes—but how do parties choose among them? Or, perhaps more accurately, how do parties diversify their portfolio of mobilization strategies, and under what conditions does the portfolio change? These are crucial questions if we hope to understand, for instance, how non-programmatic parties might become more programmatic, potentially even allowing policy makers to propel that process forward. Similarly, we know that parties organize differently both within and across countries, but where do these organizational differences come from? And how do they affect party strategies? Finally, theories of party politics necessarily involve voters, but causality in their relationship seems to go both ways. What we need to examine further are the conditions under which voters determine the choices of parties, and those under which parties instead influence voters.

Scholars have been studying parties and party systems for over a century, and yet these and many other puzzles remain unresolved. That is partly because—perhaps unlike most other political institutions—parties are uniquely dynamic organizations constantly evolving and continually interacting with individual voters. It is also because of the vast array of parties and party systems that have emerged around the world in recent decades. Making sense of this dynamic and variegated landscape is daunting. And yet, if Schattschneider (2003 [1942]: 1) is right that “modern democracy is unthinkable save in terms of the parties,” then a better understanding of party dynamics should be central to a research agenda concerned with democratic institutions and representation.

Notes

- 1 I am grateful to the editors and Matthew Mitchell for comments on previous versions of this chapter.
- 2 However, political parties also prove indispensable to some nondemocratic regimes (Brownlee 2007; Greene 2007).
- 3 The division between lumpers and splitters derives from debates in biology over taxonomy, and apparently dates back to Charles Darwin.
- 4 Hanson (2010) offers an alternative account that ideology can motivate party formation in contexts characterized by social chaos and political uncertainty. For Noel (2013), ideology exists independently of parties, though parties sometimes organize along ideological divisions.
- 5 Mair (2013) suggests that this detachment created opportunities for anti-system parties (see Capoccia 2002) to make electoral inroads in several Western European countries.
- 6 Even in these settings, party systems were not a *tabula rasa*; parties clearly built upon legacies and prior civil-society organizations (Kitschelt *et al.* 1999; LeBas 2011; Wittenberg 2006).
- 7 Similarly, party strategies may activate particular cleavages that then shape the party system (e.g. Chandra 2004; Kalyvas 1996; Tavits and Letki 2014).
- 8 In some ways, studies that emphasized critical junctures allowed for endogenous change, but only during these specific moments of uncertainty and political opening (see Capoccia and Kelemen 2007).
- 9 Tavits (2007) finds that such shifts were rewarded by voters when they involved pragmatic, rather than principled, issues.
- 10 While there is broad consensus among scholars of U.S. politics that the parties have polarized (e.g. Layman *et al.* 2006; McCarty *et al.* 2009), there is a great deal of debate as to whether the electorate has also polarized (Abramowitz and Saunders 2008; Fiorina and Abrams 2008; Hetherington 2009). Similarly, Iversen (1994) finds that party positions are more extreme than those of voters. Kedar (2009) offers a compelling explanation for this phenomenon.
- 11 Scholars have developed spatial models of party competition in which some voters vote on the basis of valence issues (e.g. Adams *et al.* 2005; Schofield 2003, 2004).
- 12 Moreover, the extent to which ethnic identity is activated as a determinant of vote choice may be a product of party strategies (Chandra 2004) or institutions (Posner 2005).
- 13 The measure of ethnic party strength cannot be comparably measured for the Latin American cases, so I exclude them here.

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11

LEGISLATIVE ORGANIZATION AND OUTCOMES

Eduardo Alemán

Interest in the details of parliamentary procedures and how they affect outcomes can be traced back to ancient times, as Riker (1986) reminds us in his story of Pliny the Younger. During the formative years of the political science discipline, in the late nineteenth and early twentieth century, scholarly work on legislative organization was already underway. During this time comparative institutional analysis of legislatures was typically restricted to comparisons between Britain and the United States or among U.S. state legislatures. Throughout the course of the twentieth century the comparative aspect of legislative research declined, and the U.S. House of Representatives became the focal point of most cutting-edge studies (Gamm and Huber 2002). The spread of democracy and democratic institutions after World War II and then again after the 1970s, fostered greater interest in the comparative analysis of legislative politics, including legislative organization. For the last couple of decades legislative scholars have examined a variety of organizational arrangements and behavioral patterns that have expanded our understanding of institutions and have moved the field forward. Theoretical insights from analysis of the U.S. Congress continue to inform many of the institutional arguments about legislative organization, but the institutional variation provided by comparative research is essential to move from narrow arguments about the House of Representatives to general theories of legislative organization.

Modern legislative studies have been significantly influenced by the arrival of rational choice institutionalism to the subfield in the late 1970s. Although legislative scholars have always maintained a deep interest in institutions, even during the heyday of behavioral approaches, the current era has brought institutions to the forefront of the sub-discipline. Institutions are commonly conceptualized as the rules of the game, constraining individual interaction, but the view of institutions as equilibrium outcomes is also embraced by several legislative scholars that emphasize the endogenous nature of many legislative rules.

Legislative organization—the set of procedures that regulate the legislative process and the related set of offices with internal authority—allocates power. As a result, it impacts the outcomes of legislatures both in terms of lawmaking and representation. This chapter reviews some of the central aspects of research on legislative organization. It is divided into three sections and a short conclusion. The first one discusses the rationale for legislative rules and their endogeneity. The second section reviews arguments that stress the exogenous impact of the

larger institutional and partisan context on legislative organization. How organization distributes legislative agenda-setting power to different offices is discussed in the [third part](#).

Legislative rules and the advantages of organization

Legislative work is organized according to a series of rules laid out in the country's constitution and the chamber's internal rules of procedure. In some countries, special legal provisions and institutional norms also regulate the work of legislatures. These rules establish the different offices created to run legislative affairs, such as the chamber's directorate and committees. Since first emerging in the late sixteenth-century Elizabethan House of Commons, a system of permanent committees has been a salient feature of most legislative chambers. The prerogatives of each office and the related procedural rules structure the work of legislators.

Legislative rules stipulate who can introduce bills and amendments and under what conditions they can be discussed and passed; whether any actor can prevent or delay discussion or voting; what is the default outcome when proposals fail (e.g. budget bill); and how speaking time should be assigned. Crucially, legislative rules establish the required majority to pass bills, constitutional amendments, and the various motions and parliamentary procedures available. Thus, rules allocate rights and impose constraints on individual legislators.

A number of legislative rules can be considered exogenous: reforming them requires the agreement of actors outside the chamber. For example, the Chilean Constitution of 1980 establishes the formation of conference committees to resolve bicameral differences in legislation. The Brazilian Constitution of 1988 gives committees the authority to pass some bills without reporting them to the plenary (*terminative* power). The French Constitution of the Fifth Republic specifies the number of committees inside the National Assembly. Furthermore, legislative rules can be stabilized by requiring high thresholds to reform them. In countries like Austria and Costa Rica, changes to the standing orders of parliament require the support of a two-thirds majority. Reforms to legislative rules can also be constrained by choices made at earlier points in time. According to Aldrich (1994) and Evans (1999), the choice of congressional rules often reveals significant path dependencies, which reinforce the exogeneity of legislative structure.

Many legislative rules, however, are endogenous: they can be changed by a simple majority of those subject to the rules. This raises questions about the relevance of these rules. If members of a majority coalition can coordinate their behavior to change a rule that binds them, does it imply that the choice of rules is inherently unstable, as Riker (1980) theorized? Are legislative procedures weak constraints because cooperation to evade them is possible, as McKelvey and Ordeshook's (1984) research implied?

Theories of legislative organization have sought to explain why a majority would choose procedural rules that can be expected to produce outcomes biased away from the majority. Shepsle and Weingast (1984) argue that institutions entail different degrees of durability for agreements because some are easier to change than others. According to their view, procedures will prove binding when the transaction costs of getting around them are high. An existing rule will be enforceable, even if a majority coalition might wish otherwise, as long as those wishes cannot be transformed into coordinated action (Shepsle and Weingast 1984). In the case of the U.S. Congress, they argue, jurisdictional complexity increases the costs of negotiating and implementing changes to the rules. Rules institutionalize agreements and practices that facilitate collective action, including preventing post-agreement opportunistic behavior (Shepsle and Weingast 1984). Changing an important rule is a costly battle where reformers themselves become vulnerable to opportunism and reneging.

Early theories of legislative organization emphasize vote trades. Legislators need the support of others to get their individual projects enacted into law, but arrangements to support each other's projects are hard to enforce. Reputation helps to build trust, but it is generally insufficient to enforce agreements. Weingast and Marshall (1988) argue that a system of committees with the ability to control changes to the status quo in their defined jurisdictions makes possible gains from trade among their membership; that is, it facilitates logrolls by ameliorating the incentives to renege on agreements. In this scenario, committee rights over the design and selection of proposals that come up for a vote mean that a legislator on committee i gives up influence over the selection of proposals in the area of committee j in exchange for members of committee j giving up their rights to influence proposals in policy area i (Weingast and Marshall 1988). The combination of self-selection into committees with jurisdictional and gatekeeping rights allow career-oriented legislators the opportunity to keep in place their preferred policies that without a trade of votes would have been difficult to enact and vulnerable to changes.

Distributive theories that point to logrolls as the *raison d'être* for organization face the challenge of explaining why vote trades that in the aggregate make a majority worse off can be the motivation for creating legislative offices with significant power—why a majority would put in place a system where committees consistently lead to inefficient results (Baron 1994; Krehbiel 2004; Diermeier *et al.* 2013).

Informational theories present an alternative view on legislative organization. They start by arguing that uncertainty about the relationship between laws and their consequences creates incentives for developing rules that promote expertise among legislators (Krehbiel 1991). Organization is seen as the solution to the challenges of promoting specialization and taking advantage of the expertise legislators already have (Cooper 1970; Krehbiel 2004).

Improvements in information reduce policy uncertainty. Gilligan and Krehbiel (1987, 1989) argue that the establishment of a system of committees that can make take-it-or-leave-it proposals to the membership works to promote specialization and induces committee members to credibly communicate their expertise in the bills they report. Epstein (1996) shows that giving committees gatekeeping power can have similar results. Kim and Rothenberg (2008) present a signaling model that builds on the assumption that legislators know more about the status quo than they do about new proposals. As long as there is considerable information asymmetry between specialized committees and the floor, they show that the pivotal legislator (i.e. the floor median) has incentives to give gatekeeping and proposal power to committees. They also show that the conditions for forcing bills out of committee (i.e. discharge petitions) are restrictive. Diermeier *et al.* (2013) advance a game-theoretic model where the standing orders remain in place because a majority prefers the asymmetry of power and policy bias they produce over the policy uncertainty that ad hoc procedures would introduce. Their model illustrates how endogenous legislative rules prevent procedural uncertainty and can be sustained *ex post* because they reduce policy uncertainty.

Another explanation for legislative organization begins with the observation that time is a scarce resource (Döring 1995; Cox 2000). An individual member determined to change the standing orders not only must invest time and effort to explain to other legislators the reasons for the reform, but he or she must also convince enough of them and the chamber's authorities that its debate and eventual passage deserves to be prioritized over other matters. This, Cox (2000) notes, is particularly difficult because time is a limited resource and revoking rules that allocate power requires significant amounts of time and effort. He underlines how demand for plenary time can result in a "plenary bottleneck" that undermines legislative work (Cox 2005). The potential for failure is particularly high when procedural rules are ad hoc. The creation of offices endowed with special agenda-setting authority can solve this dilemma. By enabling

coordination among members, organization not only facilitates agreements and specialization, but also helps the passage of legislation (Cox 2005).

To sum up, legislative rules establish the offices and procedures that organize legislative work. This includes the allocation of particular powers over the agenda to some legislative actors and the imposition of some limits on the influence of individual backbenchers. Institutional designers typically expect legislative rules to bring about some desirable effects (Shepsle and Weingast 1984). The rules can be exogenous or endogenous. In the latter case, the challenge has been to explain why a majority would accept to be constrained by an organizational arrangement that occasionally makes it worse off, when it can change the rules it is subject to. Answers have explained how the majority is compensated for this potential policy bias with informational benefits and arrangements that facilitate coordination and cooperation to enact legislation.

The impact of the larger institutional and partisan structure on organization

Legislative organization is also shaped by external forces. Comparative studies of legislative institutions illuminate why organizational features differ across legislative chambers, even when some of the fundamental problems rules are designed to help resolve are similar. Variation in legislative organization across countries has been linked to electoral and partisan incentives, constitutional structure, and the size of the chamber.

Electoral and partisan incentives

Electoral rules and political parties affect the strategies that legislators pursue to advance a political career, including their choices about legislative organization. For example, Katz and Sala (1996) link changes in electoral rules that created new personal vote incentives—the change from party ballots to the Australian ballot during the 1890s—with the development of the modern committee system in the U.S. House of Representatives. After the ballot reform, committee assignments came to be considered a property right and the chamber adopted various practices with important credit-claiming effects (Katz and Sala 1996). The role of ballot reform, primaries, and the direct election of senators in weakening the grip of parties is also underlined by Epstein *et al.* (1997), who argue that legislators who cultivate the personal vote should have incentives to strengthen the role of extra-party organizations within the legislature, such as committees.

Taylor (2006) examines cross-nationally whether electoral rules that foster a personal vote are more likely to be associated with decentralized legislative procedures. He finds a significant association between the personal vote and rules that are more permissive with respect to amendments and bill initiation. Martin (2011) argues that differences within rules that encourage the personal vote also have an impact on legislators' preferences over legislative organization. He expects committees to be stronger when legislators focus mainly on supplying pork to constituents, but weaker when legislators focus mainly on extra-legislative constituency service. Within the latter group we find, for example, the Irish and British parliaments and within the former group the U.S. Congress. To test this proposition, Martin (2011) examines a cross-national dataset of 39 legislatures. His results show that rules that produce incentives for legislators to cultivate a personal vote are associated with strong committees only when fiscal particularism is present. If not, the personal vote tends to be associated with weaker committees (Martin 2011).

Party-centered electoral rules have been associated with centralized forms of organization and lower incentives to put in place powerful committees. Jones *et al.* (2002) argue that

closed-list proportional representation in federal countries weakens incentives to specialize and build autonomous committees. They examine the Argentine case, where provincial (district) party bosses have a significant influence on the crafting of electoral lists and career politicians seek out coveted offices in the provinces rather than legislative careers. According to Jones *et al.* (2002), by moving legislators' electoral incentives away from voters and towards the interests of provincial party bosses, Argentina's electoral rules reduce incentives to specialize or promote constituency service. As a result of the electoral incentives in place, the Argentine Chamber of Deputies has a weak and amateurish committee system, which only modestly enables the development of informational capabilities.

Parties that exert significant control over the careers of legislators are typically associated with weak committees (Shaw 1979: 396; Olson 1980: 269; Olson and Mezey 1991; Epstein *et al.* 1997; Rahman 2008:76). For Weingast and Marshall (1988), the historical evidence from the U.S. Congress suggests that strong parties are substitutes for strong committees. This is similar to the view of Cooper and Brady (1981) and the subsequent literature on *conditional party government* that associates high leadership prerogatives and centralized procedures with strong cohesive parties (Aldrich and Rhode 1997–98, Rhode 1991: 24, 137). García Montero and Sánchez (2002) depict a similar association between committee influence and partisan strength in Latin America.

When strong committees are said to coexist with strong parties, as Cox and McCubbins (1993) claim is the case in the U.S. and Kim and Loewenberg (2005) claim is the case in Germany, committees are portrayed as agents of the majority party (U.S.) or the majority coalition (Germany). Committees in Germany play a significant role policing bargains among the governing parties (Thies 2001: 588; Kim and Loewenberg 2005). More generally, single-party government has been linked to weaker committees than multiparty government (Hallerberg 2000; Martin and Vanberg 2005).

Lastly, there is a literature that extends over a century that sees the lack of careerism (i.e. high membership turnover and locally oriented careers) as the source of a centralized legislature dominated by a small group of senior members (Follet 1986; Chiu 1928; Rothman 1966; Squire 1988; Epstein *et al.* 1997). Evidence from U.S. state legislatures tends to support the view that lower stints in office are associated with weaker committees (Farmer and Green 2007: 10; Kurtz *et al.* 2007). Shaw (1998) notes that in many developing countries high legislative turnover leads to discontinuity in committee membership, which contributes to few experts, absenteeism, and infrequent meetings.¹

Constitutional structure

Several works have called attention to the effects of separation of powers and bicameralism on legislative organization (Lees and Shaw 1979; Diermeier and Myerson 1999; Epstein and O'Halloran 2001; Pereira and Mueller 2004). The claim that presidential systems encourage stronger committees than parliamentary systems is one of the few “stylized facts” in the comparative study of legislatures (Diermeier and Myerson 1999; Lees and Shaw 1979). Active parliamentary committees have been portrayed as incompatible with the classical model of parliamentary government because they potentially threaten the predominance of government and party leaders (Longley and Davidson 1998). While strong assertive committees can develop in parliamentary countries (Strøm 1998), separation of powers is commonly seen as structurally more conducive to this end (Lees and Shaw 1979; Rahman 2008: 72).

For Epstein and O'Halloran (2001), committees serve to check the executive branch's policymaking. In the U.S. Congress, they argue, strong committees not only help to direct

resources to favored constituents but also provide an alternative source of expertise that rivals executive agencies. Members of Congress benefit from this competition, and committees' assignments seek to enhance such information gathering (Epstein and O'Halloran 2001). Diermeier and Myerson (1999) model the organizational choices of a chamber as a non-cooperative game between chambers (and a president) that have to interact with outside lobbyists, who in turn are trying to influence legislative outcomes. One implication of their analysis is that incentives to delegate legislative prerogatives to particular offices (e.g. authorities or committees) are likely to be affected by the presence and organizational characteristics of other chambers.

In presidential countries, constitutional rules that give the executive substantial legislative prerogatives have been associated with weaker committees. In their analysis of the Brazilian Congress, Pereira and Mueller (2004) remark how presidents can bypass committees with urgency procedures, as well as affect the committee selection process, influence the choice of committee presidents and rapporteurs, and create temporary committees. Changes in individual committee assignments occur frequently. Brazilian presidents also have control over the distribution of many of the financial resources legislators seek to access. These institutional features, Pereira and Mueller (2004) argue, work to make the committee system in the Brazilian Chamber of Deputies weak, and facilitate the concentration of power in its directorate and the committee of party leaders. According to Figueiredo and Limongi (2000), a centralized organization in Congress and significant executive prerogatives help presidents overcome the challenges brought about by personalistic electoral incentives.

The size of the chamber

The problems of cooperation and coordination faced by legislators tend to be less acute when the number of members is low. Olson (1965) believed that the provision of a collective good would decline with increases in the number of members because opportunities to free-ride on the efforts of others would be greater and so would be the transaction costs of reaching agreements. Trust and reciprocity is usually more easily developed in small groups. Large groups make voluntary information flows more difficult and increase the need for formal institutions (Dixit 2004:66). In contrast, small groups are more likely to have frequent face-to-face communication, which experiments have shown raises cooperation (Ostrom and Walker 1997). In large legislatures, centralized arrangements can help to overcome gridlock and reduce the transaction costs of legislative bargaining (Taylor 2006).

Long ago, Orfield (1935) noted that committees are "less essential" in small legislatures. Empirical studies of the U.S. Congress, state legislatures, and councils have tended to associate increases in the number of legislators with more complex and hierarchical types of organizational structures (Hedlund 1984). For Richman (2010) and Mooney (2013) smaller chambers facilitate agreement and the resolution of collective action problems. They expect the powers of the chamber's leaders to be greater when the number of members is more numerous. Richman (2010) finds that in U.S. state legislatures the formal power of the Speaker increases with chamber size, while Mooney (2013) finds that chamber size is correlated with legislators' evaluations of the power of the Speaker.

Taylor (2006) expects the size of parliament to affect incentives to choose centralized or decentralized organizational schemes. He presents evidence from 55 chambers in 32 countries that shows that as the size of the chamber increases legislators are more likely to put in place centralized procedures that limit bill initiation, amendments, and debate (Taylor 2006). Mattson (1995) finds that across Western Europe, smaller parliaments impose fewer restrictions on an

individuals' right to introduce legislation. And Rogers (2002) presents evidence that per capita legislative output is likely to decline with chamber size.

Baldwin's (2013) edited volume on small legislatures in 36 states reveals chambers with a low degree of organizational complexity, weak committees, and a rather consensual style of decision-making. In his examination of the small Costa Rican Assembly, Carey (1996: 28) notes that the substantive debate, negotiation, and detailing of legislation that takes place during plenary meetings is much greater than in more numerous chambers.

Carrubba and Volden (2000) link theoretically the choice of voting rule (i.e. the type of majority required) to the size of the chamber. They argue that in smaller chambers there are greater incentives to choose rules that require qualified majorities for passage than in chambers with numerous members. If high voting thresholds are put in place in a legislature with a large membership, greater obstacles to agreement should be expected. As examples, they mention differences in debate rules between the U.S. House of Representatives and the Senate, as well as increasing voting thresholds inside the EU as its membership expanded. Their formal model, which presents a logrolling game where the initial choice of voting rule and the legislative coalition are endogenous, shows that the voting majority selected decreases with increases in chamber size (Carrubba and Volden 2000).

To sum up, the larger institutional context and other exogenous forces appear to have consistent effects on legislative organization. Parties with significant influence over the political career of legislators, electoral rules that emphasize a partisan vote, and high turnover have all been linked with stronger leadership and a weaker system of committees. Presidential constitutions tend to generate stronger incentives for autonomous committees than parliamentary constitutions, but presidents with strong legislative powers usually weaken the power of committees. Leadership prerogatives and individual constraints usually increase with the size of the chamber.

Agenda setting

Legislative organization allocates procedural rights that affect who can bring proposals to debate on the plenary floor and how voting on these proposals may proceed. Canonical models show that committees with strong gatekeeping and proposal powers influence the location of policies.

Many recent works focusing on legislative organization and decision-making have been explicitly or implicitly influenced by Black's (1948) classic work on the rationale of group decision-making and the gatekeeping and agenda-setter models (Romer and Rosenthal 1979; Denzau and Mackay 1983). Given the inherent instability associated with majoritarian decision-making, as exemplified by the Condorcet Paradox, Black (1948) sought to find the conditions under which voting would produce an equilibrium outcome. He showed that if members' preferences can be arranged in such a way that they have single-peaked preferences, then the voting equilibrium under majority rule is the position of the median voter. The median is a Condorcet winner; it beats every alternative in a pairwise contest. Black (1948, 1958) was the first to demonstrate that under a unidimensional choice space with no restrictions on amendments, legislative outcomes decided under majority rule will tend towards the position of the median legislator. Many works on legislative politics have built on the insights provided by Black, such as Shepsle's (1979) work on structure induced equilibrium and Krehbiel's (1998) model of legislative pivots.

While the models of Black and many of their successors predicted that policy would be located at the median, it became apparent that this was not always the case. So scholars developed another generation of models to explain why policies often deviated from the median.

The general idea is that rules that provide certain actors with gatekeeping or proposal powers allow them to pull policy away from the median.

The gatekeeping model was originally formalized by Denzau and Mackay (1983), who concentrated on committee advantages.² In this two-stage game, the committee can open the gate and schedule a bill or reject it and preserve the status quo. The legislative chamber moves second, either debating the committee's proposal and eventually passing a bill, or rejecting it and therefore keeping the status quo. In this game, the dominant strategy for the committee is to close the gates on all bills whose final outcome is preferred less than the status quo. Assuming a unidimensional choice space and majority rule, if the committee opens the gate and there are no restrictions on amendments, then voting should move policy to the position of the median voter in the chamber.

The agenda-setter game differs in that the chamber cannot amend the bill proposed by the committee. It is a take-it-or-leave-it proposal. Under this condition, the committee will propose to move the status quo to its own ideal position and the chamber will accept as long as the median legislator prefers the bill to the status quo. In other circumstances, the committee makes a proposal that moves the status quo towards its position, while still making the median legislator indifferent between accepting and rejecting (in which case passage is assumed). When the status quo is located between the median legislator and the committee's position, it will stay there. Otherwise, the committee will successfully bring outcomes closer to its position.

Figure 11.1 illustrates the results of the gatekeeping and agenda-setter games for various positions of the status quo.³ The horizontal axis represents the possible status quo positions, and the labels C and M indicate, respectively, the preferences of the committee and the median legislator. This is mirrored in the vertical axis, which captures the outcome of the games. Lines indicate the result under different status quo positions (solid for the gatekeeping game and dashed for the agenda-setter game). As Figure 11.1 illustrates, gatekeeping and the

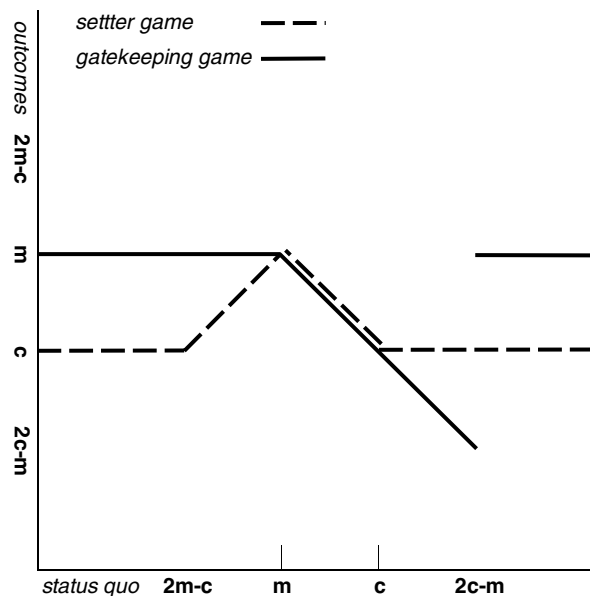


Figure 11.1 Gatekeeping and setter games

agenda setter's ability to make take-it-or-leave-it proposals can bias results away from the median voter and towards the position of the committee.

The gatekeeping power of partisan majorities and committees

Research on legislative cartels, originated by Cox and McCubbins (1993, 2002, 2005), has emphasized the gatekeeping power of the majority party. The procedural cartel thesis states that if a majority government forms, it will constitute an agenda cartel (Amorim Neto *et al.* 2003; Chandler *et al.* 2006). In the U.S. case, it is the majority party that functions as a legislative cartel (Cox and McCubbins 1993). An agenda cartel has control over the legislative offices with substantial agenda-setting power, as well as over which proposals will be allowed access to the plenary. The cartel acts as a gatekeeper, allowing policy changes only when most of its members support it. As a result, policy changes under a single-party majority require the backing of most members of the majority party. In a coalition government, changes demand the backing of most members of each pivotal party in government (Amorim Neto *et al.* 2003). When the plenary passes a bill without the support of most members of a party, such party is said to be "rolled." If a cartel exists, it should control assignments to agenda-setting offices and its constituent parts should not be rolled on the plenary floor.

Various works focused on the U.S. Congress, U.S. state legislatures, and other chambers around the world have sought to evaluate the implications of the cartel thesis. Cox and McCubbins (2005) find that between 1953 and 1998 the majority party roll rate was on average less than 2 percent (never more than 5 percent), while the minority party roll rate hovered around 26 percent. Before the 1950s, the majority party had roll rates that sometimes ran higher than the 5 percent threshold expected for a cartel,⁴ although regularly lower than minority roll-rates (Cox and McCubbins 2005). Gailmard and Jenkins (2007) present data for the period 1877–2000 that shows majority party roll rates to be on average 4.5 percent in the House of Representatives and 5.5 percent in the Senate. Their analysis leads them to conclude that in terms of keeping unwanted measures off the plenary floor, the majority party is not less successful in the Senate.

Evidence from other presidential countries with single-party majorities is consistent with the cartel thesis. The majority-party roll rate is less than 3 percent in Argentina (Jones and Hwang 2005, Alemán 2006) and 0 percent in Uruguay (Chasquetti 2013).⁵ In both countries, majority parties have significant influence over assignments to key committees and minority parties are rolled much more often in plenary votes.

In presidential countries, multiparty majority governments do not always act as legislative cartels. For example, Amorim Neto *et al.* (2003) and Lyne (2008) examine roll call votes in Brazil and show that some majority coalitions have parties with rather high roll rates.⁶ In Uruguay, Chasquetti (2013) finds that the majority coalitions made under presidents Sanguinetti and Batlle had roll rates below the 5 percent threshold, but the one led by President Lacalle in the early 1990s had higher rolled rates. In Chile during the 1997–2000 legislative period, the government was rolled in less than 2 percent of final passage votes and each party in government was rolled less than 5 percent of the time (Alemán 2006), which is consistent with the presence of a cartel. In Brazil, Chile, and Uruguay, parties in the opposition have much higher roll rates than government parties.

In parliamentary countries, the roll rates of parties in majority governments are consistent with the expectations of the cartel thesis. In the U.K. and Malta, single-party majority governments are never rolled (Cox and McCubbins 2005). In Germany, Japan, and Italy, parties in government coalitions have roll rates near 0 percent, and in Sweden the roll rates of government

parties are less than 5 percent (Cox *et al.* 2000; Chandler *et al.* 2006; Cox *et al.* 2008). An analysis of the Israeli parliament during Sharon's second administration shows that parties in government have roll rates that do not exceed 6 percent.⁷

The argument that government parties in parliamentary countries have gatekeeping power is consistent with Tsebelis (2002) veto players' theory. For Tsebelis, parties in government are considered veto players; they must agree on policy for a change to occur.⁸ He shows why increases in the number of veto players and their relative distance tend to reduce the winset of the status quo, thereby increasing policy stability.

Procedures to force bills out of committee and on to the plenary floor appear to neutralize gatekeeping (Patty 2007). Since in both chambers of the U.S. Congress there are mechanisms by which a majority can force a bill out of committee, Crombez *et al.* (2006) conclude that committees do not have gatekeeping rights. They also note that committees in the U.K., France, Germany, Italy, Norway, Poland, and Israel lack codified gatekeeping rights (Crombez *et al.* 2006).⁹ But others disagree. For instance, Cox and McCubbins (2005: 86) argue that passing discharge petitions in the U.S. House of Representatives is "extremely difficult," particularly when going against the Rules Committee or majority party leadership.¹⁰

The proposal power of committees and the executive

Agenda setters may also have the right to make proposals under favorable conditions.¹¹ The most common are (i) the prohibition of amendments (i.e. closed rule); (ii) the confidence procedure; (iii) restrictive amendment rules (e.g. the guillotine); (iv) the last move (e.g. amendatory vetoes); and (v) the first move (e.g. provisional measures).

In the U.S. House of Representatives, the classic tool embodying proposal power is the closed rule, which prohibits amendments to a bill. The Rules Committee has the authority to write special rules that specify how a bill will be debated and voted on when it arrives at the plenary floor, including limits on amendments.¹² The proposed closed rule must be put to a vote of the full membership before proceeding to address the bill. When conference committees report legislation, it is voted under closed rules in both chambers of Congress. The implications of the ability to make take-it-or-leave-it proposals are captured by the agenda-setter model introduced in the [first part](#) of this section.

Legislative theories focused on the U.S. House of Representative have advanced alternative predictions regarding when bills reported by committees will receive a closed rule. While informational theories expect closed rules to be used more often on bills that come from committees composed of legislators with heterogeneous preferences, distributional theories emphasizing vote trades imply that closed rules should be used more often with pork barrel legislation (Krehbiel 1991: 162). Marshall (2002) emphasizes the partisan nature of the Rules Committee and expects closed rules to be more likely the closer the reporting committee is to the position of the Rules Committee. When the reporting committee is positioned between the median legislator and the Rules Committee, he also expects a higher probability that closed rules would be used. His examination of special rules during the periods 1981–1984 and 1995–1998 finds support for both hypotheses. In addition, and consistent with prior findings, Marshall (2002) shows that restrictive rules are more likely to be used on bills coming from heterogeneous committees, and less likely to appear on pork barrel proposals.

In Latin America, conference committees are only mandated to resolve bicameral differences in Colombia and Chile. In both countries they make take-it-or-leave-it proposals to the plenary. In Chile, conference committees can modify any aspect of the bill, and it is an established norm that conferees belong to the committee with jurisdiction over the bill.

Conference committees in Colombia, however, can modify only the matters under bicameral dispute and the Speaker has wide discretion to choose the composition of conference committees (Alemán and Pachón 2008).¹³

The classic tool of proposal power available to the government in parliamentary countries is the confidence procedure (Huber 1996a; Döring 2003). It allows the governments to link a specific policy to an up or down vote of confidence. Prime ministers can usually use it unilaterally on a wide variety of policy issues and at any point in the legislative process (Huber 1996a). In France, Belgium, and Germany, the prime minister's motion is automatically approved unless a majority of elected members votes against it (Döring 2003). When deciding how to vote on such a motion, members of the majority must consider not only the proposed policy but also the costs of censuring the government, while the prime minister needs to be mindful of the electoral costs of making this strategic move (Huber 1996a). While most prime ministers in Western Europe have this right, some others are restricted from linking a policy proposal to a confidence motion. Such is the case, for example, in Poland and other Central European countries (Zubek 2008).

Prime ministers have other tools to restrict amendments to their bills. For instance, according to the Greek Constitution, amendments are out of order unless submitted prior to debate and the government agrees to their being discussed (Heller 2001). The British "guillotine" sets a deadline for a vote in which only amendments accepted by the Speaker can be considered. The French "guillotine" gives parliament 24 hours to censure the government or the bill proposed by the prime minister is considered passed, while the vote *bloqué* allows only amendments proposed or accepted by the government.¹⁴ Governments tend to resort more often to the vote *bloqué* than to the vote of confidence, and both procedures are used more frequently by minority government than by single-party governments (Huber 1996b: 96, 144–170; Hayward 2004). Döring's (2003) examination of the use of restrictive rules in Western Europe suggests that governments are more likely to use them towards the end of the legislative term with complex bills and when they are of the single-party minority type. In addition, he finds that the use of restrictive rules decreases with the number of parties in government and their ideological distance.

Another advantage commonly held by several governments in parliamentary countries is the ability to make the last amendment before voting. By being able to craft a last offer after seeing all amendments proposed, the government has a strategic advantage to try to improve outcomes (Heller 2001). The theoretical rationale for the last-move advantage was introduced by Weingast (1989) in his analysis of the open rule in the U.S. House of Representatives. According to Heller (2001), governments enjoy a clear last-offer authority in Denmark, France, Italy, Netherlands, Spain, and Sweden.¹⁵

The agenda-setting power of governments may also affect whether amendments can be used as a coalition management tool. Martin and Vanberg (2005) argue that in divided coalition governments, ministers face strong incentives to take positions that deviate from coalition compromises, which leads other coalition members to challenge and amend the proposed legislation to bring it in line with the coalition agreement. Using data from Germany and the Netherlands, they show that bills proposed by ministers are likely to be changed more extensively in the legislative process the greater the level of policy divisiveness among parties in government (Martin and Vanberg 2005). Franchino and Høyland (2009) confirm this finding with a larger sample of countries and go on to show that parliamentary involvement in the amendment process decreases as the government's agenda-setting power increases.

In the presidential countries of Latin America, bills reported to the plenary floor are typically voted under open rules that allow amendments. The standing orders do not commonly give

leadership committees the ability to write special rules for debating and voting a particular bill. Although restrictions to amend money bills are common, most other proposals are vulnerable to amendments.¹⁶

Presidents in Latin America have various institutional prerogatives at their disposal, although typically fewer and less far-reaching than those enjoyed by governments in parliamentary countries like the U.K., France, Ireland, or Greece. One of the most significant agenda-setting tools at their disposal is the ability to introduce a last modified version of a bill through the veto procedure (Tsebelis and Alemán 2005). In 15 countries, the president can veto a bill in its entirety (absolute veto) or issue a constructive veto, which sends a modified version back to Congress for one final round of voting with no additional amendments. Five countries limit the presidential modifications to deletions (partial veto), while 10 countries allow the president to introduce modifications and additions (amendatory observations).¹⁷ Evidence from Argentina, Chile, and Uruguay suggests that when presidents have the option between an absolute veto and a constructive veto, they very rarely choose the former. In addition, urgency provisions that force legislative proposals to the plenary floor give some Latin American presidents *de jure* authority to overcome congressional gatekeeping attempts. Presidents in Brazil, Chile, Colombia, Ecuador, Paraguay, and Uruguay have this authority.¹⁸ In the latter three countries, the default in case of congressional inaction is the automatic passage of the bill.

In Brazil, an important agenda-setting tool in the hands of the president is the ability to issue provisional measures. Presidential bills pushed as provisional measures become effective immediately but are only valid for 60 days.¹⁹ The measure can be amended or rejected by Congress, and after 45 days it is automatically scheduled for deliberation by the chamber. Many scholars have begun to analyze the power given to the Brazilian president by the right to issue provisional measures, how it is used, and how it differs from executive orders or decrees elsewhere (Pereira *et al.* 2005 and 2008; Rennō 2010; Hiroi 2008; Reich 2002; Negretto 2004).

Conclusion

This chapter has discussed the importance of legislative rules and the advantages that organization is supposed to bring to individual legislators. It also reviewed arguments that try to explain why majorities would choose institutional arrangements that sometimes lead to outcomes biased away from them. Legislative organization is also shaped by exogenous forces. Constitutional structure, electoral rules, the size of the chamber, the strength of parties, and careerism appear to influence organization in consistent ways. The power of agenda setters to propose and block proposals was also reviewed, and their implications discussed with reference to various published works.

Legislative studies appear to be in the midst of a fruitful era. Various new works have illuminated the great variation in procedural rules and offices that exists across countries. Arguments about legislative organization narrowly targeted to address the U.S. Congress have begun to be tested in other countries. Examples include studies about the use of restrictive rules, the gatekeeping power of majority parties, and committee strength. This has improved the breadth of legislative theories and allowed for generalizations that go beyond the U.S. House of Representatives. Institutional analyses of European parliaments are also rapidly increasing. Our understanding of parliamentary procedures and the impact of governments and heads of state have deepened with evidence from Central and Eastern European countries.

Several topics remain underdeveloped or demand to be tested in countries other than those that generated the arguments in the first place. Important questions about the choice of rules, the involvement of executives in parliamentary decision-making, the amendment process, and

the organization of legislative speech remain to be answered. Likewise, information about the organization of African and Asian parliaments remains elusive. A comparative research agenda on legislative organization that includes single-chamber as well as multiple-chamber studies and embraces the generalizability of legislative theories should continue to strengthen the sub field.

Notes

- 1 In Costa Rica and Venezuela, for example, incentives to cultivate a personal vote are low and most legislators serve one term. In addition, party leaders have substantial influence over the political career of legislators and regularly shuffle individual committee assignments. As a result, both countries have considerably weak committee systems (Crisp *et al.* 2009).
- 2 See also Weingast and Moran (1983), who focused on regulatory policymaking by the U.S. Federal Trade Commission.
- 3 The representation of the gatekeeping game in Figure 11.1 is similar to the one presented in Crombez *et al.* (2006).
- 4 The cutoff value of 5 percent is put forward in Amorim Neto *et al.* (2003) based on earlier findings by Cox *et al.* (2000).
- 5 Chasquetti's (2013) evidence is from the Uruguayan Senate.
- 6 Amorim Neto *et al.* (2003) hypothesized that a proportional distribution of cabinet positions relative to seat shares reduces the roll rates of government parties.
- 7 Sharon's coalition lost its parliamentary majority for a while.
- 8 If they do not, the government dissolves and a new round of government formation follows.
- 9 The authors also argue that gatekeeping procedures are Pareto dominated by the ex-post veto (Crombez *et al.* 2006). For an alternative view that sees gatekeeping Pareto dominating the ex-post veto see Kim and Rothenberg (2008).
- 10 According to Patty (2007), the support of the Speaker or a majority of the Rules Committee is needed for the discharged bill to reach the plenary floor. The leadership can also force a bill out of committee. The mechanisms available to the House leadership include: unanimous consent, suspension of the rules, enactment of a special rule reported from the Rules Committee, and expiration of the time limit imposed on the committee by the Speaker (Patty 2007).
- 11 Legislators typically have the right to introduce legislation, although restrictions are common. In the German Bundestag, for example, bills can be introduced by parliamentary parties or 5 percent of membership, but not by individual members (Mattson 1995). In presidential countries, limitations to individual members' rights to introduce bills are typically fewer than in parliamentary countries.
- 12 The modified closed rules restrict the number of amendments.
- 13 In Chile conference committees must report the bill, although they can choose to issue a negative report recommending the status quo. In Colombia, conference committees may not report the bill (but they can be easily compelled to do so by the chambers' authorities or even by a plenary vote).
- 14 Bills entering the French parliament tend to receive a large number of amendments (Hayward 2004).
- 15 In Italy, the Netherlands, and Denmark, committees can also make the last offer (Heller 2001).
- 16 In some countries amendments must be pre-filed (e.g. Argentina) or sent to committee prior to debate (e.g. Chile).
- 17 Alemán and Schwartz (2006) show that veto clauses in presidential countries are consequential. (i.e. they change outcomes), sometimes in unexpected and paradoxical ways.
- 18 In Ecuador, only economic legislation can be declared to be urgent.
- 19 There is one automatic renewal.

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12

EXECUTIVE-LEGISLATIVE RELATIONS

Sebastian M. Saiegh

Introduction

On November 22 1990, Margaret Thatcher publicly stated that she would stand down as prime minister. Her decision ended an extraordinary era in British politics that lasted more than eleven years. Interestingly, Thatcher made her announcement shortly after she had vowed to “fight on and fight to win” in her speech following the first round of leadership elections. In the eyes of many Conservatives she had become a political liability to their party’s fortune. Therefore, she was persuaded not to go forward to the second ballot. Thatcher’s Chancellor of the Exchequer, John Major, was chosen as her immediate successor. In 1992, despite considerable public disfavor over the poll tax, European integration, and the state of the economy he scored a comfortable victory over the Labor party, and kept the Conservatives in office until the 1997 elections.

The resignation of Argentine president Fernando de la Rúa was not as orderly. On October 14 2001, after almost two years in office, his administration lost control of the two Houses of Congress to the Peronist party. The electoral defeat came in the midst of one of the country’s worst economic crisis. Two months later, thousands of people marched on the Casa Rosada calling for his resignation. On December 21 2001, he fled the governmental palace in a helicopter. The Peronist Adolfo Rodríguez Saá, then governor of San Luis, was appointed by the legislature as the new interim president. Nonetheless, he also resigned a week later when he lost the support of fellow Peronist governors. A legislative assembly eventually chose Peronist senator Eduardo Duhalde to complete De la Rúa’s term. Political turmoil, however, continued. Soon it became apparent that Duhalde’s political support was more symbolic than real. Even though he was almost unanimously chosen by the legislature to take care of the government, he continuously faced serious obstacles to implement his decisions.

The two examples illustrate how the organization of power and authority may affect political stability. Argentina did not have a constitutional solution for the problem of a failed president with a fixed term in office. In contrast, the British parliamentary regime was able to resolve its political crisis by replacing the government, without turmoil and with a high degree of continuity.

It would be naïve to imply that formal rules and procedures are paramount in explaining political outcomes. Yet, the idea that the constitutional form of government affects the

performance of democratic regimes has been one of the most discussed issues among students of comparative politics in the last three decades. Of central importance in this debate has been the *Linzian* concern with concentration of executive authority under presidentialism (Linz 1990, 1994; Valenzuela 1994; Stepan and Skach 1993). Indeed, as Cheibub and Limongi (2011) note, by establishing a connection between separation of powers and the crises that led to democratic breakdowns in Latin America, Linz set out the agenda and the tone for comparative studies of executive-legislative relations. The chain of reasoning implied by Linz's argument is that (1) coalition formation is more difficult under presidentialism; (2) when no coalition is formed, a legislative stalemate ensues; and (3) given the legislative paralysis, extra-constitutional mechanisms are activated to break the political impasse.

Notwithstanding the "perils" of presidentialism, the operation of a political system can seldom be entirely derived from the mode of government formation. There are other provisions, constitutional or otherwise, that also affect the propensity for executive-legislative deadlock. After all, the record for time taken to form a new democratic government after an election—at 353 days—is held by two parliamentary democracies: Cambodia (2003–2004), and Belgium (2010–2011). Therefore, rather than discussing the relative merits of presidentialism and parliamentarism in generic terms, it seems more promising to examine how they affect executive-legislative relations.

In this chapter I focus on the effect of these different constitutional structures on three aspects of the relationship between the executive and legislative branches of government that directly affect the performance and stability of democratic regimes: (1) government formation; (2) statutory policymaking; and (3) regime breakdown. To be clear, my aim is not to provide an explanation, or even a full account, of the connection between separation of powers, government crises, and democratic stability. Instead, my goal is to debunk a series of myths regarding the relationship between these phenomena.

I present new evidence for comparative research into democratic governance. In particular, I document the patterns of coalition formation as well as chief executives' statutory achievements in more than 50 countries in Western/Eastern Europe, North and Latin America, Asia, and the Middle East for the period between 1946 and 2012. The empirical evidence suggests that the difference in the frequency of coalitions, while favorable to parliamentarism, is not large. There is roughly a 50 percent chance that the president's party will hold a majority of seats, so that coalitions are not necessary. When the president's party does not control a majority of seats, coalitions occur more than half of the time and they are more likely to occur when the legislature is more fractionalized.

The data presented in this chapter also reveal that the relationship between chief executives' passage rates and the partisan makeup of the legislature is more complex than how it is usually depicted in the literature. Single-party minority presidents do not fare much worse than coalition governments. Moreover, the evidence indicates that "legislative impasse," "deadlock," or "stalemate" are rare events, even in the case of single-party minority governments under presidentialism.

Finally, in terms of the connection between executive-legislative relations and democratic stability the analysis presented in this chapter reveals that some intermediate degree of control of the legislature by the executive (i.e. accountability) is optimal. In other words, a strong government is hardly a pre-condition for political stability. These findings are consistent with the notion that placing limitations on a government's scope of action can be conducive to socially beneficial policy outcomes (North and Weingast 1989; Henisz 2000).

The remainder of this chapter proceeds as follows. In the next section, I briefly discuss how presidentialism and parliamentarism can be conceptualized. In the "Constitutional structures

and types of government” section, I focus on these constitutional structures’ workings and effects on the government formation process. I then document how these different ways of organizing power and authority affect statutory policymaking in the “Statutory policymaking” section. In the following section, I examine the relationship between social turmoil, executive-legislative relations, and political stability. A final section concludes.

Presidentialism and parliamentarism

Before examining how the organization of power and authority affects democratic performance, it seems appropriate to define each of the constitutional structures under consideration. Following Przeworski *et al.* (2000), presidentialism is understood here as a form of government in which: (1) the president is both the Head of State and the chief executive, and he/she is elected by voters (or an electoral college chosen by them for that sole purpose); (2) the terms of office for the president and the assembly are fixed, and are not contingent on mutual confidence. By contrast, parliamentarism is defined as a form of government in which: (1) there is a Head of State and a head of government. While the former plays merely a protocolary role, the latter is the country’s chief executive and is elected by, and responsible to, the legislature; (2) the terms of office for the executive and the assembly are not fixed, and are contingent on mutual confidence (Przeworski *et al.* 2000).¹

These definitions capture the essence of separate origin and survival of government (executive) and assembly. While parliamentarism is a system of “mutual dependence,” presidentialism is one of “mutual independence” between the executive and the legislature. Under parliamentarism, the government (executive) must resign if it no longer enjoys the confidence of the legislative assembly. A defeat on a confidence motion or an “important” bill is typically enough to bring a government down. This relationship is by no means one-sided. The executive typically has the power to recommend the dissolution of the legislature, and may in practice do this for no better reason than expected gains for the government parties at the ensuing election (Strøm 1990). In contrast, under presidentialism, the president cannot dissolve the legislature to call new elections and the legislature cannot replace the government by exercising a no-confidence vote.²

These criteria also clarify the constitutional relationship between the legislative and executive branches of government, distinguishing between fused and separation-of-powers forms of government. The efficient secret of the English Constitution, as Bagehot (1867: 12) put it, is “the close union, the nearly complete fusion of the executive and legislative powers.” In terms of their membership, the executive and legislative branches often overlap almost completely under parliamentarism. Members of the executive usually are drawn from the legislature and very often continue to sit there. In fact, a cabinet post is one of the main career goals of most ambitious legislators. In contrast, under presidentialism, cabinet members are usually banned from serving simultaneously as ministers and legislators. The president may ask members of political parties other than his/her own to serve in the cabinet, but they are selected as individuals, not as members of a legislative delegation.

Note that whether powers are fused (parliamentarism) or separated (presidentialism) is an important consideration in understanding the systematic impact of constitutional structures on policy outcomes. However, it is not determinative. For example, in presidential systems, when different parties control the executive and legislative branches, either branch may veto policy changes initiated in the other. In this case, each party constitutes what Tsebelis (1995) calls a veto player.³ Presidentialism appears to create more veto players than parliamentarism when the United States and the United Kingdom are compared. Widening the focus to include other types

of parliamentary and presidential systems, however, suggests that the simple distinction between parliamentarism and presidentialism does not determine the number of veto players. Two examples are parliamentary systems governed by multiparty coalitions and presidential systems where a single party controls the two branches of government.

Presidential and parliamentary regimes may both have devices that favor majoritarian decision-making, and others that give protection to the rights of minority parties and individual legislators. To fully characterize the different types of presidentialism and parliamentarism we must thus take into account the relationship among constitutional structure, party systems, and electoral rules. A well-established literature in political science stresses the relationship between electoral rules and party systems. Hence, roughly speaking, we can focus on two fundamental features of political institutions: the electoral rule, contrasting majoritarian and proportional electoral systems, and the regime type, contrasting presidential and parliamentary regimes. This gives us four different types of regimes: majoritarian presidentialism, majoritarian parliamentarism, proportional representation presidentialism, and proportional representation parliamentarism. In the discussion that follows I will refer to presidentialism and parliamentarism in general when such distinction within them is not relevant and I will characterize the different sub-types when needed.

Constitutional structures and types of government

It is clear from the discussion presented that parliamentarism and presidentialism are different. But how do these differences in institutional rules affect the government formation process? Do parliamentary systems always produce majority governments? How often do presidents rule with the support of a legislative majority? Should minority governments necessarily be considered “failures” of coalition formation? Is the difference in institutional rules sufficient to impede coalitions in presidential systems? And is it true that coalitions are exceptional in multiparty presidential systems? This section addresses these questions by examining the relationship between constitutional structures and government types.⁴

Majority/minority governments

Parliamentarism is a regime in which the government, in order to come to and stay in power, must enjoy the confidence of the legislature. This implies that all governments must enjoy a legislative majority. In contrast, there is nothing in the presidential system that guarantees that the executive will enjoy the support of the majority of the legislature. As conclusively demonstrated by Strøm (1990), however, it is not true that parliamentary systems will necessarily produce majority governments. In fact, minority governments are not uncommon under parliamentarism. These governments are composed by political parties or parliamentary groups that collectively control less than one half of all the seats in the chamber of the legislature to which the cabinet is constitutionally responsible (Strøm 1990: 6). By implication, minority governments thus violate the expectation that executive and legislative coalitions be identical. Nonetheless it remains true that, despite being out of the cabinet, parties that support the government on confidence votes and legislative bills (or both) need to exist. As Strøm points out, their existence usually depends on the calculus made by their leaders about the costs and benefits of participating in government. He argues that the degree of policy influence, as well as the competitiveness and decisiveness of the electoral process affect this calculus. And, more than anything else – Strøm contends – it is the anticipation of future elections that predisposes party leaders to opt for minority governments (Strøm 1990: 237).

In the case of presidential systems, minority governments can be defined as those situations in which the party of the president does not control a majority of seats in the legislature. These include both the cases where no party – including the president’s – controls a majority of legislative seats, and those where a party different from the president’s holds a legislative majority. The latter is associated with majoritarian two-party systems and corresponds to the situation of “divided government” in the U.S. sense, whereas the former is typical of proportional representation multiparty systems.

Similar to the case of parliamentarism, even if the president’s party is not majoritarian, the government can rule with the support of a legislative majority. It should be noted, though, that minority governments rule under parliamentarism because no majority wants to replace them. The same, however, is not true of presidential systems. Here a legislative majority may want to topple the government and yet would be constitutionally unable to form one. Moreover, under presidentialism, every government must include the president. Therefore, even if the opposition parties control a legislative majority and even if they want to form a common government, they cannot do it without including the president (Cheibub *et al.* 2004). This means that these parties will have to join the president or remain in the opposition and wait until the next scheduled election. If non-presidential parties believe that they will benefit electorally by opposing the president, they may be willing to wait. As with the case of parliamentarism, it is the anticipation of future elections that predisposes opposition parties to withdraw their support for the government. Unlike parliamentary systems, however, where the out-of-government legislative majority can ultimately replace the government, a presidential minority government will have to live with a hostile legislative majority until the end of its constitutional term. If this is the case, then, legislative deadlock or outright executive-legislative conflict may arise (Jones 1995). This is an outcome that is structurally unavailable under parliamentarism.

So how do these two different forms of government fare in practice? The frequency of majority and minority governments under both parliamentarism and presidentialism can be calculated using the 2012 release of the Database of Political Institutions. The sample includes 3,063 annual observations on 140 democratic countries between 1975 and 2012.⁵ The data reveal that a situation where no party controls a majority of seats in the legislature exists 62.4 percent of the time under parliamentarism and in 54.4 percent of the time under presidentialism. And, when this is the case, minority governments occur during 30.4 percent of the time under parliamentarism and 51.5 percent under presidentialism.⁶

Government coalitions

Whenever any single party controls a majority of parliamentary seats or the president’s party controls a majority of congressional seats, coalitions are not necessary to govern. As the empirical evidence indicates, however, it is often the case that no party possesses an overall majority of legislative seats. This leads to the distinction between single-party and coalition governments.

Government coalitions can be of two kinds. A portfolio coalition is a set of legislators belonging to parties that hold cabinet posts, while a legislative coalition is a set of legislators from different parties who vote together. If parties are disciplined, then every portfolio coalition is a legislative coalition. Legislative coalitions may vary from one issue to another. Such variations may arise from the fact that parties may vote together on some but not all issues or from lack of party discipline among members. Moreover, the two coalitions need not be coextensive. This is typically the case of minority governments under parliamentarism. Moreover, under presidentialism a majority legislative coalition may oppose the portfolio government (Cheibub *et al.* 2004).

Portfolio coalitions were traditionally considered by the literature to be rare and unstable in presidential regimes and frequent and stable in parliamentary regimes. The assumption being that the institutional differences between the two regimes was sufficient to create divergent incentives for coalition formation (Mainwaring 1993; Linz 1994; Jones 1995). This view, however, is at odds with the empirical evidence. Cheibub *et al.* (2004) demonstrate that the circumstances under which portfolio coalitions are likely to be formed are the same under the two systems. Their conclusion is that coalition governments may be less frequent but far from exceptional under presidentialism.

Their argument is the following: under parliamentarism, whenever a single party does not enjoy a stable legislative majority, coalitions result from formal negotiations among parties and entail a distribution of portfolios. Under presidentialism, this process is more unilateral: the president may just invite the cabinet members of parties other than his own. While parties can leave coalitions at any time in both systems, under parliamentarism a departure may topple the prime minister or at least cause an early election, while under presidentialism the departing party leaves for the *desert* until the next coalition or the next scheduled election, while the president continues to be the head of government. Waiting for the next election, presidential or legislative, will be unpleasant for the opposition parties: in the meantime, they hold no portfolios. But if they believe that they will benefit electorally by opposing the president, they may be willing to wait. Note, however, that while the opposition may have incentives to oppose the president, the president has incentives and the means to induce the opposition to cooperate (Cheibub *et al.* 2004).

The outcomes, in terms of coalition formation, depend on the allocation of legislative powers: (1) If the opposition can legislate – it can initiate legislation and/or override the president’s veto – then the outcome will be either (a) minority presidential government, whose proposals are supported by a legislative majority, (b) coalition government, or (c) minority presidential government with policies set by the opposition in the legislature. (2) In turn, if the legislature cannot initiate legislation or if the president can veto legislation without being overridden, the outcome will be either (a) minority presidential government supported by a legislative majority, (b) coalition government, or (c) “legislative paralysis” in which the president proposes legislation and the opposition votes it down without being able to adopt its own policies, so that the policy remains at some status quo (Cheibub *et al.* 2004).⁷

Table 12.1 presents a classification of government types based on their coalition status.⁸ The empirical evidence corroborates the view that coalitions are less frequent but far from exceptional under presidentialism.

In fact, the coalition majority status is the modal category under both systems (with 50 percent of the observations in the case of parliamentarism and 37.7 percent under presidentialism). Single-party minority governments are more frequent under presidentialism than under parliamentarism, but the difference is one of degree, not of kind.

Table 12.1 Type of government by constitutional structure (frequencies)

Type of government	Parliamentarism	Obs.	Presidentialism	Obs.
Single majority	30.7	510	31.7	445
Single minority	7.1	118	19.3	272
Coalition majority	50.0	830	37.7	529
Coalition minority	12.2	201	11.2	158

Source: 2012 Database of Political Institutions.

Statutory policymaking

Having established that coalition governments are less frequent but far from exceptional under presidentialism, it is worthwhile to ask “So what?” The entire focus on coalition formation is predicated on the assumption that minority governments are legislatively less successful and that various deleterious consequences follow. We thus need to identify the main factors that allow chief executives to rule by statute. And more specifically, to examine what combination of institutional and partisan considerations determines whether or not legislators will support a chief executive’s agenda. The purpose of this section is to address these issues by documenting patterns of statutory policymaking around the world.⁹

Ruling by statute

Chief executives can create policy in a variety of ways. For example, they can act without the explicit consent of the legislative branch, and “legislate” on their own through executive orders, decrees, and regulatory ordinances. Yet, the use of executive prerogatives as a source of law has important limitations. Decrees, for example, are usually seen as an exceptional policymaking instrument, and thus are particularly sensitive to judicial review. And, in some countries they can be overturned by legislature. In contrast, the legislative approval of statutes often allows chief executives to better insulate their policy choices from legal review (Remington *et al.* 1998; Amorim Neto 2006).

Statutory implementation of policy, though, is often complex, as it depends on the interactions between the executive and the legislature. Hence, it is in the realm of lawmaking that we should examine the various combinations of institutional and partisan considerations that determine whether or not legislators will support a chief executive’s legislative agenda. Scholars of comparative politics have traditionally argued that chief executives require adequate partisan support in the legislature to govern. Conflicting arguments and findings about the effect of partisan support on statutory policymaking, however, leave open the questions of why and when governments are able to successfully enact policy changes through statutes (Isberg 1982; Saalfeld 1990; Shugart and Carey 1992; Lupia and Ström 1995; Foweraker 1998; Cheibub *et al.* 2004; Cheibub 2007).

From an empirical standpoint, the greatest challenge is the lack of truly cross-national research on this topic. While the study of presidential legislative success in the United States has a long and fruitful tradition, these analyses seldom provide systematic comparisons with other countries. Likewise, most comparative research on this topic relies on either case studies of particular acts of government or from country studies. Another substantial impediment to conducting research on statutory policymaking at the cross-national level is the lack of a clear definition of legislative success. Students of executive-legislative relations use several measures and various units of analysis. In fact, passage, success, productivity, support, concurrence, dominance, control, and influence all appear in the scholarly literature (Edwards 1980, 1989; Shull 1983; Bond and Fleisher 1990; Peterson 1990) and sometimes are used interchangeably.

Chief executives’ comparative statutory performance

It is safe to assume that in most, if not all cases, chief executives are not only concerned with whether their initiatives are considered by the legislature, voted upon, or almost pass, but also if the proposed legislation is enacted into law. Moreover, statutes are the definite measure of legislative output, whereas votes and positions on issues are merely means to an end of an

uncertain consequence. Therefore, if the primary aim is to investigate how successful chief executives are in promoting their policy agendas in the legislature, it is most appropriate to use a box score. This indicator is calculated as the percentage of executive initiatives approved by the legislature.

The box score is analogous to a batting average (i.e. number of hits as a proportion of times at bat). As such, it summarizes a chief executive's record of wins and losses (Bond *et al.* 1996). Despite some of its limitations, the box score is a tangible indicator that makes it possible to compare different chief executives and to assess their relative performance under varying circumstances. Indeed, as Rivers and Rose (1985) and King and Ragsdale (1988) note, this is an ideal measure from a conceptual standpoint.

Figure 12.1 presents the distribution of box scores in a sample of 52 countries in Western/Eastern Europe, North and Latin America, Asia, and the Middle East for the period between 1946 and 2008.¹⁰ The data reveal that chief executives' passage rates vary considerably across as well as within constitutional structures.

It is clear from Figure 12.1 that the passage rates of chief executives under parliamentarism are higher than presidential ones. The box scores from these two forms of government are indeed statistically distinct; a simple two-sample t-test indicates that one can safely reject the null hypothesis that no difference between the two systems exist at the 99 percent confidence level. One can also reject the hypothesis that box scores are higher under presidential regimes.

Another way to examine the variation in chief executives' legislative performance is to classify the observations according to the partisan distribution of seats in the legislature. Regardless of constitutional structures, the average box score of democratically elected leaders with majority governments is 78.5 percent, relative to an average passage rate of 72.7 percent in the case of chief executives under minority governments. When accounting for constitutional structures, some interesting patterns emerge. Under parliamentarism, majority and minority chief executives possess very similar box scores (roughly 83 percent), but the average box score for majoritarian presidents (67.5 percent) is higher than those in the minority (62.6 percent). One important consideration is that chief executives under both types of regimes may try to boost their legislative base of support by crafting government coalitions. Therefore, the legislative performance of chief executives under minority/majority governments could potentially be affected by the coalition status of their administrations.

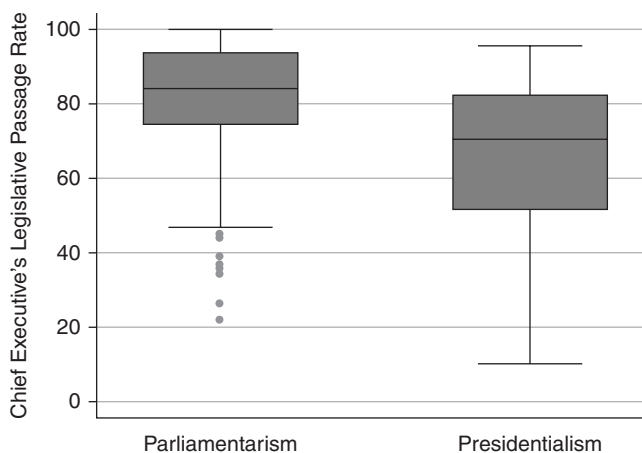


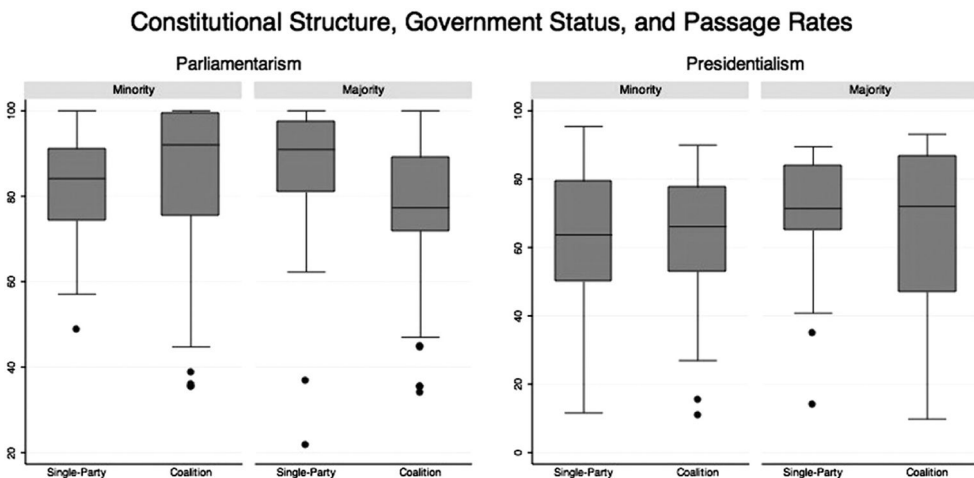
Figure 12.1 Distribution of chief executives' box scores

To analyze this possibility, I examine whether a chief executive's government is (1) a single-party majority; (2) a single, party minority; (3) a majority coalition; or (4) a minority coalition. The results are presented in [Figure 12.2](#).

Prime ministers who lead single-party majority governments enjoy the highest average legislative passage rates (88 percent), followed by those who rule under minority coalitions (84 percent). Prime ministers who rule under a majority coalition are the least effective ones (with an average box score of 76 percent), followed by those leading single-party minority governments (with an average box score of 82 percent). Still, as the data indicate, even under parliamentarism, single majority governments do suffer legislative defeats (including Westminster-type governments). In the case of presidentialism, single-party minority governments exhibit higher passage rates (an average of 70 percent) than do coalition majority (66 percent) and coalition minority (62 percent) administrations. As Cheibub *et al.* (2004) note, government coalitions tend to form when the policy distance between a minority party in government and the rest of the parties in the legislature is large. Therefore coalition governments are typically quite heterogeneous and have more players who could potentially veto a change.

Notice also that single-party minority presidents do not fare much worse than coalition governments. On average, 62 percent of single-party minority presidents' bills are approved by the legislature. Hence, it is clear that legislative paralysis is a relatively rare phenomenon, even under presidentialism. Moreover, it is apparent from these data that prime ministers possess higher legislative passage rates than presidents: the percentage of government bills approved in the legislature is higher under parliamentarism than under presidentialism, regardless of government coalition or majority status.

The patterns presented in [Figure 12.2](#) suggest that a relationship between chief executives' legislative passage rates, their country's constitutional structures, as well as the status of their governments exists. To evaluate the performance of chief executives in a multivariate setting, I estimate a statistical model with chief executives' box scores as my dependent variable and cross-country differences in institutional design as the primary correlates of interest. I also control for some additional features, such as the share of seats held by the government, the government status, electoral rules, and the structure of the legislature.¹¹ The dependent variable is the



[Figure 12.2](#) Box scores by government status

proportion of bills initiated by the chief executive and approved by the legislature of his or her respective country in a given year (expressed in its logit transformation).

Table 12.2 presents two alternative specifications.¹² The results indicate that, relative to Westminster-style parliamentary systems, passage rates are lower in non-Westminster parliamentary countries, in semi-parliamentary regimes, and especially under presidentialism.¹³

One possible explanation for the patterns presented in Figures 12.1 to 12.2, though, is that the box score data are subject to a form of self-selection bias that favors chief executives under parliamentary systems. As Cheibub *et al.* (2004) note, since prime ministers risk losing the confidence of the legislature when they are defeated, they must be careful in proposing legislation. Presidents, as the argument goes, can be more reckless: if they are indifferent to the status quo, they can initiate bills expecting to be defeated to embarrass the opposition.

According to the data, in a given year, the representative prime minister introduces 131 pieces of legislation, while the average president initiates 109 pieces of legislation. Statistically, however, there is no difference between the numbers of bills initiated by the two types of chief executives:

Table 12.2 Governments' legislative passage rates: multivariate analysis

	Country Clustered	Regional Effects
Non-Westminster Parliamentary	-1.896*** (0.531)	-1.941*** (0.252)
Semi-Parliamentary	-1.962*** (0.529)	-2.007*** (0.275)
Presidential	-2.645*** (0.533)	-2.379*** (0.388)
Government's Seat Share	1.595** (0.623)	1.785*** (0.446)
Coalition Government	-0.648*** (0.203)	-0.547*** (0.114)
Electoral Rules	0.795*** (0.261)	0.210 (0.208)
Average District Magnitude	0.004* (0.002)	0.004** (0.002)
Seats from National District	1.346** (0.618)	1.555*** (0.456)
Bicameral System	0.193 (0.283)	0.129 (0.144)
Asia		0.681** (0.296)
Latin America		-0.466 (0.372)
Eastern Europe		-0.289 (0.313)
Middle East		-0.729*** (0.249)
Intercept	2.249*** (0.563)	2.314*** (0.321)
N	272	272
R ²	0.473	0.52

Notes:

Standard errors are in parentheses.

* indicates significance at a 10% level;

** indicates significance at a 5% level;

*** indicates significance at a 1% level.

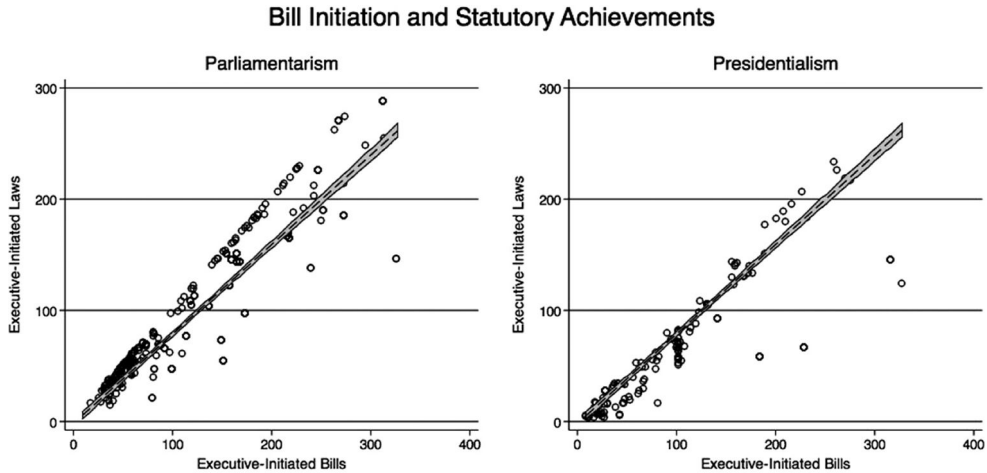


Figure 12.3 Bill initiation and statutory achievements

a difference of means test indicates that the null hypothesis cannot be rejected at conventional levels. Therefore, at least with regard to the amount of legislation introduced by the executive to the legislature every year, the evidence indicates that presidents are not necessarily more reckless than prime ministers.

It might be argued that a chief executive's carefulness is reflected in the content rather than the amount of legislation. Unfortunately the argument cannot be put to a test using the available data. Nonetheless, it is still possible to gauge how "strategic" chief executives are when it comes to bill initiation. In particular, the data can be used to address the following questions: (1) is it true that some chief executives can manage to fatten their "batting average" by withholding legislation?; and (2) what is the relationship between bill initiation and statutory achievements?

Figure 12.3 shows the number of executive-initiated bills approved by the legislature as a function of the total number of proposals introduced by the chief executive in a given year (i.e. the box score's numerator and denominator, respectively). The dashed line in Figure 12.3 represents the predicted number of executive-initiated laws obtained from a linear regression, where the number of executive-initiated laws is regressed on the number of executive bills. The shaded areas are the 95 percent confidence intervals around these estimates.

The evidence clearly rejects the notion that chief executives can obtain higher passage rates by initiating less legislation. The data on Figure 12.3 also indicate that the difference between presidentialism and parliamentarism regarding passage rates is unrelated to bill initiation. Finally, even though the data are not rich enough to test the claim directly, the patterns in Figure 12.3 suggest that chief executives under different constitutional structures do not necessarily successfully adjust the content of their bills all the time either.

Regime stability

As noted in the introduction, chief executives both under presidentialism and parliamentarism, may require adequate partisan support in the legislature to govern (in the case of the former), as well as to survive in office (in the case of the latter). Most scholars point out that the powers that the executive derives from partisan support in the legislature can be as important as those derived from authority constitutionally vested in the office. And, numerous studies have noted that

party systems influence the workability of executive-legislative relations. For example, focusing on the experiences of the Weimar Republic, the Third and Fourth French Republics, and Italy during the Cold War, some of the literature in the mid-1990s claimed that extreme executive-legislative conflict would inevitably lead to government deadlock and/or regime breakdown (Linz 1990; Mainwaring 1990; Stepan and Skach 1993; Linz 1994; Valenzuela 1994; Mainwaring and Scully 1995; Linz and Stepan 1996; Huang 1997).¹⁴

Most of this literature, however, is primarily inferential. It does not offer much evidence of governmental performance, and the term is vaguely defined via constitutive referents, such as stability or viability. Thus, the bulk of the research infers performance from probable causes, like constitutional structures and political fragmentation. But, as Di Palma (1977) notes, performance is closely related to the execution and accomplishment of an intended task. It refers to "... what is rendered, given back, returned, yielded, in short, to outputs ..." (p. 7). Rules represent one of the most significant outputs of a political system; therefore, rule-making epitomizes political performance. Therefore, by linking the notion of governability to chief executives' ability to enact policy changes, it should be possible to examine whether chief executives unable to accommodate change are threatened.

Recent research in comparative politics has demonstrated that executive-legislative confrontation is not a necessary condition for political instability (Pérez-Liñán 2007). Instead, the ability of the opposition to remove a chief executive from office ultimately hinges on the degree of popular mobilization against the government. It should be noted, though, that social conflict, turmoil and even violence can be the product of the government's incapacity to solve urgent societal problems. These phenomena, however, can also be the result of unpopular policies. Therefore, governments often face a number of dilemmas and trade-offs. In this section, I evaluate these dilemmas both from an empirical as well as a normative standpoint. Empirically, I study the relationship between chief executives' legislative passage rates and social upheaval. From a normative standpoint, the goal is to establish if, in terms of governability, some intermediate degree of control of the executive by the legislature (i.e. accountability) is optimal.¹⁵

The political gap revisited

More than 40 years have passed since the late Samuel P. Huntington argued that Great Britain, the United States, and the Soviet Union belonged to the same category of political systems. According to his landmark expression, in "...all three systems the government governs..." (Huntington 1968: 1). What he meant was that in these countries, the cabinet, the president, or the politburo could successfully enact policy changes. To establish how policy immobilism and governability are related, it would be ideal to know if regime stability is threatened when governments are unable to successfully enact policy changes. Unfortunately, endogeneity problems and the lack of appropriate data pose significant barriers to answering this question directly.

Nonetheless, this issue can be indirectly addressed, by examining the relationship between chief executives' legislative passage rates and social/political unrest. Social conflict can be the result of extraordinary problems. Yet, it can also be the product of a government's inability to address such challenges. External or domestic threats may require immediate action by the authorities, and failure to do so may lead to social and political chaos. Similarly, engaging in partisan squabbling during times of economic crisis could generate social and political unrest. Therefore, poor performance by the incumbent administration during critical times can incite popular discontent. In practical terms, there should be a correlation between chief executives' passage rates and observable manifestations of social unrest – such as protests, demonstrations, riots, strikes, road blockages, and so on. In particular, chief executives with higher passage rates

should be less likely to face popular protests than chief executives unable to accommodate change.

Political upheaval may also be the product of unpopular policies. As Przeworski (2010) notes, majority rule generates winners and losers, and authorizes the winners to impose their will on the losers, even if within constraints. The losers may try to persuade the government to modify its views; or they may be able to exercise their institutional prerogatives to block some legislation. Suppose, however, that the government is certain that its preferences will triumph in the policymaking process. Then, the political losers might graciously accept their short-term destiny and hope to do better in the future, or they may turn to violence out of desperation. Should the losers engage in the latter, the government can decide to persevere and repress the protests, wait for the protests to subside while tolerating a breakdown of order, or accommodate the demands of its opponents. So, if governments are too powerful within the institutional framework, they may achieve the paradoxical effect of undermining political stability (Przeworski 2010; Machado *et al.* 2011).

A non-linear relationship should thus be observed between a chief executive's legislative passage rate and social unrest. Specifically, chief executives' statutory performance should incite popular discontent when: (1) passage rates are extremely low (i.e. stalemate); and (2) passage rates are extremely high (i.e. lack of accommodation of opposition's demands). In contrast, social turmoil should decrease when chief executives' passage rates are at moderate levels. The solid line in Figure 12.4 presents the predicted number of violent demonstrations as a function of chief executives' box scores, while the shaded areas denote the 95 percent confidence intervals around these estimates.¹⁶

Chief executives' legislative passage rates and levels of social unrest exhibit a non-linear relationship. The predicted number of violent demonstrations first diminishes and then increases in statutory performance. Since observations of extremely ineffective governments (i.e. legislative paralysis) are few, the standard errors are large. The relationship between passage rates and social upheaval, however, is pronounced when the opposition poses little chance of blocking the executive's proposals. Countries governed by chief executives whose legislative passage rates are above 85 percent are statistically more likely to experience a larger number of riots than those ruled by chief executives with lower passage rates.

The evidence thus suggests that polities are more stable when the chief executive passes some of its agenda through the legislature. But, at the same time, the opposition must have a reasonable

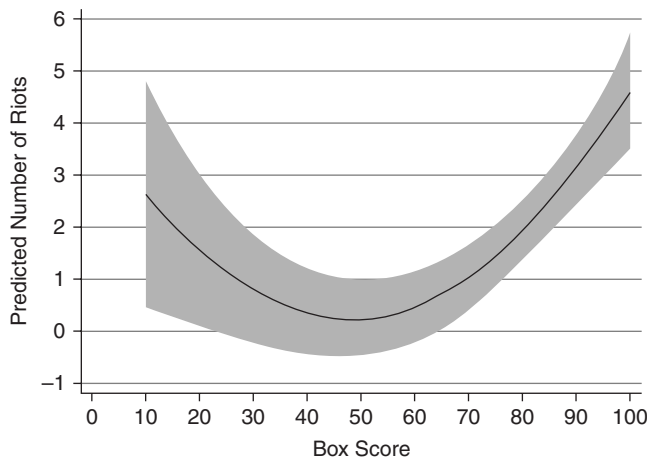


Figure 12.4 Riots and passage rates

chance of defeating some government proposals. As such, these results lend support to Ralph Dahrendorf's views on governability:

... A free society does not need a strong government. It may indeed fare better if government is fairly inactive and quiet. But a free society needs an unworried government, and that means one which is effective where necessary and legitimate throughout ...
(Dahrendorf 1980, 409–410)

Conclusions

The empirical evidence presented in this chapter indicates that, while the observation of the relatively shorter life of presidential democracy stands, the Linzian concern with concentration of executive authority under presidentialism appears to be unwarranted. Instead, the empirical evidence seems to support Przeworski's (2010) view that, at least part of the answer to the question of "What makes democracies endure?" lies in the willingness of the relevant political actors to process their conflicts through the system of representative institutions and to accept the outcomes these institutions generate.

As such, it seems like much of the variation in democratic survival across countries depends on the different strategies employed by their main political actors regarding government formation, statutory policymaking, and the institutionalized accommodation of dissenting views. In this respect, there is still much to be learned about each of these aspects of the relationship between executives and legislatures under alternative constitutional structures.

Take the case of government formation. A large and important literature that uses game theoretic models of bargaining to study this phenomenon has emerged over the last two decades. These contributions have shed light on which coalitions are most likely to form, how long governments last, and the consequences of various institutional arrangements. The lion's share of this literature, however, focuses on parliamentary democracies. Some recent empirical work has broadened our understanding of the strategic use of cabinet appointments by executives in presidential systems (Amorim Neto 2006; Negretto 2006; Raile *et al.* 2011; Martinez-Gallardo 2012). But, our understanding of government formation in presidential systems still remains limited.

For example, students of parliamentarism have yet to settle the issue of whether there is an advantage to being "*formateur*" (the party called to form the government). While a substantial body of empirical research indicates that there is little advantage (the so-called "Gamson's Law"), other studies find that, once bargaining power is accounted for, *formateurs* do enjoy sizable advantages (Warwick and Druckman 2001; Snyder *et al.* 2005).¹⁷ In presidential democracies, as discussed, the process of government formation is more unilateral. Therefore these regimes provide the natural ground where theories of *formateur* advantage should be tested. Recent empirical work by Amorim Neto and Samuels (2011) has examined some of these issues. Nonetheless more research, both deductive as well as empirical, on how the government formation process works under different constitutional structures is needed.

With regard to statutory policymaking, as the analysis presented indicates, constitutional arrangements and partisan configurations do matter. These features, however, are not the key to understanding why governments suffer legislative defeats. Saiegh (2011) identifies two major factors that shape lawmaking: the unpredictability of legislators' voting behavior, and whether buying legislative votes is a feasible option. The source of the uncertainty is the existence of cross-pressured legislators: in deciding how to vote, lawmakers consider a variety of influences, including their personal values, announced positions, the views of their constituents, and the

preferences of their party leadership. Therefore, legislators' voting behavior can seldom be perfectly anticipated.

The emphasis on the unpredictability of legislators' behavior elucidates the empirical puzzle posed by chief executives' legislative defeats. It also leads to some clear empirical implications regarding the relationship between legislators' induced preferences and statutory policymaking. The existence of a winning voting coalition depends on the partisan distribution of seats in the legislature but also on the distribution of the policy preferences of legislators' supporters. If a legislator's partisan identity accurately predicts her constituency's ideal policies, then a chief executive may be able to calculate more accurately how she will cast her votes. In contrast, if partisanship is weakly correlated with constituency interests, chief executives are more likely to make mistakes.

Saiegh (2011) argues that the uncertainty surrounding statutory policymaking is in part related to constitutional and electoral structures. For example, the extent to which legislators represent a "national" rather than a "local" constituency is an important institutional factor that affects the correlation between partisans' and districts' ideal policies. Nonetheless, we still have very little knowledge regarding the unpredictability of legislators' voting behavior at the micro level. Most studies rely on recorded votes in legislatures (roll call data) to measure politicians' spatial preferences. But, very few studies concentrate on the predictability of legislators' voting behavior rather than on their ideological location.¹⁸

Buzard and Saiegh (2014) study how trade policy is determined when lobbying effort is endogenous, ratification of free trade agreements is needed, and legislative decision-making is not perfectly predictable. In particular, they focus on cross-industry trade policy and lobbying efforts for the three trade agreements that were passed during the 112th Congress in the United States. Using roll call votes and Bayesian Markov chain simulation statistical methods, they generate estimates of legislators' ideal points as well as their 95 percent posterior confidence intervals. The latter are of particular interest to Buzard and Saiegh (2014), as they use them to gauge the unpredictability of each legislator's voting behavior. Finally, using subsets of roll call votes, they estimate how friendly or unfriendly each legislator is with respect to a certain industry, as well as the unpredictability of his/her voting behavior when matters affecting that industry are considered. The analysis of political uncertainty in Buzard and Saiegh (2014) is a step in the right direction. However, by focusing on the United States, their work has not produced cross-national measures of political uncertainty.

Finally, the cross-national evidence presented in this chapter suggests that governmental *performance*, defined as the ability of chief executives to enact policy changes through statutes, affects the governability of a polity in complex ways. The findings also lend indirect support to the notion that regime stability is not threatened by ineffective governments. Furthermore, the analysis shows that in the developing world, popular discontent is higher when chief executives' passage rates are extremely high than when they are extremely low. And, if popular uprisings are a necessary condition for regime breakdown, then even deadlocked governments may be able to *survive without governing* (Di Palma 1977; Pérez-Liñán 2007).¹⁹

The findings presented have another important implication for the study of regime breakdown. Most clearly, they demonstrate that governability is best served in developing countries when the chief executive passes some of his agenda through the legislature but not when the opposition has no possibility of blocking any government proposals. Therefore, democracy is possibly threatened when the government tries to do too much rather than doing too little.

Consider the fall of Chile's Salvador Allende in 1973. According to some scholarly interpretations, this episode exemplifies how policy immobilism leads to a breakdown of democracy (Linz 1990;

Valenzuela 1994). It should be noted, though, that despite facing a hostile Congress, Allende passed more than a hundred pieces of legislation in 1971 (Alemán 2009). His passage rate fell precipitously in 1972 (he passed fewer than 30 laws), but he hardly faced complete and absolute gridlock (Alemán 2009). The notion that Allende stood idle in the face of congressional opposition also seems unwarranted. Without the passage of any new legislation, and using an obscure law dating from 1932, Allende managed to nationalize 187 firms (including industrial establishments, banks, and media companies) by the end of his first year in office. And, while the conflict with Congress increased the illegitimacy of Allende's social reforms in the view of the opposition parties who withheld their consent, it was not enough to block them (Cohen 1994). As opposition forces found themselves increasingly limited in stopping Allende's agenda in the legislative arena, they took the struggle to the streets. By September 1973, a truckers' confederation strike that had started a few months earlier finally strangled Chile's flow of supplies, making a major contribution to the crisis atmosphere in which the coup took place (Goldberg 1975).

This historical example is quite illustrative. But, it does not provide conclusive evidence to support the view that democracy may be threatened when the government tries to do too much rather than doing too little. In other words, one could also find examples indicating that democracy is threatened when the government is legislatively unsuccessful. One possible direction would be to empirically examine the issue in a statistical fashion. Unfortunately, despite being the most comprehensive dataset on chief executives' legislative passage rates in existence, the data compiled by Saiegh (2011) are still inappropriate to conduct a direct test of the relationship between policy immobilism and regime stability. As such, this is another area that deserves future research.

Notes

- 1 Other forms of government cannot be classified as parliamentary or presidential based on these criteria. These are countries where the president is elected for a fixed term, but the government serves at the discretion of the parliament. These constitutional structures are often referred to as "premier-presidential," "semi-presidential," or "mixed" (Przeworski *et al.* 2000).
- 2 There are no pure presidential democracies in which the president has the authority to dissolve the legislature, as French and Finnish presidents may do. The Peruvian Constitutions of 1933, 1979, and 1993, though, have all allowed the president to dissolve Congress in response to repeated censures of cabinets. Uruguay's Constitution also allows for dissolution after censures, but the censure provision requires a two-third vote (rather than a majority as in Peru), making it very difficult to enact (Mainwaring and Shugart 1997: 18).
- 3 A veto player is an individual or collective actor "whose agreement is required for a policy decision" (Tsebelis 1995: 293).
- 4 This section draws from previous work with José Cheibub and Adam Przeworski (Cheibub *et al.* 2004).
- 5 The Database of Political Institutions (DPI) is compiled by the Development Research Group of the World Bank for research in comparative political economy and comparative political institutions. Beck *et al.* (2001) present the database; the coding rules and sources are described in detail in the codebook (Keefer 2005).
- 6 In a study of 132 governments in 12 parliamentary democracies, Taylor and Laver (1973) find that 34.1 percent constitute minority governments. Arend Lijphart (1984) reports the proportion as 67 of 218 (30.7 percent) governments in 20 countries over the period from 1945 to 1980. Cheibub (1998) finds a similar proportion of cases using data for 21 industrialized parliamentary regimes between 1946 and 1995.
- 7 Cheibub *et al.* (2004) also note that portfolio coalitions will be formed only when the president's party and the one closest to it are relatively distant in policy terms. Otherwise, the president governs alone, satisfied with the legislative outcomes. Therefore, they conclude that coalition governments are necessarily heterogeneous.
- 8 It should be noted, though, that the definition of government parties used by the Database of Political Institutions is different from the one used by Cheibub *et al.* (2004). Unlike, the latter (who focus on

- portfolio coalitions), the authors of the DPI dataset classify parties as being in government on the assumption that they support the president/prime minister.
- 9 This chapter includes a few excerpts from my book *Ruling by Statute: How Uncertainty and Vote Buying Shape Lawmaking*, published by Cambridge University Press in 2011. Reprinted with permission of Cambridge University Press.
 - 10 The data are displayed using a boxplot. Each box extends from approximately the first to third quartiles. Observations more than the 1.5 interquartile range beyond the first or third quartile are plotted individually. See Saiegh (2011) for information about the composition of the sample, and the sources from which the data were obtained.
 - 11 The variable *Electoral Rules* takes the value of 1 if plurality governs the majority/all of the seats in the lower house of the national legislature, 0 if proportional representation is used, and 0.5 if it is a mixed system. Source: Keefer (2005). The variable *Average District Magnitude* is calculated as the total number of seats allocated in the lowest tier divided by the total number of districts in that tier. Source: Golder (2005). The variable *Seats from a National District* indicates the proportion of legislators that are elected via a national tier to the lower house of the national legislature. Source: Wallack *et al.* (2003). The variable *Bicameral System* takes the value of 1 if the national legislature is bicameral; 0 otherwise. Source: Wallack *et al.* (2003).
 - 12 The first column reports the results of a model in which standard errors are robustly estimated and the disturbance terms for each country are allowed to be correlated, while the second column presents the results of a model with regional dummies.
 - 13 The following countries in the sample were coded as Westminster-style systems: Canada, Bangladesh, Ireland, Malta, United Kingdom, and New Zealand.
 - 14 In the case of presidential regimes, according to this view, when party systems fail to provide the president with sufficient legislative support, "...there is no alternative but deadlock..." (Mainwaring and Scully 1995: 33), and "...the norm is conflictual government..." (Jones 1995: 38). Therefore, "...the very notion of majority government is problematic in presidential systems without a majority party ..." (Huang 1997: 138), "...stable multi-party presidential democracy ... is difficult..." (Mainwaring 1990), and "...presidential systems which consistently fail to provide the president with sufficient legislative support are unlikely to prosper..." (Jones 1995: 38). Or, as Tsebelis (1995: 321) put it "...in regimes where government change is impossible (except for fixed intervals like in presidential regimes), policy immobilism may lead to the replacement of the leadership through extra-constitutional means..."
 - 15 This chapter includes a few excerpts from my book *Ruling by Statute: How Uncertainty and Vote Buying Shape Lawmaking*, published by Cambridge University Press in 2011. Reprinted with permission of Cambridge University Press.
 - 16 I model the relationship between chief executives' legislative passage rates and social unrest by fitting a second-order polynomial on the data. I use the box score measure and data on social upheaval collected by Banks (1996). The main variable of interest, Riots, measures the number of violent demonstrations or clashes of more than 100 citizens that involve the use of physical force. I restrict my attention to democratic countries with a per capita income below \$6,055 (measured in 1985 purchasing power parity dollars).
 - 17 For an exception of the latter see Carroll and Cox (2007). They extend standard bargaining models by allowing parties to form pre-election pacts, which potentially introduce a strong Gamsonian element into portfolio allocations.
 - 18 An analysis focusing both on legislators' ideological location as well as the predictability of their behavior based on their partisanship is presented in Saiegh (2011), [Chapter 8](#).
 - 19 Indeed, Pérez-Liñán (2007) concludes that popular uprisings were the only condition necessary to remove an elected president from office in Latin America between 1978 and 2005.

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13

COALITION FORMATION AND POLICYMAKING IN PARLIAMENTARY DEMOCRACIES

Lanny W. Martin and Georg Vanberg

Introduction

The majority of the world's advanced industrial democracies are parliamentary systems, and in most of those, coalition government is the norm. Thus, it is not surprising that understanding how coalition governments come about, and how they operate, has been a primary focus of comparative scholars. Until recently, most studies in this area focused primarily on the formation and dissolution of multiparty governments, rather than on the behavior of coalition partners between those events. Moreover, they adopted what was, in essence, an "institutions-free" approach to explaining coalition bargaining. Following the advent of "new institutionalism" in the 1980s, researchers began to consider the role of institutions in coalition politics more seriously, particularly with respect to coalition formation. Soon thereafter, scholars turned their attention to the policymaking process between the birth and death of coalitions. In these accounts, institutional structures at the executive and parliamentary levels play a central role. In this chapter, we survey key findings regarding the impact of institutions on coalition politics, focusing on government formation and the subsequent policymaking process.

Institutions and coalition formation

In one sense, institutions have always been at the heart of coalition research. After all, were it not for the constitutional rule that governments must maintain the confidence of a legislative majority, along with proportional electoral rules that virtually ensure that no single party controls a majority of seats, there would be no need for coalitions. However, most early accounts of coalition formation placed little explicit emphasis on the role of institutions. Relying primarily on the tools of cooperative game theory and spatial modeling, these studies produced "institutions-free" hypotheses about the impact of size, ideology, and incumbency on coalition formation. For example, the seminal contributions of Gamson (1961) and Riker (1962) focus only on the desire of political parties to acquire the benefits of office. Their well-known prediction is that only coalitions of minimal winning size—i.e. majority coalitions containing no unnecessary parties—should form, since these governments allow coalition partners to maximize office benefits.¹

As Laver and Schofield (1990) note, the inability of pure office-seeking theories to explain a large number of governments that form—i.e. minority governments and surplus majority

governments—led coalition scholars to incorporate policy considerations into their explanations. Axelrod (1970) and De Swaan (1973), for example, argue that coalitions are more likely to form if they consist of parties that are ideologically compatible. Laver and Schofield (1990) predict that governments are likely to include the median party (since its position is majority-preferred to all others in a unidimensional policy space), while minority governments are more likely if the opposition is ideologically divided.

Although incorporating policy considerations improved the predictive accuracy of coalition theories, their success rate, as Franklin and Mackie (1983) pointed out, was still modest. They argued that previous work failed to account for past experience: While parties may prefer small, ideologically compact coalitions, they also prefer partners with whom they have worked successfully in the past. This led to the prediction that an incumbent coalition should be advantaged in future negotiations (see also Warwick 1996). Expanding on this work, Martin and Stevenson (2010) argue that the advantage conferred on the incumbent coalition is conditional on how the government terminated and its electoral performance, an argument for which they find strong empirical evidence.

All these theories, which highlight the size, ideology, and incumbency status of prospective coalitions, did not explicitly consider how political institutions might impact coalition bargaining. With the emergence of the “new institutionalism,” this began to change. As Martin and Stevenson (2001) point out, these new-institutionalist theories of coalition formation can be classified into two groups: (1) those that focus on institutions that directly affect the formation process, and (2) those that focus on institutions governing policymaking after formation (which may affect formation because prospective coalition partners anticipate the effects of these institutions).

Within the first group, scholars emphasized three features of the coalition bargaining process. One is the presence of a formal *investiture requirement*, which obliges a coalition to survive a majority vote in the legislature before taking office. Strøm (1990) suggests that minority governments are more likely in the absence of an investiture requirement. The argument is that minority governments are forced to rely on the support of parties in the opposition to govern. One strategy they can use to overcome this challenge is to build ad hoc legislative coalitions on an issue-by-issue basis. To pass an investiture vote, however, a government must gain opposition support on its *entire* policy package (typically enshrined in a coalition agreement or government program), which may be difficult to do. Although the argument is plausible, it is not immediately clear why a minority government could not simply “water down” its program or avoid discussion of controversial issues altogether if doing so is critical to securing opposition support. Empirically, the impact of the investiture rule is also not clear. While Martin and Stevenson (2001) find that the need for an investiture vote decreases the probability of minority government, analysis of an expanded dataset finds no such effect (Martin and Stevenson 2010). In short, the importance of the investiture requirement for coalition formation is not settled theoretically or empirically.

Another bargaining institution that has been considered is the *continuation rule*, which allows an incumbent government to remain in office after an election without any formal action. The expectation is that such a rule should privilege the incumbent administration over other prospective coalitions.² Martin and Stevenson (2010) examine this possibility empirically, and demonstrate that the continuation rule increases the odds that an incumbent government will re-form after an election, assuming that it did not terminate due to conflict. Thus, the continuation rule, unlike the investiture rule, does appear to have a direct effect on government formation.

A third institutional feature believed to affect coalition bargaining concerns the role of the *formateur*—the individual (usually a party leader) charged with leading coalition negotiations.

In seminal work, Baron and Ferejohn (1989) (BF) model government formation as a noncooperative bargaining game in which an exogenously recognized formateur makes take-it-or-leave-it proposals to prospective partners. Because the formateur has monopoly proposal powers, his party should have a marked advantage in bargaining. Specifically, the BF model (as well as its many variants) predicts that the formateur party will get into government and extract a disproportionate share of cabinet ministries (relative to its voting weight).

In recent years, scholars have challenged the underlying premises of models in the BF tradition. For example, Laver *et al.* (2011) argue that the idea of a randomly selected formateur who can make take-it-or-leave-it offers is “a figment of the modeler’s imagination” (296). Instead, they argue, government formation is a product of private negotiations by sophisticated politicians. Partly in response, other approaches have relaxed some of the BF assumptions. For example, in the alternating-demands model of Morelli (1999), the formateur party does not enjoy the exclusive right to make cabinet proposals. Rather, it is the “first mover” in a game in which parties sequentially demand a share of ministries. The formateur party is able to guarantee its place in the coalition but is unable to extract a disproportionate number of ministries. Bassi (2013) models a formateur who emerges endogenously prior to formal coalition negotiations. As in the Morelli model, the formateur party does not receive extra cabinet posts and it is able to get into the government, though not because it enjoys a first-mover advantage, but because it is a member of the “proto-coalition” that has agreed to start the bargaining process. The empirical evidence for a formateur advantage is negligible. Given inadequate data on the initial formateur’s identity, there has been no systematic analysis of whether the party chosen as the *initial* formateur is more likely to get into government.³ There has, however, been significant work on whether the (final) formateur is able to extract a “bonus” in terms of cabinet ministries. The overwhelming conclusion is that the formateur party is unable to gain a disproportionate share of cabinet seats.⁴

Scholars have also highlighted two features of the institutions that structure policymaking *after* a government takes office that may have anticipatory effects on coalition bargaining. The first, identified by Strøm (1990), relates to the policy influence of opposition parties. Strøm argues that in legislatures with strong committee systems—which he defines as systems with specialized committees corresponding to government ministries, with non-overlapping memberships, and proportionally distributed committee chairs—minority governments should be more likely. The logic is that, in such systems, parties have fewer incentives to join a government, since they can wield policy influence from the opposition benches. Thus, where strong committees exist, opposition parties are more likely to “tolerate” a minority government, particularly in situations in which being in government might result in electoral losses.

This explanation hinges on the assumption that opposition parties enjoy policy influence that rivals that of parties in the government. However, recent work challenges this assumption. Martin and Vanberg (2011, 2014) demonstrate that opposition parties in some of the strongest committee systems in Western Europe—those of the Netherlands, Germany, and Denmark—are unable to force greater scrutiny of, or change to, government initiatives they most oppose. Nonetheless, there has been some empirical support for the argument that opposition influence affects formation outcomes, though its impact appears sensitive to model specification (see, e.g. Strøm 1990: 83; Mitchell and Nyblade 2008: 225–27).

A second aspect of post-formation policymaking on coalition bargaining—the influence of cabinet ministers—is central to the “portfolio allocation” approach of Laver and Shepsle (1996) (see also Austen-Smith and Banks (1990)). They argue that ministers enjoy informational and agenda-setting advantages that allow them to act as “policy dictators” on issues that fall under their jurisdiction. Given anticipation of the policies that result, Laver and Shepsle argue that

“strong parties” (which tend to be larger parties that hold the median position on multiple policy dimensions) should be more likely to get into the cabinet, perhaps even as a minority government. While considerable work casts doubt on the premise that ministers are policy dictators (see the following), there is empirical support for the contention that strong parties are more likely to get into government (Martin and Stevenson 2001), which suggests that parties believe that ministers have influence on policymaking, even if they are not “policy dictators.”

In short, one of the most significant developments in coalition research over the last 20 years has been to incorporate the effects of institutional structures into theories of coalition bargaining. This “institutional focus” has been even more important in the study of how coalitions govern *after* they take office.

Coalition governance as a principal-agent problem

After a coalition assumes office, “governance” in the strict sense of the word begins. Existing policies must be administered, and new policy initiatives must be drafted, and shepherded through the legislative process. This process unfolds over time and—crucially—in the shadow of a future election. The prospect that policymakers can be held to account at the ballot box is, of course, fundamental to the theory of democratic governance (Riker 1980; Powell 2000), and all governments that are subject to an electoral sanction can be expected to adjust their policy-making activities to anticipated voter reactions, at least at the margins. However, for coalition governments, expectations of future elections create a special set of tensions that are largely muted or absent under single-party government. These tensions derive from the fact that coalition governments must make policy *jointly* but will be held to account by voters *separately*.

This implies that coalition parties are playing a “mixed-motive” game. On the one hand, voter perceptions that coalition parties are unable to pursue a coherent policy program are likely to be harmful to their electoral fortunes. As a result, some level of cooperation—especially a willingness to look for workable compromise positions—is required to govern effectively. On the other hand, party leaders are aware that compromise can also be costly electorally. Parties target separate (though potentially overlapping) constituencies, characterized by different policy preferences or priorities. In consequence, parties have strong reasons to distinguish themselves from their partners, and to send credible signals to their constituencies that they are effective advocates for their concerns. Being perceived as too ready to endorse policies that depart significantly from the preferences of core constituencies can incur the displeasure of voters who believe that party leaders have “sold out” in order to reap the perks of office (Laver and Schofield 1990: 24). Indeed, Fortunato (2013) has shown that voter perceptions that coalition parties are too much like their partners generally result in a loss of votes at the next election.

Put differently, parties that participate in coalition face incentives to “jockey for position,” and to distinguish themselves from their partners. The political process affords them a number of opportunities for doing so. One avenue is legislative debate. As Martin and Vanberg (2008) argue, parties can make use of parliamentary debate on bills to argue that they negotiated a favorable compromise, and to justify the resulting policy to their supporters. Doing so becomes more important the more divisive the bill for the coalition and as elections approach. Drawing on debate data from the Netherlands and Germany, they demonstrate that coalition parties engage in significantly longer debates on divisive bills, and that this tendency increases in the period leading up to elections.

More significant than participation in legislative debate, the drafting and implementation of legislative initiatives also provides opportunities for parties to engage in “position-taking.”

Policymaking in modern democracies is highly complex, wide-ranging, and voluminous. As a result, cabinets cannot develop policy “as a group;” it is impossible for ministers to develop policy expertise in all relevant areas, and to take up each initiative in detail during cabinet meetings. Instead, drafting legislative initiatives involves delegation: the development of policies (and their implementation) is devolved to executive ministries (Laver and Shepsle 1994, 1996).

This central role of ministers in drafting legislation raises a potential problem for coalition governments. Ministers may be tempted to take advantage of their privileged position to “shade” proposals towards positions that favor their own party’s electoral fortunes. We refer to such behavior as “ministerial drift.” Sometimes, ministers may engage in ministerial drift in hopes of actually securing a policy outcome that is more desirable than the coalition compromise. But given significant media attention surrounding the unveiling of a ministerial draft bill, even a minister who expects that proposed bills will be significantly reshaped before they are adopted has incentives to employ draft bills as position-taking tools (Martin and Vanberg 2011: 13).

All of this implies that coalition governance resembles a principal-agent problem. Coalition parties, as principals, are forced to delegate policymaking to individual ministers. Ministers act as agents for multiple principals, and their preferences correspond to those of one principal (their own party), but typically diverge from those of other parties.

Confronting ministerial drift

There are two ways in which coalition governments can respond to this problem. The first is to accept that ministers exercise considerable discretion in drafting and implementing policy. This is the underlying logic of the “portfolio allocation approach” outlined previously. Ministers are treated as policy dictators within the areas under the jurisdiction, and thus, the policy pursued by a coalition will simply be the dimension-by-dimension intersection of the ideal points of the ministers who hold cabinet positions. The policies that coalitions agree to, and the drafting and implementation of those policies, are fully incentive-compatible. In landmark contributions, Austen-Smith and Banks (1990) and Laver and Shepsle (1996) use this approach to develop powerful theories of coalition formation.

At the same time, accepting ministerial autonomy is costly. The policies that emerge from “ministerial autonomy” are typically Pareto-inferior to policies that represent genuine compromises between the positions of the parties (Thies 2001; Martin and Vanberg 2004, 2011). [Figure 13.1](#) provides a simple example, using the familiar two-dimensional illustration of the portfolio allocation approach. Consider two parties, A and B , characterized by ideal points $x_A y_A$ and $x_B y_B$. Under the portfolio allocation approach, two policy outcomes are consistent with a coalition formed by A and B . If party A receives the portfolio with jurisdiction over Dimension 1, and party B the portfolio with jurisdiction over Dimension 2, the policy outcome will be $x_A y_B$. If the parties switch portfolios, policy shifts to $x_B y_A$. As [Figure 13.1](#) illustrates, there are many policies that are preferred by both parties to the latter scenario: All policies inside the intersection of the parties’ indifference curve through $x_B y_A$, including the point X , are Pareto-superior.

Yet, in agreeing to pursue a genuine compromise—such as at X —parties are confronted with the tensions created by the coupling of ministerial discretion with separate electoral accountability. To implement X , each party will have to develop policies that deviate from the party’s preferred policy on the relevant dimension. Ministerial drift thus poses a potential threat: Ministers may attempt to take advantage of their informational advantage, and their agenda-setting powers to “shade” policy in their favor. In order to pursue genuine compromises, coalition parties must therefore develop mechanisms that constrain the ability of individual ministers (representing their parties) to undermine them. Doing so requires two things:

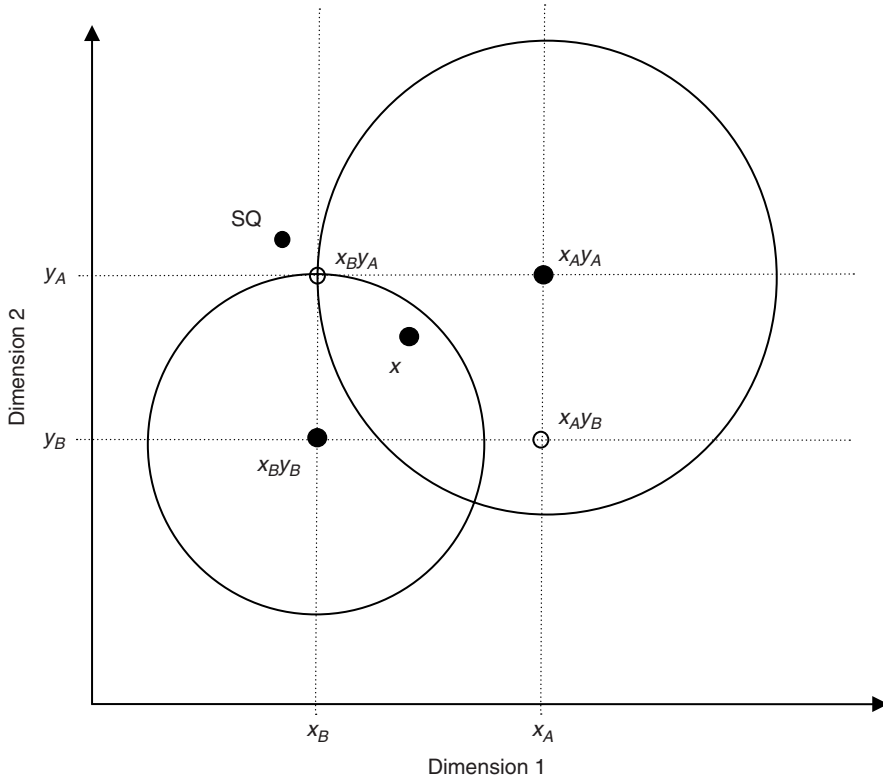


Figure 13.1 Coalition compromise vs. ministerial autonomy

- 1 Party leaders must be able to counteract the informational advantage of ministers by developing the policy expertise to evaluate draft proposals and to present viable alternatives.
- 2 Party leaders must have opportunities to push for meaningful change to draft bills to correct ministerial drift.

The institutional framework within which coalition governments are embedded is critical in this context. Coalition governments have come to rely on a number of institutional solutions to containing ministerial drift. Some of these operate at the cabinet level. Others are housed within executive ministries. Finally, the legislative process can play an important role in the monitoring and correcting of ministerial draft bills.

Cabinet-level institutions and junior ministers

Within cabinets, arguably the most important institution that allows monitoring—and potential correction—of draft bills are so-called “cabinet committees,” “inner cabinets,” or “coalition committees” (Müller and Strøm 2000; Andeweg and Timmermans 2008). Although there are distinctions among them (largely based on their precise membership), all of these bodies are comprised of cabinet ministers and may include senior party leaders in parliament as well as outside the legislature. They are typically established to vet particular policy proposals. However, these institutions—although undoubtedly important—face a significant limitation. Cabinet ministers cannot routinely acquire the policy expertise to evaluate and make policy across the range of issues a government must confront.

While it is possible to deal with specific issues in this manner, committees that rely to a significant degree on the participation of cabinet ministers cannot function as routine monitoring mechanisms.

In a seminal article, Thies (2001) identified a key monitoring mechanism *inside* cabinet ministries: so-called “junior ministers” (JMs). Besides cabinet ministers, the political leadership of executive ministries typically involves a junior minister (and sometimes more than one) who serves as “second in command.” Often, these are younger, “up and coming” politicians who are being groomed for ministerial positions. In addition to their duties in running a ministry and “learning the ropes,” these junior minister also often serve a less advertised function: Coalition parties can strategically assign junior ministers controlled by their own party to “shadow” the work of ministers from other coalition parties. Such watchdog junior ministers develop the relevant policy expertise, are privy to the policy process within the ministry, and have access to detailed technical information regarding draft bills and viable alternatives. As a result, junior ministers can serve as information conduits for parties to “keep tabs on their partners” (Thies 2001).

As Thies demonstrated in surveying appointment patterns in Italy, Germany, and the Netherlands, parties appear to assign junior ministers with this “watchdog function” in mind. Lipsmeyer and Pierce (2011) demonstrate for a larger set of countries that the cross-party appointment of junior ministers increases as coalitions are more divided, and for more salient issues. Martin and Vanberg (2011) investigate how junior minister appointments vary with party size. Because junior ministers are allocated in proportion to party size, parties that contribute more than half of a coalition’s seats (“large partners”) are at least in principle able to shadow *all* ministries belonging to their coalition partners, while those parties that contribute less than half of the coalition’s seats (“small partners”) do not have sufficient positions to monitor all ministries controlled by other parties. As a result, small partners must be more strategic in the assignment of their JMs.

Figure 13.2 illustrates this logic. The figure, which is based on a logit model that predicts the appointment of a watchdog junior minister as a function of the ideological division on the issue

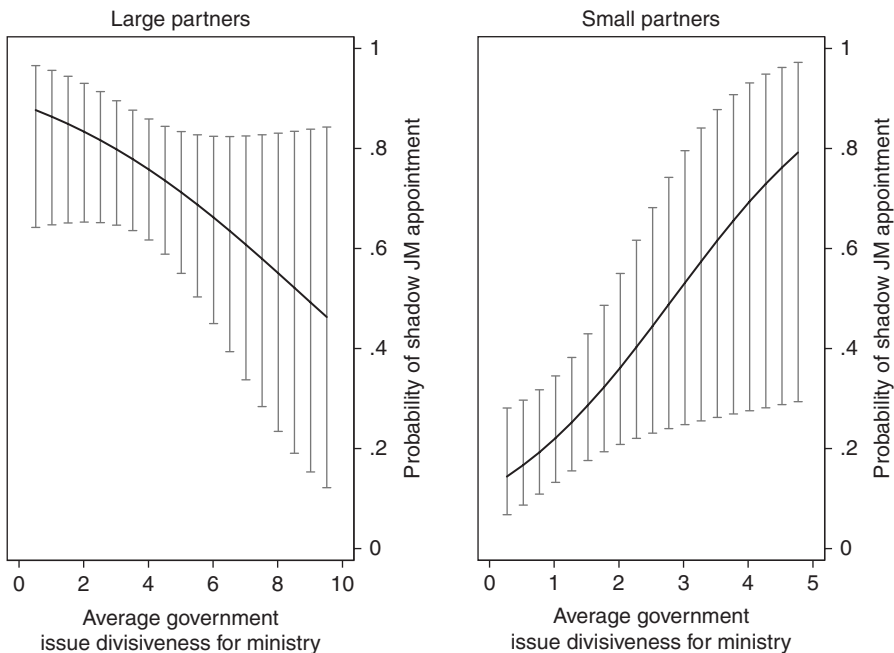


Figure 13.2 Probability of watchdog junior minister appointment as function of party size

dimension under the jurisdiction of the relevant ministry, shows that large parties are highly likely to have a watchdog junior minister in ministries controlled by other parties, and that this probability does not vary significantly with ideological division—a result that is consistent with the fact that large parties are relatively unconstrained in appointing junior ministers. In contrast, small partners—which cannot monitor all hostile ministers—respond to this constraint by assigning their scarce junior ministers primarily to those ministries that deal with issue dimensions that divide them significantly from their partners.⁵

Policing the coalition bargain in the legislative process

Parliament constitutes the quintessential institution of democracy, in large part because it is the institution that most directly represents citizen preferences in the political process. Despite this centrality, scholars of parliamentary systems have traditionally assigned legislatures only secondary importance in the policy process, arguing that major policy decisions are taken at the cabinet level rather than in the parliamentary arena (see, e.g. Laver and Shepsle 1996). To the extent that scholars have argued for the policy relevance of parliaments, they have done so primarily in the context of opposition influence, arguing that strong committee systems can provide opposition parties with influence over government-sponsored legislation (Strøm 1990; Powell 2000; Lijphart 1999).

A number of recent contributions argue that parliament is not only an arena in which governments confront the opposition, but that parties in coalition can also take advantage of the legislative process to confront the potential for ministerial drift (Kim and Loewenberg 2005; Martin and Vanberg 2004, 2005, 2011; Carroll and Cox 2012). Specifically, these scholars argue that following the introduction of a legislative proposal by a cabinet minister, partner parties can potentially use the legislative process to *scrutinize* the proposal and, if they believe that the proposal represents unacceptable drift, generate pressure to *amend* the draft bill. Naturally, such use of the legislative process requires that the internal structure of the legislature allows for effective scrutiny, and provides opportunities for meaningful change of bills.

As scholars have long argued, such features are intimately bound up with the nature of the legislative committee system (LaPalombara 1974; Lees and Shaw 1979; Mattson and Strøm 1995). In particular, standing committee systems that a) correspond to cabinet ministries in their jurisdictions; b) that are restricted in their membership; c) have extensive powers to compel documents and testimony (including that of senior civil servants); and d) have powers to rewrite draft bills or introduce amendments, make it more likely that parties can effectively supervise the work of hostile cabinet ministers. In addition to a strong committee system, however, meaningful opportunities for changing ministerial draft bills require the absence of procedural rules that provide ministers with strong agenda-setting powers. Most important among these are so-called *urgency* and *guillotine* procedures that allow ministers to curtail the legislative process or to restrict amendments (Huber 1992). Building on the work of Mattson and Strøm (1995) and Harfst and Schnapp (2003), who collected cross-national data on the nature of legislative committee systems for European democracies, Martin and Vanberg (2011) develop an index of parliamentary strength that captures how well the institutional structure of the legislative process provides parties with meaningful opportunities for *scrutiny* and *change*. Figure 13.3 reproduces this index for 16 European legislatures. The picture that emerges is clearly consistent with conventional scholarly impressions: There is considerable variation in parliamentary strength, with some of the strongest legislatures found in the “consensus” systems of the Netherlands, Austria, and Germany and some of the weakest legislatures confined to the “majoritarian” democracies found in the UK, France, and Ireland.⁶

If coalition parties employ the legislative committee system to shadow their partners, we might expect that just like junior ministers, committee chairs will be filled strategically so that

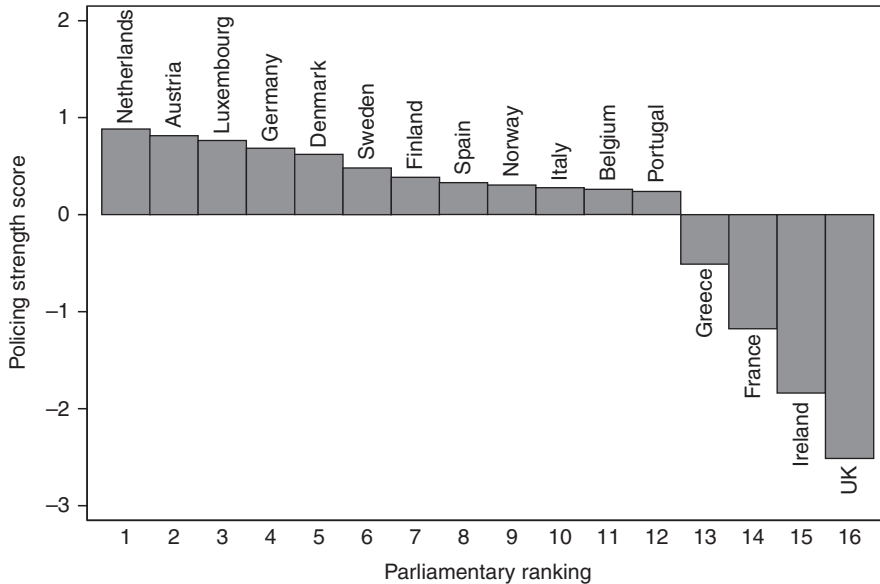


Figure 13.3 Index of policing strength for sixteen European parliaments

hostile ministers are shadowed by committees led by a “shadow chair.” Focusing on the distribution of committee chairs in Germany, Kim and Loewenberg (2005) test this expectation, and find strong support for it. Carroll and Cox (2012) extend the analysis to 19 parliamentary systems, and demonstrate that shadow committee chairs are more likely to be appointed for ministries with jurisdiction over issue areas on which the coalition partners are more heavily divided.

While these contributions clearly demonstrate that committee chairs are assigned in ways that are consistent with the use of legislative committees as monitoring institutions, they do not directly demonstrate the impact of committees on the fate of ministerial draft bills. This is the task at the heart of a study by Martin and Vanberg (2011), which tracks the detailed legislative history of more than 1,300 government bills introduced in five European legislatures over a 20-year-period. Focusing on three legislatures with strong legislative institutions (Denmark, Germany, and the Netherlands) and two with weak legislative institutions (France and Ireland), they are able to show that in strong legislatures, ministerial draft bills are significantly more likely to be scrutinized heavily, the more divided the coalition on the bill at hand. In addition, bills that are divisive for the coalition are amended at significantly higher rates before they are adopted than bills that are not divisive, and these amendments are driven primarily by divisions within the coalition, not by divisions between the coalition and the opposition.⁷ In contrast, in the two weak legislatures, they do not find these effects. Where legislative institutions prevent effective scrutiny and make pressuring for changes opposed by ministers difficult, bills are not amended systematically in response to divisions within the coalition (or divisions between government and opposition).

Taken together, these studies significantly revise our understanding of the role of legislatures in parliamentary systems. Parliaments that feature strong committee systems may not primarily be institutions that provide opposition parties with an opportunity to influence government policy; rather, strong parliaments may play a central role in allowing coalition parties to police the coalition bargain. Indeed, consistent with this argument, the rise of coalition government in Ireland and New Zealand quickly led to a strengthening of the legislative committee system

(Martin and Vanberg 2011, 55). Similarly, Martin (2009) finds evidence in a cross-national dataset that parliamentary strength tends to be higher in democracies with greater rates of multiparty governance.

Multiparty governance, government size, and fiscal institutions

The literature reviewed in the previous section focuses on how multiparty governments use institutions to manage the tensions introduced by delegation to cabinet ministers and the threat of ministerial drift for general policymaking. An additional strand of the literature on coalition governance revolves around a more specific—although particularly salient—aspect of policy-making by multiparty governments: budget-making. Perhaps the most prominent conclusion to emerge from this literature is that budget-making by coalition governments represents a “common-pool resource” (CPR) problem. As a result, scholars have argued, coalition government typically leads to significantly higher government outlays than single-party government under similar economic and demographic conditions, and this tendency increases as more parties are added to a coalition (Bawn and Rosenbluth 2006; Braeuninger 2005; Perotti and Kontopoulos 2002; Persson *et al.* 2007; Volkerink and de Haan 2001).⁸

Although there are subtle differences in the details, the general theoretical mechanism underpinning this expectation is similar across studies. Parties have spending priorities, often rooted in the particular concerns of their target constituencies. Suppose parties are held accountable primarily for spending decisions on issues of concern to their voters, but can deflect blame for spending in other areas on their coalition partners. In such an environment, parties face strong incentives to press for increased spending in “their” areas, and few incentives to resist spending in areas favored by their partners. The result is that spending tends to increase as each party demands higher spending, and few incentives to resist these demands exist. In contrast, single-party governments are held to account for all spending decisions, and are therefore more sensitive to the costs of increased spending than coalitions (Bawn and Rosenbluth 2006; Persson *et al.* 2007).

There is considerable empirical evidence in support of this argument. Controlling for factors that affect the size of the public sector, including economic conditions, the demographic make-up of the polity, and the generosity of the welfare state, spending levels increase as the number of parties in government gets larger. Coalition governments spend more than single-party governments (Persson *et al.* 2007), and this tendency increases as more parties are added to a coalition (Bawn and Rosenbluth 2006; Braeuninger 2005; Perotti and Kontopoulos 2002; Volkerink and de Haan 2001).

However, these explanations abstract away from the institutional features of the budget process. Recent work has begun to explore the manner in which the common pool resource problem is shaped by the institutional framework of the budget process. The CPR logic turns on two critical assumptions. First, parties have opportunities to push for higher spending in areas favored by their constituents. Second, parties have few incentives to oppose the spending demands of their partners. As Martin and Vanberg (2013) argue, the extent to which these assumptions accurately characterize budget-making depends on the institutional details of the budget process. At least in principle, fiscal institutions can vary in the degree to which they provide opportunities for individual parties to exert pressure for spending, and in the degree to which they provide incentives for parties to resist spending demands by their partners.

Analyzing spending patterns in 15 European democracies over roughly forty years, Martin and Vanberg (2013) are able to show that the institutional features of the budget process condition the CPR logic. In “permissive” institutional environments that provide opportunities for

parties to push for spending and create few incentives to resist spending demands by other parties, an increase in the number of government parties leads to significant increases in spending. In contrast, in more restrictive budgetary environments, which limit the ability of individual parties to push for spending, and generate incentives for parties to resist spending demands by their partners, the CPR logic breaks down: Adding an additional party to a coalition does not result in increased spending, and the spending patterns of coalition governments are indistinguishable from those of single-party governments.

Like the literature on general policymaking by coalition governments, this finding once again highlights the importance of being sensitive to the institutional framework within which coalition governance is embedded. Just as certain institutions—including cabinet committees, junior ministers, and strong legislative institutions—can mitigate the dangers of ministerial drift, so certain fiscal institutions can effectively counterbalance the underlying CPR logic of budget-making by coalition governments.

Conclusion

From its largely “institutions-free” beginnings in the 1960s, coalition theory has developed into a voluminous literature. One of the most significant recent developments in this area, ushered in by the emergence of the “new institutionalism,” has been sustained attention on how political institutions—the rules that frame government formation, as well as those that govern the policymaking process—shape multiparty governance. As this chapter has made clear, scholars have made great strides in understanding how institutions affect the types of governments that emerge, and the policy choices that these governments are likely to make.

While considerable progress has been made in exploring how institutions shape coalition governance, our survey readily suggests a number of avenues for future work. In closing, we briefly highlight two of these—one theoretical, the other empirical. Theoretically, the study of how institutions structure how multiparty governments manage conflict and make policy is still in its infancy. More detailed work on the variety of institutional mechanisms that are available to coalition parties, both inside legislatures but also outside of parliaments, is warranted. We know little about the relative efficacy of these institutions, and about the extent to which parties use these institutions as substitutes or complements in confronting the challenges of multiparty governance. We are also only beginning to explore how the institutional framework within which coalitions govern shapes the policy outcomes that emerge. To the extent that we care about democratic governance in part because it is thought to enhance the responsiveness of public policy to citizen preferences, a more direct focus on policy outcomes and their relationship to citizen preferences is critical. Doing so poses not only theoretical puzzles, but also an empirical challenge in developing more direct measures that allow scholars to assess the correspondence of citizen preferences and policy outcomes across space and time.

Notes

- 1 Given that there exist multiple minimal winning coalitions, Riker (1962) assumes that a coalition is more valuable to its members the smaller its total weight, which leads to the prediction of the minimum-size minimal winning coalition. Gamson (1961), who assumes that the share of ministries received by each party is proportionate to its seat contribution, also makes the minimum-size prediction.
- 2 It should also reduce the length of the coalition bargaining process, which several studies have found to be the case (Diermeier and van Roozendaal 1998; Martin and Vanberg 2003; Golder 2010).
- 3 Carroll and Cox (2007) state that they “found direct evidence concerning which party was the formateur for only a portion of the cases” (307). Although this missing data problem might appear easy to solve,

- Laver *et al.* (2011, 290) note that *Keesing's Contemporary Archives* (the primary source used to code formateur status) “almost never contains statements of the form ‘...after the September election in X, the formateur was Y’.”
- 4 Warwick and Druckman (2006) show that formateur parties are actually *under*-compensated relative to their seat share. See also Laver *et al.* (2011).
 - 5 The model underlying the figure is based on data for the 164 cabinet ministries over the period from roughly 1983–2001 in France, Germany, Ireland, and the Netherlands. For details of the estimation technique, see Martin and Vanberg (2011), especially section 4.4 (pages 88–93).
 - 6 For specifics regarding the derivation of this index, see Martin and Vanberg (2011: 49). The values of the index are: Netherlands (0.88), Austria (0.81), Luxembourg (0.76), Germany (0.68), Denmark (0.62), Sweden (0.48), Finland (0.39), Spain (0.33), Norway (0.30), Italy (0.28), Belgium (0.26), Portugal (0.24), Greece (–0.51), France (–1.18), Ireland (–1.84), and UK (–2.51).
 - 7 They also consider the potential interactive effects of different monitoring institutions, and demonstrate that when a watchdog junior minister is present in a ministry, the amount of parliamentary scrutiny falls dramatically, although the bill will be amended at the same rate as in the absence of a watchdog junior minister. They interpret this finding as evidence that junior ministers can serve as a substitute for information gathering, but are not able to pressure effectively for change prior to bill introduction.
 - 8 In addition to the literature on government spending, there is considerable literature on government deficits and debt that explores the extent to which both are shaped by fiscal institutions. The distinction between coalition and single-party governments is examined in this literature, but primarily with respect to the institutional rules that are appropriate to each type of government. The *size* of coalitions, and the *interactive* effects of coalition size and fiscal institutions, which are at the heart of the argument here, do not figure into this literature (see, e.g. Hallerberg *et al.* 2009).

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JUDICIAL INSTITUTIONS

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Judicial institutions arbitrate and adjudicate disputes of various sorts within a political community. The basic social logic of courts and judges is rooted in the triad for conflict resolution: whenever two actors come into a conflict that they cannot themselves solve they call upon a third for assistance (Shapiro 1981: 1). According to this simple but universal view of judicial institutions, the effectiveness and efficiency of the third-party is related to the extent that he or she is neutral to the issue in dispute and independent from the parties in conflict, as well as to the extent to which it applies pre-existing legal norms after adversary proceedings (Shapiro 1981). The immediate role of judicial institutions would then be to resolve specific disputes.

But judicial institutions are also means to achieve other, more general or proximate, normatively appealing goals through the resolution of those specific disputes. According to modern political thinkers, along with the other branches of government judicial institutions partake in creating a moderate or *balanced* system of rule. This is what Montesquieu famously called “the liberty of the constitution” in *The Spirit of the Laws*. Montesquieu argued that judges should be either the “mouthpiece of the law...mere passive beings incapable of moderating either its force or rigor” (VI 6), or political actors that “deliberate, communicate their thoughts, argue [...and...] change their minds” (VI 4).² There is no contradiction: for Montesquieu the role of judges and of the judicial organ depends on how close a given regime is to the desired moderate government. In republics, where non-arbitrary government is already established by the separation of executive and legislative powers, judges should play a minimum role and the judiciary should be so weak as to become a “null power.” But in monarchies, where the executive and legislative powers are concentrated in the king, a permanent judicial organ and deliberative judges are necessary for checking power and preserving the balance of government.³

The list of goals that judicial institutions help to achieve has been expanded in recent scholarship. More interestingly, recent research emphasizes the theoretical role played by judicial institutions in achieving such outcomes. Specifically, scholars have made efforts to theorize the role that courts play in bringing about outcomes such as regime stability, human rights protection, corruption control, or investment and economic growth either by enhancing the *credibility* of government *commitments*, by providing focal points that help solve *coordination* problems, or by providing information that reduces the uncertainty that partly causes *cooperation* dilemmas (e.g. Barro 1997; Blomquist and Ostrom 2008; Frye 2004; Gibler *et al.* 2009; Milgrom, North, and Weingast 1990; North and Weingast 1989; Powell and Staton 2009; Reenock, Staton, and

Radean 2012; Ríos-Figueroa 2013; Sutter 1997; Weingast 1997). It is thus not surprising that NGOs, states, international, and supranational organizations have promoted vigorously the introduction of more courts, the construction of judicial independence around the globe, and more broadly, the rule of law (e.g. Carothers 2006).

This chapter is divided into three sections. The first focuses on two central attributes of judicial institutions: independence and power, and introduces a distinction between two different sets of judicial institutions: judges and prosecutors. In the second section, I review recent scholarship that analyzes judicial institutions as means to achieve appealing outcomes by solving commitment, coordination, and cooperation problems.⁴ In this section, I also re-state some of the arguments making use of the distinctions introduced in the [first part](#), and on occasion I also extend the reasoning to suggest how judicial institutions would work in authoritarian regimes. In the third section, I discuss some of the most pressing and exciting challenges in this area of research.

“Independence” and “powers” of judges and prosecutors

Any justice system has two key sets of institutions with their corresponding crucial actors: (1) the prosecutorial set, in which prosecutors of different types and levels are in charge of detecting, investigating, and taking cases to court for an eventual trial; and (2) the administration of justice set, in which judges sitting in different types and levels of courts decide over the cases that are brought before them. The bulk of social science research on judicial institutions focuses on courts and judges forgetting the prosecutorial organ and the prosecutors—as if scholars working on these topics had never watched *Law & Order*. Of course, strictly speaking prosecutors are part of the criminal justice system. But their job is the prosecution of *crimes*, i.e. violations of a law in which there is injury not only to the victim but also to the public. And crimes can take place in any area of political or social life—a family dispute, a business transaction, a sports venture, a political campaign, or a government decision. Therefore, the functioning of the criminal justice system affects all other areas of social and political life, which makes the prosecutors and the prosecutorial organ an essential part of any justice system (see e.g. Brinks 2007; Gonzalez Ocantos 2012; Gordon and Huber 2009; Michel-Luviano 2012; Michel-Luviano and Sikkink, 2013; Tonry 2012).⁵

Two features of judicial institutions are particularly relevant to analyze whether, and if so how, they can become effective means to obtain appealing outcomes. The first is their degree of “independence.” At the broadest level, “independence” of judicial institutions and actors “expresses the aspiration that judicial decisions should not be influenced in an inappropriate manner by considerations judged to be normatively irrelevant” (Vanberg 2009). In the most basic scheme of courts as third-party dispute settlers (Shapiro 1981) “independence” breeds the neutrality and credibility of judicial decisions and thus is the bedrock for their legitimacy before the parties in the dispute, the political actors, and the public at large (see e.g. Bybee 2010). Independence is also crucial for prosecutors whose legitimacy depends on the perception that they represent the state and the public, and not a particular government or political party.

The second key feature of judicial institutions is related to their capabilities or, more generally, their “powers.” The “powers” of judicial institutions and actors are related to both the formal right to decide particular disputes on behalf of particular litigants and the likelihood that those decisions will produce compliance. In general, judges are more powerful the broader the set of parties that can bring their disputes to them, the broader the set of issues they can resolve, and the broader the legal base they have to use to decide on them (see e.g. Ginsburg 2003). In turn, prosecutors are more powerful the broader the set of issues they can pursue, the more

discretion they have to choose which crimes to investigate and how, and the more leverage they have to negotiate with suspects and other parties involved in a crime (see e.g. Tonry 2012).

Notice that the two basic features of judicial institutions, “independence” and “powers,” apply to both judges and prosecutors. Moreover, notice also a key difference between prosecutors and judges that can affect the institutional design of the justice system: while the former are “active” the latter are “passive.” In other words, judges can only decide the cases that are brought to court by someone else (in the criminal justice system generally by prosecutors). In the famous words of Hamilton, judges do not have “will but merely judgment.” In contrast, prosecutors have both will and judgment; they can actually decide whether to go after specific cases in order to investigate crimes that have been committed (and to prevent others that could be committed). Moreover, after they decide which cases to pursue they also have discretion to decide whether or not to take a particular case to court. In essence, prosecutors have the keys that open the courtroom’s door in criminal cases.⁶

The institutional design of the justice system involves, then, combining in a specific way the institutional actors—judges and prosecutors—and their institutional features—independence and powers. For instance, arguably a stable and consolidated democracy requires that both sets of actors are independent from the government and also powerful. In an autocratic regime, however, the ruler may prefer to have independent judges with not much power (so that potential decisions against the regime do not cause much damage) and dependent prosecutors that can monitor political opponents and serve as the “legal arm” of the autocrat. Even in democracies, however, we find some courts that enjoy less independence than others (as the non-Article III courts in the United States). As will be argued, theories that explain the particular institutional architecture of a given system focus on different roles that judicial institutions may play.

Conceptual and measurement issues⁷

There is a lot of debate on how to conceptualize and measure key features of judicial institutions such as their “independence” and their “powers.” A familiar distinction in the literature involves the difference between *de facto* and *de jure* concepts. The latter deals with formal rules designed to insulate judicial actors from undue pressure, either from outside the judiciary or from within. Institutions like fixed tenure, multilateral appointment procedures, budgetary autonomy, and judicial councils are thought to provide such insulation and thus influence behavior. The concept of *de facto* judicial independence is behavioral and can be further differentiated between two subconcepts. The first demands that judges or prosecutors be the “authors of their own opinions” (Kornhauser 2002: 42–55). On this account, a judge is independent when he or she does not respond to undue pressures to resolve cases in particular ways,⁸ in other words, when her decisions reflect her preferences. Judicial independence in this sense reflects *autonomy*. A second concept of *de facto* emphasizes that it makes little sense to call a judge or prosecutor independent if her decisions are routinely ignored or poorly implemented, thus requiring not only that judges resolve cases in ways that reflect their sincere preferences, but also that these decisions are enforced in practice (Cameron 2002). Judicial independence in this sense reflects *influence*.

Diverse research teams have made important efforts to measure the previous concepts. Some teams trying to capture the incentives and rules are likely to induce usually aggregate information via an index of multiple institutions, such as length of tenure and methods of appointment and removal (Camp Keith 2002; Feld and Voigt 2003; La Porta *et al.* 2004). Other teams try to capture independent behavior directly, either through the coding of judicial decisions according to a certain standard of independence (e.g. decisions against the government) or through surveys of experts, users of the courts, and the general public (e.g. Cingranelli and Richards 2010).

Still other teams try to capture independent behavior indirectly through a measure of the behavioral or attitudinal consequences of autonomy or influence, such as higher ratios of non-currency money to total money supply that arguably reflect more confidence in independent judicial institutions and actors that prevent predatory state behavior (e.g. Clague *et al.* 1999).⁹

In addition to conceptual distinctions and measurement strategies, existing research also tells us that institutions in general, and judicial institutions in particular, do not work in a vacuum. It is thus important to analyze the conditions under which, for instance, a sincere evaluation of the judicial record, or compliance with judicial decisions, is more likely. The most relevant conditions for the effectiveness of judicial institutions include the presence of a certain legal culture or ideology (e.g. Gonzalez Ocantos 2012; Hilbink 2012; Ingram 2009), higher levels of political competition or a fragmented political arena (Aydın 2013; Helmke 2002; Popova 2012; Pozas-Loyo and Ríos-Figueroa 2007), strong and sophisticated societal organizations that use the legal system to pursue their interests (Epp 1998; Wilson 2006), and higher levels of public support for the judicial institutions (Gibson, Caldeira, and Baird 1998; Staton 2006; Vanberg 2005).

All in all, each one of the different strategies to conceptualize and measure a given concept of judicial independence has advantages and limitations. Arguably, the most important challenge in this regard is that scholars make sure that their concepts match their theories and that their measures match their concepts. For instance, as will be clearer in the next section, arguments in which independent courts allegedly enforce previously made commitments assume compliance and thus require a specific concept and measure of *de facto* judicial independence, i.e. a concept and measure that reflects influence. In contrast, arguments in which citizens can make use of judicial decisions as signals to help coordinate their responses to a government that has exceeded its constitutional limits, arguably might only require a court issuing decisions that reflect its sincere evaluation of the record.

Three theories on the roles of judicial institutions and actors

Making commitments credible

Governments can get something they want from another actor by committing to do something else in return. But how can the other actor be sure that the government will do tomorrow what it commits to do today? One way the government can make its commitment credible is by increasing the costs of reneging (Shepsle 1991). In this framework, empowering and giving autonomy to a court allowing it to punish deviances from previously made commitments is a device to make such commitments credible (Ferejohn and Sager 2002). In a constitutional democracy, citizens know that politicians cannot easily abuse their decision-making power by changing the rules in their favor, because there is a constitution that is harder to change. But this is not enough. A constitution is not effective if politicians have latitude in interpreting its meaning. Hence, an independent court with some kind of judicial review power is needed. As Sánchez-Cuenca puts it: “representation and constitutions are institutions based on some commitment technology, through delegation or through rules, and judicial review is an essential mechanism to make these commitments credible” (Sánchez-Cuenca 1998: 94).

By making constitutional commitments credible judicial institutions can contribute to obtain desirable outcomes such as promoting investment and economic growth, protecting human rights, democratic survival, and curbing corruption. Independent judiciaries with a sufficient amount of power, which constrain arbitrary executive and legislative power, ensure that state constitutional promises to respect property rights are perceived as credible (e.g. North and Weingast 1989).

In turn, credibility breeds investment, state solvency, growth, and development (e.g. Acemoglu, Johnson, and Robinson 2001; Barro 1997; Frye 2004). The government can also commit to protect the human rights enshrined in constitutions and recent findings suggest an inverse relationship between judicial independence and human rights violations (e.g. Hathaway 2007; Poe, Tate, and Keith 1999; Powell and Staton 2009). Judicial institutions can also facilitate credible commitments and the enforcement of compromises that matter for democratic survival (Reenock, Staton, and Radean 2012). Independent courts and prosecutors are also mechanisms to make credible a government's commitment to punish those who use public funds for private benefit (Van Aaken, Feld, and Voigt 2010; Ríos-Figueroa 2012a).

Notice that arguments in which independent courts allegedly enforce previously made commitments assume compliance. In other words, it makes little sense to argue that a court would make credible the government's commitment if it is routinely ignored or its decisions poorly implemented. Thus, arguments in which the theoretical role of courts is to tie the hands of the government to enhance its credibility imply a specific concept of *de facto* judicial independence, i.e. *influence*.¹⁰ However, it is well known that lacking financial or physical means of coercion, courts depend on the assistance of other political authorities to enforce their decisions, making the likelihood of enforcement of judicial decisions an important element for judges to consider when crafting their decisions. The issue of enforcement of judicial decisions looms large and poses one of the most difficult and exciting research challenges regarding judicial institutions. The literature has pointed to some conditions under which enforcement is more likely because the costs of not complying are higher; perhaps the most important in democracies is public support for the courts (Vanberg 2005).

In authoritarian regimes things are different. Some studies have argued that courts in dictatorships can also make credible the authoritarian government's commitments (Ginsburg and Moustafa 2008; Moustafa 2007). For instance, autocrats may allow independent judicial decisions only in the economic sphere but not in other politically sensitive areas, such as public security (Moustafa 2007).¹¹ However, enforcement of courts' decisions under autocracies is an even more complicated problem in large part because in those regimes it does not make sense to think of independent institutions of any kind (Schedler 2013; Svolik 2012, 2013). Judicial institutions and actors under authoritarian regimes share the broader goal of wanting the regime and their own benefits to survive: if or when they don't, they would be undermined, sacked, or worse by the dictator (as in Egypt when courts went beyond the purely economic sphere and entered the territory of public security, cf. Moustafa 2007).

This does not mean that judicial institutions cannot contribute to authoritarian power sharing. Most dictators do not directly control enough resources to govern alone and therefore seek the support of notables with whom they promise to share power. This creates, according to Boix and Svolik (2013: 300), "the central dilemma of any dictatorship, which is to establish a mechanism that allows the dictator and his allies to credibly commit to joint rule." Institutions such as autocratic political parties can contribute in solving that dilemma. According to Beatriz Magaloni, "if the dictator delegates control to the access-to-power positions and the state privileges to a parallel political organization, such as a political party, he can more credibly guarantee a share of power and the spoils of office to the groups that support him" (Magaloni 2008: 716). Judicial institutions in authoritarian regimes can play a role analogous to that of political parties.

Specifically, the autocrat can create a special jurisdiction for a group within his ruling coalition in exchange for loyalty. Each special jurisdiction has its own system of courts that are non-independent vis-à-vis the specific group and have powers limited to that domain. Members of this group accept the exchange because having a special jurisdiction means that they will enjoy

autonomy to deal with problems internal to this group. The existence of these special jurisdictions, each with its own system of non-neutral-limited-reach courts, tends to be a clear sign of the regime's intention to respect a significant level of autonomy for the most relevant parts of the ruling coalition to deal with their own issues. The most common special jurisdiction that serves this purpose is the military jurisdiction, but in some dictatorships such as the Franco regime in Spain, there were up to 22 special jurisdictions to cement the power sharing arrangements with important groups of the ruling coalition, including the military, the business elite, and the Catholic Church (Aguilar and Ríos-Figueroa 2014).

Providing focal points for citizens' coordination

A coordination challenge exists when the interests of actors are in agreement but to realize mutual gains the actors have to make mutually consistent decisions. For instance, according to Barry Weingast, the transition to stable democracy "requires the construction of a coordination device that specifies widely accepted and unambiguous limits on the state. By allowing citizens to react to violations in concert, such a device makes limits on political officials self-enforcing" (Weingast 1997: 251). Although Weingast does not explicitly mention judicial institutions, other authors have considered that an independent court with judicial review power can be such a coordination device (Law 2009; Sutter 1997).¹² For instance, David Law argued that by conveying relevant information about government misconduct in a highly public fashion, the courts enable the people to control their government in an informed and coordinated manner (2009: 730).¹³ More recently, Gibler and Randazzo (2009) argue that courts and judicial institutions can contribute to prevent democratic backsliding by signaling when rulers overstep their constitutional bounds and facilitating civil society coordinated responses to restrain them. In this connection, then, judicial decisions enable citizens to coordinate to (implicitly) protest against or overthrow the government if it is overstepping its boundaries.

For judicial decisions to effectively provide a focal point for citizen coordination there should be a monitoring system for the government's violations. Citizens themselves can ring the "fire alarm," especially if the legal standing to file cases against the government is very broad. In this line, scholars have argued that court decisions serve as a focal point when they set the agenda that helps mobilize litigants (e.g. Baird 2008; Cichowski 2007; Moustafa 2007). But prosecutors, depending on their degree of independence from the government, can serve also as a "fire alarm" or a "police patrol" mechanism of monitoring (McCubbins and Schwartz 1984). To solve citizens' coordination challenges to keep the government under limits, therefore, both judges and prosecutors can be helpful. In particular, under a constitutional democracy perhaps the best design of the justice system to carry out this function is to give independence and power to both judges and prosecutors.

Arguments such as Weingast's (1997) or Law's (2009), in which citizens can make use of constitutional or judicial signals to help coordinate their responses to a government that has exceeded its constitutional limits, arguably might only require a court issuing decisions that reflect its sincere evaluation of the record. Therefore, these arguments imply a concept of *de facto* judicial independence in the sense of *autonomy*. In contrast to credible-commitment arguments, coordination arguments do not assume that government actors comply with judicial decisions. The reason is, in part, that the coordination arguments reviewed deal precisely with the issue of government compliance either with its constitutional limits. A different but closely related argument is that judicial institutions can also help coordinate citizens to support the court's rulings and act as its enforcement arm if governments refuse to obey. Specifically, judges can "go public" when they need to inform citizens in an effort to rally support for rulings that go against the

government (Staton 2010). In either case, coordination arguments do not assume compliance but rather deal directly with it.

In authoritarian regimes it is hard to believe that an autocrat would allow an independent court that could provide a focal point for citizens to coordinate against him. But the autocrat would surely like to have a good monitoring system, especially of political opponents. Because judges are passive but prosecutors are active, the more important actor in the justice system to play the police-patrol monitoring role under autocracies would be the prosecutor. Moreover, prosecutors are typically non-independent from the head of the executive power in autocracies. This makes them particularly useful to play the monitoring function and deal with the problem of authoritarian control: the threat the dictator faces that emerges from the unsatisfied masses excluded from power and the opposition groups also excluded (Svolik 2012).

Specifically, in authoritarian regimes non-independent prosecutors could investigate opposition groups, get information about their activities, and build “legal leverage” to be used by the regime in negotiations with them. In other words, prosecutors can become the “legal arm” of the dictator. They are a key element in the legal control of the opposition. The more powerful the prosecutors are the more efficient they could be in performing this function.¹⁴ In this story, judges would simply rubber stamp whatever the prosecutors bring before them. The judges, thus, would not be independent but, in contrast with prosecutors, they also would not be powerful. The use that the authoritarian regime of the PRI in Mexico made of the justice system can be an example of this function (see Aguilar and Ríos-Figueroa 2014; Ríos-Figueroa 2012b).

Transmitting information that reduces uncertainty and promotes cooperation

A cooperation dilemma exists when the interaction of two actors pursuing their private interests produces a worst outcome than the one each actor could get if they could act together. Milgrom, North, and Weingast (1990) argue that judicial institutions can solve cooperation dilemmas. Specifically, when actors seek to exchange goods, a judicial institution that serves as a repository of information and as an adjudicator of disputes can reduce transaction costs (e.g. the cost of transmitting information) reinforcing the reputation system as a means of promoting honest trade (1990: 3–11).

Blomquist and Ostrom (2008) also argue that courts applying principles of equity may be able to transform conflictive relations into cooperative ones when actors deal with extractive resources such as water. In particular, these authors highlight the fact that individuals may use particular forms of litigation not only for the purpose of obtaining a judge’s decision of their controversy but also for the purpose of conducting their own inquiries about institutional performance, investigating problems, sharing information, and negotiating agreements (Blomquist and Ostrom 2008: 182).¹⁵

In general, when the cause of conflict between two actors is uncertainty over the right solution to their conflict or over each other’s willingness to cooperate, a third-party mediator can facilitate cooperation by providing relevant information (Kydd 2006). Independent constitutional courts that are widely accessible and have ample powers of judicial review can provide that information. Independence is linked to the court’s neutrality or impartiality whereas wide access is related to the court’s capacity to get information on the issue in dispute. In addition, courts that also have control over docket and sentencing guidelines are more capable of transmitting such information in a more effective way, strategically managing conflict between the actors and avoiding setbacks for its own decisions (Ríos-Figueroa 2013).

When parties have agreed to cooperate but they face informational challenges in knowing whether each actor is complying with the agreement, a third-party mediator such as a court can monitor the parties, transmit information, and enhance their cooperation. For instance, Carrubba (2005) argues that the judiciary can serve as a monitor of cooperative agreements and eliminate costly conflicts by offering strategic declarations on the nature of alleged breaches, declaring a violation only when cooperation would have been mutually beneficial. Interestingly, in dictatorships and transitional democracies third-party mediation by court-like institutions can also contribute to compliance with election results (Svolik and Chernykh 2012). In particular, Svolik and Chernykh argue that electoral courts can improve compliance with the outcomes of elections where the incumbent has an “informational advantage” but this depends on how accessible the courts are to the opposition as well as on the political value of the information revealed by their rulings (2012: 35).

Theoretical and empirical challenges

Despite the recent advances in the study of judicial institutions—recognizing them as means and not ends in themselves and producing theories on judicial institutions’ role in producing valuable outcomes—there are still important challenges. First, as discussed throughout the chapter, key features of judicial institutions—such as their independence, powers, and the crucial issue of compliance with judicial decisions—are still under considerable conceptual and measurement debates. Second, as already mentioned, the bulk of the scholarship focuses on the study of courts and judges under democracy without paying much attention to their working under autocracy nor to the other important half of the institutions of the justice system, i.e. prosecutorial organs and prosecutors. Finally, judicial institutions are generally taken as a given so that their effects can be analyzed, but of course actors empower independent judicial institutions based on expected effects. Moreover, what have been identified as effects of such institutions can be as well produced by the determinants of the institutions themselves. In the remainder of this chapter, I focus on the latter challenge.

Causes and effects of judicial institutions

The analysis of institutions has two basic concerns. One is to explain institutions—where they come from, why they take the form they do. The second is to understand their effects for political and social behavior. These are not, of course, really separate. Institutions arise from the choices of individuals, but individuals choose among structures in light of their known or presumed effects (Diermeier and Krehbiel 2003; Moe 1990). It is more common and probably easier to “understand the effects of institutions without having any idea where the institutions come from” (Moe 1990: 215), than to understand why a certain institution exists without knowing “with reasonable confidence ... the consequences of the focal institution” (Diermeier and Krehbiel 2003: 133).

Regarding judicial institutions, scholars have asked why self-interested governments would willingly constrain themselves by empowering independent judicial institutions. Political actors may want to sustain judicial institutions that decide against them because they created such institutions not only to adjudicate specific conflicts but also to help them accomplish a more general outcome by playing a specific role (see Vanberg 2009). But notice that if there are conditions under which actors need judicial institutions to solve certain problems (such as making commitments credible, or solving cooperation or coordination problems) then these conditions should at least be some of the factors that “cause” judicial institutions. It is key then to explain

why the actors that create them (e.g. elected officials, politicians, political parties, or autocratic leaders) will not want to, or will not be able to, undermine them.¹⁶

Recent scholarship has found that the creation of *de jure* independent and powerful judicial institutions is more related to domestic political factors—such as a multilateral constituent body (e.g. Pozas-Loyo and Ríos-Figueroa 2010) or the need to get some insurance in the face of electoral defeat (e.g. Versteeg and Ginsburg 2013)—than to ideational concerns (e.g. Ingram 2009) or diffusion dynamics (e.g. Elkins 2010). These findings are interesting and suggestive but they should be taken with caution because they are based on assumptions about the expected effects of judicial institutions. In part because of the conceptual and measurement issues discussed earlier, and in part because of the identification problems caused by endogeneity, the degree to which we know the effects of judicial institutions “with reasonable confidence” (Diermeier and Krehbiel 2003: 132) still varies considerably across the scholarly literature.

Political actors may not want to undermine judicial institutions, but also they may not be able to do that even if they want to. Thus, scholars have also analyzed the conditions under which political actors may not be able to undermine judicial institutions that decide against them: when they face coordination problems due to political fragmentation (e.g. Cooter and Ginsburg 1996); or when another actor or force—such as public opinion or a free press—imposes high costs for doing so (e.g. Staton 2010; Vanberg 2005). Indeed, while we know some of the factors that lead to the creation of independent and powerful courts as well as some of the factors that lead to their maintenance, these two questions have generally been addressed separately and their joint systematic analysis is still an open question.

Conclusion

Recent scholarship on judicial institutions mostly consists of efforts to theorize the role they play in bringing about appealing outcomes. The traditional account of judicial institutions focused on their capacity to check the government by disabling arbitrary actions. In contrast, to argue that judicial institutions help solve commitment, coordination, and cooperation problems implies that judicial institutions not only disable but also enable. The focus on the theoretical roles of judicial institutions, moreover, allows for a more direct engagement of the question of why they exist not only in democracies but also in autocratic regimes. Incorporating prosecutors, and exploiting the differences between them and judges, is probably a fruitful way to theorize about the roles of these institutions and also to better identify their effects.

The challenges for this type of research on judicial institutions—conceptual, measurement, and endogeneity concerns—are common to the research program of “institutionalism as a methodology” (Diermeier and Krehbiel 2003). This makes the judicial institutions subfield an integral part of this exciting program, in the frontier of comparative politics, constitutional politics, and political economy.

Notes

- 1 Thanks to Paloma Aguilar, Claudio López-Guerra, Sean Fern, Jennifer Gandhi, Ezequiel Gonzalez Ocantos, Verónica Michel-Luviano, and Rubén Ruiz-Rufino for their careful reading and valuable suggestions.
- 2 Quotes refer to the book and chapter (e.g. XI, 6) of the English edition of *The Spirit of the Laws* cited in the references section (Montesquieu 1977).
- 3 The great innovation of Hamilton (2000: 496) in Federalist 78 is to argue for independent and powerful judicial institutions *in a republic*: “In a monarchy it [the judiciary] is an excellent barrier to the despotism of the prince; in a republic it is a no less excellent barrier to the encroachments and oppressions of the representative body.”

- 4 The explicit goal of the chapter implies that the review includes mostly studies within the rational institutionalism tradition. Other rich traditions, such as socio-legal or historical institutionalism, are not thoroughly covered.
- 5 Of course, there are institutional variations depending on the country regarding the links between the prosecutors and the police, and those between the prosecutors and the judges. Because prosecutors are understudied from a social science perspective there are still many open questions, starting from whether or to what extent we can use what we know of courts to study prosecutors.
- 6 There is a vast and important literature on “judicial activism” that underscores proactive moves by judges, such as making bold interpretations of law, creating judges’ associations, or providing guidance to victims’ associations (see e.g. Shapiro 2013). Nonetheless, it is accurate to characterize judges as passive in the sense that in general they have to be activated by a party that brings a case before them, whereas prosecutors are *active* because they can go after cases they deem important.
- 7 This subsection draws from Ríos-Figueroa and Staton (2014).
- 8 Of course, political authorities always apply some pressure when they are parties to a case or when they file briefs as interested parties (e.g. *amicus curiae* briefs and other similar institutions). The key here is that this pressure is perfectly acceptable within the rules of the legal system.
- 9 Ríos-Figueroa and Staton (2014) review 13 extant cross-national measures of judicial independence, the concept that each measure tries to capture, the type of sources from which the measures were created, and the coverage of each measure across time and space, and evaluate their validity. In a nutshell, more research teams have attempted to measure *de facto* independence than *de jure* independence; and, within the former category, scholars largely seem to have targeted the influence concept. Evidence suggests the validity of extant *de facto* measures, though their proper use requires attention to correlated patterns of measurement error and missing data. The evidence for the validity of extant *de jure* measures is weaker.
- 10 Nonetheless, some influential empirical articles making this argument do not use a measure of judicial independence that captures this particular concept. For instance, La Porta *et al.* (2004) use a *de iure* measure of judicial independence as a proxy for *de facto* independent judicial behavior in the sense of influence.
- 11 In authoritarian regimes it is easier to take over courts when they start making decisions the autocrat dislikes: the Egyptian regime supported the decisions of “independent” courts in the economic sphere, but it did not when the judges made uncomfortable decisions regarding public security (Ginsburg and Moustafa 2008).
- 12 Interestingly, a court can coordinate not only citizens but also aggregated actors such as states in federal polities where constituent units will be unable to act in the presence of externalities and other kinds of collective action problems (cf. Halberstam 2009).
- 13 For a court’s decision to become an effective focal point there are some other conditions. For instance, the signal should be clear and understandable for the public (Vanberg 2005), which implies that very technical issues, ambiguous writing, or contradictory decisions by different courts weaken the focality of the signal. However, judges sometimes have incentives to write ambiguous decisions to minimize the costs of non-compliance (Staton and Vanberg 2008).
- 14 The police and the investigative services generally work in coordination with the prosecutors, sometimes under their direct supervision.
- 15 The transmission of information by the courts and the actors’ learning that take place in theories where judicial institutions serve as cooperation enhancing mechanisms differentiate them from theories that posit judicial institutions as mechanisms to make commitments credible.
- 16 It is worth noting that scholars who study judicial institutions from the perspective of historical institutionalism care about their origins and effects though in a different way. Specifically, when analyzing the origins of judicial institutions historical institutionalism does not place too much emphasis on the actors and their institutional design preferences, but instead it highlights the role of context and specific circumstances, i.e. critical junctures (Gillman 2002), as well as the historic trends in court crafting (Kapiszewski 2012). In turn, when analyzing the effects of judicial institutions historical institutionalism emphasizes the “court character” as shaped by long patterns of jurisprudence, institutional ideology, and interbranch relations (e.g. Lisa Hilbink 2007; Kapiszewski 2012). From this perspective, the connection between origins and consequences of institutions is intertwined with the complex question of institutional change, which in a sense is the very object of study of this scholarship (cf. Pierson 2004; Smith 2009).

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15

FEDERALISM

Pablo Beramendi and Sandra León

The failure of the European Union to offer an institutional response to the sovereign debt crisis, the growing challenge in Catalonia to the existing design of the Spanish state, or, in a different context, the inability to craft a stable framework for co-existence of different identities in Iraq, all relate to the question of the virtues (and vices) of different forms of federalism. The evolving geography of political conflict and contestation in the post-Cold War world is rekindling academic interest in the subject. This chapter begins by reviewing basic conceptual issues. We then turn to discuss the origins of federalism and its implications for the functioning of democracy and markets. Finally, we close the chapter by briefly addressing the dynamic aspects of federalism as an incomplete contract and its implications for the question of institutional stability.

Conceptual issues: the nature of federalism

Riker characterized federalism as “a political organization in which the activities of government are divided between regional governments and a central government in such a way that each kind of government has some activities on which it makes final decisions” (Riker 1975: 101). A formal statement in the constitution and the existence of a strong judicial review system typically guarantee the autonomy of these two levels of government. Moreover, both the center and the regions have their own fiscal bases and are directly accountable through elections. As a result, central governments in federations (as opposed to confederations) enjoy a much stronger institutional position vis-à-vis subnational governments, and must find ways, jointly with subnational units, to manage their political and economic interaction effectively.

The comparative politics of federalism has come a long way in understanding the nature of this interaction between federal and state governments in political unions. This required abandoning the notion that federal and unitary states constitute uniform categories. The gradual opening to federal experiences beyond the U.S. case led to the recognition of the multidimensional character of federal structures (Stepan 1999). Not surprisingly, a more accurate description of federal realities came at the price of a less precise definition of the essential qualities of the federal model. Certainly, a strict definition of federalism based on one of its most defining features—the representation of regional interests in the federal legislature—would leave Canada, one of the classic federations, out of the federal sample, as the Canadian senate has no capacity

to shape federal decision-making (Rodden 2004). This example illustrates that characterizing federal credentials requires subtler accounts of relevant institutional features that might not necessarily be identified in the federal constitution. Delving into variation in constitutional and non-constitutional institutions across federations is not only required for the purpose of achieving more precise descriptions of federal realities, but, as we will show in the remainder of the chapter, it is crucial to provide a better understanding of the consequences and evolution of federalism.

Federations exhibit significant differences in terms of both the set of rules that regulate the interaction between levels of government, such as judicial review or the representation of regional interest in the federal legislature, and the specific decentralization agreements that define the distribution of powers between the center and the subnational units. The former institutions have a more resilient nature, as they are most likely identified in the constitution and therefore are less amenable to change over time, whereas the distribution of fiscal and policy authority between levels of government is more likely to experience variation according to changes in governments' preferences or relative bargaining power. Addressing a detailed description of institutional variation across federations is beyond the purpose (and length) of this chapter, so in the next paragraphs we will focus on two of the most important institutions that mediate the interaction between the center and the units, namely the representation of regional interests in federal decision-making and the structure of the party system and electoral competition. In addition, we will present some indicators on the distribution of policy and fiscal authority and discuss along the way current improvements as well as unresolved debates on how to measure variation in institutional design across federations.

Federal countries exhibit enormous variation in the way regional interests are represented in the federal legislature, both in the way upper chamber's representatives are elected and in its relative power to reshape majorities in the lower chamber (Elazar 1991; Watts 2008). In some countries representatives are directly elected from territorial districts (Brazil, the United States), whereas in others they are appointed by regional incumbents (Germany, Austria), the prime minister (Canada) or national legislators (India and Austria). The way delegates are elected may have an impact on how regional electorates are represented in the federal legislature: direct election by the territory's population may facilitate direct representation of local preferences, whereas indirect appointment paves the way for a stronger role of national political parties in mediating the representation of regional constituencies.

The capacity of subnational units to shape the decision-making process of the federal government ranges from the absolute veto power of the upper houses in the United States and Switzerland and the more limited veto in Germany or Belgium (veto on some federal legislation), to the virtually powerless upper chambers in Canada, India, or Spain. In federations with weak upper chambers, the representation of regional interests in the federal arena is more likely to be channeled through negotiations between the federal and regional executives (Watts 2008). In Canada and Spain, for instance, the opportunity of regional governments to shape federal decision-making takes place in a variety of more or less formal policy-specific multilateral bargaining bodies (León and Ferrín Pereira 2011). Political parties play a crucial role in greasing the wheels of intergovernmental negotiation in those bodies, particularly when the party system is highly integrated and strong, disciplined national political parties facilitate cooperation between federal and regional chief executives that share party affiliation.

The relevance of the party system goes well beyond its role in channeling intergovernmental relations, and was defined by William Riker as the most important dimension to understand cross-country variation in the structure of federalism (Riker 1964). The degree of integration of the party system and partisan harmony (the number of regional chief executives sharing party

affiliation with the federal chief executive; see Riker and Schaps 1957) have long been used as proxies of the degree of centralization of federations, as these measures convey information about the ability of the federal government to “overawe” subnational units. More recently, scholars have continued to develop subtler characterizations of patterns of political competition in federations by exploring the internal organization of parties and the degree of interdependence between the regional and federal electoral arenas. Variables such as the dependence of local politicians on central government party officials for nomination, finance, or career advancement define vertical lines of authority and accountability within parties, which appear to favor subnational politicians in federations like Brazil and, less consistently, in Argentina, whereas it favors central party officials in Mexico (Eaton 2004; Montero and Samuels 2004; Thorlakson 2009; Willis *et al.* 2001).

Another dimension of variation across federal countries concerns the distribution of policy and fiscal authority between the center and the subnational units. The most defining feature of policy decentralization in federations is the predominance of shared authority between different levels of government (see Henderson’s *decentralization index*¹; Watts 2003). This means that the widespread move towards decentralization in the last decades (Arzaghi and Henderson 2005; Dillinguer 1994) has mainly consisted of the establishment of joint policy authority between the center and the units (Rodden 2006).

Certainly, the neat distribution of powers envisaged in *The Federalist Papers* is a far cry from the highly intertwined division of governmental authority in modern federal states. There are, however, some differences across federations in the extent of overlapping jurisdictions. In Canada, Australia, and the United States, executive and legislative jurisdictions tend to coincide at one level of government, which results in low coordination requirements in policy-making. In Germany, Austria, or Switzerland state and provincial governments hold executive authority over policy areas that are legislated at the federal level (Watts 2003). This involves a more intertwined distribution of executive and legislative authority that allows for further flexibility in policymaking, but at the cost of lower clarity of responsibilities.

Finally, the most controversial issue in the distribution of authority between the center and the subnational units relates to fiscal powers, as authority over revenues and expenditures represents the nuts and bolts of subnational autonomy. High levels of policy decentralization may involve limited subnational autonomy if decisions about how money is raised, distributed, and spent remain at the central level. Measuring levels of fiscal decentralization across federations has traditionally been a challenging task for scholars working in the area, mostly due to lack of comparable data on subnational revenue autonomy. Data on expenditure decentralization drawn from the IMF’s *Government Finance Statistics* (share of state and local expenditures over total government expenditure) has been often used as a measure of fiscal decentralization. However, expenditure decentralization fails to capture the extent to which subnational governments possess full autonomy over subnational revenues. As a result, it tends to overestimate overall levels of fiscal decentralization and overlooks significant variation in subnational fiscal autonomy across federal countries.

Recent contributions in measuring subnational fiscal autonomy have succeeded in providing a more refined portrayal of variation in fiscal decentralization across federations (Rodden 2004; Stegarescu 2005). When it comes to levels of subnational revenue autonomy, the United States, Canada, and Switzerland exhibit the highest levels of fiscal decentralization, whereas the regulatory framework of subnational revenue autonomy is more centralized in Austria, Belgium, and Australia. Future improvements in data collection require additional developments in comparable measures of subnational fiscal autonomy, as well as an extension of the data beyond the OECD sample (see [Figure 15.1](#)).

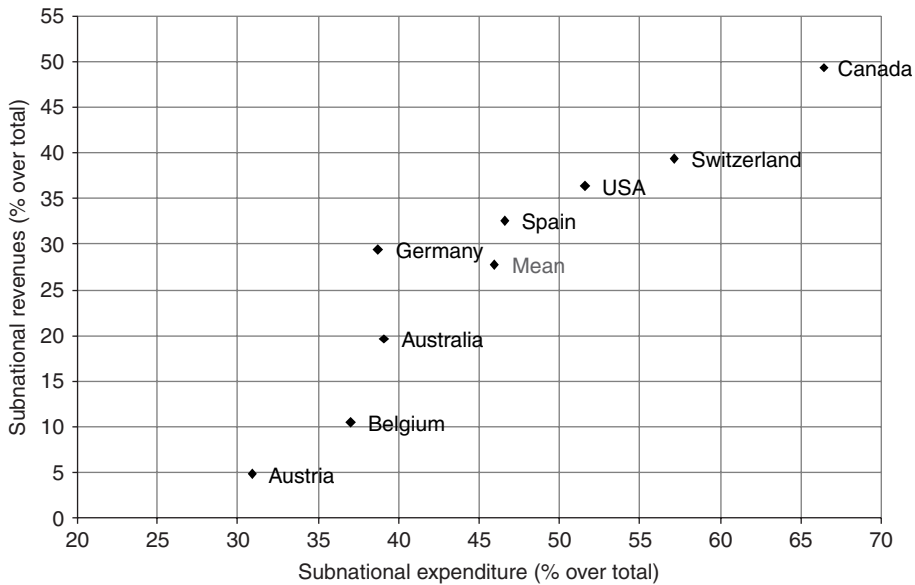


Figure 15.1 Subnational tax revenues and expenditures (% over total), 2011

Source: Government at a Glance 2013 (OECD) and OECD National Accounts Statistics Canada (2010). Source for USA: www.usgovernmentspending.com

To conclude, federations exhibit significant differences both in terms of the institutions and procedures that regulate the interaction between levels of government and the specific distribution of powers between the center and the subnational units. How to narrow this multidimensional variation down into a comparable index of decentralization has long remained a methodological challenge barely addressed by empirical scholars (Arzaghi and Henderson 2005). The most important recent contribution in the area has been the publication by Hooghe, Marks and Schakel (2010) of a Regional Authority Index that measures the authority of regional governments along eight dimensions in 42 democracies or quasi-democracies over the 1950–2006 period. The Regional Authority Index is formed by two compounded indicators, one of *self-rule*, which measures the authority exercised by a regional government over those who live in the region, and another of *shared rule*, an index that measures the authority exercised by a regional government to its representatives in the country as a whole.² Notwithstanding the significant contribution of this dataset to the empirical measurement of the constitutional and non-constitutional aspects of decentralization across countries, there is room for the field to produce a more nuanced account of fiscal arrangements across different forms of decentralization.

The origins of federalism revisited: external pressures, domestic trade-offs, and institutional designs

Riker's (1964) *Federalism* provides an inflexion point in the study of the origins and functioning of federalism. Until then, the field had been dominated by largely inductive and apolitical approaches (Filippov 2005). Federalism would emerge as the institutional correlate of a number of cultural, historical, or even ideational features of societies.³ Riker's breakthrough builds on a distinctive analytical innovation: the "establishment of a federal government must be a rational bargain among politicians" (Riker 1975: 116). Such a bargain is approached by

actors with (1) “a desire [...] to expand their territorial control by peaceful means, usually either to meet an external military or diplomatic threat or to prepare for military or diplomatic aggrandizement”; and (2) “a willingness [...] to give up independence for the sake of the union either because they desire protection from an external threat or because they desire to participate in the potential aggression of the federation” (1975: 114).

The key question is what triggers these bargains, what determines the distribution of winners and losers and, therefore, the stability of the specific institutional solution ultimately adopted. We can distinguish three sets of (non-mutually exclusive) mechanisms speaking to these issues directly: external pressures, domestic trade-offs, and endogenous institutional bargains.

The paramount form of external pressure, as in Riker’s own writings, takes the form of a military or diplomatic threat (Lemco 1991). Forced to pool resources by a common danger, smaller units are forced to cooperate. Empirical research (Panizza 1999; Stepan 1999, 2001; Ziblatt 2006), however, suggests that external military threats do not account for the majority of actual processes of formation of federations. Along with military or geopolitical considerations, economic pressures, such as large scale economic externalities across the members of the federation (Casella and Frey 1992; Casella and Weingast 1995) play an increasingly important role as sources of demand for higher levels of political integration.

Yet, however strong the effect of cross-unit externalities on the demand for integration, political actors must balance the expected efficiency gains from federation against their private distributive considerations. This brings us to a second set of mechanisms associated with trade-offs in the future allocation of resources within the emerging federation (Alesina and Spolaore 2003; Bolton and Roland 1997; Beramendi 2012; Wibbels 2005a; Diaz-Cayeros 2007). In this line of research excessive differences among the constituent members in terms of assets (wealth), dominant type of economic activity (extraction versus production, mobile versus non mobile), and income distribution foster fundamental distributive trade-offs that jeopardize the feasibility of the union. The trade-offs are both in terms of net contributions to the common pool as well as in terms of political autonomy: the current delegation of power may come to haunt members in future contention over power or the distribution of assets.

The core of the matter lies in the fact that any constitutional contract is incomplete (Bednar 2009; De Figueiredo and Weingast 2005) and therefore open to opportunistic behavior. Zooming into this problem precisely, Rector (2009) argues that federalism emerges as an institutional solution precisely when states cannot agree on cooperative solutions that could be mutually beneficial due to commitment problems: “Cooperation is risky because it can lead states to invest in assets that are valuable only as long as cooperation lasts” (2009: 31) To the extent that one state invests more in the relationship than the others, its position becomes weaker by virtue of its very investments. At the extreme, fear of excessive exposure deters cooperation. Rector’s theory suggests that federalism is not the result of an agreement based on trust, consensus, and loyalty. Rather, mutual distrust is the very engine of federalism: “federations form when states benefit from cooperation but cooperation requires unequal levels of relationship-specific investments” (2009: 61). In other words, federalism is a cooperative solution that emerges out of distrust. In the presence of potential gains for all parties involved, federalism emerges as a way to manage the commitment problem among uneven partners, as a source of “contrived symmetry.” The management of such symmetry is easier in the presence of some particular features of the organization of power within the federation: Integrated party systems monitor the potential of local elites to deviate from previous contracts (Filippov, Ordeshook, and Shvetsova 2004; Diaz-Cayeros 2007).⁴ And along with the party system, a set of complementary institutions such as constitutional safeguards and the judiciary (Bednar 2009, 2013; Bednar *et al.* 2001), help keep at bay different forms of opportunism. The role of these factors becomes

particularly prominent when analyzing the political and economic consequences of federalism, to which we turn now.

Varieties of federalism and their effects

The democratic and efficiency promises of federalism

The study of federalism was initially anchored in the analysis of the design of constitutions, a legal approach that mostly consisted of descriptive accounts of case studies. The seminal work of Riker (1964), however, prompted the development of an extensive comparative literature with larger analytical grounds and more realistic accounts of politics and institutions. Along the way, normative concerns associated with the optimal design of federal institutions gradually gave way to a more positivist approach to the workings of federations, focusing on the tension between the promises raised by federalism and its actual political and economic consequences (Weingast 2014; Weingast 2009).

Nowadays, the cumulative empirical and theoretical research developed by the comparative literature on federalism tempers the initial enthusiasm over the democratic and efficiency promises of federalism. As we discussed earlier, the actual operation of federations is quite distant from the stylized models of federalism that assume dual accountability and a clear-cut division of authority between levels of government. Indeed, the gap between the ideal design of federalism and the complex and multidimensional nature of federations looms large in explaining why the old expectations of federalism have turned out to be a mere *federal illusion* (Beramendi 2007).

Federalism has shown up time and again as a solution to a variety of institutional, political, and economic problems. However, the cumulative body of theoretical and empirical research about its consequences has balanced the promises of federalism with more realistic accounts about its perils. The existing literature in the area shows that the actual political and economic consequences of federalism are complex and multidimensional and highly contingent upon the specific design of federal institutions as well as economic and social contextual factors. Certainly, the history of the comparative politics of federalism during the last decades has consisted in the rediscovery of these complexities as part of a research agenda aimed at articulating a general theory of federal performance. In the remainder of the section we will review the main theoretical and empirical contributions of this literature, emphasizing the extent to which these works depart from the old promises of federalism.

Federalism and democracy

The oldest promise of federalism is the democratic one. Federalism has been widely considered as an institutional solution to enhance accountability and the control of governments. This promise rests on the assumption of clear-cut distributions of vertical authority between levels of government, but empirical evidence suggests that the intertwined division of governmental authority in some federations is associated with blurred responsibility attribution (Cutler 2004, 2008; Johns 2011; León 2012; Rudolph 2003a, b), although evidence for the U.S. and the European Union shows that voters distinguish among levels of government (Arceneaux 2006; Hobolt and Tilley 2013). Insofar as incomplete information makes individuals unable to establish a causal link between outcomes and politicians' actions, accountability might be undermined. These caveats are not new and were already stressed long ago by Alexander Hamilton in *The Federalist Papers*.⁵ A recent strand of the literature on economic voting has provided evidence of a weaker relationship between economic performance and support for the national

incumbent in federal states (Anderson 2006b, 2009). So far research in the area has been predominantly case-specific, mainly due to the lack of comparable cross-country data on attributions of responsibility. As a result, there is still much to be learned about how different federal designs affect clarity of responsibility and whether this in turn has an impact in variation across federal countries in performance voting.

The presupposed advantages of decentralized government over accountability are clearly undermined when the electoral fates of subnational politicians are shown to be dependent on variables exogenous to subnational performance. The lack of comparable data in the area has resulted in very limited knowledge about whether national dynamics play a larger or smaller role in different federations and what factors account for this variation. Work in this area has been largely built around country cases (mostly the United States) and provides evidence that competition dynamics at the national level and national economic performance have a strong impact in subnational elections.⁶ These findings raise questions about the classic view of “dual accountability” in federations, which presupposes that voters will hold subnational (national) governments accountable for distinctly local (national) responsibilities. The few comparative studies in the field have adopted different empirical strategies to explore the contamination between the federal and regional arenas⁷ (Rodden and Wibbels 2011; Schakel 2013), but their empirical results suggest that the congruence between national and subnational elections decreases in contexts where subnational self-rule is more prominent.

Finally, a growing literature emphasizes the “peace-preserving” qualities of federalism because it allows federal shared-rule to coexist with self-rule (Elazar 1987; Riker 1964). The virtues of federalism in “holding the state together” (Stepan 1999) lie in the provision of “more layers of government and thus more settings for peaceful bargaining” (Bermeo 2002: 99) as well as checks upon the central government’s attempts to dominate ethnic minorities (Gurr 2000; Horowitz 1985). Gurr’s *Minorities at Risk’s* project (2000) offers detailed information on a large set of minority groups in both federal and unitary states, showing that federal regimes facilitate accommodation better than unitary systems. Further evidence on the accommodating success of federalist arrangements is also uncovered in a large comparative analysis across federations coordinated by Amoretti and Bermeo (2004). Yet, many of the less enthusiastic views about the palliative qualities of self-rule come from studies that explore the fate of federations from the communist bloc of Eastern Europe, all of which ended up in disintegration (Bunce 1999; Cornell 2002; Treisman 1997).

The paradox of federalism lies in that the same institutions, policies, and practices that are designed to contain divisions may contribute to perpetuating conflict. Those who take the most optimistic views to task state that federalism contains the seeds of its own disintegration. They argue that federal arrangements divert government functions and resources to territorial subunits that end up strengthening nationalist leaders’ capacity to mobilize for nationalist ends (Roeder 2009; Snyder 2000). Above all, the disintegration dynamics are more likely to unleash in contexts where the internal federal boundaries overlap with ethnic boundaries, strengthening identities and division among political communities (Elazar 1994; Lipset 1959; Watts 2008); or where the existence of regionalist parties spurs on separatist identities and mobilizes groups to engage in secession (Brancati 2006).

In turn, the literature on ethnic conflict regards federalism both as a cure and a curse for state disintegration (Bakke and Wibbels 2006). Evidence based on case studies suggests that the ability of federalism to contain conflict and prevent disintegration is highly contingent upon the specific design of federal institutions (Filippov *et al.* 2004; Hechter 2000; Stepan 2001). However, other works point to the importance of non-institutional factors such as income disparities to account for federal stability. Bakke and Wibbels (2006) find that in contexts with high levels of

interregional inequalities fiscal decentralization increases the likelihood of conflict through its tendency to amplify cross-regional income inequalities. When wealthy regions find that the costs of financing redistribution to poorer regions offset the benefits provided by the undivided state (military security, economies of scale), pressures towards secession increase (Alesina and Spolaore 2003; Sambanis and Milanovic 2011). In turn, in contexts with high regional inequalities, federalism might survive only as long as control over redistribution is decentralized (Boix 2003). Both the Scottish and Catalan cases illustrate the role of interregional inequalities in fuelling reforms of federal institutions, particularly a redefinition of fiscal tax and revenue powers that grant regions further fiscal autonomy.

Federalism and the market

The federal illusion has two pillars: democracy and markets. Interestingly, the notion that federalism leads to better markets is common intellectual ground for both welfare economists and public choice theorists. For *welfare economists* (Musgrave 1997; Gramlich 1973, 1987; Oates 1972, 1991, 1999; Wildasin 1991), federalism and decentralization facilitate the goal of an optimum allocation of resources by ensuring a better fit between preferences, needs, and policies via informational matching and experimentation. Within this framework, factor mobility operates primarily as a factor of preference revelation for welfare maximizing incumbents.⁸ In turn, for *public choice theorists* (Brennan and Buchanan 1980; Buchanan 1995: 19–27; Inman and Rubinfield 1997: 73–105; Qian and Weingast 1997: 83–92; Weingast 1993: 286–311; 1995: 1–31; Weingast *et al.* 1995: 50–81) federalism enhances markets by containing Leviathan's wasteful appetite. Within this framework, factor mobility operates as a factor constraining government's predatory tendencies. By allowing voters and factors to vote with their feet across jurisdictions, federalism facilitates a better monitoring of incumbents by markets and voters.⁹ Accordingly, less corruption and smaller states facilitate a better functioning of markets (Prud'homme 1995).

A wealth of comparative and empirical research over the last three decades allows academics and practitioners to know better by now (Rodden 2006; Wibbels 2006). Federations and decentralized structures vary widely in the outcomes they generate. The range in outcomes is large. In non-democratic contexts, the removal of a subnational layer of government improves governance by limiting corruption (Malesky *et al.* 2014). Autocracies also use the promotion of local politicians as a mechanism to keep localities under the political control of the center (Landry 2008). In democratic but corrupted and clientelistic societies, capital mobility and federalism not only fail to discipline governments and boost experimentation; they produce worse outcomes by reinforcing a low productivity, high extraction equilibrium (Cai and Treisman 2005). On the contrary, federalism has facilitated sustained economic growth in the USA and Switzerland.

There is no gainsaying that the economic consequences of federalism depend on the interaction between two variables: the specific design of federal institutions and the context in which these institutions actually operate. The specialized literature has identified three distinctive mechanisms governing this interaction: (1) the organization of representation in federations; (2) the specifics of the fiscal constitution; and (3) the dimensionality of the political space and the polarization within dimensions in the federation.

Representation

Representation matters because it determines the balance of power between the center and the units. In centrifugal federations (Beramendi 2012), the center lacks monitoring ability and regions enjoy opportunities to free-ride and abuse the common pool. In centripetal federalism,

by contrast, an overpowered center has incentives to overrule constituent units and extract rents from its privileged position. In centrifugal federations, unruly, vested local interests risk rendering federalism inefficient (Inman and Rubinfeld 1997; Breton 1996; Eichengreen and Von Hagen 1996). In centripetal federations, the effective disappearance of self-rule defeats the very purpose of federalism (De Figueiredo and Weingast 2005; Wibbels 2005a; Rodden 2006). The core issue still remains of how to preserve the autonomy of local elites while limiting their incentives to distort the market at the same time. More generally, the optimal balance of power is a delicate, multidimensional problem (Bednar 2005, 2009) where several aspects of the institutional design of political unions play an important role.

On the premise that under any given design actors will set out to maximize the amount of rents they can extract (Inman and Rubinfeld 1997; Cremer and Palfrey 1999, 2000; Dixit and Londregan 1998), the representation of regional interests in the national arena plays a fundamental role in the working of federations (Wibbels 2005a, Rodden 2006, Beramendi 2012). Representation ultimately reflects the interplay between formal institutions and the organization of the party system.

Given that policies are made at the national level, market regulations, fiscal discipline, and income redistribution are in part explained by the ability of regions to use their formal representation in national institutions to obstruct or shape reforms. Wherever upper chambers play an important role in defining economic outcomes, the nature and scope of reform reflects the bargaining power of the regional coalitions for and against specific policy changes.¹⁰ A strongly malapportioned senate facilitates veto power by specific regions and increases their ability to extract resources. This leverage, however, can be offset by the organization of the party system. Party systems matter because of the career concerns of politicians and because they help overcome time inconsistency problems between elites across levels of government. Weak party systems create uncertainty about earlier promises by either the state or federal government.¹¹ Strong party systems have the organizational ability to impose sanctions on regional politicians violating national agreements and therefore reduce their incentives to behave irresponsibly, either fiscally or politically. Put differently, strong, integrated party systems tie together the fate of political elites across levels of government and generate strong electoral externalities (Rodden and Wibbels 2002, 2011), ultimately rendering the federal contract more stable.

Fiscal constitution

In addition to the organization of representation, the second feature mediating the economic outcomes of federations concerns the degree of fiscal accountability and autonomy granted by the constitution. Fiscal accountability refers to the extent to which subnational units actually internalize the consequences of their economic behavior. Fiscal autonomy implies that subnational governments rely more on their own revenues and less from transfers from the federal government. Yet, the reverse is not necessarily true. Fiscal autonomy does not always imply high levels of fiscal accountability. That depends on how hard the budget constraint is.

Given soft budget constraints, fiscally autonomous regions incur large debts, thereby shifting the costs of fiscally irresponsible policies to other units in the federation. As a result, if the central government bails out subnational ones from their financial obligations, macroeconomic and distributive outcomes tend to worsen as subnational units become more transfer dependent. To this effect, regional transfer dependency is associated with a higher demand for bailouts (Rodden 2006), resistance to market reforms (Wibbels 2003), and non-cooperative relationships between levels of government and their constituencies. The logic behind poor economic outcomes follows from political incentives as shaped by the existing institutional design.

By introducing competition between several policy suppliers, federalism sets the stage for central and subnational governments to behave non-cooperatively. Incumbents at both levels of government seek political credit for the goods and services provided to citizens while they aim to minimize the costs incurred in satisfying citizens' demands (Migué 1997; Volden 2004, 2005; Inman and Rubinfeld 1997). They also seek to minimize the electoral impact of unpopular policy reforms. As a result, subnational governments often incur high levels of debt to be paid by the rest of the federation through a federal bailout. More generally, federations often confront a moral hazard problem: local authorities take advantage of federal risk-sharing schemes to enact policies that increase local risks. Symmetrically, by decentralizing social programs without transferring the necessary resources, central governments manage to off-load to regional incumbents the political costs of retrenching publicly provided social welfare. Soft budget constraints facilitate and reinforce these dynamics.

By contrast, given hard budget constraints, fiscal autonomy leads to better economic and social outcomes by reducing capture via transfer dependency, facilitating experimentation, and ensuring a better fit between preferences and policies at the local level (Qian and Weingast 1997; Bardhan and Mookherjee 2005).¹² Fiscally autonomous subnational units reduce aggregate deficits and inflation rates and facilitate sustained economic growth. More fiscal autonomy shapes the extent to which subnational governments internalize the benefits of their economic progress, creating incentives for subnational incumbents to create a market preserving environment. Stein's (1999) analysis of Latin American federations illustrates the logic: decentralization tends to worsen economic outcomes where vertical imbalance is high, transfers are discretionary, and the degree of borrowing autonomy by subnational governments is high. More generally, both the size of government and the macroeconomic effects of expenditure decentralization are shown to be contingent on the levels of fiscal autonomy (Rodden 2006; Rodden and Wibbels 2002).

To sum up, well-defined allocations of fiscal powers facilitate both autonomy and accountability, leading to better efficiency and distributive outcomes. By contrast, convoluted, opaque, ambiguous, ever changing intergovernmental fiscal arrangements provide a natural breeding ground for political and economic opportunism (León 2010).

Dimensionality

Federations are by definition two dimensional political spaces: citizens' preferences are weighted against those of territories. And, as discussed, the system of representation reflects the balance. Beyond this basic fact, however, federations vary in the degree of complexity of their political space and such complexity is an important mediator of the feasibility of self-enforcing, stable agreements. The logic is straightforward: as the dimensionality increases, and the intensity of preferences of specific groups over one particular issue makes them less flexible in negotiations, the room for either stable political arrangements and/or the sort of fiscal and representative arrangements that facilitate efficient and fair market outcomes shrinks. Ultimately, once the degree of preference dimensionality and heterogeneity passes a threshold, federalism is no longer a viable institutional solution, and any attempt to impose it, may in fact backfire.

There are many sources fuelling preference heterogeneity within federations. A relatively well understood one concerns the degree of asset specificity across regional economies. Asset specificity conditions the effectiveness of the constraints typically associated with labor and capital mobility as the degree of factor mobility decreases with the degree of specialization of the regional economy (Boix 2003; Beramendi 2012). In a specialized, asset specific economy, human capital and skills are tied to the regional labor market. In addition, capital is less responsive to tax

advantages and more sensitive to the fit between its production needs and the characteristics of the labor force and educational system. As a result, incumbents in richly endowed units may be less constrained by the potential externalities of decisions adopted in other jurisdictions and more constrained by the likely formation of cross-class regional coalitions. Moreover, heterogeneity among units alters the incentives of the poorly endowed units. Since capital is likely to flow from poor to rich regions, they abandon any attempt to promote economic efficiency through policy. As a result, capital mobility facilitates rather than disciplines the ability of poor units' incumbents to engage in ineffective public policy (Cai and Treisman 2005). This introduces a different dimension that cuts across other aspects of the organization of federations and their economic implications.

A second, and more prominent, source of preference heterogeneity concerns ethnic, religious, or national identity differences among the members of the federation. As we discussed earlier, federalism emerges as a source of artificial symmetry among uneven partners (Rector 2009), and the management of such symmetry is more difficult where the borders of the political units are drawn along ethnic or religious lines.¹³ The room for stable arrangements that facilitate efficient economic outcomes declines with heterogeneity in preferences about basic political and fiscal arrangements, which are in turn proportional to cross-regional variation in ethnicity, religion, or national identity. Asymmetric arrangements for regions with strong identities such as the existing ones in Quebec or in the Basque Country often offer the only viable possibility for preventing conflict. However, asymmetric federalism may not be a sufficient condition to appease centrifugal tensions, which may crucially depend on the existence of identity-based and regional parties that dominate the representation of territorial interests (Brancati 2004). Interregional economic inequalities may remain dormant until regional political entrepreneurs succeed in exploiting them by constructing identities and/or activating ethnic or identity issues in the electoral arena. Decentralization of political power and resources to territorial subunits may actually contribute to strengthen regionalists' capacity to reinforce those identities (Brancati 2006), which confers federal structures with a "double-edge" nature in the execution of stable political and economic agreements (Erk and Anderson 2009).

Concluding remarks: federalism, distribution, and stability

A precise definition of the essential qualities of the federal model has become a difficult task given the enormous institutional variation associated with federal realities. However, all federal countries are grounded in the same delicate balance between unity and autonomy and, in turn, in a fundamental tension between two devils: a too strong center that "overawes" subnational units and the potential abuses by opportunistic subnational elites. The stability of federal institutions cannot be enforced by the constitutional contract due to its incomplete nature, so a better understanding of the dynamic aspects of federalism and their implications for the question of institutional stability requires focusing on other mechanisms, namely the specific design of federal institutions and the context in which these institutions operate.

The secessionist pressures in Catalonia or the failure of the European Union in dealing with the sovereign debt crisis illustrate the importance of the specific design of representation and fiscal arrangements and its interaction with contextual factors for understanding the self-enforcing dynamics of federal institutions. These two experiences show the nature of federalism as an institutional form that evolves endogenously by virtue of its own distributional effects (Beramendi 2012). Its success as an institutional engine for stability depends critically on securing an institutional design that prevents defection across levels of government by affording everyone input in the political process and anticipating, to the extent possible, future distributive struggles.

Centrifugal representation in the European Union explains the design of a monetary union without fiscal integration, which preserved territorial inequalities. The asymmetric impact across member states of the sovereign debt crisis triggered a territorial distributive conflict and increased the heterogeneity of preferences about the design of distributive mechanisms within the Economic Monetary Union. Increasing polarization between member states' preferences about the potential reform of distributive institutions (more or less Europe?) has raised speculations about the potential break-up of the Eurozone. However, the diversity of preferences among member states seems too strong to allow for further fiscal integration in the future.

In Spain, centripetal representation at the time of the constitutional arrangements in 1978 explains the centralist design of redistributive policies (social security). The distributional consequences of this initial fiscal pact as well as the constitutional fiscal privileges granted to the Basque Country and Navarre explain Catalan elites' continuous demands for a reform of fiscal arrangements. Catalan representatives have demanded the same levels of fiscal autonomy as in the Basque Country and Navarre, arguing that the level of redistribution of the system is excessive. These claims have triggered different reforms of federal fiscal arrangements during the last decades and, in turn, have increased the distributive conflict about the allocation of resources among regions. A crucial factor in fuelling heterogeneous preferences has been the presence of dominant nationalist parties both at the regional and national level and the resulting centrifugal push of political competition. The increasing polarization of the preferences over political and fiscal federal arrangements among regions is currently challenging the territorial organization of the Spanish state, as a majority of Catalan elites regard secessionism—and not reforms of fiscal federalism—as the only viable solution of the conflict.

Notes

- 1 www.econ.brown.edu/faculty/henderson/papers.html
- 2 Further information on the project and data can be found at www.unc.edu/~gwmrks/data_ra.php
- 3 For example see Deutsch (1957) on social and communication conditions for federalism to emerge as a "security community." See also Wheare (1946) and Bowie and Friedrich (1954).
- 4 In turn, the link between parties and the functioning of institutions tends to be reinforced over time. Focusing on the reverse causal path, Chhibber and Kollman (2004) examine the experiences of Canada, India, the United States, and the United Kingdom to argue that fiscal and administrative centralization is an important factor driving the centralization of the party system.
- 5 "It often becomes impossible, amidst mutual accusations, to determine on whom the blame or the punishment of a pernicious measure, or series of pernicious measures, ought really to fall. It is shifted from one to another with so much dexterity, and under such plausible appearances, that the public opinion is left in suspense about the real author" (Alexander Hamilton, *The Federalist Papers*, n. 70).
- 6 Some studies show that national conditions matter in explaining electoral results in regional elections (Anderson 2006a, b; Atkeson and Partin 1995; Carsey and Wright 1998; Gélinau and Bélanger 2005; Gélinau and Remmer 2006; Hansen 1999; Niemi *et al.* 1995; Partin 1995; Remmer and Gélinau 2003). Others have found that the electoral support of regional incumbents depends on regional economic conditions (Lowry *et al.* 1998; Partin 1995; Squire and Fastnow 1994).
- 7 Rodden and Wibbels (2011) measure electoral externalities between national and regional elections as the correlation between vote-shares in federal and regional elections; whereas Schakel (2013), drawing from recent works on territorial politics in Europe (Jeffery and Hough 2009), develops a *dissimilarity index* that measures congruence between national and regional elections.
- 8 Provided that the demand for local public services is income elastic, that these services are financed by income taxes (Oates 1972, 1991) and that there is perfect mobility, Tiebout's (1956) model predicts that communities become homogeneous in income and heterogeneous in capacities. For a critical evaluation of the benefits and shortcomings of these assumptions, see Panizza (1999).

- 9 The relationship between mobility and redistribution in federal systems is a field of its own. For a more detailed treatment on the subject, see Peterson and Rom (1990) and Epple and Romer (2001).
- 10 There is a wealth of empirical results on this subject. Gibson, Calvo, and Falleti (2004) show that if poor, underpopulated units are overrepresented in the upper chambers, federalism severely constrains macro-economic efficiency. Finally, Wibbels (2003) finds that influential and malapportioned senates facilitate the emergence of pro-bailout coalitions.
- 11 Weak party systems correlate quite closely with low levels of development and clientelism (Kitschelt and Wilkinson 2007) which in turn shape the economic consequences of political exchanges. If the principal (citizens) is poor, uneducated, or socialized in a polity where the rule of law has given into private exchanges between patrons and clients, agents (i.e., political elites) likely hold office with the purpose of maximizing their clients' rents, as well as their own. In these contexts, civic virtues lag behind in society's preeminent values. As a result, the dynamics of local politics disrupt rather than facilitate the efficient working of the economy (Wibbels 2005a; Treisman 2007). In contrast, a better educated principal, coupled with a generally endorsed set of principles guiding public life and a consolidated legal system, creates a very different interaction between principal and agents. Electorates are more likely to punish outright rent extractions by public officials and reward good governance and economic performance. Incumbents will seek to remain in office by maximizing the satisfaction of a majority of the members of their respective *demoi*.
- 12 The effectiveness of hard budget constraints is not straightforward. Legal provisions not to bail out subnational governments may not be enacted, giving way to a strategic interaction between the center and the units where the identification of bailout expectations becomes crucial. On the difficulty of empirically identifying bailout expectations and their implications for fiscal outcomes in federations see Rodden (2006).
- 13 This is the most common scenario in multinational federations, as one of the reasons why federalism is advocated as a "peace-preserving" institutional device is precisely that it grants ethnic minorities with extensive political and financial powers.

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16

BUREAUCRACY

*Victor Lapuente*¹

The dinosaur in the institutionalist room

The intellectual parents of our current democracies – among others, the likes of Locke, Montesquieu, Smith, Burke, Tocqueville, and the founding fathers—devoted very little attention to the bureaucracy in their cornerstone discussions on how political power should be allocated in a society. They had a relatively well-grounded justification: the bureaucracies of their time were objectively small. For instance, the overall U.S. federal bureaucracy contained less than eight hundred people in the 1790s, and the currently all-mighty Department of State, fit into two rooms, where a doorkeeper, a messenger, and four officials assisted the secretary (Grindle 2012: 61).

Nowadays, with bureaucracies employing millions of employees in most countries, there is little excuse to leave bureaucracies aside when deliberating on the appropriate design of government. And, yet, the most influential contemporary institutional thinkers, *as if* path depended on their predecessors' mental frameworks, continue to overlook the role of public administrations. The vast and methodologically very heterogeneous literature, linking good institutions to economic development (North 1990; De Long and Shleifer 1993; Mauro 1995; Acemoglu *et al.* 2001, 2002), has largely focused its attention on the two extremes of the policy process—decision-making and policy outcomes—overlooking the bureaucratic apparatus that lies in-between.

First and foremost, scholarship concern has concentrated on the institutions involved in the process of decision-making, such as: who takes the major policy decisions in a country (e.g. an autocrat ruler, a cabinet supported by a PR-elected parliament, a directly-elected president in combination with a first-past-the-post legislature); constraints to take opportunistic actions (e.g. expropriation of private property); and incentives to adopt socially beneficial policies (e.g. provision of public goods). We have undoubtedly learned a lot about how these decision-making institutions shape socio-economic progress (e.g. Persson and Tabellini 2003), but this literature has produced few normative implications on the question of which policymaking institutions are more desirable to deliver good policies. A normative answer has not even been given to the question of whether a democratic policymaking is an unequivocally better provider of quality of government than an autocratic one (Charron and Lapuente 2010).

Authors have frequently relied on composite indicators of the nature of policymaking in a policy—chief among them being the degree of unilateral control of the ruler over policymaking, in other words: up to which extent does the ruler have margin of maneuver to do as she (although generally he) pleases? For instance, Polity’s famous variable “constraints on the executive” has been associated with good government (Acemoglu *et al.* 2001, 2002). Nevertheless, the macro correlations between constraints on the executive and good outcomes tell us little about which constraints matter more. For instance, if we look at Polity IV’s constraints on the executive,² it is unclear whether it is more important that “the ruling party initiates some legislation or takes some administrative action independently of the executive” or if the key is that “the legislature or party approves some categories of appointments nominated by the executive”? Or simply is it the existence of “an independent judiciary,” irrespective of the particular institutional equilibrium between the legislature and executive, that matters?

If not concerned about the decision-making process, scholars have directly looked at the outcomes—e.g. the quality of the policies—emerging out of this sort of black box, which the state apparatus constitutes for many researchers. Researchers have coined concepts aimed to encapsulate the quality of the policies delivered by a given government—e.g. “state capacity” (Besley and Persson 2009), “bureaucratic quality” (Öberg and Melander 2010), “impartiality” (Rothstein and Teorell 2008), or “inclusiveness” (Acemoglu and Robinson 2012). Yet, the answer to the relevant comparative question of “how come some countries have developed these high-quality polities” remains elusive. In addition, focusing on government outcomes, we again face a problem of identification, given the composite nature of indicators generally used to capture the overall levels of quality of government. For instance, which of the characteristics, compounding the highly used International Country Risk Guide’s “bureaucratic quality index,”³ matter more and why? Is it the “strength and expertise to govern without drastic changes in policy”? Is it being “autonomous from political pressure and having an established mechanism for recruitment and training”? In other words, is it a question of size and resources of a state bureaucracy? Or is it a question of having a certain type of bureaucrat?

All in all, mainstream comparative literature on institutions has tended to look at both ends of a state apparatus—i.e. who takes the decisions and which type of outcomes we do get—while its inner workings—i.e. how the bureaucracy transforms decisions at the top into street-level outcomes—are largely treated as a black box. Outside academia, bureaucracy has not enjoyed very good press either and the term bureaucracy itself has a pejorative connotation. The “mobilization of bureaucratic sentiments” (Olsen 2006: 1), and the replacement of bureaucratic organizations for more market-like solutions, has fostered the conviction that bureaucracy is an “organizational dinosaur” to which “it is time to say good-bye” (*ibid.*).

This chapter is devoted to show that the bureaucratic dinosaur is alive and plays a critical role in understanding the overall prosperity of nations, on a large variety of dimensions. It does so by, in the first place, reviewing recent contributions that have aimed to dissect the institutional underpinnings of bureaucracies. In particular, it focuses on studies that attempt to operationalize and measure bureaucratic characteristics, which can “travel well” across countries and, thus, can provide the basis of fruitful comparative analyses. This strand of literature is still in its infancy, but is already providing stimulating theoretical insights and empirical evidence, on the decisive role of bureaucratic institutions for both facilitating the best (e.g. economic growth, quality of government) and preventing the worst (e.g. corruption, social conflict). In addition, the next sections offer some original empirical indications on the importance of bureaucracy for understanding long-term economic performance and quality of government.

The review largely relies on research conducted mostly together with Carl Dahlström (2012, 2013),⁴ but also with Agnes Cornell (2012, 2014). Many relevant questions will remain

unanswered after reading this chapter, not least the question of how “good” bureaucracies emerge in the first place. Consequently, the chapter is merely an invitation to deepen our, so far limited, understanding of the “nuts and bolts” of state machineries. In order to fully comprehend the effects of institutions, we need to have a paused look at this big organizational dinosaur we have had in the room, for at least several decade, – and that has largely remained unnoticed while research efforts have been devoted elsewhere. As Olsen (2006: 1) stated, “maybe it is time to rediscover bureaucracy.”

Bureaucracy and the economy

Max Weber is the first known scholar to have remarked on the role of what later would be called *Weberian Bureaucracy* for socio-economic progress. In his own words, bureaucracy was “technically the most perfectly adapted form for achieving the highest level of performance” (Weber [1922] 1978: 973). Weber triggered a strand of thought, where we could also include Polanyi ([1944] 1957), contending a sort of “bureaucracy as a tool of growth” hypothesis (Evans and Rauch 1999: 479). The view of these scholars is obviously in sharp contrast to what could be labelled the “bureaucracy (except for protecting property rights) as enemy of growth” hypothesis or “Smithian” view of government (ibid.), such as Niskanen (1971) or Buchanan, Tollison, and Tullock (1980).

One of the main shortcomings of this debate between defenders and opponents of bureaucracy has traditionally been the lack of comparative measures of bureaucracies. We have benefited a lot from case studies showing particular mechanisms, through which bureaucracies have played a decisive role on economic development, particularly, in Asia (e.g. Johnson 1982, on Japan, Amsden 1989, on Korea, or Wade 1990, on Taiwan). Despite the useful insights emerging from these studies on the positive role of bureaucracy in the “East Asian Tigers,” such as the intriguing and thought-provoking Evans’ (1995) concept of “Embedded Autonomy,” there are palpable problems of inference from research of this nature.

In order to address the lack of comparative data, Evans and Rauch (1999) built their highly cited *Weberianness Scale*—an indicator based on country-expert evaluations collected in the early 1990s. The index captures the “degree to which core state agencies are characterized by meritocratic recruitment and offer predictable, rewarding long-term careers” (Evans and Rauch 1999: 749). Following Weber, Evans and Rauch focus on a specific bureaucratic feature—the civil service—and the underlying assumption is that what matters the most in a bureaucracy and what determines most of its behavior, is the conditions under which public employees work, and not so much other characteristics, conventionally associated with a Weberian bureaucracy, such as “hierarchy” or “standardized procedures.” For example, some items in Evans and Rauch’s *Weberianness Scale* measure the importance of exams (instead of political appointments) in recruiting civil servants, or whether civil servants have secure tenure and are likely to stay in the civil service (instead of being dismissed by politicians). The mechanisms through which a Weberian bureaucracy would foster economic growth are, according to Evans and Rauch (1999; see also Rauch and Evans 2000), multiple, including: more competent public employees (vis-à-vis a politicized administration); longer time horizons for employees (unlike political appointees, who are more likely to behave myopically, given their shorter time horizon: until the next elections); a bureaucratic esprit de corps and a sense of shared norms among peers (vis-à-vis the more unstable working environment when concerns of political loyalty dominate) that incentivizes good performance and deters corrupt practices. Nistotskaya (2009) adds another mechanism, namely that meritocratic bureaucracies foster economic growth because their impartiality allows would-be entrepreneurs to take the risky decision of investing.

Evans and Rauch (1999) show how countries that rank higher in their Weberianness Scale (e.g. Spain, Singapore, Taiwan) exhibit higher growth rates, in the 1970–1990 period, than countries with a less Weberian administration (e.g. Syria, Haiti, Ecuador), even when controlling for other variables used in standard growth models. On the one hand, their solid result could be conditional on the fact that Evans and Rauch’s dataset only covers 25 “semi-industrialized” countries. It may thus be the case that bureaucracy matters at a crucial moment of industrialization, but, that it could have a negligible or even negative effect on growth, both before and afterwards. Additionally, in their sample there is clearly an overrepresentation of East Asian Tigers, and of other countries ruled by “developmentalist dictators” in the post-war period, such as Spain, and, consequently, we do not know whether the effect of a Weberian bureaucracy is conditional or a by-product of a historically specific type of authoritarian rule. On the other hand, Evans and Rauch’s study is a very important contribution to a field that badly needs to put some “numbers” to the vague ideas of what constitutes a Weberian bureaucracy. Although only an exploratory cross-country analysis, an (in their own words) uncontroversial conclusion of their research is that the “Weberian-state hypothesis” deserves more attention from social scientists. An attention that, unfortunately, it has not yet received.

Figure 16.1 visually revisits the relationship between Weberian bureaucracy and economic growth, studied by Evans and Rauch (1999), with a dataset collected almost two decades after, the Quality of Government country-expert survey. In three waves (in 2008–2009, 2010, and 2011–2012), 1,053 public administration experts gave their views on several characteristics of the public administrations of 135 countries.⁵ The figures show that there seems to be a high correlation between the level of economic development of a country (i.e. income per capita) and the average answer of the experts on the country’s public administration to the following question: “When recruiting public sector employees, the skills and merits of the applicants decide who gets the job?”

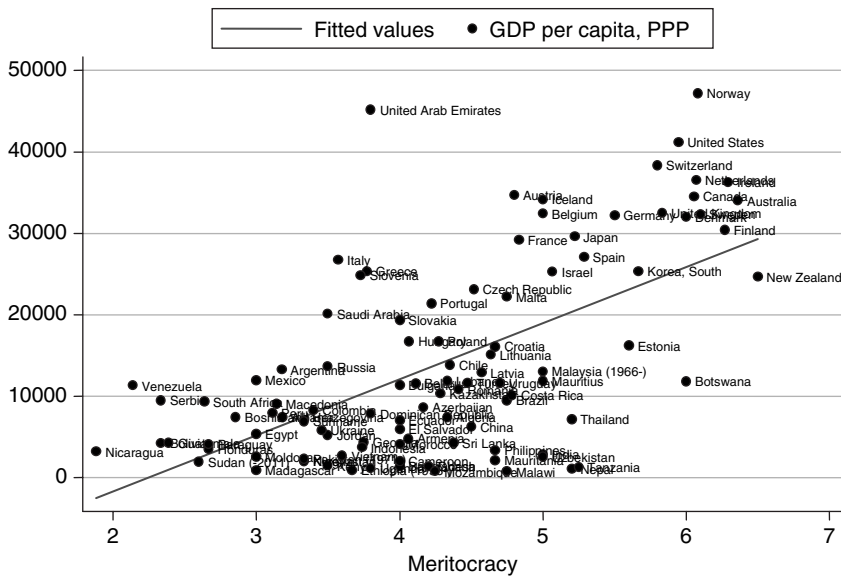


Figure 16.1 Degree of merit-recruitment in the public sector and GDP per capita

Notes: The figure shows the correlation between merit-recruitment in the public sector and wealth. GDP data from the World Development Indicators Dataset by the World Bank, refers to year 2009. The meritocracy data is from Teorell et al. (2011). The 102 countries with available data are included in the sample.

(with 1 meaning *hardly ever* and 7 *almost always*). One can see that countries where public employees are meritocratically recruited are richer than countries where politicians decide at will who works in the public bureaucracy.

Unlike Evans and Rauch (1999), we are in a position to argue that this association, between a merit-based administration and economic wealth, not only holds for developing or emerging countries, but also for advanced industrialized economies. This can be better seen in Figure 16.2, where the sample is restricted to OECD countries. Advanced capitalist economies also seem to benefit from a more merit-based administration. OECD countries, where public employees are recruited according to their merits (e.g. Netherlands, Norway, Australia), are wealthier than other advanced economies, where both public administration scholars (Piattoni 2001) and this survey's country-experts consider that civil servants owe their positions to political connections (e.g. Greece, Italy, Mexico).

Figure 16.3 allows us to address—if far from solve, then, at least, preliminarily address—an obvious problem of endogeneity. What if the arrow of the causal relationship between merit-based bureaucracy and economic development actually goes in the opposite direction? Maybe what simply happens is that, as countries become wealthier, they can allow themselves to have a better trained and merit-recruited bureaucracy. Figure 16.3 provides an intuitive answer that, at the very least, seriously questions an interpretation of the causality as mostly going from economic development to bureaucratic institutions. The countries in red are those that Charron *et al.* (2012) coded as having a non-bureaucratic (or patrimonial) “state infrastructure” around the year 1800 and the countries in green are those coded as having a bureaucratic (or merit-based) state infrastructure, also at the beginning of the nineteenth century. The codification is mostly based on the classification made by Thomas Ertman in his encompassing *Birth of the Leviathan* (1997), probably the most documented typology of the administrative structures that Western countries

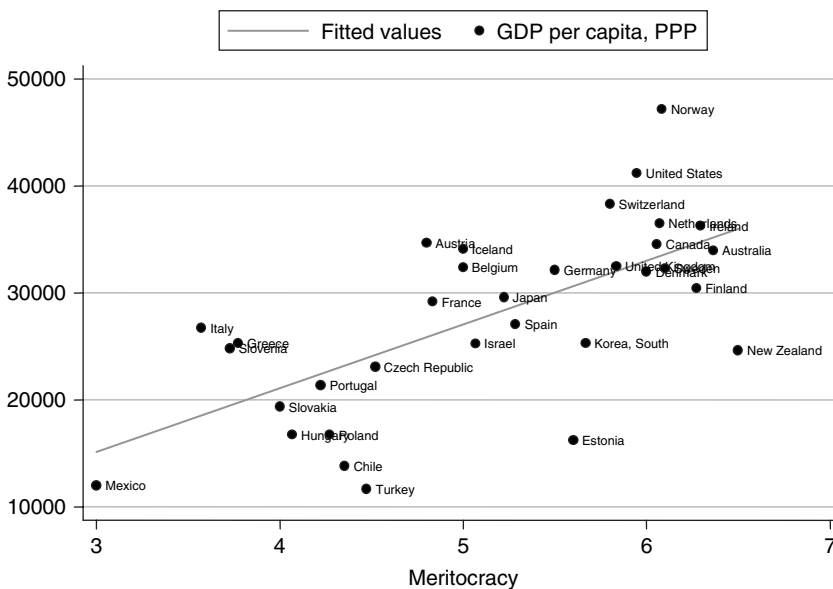


Figure 16.2 Degree of merit-recruitment in the public sector and GDP per capita in the OECD

Notes: The figure shows the correlation between merit-recruitment in the public sector and wealth in the OECD. GDP data from the World Development Indicators Dataset by the World Bank, refers to year 2009. The meritocracy data is from Teorell *et al.* (2011). The 33 OECD countries with available data are included in the sample.

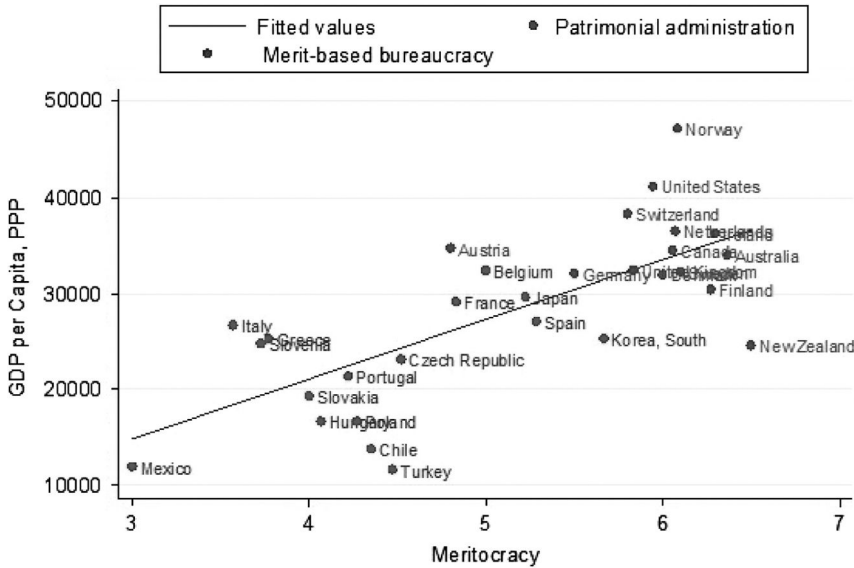


Figure 16.3 Historical legacies of administrative structures and GDP per capita for 30 OECD countries

Notes: The figures show that countries with a legacy of merit-based meritocracy from the eighteenth century are the wealthiest OECD countries of today. The colour coded historical legacies dichotomy is from Charron *et al.* (2012) and refers to the twelfth century bureaucracy. GDP data from the World Development Indicators Dataset by the World Bank, refers to year 2009. The meritocracy data is from Teorell *et al.* (2011). The 30 OECD countries with available data are included in the sample.

developed during their state-formation processes. We see quite clearly how countries that—for whatever reason (I will cover this question later, yet it is worthwhile to anticipate that there is no clear connection between political or democratic development and state infrastructure)—had consolidated a patrimonial state infrastructure during their process of state formation, are notably poorer than countries that started the nineteenth century with a bureaucratic state infrastructure. If—and this is a big “if”—it is economic development that drives a country’s type of bureaucracy, we need a theory and empirical evidence that goes back, at the very least, more than two centuries.⁶ If not, what the graph indicates is that historical administrative legacies seem to exert an important effect on economic development, i.e. countries that used to have merit-based administrations also tend to have more meritocratic administrations today and tend to be substantially wealthier.

In other words, Figure 16.3, despite its exploratory character, points out that institutional legacies do seem to matter, as a large institutionalist literature has long noted. The novelty is that, while the existing literature has focused on the *legacies left by political institutions*—i.e. think of the research in political economy from the path-breaking work by North and Weingast (1989) to the influential book by Acemoglu and Robinson (2012)—here we see that the *legacies left by bureaucratic institutions* established centuries ago also may be of crucial importance for understanding contemporary economic success.

The bivariate observation in Figure 16.3 is obviously not satisfactory enough and we need to know much more on the micro-foundations of this bureaucratic hypothesis on the wealth of nations. We will discuss this as follows. Before doing so, it is important to mention some qualitative evidence showing the implausibility of the hypothesis that a meritocratic bureaucracy is

merely an outcome of economic development. Indeed, it seems that a *struggling economy* may actually present better ground for the emergence of a merit-based bureaucracy than a buoyant one. For instance, Doner, Ritchie, and Slater (2005) note that one of the reasons why a meritocratic administration was consolidated in South Korea, Taiwan, and Singapore to a larger extent than in Indonesia, Malaysia, the Philippines, and Thailand, is that the former three countries suffered from “systemic vulnerability.” South Korea, Taiwan, and Singapore were countries with “hard budget constraints” and countries where a “deterioration in the living standards of popular sectors” (ibid.: 328) was very likely. On the contrary, the economies of Indonesia, Malaysia, the Philippines, and Thailand were less vulnerable—which allowed rulers to “retain power with much less ambitious state-building efforts” (ibid.: 327). Along similar lines, Lapuente (2007) recalls the moment when state-building reforms were introduced in Spain under the rule of Dictator Franco (1939–1975): after a series of small-scale social revolts in Barcelona, the Basque country, and Madrid against the tough socio-economic conditions in the early 1950s Spain. When “facing the potential threats, derived from a struggling economy, the regime seemed ready to start a shift” (ibid.: 213), and Franco “became a developmental dictator” (ibid.: 173). That a bad economic situation was a driver for a meritocratic reform of the public administration was acknowledged by the head of the administrative reform, Mr. López Rodó, who argued that an efficient state could contribute to a per capita income of \$2,000 and that this, in turn, would evaporate social tensions (ibid.: 214). In other words, many rulers—especially authoritarian ones—seem to foster a merit-based bureaucracy, instead of an easier-to-control patronage-based one, not as a result of economic development, but, on the contrary, as a consequence of bad economic performance.

Bureaucracy and good government

Charron *et al.* (2012) test a battery of potential mechanisms that could be mediating in relationship plotted in [Figure 16.3](#), between the type of historical administrative structure and current levels of economic development. In particular, they find a notable effect of their historical administrative dummy on 17 variables capturing “good” institutions and outcomes that have also been subject to intense scrutiny by the so called *Legal Origins Theory* (La Porta *et al.*, 2008), such as the strength of judicial institutions, the type of financial laws and protections, the pervasiveness of government regulation of business, and standard indicators of good governance, such as the extent of corruption and the prevalence of the rule of law. Furthermore, Charron *et al.* (2012) show that their hypothesis on bureaucratic origins better accounts for the variation among the OECD countries, explored in terms of business-friendly policies, than the legal origins theory. Put simply, having an English or Common Law tradition instead of a German or French Civil Law one—a distinction that has been considered critical for the fate of a country at least since Hayek’s (1960) famous argument on the superiority of the Common Law—may matter less than having a given (bureaucratic or patrimonial) “state infrastructure.” When controlling for type of state origin (i.e. meritocratic or patrimonial), the type of legal origin does not seem as relevant as conventionally thought.

There are several reasons to disregard the assumption that the statistical connections between historical levels of meritocracy and contemporary outcomes, found by Charron *et al.* (2012), are spurious. In the first place, a large number of administrative scholars have noted that the robustness of different types of administration (i.e. more patronage-based vs. more merit-based) change because of the vested interests created. Famously, even Alexis de Tocqueville noted that the French Revolution was not able to alter the nature of the French bureaucracy (Rugge 2003). Shefter (1977) provides some mechanisms to support that observation, namely that in countries

with meritocratic administrations, bureaucrats—in alliance with the middle-classes from where civil servants come from—form coalitions that are capable of mobilizing economic and know-how resources against eventual attempts, by political incumbents, to restore a more politicized administration. Conversely, in countries where public employment depends on loyalty to the executive, those networks of individuals benefiting from it will also react against meritocratic reforms. In other words, although many Western countries—take Spain as one of the most paradigmatic examples of change—have undoubtedly moved to having more meritocratic administrations than in the early nineteenth century, the history of their bureaucracies exerts a certain weight or inertia, namely that while meritocratic criteria matter more in some countries, in others it is loyalty to the incumbent that is more relevant for a successful career in the civil service.

In addition, the effects of the type of administration or state infrastructure that a country developed centuries ago may have “survived” across time through other mechanisms. For instance, there are reasons to think that the adoption of a certain legal system could be endogenous to the type of state a country had. Take Napoleon, considered to be the founding father of the French Civil Law system—a legal system that pushed for a more “top-down” approach to Law than the “bottom-up” Common Law prevailing in Britain. Could he have developed a legal system, which granted interventionist powers to the executive, if the French state had lacked an interventionist executive to start with? And, why did the Civil Law, which the Napoleonic armies spread through the European continent in the early nineteenth century, take a more interventionist edge in countries like Italy, Spain, or Portugal and a less interventionist turn in Northern Europe? In other words, there are reasons to think that the type of law a country developed (i.e. French Civil Law vs. less interventionist legal systems) was partially the result of—and also partially reinforced—the type of “state infrastructure” (i.e. bureaucratic vs. patrimonial). Relatively unconstrained executives, in terms of managing the strings of the state apparatus, pushed for the consolidation of legal systems that also gave executives as much powers as possible to control the judiciary.

The findings by Charron *et al.* (2012) also give support to the original and incisive, yet empirically limited, critique of the constitutionalist view by Avner Greif (2006, with González de Lara and Jha 2008). According to the “constitutionalists” (among many, North and Weingast 1989; Acemoglu *et al.* 2001, 2002) rules and institutions, limiting the (decision-making) power of rulers, represent the institutional foundations for credible commitment and, thus, for good government. Yet, as Greif astutely observes, the Magna Carta—or, for that matter, the constitutional arrangement after the Glorious Revolution—did not make the rule of law prevail over the discretionary rule of the ruler. What prevented the latter from abusing his (or her) powers was the absence of a body of directly accountable administrators willing to implement the monarchs’ desires. The English monarchs—like their counterparts in Europe—wanted to take arbitrary decisions, but could not, as they literally lacked the “physical capacity” (González de Lara *et al.* 2008: 105) to execute abuses of property rights.

Thus, the picture that emerges from these recent studies is that we should revise the role played by legal origins and constitutions—the two main institutional foundations of credible commitment according to the conventional view in the scholarship—and, at the very least, include a third market-preserving institution—the existence of a merit-based bureaucracy. In light of these empirical explorations on the structure of state apparatuses, either nowadays or in Modern Europe, there seems to be a strong association between the way public employees have been recruited (meritocratically or at will) and long-term and non-distortionary policies. [Figure 16.4](#) provides indications of a potential mechanism mediating between the personnel recruitment type in the public sector and economic development. It plots a high association

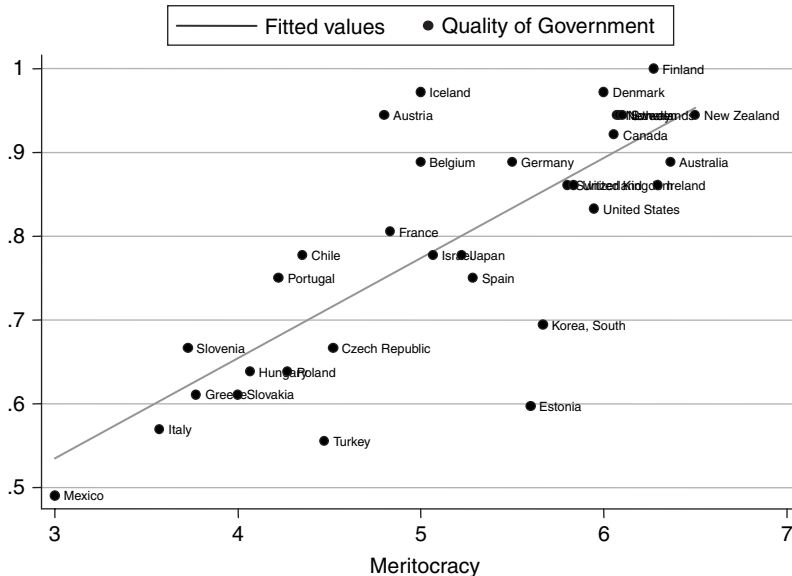


Figure 16.4 Meritocracy and QoG for OECD countries

Notes: The figure shows the correlation between merit-recruitment in the public sector and quality of government in the OECD. Quality of Government is the mean value of the *International Country Risk Guide* variables “Corruption,” “Law and Order,” and “Bureaucracy Quality,” scaled 0–1, from Teorell *et al.* (2013). The meritocracy data is from Teorell *et al.* (2011). The 33 OECD countries with available data are included in the sample.

between the level of meritocracy in the public bureaucracy and a standard indicator of good governance—the *International Country Risk Guide*’s Quality of Government Index, that averages the levels of corruption, the respect for law and order, and the quality of the country’s bureaucracy – for OECD countries.

There are some well-known exceptions, such as Austria and Belgium—two countries that have “not made the expected full transition from patronage politics to programmatic policies” (Kitschelt and Wilkinson 2007: 3)—and, yet, perform relatively well. Nevertheless, a closer look at these exceptions may shed some light on why their relative lack of meritocracy may not affect the overall quality of their policies that much. The position of Belgium could also be partially explained by the marked differences in subjective perceptions on quality of government in Flanders and Wallonia (Charron *et al.* 2014). Austria is a “case in point” (Müller 2006: 191) of a country, in which patronage has played a unique role as social integrator. In particular, patronage allowed the reconciliation of the major political subcultures—e.g. the reintegration of the former Nazis in the political system—that, for the first time in history, could be incorporated in the Austrian state (Secher 1958; Müller 2006).

Leaving aside these exceptions, the pattern emerging from Figure 16.4 seems quite straightforward: the more merit matters when recruiting public employees, the higher the quality of government. There are other additional mechanisms, through which meritocracy may lay the foundations of economic development. In the analysis of a “natural experiment”—i.e. the adoption of a merit system by many U.S. cities in the Progressive Era—James Rauch (1995) shows how merit-based local governments allocated more resources to long-gestation period projects, such as infrastructure which, in turn, are associated with significant increases in the growth rate of manufacturing employment. Similarly, where the public workforce is mostly composed of

at-will employees, the policies with positive long-term economic effects tend to be, *ceteris paribus*, relatively neglected. Furthermore, Rauch and Evans (2000) indicate that the higher levels of expertise of bureaucrats recruited through formal examination systems may help to explain the higher rates of growth of developing countries with meritocratic administrations. Additionally, Dahlström and Lapuente (2013) provide a large-N comparative study where the level of meritocracy of a country's administration is found to exert a significant effect on the effectiveness of its government, even after subjecting the relationship to controls for usual political, economic, and cultural factors explaining government effectiveness. Dahlström and Lapuente's (2013) argument is not so much that meritocratic bureaucrats are "better types"—i.e. with more expertise than employees selected at will by political incumbents—along the lines of what Rauch and Evans (2000) argue, but that meritocratic bureaucrats are simply "different types" than political incumbents—and, consequently, bureaucrats become a check for politicians, and vice versa.

Bureaucracy and democracy

Against the theoretical discussions and the empirical material presented, it could be argued that a merit-based bureaucracy may in fact be endogenous to political institutions—i.e. that you need a democratic system in place in order to create a well-functioning bureaucracy. Virtuous bureaucracies would be the result of virtuous political systems. Nevertheless, I argue that there are two major objections to this interpretation. First and foremost, there is no empirical support for a "democracy comes first" hypothesis. In the most classic comparative accounts of the emergence of bureaucracies, the relationship between democratic institutions and bureaucracies is far from straightforward.

Second, as mentioned, Shefter considers that a Weberian administration requires the emergence and consolidation of a "constituency for bureaucratic autonomy" (1977: 413), yet, this could happen both in autocratic systems (think of nineteenth century Prussia) and in democracies (e.g. Britain). Democratic rulers do not necessarily foster merit-based bureaucracies, as shown by examples such as postwar Italy (Shefter 1977; Piattoni 2001), many Latin American countries since the early nineteenth century (Grindle 2012), or contemporary Greece (Sotiropoulos 2004; Papakostas 2001). Rather the opposite, many incipient and key pro-merit reforms have been undertaken by extremely authoritarian rulers. For instance, it was precisely in times of the all-powerful Sun-King Louis XIV—i.e. when the ruler's decisiveness was especially high—that the main characteristics of the French administrative state, and in particular the autonomy of the Grand Corps, took shape (Peters 1995).

Likewise, Ertman (1997) shows a dual effect of democratic institutions—or, to be more precise, proto-democratic parliamentarism—on the consolidation of meritocratic (or patronage-based) bureaucracies in Modern Europe. For countries that experienced sustained pre-1450 war, the survival of independent assemblies (i.e. England) allowed them to avoid the monarch-controlled patrimonialism, which the Spanish or French rulers were able to create, because they were less constrained by parliamentary checks. Before 1450, thus, parliamentarism had a positive effect: it fostered the development of meritocratic bureaucracies. Nevertheless, Ertman argues that the effect of independent parliaments was negative on the countries that confronted sustained wars in the post-1450 period. In these cases—think of Prussia, Poland, and Hungary—authoritarian rulers had developed a strong preference for merit-based bureaucracies given their proven efficiency and, on the contrary, parliaments opted for patrimonial administrations. Consequently, authoritarian states, like Prussia, were capable of creating highly meritocratic administrations, while constitutional monarchies, like Poland and Hungary, ended up consolidating patrimonial state machineries.

In sum, there is no historical support for the argument that merit-based bureaucracies are the result of more democratic rulers. Yet, and this issue has also been overlooked in the literature until very recently, there are indications that a merit-based bureaucracy may have an important effect on democratic consolidation. For instance, Cornell and Lapuente (2012) explore the effect of the abovementioned type of “state infrastructure,” which Western countries had acquired by 1800, on the stability of their democracies in the following two centuries. Their results show how the countries who entered the nineteenth century with a meritocratic state infrastructure—think of Scandinavian or Anglo-Saxon countries—had a very low probability of suffering a democratic breakdown, while those with a legacy of patrimonialism—think of France or Spain—had a much higher risk of experiencing an authoritarian reversal, either in the form of coups or auto-coups. That is, the historical consolidation of meritocratic bureaucracy seems a solid guarantor of democracy.

Nevertheless, as in all statistical correlations, we need to ask which specific mechanisms connect a meritocratic tradition within the state to the stability of democracy. Cornell and Lapuente (2012)—using the well-known case of President Nelson Mandela, who declined to dismiss the civil servants he inherited from Apartheid South Africa—argue that meritocratic bureaucracies minimize the “winner-takes-it-all” dynamics, characteristic of highly politicized administrations. Similarly, Lapuente and Rothstein (2014) argue that meritocratic bureaucracies prevent the spread of political disputes to all government layers, igniting, in turn, an overt social conflict, namely:

using a basic water metaphor, it could be argued that the coalition of autonomous bureaucrats with a self-interest in keeping the neutrality of the state, because their career prospects in the administration would be diminished should they join active politics, plays the role of a dam that keeps the conflict confined to the policy-making arena.

On the contrary, in a politicized administration,

bureaucrats with a self-interest in a partisan policy implementation – because their career prospects in the administration are augmented if they engage in politics – trigger a cascade effect, that expand the conflict from the policy-making arena – where elites could enjoy unique opportunities for a compromised face-to-face solution – to rank-and-file public agencies and administrative bureaus, and (critically) to citizens interacting with them.

(*ibid.*)

An example of the former would be Sweden in the 1930s, where a potentially explosive labor-capital conflict—let’s keep in mind that the Finnish civil war had occurred chronologically (1918) and geographically very close—was kept “inside the dam,” The Swedish state, formed by highly autonomous meritocratic bureaucrats, was perceived by all social groups as impartial and thus no temporary “losers” felt the urgency to resort to subversive actions. On the contrary, both the left and the right governments during the Spanish Republic (1931–1936), politicized key positions in the public administration—in the security and armed forces, but also in the territorial administrations—that allowed them to implement policies in a partial way, which benefited supporters and harmed opponents. Consequently, two countries with a relatively high level of social conflict in interwar Europe—Sweden and Spain—took opposite paths to solve them: productive collaboration versus brutal confrontation. Exactly the same year (1936), in which labor and capital

started to bargain the foundations of the Swedish welfare model in the iconic negotiations that took place in Saltsjöbaden, Spain witnessed the collapse of its democratic regime and the beginning of its disastrous civil war (Lapuente and Rothstein 2014).

Tentative policy implications and future research

The importance of state capacity in promoting the best (e.g. socio-economic development) and preventing the worst (e.g. violent civil conflicts) has been argued for a long time. Yet, an overview of the proxies for state capacity, used in the literature, shows us how difficult it is to extract policy implications in which institutions may be fundamental to prevent violent social confrontation, since they mostly are variables that Gourevitch would label as low-agency, as they leave “little room for human action and hence no room for politics” (2008: 137). For instance, Fearon and Laitin (2003) proxy state capacity with GDP per capita and De Soysa (2002) uses trade over GDP. Should we thus just wait and see until a country reaches the level of income needed to have good institutions? I would argue not: As numerous qualitative studies indicate that it might not have been the richest, but actually countries experiencing acute economic problems, which have built better institutions (Doner *et al.* 2005), it is doubtful to assume that economic development will eventually bring state capacity. Rather it seems like we need to build it.

Then the question is how? This chapter has suggested that recent studies, on the positive effects of meritocratic bureaucracies on economic growth, and the consolidation of democratic regimes, are a clear indication that state capacity may critically depend on the structure of public administration. Obviously more research is needed, but it seems that the same specific bureaucratic characteristics may be playing an important role in both of the “big” outcomes: promoting the best and preventing the worst, in a given society. For instance, in light of the evidence on the relevance of non-politicized bureaucracies, a message for democratic promoters, interested in the consolidation of democracy after the “Arab Spring,” is that they “should evaluate the possibility of proposing administrative reforms that are – compared to other more complex institutional arrangements – easy to implement” (Lapuente and Rothstein 2014: 29). Among those administrative reforms, one could list the following: non-partisan (or bipartisan) civil service commissions to recruit public employees, like those operating in many Anglo-Saxon countries; establishing limits to civil servants’ political activities, like those in place in late nineteenth-century Britain or Sweden. Likewise, the generous leaves of absence for bureaucrats who accept a political appointment or run for office, which exist in many countries, should also be eliminated because these incentives reward politicized, instead of impartial, actions.

These measures, aimed at creating a politically autonomous bureaucracy, could lead to improved levels of quality of government in light of the research we have summarized here. Nevertheless, there are reasons to suspect that a “too autonomous” bureaucracy may generate many governance problems. Fukuyama (2013) provides examples indicating that a more realistic approach is to think of an inverted-U-shaped relationship between bureaucratic autonomy and quality of government. A mostly politically dependent bureaucracy performs worse than a moderately autonomous one, but a highly autonomous bureaucracy can become dangerously unaccountable. That was, for instance, the case of early twentieth century Germany and Japan. Their aggressive foreign policies, which, in turn, fostered WWI and WWII, can, to a large sense, be explained by the fact that “both countries had developed very high quality, autonomous bureaucracies, particularly their military services, which then took over from the political authorities the task of formulating foreign policy” (Fukuyama 2013: 358).

Indeed, although in a much less dramatic way, the issue of how to control an autonomous bureaucracy is the cornerstone of the vast and sophisticated research on the American bureaucracy.

Building upon the classical rational choice views of bureaucracy of Tullock (1965) and Niskanen (1971), the late decades of the twentieth century witnessed an explosion of literature on how politicians could control bureaucratic agents (e.g. McCubbins and Schwartz 1984; McCubbins *et al.* 1987), which paved the way for the development of highly influential formal models (e.g. Epstein and O'Halloran 1994; Huber and Shipan 2002). The length and theoretical and empirical sophistication of this strand of bureaucratic literature cannot be summarized here, but, fortunately, a large number of encompassing literature reviews is available, among others, Bendor, Glazer, and Hammond (2001), Knott and Hammond (2003), Miller (2005), and Moe (2013).

It is plausible to expect that the future of research on public bureaucracies, and its contribution to answer the “big questions,” will critically depend on the ability of the current generation of scholars to link the theoretical developments and sophisticated data treatments of this “American” approach to bureaucracies, to the rich insights of the comparative literature that has been surveyed in this chapter.

Notes

- 1 Special thanks to Lovisa Möller for invaluable research assistance.
- 2 For more information on the composition of this and other indicators from Polity IV, see www.systemicpeace.org/polity/polity4.htm.
- 3 On the International Country Risk Guide, see www.prsgroup.com/ICRG.aspx
- 4 See also Dahlström, Lapuente, and Teorell (2011) for an early presentation of this argument and its empirical backing, as well as Charron, Dahlström, and Lapuente (2012) for an application to the institutionalist debate on historical legacies.
- 5 For a more detailed discussion of the dataset, see Dahlberg *et al.* (2013) and Dahlström, Lapuente, and Teorell (2011).
- 6 Such a theory seems, at first sight, quite implausible given the fact that some of the most powerful or richest European powers in Modern Europe—e.g. Spain, France, or Italian city-states—did actually create patrimonial administrations, unlike other relatively less successful nations, such as, for instance, the Scandinavian countries.

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17

ELECTION ADMINISTRATION, ELECTION OBSERVATION, AND ELECTION QUALITY

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Article 21 of the Universal Declaration of Human Rights states that the “will of the people... shall be expressed in periodic and genuine elections which shall be by universal and equal suffrage and shall be held by secret vote or by equivalent free voting procedures.” Most countries in the world hold periodic elections, formal provisions for universal and equal suffrage are common, and election rules guaranteeing a secret ballot are nearly universal. However, despite the apparent progress made towards the widespread adoption of democratic political institutions, the quality of elections held throughout the world continues to vary widely, with some elections approaching the democratic ideal type and others used as tools of authoritarian resilience.² [Figure 17.1](#) illustrates several of these trends.

This chapter provides an overview of recent findings on election administration and election observation, two of the more prominent institutions that influence the quality of elections. Electoral management bodies (EMBs) are the institutions that run elections, whereas election observation aims to provide an independent evaluation of election quality, often making recommendations for improvements in future elections directly to the EMB and host-government. In some countries, the relationship between EMBs and observers is cooperative. In others, the relationship is highly adversarial.

Both EMBs and election observers affect the quality of elections. The conduct of EMBs directly bears on the quality of the electoral process, from the efficiency and accessibility of voter registration to the accuracy of the final vote tally. Election observation may affect election quality by deterring fraud or potentially leading to the substitution of some types of electoral manipulation with others. Observers may also influence election quality through their short-term and long-term effects on EMBs. In the short term, the presence of observers and their recommendations to EMBs may influence the actions of election administrators. In the long term, the recommendations of election observers often include suggestions about EMB institutional design, and over time, recommendations from observers about changes to the election administration may lead to higher-quality elections.

Not only do EMBs and election observers affect the objective quality of elections, but they both also influence how citizens and voters view elections. In general, citizens’ peaceful acceptance of election results helps to validate the process as a credible reflection of democratic choice. Alternatively, if citizens perceive an election to be seriously flawed, they are more likely to protest the results and, under some conditions, become disillusioned with the democratic process

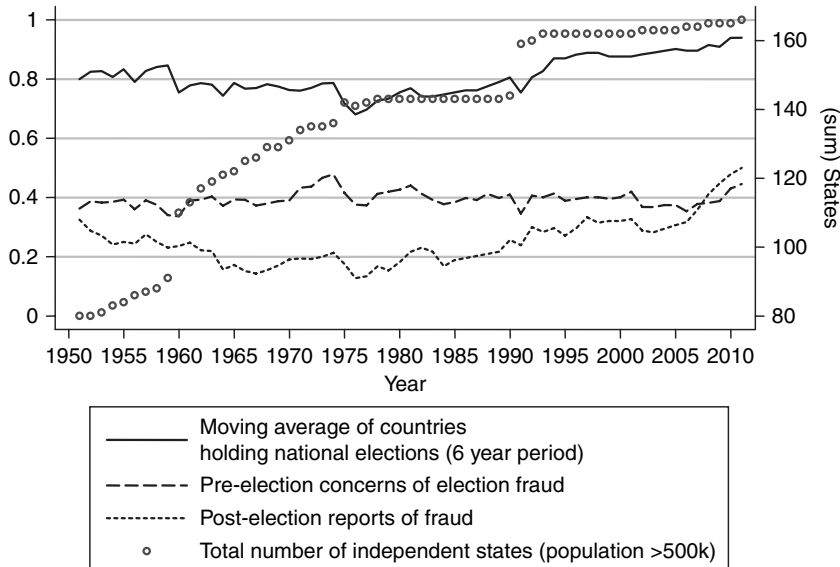


Figure 17.1 Trends in national elections over time

Source: Hyde, Susan D., and Nikolay Marinov. 2012. "Which Elections Can be Lost?" *Political Analysis* 20 (2): 191–210.

(see, for example, Magaloni 2006; Simpser 2013; Svobik 2013). As discussed as follows, public perceptions of election quality may be just as consequential as the true quality of elections.³ Elections can be run fairly and competently, but unless this fact is recognized by the public, many of the purported benefits of democratic elections are undermined. The reports of impartial election observers, both during the pre-election period and following election day, and (partly as a result of these reports) whether EMBs are partisan or neutral, competent or ineffective, can influence election quality—and citizen perceptions of election quality—in important ways.

Many of these relationships remain underexplored in the existing academic literature. Until recently, the administrative conduct of elections—the details of how voters are identified and registered, how ballots are prepared, how votes are tallied, and so on—was a relatively neglected topic. Yet failures of election administration—real or perceived—are a central means by which election quality can be undermined⁴ and, in countries with a history of problematic elections, reforming the EMB is a commonly proposed solution.

This chapter provides an overview of recent work on two types of electoral institutions—electoral management bodies and electoral observation—with particular attention to their consequences for the democratic quality of elections, by which we mean the extent to which electoral processes meet internationally agreed upon standards for fair competition and free participation throughout all stages of the election cycle.⁵ Because both literatures are new, we summarize lessons learned to date, and suggest areas in which future research is likely to be productive.

Electoral management bodies

Despite its centrality to the functioning of representative democracy, election administration attracted little comparative attention until the early 2000s. Empirical research on election administration remains in the early stages, and much of the scholarship has focused on the United States following the problems surrounding the 2000 presidential election. But comparative research has

begun to look more closely at “electoral governance,” or “the wider set of activities that creates and maintains the broad institutional framework in which voting and electoral competition take place” (Mozaffar and Schedler 2002: 7). Scholars began to emphasize the importance of high quality election administration for the legitimacy of new democracies (Elklit and Reynolds 2002) and for stability in post-conflict societies (Lyons 2004), while recent work has focused on the causes and consequences of “electoral malpractice” (Birch 2011), “electoral misconduct” (Donno 2008, 2013), and “electoral integrity” (Norris 2014; Norris *et al.* 2014). In short, there has been a trend towards seeing election administration and election quality more generally not only as indicators of democratic progress, but as independent variables affecting democratization (cf. Mozaffar and Schedler 2002: 12) and as dependent variables worthy of study.

To date, much of the scholarship on comparative election administration focuses on the formal institutional structure of electoral management bodies (EMBs). EMBs carry out essential electoral tasks such as conducting the polling and counting the votes, and often are also responsible for election-related activities such as voter registration, voter education, campaign regulation, and dispute resolution (International IDEA 2006: 5). Not surprisingly, efforts to systematically assess the quality of elections across countries explicitly include features of electoral authorities in their measurements (Elklit and Reynolds 2005; Norris *et al.* 2013).

Among both scholars and democracy assistance practitioners, it is now almost conventional wisdom that an independent election commission is necessary for high quality elections and helps improve the chances for democratization, with the 2000 Mexican elections widely used as the ideal case in which a newly independent election commission helped bring about the country’s first partisan transition in power in over 70 years. As Mozaffar and Schedler (2002: 15) put it, “removing electoral governance from executive control has turned into a rallying cry of democratizing forces all over the globe. Establishing an independent electoral commission, in fact, has become a compelling international norm, a *sine qua non* of electoral credibility.” Birch (2008: 308) similarly notes that “[a]mong practitioners in the fields of electoral assistance and observation, independent central electoral commissions have come to be regarded as the hallmark of accountable electoral administration.”⁶ Some scholars have even argued that independent electoral commissions are key to ensuring democratic consolidation and legitimacy, with Lehoucq (2002: 31) going so far as to argue that in early twentieth century Latin America the independence of electoral bodies was “one of the central institutional developments that made democratization stick in some places, but not in others.”

The prevailing view that EMB independence from government and partisan interests helps determine whether elections are likely to be credible has helped generate several typologies of EMB structure, but there is not yet widespread agreement as to which structural elements are most important for the creation of a truly impartial EMB. An early and influential study by Rafael López-Pintor (2000) distinguished between governmental, independent, and mixed models of election administration,⁷ a typology that has been employed by others in subsequent work. In the governmental approach, an agency of the executive branch (such as the Interior Ministry) administers elections. This model is common among the established democracies in Western Europe, as well as among nondemocratic states in the Middle East.

In contrast, the independent model involves an electoral management body (variously called an electoral commission, council, or tribunal, among other designations) that is formally independent of the executive branch. In some cases, two independent bodies share authority over electoral administration—for instance, with one body responsible for conducting elections and the other playing a supervisory role and acting as a court of appeals for legal challenges to the other body’s decisions. López-Pintor (2000) observed over a decade ago an international trend towards the establishment of permanent EMBs independent of the

executive branch. Today the independent model is most common globally, with most countries in Latin America, Eastern and Central Europe, and the former Soviet Union having independent EMBs. During post-Arab Spring reforms, Egypt and Tunisia both established independent electoral management bodies, and moved away from the governmental model.

In between these two poles is a mixed model of election administration, in which the government administers elections under the oversight of a judicial or party-based body. This model is least common, and is used in France and Japan, among other countries (ACE Project 2013; Birch 2011: 120–121; López-Pintor 2000: 20–24).

Table 17.1 presents data on the proportion of countries that employ each EMB model. While the data suggest a trend towards independent EMBs and away from the mixed model since the turn of the century, comparisons of the data over time should be made cautiously. While data from 2000 and 2004 come from the same author, as do the data from 2006 and 2012, differences in coding rules may make comparisons between 2000–2004 and 2006–2012 problematic.⁸ With this proviso, the data suggest that by 2012 the number of countries with formally independent EMBs had risen to nearly 70 percent from 54 percent in 2000. Rates of governmental EMBs appear to stay constant at around 20 percent. And in 2012 the rate of mixed models had fallen to almost 10 percent from about 27 percent in 2000.

Other potentially important determinants of an EMB’s impartiality include who administers and staffs an electoral body and how they are selected. Although civil servants run the election administration under the governmental model, independent and mixed EMBs vary significantly in the legal requirements and selection processes for election commissioners. A central concern has been the extent to which political parties influence the selection and behavior of election commissioners or magistrates, or what Rosas (2010) terms “partisan autonomy.”

Methods of measuring partisan autonomy are varied. Policy-oriented organizations have distinguished between EMBs whose members are selected on the basis of partisanship, expertise, or a combination of the two. Partisan EMBs may be dominated by a single party or, more commonly, staffed by representatives of several parties (as in Guyana, Israel, Mozambique, and many local election boards in the United States).⁹ In this type of EMB, commissioners are selected partly with the purpose of protecting the interests of their sponsoring parties within the election administration. In expert-based EMBs, the criteria for the selection of EMB commissioners typically include political neutrality and academic training, and the election law may prohibit EMB commissioners from having been active in a political party for a specified period of time prior to appointment (as in Guatemala) (International IDEA 2006: 89). In empirical work, Hartlyn, McCoy, and Mustillo (2008) have adapted the partisan-expert-combined typology of EMB partisan autonomy (while further distinguishing between single-party and multiparty EMBs).

Table 17.1 Formal models of EMB independence (percentage of countries)

	2000 ^a	2004 ^b	2006 ^c	2012 ^d
Governmental	19.3	15.6	20.9	18.2
Mixed	26.9	26.6	13.7	12.3
Independent	53.8	57.8	65.4	69.5
Sample Size (n)	145	154	153	154

^aData from López-Pintor (2000, 25).

^bData from López-Pintor (2004, Annex 1).

^cData from International IDEA (2006, 8).

^dData from International IDEA (2012b).

Table 17.2 Selection criteria of independent and mixed EMBs

	2006		2013		Change (2006–2013)
	#	%	#	%	
Expertise	89	62.7	110	69.2	+6.5
Combination	30	21.1	41	25.8	+4.7
Partisanship	23	16.2	8	5.0	–11.2
Total	142	100	159	100	

Sources: ACE Project 2013; International IDEA 2006, Annex A. Data comes from all countries with independent and mixed EMB models for which there is clear coding of the basis on which EMB members are selected. For mixed (governmental-independent) EMBs, countries are coded according to the criteria by which members of the independent branch of the EMB are selected.

Table 17.2 presents data on the number of countries with independent and mixed EMBs that employ party-based, expert-based, and combined criteria for selecting EMB officials. Election administrators are formally selected by the criteria of expertise in a majority of countries. Although data are currently available for only two points in time, there seems to be a trend away from purely partisan criteria in selecting all EMB members, with only eight countries in total coded as such in 2013, down from 23 countries in 2006.

A related but distinct question from the selection criteria of EMB members is which agencies control the selection process. The exact selection process varies by country, and may involve any combination of actors, including the executive, legislature, judiciary, political parties, and civil society. For instance, the executive may nominate EMB members who are subject to confirmation by the legislature (e.g., Kenya, Sierra Leone), in some cases by qualified majority (e.g. Mexico). Elsewhere the supreme court names the EMB members (e.g. Costa Rica), or non-state actors are involved in the nomination process (International IDEA 2006: 95–96). In some countries, nomination of EMB members is divided between different actors, for instance with some commissioners named by political parties and others named by the courts (International IDEA 2006: 90). Rosas (2010) considers EMB selection processes in empirical work, employing a three-point scale of partisan autonomy based on the extent to which parties have a role in nominating and selecting top EMB officials.¹⁰ Hartlyn *et al.* (2008) also employ a measure of partisan autonomy based on the nomination process and the tenure of EMB appointees relative to those who appoint them.¹¹

All of these measures capture elements of EMB neutrality, yet they also rely predominantly on formal institutional arrangements and thus may fail to capture how these institutions work in practice (Rosas 2010: 80).¹² Different operationalizations of partisan autonomy also produce widely divergent measurements: as Table 17.3 shows, most “expert-based” EMBs are appointed entirely by partisan actors, and thus score very differently depending on the measurement used. Without knowledge of the informal institutions surrounding EMB appointment, it is difficult to determine which method of measurement yields a more meaningful score of partisan autonomy. Reconciling and improving upon existing measures of EMB partisan autonomy will be a priority for future research.

Effects of EMB structure

Overall, existing scholarship suggests that the simplistic view that an independent election commission can automatically bring about high quality elections is not supported—although there is

Table 17.3 Selection process and criteria of independent and mixed EMBs

		Selection criteria		
		Expertise	Combination	Partisanship
Selected by	Judiciary	7	–	–
	Executive and/or legislature/parties and judiciary or civil society	30	13	2
	Executive and legislature/parties	27	14	3
	Legislature/parties	16	10	1
	Executive	25	2	2

Note: Cells present the number of countries in each category. For mixed (governmental-independent) EMBs, countries are coded according to the criteria by which members of the independent branch of the EMB are selected.

Source: Authors' recoding of data from ACE Project 2013.

widespread consensus that independent EMBs are preferable to the governmental model. The desirability of partisan- or expert-based EMBs is more contentious. On the one hand, nonpartisan electoral management bodies are often considered ideal to ensure impartial electoral administration (Pastor 1999: 18; Reilly 2003: 21), and international election observers often recommend that countries move towards non-partisan electoral commissions to improve election quality. The dangers of partisan election officials manipulating the administration of elections to benefit their parties—whether through decisions regarding the electoral process or through outright tampering with the vote count—are obvious. Non-partisan election administrators, in contrast, will presumably have no reason to attempt such tactics, although formal status as non-partisan is not a guarantee that an EMB will behave impartially. It will continue to be difficult to judge whether biased EMBs are masquerading as non-partisan.

On the other hand, multiparty EMBs may provide neutral election administration if the representation of different parties produces a system of checks, while the participation of parties may produce consensus and transparency (International IDEA 2006: 88–89; López-Pintor 2000: 63). In some polarized contexts, moreover, there may be no tradition of administrative independence and a lack of independent notables to staff the EMB, thus making a multiparty body more appropriate. This may particularly be the case in post-conflict situations in which parties are distrustful of each other and seek mutual guarantees. Yet multiparty EMBs may face challenges in decision-making if parties cannot come to agreement, and small parties may be disadvantaged vis-à-vis larger parties if they lack representation on the electoral commission (Birch 2011: 116–117; International IDEA 2006: 88–89; 2012a, 6, 14; Lyons 2004). These problems may be particularly acute in electoral authoritarian contexts.

Beyond categorizing EMBs, scholars have begun to empirically test the consequences of institutional variation among EMBs. These studies have used several measures of EMB independence and of electoral quality, and have employed quantitative and qualitative methods. However, convincingly demonstrating the independent effect of EMB structure on electoral behavior remains extremely difficult. Researchers have not yet uncovered why some countries adopted “better” EMB models than others, and it is still plausible that the correlations discussed as follows are spurious, as countries that are more likely to comply with international norms about EMB structure may be the very same countries also likely to hold higher-quality elections.

Quantitative studies have used both subjective measures of election quality based on the perceptions of citizens (Birch 2008) and elites (Rosas 2010), and ostensibly more objective measures based

on election observer reports (Birch 2011; Hartlyn *et al.* 2008). Birch (2008) studies the impact of formal EMB independence from the government on voter confidence in the fairness of elections across 28 countries, finding a surprising negative correlation between EMB independence and popular confidence. As Birch acknowledges (312–313), endogeneity and measurement problems may affect the results: independent EMBs may tend to be introduced in countries with low quality elections, and the formal independence of EMBs does not necessarily measure their actual independence from the government or partisan interests.¹³ Indeed, Birch's (2011) analysis shows that formal EMB independence is not a statistically significant predictor of *de facto* EMB independence (121–123), which raises serious questions about what can be learned from studies of the effects of formal EMB structure on electoral quality. As referenced, the remedy to this measurement problem is not as easy as using measures of *de facto* EMB independence instead; as such measures are complicated by the fact that biased EMBs may have strong incentives to conceal their biases.

Other studies have focused on the effects of EMB partisan autonomy. Limiting their analysis to presidential elections in Latin America and the Caribbean from 1980 to 2003, Hartlyn *et al.* (2008) find that electoral processes are significantly more likely to be found acceptable by observers where the electoral body is professional and non-partisan.¹⁴ They conclude “that independent, professional [electoral] bodies are close to being a sufficient condition for successful elections” (89). Non-partisan EMBs are not a *necessary* condition for successful elections, however; the multiparty EMB model “can bring confidence if all major political parties feel represented,” although “it can also lead to stalemates or to lower technical competence if directors are chosen for political affiliation rather than skills” (Hartlyn *et al.* 2008: 90). Rosas (2010) studies the impact of EMB structure on the confidence of Latin American politicians in electoral quality. He finds that formal autonomy is associated with elite confidence in elections, but that in countries with high levels of democracy, elites have less confidence in electoral processes when partisan autonomy is high.

In a wider set of cases, Birch (2011: 121–123) finds that political party involvement in naming EMB members is associated with lower *de facto* EMB independence and greater election administration manipulation. Yet partisan involvement in EMB appointments is also associated with less exclusion of political parties from the electoral contest, suggesting that partisan watchdogs are wary of certain forms of manipulation—such as revoking the registration of parties that enjoy representation on the electoral body.

Several qualitative studies have also addressed the effects of EMB structure in the context of broader examinations of comparative election administration. In a wide-ranging comparative study, Schaffer (2008) shows how efforts to clean up elections by reducing fraud can raise barriers to voting, in part through intentional manipulation or mismanagement on the part of election officials. While the partisan staffing of the electoral management body does not lead inexorably to administrative disenfranchisement, Schaffer does suggest the dangers of both partisanship and excessive legal independence of EMBs, arguing that “[w]here electoral bodies are shielded from public scrutiny by a veil of constitutional independence...or where political parties are positioned to quietly manipulate electoral procedures..., the dangers of opacity and abuse increase” (89–90).

In a study of election administration practices that facilitate and impede voter participation in three Central American cases, Pallister (2013) finds that partisan EMBs have generated the administrative exclusion of some voters, although non-partisan election administration is neither a necessary nor sufficient condition for inclusive electoral procedures. He also finds that where non-partisan electoral management bodies do contribute to inclusiveness, they often do so not just by neutrally applying electoral rules but by developing bureaucratic capacity that partisan EMBs often lack and by lobbying for legal reforms. Evidence of this latter mechanism of EMB influence on electoral conduct provides empirical support for Elmendorf's (2006) suggestion that electoral commissions

may influence electoral reform through such channels as shifting public opinion towards their proposed reforms, constraining legislative actions, and influencing judicial review decisions of election legislation. This lobbying role of electoral management bodies has been largely overlooked in the literature, but given their expertise and bureaucratic authority, non-partisan EMBs are likely to be influential actors in processes of election law reform.

In sum, the comparative literature emphasizes the importance of election administration for overall election quality and potentially for democratic consolidation, and the evidence so far suggests tentatively that non-partisan electoral management bodies are associated with better quality elections than are partisan bodies. Yet the effects of EMB structure are only beginning to be studied systematically. It is unknown whether, and under what conditions, expert-based electoral management bodies are more likely to carry out particular types of election administration initiatives (such as lowering barriers to voting or adopting stringent safeguards against fraud), or whether expert-based EMBs foster more confidence in electoral integrity among citizens or elites.

Explanations of EMB choice

Far less is known about what leads countries to adopt particular EMB institutional arrangements, even though this question is important for evaluating the extent to which selection problems affect research on the effects of EMB institutional design, as well as for efforts to establish impartial EMBs. As Schaffer (2008: 165) notes, proposals for creating independent non-partisan electoral bodies face “the problem of getting from here to there. Where partisans control both the legislative agenda and the electoral administration, why should lawmakers agree to relinquish their party’s control over the electoral administration?”

Several hypotheses are raised throughout the literature. López-Pintor (2000) suggests that the trend towards independent rather than governmental EMBs is driven by mistrust between parties (63). Schaffer (2008) suggests that governing parties may delegate electoral authority to an independent body

if they fear that the legislative balance of power will soon change or that the electoral administration may fall into the hands of an opposing party. ...Fearing what might happen if the electoral administration were to be captured by their opponents, lawmakers might agree to “tie the hands” of future electoral administrators.

(Schaffer 2008: 165)

The electoral system and effective number of parties may also matter, with partisan EMBs perhaps more likely in two-party systems where the dominant parties will balance each other and non-partisan EMBs likely to be chosen in more fragmented party systems (Mozaffar and Schedler 2002: 17).

Other possible explanations of the adoption of non-partisan EMBs include the miscalculation of incumbents who believe that an independent EMB will not prevent fraud (or that the party will be victorious without fraud), and pressure from the opposition and civil society and the government’s attendant need for legitimacy (Hartlyn *et al.* 2008: 75; Schaffer 2008: 165). The latter applies to Mexico and Senegal in the 1990s, as “[r]uling parties in both countries acquiesced to opposition demands and transferred authority over electoral administration from the executive branch to an independent body—and soon after were voted out of power” (Schaffer 2008: 166).

Finally, the formal design of electoral management bodies may be influenced by international factors, such as the recommendations of election observers or other international actors.

If so, then EMB institutional design may be a pathway by which election observation influences election quality. Of course, countries may adopt a non-partisan EMB model merely as window dressing for observers, and nevertheless attempt to manipulate elections. Even in such cases, though, the establishment of a non-partisan EMB could have unintended consequences and lead to improved elections despite the intentions of incumbents.

Thus while the literature suggests several hypotheses, relatively few studies have empirically addressed the institutional development of electoral management bodies. Some work has focused on particular cases, such as the establishment of Mexico's independent Federal Electoral Institute (Eisenstadt 2004). Comparatively, Mozaffar (2002) takes a quantitative approach to studying EMB structure in Africa, finding that colonial legacies, postcolonial authoritarian regimes, ethno-political cleavages, and resulting political negotiations influence institutional design. Yet Mozaffar's analysis is limited to a relatively small number of cases, and focuses on the formal legal independence of electoral bodies rather than partisan autonomy. Understanding the question of why some countries adopt particular arrangements will be crucial to understanding whether and how changing the structure of EMBs can help bring about higher-quality elections.

Election observation and public perceptions of election quality

Like EMBs, impartial election observation may influence election quality, but through more complex mechanisms. A central goal of international and domestic non-partisan election observation (or citizen observation) is to provide an impartial assessment of election quality and, as a result, contribute to higher-quality elections. If citizens already trust their electoral process and democracy is widely perceived as the "only game in town" (Przeworski 1991), then election observation may not be as useful. But in countries in which elections are new, or which have a history of problematic elections, the impartial assessment provided by international and/or domestic election observers can be an important inducement for governments to hold more democratic elections, or a way in which cheating incumbents are recognized and sanctioned (Bjornlund 2004; Hyde 2011b; Hyde and Marinov 2014; Kelley 2012a).

Multiple audiences consume the reports from election observers, including citizens within the host country; the EMB and government of the host country; international audiences interested in democratization, often including foreign aid donors, investors, or international partners; and the governments of other countries undergoing similar transitions.

Since the early 1990s, scholars have developed a large and increasingly sophisticated literature on international election observation (Bjornlund 2004; Carothers 1997; Hyde 2007, 2011b; Kelley 2008, 2012a).¹⁵ Domestic election observation, or citizen observation, has attracted somewhat less scholarly attention (Lean 2007, 2013), but is widely viewed as complementary, and in some ways, better, than international observation (Bjornlund 2004). By 2006, about 80 percent of all national elections were formally observed by international groups, and roughly 70 percent were observed by domestic non-partisan groups.¹⁶

The literature on election observation suggests that observers have complex, and sometimes unintended, effects on election quality. To be clear, the vast majority of international election observation groups adhere to a strict policy of non-interference in the electoral process (Declaration 2005). Although one could argue that their mere presence is a form of "interference," in practice, this standard means that observers stick to observing the process from the sidelines, summarizing the findings of their observation, and making recommendations for improvements in the quality of the process. They tend to avoid direct actions, such as attempting to stop an incident of election fraud, which would give the appearance of intentional influence on the process.

That said, it is clear that international observers document fraud in more than a quarter of observed elections (1991–2006), and that international observer groups with a demonstrated willingness to criticize elections were present in about 40 percent of elections in the post-Cold War period.¹⁷ However, the effects of observation are difficult to summarize in part because some of their intended effects are long term, and the effects of observers on election quality may be evolving over time (Hyde 2011b). Observers have been associated with higher-quality elections (Kelley 2012a: ch. 7), can directly reduce election fraud on election day (Hyde 2007), and may make post-election protest more “accurate” and more likely to follow fraudulent elections (Daxecker 2012; Hyde and Marinov 2014; Little 2012; Svolik and Chernykh 2012).

Some scholars have highlighted the possibility that incumbent governments wishing to benefit from the legitimacy of democratic elections but avoid risking their hold on power attempt to stack the deck in their favor using methods less likely to provoke condemnation from election observers (Beaulieu and Hyde 2009; Hyde 2011b: ch. 5; Hyde and O’Mahony 2010; Schedler 2002; Simpser 2008; Simpser and Donno 2012).¹⁸ This line of inquiry is particularly relevant when debating the likely consequences of EMB reform. If incumbents grant *de jure* independence to an EMB, they could be doing so because they are sincere reformers or because they are working to create a veneer of legitimacy over what is still a corrupt electoral process. As Alvaraz *et al.* (2008: 12) note, “it can be difficult to distinguish between blatant attempts to manipulate the election and isolated anomalies, incidents, or irregularities that may be completely unintentional.” For EMBs, problems with elections can be intentionally created, but to evade the more serious criticism from election observers, can be made to look like incompetence, a lack of technical capacity, or may be blamed on a lack of resources.

A popular criticism of international election observation centers on the possibility that they will legitimize electoral autocrats, and relatedly, that their standards for democratic elections are not consistent over time or between countries (Geisler 1993; Kelley 2009, 2010). Some have argued that this false legitimization occurs if observers agree to observe an election of a regime that stands little chance of holding free and fair elections, regardless of whether observers ultimately criticize the elections. Others argue that so long as observers are willing to issue strongly worded condemnations of problematic elections, such concerns are overblown.

What is clear is that international and domestic observers are now present at nearly all elections that have pre-election concerns about fraud (a proxy for those elections in which they are most likely to be needed). Comparisons between observed and unobserved elections are difficult to interpret, in part because the selection process is non-random (Hyde 2007; Kelley 2012a). For an election to be observed, there must both be willing observers and a host-government willing to accredit them. This is particularly true for international observation. If observed elections are of higher-quality than unobserved elections, it is difficult to rule out the possibility that observers are simply permitted (and choose) to go to elections that would have been of higher-quality in the first place. But observers do not appear to be selecting elections that are already problem-free (Hyde 2011b; Hyde and Marinov 2014).

What is not yet clear is how the dynamics between host country governments, pseudo-democrats, and election observation groups play out over time. Many of the cross-national findings about the “effects” of election observation could be causally attributed to election observers or could be due to some other omitted variable such as increased international and domestic attention to the electoral process. Although these concepts can be analytically distinct, empirical work is much more difficult.

It is also increasingly clear that the effect of observers on election quality is unlikely to be revealed in the course of a single electoral cycle. The dynamics between EMBs, incumbent governments, election observers, and the audiences that react to the reports from observers are

complex, and may be part of what Schedler (2002) has called a “nested game of democratization by elections” (see also Beaulieu 2014 and Lindberg 2006, 2009). In a new treatment of this overtime dynamic, Beaulieu (2014: 3) highlights political protest surrounding elections as an important element, and argues that “where the incumbent can enact reforms to appease international actors without fear of certain electoral defeat, electoral protests can have positive consequences for democracy.”

Another potentially important but understudied point is that some of the effects of election observation may be demonstration effects: the effects of observers on the quality of elections in one country may serve as an example to other countries undergoing similar processes, with the potential for diffusion of both technology for higher-quality elections and techniques for evading pressure for democratizing reforms.

Directions for future research

The literature reviewed has contributed significantly to our understanding of the impacts of international election monitoring and the institutional design of electoral management bodies on electoral quality. We suggest several avenues for future research to further test these findings and expand the research focus to heretofore neglected aspects of electoral bodies.

Scholars studying election administration from a comparative perspective might consider four directions for future research. First, comparative studies of the effects of variations in EMB partisanship have so far focused largely on Latin America (Hartlyn *et al.* 2008; Pallister 2013; Rosas 2010). While researchers have undertaken individual case studies from other regions (e.g. McMillan 2012), our knowledge of the consequences of EMB institutional design would be expanded through further cross-regional research, either through comparative case studies or through more comprehensive data collection that extends existing quantitative studies.

Second, our understanding of the consequences of partisanship or expertise in electoral management bodies would be enhanced by greater understanding of the interests and worldviews of non-partisan election officials and of the internal politics of EMBs. Whereas partisan appointees are widely considered to act as party watchdogs whose behavior can be analyzed through a principal-agent framework, the literature is largely silent on the goals of neutral election administrators and the decision-making processes among electoral commissioners. Along these lines, Elmendorf (2006: 443) suggests the need for “case studies that seek to document the self-understanding and thought processes of election administrators and law-reform commissioners, as well as the debates that take place inside such organizations.” This is an area where fine-grained case studies or ethnographic work on electoral bodies may prove especially fruitful in shedding light inside the black box of EMBs to provide richer insights into how partisanship and autonomy influence election officials.

Third, the comparative literature on election administration might also be advanced by considering a largely neglected dimension of institutional design: the degree of EMB centralization. In some countries (e.g. Sweden, Switzerland, and the United States), a significant degree of authority over election administration is devolved to provincial or local election boards (International IDEA 2006: 17–19). The distribution of electoral authority between national and subnational bodies creates the potential for vested bureaucratic interests, as evident by the resistance of state and local election officials in the United States towards any efforts at centralizing election administration (Ewald 2009: 6; Hasen 2010: 1098). Such bureaucratic turf struggles have not been the focus of research, and may have important consequences for the quality of election administration and for attempts at electoral reform.

Fourth, future research might also focus on testing hypotheses about the conditions under which independent and non-partisan EMBs are likely to be established, the conditions that lead professional electoral bodies to become politicized, and when strategic manipulation involving the EMB is most likely to occur. While many cases evidence the resilience of EMB institutional independence over time (International IDEA 2012a: 9), other cases show that the professionalism and independence of EMBs can be undone by determined political interests (e.g. Pallister 2013: [ch. 6](#)). In general, we know little about the structural, strategic, and normative influences on the choice of EMB institutional structures and their subsequent evolution. Further research is needed on how political party systems, historical legacies, international influences, and other factors shape the construction and evolution of electoral management institutions and their effectiveness.

Further research is also needed on the international influences on domestic electoral institutions and conduct. While research on election observation has come to important findings, much of that work has focused on the short-term effects of observers in preventing fraud (though see Kelley 2012a: [ch. 8](#)). Less is known about the medium- to long-term effects of election observation, especially the influence of observers' recommendations for electoral reform. We know little, for example, about the extent to which recommendations for reform offered by different observer groups are consistent or contradictory, or the extent to which observer recommendations influence the decisions of domestic actors.

Beyond election observation are several channels of potential international influence on electoral conduct that have not been studied systematically, including regional and global networks of EMBs and electoral experts and international flows of technical and financial electoral assistance.¹⁹ Regional associations of EMBs began to form in the 1980s in Central and South America, and in the 1990s regional associations were formed in Central and Eastern Europe, Africa, Asia, the Caribbean, and the Pacific.²⁰ In 1999 the Global Electoral Organization (GEO) was convened with the goals of providing a forum for collaboration, professional networking, and agenda setting for electoral management bodies (International IDEA 2006: 282). Interwoven with these EMB associations are a variety of intergovernmental and international nongovernmental organizations that provide financial and technical assistance for elections, including support for the institutional development of electoral management bodies and for the conduct of all aspects of the electoral process (voter registration, voter education, transmission of election results, and so on).²¹

Despite the extensiveness of these international ties, little is known about their effects on domestic electoral institutions and practices. International technical assistance and regional networks of EMBs—and the associated conferences, seminars, publications, election management courses, and information clearinghouses—offer opportunities for election officials and experts to directly share experiences, offer policy advice, and devise sets of best practices. This presents an opportunity for scholars to study dynamics of socialization and norm diffusion and the influence of epistemic communities (Haas 1992): just as election observation has become a global norm, scholars might study whether transnational contacts have facilitated the articulation and diffusion of norms related to electoral administration (Slaughter 2005), or whether some countries attempt to comply with international expectations about independent election administration without complying with the spirit of these reforms. These “pseudo” reforms are one of the major challenges to understanding the effects of EMBs on electoral quality.

Overall, it is clear that election administration and election observation both have significant potential to influence the quality of elections. However, due to the potential for reforms to be hollow and changes to take place over multiple electoral cycles, future research will be necessary. Additionally, because some governments will always attempt to corrupt the electoral process, the

potential for backsliding to follow successful reform will continue. How can reforms in election administration be protected from corruption, strategic electoral manipulation, and partisan biases? Is election administration reform a leading trend that can cause significant improvements to election quality, or a trailing indicator of a country's likely democratization? Will election observation always be helpful, or can it become obsolete with sufficiently high quality election administration? These and other questions remain, and we look forward to future developments in this vibrant literature.

Notes

- 1 Authors' names are in alphabetical order.
- 2 See Boix and Svolik (2008), Gandhi and Przeworski (2009)–, Gandhi (2010), Gandhi and Lust-Okar (2009), Geddes (1999), Lust-Okar (2006), Magaloni (2006), Malesky and Schuler (2010), Schedler (2006), Simpser (2013), and Wright (2008).
- 3 Pastor (1999) noted that because public acceptance of elections as fair by all major political forces is an important part of democratization, election administration had been a missing variable in the study of democratization.
- 4 Other threats to the objective quality of elections include forms of election manipulation perpetrated by political parties or governments, such as gerrymandering, campaign finance abuses, limits on media access, and electoral repression.
- 5 Election quality includes dimensions such as the fairness of constituency boundaries, the quality of voter registration, equitable access to campaign finance and media, the fairness and efficiency of voting procedures on election day, the accuracy and transparency of the vote count, and so on. See Elklit and Reynolds (2005) and Norris, Frank, and Martínez i Coma (2013).
- 6 López-Pintor (2000: 12) also argues the point forcefully: “Historical evidence, coupled with conclusions by observers and advocacy by electoral professionals, almost unanimously indicates that independent electoral bodies serve democratic stability better than elections run by the executive branch.”
- 7 Two additional categories identified by López-Pintor (2000: 24) are highly decentralized election administration systems (such as the United States) and two or more independent bodies responsible for election administration. However, the former can often be considered a subtype of the governmental model, while the latter is a subtype of the independent model. Subsequent work has classified these systems as such (e.g., Birch 2008; International IDEA 2006).
- 8 To maintain comparable samples, data for 2006 and 2012 are limited to countries for which data was available from 2000 and/or 2004. Thirty-nine countries were coded differently between the 2004 and 2006 sources, compared with 24 countries coded differently between 2006 and 2012 and only five countries coded differently between 2000 and 2004. The large number of differences between the 2004 and 2006 sources suggests that different sources made slightly different coding decisions.
- 9 Such multiparty EMBs may include representatives of all political parties, “or a threshold may restrict representation – for example, to those parties represented in the legislature or with more than a specified proportion of members in the legislature” (International IDEA 2006: 88).
- 10 Those EMBs whose members are chosen by the judiciary are scored most autonomous, while EMBs are scored least autonomous where parties directly select election commissioners. Cases where parties play a role in selecting EMB members through confirmation in the legislature are scored as intermediate (Rosas 2010: 79–80).
- 11 In their coding scheme, “the least autonomous agencies are appointed by one chamber of the legislature only, with autonomy increasing as agencies are named through processes involving multiple branches of government, to processes dominated by the judiciary, to those dominated by civil society” (Hartlyn *et al.* 2008: 80). EMBs are also considered more autonomous when their members enjoy longer tenure than the legislative and executive officials who appoint them. Rosas (2010) employs this alternative measure in his study as well.
- 12 For instance, a nomination criterion of expertise specified by law may be undermined in practice by partisan actors who appoint the EMB, while at the same time EMB members named by partisan actors may not behave as party watchdogs. As one source notes, “In some countries, political party nominees to an EMB are eminent persons who are required to maintain high standards of impartiality and professionalism, and thus they do not serve as political party representatives on the EMB” (International IDEA 2006: 88).

- Estévez *et al.* (2008) also present evidence of party influence on roll call votes in Mexico's purportedly non-partisan EMB. Hartlyn *et al.* (2008) measure of partisan autonomy for EMBs in Latin America is attentive to this issue, as it relies not only on formal appointment processes but also on verification by country experts.
- 13 A recent policy document makes a similar point: "The formal model [of EMB structure] says very little about an EMB's actual independence. In fact, most nondemocratic regimes in today's world boast an Independent Model of electoral administration" (International IDEA 2012a: 9).
 - 14 In their statistical model, the effect is large: a multiparty EMB improves the odds of an acceptable election by 37 percentage points over a single-party EMB (from .48 to .85), while a fully independent EMB improves the odds of an acceptable election by another 13 percentage points (to .98) (Hartlyn *et al.* 84).
 - 15 At the same time, election observation practitioners have developed policy guides and shared standards of evaluating electoral quality (Davis-Roberts and Carroll 2010; Declaration 2005; Elklit and Reynolds 2005; European Commission 2007; European Union 2008; International IDEA 2002; OSCE-ODIHR 2005).
 - 16 Calculations used data collected by Hyde. Domestic observation is somewhat harder to document, and coders were unable to establish whether domestic non-partisan observers were present in about 3 percent of elections.
 - 17 Author's calculations using Hyde (2011b) data.
 - 18 Somewhat contradicting this possibility is Kelley's finding that many governments use multiple forms of manipulation simultaneously (2012a).
 - 19 Kelley (2012b) suggests similar avenues for future research.
 - 20 These regional associations are: the Association of Electoral Institutions of Central America (known as the Tikal Protocol) and the Association of South American Electoral Organizations (the Quito Protocol), which coordinate under the Inter-American Union of Electoral Organizations (UNIORE); the Association of Central and Eastern European Electoral Officials (ACEEEO); the Association of African Election Authorities (AAEA); the Association of Asian Election Authorities (AAEA); the Pacific Islands, Australia, and New Zealand Electoral Administrators Network (PIANZEA); and the Association of Caribbean Electoral Organizations (ACEO) (International IDEA 2006, 280–281).
 - 21 Organizations involved in such assistance include the International Foundation for Electoral Systems, the Organization of American States, the European Commission, the United Nations Development Program, and the United Nations Electoral Assistance Division.

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THE MILITARY'S ROLE IN POLITICS

Aurel Croissant and David Kuehn

Introduction

Civil-military relations are a central feature of political life in all nation-states that maintain military organizations. In most liberal democracies, the armed forces support their governments in responding to natural disasters, are involved in border security, and assist the police in dealing with organized crime and terrorism, in addition to their core function of defending the state against external security threats (Bruneau and Matei 2012: 2–3). Civil-military relations are also relevant for many new democracies, which often struggle with the double challenge of creating and preserving a military that is strong enough to fulfill its functions but that is subordinate to the authority of civilian political leaders and institutions (Feaver 1996). Finally, in authoritarian regimes, the armed forces are key components of the regime coalition (Barany 2012: 2) as they are ultimately the only state institution capable of defeating a mass-based, organized, and potentially violent opposition movement (Skocpol 1979).

Studying the military's role in politics is an immensely rich field that falls into a sociological and a political science strand. The former is concerned primarily with the military as a social organization and the social functions of military systems, as well as the ways in which these have changed over time, for instance as the result of broader social changes or the changing nature of warfare (Moskos *et al.* 2000; Caforio 2006). The political science strand of civil-military relations research, in contrast, focuses on the more narrow relationship between the military and the political system. This strand can be distinguished further into an American and a comparative literature. The former is concerned primarily with civil-military relations in the United States, and the way in which military institutions are embedded in American society; how Congress, the executive, and the military interact in the making of national security policies; and how military policy, or practices of civilian control, affect defense policies (Nielsen and Snider 2009).

The comparative literature has moved in different directions. Its initial preoccupation was the question of how to ensure political control of the armed forces while allowing for effective defense policies in Western democracies (Huntington 1957). In the 1960s, the scholarship moved towards analyzing the origins of military coup d'états and military regimes in developing countries, such that by the mid-1980s, a large body of literature on the causes of military intervention in politics had been generated. A second line of research investigated

party–military relations in communist regimes, which operated under societal and institutional circumstances that were quite different from those in liberal democracies and the non-socialist developing world. When the third wave of democratization began in 1974, it inspired a new generation of civil–military studies, which turned to questions of institutionalizing civilian control over the armed forces and security sector reform in the emerging democracies (Agüero 1995; Pion-Berlin 1997; Croissant *et al.* 2013). In the post-Cold War era, the analysis of civil–military relations in Western democracies regained prominence (Born *et al.* 2004). Finally, the investigation of political–military relations in non-democratic regimes experienced a renaissance in recent years.

Definitions and concepts

Two concepts are central to the analysis of the military's role in politics: civil–military relations and political control of the military.

The military, civilians, and civil–military relations

The standard definition for “military” refers to all segments of the state-organized and uniformed armed services that share three defining criteria: (1) they possess the monopoly over weapons of war; (2) their primary purpose is the defense of the nation-state and its citizens against external military threats; and (3) they are legalized and legitimized as instruments of the state (Edmonds 1988).

Based on this definition of military, the term “civilians” refers to all non-military social actors and organizations. In the context of research on the political role of the military, however, the term is used more narrowly and comprehends all organizations, institutions, and actors that formulate, implement, and monitor political decisions and substantive policies, that is, the state institutions of the executive, legislative, and judiciary branches of government, but also non-state political actors such as political parties, interest groups, social movements, and associations of civil society as well as certain international actors such as foreign governments, international financial institutions, and international NGOs.

Most generally, therefore, the term “civil–military relations” encompasses “the entire range of relationships between the military and civilian society at every level” (Feaver 1999: 211). Yet most political scientists more narrowly focus on political–military relations, that is, the structures, processes, and outcomes of the interactions between the political system and its actors on the one hand, and the military on the other. This focus has been criticized by scholars who argued that it is no longer able to capture the more complex realities in most societies after the Cold War era and should be replaced by broader analytical lenses such as “security sector” (Hänggi 2003). This includes all security related state-organizations and agencies, such as the police, paramilitaries, the intelligence services, the judicial and penal system, and other ministries and state bureaucracies involved in the formulation or implementation of security-related policies, as well as non-state organizations such as neighbourhood watches, private security companies, rebel groups, and warlords (Edmunds 2012).

Political control of the military

Political scientists who study civil–military relations have always been particularly concerned with the “civil–military problematique”: how to create and preserve a military that is strong enough to fulfill its functions but that is subordinate to the authority of a regime's political

leaders (Feaver 1996: 149). At the core of the problematique, therefore, is the notion of political control over the military.

There is no agreement among scholars on what “political control over the military” entails and how it should be measured. In the early literature, it often had been equated with the absence of military coups. Yet this assumption is flawed since the absence of coups might actually indicate that the military enjoys a high degree of political influence vis-à-vis political authorities that makes coups unnecessary (Feaver 1999). Furthermore, the “fallacy of coup-ism” (Croissant *et al.* 2010) distorts the conceptual continuum of political–military relations, defining the most extreme event as the conceptual benchmark while ignoring other, more discreet, methods by which militaries exercise political power.

In recent years, scholars have advanced gradualist conceptions of political control over the military that share two fundamental assumptions. First, the degree of political control depends on *who* has the authority to make political decisions over a range of political matters—the government or military elites. Political control exists when political leaders have authority over decisions concerning all relevant policy matters, can delegate the implementation of state policies to the military and repeal this delegation, and can effectively oversee the implementation of those decisions that have been delegated to the military (Agüero 1995: 19–21; Trinkunas 2005). Second, and related to that, political–military relations can best be understood as a continuum ranging from full civilian control to complete military dominance over the political system (Welch 1976: 1–3). Consequently, the degree of political control can be gauged by identifying military challenges to the authority of political leaders, which can take two analytically distinct shapes: institutionalized prerogatives, which include formal rights that allow the military “to structure relationships between the state and political or social society” (Stepan 1988: 93); and contestation, that is, informal military non-compliance with decisions made by the political leadership.

While political control of the military is the general term, “civilian control” and “democratic control” over the military are sub-types of political control. In the most minimal sense, civilian control of the military means that political authorities and organizations that serve in government and exercise authority and oversight over the military are not officers in uniform. Democratic control requires that these authorities and organizations must themselves be subject to the democratic process.

Of course, political control is not the only relevant issue in political–military relations. Bruneau and Matei (2008), for instance, have convincingly argued that the military’s ability to achieve the roles and missions assigned to it by political leaders (“effectiveness”) at an acceptable cost in lives and resources (“efficiency”), is of fundamental importance for national security and the legitimacy of both the political order and the military institution (Edmonds 1988: 96; Feaver 2003: 5). Therefore, an exclusive focus on issues of political control must fall short of a complete understanding of civil–military relations as defined previously. Moreover, the idea of political control does not assume an apolitical military. As any other organization, the military has organizational needs and interests, some of which may be legitimate while others may not, and it has a responsibility to advise policymakers on matters of national security. In fact, taking military expertise into account is crucial for effective and efficient defense policies. The question for political control is therefore not whether the military yields political influence, but how and how much (Welch 1976: 1).

The military in authoritarian politics

The scholarship on the military’s role in authoritarian politics is relatively underdeveloped. Much has been written about the causes of military coups and military rule. Overall, however,

military regimes have been studied “more in terms of processes of transition to and from democracy than as a political system in its own right” (Remmer 1989: 23). While there is a rich literature about party–military relations in pre-1989 communist regimes, little attention has been paid to the ways in which contemporary civilian dictatorships organize their political–military relations. This is somewhat surprising as authoritarian regimes lack most of the institutionalized feedback mechanisms that enable the peaceful settlement of social conflicts in democracies and, therefore, autocracies develop strong military and non-military security apparatuses. A strong military is a double-edged sword for autocrats, though: A more powerful military is more effective in repressing political conflicts, but at the same time it is in a better position to demand political and economic concessions in exchange for its role in maintaining the regime (Acemoglu and Robinson 2006: 219).

Studying military coup d'états

One way to analyze the military's role in authoritarian politics is to focus on the issue of coups. Even though military coups occur in all kinds of political regimes, authoritarian regimes, especially military-ruled ones, are particularly prone to this kind of military intervention in politics. The essence of a military coup is the rapid and illegal seizure of government by a group of military officers (Quinlivan 1999: 132). Yet, military coups differ in regard to goals and methods. First, coups can be categorized as “breakthrough,” “guardian,” and “veto” coups (Huntington 1968): the “guardian coup” aims to protect the political status quo against intra-elite conflicts or deepening tensions between vested social interests and newly emerging social groups; “breakthrough coups” are undertaken by military officers who intend to create a new social order; and “veto coups” are usually launched on behalf of the middle and upper classes in order to suppress a mass uprising. Second, there are two basic coup modes, the corporate military coup and the factional coup (Finer 1962). While the latter is staged by a military faction and is often led by mid-level or junior officers, a “corporate coup” is typically conducted by senior military leaders and supported by the military institution. As Paul Brooker observes, the factional coup is “a sign of weakness in a key and distinctive aspect of the military's capacity to seize power—its highly centralised and disciplined organisational structure,” and, thus runs a greater risk that it will fail (Brooker 2009: 200).

As shown in [Figure 18.1](#), military coups have been a common occurrence in the twentieth century. Based on a revised dataset by Powell and Thyne (2011), we count 411 military coups, 211 of which succeeded and 200 failed, for the period between 1950 and 2010, most of which took place between 1950 and 1980. Since the 1980s, however, the number of military coups has declined significantly and successful coups especially have become less frequent.

The literature on the causes of military coups is extensive, but most approaches can be separated into two categories. The first identifies the origins of military interventions in the military's corporate interests (Nordlinger 1977), its professionalization or lack thereof (Huntington 1957; Stepan 1971), or in the operational aspects of military interventions (Luttwak 1968). These “push” elements provide the military the “means and motives” (Finer 1962) for political intervention. The second perspective emphasizes the “opportunity” (ibid.) for the military to intervene into politics, and stresses political causes such as political and institutional structures or social conflicts, which “pull” the military into a “politically pivotal role” (Svolik 2010: 7).

In this research, however, the empirical evidence often has remained inconclusive, and many findings have been the subject of considerable controversy. In recent years, scholars have therefore begun to link the study of background conditions of coups with the analysis of the

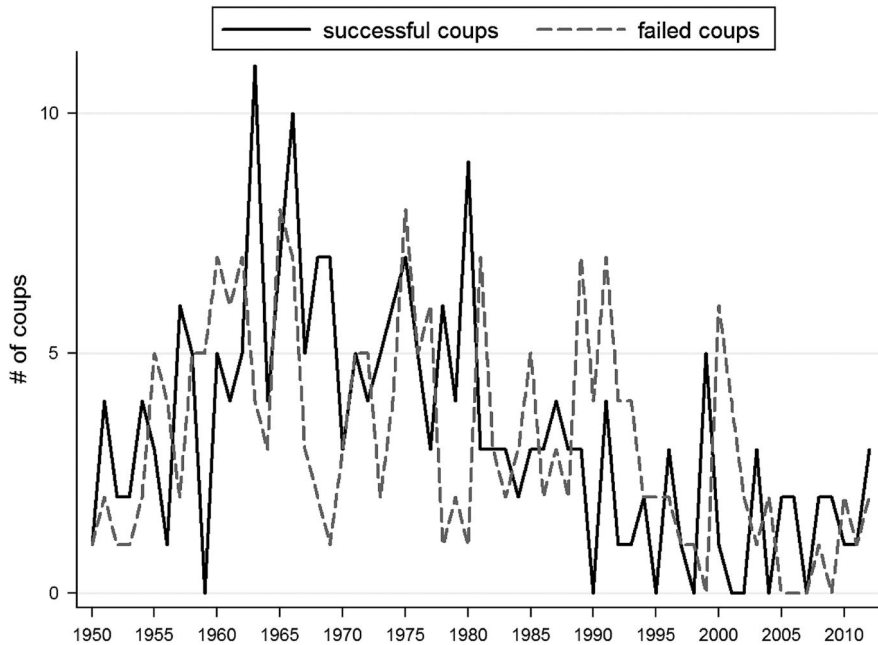


Figure 18.1 The number of military coup d'états, 1950–2010

Note: Powell and Thyne (2011) characterize a coup attempt as successful if the perpetrators seize and hold power for at least seven days. Unsuccessful coups must involve a recorded and recognizable physical and illegal attempt to unseat a government.

political strategies with which political leaders attempt to stabilize their rule against the threat of a military coup. Belkin and Schofer (2003), for instance, point out that structural variables, such as the degree of regime legitimacy, or the strength of civil society, do not determine the outcome of political–military relations but rather define the structural “coup risk” of a regime. Yet coups may be rare in both “high-” and “low coup risk regimes,” and regimes at similarly high risk-levels may vary widely in regard to the frequency of coups. Belkin and Schofer (2003: 597) suggest that this reflects the ability of political leaders to use “coup-proofing strategies” to protect their regime. Next to the attempt to “counterbalance” the military’s political power by playing military-internal factions or multiple security agencies against each other (Quinlivan 1999), political leaders in authoritarian regimes employ a wide variety of strategies for preventing military elites from seizing political power (Powell 2012b). Recent research has shown that the concrete set of coup-proofing strategies varies across regime types, with personalist regimes being more likely to use counterbalancing and patronage, whereas political leaders in one-party regimes tend to use institutional or psychological means such as monitoring, sanctioning, or political education (Pilster and Böhmelt 2011). All this suggests that a complete theory of military coups is likely to be much more complex and harder to verify empirically than it seems at first glance.

Studying military regimes

As Huntington (1968: 144) notes, the problem for the military is not to seize power but to organize and keep it. After a successful coup d'état, coup leaders face the problem of what type

of political regime to establish and how to consolidate their rule. By definition, all military regimes share the characteristic that “a group of officers decides who will rule and exercise some degree of control over policy” (Geddes 1999: 121). However, there are a number of institutional differences between military regimes that affect policy outcomes and the institutional evolution of military rule (Falleti 2011: 137). Furthermore, military governments often progress through different stages of institutional development as their tenure lengthens, and there is a tendency among coup leaders to concentrate personal political power (ibid.). Moreover, Magaloni and Kricheli (2010: 132) show that “33.33% of dominant-party regimes and 23.33% of the single-party regimes established during 1950–2006 emerged out of military dictatorships.”

Tracing the development of military rule in the post-World War II period depends, therefore, heavily on the classification scheme. Nonetheless, the three most comprehensive recent datasets on authoritarian regimes (Cheibub *et al.* 2010; Geddes *et al.* 2012; Wahman *et al.* 2013) agree that the period from 1962 to 1978 marks the heyday of military rule and that their number begins to decline in the early 1980s (see Figure 18.2). Similarly, despite differences in the concrete distribution of cases, all three datasets agree that military regimes were mainly clustered in Latin America, sub-Saharan Africa, and the Middle East (Croissant 2013).

In addition, the three data-sets agree that, on average, military regimes tend to be short-lived relative to other forms of authoritarian rule (cf. Table 18.1). However, the average life span across regime types conceals considerable variance within the individual categories. While it is true that 45 percent of military regimes break down within 5 years, and 68 percent within a decade (Geddes *et al.* 2012), there are a considerable number of military regimes such as Myanmar, South Korea, Brazil, Rwanda, and Indonesia that survive for 20 years or more. In addition, there are important cross-regional differences: overall, military rule in Latin America exhibited a significantly lower durability than in Asia, the Middle East, and sub-Saharan Africa (Croissant 2013).

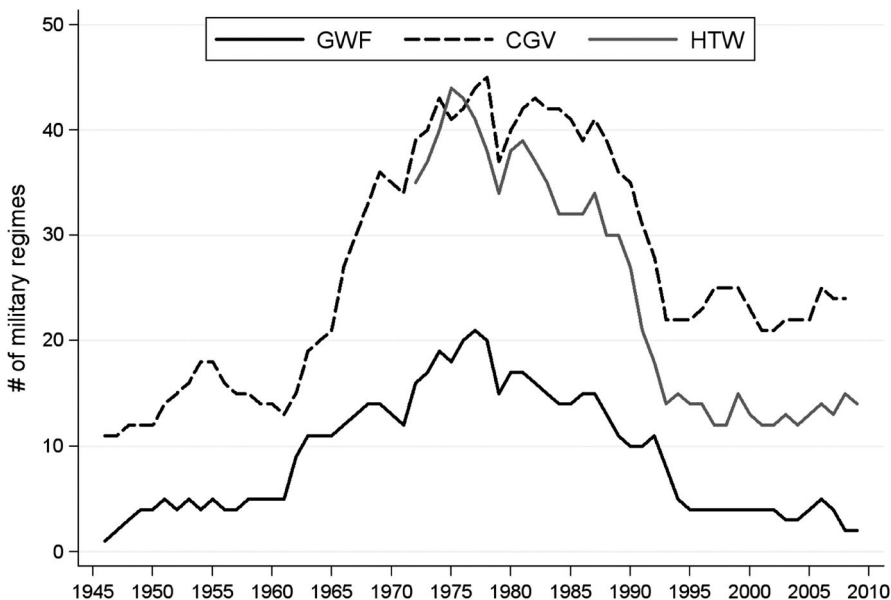


Figure 18.2 The number of military regimes, 1946–2008

Note: GWF: Geddes *et al.* 2012; CGV: Cheibub *et al.* 2010; HTW: Wahman *et al.* 2013.

Table 18.1 Average lifespan of political regimes and coup frequency (1946–2008)

<i>Dataset</i>	<i>Regime type</i>	<i>N</i>	<i>Mean duration (in years)</i>	<i># coups</i>	<i># failed coups</i>	<i># of successful coups</i>	<i>Mean # coup per regime</i>
CGV	Parliamentary democracy	83	26,4	32	11	21	0,39
	Semi-presidential democracy	40	17,2	11	5	6	0,28
	Presidential democracy	73	15,3	63	34	29	0,86
	Cvilian dictatorship	145	18,3	101	42	59	0,70
	Military dictatorship	137	12,4	164	88	76	1,20
	Royal dictatorship	26	29,9	15	7	8	0,58
GWF	Democracy	165	19,8	90	40	50	0,55
	Military	78	7,4	105	54	51	1,35
	Monarchy	19	31,3	15	7	8	0,79
	Party	82	27,6	62	25	37	0,76
	Personal	101	11,4	97	52	45	0,96
HTW	Democracy	164	17,3	27	12	15	0,16
	Military	97	9,9	100	62	38	1,03
	Monarchy	18	26,7	2	0	2	0,11
	Multi-party	154	8,5	50	23	27	0,32
	No-party	2	18	0	0	0	0
	One-party	56	14,7	18	4	14	0,32

Note: Other regime types in GWF and HTW excluded; regimes coded since 1946 for CGV and GWF; but coups since 1950.

Finally, there are considerable differences between sub-types of military rule: the mean durability of personalized military dictatorships is twice as long (11.4 years) as for direct and indirect military regimes (5.9 and 6.1 years, respectively). Traditional, junta-led military regimes survive for 5.3 years on average, which is significantly lower than military regimes with multi-party elections (6.7 years), without political parties (9.3 years), or one-party military regimes (11.9 years) (*ibid.*).

The reasons for this are many and probably not systematically known. One view argues that the short life span of military dictatorships results from the fact that military regimes face a much greater risk of being toppled by another military coup than any other regime type (see Table 18.1), because most members of the elite coalition in military dictatorship have control over at least some parts of the security forces, which reduces the coordination costs for coup-plotters (Frantz and Ezrow 2011: 23–24). Other scholars point to the initial deficit in legitimacy of military regimes and their difficulties in creating loyalty or support among the broader populace. For example, Samuel Finer (1985) argues that the major hurdle coup-plotters confront after seizing power is how to legitimize the usurpation of power. However, military dictators “rarely elaborate full-blown regime ideologies to justify long-term authoritarian rule.” As a result, “they tend to enjoy less of a cushion of ideological legitimacy to help them weather [...] tough times” (Lai and Slater 2006: 116–117). Moreover, most military regimes have weak roots in society, which means they find it hard to control or withstand popular protest.

This, finally, is also the case because military regimes are constantly challenged by the inherent conflict of interest between the regime leaders’ preference for staying in power and the military’s institutional demand for autonomy and the maintenance of internal cohesion and hierarchy (Geddes 1999). Military intervention into politics creates military-internal fissures, such that

“[m]ilitary regimes thus contain the seeds of their own destruction” (Geddes 1999: 131). Confronted with mass protests, military leaders usually prefer returning to the barracks before risking the breakdown of the military institution. Consequently, the shorter life expectancy of military regimes correlates with a higher likelihood of democratic transitions if compared to other non-democratic regimes (Ulfelder 2005).

Another major shortcoming in the literature is the lack of inter-regional, quantitative and qualitative-comparative research on policy and policy outcomes under military rule. There is an important but small, and somewhat outdated, literature on the policy consequences of military rule, mostly in Latin America (Stepan 1988; Remmer 1989; Biglaiser 2002). Its findings do not lend much support to the old argument, brought forward in the modernization literature of the 1960s, that the military as the most advanced, unified, and “modern” institution in most of the developing countries, is a force of societal modernization and economic development (Huntington 1968). While it may be true that the military as an institution is in a better situation to initiate, accelerate, and monitor processes of socio-economic modernization, it seldom does so. There is anecdotal evidence from countries where the military forcefully initiated successful modernization from above, i.e., South Korea, Chile, Brazil, Turkey, and Ghana under military-president John Rawlings in the 1970s and 1980s (Straßner 2013). However, more often than not, military regimes are conservative and status quo-oriented and, hence, they lack the political motivation to introduce large-scale modernization programs (Janowitz 1964). Or they have the motivation to do so, but as “experts in the management of violence” they lack the skills and know-how to successfully manage economy and society (Nun 1967). Often, the circumstances which favour military rule hinder economic development and modernization (Remmer 1989). Finally, as Wintrobe explains in his *Political Economy of Dictatorship* (1998), military organizations are budget maximizers. After the military come into power, their objective is to raise the military budget, and the salaries of military personnel. Therefore, it is difficult for them to find solutions to the economic problems of their societies (cf. Bowman 2002).

Yet, in recent years there has been an emerging scholarly literature investigating the output and outcome performance of different types of dictatorships in terms of social performance, ecological sustainability, the protection of property rights, the provision of domestic security, the management of internal conflict and foreign policy performance, the effectiveness of anti-corruption policies, etc. (cf. Croissant and Wurster 2013). While this literature is not specifically interested in military rule, it helps to understand how military regimes perform relative to other forms of non-democratic rule and relative to democracies. However, the causes for the variance among military regimes and if there are different patterns among the various forms of military regime, remain unclear (see also Falleti 2011).

Studying political-military relations in civilian-led authoritarian regimes

Political-military relations are also relevant for regime security and leadership survival in civilian-led autocracies, as evidenced by the recent regime crises in the Middle East and North Africa. Nonetheless, political-military relations in civilian-led dictatorships—either personalist, royal, or one-party—remain understudied. This is especially true for those post-communist countries that remained or reversed to authoritarian rule after 1990 (except Russia), and authoritarian regimes in Africa and in the Arab world (Barak and David 2010; Basedau and Elischer 2013).

In comparison to these regimes, political-military relations in communist one-party regimes before 1990 are well studied. Scholars have attempted to capture party-military relations and communist rule in a number of different analytical models. Huntington’s model of

“subjective control,” for instance, emphasized the ideological penetration of the armed forces and the installation of monitoring and oversight devices within the military institution (Huntington 1957: 81–84). Kolkowicz (1967) argued that communist parties ensured the military’s strict subordination through powerful and effective instruments of control, especially the political commissariat. Colton (1979) criticized this view and argued that political-military relations in the Soviet Union were characterized by a “symbiosis” of military and party elites, and the military’s loyalty was mainly guaranteed by the co-optation of military leaders into the party leadership. Similarly, Odom (1978) stressed that party elites and military elites cooperated because their specific interests converged towards the same national goals. As Perlmutter and LeoGrande (1982) have highlighted, however, these different models must not be understood as mutually exclusive, but rather capture different aspects of party-military relations in the Soviet Union at different periods of time.

The insights that can be gleaned from this body of research for the military’s political role in other types of non-democratic regimes are, however, limited. Political-military relations in “electoral authoritarian regimes” (Schedler 2006) in post-Soviet countries and in sub-Saharan Africa, or in the autocracies of the Middle East, differ starkly from those of the pre-1990 communist one-party regimes. While all these countries maintain militaries that are capable of executing coups, the declining numbers of coups and military regimes suggest the consolidation of political-military relations in these countries. This is particularly obvious for the Middle East and North Africa, where military coups or direct military rule have disappeared almost completely since the early 1980s. The military’s formal retreat from the centers of power did not, however, end their political influence. Rather, military officers now pursued their political interests through informal channels and personal contacts into the political leadership (Kamrava 2000; Cook 2007). Some leaders, such as Tunisia’s Ben Ali, or Libya’s Muammar al-Ghaddafi succeeded in neutralizing the political power of the military. In general, however, post-1980 political-military relations in the Arab world saw a high degree of stability and intra-elite cohesion, which is often evoked to explain the seeming immunity of the Near and Middle East from the ripples of the third wave of democratization (Bellin 2012).

While there are considerable differences between the individual cases, recent research has highlighted the importance of effective coup-proofing mechanisms for regime persistence in the region (ibid.; Quinlivan 1999). The rebellions of 2010/2011 suggest, however, that not all these coup-proofing instruments were equally successful. For example, as a relatively autonomous institution, the military played an essential role in the breakdown of authoritarian rule in Tunisia and Egypt (Droz-Vincent 2014). In Syria and Bahrain, however, military elites stayed loyal to the regime, whereas in Libya and Yemen the military institution fractured. These varying outcomes can be explained, *inter alia*, by the different strategies the political regime elites employed to control their armed forces. On the one hand, anecdotal and systematic evidence suggests that counterbalancing, monitoring, and the selection of officers along sectarian lines, as in Bahrain and Syria, have been effective instruments to secure the military’s loyalty despite anti-regime mass mobilizations. On the other hand, maintaining policies to protect military institutional autonomy, prerogatives, or economic benefits (e.g. in Egypt), or to marginalize the armed forces and discriminate them from the non-military security forces (e.g., Tunisia) had negative, unexpected consequences for regime survival as it increased the autonomy of the military institution from the regime and created incentives for military leaders to defect from the regime (Frisch 2013).

Furthermore, recent research finds that coup-proofing, especially ethnic purges and counterbalancing, is indeed an effective strategy for preventing successful coups, but also produces other, unintended, consequences that put at risk the political survival of dictators. For example,

Philip Roessler's (2011) analysis of coup-proofing and civil war in sub-Saharan Africa demonstrates that ethnic exclusion reduces the probability for military coups from within the regime coalition, but raises the risk of ethnic insurgency and civil war. Similarly, Jonathan Powell finds that coup-proofing reduces a dictator's ability to initiate diversionary tactics and inter-state disputes, and at the same time increases the probability for authoritarian leaders to face an insurgency, at least when high levels of counter balancing coincide with what Belkin and Schofer identify as high levels of coup risk (Powell 2012a). This is true for all forms of authoritarian regimes but especially for civilian-led dictatorships because military regimes are less willing to employ tactics of structural coup-proofing or ethnic purges compared to non-military regimes, as coup-proofing by definition has an eroding effect on the cohesion of the military institution.

The military's political role in new and established democracies

The "civil-military problematique" is also crucially important for the inner workings of democratic regimes (Croissant *et al.* 2010; Barany 2012). Consequently, there is a large body of literature on the civil-military relations in democracies, which falls into two major strands: the study of the emergence of civilian control in new democracies, and research on civil-military relations in established democracies.

Studying the military's political role in new democracies

Regarding the mode and outcomes of transition from military rule, Talukder Maniruzzaman (Maniruzzaman 1987: 22–23) observed that from 1948 to 1984, 26 out of the 71 total cases in his sample took place by means of planned elections held under the auspices of the outgoing military regimes, whereas another 36 cases occurred when the military was forced to relinquish power due to internal quarrels, social revolution, or mass uprisings. In contrast to his findings, analyses of the many transitions from military rule during the third wave of democratization since 1974 suggest that military governments are more likely than other forms of dictatorships to negotiate orderly transitions (Agüero 1998). Moreover, until the 1980s, the military's disengagement from politics was often only temporary (Welch 1992: 325). With the onset of the third wave, however, military withdrawals became more permanent, especially in Latin America and Southern Europe (Agüero 1998). Despite their retreat to the barracks, however, military leaders often successfully carved out political niches within their new political orders, a process that Samuel Valenzuela (1992) called the "perverse institutionalization" of reserved domains of policymaking and the military's assertion as a tutelary power.

In explaining these developments, Felipe Agüero (Agüero 1998) emphasized the military's political power during the transition to democracy: The stronger the military's influence, the better able it was to maintain its prerogatives and stifle post-authoritarian reforms. Of the third wave democracies, only a small number of military regimes (e.g. Greece and Uruguay) were so thoroughly weakened that they were unable to carve out significant privileges during the transition. Not only in Latin America, where 13 out of 16 transitions took place from military-dominated regimes from 1979 to 2000, but also in Asia military leaders were often able to assert political and institutional niches (Croissant and Kuehn 2011: 181). In this, military leaders were better able to safeguard their prerogatives the longer the regime had existed, the deeper the military's rule was institutionalized, and the greater was the military's internal cohesion.

Recent research on civil-military relations in new democracies, and especially former military regimes, seems contradictory, however. On first sight, the successes in establishing civilian

Table 18.2 Civil-military relations in 42 third wave democracies (as of 2010)

	<i>Military dominance</i>	<i>Military tutelage</i>	<i>Limited military subordination</i>	<i>Civilian control</i>
Southern Europe				<i>Greece, Portugal, Spain,</i>
Eastern Europe			<i>Bosnia and Herzegovina, Macedonia, Montenegro, Russia, Serbia</i>	<i>Albania, Bulgaria, Estonia, Croatia, Latvia, Lithuania, Poland, Romania, Slovakia, Slovenia, Czech Republic, Hungary,</i>
Latin America		<i>Ecuador, El Salvador, Guatemala, Honduras</i>	<i>Bolivia, Brazil, Chile, Dominican Republic, Nicaragua, Paraguay, Peru</i>	<i>Argentina, Mexico, Uruguay</i>
Asia	<i>Thailand, Pakistan</i>	<i>Philippines, Bangladesh</i>	<i>Indonesia, Timor Leste</i>	<i>South Korea, Taiwan</i>

Note: Countries in italics are former military regimes.

Source: Croissant and Kuehn (Croissant and Kuehn 2011: 225); (Croissant *et al.* 2013: fig. 1.2)

and democratic rule during and after the transition to democracy are astonishing. While democracies that had developed out of military rule had been particularly prone to authoritarian reversal and renewed military intervention during most of the twentieth century (Svolik 2012), in the last three decades successful military coups have become rare in new democracies. This is a remarkable finding, which suggests a new-found stability in civil-military relations in many formerly coup-prone countries of Latin America (Pion-Berlin and Trinkunas 2010), and Asia (Croissant *et al.* 2013). But the picture is much more complex if one employs a more comprehensive and fine-grained conceptualization of civil-military relations than the coup d'état. Building on proposals by Peter Smith (2005) and Alan Siaroff (2009), we can classify new democracies along a continuum of four degrees of civilian control: military dominance, military tutelage, limited military subordination, and civilian control (Table 18.2).

In 20 out of the 42 cases depicted in [Table 18.2](#) civilian control was successfully established, such that today the armed forces are subordinate to the democratically elected civilian elites and act according to democratic norms and procedures. This cluster includes Greece, Spain, and Portugal as well as most democracies in East Central Europe, but only four formerly military-dominated regimes. A total of 14 cases, amongst them six former military dictatorships, have made substantial success in strengthening civilian control but effective oversight over the armed forces remains tenuous and the military continues to enjoy significant degrees of institutional autonomy and political influence in political arenas such as internal security. Within this group there is considerable variation, with Chile having made great strides toward civilian control despite highly unfavorable initial conditions, while other cases, such as Bolivia, Peru, or Indonesia, still have a long way to go. The remaining two clusters include a total of eight states, seven of which are former military regimes, where the institutionalization of civilian control has failed and the military remains a crucial political player that continues to be involved in the making and breaking of governments (e.g. Honduras, Bangladesh) or has even toppled the democratic regime (Thailand, Pakistan).

One major factor to explain these differences are the initial conditions the new democratic regimes were faced with during and after the transition to democracy. In contrast to most cases

in Latin America and East Asia, the civilian elites in Eastern Europe and the former Soviet Union were not only confronted with militaries whose political prerogatives were much less pronounced, they also had at their disposal a well-established set of effective instruments to control the military. Therefore, the military's coup potential was all but irrelevant and in 1989/1990 the militaries did not interfere with the institutionalization of democracy (Barany 1997). A second aspect that helped in establishing civilian control over the military in Eastern Europe was the civilian democratic elites' interest in joining NATO and the EU, which provided a powerful incentive to reform civil-military relations that was absent in other regions (Croissant and Kuehn 2011: 232). With the exception of Yugoslavia the peaceful nature of political transition further facilitated civilian control because civilians did not have to rely on the military in order to establish the new politico-economic order. Finally, the civilian institutions continued to enjoy high degrees of legitimacy throughout the transition process, such that the military could not have justified an autonomous political role for itself (Mares 1998).

In Latin America, the end of the anti-regime wars in Central America and the decline of the revolutionary movements in the Cono Sur have enabled the military's withdrawal from politics as much as the post-1980 changes in Washington's policy towards the region. In addition, the often conflictuous relationship between the President and Congress, which in the past had triggered military coups in many Latin American countries, today are settled either within constitutionally sanctioned procedures, through cross-party negotiations, or by public mass mobilizations without military involvement (Pérez-Liñan 2007). Finally, the low (but recently increasing) trust of most Latin American citizens into their armed forces has strengthened the position of civilian elites vis-à-vis the military (Hagopian and Mainwaring 2005).

In Asia, on the other hand, the situation is less clear-cut. In most countries in the region, the military continues to be the most highly trusted public institution, while the weakness of democratic institutions, political parties, and civil society lead to repeated cycles of political violence. In countries such as Bangladesh, Thailand, the Philippines, and Pakistan, this allows military officers to assume the position of power broker and expand their political influence while hampering the strengthening of civilian control (Barracca 2007; Croissant *et al.* 2013).

Overall, the recent scholarship suggests three important conclusions concerning the reasons for the success or failure of civil-military reforms in new democracies. The first is that the acceptance of the democratic institutions by the political elites and the mass public is a necessary, though not sufficient, condition for successful civil-military reforms. Where democratic norms and procedures are supported by broad segments of the citizenry, and where civilian elites are unified in the goal of keeping the military out of politics, the institutionalization of civilian control is possible. Where there is no agreement between political parties to not seek the military's support for their respective stances, it is extremely difficult to implement reforms for civilian control over the military (Serra 2010: 239).

Second, contrary to earlier traditions of civil-military relations theory (e.g. Huntington 1957, 1995), the existence of pro-democratic norms and values *within* the military seems not to be a necessary condition for the establishment of civilian control in new democracies. While there is wide agreement in the literature that in the long run stable democratic civil-military relations can only be guaranteed if the military accepts the norm of civilian supremacy (Cottey *et al.* 2002; Taylor 2003), in new democracies such intra-military normative consensus on the subordination to civilian leadership still has to develop (Barany 2012). In the short to medium term, the military's acceptance of civilian control in new democracies is the result of rational calculations of the military leadership and depends on the establishment of effective institutions such as formal rules, regulations and bureaucratic agencies of civilian authority, and oversight in all relevant policy matters including defense and military policy (Agüero 1995; Hunter 1998; Feaver 2003;

Croissant *et al.* These institutions reduce the military's propensity for "shirking" by reducing the civil-military information asymmetry and raising the costs for military insubordination. The military's self-interest and the civilians' ability to reward loyalty to the democratic regime and punish insubordination also explain the military's behavior during the transition and in the civilians' attempt to make use of, establish, or strengthen these institutions of civilian control: This will only occur if civilians are able "to co-opt, recruit, or intimidate a sufficiently large number of military officers into supporting the government's agenda so as to prevent the armed forces from acting cohesively to oppose civilian control in a new democracy" (Trinkunas 2005: 10).

Third, the differences between countries such as Indonesia or Chile, which despite unfavorable initial conditions have made significant if incomplete advances toward robust civilian control, suggest that the development of civil-military relations in new democracies is not determined by the initial conditions during and after the transition from authoritarian rule. Democratic transitions are rather characterized by "structured contingency" (Schmitter and Karl 1991), which allows for political entrepreneurship. The political will and ability of civilian decision-makers to strategically engage the military are, therefore, crucial for their chances to challenge the military's political prerogatives (Trinkunas 2005). This has important implications for theory-building in the field of civil-military relations research, which we will address in the conclusion of this chapter.

Studying civil-military relations in established democracies

The study of civil-military relations in established democracies such as the United States and in Western Europe is not so much concerned with the institutionalization of effective mechanisms of civilian control. Instead, it is premised that in these regimes the hierarchical relationship between the civilian "principal" and the military "agent" (Feaver 2003) is normatively and factually accepted by both sides of the civil-military equation. The main questions of interest in this body of research are how civil-military relations play out within the confines of established institutions of civilian control, and what explains civil-military conflict and cooperation in established democracies. As noted, the literature on the military in established democracies is strongly skewed. Most studies focus on civil-military relations in the United States. Moreover, the literature on civil-military relations beyond North America tends to be weaker in terms of theoretical reflection and innovation, and is often more strongly policy-oriented than its American counterpart (Nelson 2002). Consequently, we will focus our attention on literature that is concerned primarily with civil-military relations in the United States in the following.

For more than four decades, research on American civil-military relations was overshadowed by Huntington's seminal work, *The Soldier and the State* (Huntington 1957), in which he proposed the "objective" model of civilian control, which clearly separates civilian and military spheres of responsibility, and maximizes military professionalism. Soon after its publication, Huntington's theory had become the analytical and ideological point of reference for both researchers and practitioners of civil-military relations (Feaver and Seeler 2009), despite serious criticisms of its underlying assumptions, logic of argumentation, and policy conclusions (e.g. Bruneau 2012).

It was only in the 1990s that the Huntingtonian paradigm of military professionalism and "objective control" of the military was successfully challenged by a new and innovative "wave" of research that combined more robust empirical evidence against his theorem with alternative analytical approaches and theoretical models. Next to the ongoing dissatisfaction with the

explanatory power of Huntington's theorem, the renewed interest in U.S. civil-military relations was motivated by real-world events, most notably the end of the Cold War and the resulting re-calibration of the U.S. military's goals and missions, the ongoing "revolution in military affairs," and particularly the—at least perceived—"crisis of civil-military relations" (Kohn 1994) under the Clinton administration (1993–2001), which some observers have identified as the most contentious period of civil-military relations in American history (Kohn 2002).

In his book *Armed Servants*, Peter Feaver (2003) highlights the inability of Huntington's theory to explain American civil-military relations after World War II. Instead of a clear separation between civilian and military spheres, civil-military relations were characterized by a "convergence" of civilian and military roles and the intrusion of civilians into military affairs. Consequently, Feaver proposes an alternative theoretical explanation. His game theoretic model of the day-to-day interactions between civilians and military leaders assumes that the degree of civil-military friction and the military's propensity to violate civilians' demands in a specific conflict of interest depend mainly on the civilians' willingness and ability to monitor the military and punish its misbehavior, and on the severity of the conflict of interest: the more intrusive the civilians' oversight over the military, the higher the likely costs of punishment for military "shirking," and the smaller the differences between civilian and military interests, the greater the chance that the military will do as the civilians wanted, and the more harmonious civil-military relations will be. Feaver's empirical analyses of civil-military relations in the Cold War, and in the post-1990 "crises" under the Clinton administration, demonstrate the explanatory power of his argument. Following his model, the "crisis" in the 1990s can be explained by the increasingly severe civil-military clash of interests regarding the core military reforms on force restructuring and reduction of troops, the expansion of military roles and missions to include non-traditional missions, and the inclusion of homosexuals in the military, all of which were interpreted as deep incisions into the military's core sphere of expertise and autonomy. At the same time, President Clinton suffered from idiosyncratic weaknesses of authority vis-à-vis the military while the military was led by a strongly charismatic and popularly supported Chairman of the Joint Chiefs, Colin Powell, which reduced the willingness and ability of civilians to monitor the military intrusively and punish military transgressions.

A different theoretical argument was proposed by Michael Desch (1999) to explain the significant changes in civil-military relations in the United States after the end of the Cold War. Similar to Feaver, Desch is mainly interested in the causes of civil-military conflict within the confines of the robustly established institutions of civilian control in the U.S., and he agrees that both the conflict of interests and the concrete mode of civilian oversight and direction of military activities are important for civil-military relations. Different than in Feaver's model, however, Desch stresses that civil-military frictions are determined mainly by structural factors, which civilians and military leaders cannot easily influence: the severity of external (international) and internal (domestic) threats the state is facing. According to Desch, civil-military relations will be harmonious if external threats are high and internal threats are low, because the interests of civilian and military elites will converge toward the common goal of defending the state against the external aggression. In a situation of low external threats and high internal threats, in contrast, civil-military relations will be contentious, as there is no external incentive to "rally round the flag," and civilian elites will split and jockey for the military's political support, while the military can exploit politically its increasing importance for the regime elites to uphold social order and regime survival. While the internal threat dimension remained inconsequential for the U.S. all through the twentieth century, Desch identifies the end of the Cold War as the crucial turning point in civil-military relations: With the demise of the Soviet Union, the United States lost its clearly defined external threat, which not only incited conflicts about the military's future

missions, but also made room for the Clinton administration to challenge the military's policies on homosexuals without having to fear that civil-military frictions would endanger national security.

A third, sociological explanation for the worsening of U.S. civil-military relations in the post-Cold War era argues that civil-military conflicts were the expression of a cultural gap between civilian and military elites. Following this thesis, the establishment of the all-volunteer force in the 1970s, the lack of military experience among civilian political leaders, value changes in the American society, and the increasing isolation from civilian society of the military have led to an increasing and self-reinforcing divergence of norms and values between civilian and military, which, in turn, led to the drastic instances of civil-military conflict (Kohn 2002). Different than Feaver's and Desch's explanations, however, the "gap" thesis is neither built on convincing theoretical foundations, nor corroborated empirically (Feaver and Kohn 2001). Research using a survey-based dataset prepared and administered by the Triangle Institute for Security Studies in 1998/1999 does identify diverging political attitudes between civilians and soldiers, with significantly larger shares of military officers than civilians expressing socially conservative values, preferring the denial of access to the military for homosexuals, and sharing the political positions of the Republican Party. At the same time, these studies cannot show convincingly how the existing cultural "gap" was related to the observable conflicts between civilians and the military. In fact, concerning the most crucial questions related to civil-military relations and the subordination of the military under civilian authority, the survey results show remarkable degrees of agreement between civilians and military officers (Feaver and Kohn 2001; Szayna *et al.* 2007).

Conclusion

The last five decades or so of research on the political role of the military have produced important insights on political-military relations in different regime settings. However, a number of oversights and lacunae remain in the literature that should be addressed in future research. In this, we see three broad areas in which progress will be particularly productive for the accumulation of knowledge in the research field. The first pertains to the degree of theoretical development. Past research on the military's political role has been criticized repeatedly for its tendency to ignore the theoretical progress made in the broader social science disciplines (Pion-Berlin 2001). In addition, the lack of theoretical development in the field is attributed to the dominant modes of theorizing in civil-military relations research, which is characterized by inductive generalization of case study findings, and the attempt to identify individual causally relevant factors, without theorizing the processes of actors' agency that link these factors to the outcomes to be explained (Feaver 1996). Of course, these characterizations are extremely broad-stroked as there are considerable differences within the field. Recent research on military coups, for instance, is theoretically highly developed and derives its propositions from clearly specified theoretical models of the political interactions of civilian and military elites (Acemoglu *et al.* 2009; Svolik 2012). Similarly, and as noted, scholars of civil-military relations in established and new democracies have taken up arguments from the new institutionalism (Avant 1994; Pion-Berlin 1997), International Relations structuralism (Desch 1999), and game theory (Feaver 2003), and have proposed integrated theories that include convincing causal mechanisms. In general, however, the field would benefit greatly from being more receptive to the innovations of the broader disciplines of social science, and paying greater attention to clearly specified causal mechanisms as it would allow the integration and comparative evaluation of the hitherto unconnected and ultimately untestable plethora of "partial theories" (Kennedy and Louscher 1991) and "theoretical eclecticism, sometimes bordering on anarchy" (Fitch 1977: 206). In addition, much more

conclusive tests of the arguments, as from complete causal arguments that integrate arguments on both causal effects of structural variables and the causal mechanisms of human agency hypotheses can be derived for a number of different analytical levels, which can be tested with a variety of methodological tools (Kuehn and Lorenz 2011).

Of course, consolidating the body of theory and improving the quality of explanatory frameworks will remain meaningless if they are not coupled with solid empirical testing. However, in the past, the field has often suffered from narrow methodological approaches and the focus on a few select geographical regions. Concerning the former, much empirical research on the political role of the military has been limited to case studies and small-*n* comparative analyses. Systematic, inter-regional comparisons of political-military relations are rare. Large-*N*, cross-national studies play a role only in the subfield of coup studies (Belkin and Schofer 2003; Powell 2012a), but even here methodological innovations such as fuzzy-set/Qualitative Comparative Analysis (Ragin 2000), and the systematic combination of large-*n* methods and case studies (Lieberman 2005) are mostly absent. Moreover, recent innovations in qualitative and case study methodology (Brady and Collier 2010) have not been adopted in the field. In addition, research on the political role of the military has suffered from its limited geographical breadth and sampling variance. For example, research on political-military relations in authoritarian regimes has mainly focused on the military regimes of Latin America, while the states of the former Soviet Union (except Russia), Africa, and the Middle East are mostly unstudied. The same is true for research on civil-military relations in new democracies: there are a large number of studies on the former Latin American military dictatorships and the former communist regimes of Eastern Europe, while we still know little on the establishment and consolidation of civilian control in the new democracies of Asia and Africa. Finally, and as noted, the study of civil-military relations in established democracies has been mainly focused on the U.S. and a small number of European countries, with military sociologists accounting for the few truly comparative studies (Moskos, *et al.* 2000; Caforio 2006). Together, these characteristics have limited the conclusiveness of empirical analyses, and hampered the accumulation of robustly tested and general theoretical knowledge on the political role of the military in various regime types. In order to address these shortcomings, future research should, for one thing, broaden its methodological perspective, open itself up to methodological innovations and make use of the whole toolkit of social science methods, both qualitative and quantitative. For another thing, future research should consciously address civil-military relations in hitherto understudied cases and regions in order to broaden the empirical basis of our theories.

A third suggestion for future research would be to further open up the agenda of civil-military research to understudied research questions. In this, we see two particularly promising topics. First, scholars should pay closer attention to political-military relations in authoritarian regimes, especially in the “new” forms of non-democratic rule that have developed after the end of the Cold War, e.g. “electoral authoritarian” or “hybrid” regimes. It is substantively important to comprehend how electoral authoritarian leaders maintain their militaries’ cooperation, in order to fully understand the internal dynamics of these regimes and their likelihood to withstand crises, such as the recent mass protests in the Arab world. However, as noted, this research is still in its infancy, such that researchers are likely to make a meaningful and original contribution even if they focus their research on a limited set of cases. The second agenda in which we see great future potential treats the military’s political role as the independent variable and asks for the effects of political-military relations and the mechanisms of political control on various aspects of politics and public policy in authoritarian and democratic regimes, including the ability of dictators to generate output legitimacy and stabilize their rule (Croissant and Wurster 2013), and the complex interplay and potential trade-offs between the degree and concrete

mechanisms of civilian control, and military effectiveness and efficiency (Bruneau 2012). All this suggests that the military's political role will remain a substantively important and intellectually rewarding research topic in the foreseeable future.

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AUTHORITARIAN INSTITUTIONS

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Introduction

In democracies, institutions are often taken as “given,” allowing us to examine the effects of those institutions on outcomes. Electoral rules influence the behavior of voters and parties, resulting in a variety of consequences for party systems (see Negretto, [Chapter 9](#)). Rules governing center-peripheral relations influence the economic policies of regional governments and the social and political demands of ethnic minorities (see Beramendi and León, [Chapter 15](#)). The internal organization of legislatures determines how legislation is proposed and voted upon, strongly influencing policy outcomes (see Alemán, [Chapter 11](#)). In all of these cases, scholars examine institutions-as-rules, or how institutions affect behavior which, in turn, determines outcomes.

In dictatorships, however, those same institutions—legislatures, parties, elections, courts, and federalism—exist, but usually at the discretion of rulers and their supporting elites. As a result, elections are sometimes postponed or cancelled; partisan activity is sometimes banned; the realm of legislatures and courts may be radically curtailed; and the autonomy of lower levels of government may disappear. And just as quickly, these institutions may be reinstated or changed in other ways. Because these institutions cannot be taken as “given,” any analysis of their effects often must deal with the prior question of why they exist in the first place. In this sense, scholars of authoritarianism often do not analyze institutions-as-rules alone, but also institutions-as-equilibria. Given the nature of authoritarian institutions, they must engage in full, not only partial, equilibrium analysis (see Svobik, [Chapter 6](#)).

Because of the focus on institutions-as-equilibria in dictatorships, we review the different functions institutions—elections, parties, legislatures, courts, and federalism—have been purported to serve. They enable autocrats to carry out some critical tasks, including information gathering, co-optation, credible commitments, conflict resolution, and signaling. In addition, institutions can help autocrats minimize future losses and diffuse responsibility. Thinking of institutions as solutions to problems faced by autocrats, however, leads to the emergence of deeper questions. For instance, many of the functions enumerated require that institutions actually be able to constrain dictators. But given endogeneity issues, can they ever really act as exogenous constraints? Further, institutions are often studied in isolation from each other, which means little has been done to understand how they might be either complementary or

redundant, and what these relationships say about the choices autocrats make. How do autocrats come to adopt “bundles” of institutions? Are institutions sometimes adopted as a solution to problems created by other institutions? This last question is also a reminder that while institutions can be solutions, they also carry their own risks. By discussing such issues, the [second part](#) of the chapter highlights tensions in our understanding of authoritarian institutions that deserve further inquiry.

Functions of authoritarian institutions

Information gathering

In order to maintain power (and possibly govern) effectively, dictators require information. They may need information about citizens’ electoral and policy preferences. National leaders also may want to keep tabs on local and bureaucratic officials. Due to circumscribed media and the costliness of secret police, dictators may use institutions, such as legislatures, parties, and elections, in order to obtain these different pieces of information.

Authoritarian leaders may seek information about the identity of their supporters in order to know the location and strength of their loyalist bases. Conversely, they may want information about opposition supporters in order to target suppressive efforts. Elections provide this information by revealing the geographical distribution of citizens’ preferences for (or against) the government, although it is not clear how this goal can be reconciled with the need to win elections through means that obscure the accuracy of this information (e.g. fraud, manipulation) (Malesky and Schuler 2011). The degree to which this trade-off is managed may depend on the type of fraud committed. Alternatively, incumbents may glean information on popular support in the presence of manipulative and fraudulent electoral practices if such practices remain stable over the course of multiple elections. Finally, notwithstanding fraud and the manipulation of rules, campaign rallies, illegal protests, and the size of crowds at polling stations are all publicly observable and should remain unaffected by electoral fraud and manipulation (Cox 2009: 12).

Authoritarian leaders may want information about citizens’ preferences over policies as well—in order to maintain the support of a sufficient proportion of the population. Parties are generally able to gather such information when they are institutionalized, and more specifically when they are organizationally complex and rooted in society (Huntington and Moore 1970). For instance, party cells are the most basic units of the Vietnamese Communist Party (VCP) and usually include 3 to 35 members (Koh 2006: 45). The information they gather, through close contact with the population but also through the election of party cell representatives, can then travel upward within the party structure and reach higher-level officials. In general, rootedness in society

allows the party to maintain constant dialogue with its constituents, which enables the organization to respond to needs at the grass roots effectively. In turn, constant feedback from the grass roots strengthens the party’s grip on power by ensuring that it is responsive to the members’ needs.

(Kuhonta 2011: 26)

Alternatively, legislatures can provide information about citizens’ preferences depending on how they are designed. Malesky and Schuler (2010) find that a higher proportion of local, full-time delegates who obtain their seats through more competitive elections to the Vietnamese

National Assembly increases the likelihood that they will provide information to the regime by acting as “real” representatives. Likewise, elections may provide information about the intensity of citizens’ views. Enough citizens willing to vote against the regime—even at the risk of losing their material benefits—serves as a signal to the government about the intensity of their dissatisfaction over certain policies (Miller n.d.).

Like democratic rulers, authoritarian ones must delegate tasks to local officials and bureaucrats, requiring monitoring of these agents in an effort to prevent or punish moral hazard. Local elections may serve as a venue in which citizens—through their electoral choices—provide national leaders with this information about their local agents (Landry 2009). If citizens suffer under corrupt or incompetent local officials, they can send information about this state of affairs to national leaders by voting their incumbent local leaders out of office. High turnover in legislative seats in Egypt, for example, enabled the Mubarak regime to determine which elites command the most popular support within their constituencies and require co-optation (Blaydes 2010). In Vietnam, the Communist Party has stated that centrally nominated candidates who fail to win more than 60 percent of the vote must undergo self-criticism, indicating the importance of information on delegates’ popularity (Salomon 2007: 203). Nonetheless, the evidence about whether and how authoritarian leaders use citizens’ evaluations to identify whom to promote and punish is at times contradictory. Malesky and Schuler (2013), using data on the 2007 Vietnamese National Assembly, argue that promotion to leadership positions has not been related to the electoral performance of delegates, but rather to whether or not the VCP had previously nominated them as electoral candidates. Similarly, Chinese authorities have devised a number of ways to subvert “disagreeable” electoral outcomes at the village level (Zhenglin and Bernstein 2004).

Alternatively, central government leaders may want information about how their agents perform in ways that are not a reflection of citizens’ evaluations, and both legislatures and elections can help in this regard. Legislatures can provide opportunities for delegates to signal their loyalty when they choose to speak less often and less critically of the regime (Malesky and Schuler 2013). Elections may serve as a test of local officials’ willingness and ability to deliver votes to the regime (Malesky and Schuler 2011; Blaydes 2010). In Russia, for example, the regime uses elections to gather information about the political loyalty of governors (Reuter and Robertson 2012). Governors who manage to deliver votes to the regime signal their political loyalty, and this information is, in turn, used by the regime for future appointments. Gathering information about both citizens and elites would seem to be a prerequisite for authoritarian institutions to perform other functions, such as co-optation.

Co-optation

In any authoritarian regime, there are three (sometimes overlapping) populations with whom autocratic leaders must manage their relations: the mass public, elites, and the opposition. People who are located outside of the regime and do not have access to significant resources constitute the mass public while elites are individuals with access to resources that give them some measure of power. Regime elites, for example, are a type of elite who derive (some of) their power from their participation in the regime. Finally, the opposition includes both elites and citizens who oppose the regime. Co-optation is a broad, general term that refers to “bringing people in” with any of these three populations being the target. And while co-optation can occur outside of institutional settings, it also occurs in distinct ways through parties, elections, and legislatures.

Co-optation of the mass public can occur either when individuals are given benefits directly through regime intermediaries or when the regime provides diffuse benefits to the whole

population. Both give the mass public a vested stake in the continuation of the regime. For instance, focusing on economic growth can be one way for authoritarian regimes to generate support by providing widespread benefits to their population. A more direct form of co-optation of the masses occurs when intermediaries between the regime and the masses help procure and distribute the benefits to individuals. To the extent that these types of clientelistic programs and other types of policy concessions are enacted through law, legislatures are an important institution (Gandhi 2008; Kim and Gandhi 2010). Policy congruence between constituents and their public officials may occur through legislatures (Truex 2014a) or through elections (Manion 1996). The mass public can also be co-opted through mass organizations when these organizations are used to distribute benefits to their members.

Co-optation of the opposition usually occurs through the legalization of political parties and their participation in elections and the legislature, which may serve to create “insiders” who develop a vested interest in the maintenance of the regime (Lust-Okar 2005). For “insiders,” the boundary between opposition and regime elite often becomes progressively blurred. “Outsiders”—who have forgone the benefits of legalization—in turn become increasingly marginalized. The fact that legislatures increase the likelihood of authoritarian regime survival may be one direct consequence of this dynamic (Gandhi 2008; Wright and Escribà-Folch 2011).

Co-optation of elites is necessary to deter threats from among their ranks to create a stable basis for authoritarian rule. When no one in a nascent regime has enough power to rule alone, the initial recruitment of elites is critical for regime stability. In these circumstances, the ability of elites to act collectively often determines the regime’s chances of survival and the form it will adopt (Brownlee 2007; Slater 2010). The incentives for elites to act collectively depend on the distribution of material and coercive resources (Svolik 2012) and the presence of mass or external threats (Slater 2010). But once a coalition of elites supports the regime, institutions are critical in maintaining these alliances.

In maintaining elite cohesion, institutions are important for distributing rents. Legislatures serve this purpose (Truex 2014b; Lust-Okar 2006) along with regime parties (Geddes 1999; Magaloni 2006). For recruiting elites, legislative seats and positions within the regime party provide avenues for career advancement. To the extent that these institutions provide rules or norms about the criteria and procedure for promotion, they routinize the process, which generates stable expectations for elites (Svolik 2012; Reuter and Remington 2009). These institutions, however, must balance the demands for recruiting fresh members with new ideas, resources, and popular constituencies against the continuous cultivation of support among existing elites.

Credible commitments

Institutions that are independent of and constrain executive authority enable leaders to credibly commit to policies that are favorable to the interests of asset-holders. This argument has been made about institutions such as legislatures, courts, and central banks in democracies. The same argument has been made about institutions in autocracies. In military and party dictatorships, legislatures are able to bind autocrats so that the latter can make credible commitments that lead to higher investment and growth (Wright 2008). Courts with independent authority also indicate that the government is constrained and therefore capable of making a credible commitment (Moustafa 2009). Federalism can act in a similar manner when the central and regional governments constrain each other. When regional governments control some realms of economic policymaking and can operate competitively, “market-preserving federalism” provides incentives for agents to make growth-enhancing investments (Weingast 1995; Montinola *et al.*

1995).¹ Greater investment, in turn, can strengthen the ability of regional governments to serve as a check on the center (Malesky 2008).

The issue is that with institutions that cannot enforce their own decisions, let alone maintain their institutional integrity in the face of executive dominance, it is unclear just how credible are the commitments of dictators. In Egypt, for example, Sadat established an independent high court and Mubarak encouraged it in order to attract greater investment (Moustafa 2009). The government obeyed the court's rulings (even those that were against the government) until it decided that it no longer wanted to do so (e.g. in human rights and security cases). With the high court helpless in enforcing its rulings and unable to stop the executive from curbing its powers, to what extent could investors have found the court's existence as evidence of the government's credibility? The same is true for legislatures: if dictators do not like the actions or policies produced by assemblies, what would prevent them from curtailing their powers or closing them?

As a result, institutions probably play a more modest and nuanced role in enabling credible commitments. Svolik (2012) argues that they play an important, albeit partial, role in enabling autocratic leaders to credibly commit to power-sharing with elites. A leader follows through on his commitment to share power only when he faces a credible threat of punishment from elites, and the credibility of this threat depends on the extent to which elites can collectively act to punish the leader for any defection from the power-sharing bargain. Formal deliberative, decision-making institutions do two additional things. First, regular interaction within institutions such as politburos, advisory councils, and legislatures increases transparency, enabling elites to monitor what the leader is doing. The ease of monitoring, in turn, "reassures the allies that actual attempts by the dictator to usurp power will be caught before it is too late" (90). Second, formal rules about membership, jurisdiction, and procedures enable the dictator and elites to have a shared understanding of what constitutes a transgression of the power-sharing agreement. As such, institutions facilitate detection of the dictator's non-compliance (easing the collective action among elites à la Weingast's 1997 argument) and prevent misperceptions of the dictator's actions from escalating into conflict. Svolik (2012) applies this argument to legislatures and Reuter and Remington (2009) evoke a similar idea when it comes to the creation of dominant parties. Institutions facilitate power-sharing, but on their own, they are not enough for dictators to be able to make credible commitments since elites still need to punish transgressions themselves.

Conflict resolution

Institutions or rules that govern the relationship among elites within the regime may also prevent costly conflicts that potentially can destabilize regimes. In Chile, the rules that governed the operation of the junta allowed for the regularized processing of conflicts among the junta members since its members frequently were not united in their policy preferences (Barros 2002). Institutions may simply enable for smoother, more regularized division of the spoils among regime elites. Regime parties provide multiple opportunities for advancement and rules for obtaining them so that elites need not view the fulfillment of career ambitions as a zero-sum game (Geddes 1999; Reuter and Remington 2009). In Mexico under the PRI, for example, those who were not chosen to become successor to the outgoing president were given important party positions (Castañeda 2001). Rules within dynastic monarchies diminish the likelihood of internecine conflict about succession, enabling these regimes to avoid the Achilles' heel of many autocracies (Herb 1999). What these examples suggest is that whether dealing with policy, spoils, or succession, in the absence of rules and institutions, elites would engage in costly conflicts.

Signaling

Authoritarian institutions also provide signals to a variety of different actors about particular attributes of the regime. Most important is the ability of institutions to convey government credibility. Legislatures and courts, for example, may signal to investors that executive power is constrained enough so that they need not worry about expropriation. Similarly, institutions may make the actions of governments in the international arena more credible. Weeks (2008) argues that party and military regimes (as opposed to personalist ones) face credible audience costs in that regime elites have the means and incentives to coordinate to punish the leader for backing down. Critically, other states can observe these mechanisms for domestic accountability and hence will take them into account when dealing with threats made by these autocracies. Similarly, even flawed elections can provide costly and confirmatory signals of resolve to international actors. As pro-incumbent bias in an election declines and the likelihood that incumbents transfer *de facto* political power increases, the more credible the signal of resolve becomes (Kinne and Marinov 2013). Not only do more competitive elections demonstrate higher audience costs, but when opposition parties that have an incentive to disagree with the government actually support its positions, elections also show that the strength of public opinion in favor of the government's position must be high. Domestic institutions also signal how likely a government is to renege on cooperative agreements made with other states (e.g. trade, security). Because of domestic constraints on power and their visibility to other states, party and military dictatorships cooperate mostly with other states that also have less flexibility to renege on agreements (e.g. other party and military regimes as well as democracies) (Mattes and Rodríguez 2013).

Institutions also can signal regime strength to the population and the opposition. For example, elections are sometimes used by authoritarian regimes to show their strength and deter challenges when they are won by wide margins (Simpser 2013; Magaloni 2006). In a similar fashion, the mobilization capacity of parties can be used to organize large protests that are a show of regime strength.

Minimizing future losses

Authoritarian leaders sometimes consider conceding or losing office. When they do so, institutions can help them minimize potential losses in influence once they are out of power by either binding successors or serving as insurance. One way of doing so is to put in place institutions that are designed to constrain the options available to successor governments. This seems to be a practice most commonly used by military governments that plan a return to civilian rule. In Chile, the military junta increased the autonomy of the central bank in an effort to insulate it from "the vicissitudes of the democratic political process" (Boylan 1998: 444). The junta also wrote the 1980 constitution and agreed to put as much of it into operation as possible in an effort to legitimize the document (Barros 2002). Indeed, military governments often adopt new constitutions when they seek to entrench broad political, economic, or social transformations (Negretto 2014).²

Military regimes also may preserve their power after a transition by influencing the composition of constituent assemblies (Wright and Escribà-Folch 2011). In Myanmar, for example, the 2008 constitution adopted under military rule stipulates that 25 percent of parliamentary seats must be reserved for military officers. In this way, institutions serve as insurance, providing a means by which to remain politically active and influence policy, particularly after democratic transitions. Regime parties can also act as insurance mechanisms for outgoing authoritarian

leaders and elites. If parties are well institutionalized and have extensive reach and capability in mobilizing voters, they can be used to win elections even after a transition to democracy. Wright and Escribà-Folch (2011) show that when dominant party regimes democratize, all former regime parties are competitive, winning at least the second largest seat share in at least one legislative election after the transition. After the transition, regime parties act to protect the former elite's interests, making them more likely to support a democratic transition in the first place. Such regimes, after all, care first and foremost about ruling, and not necessarily about remaining authoritarian (Slater and Wong 2013). While insurance might not be the original reason why autocratic leaders and supporters create a regime party, it becomes an important purpose when regime change is on the horizon and a critical factor in determining when change is likely to occur.

Diffusing responsibility

When authoritarian regimes make decisions without consulting the population, they alone shoulder the blame if their decisions turn out to have negative consequences. However, when regimes share decision-making power, blame attribution becomes more ambiguous. For example, when the legislature includes members of the opposition or even independent candidates, the policies proposed or supported by the opposition are no longer strictly regime policies. Therefore, in the face of public discontent, the regime will not be weakened relative to the opposition if they both supported the same policies. If the policies were actually suggested by the opposition, public discontent can actually provide the regime with an opportunity to blame and weaken the opposition. As such, one of the advantages of political liberalization is that the opposition becomes implicated in the painful policies sometimes required to solve domestic problems. Such policies, "when implemented, can now be blamed on the legislature as well as state leaders" (Lust-Okar and Jamal 2003: 343). Allowing the opposition to be represented in the legislature is not the only way in which authoritarian leaders can diffuse responsibility. Writing about modes of participation that have deliberative dimensions in China, such as citizen evaluation forums and local elections, He and Warren (2011) mention that one of the benefits of deliberation is that the people themselves now share in the responsibility of decisions.

Are institutions always solutions?

Institutional bundles

Autocrats clearly face multiple problems of governance, and they have a variety of institutional means by which they can address them. Why do they often appear to have multiple institutions to address the same problem? For instance, if autocrats can obtain information about citizens' preferences from the grassroots extensions of a regime party, do they also need elections? One answer may emerge in looking more closely at the many dimensions of information provision. Elections provide information only periodically, while parties and legislatures are arenas in which information flows continuously. Likewise, parties and legislatures are better suited to give information about policy preferences than elections, while the secrecy of the ballot might encourage a more sincere expression of regime support, making the information gathered more accurate. Autocrats might adopt different institutions for the same purpose because there is variation in how these institutions actually serve this purpose. Thus, we might pay closer attention to how the problems we outlined actually have multiple dimensions—only some of which will be addressed by a specific institution.

Conversely, it could be that autocrats sometimes adopt an institution because of its potential to solve multiple problems. For example, are elections so attractive because they are a mechanism by which information about citizen preferences can be aggregated and spoils can be distributed to both elites and voters? Alternatively, it may be that an institution solves some problems, but creates others, requiring another institution as a “patch.” Having inquisitive legislators may produce information for autocrats, but making sure that those same legislators do not become too rebellious requires strong nomination procedures within the regime party (Malesky and Schuler 2010). Decentralization may enable the central government to extend its infrastructural power to the peripheries, but then local agents must be monitored, which can be done through the use of local elections (Landry 2009). While studies of individual institutions such as elections, legislature, and parties have produced many compelling arguments about why and how dictators use them, we understand less about the institutional bundles with which autocrats govern. It may be that scholars of individual institutions need to converse more with each other (Schedler 2009), or that as they delve into the details of these institutions, they would benefit from what the deep literature on democratic institutions has to say.

Institutions as constraints

That dictators choose and modify institutions at will, depending on the types of problems they need to solve and the severity of those problems, raises the specter of functionalism. It also casts legislatures, parties, elections, courts, and federalism as institutions-as-equilibria, which has more thought-provoking implications than the functionalist critique—namely, the tension between institutions-as-equilibria and institutions-as-rules (Przeworski 2004; Pepinsky 2013). To the extent that these institutions are an outcome of the choices that autocrats make, they are the product of other factors. Yet if institutions are endogenous, then to what extent do they serve as exogenous constraints that incentivize behavior and “cause” other outcomes? For example, if a dictator can establish a regime party to share power with elites only when the balance of power between them is equal, then to what extent is the absence of conflict within the regime a function of the party or of the balance of power that gave rise to the party in the first place?

On some level, the problem needs to be addressed by research design so that we can ensure that the causal effect we ascribe to institutions has empirical verification. But the tension also needs to be addressed by theory insofar as we carefully specify what institutions are doing and how. Institutions under authoritarianism may be more than epiphenomenal, but they may end up playing a somewhat “modest” role in authoritarian regimes. In and of themselves, legislatures and parties, for example, do not constrain rulers or force them to share power. Rather they may enable the transparency and monitoring necessary to preserve power-sharing bargains between leaders and members of their support coalitions (Svolik 2012). Alternatively, they simply serve as a forum in which the dictator and the potential opposition may hammer out concessions (Gandhi 2008). In neither of these accounts do institutions themselves constrain rulers.

Institutions may indirectly constrain dictators to the extent they preserve the distribution of power that allowed them to be stable in the first place. If an equal distribution of power is what enables the creation of strong ruling parties, for example, these institutions can help ensure that the distribution of power remains somewhat equal in the future. Under PRI rule, the fact that presidents could not remain in office for more than one mandate clearly helped avoid a progressive concentration of power in the hands of a single individual (Magaloni 2006). Another example is the use of divided-executive constitutions to prevent the concentration of power in the hands of the executive in transitioning countries (Hale 2011). Maybe institutions do not

constrain power directly, but they can help maintain a balance of power in which some actors are powerful enough to credibly threaten to punish the dictator and thus constrain him.

Risks and trade-offs

Even if we accept that institutions play this more ambitious, constraining role, they result in the following conundrum: In order to survive in power, dictators use institutions to address certain problems, and these institutions are solutions only to the degree to which they constrain dictators. But constraints enable challenges to their rule which, in the end, may contribute to their losing power. When autocrats do not use their “menu of manipulation,” they risk losing elections (Schedler 2002). When they comply with legislation and judicial decisions that run counter to their preferences, they enable the emergence of institutions that can become the site of greater political contestation. Therefore, scholarship on authoritarian institutions is increasingly examining how institutions can pose risks to authoritarian incumbents.

The most fundamental risk associated with authoritarian elections is obviously the risk of losing them. Elections can provide the opposition with an opportunity to use a bundle of strategies that can lead to their victory (Bunce and Wolchik 2010). Similarly, Howard and Roessler (2006) show that liberalizing electoral outcomes take place when the opposition manages to form a pre-electoral coalition, which channels votes to a single opposition candidate and raises the cost of fraud and manipulation. The emphasis these authors put on the opposition’s strategy shows that, ultimately, whether elections are won or lost can be out of the authoritarian ruler’s hands. In addition, fraud and manipulation carry their own risks, since they can provide a focal point for the opposition and masses to rally around after elections take place, thus facilitating opposition coordination after the fact (Tucker 2007). Alternatively, Reuter and Robertson (2012) suggest that rewarding officials who deliver votes is another way to deal with the necessity to win elections. This strategy nonetheless carries its own risks, since competence is no longer the basis for rewards, and rewarding loyalty instead of competence can have long-term negative consequences such as hindering growth.

As for legislatures, the main concern is that they can empower the opposition by giving it a voice. However, this risk can be mitigated in a number of ways. Malesky and Schuler (2010) mention that while the VCP provides some delegates with incentives to speak up by increasing the proportion of local, full-time delegates, the vast majority of delegates are still selected in a way that ensures upward accountability. Another risk is the potential loss of autonomy, as in cases in which a regime fails to win the legislative supermajority required for constitutional amendments. In Malaysia, the Barisan Nasional (BN) failed to win the two thirds of seats required for constitutional amendments in 2008 and 2013, making it impossible for the regime to make major institutional changes at will. This can be problematic since the power to modify the constitution is what allows authoritarian rulers to modify institutions so they can serve them better in all the ways outlined.³ Further, legislatures can pose a risk by increasing voters’ confidence in the opposition. Once they win seats, opposition members can start giving them more information about how they would actually govern and the policies they support. Likewise, if they are associated with the adoption of popular policies, opposition members can improve their reputation. As blame can be shared, so can merit. Last, other institutions may help mediate the risks posed by legislatures. Ruling parties mitigate these risks by providing authoritarian leaders with stable legislative majorities (Reuter and Remington 2009).

But parties come with their own risks as well. For one, they can facilitate elite collective action and help them coalesce into an opposition, since parties increase interactions among elites and are not monitored as heavily as legislatures (Wright and Escribà-Folch 2011).

Moreover, parties may provide an opportunity for factions to emerge and fight. This becomes problematic when parties cannot mediate this conflict internally and factions leave the party to contest elections (Reuter and Gandhi 2011). This happened in Malaysia in 1987, when Razaleigh challenged the incumbent Mahatir for the UMNO presidency. Ultimately, UMNO split, with Razaleigh and some of his supporters leaving to form their own party. Alternatively, parties also carry a risk for authoritarianism because they can increase the likelihood of democratization by helping authoritarian elites maintain some measure of power and secure their interests in the context of democratic transitions (Wright and Escribà-Folch 2011). By helping authoritarian rulers preserve their interests, even in a democratic context, they diminish the need for authoritarianism itself.

Similarly, decentralization helps solve problems faced by authoritarian leaders while carrying its own risks. That some authority is given by the central government to regional governments requires the relinquishing of some control over the regions. As localities are able to cultivate their own sources of revenue, they may further assert their independence from the center, as is the case of regional governments in Vietnam that manage to attract significant foreign direct investment (Malesky 2008). Why might local governments deviate from the preferences and mandates of the center? In some cases, it may be that local officials—especially if elected—care more about their constituents than the central government. In China, the elected village committee chairperson sometimes heeded the wishes of his constituents, challenging the authority of the village party branch (Zhenglin and Bernstein 2004). Unhappy with such popular challenges to the party, the central government now requires that village party secretaries also compete in elections for the position of village committee chairperson to avoid policy conflicts between them. Last, once the center has a way of monitoring the local agents empowered by decentralization, how should it deal with those who underperform? Demotion would create a group of disgruntled elites who may pose a danger to the regime. As a result, in China, for example, mayors who underperform (as determined by the measurable criteria set by the center) are rarely demoted (Landry 2009). They are instead left in place rather than given promotions.

That authoritarian institutions pose risks for their creators helps the voluminous literature on how autocrats use institutions to solve problems of governance distance itself from the problem of functionalism. But it also raises the question of whether authoritarian legislatures, courts, parties, elections, and decentralization are open-and-shut cases of “institutions-as-solutions.” They are solutions to some problems, but they also seem to create others. Under what conditions do these risks emerge? When creating or maintaining institutions, are autocrats aware of such risks? If they are aware of the risks and proceed with these institutions, are they myopic, making mistakes, or simply managing trade-offs? Or were they not aware of such risks because they adopted these institutions for other reasons?

To the extent that institutions serve multiple purposes, they may force dictators to live with certain risks in order to minimize others. Legislative elections, for example, may be a means both to fairly distribute spoils among regime elites and to obtain information about citizen preferences. So when the opposition unites and draws significant popular support, autocrats may have to live with such risks because they need legislative and electoral institutions to satisfy elite demands for patronage. If potential opposition or mass actors are sufficiently threatening, autocrats may need institutions such as parties to contain them even if these institutions create other types of risks. In other words, dictators may face multiple problems of governance. To the degree that institutions address multiple problems, they create trade-offs for autocrats.

Moreover, we know that some autocrats, at the first sign of trouble, modify or close institutions. In the Philippines, Marcos declared martial law and assumed absolute power in response

to rising opposition. In Algeria, the military cancelled second-round elections and closed the legislature after an opposition party won a majority of legislative seats in the first round. So identifying the conditions under which autocrats choose to modify or close institutions in the face of these challenges is important. This line of inquiry leads to the more complicated project of understanding institutional change in authoritarian regimes. It also implies that the degree to which autocrats can modify institutions should influence just how much these institutions actually solve some types of problems (Pepinsky 2013). If authoritarian incumbents are free to modify and close institutions whenever these rules and structures pose risks, it seems doubtful that the institutions will be able to perform certain functions that logically speaking require constraining executive power. For example, if institutions allow for credible commitments only when the balance of power is relatively equal (Svolik 2012), then under these circumstances, it is doubtful that they could be closed without dire consequences for the ruler, given that elites should be able to punish him for doing so.

Conclusion

Autocrats adopt institutions typically associated with democratic regimes: elections, parties, legislatures, courts, and decentralization. These institutions often are more than mere window-dressing, instead serving multiple functions that facilitate their rule. They can be used to gather important information about regime support, popular preferences, and the performance of regime officials, all of which are crucial in dealing with challenges before they become serious threats. Further, institutions can help autocrats co-opt the mass public, the opposition, and elites, giving them a vested stake in regime continuation. Under some conditions, institutions may also facilitate the constraining of autocrats in addition to sending signals to relevant audiences and resolving conflicts. Institutions can also help autocrats minimize future losses by constraining democratic and authoritarian successors alike, while ruling parties can even be used to remain in power after a transition. Lastly, when institutions become arenas in which power is shared with regime outsiders, they can allow autocrats to diffuse responsibility by blaming others for their mistakes.

There is an equally large and growing literature on the effects of these institutions on a variety of outcomes, ranging from economic performance and democratization to interstate conflict. Yet in tackling the question of how institutions influence behavior and outcomes, it is difficult to evade the question of why such institutions exist in the first place. As a result, the focus of the recent literature on authoritarian institutions has been the reason for these institutions and the conditions under which they arise. This focus on institutional choice has been fruitful, but also raises more questions for which we currently have few answers. To begin with, more attention needs to be paid to institutional configurations as a whole rather than to single institutions in isolation. In addition, given that these institutions sometimes entail risks for autocrats, it would be useful to better understand whether autocrats are aware of such risks and how they manage these trade-offs.

Finally, while our concentration here has been largely theoretical, we do not mean to ignore the empirical challenges that should naturally emerge with greater theory development. The focus on how the existence of these institutions helps autocrats rule has enabled scholars to test their arguments using data that are collected based on clear, objective criteria. Elections are held, or they are not. Legislatures exist, or they do not. Parties operate, or they are banned. At this point, it may be fruitful to investigate not only whether such institutions exist, but also how they operate—their internal organization and rules. Analyzing these institutional features may allow us to make greater progress in understanding the reasons for these institutions as well as

their effects. But for this, we will need more efforts to collect such detailed information. In addition, to the extent that the effect of institutions are underpinned by non-institutional factors (e.g. balance of power between rulers and elites, collective action capacity of elites), we will need to consider how to operationalize such concepts. This task requires not only significant man-hours, but also creativity. But only until we have better measures of such concepts can we verify whether formal institutions indeed have significant effects.

Notes

- 1 See Cai and Treisman (2006) for an illuminating discussion of the Chinese case.
- 2 It is also possible that institutions can be used not only to bind democratic successors, but also authoritarian successors. Svobik (2012), for example, argues that the institutional reforms implemented by Deng Xiaoping would probably not have been able to overcome his personal authority but did manage to bind his successors (86). Bunce and Wolchik (2010) also mention that institutions can be used to constrain the autonomy of authoritarian successors. When authoritarian rulers face term limits, one possible strategy is to select an ally as successor and to implement institutional reforms to constrain that ally at the same time. Putin and Medvedev are an example of this strategy.
- 3 In Mexico, for example, the PRI frequently used this strategy and modified the constitution in its favor nearly 400 times (Magaloni 2006: 15).

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20

FISCAL INSTITUTIONS

Joachim Wehner

The last two decades have seen a substantial growth in the comparative literature on the design of fiscal (or budget) institutions and their effects. Fiscal institutions are the rules and procedures that govern the budgetary process. The budgetary process covers the development of a draft budget by the executive, legislative review and approval, the execution of the budget during the fiscal year, and the production of annual accounts and their audit. For instance, the rules of the budgetary process determine whether the finance minister, prime minister, or president can impose ceilings on spending bids by individual line ministers, while amendment powers in relation to tax and spending bills determine the scope for legislators to influence budgetary choices. Such rules and procedures are often codified in law, but they can also be deeply entrenched conventions.

Largely based on theories of pork-barrel politics and common pool resources, empirical work has expanded steadily to cover different and diverse sets of countries. This work highlights that budget institutions can play an important role in the governance of public finances, and that they can help to explain variation in fiscal policy across countries and over time. As a result, the comparative study of budget institutions is not only of interest to academics, but also for policy-makers in national governments and international organizations that seek to promote prudent management of financial resources.

This chapter provides an overview of the cross-national literature on the design of budget institutions. Section one looks at the theoretical foundation of most of this literature, which examines the common pool resource problem in budgeting and develops implications for the design of the budgetary process. The second section summarizes existing empirical work with cross-national data. Section three focuses on the complexity of institutional design. It highlights the potential for side effects and unintended consequences of budget institutions, and how they interact with other political variables to determine policy outcomes. The conclusion notes under-researched areas that should receive attention in further research.

The common pool resource problem

Most comparative work investigating the effect of budget institutions on fiscal policy draws on the basic idea that spending will be higher when decision-makers do not internalize the full costs of their actions. Weingast *et al.* (1981) expressed this as the “Law of 1/n” (see also Shepsle and

Weingast 1981). In their model, expenditures can be targeted at a particular geographical district where they produce benefits, while costs are shared equally across all districts. The larger the number of districts, the smaller the share of the tax burden considered in spending decisions. Hence, assuming universalistic logrolls, “the degree of inefficiency in project scale... is an increasing function of the number of districts” (Weingast *et al.* 1981: 654). In other words, the possibility to disperse costs and to target benefits engenders a pro-spending bias as the number of decision-makers increases.

A number of studies support the prediction. Fiorino and Ricciuti (2007) and Bradbury and Crain (2001) find some evidence of the predicted effect of legislature size on expenditures, although in the latter case bicameralism is found to dampen the effect. Focusing on the executive arena, Perotti and Kontopoulos (2002) find that cabinet size is a determinant of fiscal outcomes in a panel of 19 OECD countries over the 1970 to 1995 period. Volkerink and De Haan (2001) show that deficits increase with the number of spending ministers, i.e. the total number of government ministers minus the minister of finance and/or the budget as well as the prime minister. The most comprehensive cross-national study so far, based on a global sample of 58 countries over a 24-year period, also reports a strong positive association between the number of spending ministers and central government budget deficits and expenditures (Wehner 2010a). A subnational study by Schaltegger and Feld (2009) on the fiscal consequences of cabinet size in Swiss cantons comes to similar conclusions.

Where party discipline is strong, the relevant fiscal decision-makers might be political parties instead of individual politicians in the legislature or the executive (Hallerberg 1999, 2004: 22–27). Bawn and Rosenbluth (2006) show a positive association between the number of governing parties and the size of government in 17 European countries between 1970 and 1998. The degree of party cohesion is related to the design of the electoral system (Carey and Shugart 1995). In a closed list proportional representation system with one nationwide constituency, party leaders may exercise very substantial control over the inclusion and placement of particular candidates on their party’s list, which can be used to induce loyal behavior. On the other hand, where candidate selection is heavily decentralized to local constituencies, and candidates from the same party compete against one another for local support, party cohesion can be tenuous. Edwards and Thames (2007) and Hallerberg and Marier (2004) explore the fiscal consequences of electoral systems that engender such a “personal vote.” The idea of the “Law of $1/n$ ” can be adapted to such different contexts. Whether the relevant actors are parties, legislators, or ministers, the basic insight is that a failure to internalize the full cost of their actions induces a pro-spending bias (see also Velasco 2000).

Yet, not all decision-makers have the same incentives. Alesina and Perotti (1996: 20–21) explain:

The constituencies of spending ministers are groups and industries who benefit from certain spending programs while, at least in theory, the constituency of the Treasury Minister is the “average” tax-payer. Thus the spending ministers do not internalize the aggregate costs of certain spending programs, while the Treasury has an incentive to internalize.

Hence, one possible response to the “common pool resource” problem could be to strengthen the role of decision-makers who are more likely to consider overall costs than spending ministers (Poterba and von Hagen 1999; Strauch and von Hagen 1999).

Von Hagen and Harden (1995: 772–775) present a much-cited model that centers on this very idea, and yields concrete recommendations for the design of the budgetary process. They model decision-making in a government consisting of several spending ministers, each of whom

gets funds that are used to produce activities in order to achieve a policy target. While each has an interest in achieving his or her policy target and minimizing the excess burden from taxation, each also receives a private utility gain from her budget allocation. Moreover, each spending minister only considers her constituency's share of the total excess burden—as posited by the “Law of $1/n$.” Indeed, if the process follows a bottom-up approach that allows each spending minister to separately draft a budget for his or her department, so that the total budget consists simply of the sum of all bids submitted by the spending ministers, the aggregate outcome resulting from this bottom-up process is larger than optimal for the government as a whole.

Von Hagen and Harden (1995) go on to show that when a minister without portfolio, who has an incentive to consider the overall impact of excess taxation, is given strategic power vis-à-vis his or her colleagues in spending ministries, the resulting amount of total spending is closer to the joint optimum than under the bottom-up process: the centralization of the budgetary process in the hands of an actor with incentives to internalize costs mitigates the common pool resource problem. A second solution they identify is for decision-makers to commit to binding fiscal targets, and they hint that the choice of approach is linked to the partisan composition of governments.

Hallerberg (2004) and Hallerberg *et al.* (2009) expand the latter idea and argue that the party system and the majority-minority dichotomy determine the fiscal framework. In two-party systems, governments with little internal discord delegate to a strong finance minister. This accounts for the dominant role of the Treasury in the United Kingdom under single party government. However, in more ideologically fragmented multi-party governments, coalition partners may be unwilling to delegate to a single individual from one of the governing parties, and instead prefer commitment to binding targets through a fiscal contract or coalition agreement. Practices adopted in countries such as the Netherlands or Finland illustrate the second approach. In addition, a “mixed” approach is possible with minority governments, involving delegation during the executive stage and a fiscal contract with supportive parties in the legislature. Finally, with low levels of political competition, degeneration to fiscal fiefdoms is likely, where institutional controls on the common pool resource problem are lacking.

Empirical evidence on the fiscal impact of budget institutions

Early empirical work on the effects of budget institutions focused on the American states. Examples include a study by von Hagen (1991) that casts doubt on the effectiveness of formal budget rules, an issue to which I return in the following section. Crain and Muris (1995) investigate how legislative committee structures affect fiscal policy outcomes, and work on fiscal adjustment has examined the role of budget institutions (Alt and Lowry 1994; Poterba 1994). Due to the relatively good availability of fiscal and institutional data, as well as the variation in practices and outcomes across a larger sample of comparable units, the American states provide a particularly good laboratory for such questions.

For a number of years, a major impediment to cross-national work on the fiscal consequences of budget institutions was the scarcity of comparative data, but this situation is changing (Hallerberg 2013). The most consistent data gathering effort is the fiscal governance database compiled for 15 European Union countries and covering the years 1985 to 2004 (Hallerberg *et al.* Strauch, and von Hagen 2009). The dataset contains detailed information on the use of fiscal targets and contracts, as well as the procedural arrangements that govern the formulation and execution of budgets, consistent with the authors' theoretical work.

Some other researchers have compiled time series data on the evolution of budget institutions. One example is work by Filc and Scartascini (2007) that builds on budget institutions data

collected for Latin American countries in the mid-1990s (Alesina *et al.* 1999). Other efforts include the Budget Practices and Procedures Database of the OECD, which in 2007 was extended to a large number of developing and transition countries. Several datasets have been compiled to assess fiscal transparency (for a review, see Wehner and de Renzio 2013). An impressively rigorous effort is the Open Budget Index initiative, which systematically tracks the evolution of budget transparency practices across almost 100 countries on a regular basis, going back to 2006 (International Budget Project 2006). The questionnaire also covers procedural aspects such as the role of the legislature and audit institutions in the budgetary process. Hence, the availability of data on fiscal institutions has been improving steadily.

Cross-national empirical work on the fiscal effects of budget institutions initially had a strong geographical focus on Western Europe. This work was driven by concerns in the run-up to the creation of the monetary union about the ability of different countries to achieve and maintain the required convergence in fiscal policy. Using data for European countries in the 1980s, von Hagen (1992) finds that certain institutions safeguard fiscal discipline, namely a strong position of the finance minister or prime minister in negotiations within the executive, a parliament with limited powers to amend the budget, and limited flexibility and strong finance ministry control during the implementation of approved budgets (see also Hallerberg 2004; Hallerberg *et al.* 2007, and Hallerberg *et al.* 2009). More recent work has expanded this research to Central and Eastern Europe, also finding evidence in support of the fiscal institutionalist argument (Gleich 2003; Yläoutinen 2004; Fabrizio and Mody 2006; Hallerberg and Yläoutinen 2010).

In one of the first cross-national studies on countries outside Europe, Alesina *et al.* (1999) look at a sample of 20 Latin American and Caribbean countries. They conduct a survey of their budget systems and classify them as “hierarchical” or “collegial.” Their empirical work suggests that more hierarchical institutions were associated with lower primary deficits in the 1980s and early 1990s (see also Stein *et al.* 1998; Hallerberg and Marier 2004; Filc and Scartascini 2007). Recent work on the region has contributed detailed case studies on the functioning and evolution of budget systems in several of the countries (Hallerberg *et al.* 2009). Overall, the empirical support for the fiscal institutionalist argument is impressive, given that these studies cover different time periods and country samples, and that they are based on different although related institutional measures.

One of the cases most cited in support of the fiscal institutionalist argument is Sweden. Prior to far-reaching institutional reforms, Sweden had a highly fragmented budgetary process that imposed little discipline on spending requests from line ministries, and where Parliament made substantial upward amendments to spending on a regular basis. In the early 1990s, the country was hit by a pronounced macroeconomic crisis, which led to a sharp deterioration of the deficit. A parliamentary commission tasked with reviewing budget procedures considered the unflattering findings of a study prepared by a finance ministry official that assessed Sweden’s budget institutions on the basis of von Hagen’s (1992) framework. It found that Sweden had the second worst set of institutions among thirteen countries, only slightly ahead of Italy (Molander 1999: 202–208).

Sweden implemented a wide range of changes in the mid-1990s (Hallerberg 2004: 160–166). The role of the minister of finance during budget preparation was strengthened with new powers to propose ceilings on the budget bids of line ministers (Blöndal 2001). The move to top-down budgeting also changed the sequence of the parliamentary process (Wehner 2007). Parliament would from now on vote first on budget totals before deciding individual appropriations. The Finance Committee gained responsibility for the aggregate spending totals as well as frames for each of the 27 expenditure areas. Based on the work of the Finance Committee,

fifteen sectoral committees then make allocational proposals within their approved ceilings. Sectoral committees may propose to shift funds between items within an expenditure area, but they may not breach the total set for that area. In effect, a hard budget constraint has been imposed on sectoral committees.

By any standards, Sweden managed an impressive fiscal turnaround. The gap between general government revenues and expenditures had widened dramatically at the beginning of the 1990s, with the deficit exceeding 11 percent of Gross Domestic Product in 1993. By the end of the decade, the economy had stabilized and the government was back in surplus. Commentators attest that fiscal discipline has improved markedly (Blöndal 2001: 42; Molander 2001). The parliamentary process no longer produces spending increases on a regular basis (Wehner 2007, 2013). Sweden is one of the very few countries in Europe that has successfully contained deficits and the stock of debt in the wake of the 2008 financial crisis.

In addition to the common pool resource perspective, the role of budget institutions has also been examined in the context of fiscal adjustment. The focus in this literature is not on the *level* of spending, deficits, or debt, but on their *change* in response to an economic shock. Roubini and Sachs (1989) and Alesina and Drazen (1991) have shown that politically fragmented governments are less likely to adjust quickly, while Tsebelis and Chang (2004) find that the number of partisan veto players and their ideological dispersion relates to changes in budget composition over time. Some empirical papers find that budget institutions can help speed up fiscal adjustment (Alt and Lowry 1994, Poterba 1994, Guichard *et al.* 2007).

Overall, detailed comparative work on budget institutions has often had a regional focus, especially on Europe and also Latin America. This allows researchers to take into account the very different political and constitutional environments in which budget institutions are embedded. For instance, parliamentary government in Western Europe is combined with different electoral systems that affect the likelihood of coalition government and hence the choice between the delegation and commitment forms of fiscal governance (Hallerberg *et al.* 2009). Given the prevalence of coalition government in Eastern European countries, commitment-based approaches would seem most appropriate. The adoption of a suitable approach to fiscal governance is associated with better fiscal outcomes (Hallerberg and Yläoutinen 2010). Presidential systems in Latin America are arguably more suitable for centralizing budget preparation within the executive, but may require coalition building with legislative actors. Hence, executive-legislative relations are an important focus of scholarly inquiry into the role of budget institutions in these countries (Hallerberg and Marier 2004). The regional focus of much of the work in this literature thus reflects the importance of accounting for political context, especially the form of government as well as the electoral system and the party system.

Nonetheless, there remains scope to explore the extent to which fiscal institutionalist arguments generalize across more diverse samples. Work by Wehner (2010b, 2010c) on legislative budget institutions and fiscal policy outcomes is based on larger and more diverse samples, and yields results that are in line with the literature: countries where the legal framework prohibits legislative amendments to the budget that increase spending or deficits tend to have more prudent fiscal outcomes. There is scope to further expand the comparative ambition of research on fiscal institutions.

In sum, there is substantial evidence that the quality of budget institutions is associated with various indicators of fiscal performance. Of course, budget institutions are not randomly assigned, and none of these analyses can make a convincing claim to detect causal effects. The example of Sweden in this section demonstrates reciprocal causation: fiscal conditions on the one hand can provide the impetus for reform, while on the other hand the resulting institutional changes can also impose constraints that affect fiscal decisions. Since budget procedures are

typically slowly changing, existing procedures are likely to constrain decision-makers at least in the short run.

The complexity of institutional bundles

More recent work highlights the importance of paying close attention to the nuances of designing specific budgetary procedures, and how different bundles of institutions combine to affect policy outcomes. In part, this work stems from a growing realization that fiscal institutions that are meant to constrain decision-makers and safeguard fiscal discipline can also have unintended consequences. For example, Milesi-Ferretti (2003) examines the effect of fiscal rules, which impose numerical constraints on fiscal aggregates such as deficits or debt (Kopits and Symansky 1998). His analysis suggests that fiscal rules may induce “creative accounting” rather than genuine fiscal adjustment, when they are imposed in a context of low budget transparency.

A body of empirical work documents the use of “fiscal gimmickry” in the European Union. For instance, Vincent Koen and Paul van den Noord (2005) find that Greece has made more extensive use of one-off measures and creative accounting than any other of the 15 countries in their study. Their calculations show that Greece qualified for membership of the Eurozone only because it embellished its public finance statistics so that it met, on paper, the required fiscal targets. Interestingly, Greece also has the lowest levels of budget transparency among the Eurozone countries. Alt *et al.* (2014) offer the first direct empirical test of Milesi-Ferretti’s (2003) hypothesis, using stock-flow adjustments to proxy fiscal gimmickry, and confirm his prediction. They show that fiscal rules have harmful side effects when they are imposed in countries with poor budgetary reporting practices.

In response to these problems, and acknowledging that fiscal rules do not operate in isolation, international financial institutions and European policy-makers started to promote the creation of independent fiscal watchdogs to enforce fiscal rules (Debrun *et al.* 2009; Hagemann 2010; Cangiano *et al.* 2013). However, the experience of fiscal councils has been mixed. In some instances, independent institutions have greatly enhanced the credibility of fiscal policy, for instance the Central Planning Bureau in the Netherlands. Khemani (2007) shows that in India, intergovernmental transfers distributed by an independent body were insulated from partisan pressures. However, a number of fiscal councils have encountered significant difficulties when they became too independent for the taste of their governments, for instance in Canada and Hungary, as well as Sweden (Calmfors and Wren-Lewis 2011; Coene and Langenus 2011; Kopits 2011).

This discussion illustrates a growing sensitivity of fiscal designers that the effects of institutions may be more complex than often thought at first. Marcela Eslava’s (2011: 662) summary of the current state of knowledge about the effect of numerical fiscal rules reflects this new awareness:

[T]he response to these rules varies widely across countries, apparently in relation to other budgetary institutions [...] and the political context; it is plausible that these differential environments may also change the incentives to engage in creative accounting and the feasibility of doing so. The evidence seems to suggest that effective rules would need to be more comprehensive, in the sense of imposing strict limits not only on deficits but also on debt, and covering the different possible sources for deficits. However, more comprehensive rules are also more complicated rules, and the possibility of enforcing them seems questionable. In that sense, it seems that the use of fiscal rules should be called into question in a more general sense.

This realization has given rise to efforts to strengthen fiscal transparency. Principles for budget transparency were established by international organizations some time ago (Kopits and Craig 1998; International Monetary Fund 1998; OECD 2002). In the context of the recent Eurozone debt crisis, these efforts are receiving renewed attention (International Monetary Fund 2012). Whether this is enough to fulfill the promise of rules-based fiscal policy remains to be seen (Alt *et al.* 2014).

Other institutional “solutions” to the common pool resource problem have also not always held up to thorough empirical as well as theoretical analysis. One institutional adjustment that has been extensively promoted in recent years is the use of “top-down” decision-making procedures (Kim and Park 2006; Ljungman 2009). Under such a process, totals are set prior to the distribution of funds to sectors and programs. Von Hagen (1992: 36) initially promoted the use of top-down budgeting and suggested that fiscal discipline is enhanced when a vote on aggregate spending precedes allocational decisions. However, Ferejohn and Krehbiel (1987) demonstrate that such a process may sometimes result in relatively large budgets. Serritzlew (2005) extends and confirms their analysis. Alesina and Perotti (1996: 12) comment that this is “a useful warning against oversimplifying the effect of certain procedures.”

Subsequently, von Hagen revised his initial claim and argued that it is not a reordering of the voting sequence that is decisive, as it has no impact on the share of the tax burden that actors consider, but rather the centralization of decision-making (Hallerberg and von Hagen 1997; Ehrhart *et al.* 2007). Indeed, the model by Ferejohn and Krehbiel (1987) assumes that the same actors make both the aggregate as well as allocational decisions. This is the key to a more refined argument about the benefits of top-down budgeting: The effect of the two-step process in terms of fiscal discipline depends crucially on who makes the first decision on aggregates. More specifically, top-down budgeting is likely to systematically contain overall spending only when the initial decision on aggregates is delegated to actors who are more likely to consider total costs than those who decide individual spending items (Wehner 2010b: 106–109). Sweden’s redesigned parliamentary procedures illustrate the point.

In addition, there is limited work on how budget institutions combine with other political variables to influence policy outcomes. One aspect of this, discussed earlier, relates to how the party system affects the most appropriate form of fiscal governance (Hallerberg *et al.* 2009). But questions remain about how budget institutions interact with such political factors. For instance, Persson *et al.* (2007) argue that, in parliamentary democracies, proportional electoral systems increase the likelihood of coalition governments compared to majoritarian electoral systems. They also show that electoral competition inside coalitions engenders greater public spending than single party government. This suggests that budget institutions have an especially important role to play in mitigating a pro-spending bias under multi-party government.

Scholars have paid limited attention to how “size” fragmentation (the number of actors) is mediated by “procedural” fragmentation (the structure of the process in which they interact). Some authors have assessed the impact of both size and procedural fragmentation in the same study. Examples include Perotti and Kontopoulos (2002), Woo (2003), and Fabrizio and Mody (2006). However, none of them investigate the interaction between these different types of fragmentation. Some cross-national studies have considered how budget institutions interact with other variables, but not measures of size fragmentation (Hallerberg and Marier 2004; Alt and Lassen 2006a, 2006b).

There are only a few examples of empirical work that directly test the interaction between “size” and “procedural” fragmentation. Using data for a diverse panel of industrialized and developing countries, Wehner (2010c) shows that partisan fragmentation in the legislature is

associated with higher deficits only when it is not moderated by limits on parliamentary amendment authority in budgetary matters. Focusing on executive fragmentation, Vanberg and Martin (2012) construct a “budgetary constraint index” for a panel of European countries and show that the number of government parties is positively associated with higher public spending only if budgetary institutions do not impose discipline. Looking at the interaction the other way around, De Haan *et al.* (2013) find that the effect of budget institutions on deficits is conditional on the ideological fragmentation of the government. Further work along these lines is important for advancing our understanding of the conditions under which institutional engineering might be most effective.

Conclusions

Theories of pork-barrel politics developed in the context of the United States have given rise to a cross-national literature on the common pool resource problem in budgeting. This literature proposes institutional solutions for settings where budgetary decision-making is fragmented across diverse decision-makers with limited incentives to internalize the cost of their actions. To contain a pro-spending bias in budgetary decisions, one solution is to centralize authority over fiscal policy in the hands of the finance minister or head of government. In the context of multi-party coalition government, this may not be feasible. Here, the use of binding fiscal targets, enshrined in coalition agreements, is an alternative option.

Empirical support for the predictions of the theoretical literature on the common pool resource problem in budgeting is impressive. Initial work focused on Western Europe, but increasingly geographical coverage has been extended to Eastern and Central European countries, Latin America and the Caribbean, as well as more diverse samples of countries. The conclusions of this empirical literature, despite its diversity in samples and measures, are consistent and strongly supportive of the theoretical literature.

However, a more recent strand of the literature cautions against an excessively naïve belief in the ability of institutional engineers to contain political pressures for fiscal profligacy. Experience with numerical fiscal rules suggests that they may have serious and systematic side effects—in the case of the European Union, countries with low standards of budget transparency have systematically circumvented these rules through creative accounting and, occasionally, intentional misreporting of fiscal data to statistical authorities. This highlights that particular fiscal institutions do not operate in isolation. Other institutional reforms, such as top-down budgeting procedures, also have more nuanced effects than initially anticipated. We need to understand much better how exactly specific institutions work as part of complex bundles, and under what conditions they are most likely to be effective. This also requires further analysis of interactions between budget institutions and political variables. Hence, the need for comparative research on fiscal institutions is far from saturated.

In the context of mounting pressures for fiscal consolidation and multiple initiatives at institutional engineering of the budgetary process by international organizations and supranational bodies such as the European Union, a better understanding of the effects and efficacy of proposed procedural “solutions” is arguably more urgently required than ever. Otherwise, procedural reforms guided by the latest public financial management fads may well deliver disappointing results, or do more harm than good. It is far from clear whether externally imposed institutional reforms will be as effective as those engendered by domestic political pressures (Andrews 2013). For instance, scholars should explore whether the credibility of fiscal rules as signals of commitment to fiscal discipline depends on whether external actors forced governments to adopt them.

Many more questions remain. We still know rather little about the evolution and determinants of budget procedures, and why cross-country patterns differ so markedly. Often, there is a rush to include budget institution on the right-hand side of regression equations, instead of thinking about them as dependent variables as well. In Europe, such questions have been linked to the electoral system and the party system of a country. It is far less clear what this means in other contexts, such as presidential systems, as well as developing and other countries where informal institutions can be more important than formal procedural rules. Tracing the origins of budget institutions requires more systematic historical work, for instance on countries that have seen significant adaptations over time. The role of colonialism in spreading specific budget institutions also deserves attention in comparative inquiries (see Lienert 2003).

Another area for future comparative research involves diversifying the dependent variable. While fiscal policy is of central interest in examining the performance of political systems, other outcomes are arguably no less important. For instance, how do budget institutions affect the quality of public expenditures (Stasavage and Moyo 2000)? What is their effect on corruption and government accountability (Ablo and Reinikka 1998)? When are executive-favoring budget institutions, promoted in much of the fiscal institutionalist literature, inimical to democracy (Cox 2013)? It may well be that there are essential trade-offs between such different outcomes, which can be overlooked with an overly narrow research focus on fiscal aggregates. Comparative political economists are ideally positioned to develop such new perspectives.

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LABOR MARKET INSTITUTIONS AND ECONOMIC PERFORMANCE

Irene Menendez and David Rueda

The last few decades have seen the emergence of a large amount of literature examining the links between labor market institutions and macroeconomic outcomes. The interdisciplinary efforts of economists, political scientists and sociologists have given rise to a theoretically lively and methodologically diverse literature. This research attempts to answer a range of important questions. Some are policy related: what can governments do in the face of economic challenges? What defines a state's capacity to meet such challenges? Others are institutional: do some institutions promote lower rates of inflation and unemployment or higher rates of growth than others? What are the trade-offs? Can we expect technological change and globalization to lead to institutional convergence? And what factors are likely to affect the adjustment of a political economy in the face of such challenges?

Early research focused on understanding the relationship between specific features of collective bargaining systems and macroeconomic adjustment. Given the failings of conventional macroeconomic models of wage and price behavior to explain variation in adjustment to the macroeconomic shocks of the 1970s, social scientists began to examine the impact of alternative institutional structures of economic performance and initially concluded that so-called "corporatist" arrangements produced better macroeconomic outcomes. Spurred by the gap in employment performance between European countries and the US in the early 1990s, on the other hand, many economists endorsed the view that rigid labor market institutions were at the root of unemployment.

In turn, research by political scientists has contributed important insights by emphasizing that labor market institutions (and policies) and macroeconomic outcomes clustered in distinct welfare regimes (Esping-Andersen 1990) or varieties of capitalism (Hall and Soskice 2001). However, such studies disagree on the relative importance of different institutional and political characteristics driving differences in macroeconomic outcomes.

In this chapter, we argue that the study of labour market *dualization*, or the differential treatment of labour market insiders and outsiders, provides fertile ground for research on labour market institutions and economic outcomes. Recent research has documented distinct patterns across and within countries that need systematic explanation. First, advanced industrialized economies have not converged towards a neoliberal labor market model characterized by labor market flexibility and limited social protection systems. Rather, a trend towards structural segmentation differentiating labor market insiders and outsiders is discernible across these economies

(Emmenegger *et al.* 2012). Second, labor market dualization translates into very different economic (and social) outcomes across countries. Advanced economies exhibit strong variation in the distributive implications of policy areas (such as labor market regulation and social protection regulation). Continental European economies, for example, rely on a combination of labor market regulation to circumvent labor costs in the private service sector (Eichhorst and Marx 2012) and social protection systems that appear to *exacerbate* labor market divides (Häusermann and Schwander 2012).

These changes have important implications for the study of labor markets. To what extent can existing theories based on institutional rigidities and varieties of capitalism account for the recent transformations apparent in most OECD economies? What particular institutional features matter and why? And how can theory move beyond existing views about the role of institutions to explain differences in labour markets? In what follows, we review the major approaches underpinning the study of labour market institutions and economic performance and assess the degree to which they are able to account for new labour market developments.

The consensus view: the labor market rigidity hypothesis

Labor market institutions are the set of implicit or explicit rules, norms or contractual arrangements and organizations that govern labor transactions. These rules shape demand and supply in the labor market. Demand for labor is determined not only by conventional market elements such as output prices, productivity of labor and nominal wages, but also by regulations relating to layoffs, work time and collective wage-setting rules. In turn, the supply of labor is affected by passive labor market policies (such as unemployment benefits). Economists often argue that labor market institutions are at the root of unemployment. In this view, institutional arrangements have contributed to restricting the scope of market clearing, giving rise to “rigidities” in a number of areas that limit the freedom of employers to re-organize and adjust the size and wage of their workforce in response to market fluctuations.

The intellectual rationale for the labor market rigidity thesis is solidly rooted in neoclassical economic theory, which maintains that under perfect competition the optimizing decisions of market participants produce efficient outcomes for both individuals and society. In such models, full employment is guaranteed provided there are no rigidities. The introduction of a minimum wage above the competitive wage provides one example. Although some employees benefit from an improvement in their incomes (and labor income on aggregate may rise), for society as a whole the gain is limited. Most of the gain to labor comes from the redistribution of income from other productive inputs. Higher wages also reduce employment as employers substitute other inputs for the more expensive labor, and consumers respond to higher production prices by reducing consumption. Lastly, while producers affected by the minimum wage use too much capital, energy and other inputs relative to society’s endowment, labor released from production is underutilized, being either unemployed or employed in an unregulated sector at a lower wage. Thus, a minimum wage raises costs, reduces output and results in a smaller and less desirable set of consumption goods than was otherwise available Block *et al.* (2005: 97–98).

In an influential article, Siebert (1997) identified a number of ways in which labor market institutions can influence market clearing and negatively affect employment. First, they can weaken the demand for labor, making it less attractive to hire a worker by explicitly pushing up the wage costs or by introducing a negative shadow price for labor. Second, they may distort the labor supply, for instance, by increasing the reservation wage through benefits and making the unemployed both more reluctant to actively seek work and to accept available jobs

(Nickell 1997; Holmlund 1998). Lastly, institutions can weaken the equilibrating function of the market mechanism (for instance, by influencing bargaining behavior). In short, if markets are believed to be competitive, social welfare is best attained by policies that lessen market imperfections and remove barriers to competition.

Most research in labor economics thus starts from the premise that rigidities that reduce competition increase unemployment, and goes on to ask whether labor market institutions affect unemployment directly by raising the equilibrium unemployment rate (Nickell 1997; Elmeskov *et al.* 1998) or indirectly, by magnifying the adverse consequences of exogenous shocks (Blanchard and Wolfers 2000) or by exacerbating the effects of other institutional rigidities. A first set of studies in the 1990s and early 2000s focused on the impact of union density, employment protection legislation, the amount and duration of social benefits and employment tax rates (Siebert 1997; Layard *et al.* 1991; Nickell 1997; Nickell *et al.* 2005). A longstanding literature on collective bargaining viewed unions as rent-seeking labor market cartels, whose main function is to raise wages above market-clearing levels (Johnson and Mieszkowski 1970; Johnson 1975).¹ In this view, a high share of workers belonging to unions was expected to increase unemployment, particularly in contexts of a highly elastic labor supply.² In turn, employment protection legislation was argued to weaken demand for labor by limiting the freedom of employers to quickly adjust to market fluctuations (Addison and Teixeira 2003). Standard neoclassical arguments claimed that employers facing high employment protection would be reluctant to hire new workers because of high potential severance costs (Van Long and Siebert 1983). However, standard theory also points out that the effects of employment protection legislation are likely to be indeterminate, as firms faced with high firing costs smooth their employment, leading to lower turnover, and lower flows in and out of employment (Bertola 1992).

A related set of arguments focused on the role of unemployment benefits in shaping wages and unemployment. An influential model incorporated the ability of unions and firms to set wages and prices, and started from the premise that unemployment benefits are always raised or lowered in line with wages, so as to keep the replacement ratio constant (Layard *et al.* 1991). In this view, unemployment benefits increased the reservation wage and made the unemployed both more reluctant to seek or accept jobs (Nickell 1997; Holmlund 1998). This disincentive, in turn, increased the bargaining power of both new job applicants and current employees. Higher wages in turn reduced labor demand and thus raised unemployment (Holmlund 1998). This framework implied that the greater the real wage rigidity, the greater the equilibrium rate of unemployment (or Non Accelerating Inflation Rate of Unemployment). Later research aimed to substantiate the view that unemployment was caused by rigid labor market institutions (OECD Jobs Study 1994; Siebert 1997; Elmeskov *et al.* 1998; Belot and van Ours 2001, 2004; Saint-Paul 2002; Nickell *et al.* 2005; Bassanini and Duval 2006). This research suggested that differences in institutional arrangements explained the increasing gap in employment performance between large European countries and the US in the 1990s (Bruno and Sachs 1985, Blanchard and Summers 1986, Lindbeck and Snower 1988).

A major limitation underlying these studies concerned their inability to explain why the evolution of labor market institutions in continental European countries has failed to match the trajectory of unemployment. Rigid labor market institutions were already present in the 1970s and did not become more so in the 1980s (Solow 2000; Blanchard and Wolfers 2000). As succinctly put by Solow (2000: 5),

[at] the crudest level, the timing is wrong. One of the two big increases in unemployment took place in the early eighties, although there was no change in labor-market

regulation to account for it. The large continental economies do not seem to have suffered from noticeably more rigid labor markets during the high-unemployment 1980s than they did in the low-unemployment 1970s.

A second set of arguments argued that labor market institutions have an indirect effect on unemployment by amplifying the adverse consequences of exogenous shocks. In an influential paper, Blanchard and Wolfers (2000) argued that labor market institutions affect the impact of shocks on unemployment as well as the persistence of unemployment in response to shocks, and that this could help explain why the same institutions were apparently not employment-unfriendly in previous decades. A slowdown in productivity growth could result in unemployment unless wages are adjusted downwards, and this adjustment may be more difficult in systems with strict employment protection or generous unemployment benefits. Once the adverse shocks generate an increase in unemployment, such institutions may prolong the time needed for unemployment to return to its normal level.

The evidence of a causal connection between labor market rigidities and unemployment levels, however, has been remarkably inconclusive.³ The empirical literature has reported significant impacts for employment protection, benefit generosity and union strength, but the effects of such institutions do not appear to be robust, and exhibit widely differing coefficients and levels of significance. While there appears to be some agreement on the unemployment effects of unionization (Nickell and Layard 1999; Nickell *et al.* 2005; Baccaro and Rei 2007), the evidence on other aspects of wage determination remains unconvincing. Using a cross-section of 20 countries over the period 1983–88 and from 1961 to 1995 for OECD countries, Nickell and Layard (1999) and Nickell *et al.* (2005), respectively, find that higher union density and coverage raise unemployment, while union and employer coordination lower it. However, Baccaro and Rei (2007) look at 18 OECD countries between 1960 and 1998 and, with the exception of union density, find little support for a causal connection between labor market rigidities and negative economic outcomes.⁴ The findings for employment protection legislation are equally inconsistent. While Nickell and Layard (1999) find a positive effect of employment protection legislation on unemployment, Nickell (1997) finds no effect.⁵ Nickell *et al.* (2005) and Blanchard and Wolfers (2000) find a negative effect, while others tend to find no robust positive association with unemployment (Baccaro and Rei 2007).

The labor market rigidity hypothesis suffers from important theoretical and empirical limitations. One major limitation of these arguments is the absence of political mechanisms linking micro-level labor market strategies and aggregate outcomes. Specifically, such studies tend to focus on the outcome level at the expense of the *politics* of unemployment. Thus, the rigidity-cum-unemployment argument tends to focus on the micro-level strategies of labor market actors (employers responding to changed economic conditions, workers reluctant to look for work), and political processes play a minor role. However, whether micro-level responses translate into greater or lesser unemployment is contingent on the links between the micro-level, broader institutional context and the political articulation of labor market interests.

A second critique revolves around the inability of such theories to explain why countries with similar levels of institutional rigidities exhibit different levels of unemployment. Why did similar institutional reforms not lead to similar outcomes in different countries? Increases in bargaining coordination between the mid 1980s to the mid 1990s brought about wage moderation in the Netherlands (Visser and Hemerijck 1997) and Ireland (Baccaro and Simoni 2007), but had little effect on unemployment performance in Italy (Baccaro 2000; Baccaro and Rei 2007). In addition, labor markets in those countries that were able to lower unemployment rates in the 1990s (Ireland, Denmark, the Netherlands and the United Kingdom) are considerably

more regulated than in the U.S. In none of these countries was unemployment accompanied by a reduction in institutional protection.

Third, the composition is arguably as important as the aggregate level of unemployment. Countries with similar levels of unemployment vary significantly in the degree to which different subgroups in the population are affected, and often distribute labor market policy and regulations unevenly across areas of policy. The incidence of unemployment exhibits a great degree of variation within the OECD (with respect to gender, age or education). Some countries target social policy to narrow political constituencies, while others may distribute broadly across the entire population. The focus on aggregate levels of unemployment thus overlooks important information about the distributional implications of a number of labor market policies. Recent research aims to overcome this reliance on outcome-level analyses by employing measures that capture policy differences in the level of labor market regulations and social protection (Emmenegger *et al.* 2012).

How well can neoclassical arguments explain labor market developments in recent years? In their study of labor market divides in industrialized democracies, Emmenegger *et al.* (2012) find significant variation in the degree to which structural job growth in the service sector translates into economic and social divides (unemployment and inequality). Their findings emphasize inter- and intra-regime (cross-national) differences across policy areas. Continental European economies rely on different aspects of labor market regulation to circumvent labor costs in the private service sector (Eichhorst and Marx 2012). In distributional terms, social protection systems in continental European economies appear to *exacerbate* labor market divides (Häusermann and Schwander 2012). However, Howell *et al.* (2007) report unemployment rates for 2003 with OECD data in three groups of countries, and show that while corporatist economies of “high-unemployment Europe” show worse employment performance than the Liberal economies, “low-unemployment Europe” exhibits moderately good performance and comprises both corporatist and heavily regulated countries.⁶ Such findings point to the need for a more nuanced theoretical approach to understand the conditions under which these effects are likely to prevail.

The role of institutions: the corporatist and partisan hypotheses

Spurred by the wave of inflation that gripped most developed economies in the 1970s, the “neo-corporatist” hypothesis explicitly sought to address the role of political mechanisms neglected by the neoclassical literature (Schmitter and Lehmbruch 1979; Goldthorpe 1984). Two conceptions of neo-corporatism emerged in the 1980s. In an influential article, Schmitter (1974) defined neo-corporatism as a form of interest representation distinct from pluralism. Subsequently, Lehmbruch (1977, 1979) defined neo-corporatism as a form of policymaking in which concertation assumed central importance. Both conceptions emphasized the structured participation of interest organizations in policymaking.

Corporatist arrangements are argued to produce implicit or explicit “social contracts” in which unions restrain wage demands in exchange for policy concessions from the government, leading to lower inflation and unemployment. This conception of corporatism is based on an Olsonian logic of collective action, proposing that institutions that are encompassing in relation to firm or industry are better able to internalize the economic effects of their wage settlements (Olson 1965; Calmfors and Drifill 1988; Golden 1993). Within this framework, features such as the centralization, concentration and density of labor and employer associations become particularly important in shaping macroeconomic outcomes (Calmfors and Drifill 1988; Golden 1993).

A first set of studies emphasized the role of the centralization of wage bargaining in shaping employment levels. In a prominent study, Calmfors and Driffill (1988) argued that good economic performance would result if wage bargaining took place either at the company or at the national level. In the first case, the actions of unions would not be powerful enough to distort efficient market outcomes. In the second, unions would be encompassing enough to act in favor of the interests of society as a whole. Poor economic performance would be associated with wage bargaining at the industry level because wage bargaining would be powerful enough to affect the market equilibrium outcome while insufficiently encompassing to take society's interests into consideration. The implications of this argument for the evolution of unemployment were subsequently explored by Rowthorn (1992) and Pekkarinen *et al.* (1992). Extending the previous framework, Rowthorn contended that there is, particularly in the early 1980s, a U-shaped relationship between the centralization of wage bargaining (i.e. centralized unions and employer associations) and employment.

A related line of argument suggested that the degree of wage coordination is more relevant to macroeconomic outcomes than the level of wage bargaining (Soskice 1990; Golden 1993; Hall and Franzese 1998). In this view, increasing levels of union concentration (a small number of unions that do not compete) are associated with better employment outcomes. The concentration logic stresses collective action by considering that fewer actors reduce coordination problems and that unions that do not compete "have fewer incentives to engage in wage militancy" (Golden 1993: 441). Although this approach emphasizes a different factor, the analysis is compatible with the centralization argument and the conclusions are similar (the main difference being the suggestion by the concentration framework that a linear relationship may be more accurate than a U-shaped one).

Empirically, many studies found support for the beneficial effects of corporatist institutions. Exploring macroeconomic responses in the wake of economic crisis in the 1970s, most authors concluded that a positive relationship existed between the degree of corporatism and macroeconomic performance (Flanagan *et al.* 1983; Pekkarinen *et al.* 1992; Scharpf 1997). Corporatist countries were found to be more successful at controlling inflation and adjusting in periods of crisis than less corporatist or noncorporatist ones (Western 1991; Crepaz 1992). However, others have found that corporatist institutions do not make much of a difference. Analyzing eight small countries, Woldendorp (1997) showed that countries ranking higher in the neo-corporatist scale do not perform better than countries ranking lower. Finally, exploring differences in macroeconomic outcomes in contexts of low monetary autonomy, other scholars have explored how institutions interact with monetary regimes (Hall and Franzese 1998; Iversen *et al.* 2000). These studies emphasize the relationship between coordinated bargaining and central banks, and argue that "the key to effective wage and labor-cost setting is coordination among actors and an emphasis on cost containment rather than on centralization and elaborate redistributive goals" (Molina and Rhodes 2002: 311–312).

The neo-corporatist approach addresses previously neglected and important issues, but suffers from a number of theoretical limitations. While accounts emphasizing changes in economic context may help identify the presence of new incentives for governments, employers and unions to engage in concertation, they do not explain how or why attempts at concertation achieved greater or lesser degrees of success in different countries. Scholars have noted that many countries that successfully coordinated policies of wage restraint could not rely on the strong, centralized interest associations prevalent in the 1970s (Hemerijck and Schludi 2000). In other words, such accounts failed to address the mechanisms or processes that linked institutional structures with macroeconomic outcomes. Subsequent research has focused on corporatism as

a policymaking process and explored the goals and strategic behavior of actors rather than institutional features (Molina and Rhodes 2002).

A prominent literature stresses the importance of political parties in shaping outcomes such as unemployment and inflation. The traditional partisanship school emphasized that political parties promote the interests of their core constituencies. In this view, labor is assumed to be more affected by unemployment than capital owners, and social democratic governments are expected to design economic policies that promote employment. In turn, inflation is assumed to disproportionately influence upscale groups, so conservative governments are expected to promote policies that reduce price increases (Hibbs 1977; Alt 1985). Stressing the capacity of left power to mobilize resources to promote full employment, Esping-Andersen (1990) claimed that a strong presence of labor interests—including high union density, centralization of wage bargaining and a high share of seats held by social democratic parties—in Nordic welfare regimes would result in higher levels of employment relative to Liberal and Continental welfare regimes. The former were more likely to create employment through the welfare state and the public sector. In Continental welfare regimes, the existence of a high social wage and centralized industrial relations were expected to hinder the development of a large low-wage service sector. However, this would likely come at the expense of a large “outsider surplus population” with no access to the labor market. In contrast, the minimalist nature of the welfare state together with decentralized industrial relations in Liberal countries was expected to facilitate the emergence of a large, low-wage service sector segment. In the absence of comprehensive employment and training programs, these workers were likely to remain in such low-wage service sectors. A related view emphasizes the role of social democratic governments in promoting active labor market policies to attain both equality and economic efficiency in contexts where orthodox left policies are considered unfeasible (Boix 1998; Huo *et al.* 2008).

Partisan and power resources theories have provided key insights to explain differences in macroeconomic outcomes, but suffer from a number of shortcomings. First, at the root of both approaches is a zero-sum conflict among workers and employers (Carnes and Mares 2007: 873). Workers are assumed to demand employment friendly policies to offset their disadvantaged position in the labor market. In contrast, employers are expected to resist all policies that weaken their absolute authority over workers (Esping-Andersen 1990). Second, both assume that labor and social democratic parties are internally homogeneous and relatively context insensitive. Yet both labor and parties have been shown to vary greatly over time and across countries, and can be divided internally. These studies also overlook the electoral constraints under which parties operate. Rueda (2005) stresses the electoral dilemmas faced by social democratic parties, who have to cater to the interests of insiders (at the expense of outsiders), and have thus not been univocal in implementing measures to reduce unemployment for all workers.⁷

The literatures on neo-corporatism and power resources offer limited guidance in understanding the development of labor markets across and within the universe of institutional regimes. Recent scholarship has identified considerable variation in the degree to which continental European economies are capable of integrating low-skill labor in what was claimed to be an adverse institutional scenario. Research shows that while these economies are constrained by institutional barriers to hire at low wage levels, they have developed different strategies to overcome such obstacles (Eichhorst and Marx 2012). It also provides little guidance to understand the preferences and strategies pursued by labor-based parties in OECD countries. How do social democratic parties weigh ideological concerns and electoral calculations (Carnes and Mares 2007: 874)? Given heterogeneity in the support base of many of these parties, what role does labor market policy play in crafting together such heterogeneous coalitions?

The dualization hypothesis

Recent scholarship on the evolution of labor market institutions in advanced industrialized economies offers a number of insights into such questions. It challenges the theoretical assumptions and empirical results of both neoclassical and welfare regimes scholarship, and explores the conditions under which different labor market actors can be expected to support greater or lesser flexibility. A first set of studies seeks to identify the interests of actors other than (insider) workers in labor market policy.

Recent work by political economists and political scientists has focused on insider-outsider politics in an attempt to highlight the mechanisms leading to the emergence of outsiders and the translation of such cleavages into economic and political outcomes (Emmenegger 2009; Rueda 2005, 2007). In his study of labor market policy in industrialized democracies, Rueda (2007) provides a strong critique of the thesis that social democratic governments care more about high employment than low inflation. He examines the extent to which social democratic governments promote policies that further employment for the unemployed as opposed to job security for the employed insiders. He finds that social democratic parties have become staunch supporters of insider interests—at the expense of outsiders demanding active labor market policies, the relaxing of employment protection legislation and training programs.

This approach draws on insights from non-neoclassical segmentation economic theories that emphasize heterogeneity among labor market actors. Partly in response to the failings of neo-classical theory to account for the workings of labor markets in advanced democracies, dual labor market theory argues that labor markets are composed of a primary sector with well paid, stable and secure jobs and a secondary sector marked by poorly paid, unstable employment from which workers would find it difficult to escape due to the intermittent nature of employment (Piore 1979; Döringer and Piore 1971). The economic insider-outsider approach developed such insights and stressed that labor market insiders and outsiders have partly conflicting interests and that a potential for cross-class coalitions exists between insiders and employers (Lindbeck and Snower 1988; Blanchard and Summers 1986). Rueda's (2007) conceptualization of labor as a heterogeneous labor market actor has important theoretical implications. In this account, conflict over labor market policy and outcomes does not pit a disadvantaged labor force against employers.

Building on these insights, a second set of studies questions the assumption of zero-sum conflict between employers and workers over the introduction of flexibility in labor market policy. In their study of the causes and consequences of dualization, Emmenegger *et al.* (2012) document the extensive variation in labor market divides that characterizes the labor markets of advanced industrialized democracies. The authors argue that while structural trends such as globalization and de-industrialization are common to most countries, the resulting insider-outsider divides vary considerably across welfare regimes (Häusermann and Schwander 2012) and within welfare regimes (Eichhorst and Marx 2012). Eichhorst and Marx (2012) identify five different ways (“transformative pathways”) used by continental European regimes to circumvent existing rules and costs associated with standard employment. They find that (1) defection from permanent contracts plays a role in labor market dualization in France and the Netherlands; (2) defection from full-time jobs is particularly pronounced in the Netherlands but also in Austria and Germany; (3) defection from dependent employment in the form of precarious self-employment is common in Belgium and France; (4) increasing wage dispersion to integrate low-skilled workers into the labor market is prominent in Austria and Germany; and (5) large government-sponsored labor-subsidizing schemes are prominent in France and Belgium. The heterogeneity in the type of labor divides within welfare regimes emphasizes the degree to

which both labor market policies and outcomes are likely to be country-specific, and has given rise to two questions. A first question concerns the specification of preferences of labor market actors. A second question concerns the coalitional dynamics that are likely to emerge.

What are the conditions under which unions are driven to support flexible labor market policies? When do the benefits of such policies outweigh the costs of flexibility, among which is severing the employment relationship at the root of union power? The dualization literature provides a number of answers. One set of arguments focuses on unions' need for political survival. The expansion of private service sector employment together with the growth of non-standard employment in many industrialized democracies contributed to the erosion of the traditional base of unions in manufacturing. In the context of state-led industrial adjustment, unions traded workforce reductions leading to greater productivity for greater job security (Palier and Thelen 2012). Focusing on France and Germany, Palier and Thelen (2012: 203) argue that "[early] responses to the economic crisis of the 1970s and 1980s were organized around saving the core manufacturing economy, which was the foundation for both the economic and the social model." However, this insider-centered mode of adjustment came at the expense of outsiders. Focusing on corporate unions in East Asian economies, Peng (2012) also documents the "consenting" role of unions. In these cases, labor market flexibility was a strategy of union survival.

Other studies have argued that encompassing labor unions facilitate the consideration of general goals by actors that may be tempted not to act solidaristically (Obinger *et al.* 2012). Corporatism in this view is expected to promote pro-outsider policies through two channels. First, corporatist arrangements promote solidaristic preferences for individuals. Second, they may promote solidaristic behavior by the social partners (Rueda 2007). Corporatism facilitates the consideration of general goals through, for example, the existence of tripartite institutions. The Olsonian view implies that when matters of general economic or political interest are debated in fora characterized by an ongoing dialogue among relatively cooperative social partners, the possible future costs of particularistic behavior become clearer and more widely known. This affects both individuals (insiders become more likely to understand the negative implications of demanding high employment protection at all costs) and social partners (unions would become more likely to understand the negative implications of acquiescing to the particularistic demands of insiders).

The dualization literature has also provided new insights into the political coalitions underlying the introduction of labor market policy. A number of studies emphasize the strategic alliances or coalitions of various actors driving policy. In their study of Sweden in the period 1994–2006, Lindvall and Rueda (2014) show that insider-outsider divisions are relevant even in a proto-corporatist country such as Sweden. Looking at how political party strategies shape the political behavior of insiders and outsiders, they find that mainstream left parties that focus on issues primarily relevant to insiders are punished by outsiders. They also find that social democratic parties may face an "insider-outsider dilemma": tailoring their message to one group may alienate the other. This suggests that failure to reconcile insider-outsider interests may lead to new coalitional dynamics, with centre-right parties tailoring to insider interests (as appeared to be the case in 2006).

In other accounts, dualization stems from complex cross-class coalitions. Some studies emphasize that such cross-class coalitions have often involved manufacturing unions and the state. Palier and Thelen (2012: 202) conclude that "current trends point to a more durable new pattern based on cross-class coalitions, progressively institutionalized across successive policy domains and underwritten by state policy." Other studies have focused on the conditions under which firms are more likely to support labor market protection. Acknowledging that employers exhibit considerable heterogeneity in their preferences over labor market policy, the Varieties

of Capitalism (VoC) literature conceived of production regimes as institutional complementarities that reinforce one another in addition to reinforcing particular ways of producing and competing in the international economy (Kitschelt *et al.* 1999; Hall and Soskice 2001). In this view, regulated labor markets have beneficial employment effects in production regimes where institutionalized job security and unemployment insurance constitute institutional complementarities (as in coordinated market economies). Strong job protection encourages investments in training and may enhance productivity performance (Estevez-Abe *et al.* 2001; Mares 2003), but may increase dualization. In his study of labor market policy in Sweden, however, Swenson (2002) argued that the adoption of active labor market policies specifically targeted to outsiders in the 1950s was strongly supported by firms keen to address labor market shortages. Admittedly a unique feature of Swedish economic history, this nonetheless points to the possibility that under some conditions employers may support outsider-friendly policies.

Finally, research on labor markets in developing countries shows that insider-outsider dynamics are not specific to OECD countries. A longstanding literature in development economics emphasizes the strong income gaps in dual economies (Ray 2010). More specifically, research on the development of labor markets in Latin America shows that the increase in labor market flexibility in the wake of economic reform in the 1980s brought about rising levels of unemployment (Weller 2001) and atypical employment (Marshall 1996; CEPAL 2000). Labor market economists trace the rise in unemployment to the sharp increase in female participation triggered by reform (Ocampo *et al.* 2001). In turn, scholars of structural reform in Latin America have emphasized that by empowering unions and business groups, import-substitution industrialization benefitted the interests of formal sector workers and powerful capital owners (Murillo 2001, 2009; Etchemendy 2011), exacerbating labor market segmentation. Drawing on an insurance framework, recent work in comparative political economy argues that inward-looking industrialization increased incentives to protect labor market actors through generous labor market protections that address labor market risk. This dramatically increased dualization in many developing countries (Rueda *et al.* forthcoming).

Conclusion

The study of labor market institutions and economic outcomes constitutes a rich field of research marked by great interdisciplinary efforts to understand the causes of an eminently economic and political outcome. Extant efforts to emphasize the preferences of key actors and explore the political consequences of existing institutional configurations have greatly expanded our understanding of the drivers of macroeconomic performance.

In this chapter, we have argued that interesting opportunities lie in the study of labor market dualization. Building on rigorous microeconomic foundations, the dualization literature bridges economic and political insights and provides an exciting and worthy opportunity to analyze the variety of labor market outcomes prevalent in industrialized democracies. Equally important, this literature also offers an attractive framework to make sense of labor market dynamics in developing countries.

Notes

- 1 In the monopoly view, unions reduce output in three ways. First, union-won wage increases above competitive levels lead to too little labor being hired relative to capital in unionized firms, causing a misallocation of resources. Second, union-influenced work rules or contract provisions (such as limits on the load handled by workers, restrictions on tasks performed or requirements that unnecessary work

- be done) reduce productivity. Third, strikes to force management to accept union demands cause a substantial reduction in gross national product (Freeman and Medoff 1979).
- 2 Subsequent work on insider-outsider politics shared this starting point and stressed differences between the employed and the unemployed. These authors argued that labor market insiders face incentives to seek higher wages at the cost of jobs for outsiders (Blanchard and Summers 1986; Lindbeck and Snower 1988; Saint-Paul 2002).
 - 3 For a thorough review of the empirical evidence, see Howell *et al.* 2007.
 - 4 Earlier cross-national studies find inconsistent results. While Scarpetta (1996) finds a significant effect, Elmeskov *et al.* (1998) does not. In addition, the size of the estimated effects varies considerably. Scarpetta (1996) and Elmeskov *et al.* (1998) both find significant effects for bargaining coordination, but the effect in Scarpetta (1996) is twice as large.
 - 5 Baker *et al.* (2005) replicated Nickell's analysis with newer versions of institutional variables and found that only union coverage is significant, and three of the remaining seven institutional variables have the wrong sign (in contrast to the Nickell 1997 results in which 7 out of 8—except EPL—were significant).
 - 6 The English-speaking Liberal market economies are Australia, Canada, Ireland, New Zealand, UK, USA. High unemployment continental European countries are Belgium, Finland, France, Germany, Italy, Spain and the low unemployment European countries are Austria, Denmark, Netherlands, Norway, Switzerland, Sweden.
 - 7 Kitschelt (2000) also shows that the closeness of the political competition and the identity of the challenger of left-wing parties affect both the timing and the character of policy adoption.

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DOMESTIC POLITICS AND INTERNATIONAL INSTITUTIONS

Cooperation, sacrifice, and change

James Raymond Vreeland

Ostensibly, governments participate in international institutions to provide global public goods. But the real motivations behind their participation usually have roots in domestic politics. The most powerful members often use their political leverage over international institutions to pursue domestically-motivated foreign policy goals. Governments also use international institutions to impact domestic politics directly. So, while the subject of international cooperation has long been the purview of the field of International Relations, the field of Comparative Politics has much to contribute to and learn from the study of international institutions.

Scholars usually view international institutions as falling within the domain of International Relations because many of them are centralized organizations that enjoy some degree of independence (Abbot and Snidal 1998). The founding of international institutions usually entails some small sacrifice of sovereignty on the part of member-states, with the apparent objective of achieving an objective in global politics. Yet, governments may endow these organizations with some degree of independent authority precisely because doing so allows them to more effectively pursue *domestically-motivated* purposes. Focusing on major intergovernmental organizations, this chapter is thus organized around the related ideas of (1) sacrificing sovereignty and (2) using international institutions for domestic purposes.

The chapter begins with a discussion of how governments must be incentivized to join international institutions. The intuition is straightforward: In return for contributions to international organizations (IOs), powerful countries require substantial benefits (or “rents”), usually in the form of political influence over the institution. Less-powerful governments must also be incentivized to contribute in order to create institutions with global membership—so some voice may also be reserved for them. Designed to serve distinct purposes, various international institutions strike different balances across their great and small members to solicit their cooperation.

Comparativists should take two lessons from this section: (1) The design of an international institution parallels issues involved in writing a constitution. As Hardin (1999: 86) has observed, constitutions are not “contracts” because they lack an exogenous enforcement mechanism. Instead, they can stand only as self-enforcing equilibria.¹ Participation must be incentive-compatible and individually rational. (2) Control over IOs varies across countries. Precisely because participation must be incentive-compatible, countries with superior outside options

require more perks from membership. Note that incentive-compatibility refers to both inter-state relations and domestic conditions. The domestic side of the story may increase both constraints and opportunities.

With these lessons in mind, the chapter then turns to a specific incentive for international cooperation: domestically-motivated policy objectives. First, the chapter discusses how powerful countries can use IOs to pursue domestically-motivated foreign policy goals. The foreign policy examples include the ways in which the United States uses its power over the IMF to protect the financial interests of US banks, and Japan's broad interest in pursuing foreign policy through IOs. Then, the African Development Bank provides a crucial example of an organization observed both with and without Western political influence in its formal governance structure.

Next, the chapter turns to the ways in which governments can use IOs to pursue domestic policy (Gourevitch 1986). Examples abound. This chapter focuses on instances where governments employ IOs as credible-commitments.

The chapter ends with a discussion of changes on the horizon. Emerging market countries are currently vying for more powerful positions in existing organizations and also designing alternative international institutions. As they do so, domestic political goals and constraints across old and emerging powers are clashing in new ways. Within the context of these dynamics, the chapter concludes that the prospects for collaboration between scholars of International Relations and Comparativists are on the rise.

Sacrifice and benefits

Cooperation requires sacrifice. This is true for collaboration between individuals and amongst nations as well. *Institutionalized* cooperation across independent countries usually requires the sacrifice of at least some degree of national sovereignty.² People often only make sacrifices if they can expect some benefit in return.

Consider the famous case of President Woodrow Wilson, who won the Nobel Prize for his role in founding the League of Nations in 1919, but proved unable to win the support of the US Senate to ratify his own country's membership. Article 10 of the League's Covenant called on members to assist in preserving the borders of all member-states. Some key senators considered this obligation too great a sacrifice of US sovereignty (Northedge 1986: 85–86). Of course, the League of Nations made clear its commitment to national sovereignty by requiring unanimity for most decisions—effectively giving veto power to all members (Northedge 1986: 53). Still, a loophole remained: on matters involving a dispute, decisions did not require the consent of the parties to the dispute.³ This eventually contributed to other powerful countries (such as Japan and Germany, which had joined the League) to abandon the organization (Northedge 1986: 256–57, 278).

The pursuit of international cooperation should thus begin with a crucial question: Is participation incentive-compatible? Governments demand side-benefits—even unofficial ones. Real-world organizations should therefore not be judged by utopian criteria, but rather should be compared to other realistic equilibria, including the option of zero participation.

What, then, do governments get in return for their sacrifices? Some get more than others—and with good reason. As the example of the League of Nations illustrates, the most powerful countries may demand the most in return. The global community may be willing to meet the demands of such powerful countries if their participation is deemed important for the effectiveness of the IO.

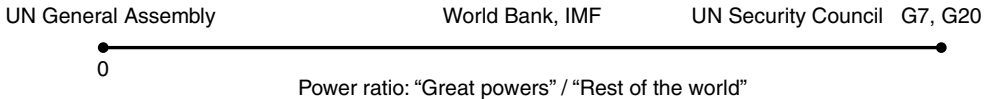


Figure 22.1 Ratio of power in international organizations—"Great powers" to the "Rest of the world"

Notes: Different international organizations offer different trade-offs to incentivize the "great powers" and the "rest of the world" to participate. At the left extreme are organizations where all countries are treated equally, so no one receives "great power" status. These organizations favor smaller countries. At the right extreme are clubs where only "great powers" have membership, leaving out the rest of the world.

Thus, one can conceive of IOs along a continuum of how much power is allocated to the "great" powers and how much voice is left for the "rest of the world" (see [Figure 22.1](#)). This conception takes its inspiration from one of the foremost scholars of International Relations, David Lake (1999, 1996: 7), who conceives of cooperation in international security along a continuum from anarchic alliances to hierarchical empires. [Figure 22.1](#) places the UN General Assembly at the anarchic extreme, where each country gets one vote. There simply are no "great powers" with special privileges—all countries are treated as equals, from China to Palau. In the middle are international financial institutions, where the formula resembles a "one dollar, one vote" principle. The more money a government contributes, the more the IO allocates power to it. At the hierarchical extreme is the UN Security Council, where the vast majority of formal voting power is allocated to the "great powers," which have permanent status and veto power over resolutions (O'Neill 1996). Beyond this point are "clubs," such as the G20, where only members have voice, and there are no criteria for the rest of the world to join.⁴

Comparativists should seek to understand the power dynamics at IOs because the mechanism through which these organizations impact domestic politics depends on the influence of a given government. The United States, for example, can use the IMF to pursue foreign policy goals, perhaps by pressing the institution to provide a loan with favorable conditions to a strategically important country. The IMF, however, has little (no?) leverage over US fiscal policy. Conversely, Uruguay is not usually in a position to use its membership in the IMF to pursue Uruguayan foreign policy objectives, but the IMF can act as a credible commitment for a Uruguayan government seeking to restructure domestic public expenditures (Vreeland 2003a). Indeed, an IO can sometimes best serve the interests of a government when the organization acts independently. As such, the international institution may provide a credible-commitment, useful in domestic politics, as discussed in the following section.

Domestic motives behind international organizations

This section addresses side-benefits, those serving domestic interests, which may help entice governments to participate in IOs. The discussion is broken up into two sub-sections. The [first part](#) discusses the privileges of "great powers"—they can use their influence over international institutions to pursue domestically-motivated *foreign policy* goals. The [second part](#) focuses on the role that IOs can directly play in *domestic policymaking*, specifically by providing a credible-commitment.

Great power privilege and foreign policy

The countries with the most power over an international institution can use their privileged influence to pursue foreign policy goals, which often derive from their domestic politics.

Consider the example of the IMF, where the United States has, by far, the largest share of the votes (about 17 percent—the next largest shareholder is Japan with just 6 percent). Oatley and Yackee (2004) find that countries receive more loans from the IMF if they have a higher degree of US bank exposure.

Lipsky (2003) suggests a domestic politics explanation for this pattern. He points out that where economic integration with the United States is high (for example, Mexico), the US domestic economy has a lot at stake from avoiding a major economic catastrophe, and thus the US government pressures the IMF to supply ample liquidity to quench a financial fire. Yet where US economic interests are weaker (for example, Southeast Asia), US pressure may run the other way: smaller loans with more strings attached. Interestingly, Lipsky presents evidence showing that when the US and Japanese preferences diverge because of their varying economic ties to different countries, the IMF sides with the preferences of the United States over those of Japan.

Broz (2011) provides political micro-foundations for the relationship, analyzing US Congressional votes on requests for increased contributions to the IMF. These votes appear to reflect campaign contributions from banks that specialize in international banking (such as Citibank). These banks have an interest in supporting the IMF, since IMF lending can go to countries indebted to them. The presence of a strong IMF mitigates the risks that these banks face when lending to developing countries. If the IMF can bail developing countries out of an economic crisis, it reduces the risk of default on loans that they owe US banks. Thus, US Congressmen who rely on campaign contributions from money-center banks face pressure to support increases in US contributions to the IMF.

The United States is not the only powerful country that uses its political influence over an IO to pursue domestically-motivated goals. When it comes to the Asian Development Bank (AsDB), most observers agree that Japan has the most political influence (Yasutomo 1993). Kilby (2006) and Lim and Vreeland (2013) show that countries important to Japan receive a larger share of AsDB lending.

Why does Japan go through an IO to favor strategically important countries rather than pursue goals unilaterally? Due to Japan's imperial legacy leading up to World War II, neighboring China and Korea continue to scrutinize Japanese foreign policy—as does the Japanese domestic audience. In particular, the Japanese political-left strongly opposes unilateralist foreign policies (Rosenbluth and Thies 2010: 159–60). Thus Prime Minister Shigeru Yoshida proposed the doctrine of maintaining a low profile in international politics, pursuing foreign policy through multilateral institutions (Green 2003). The AsDB, for example, provides political cover for Japan to favor strategically important countries in Asia, while skirting these domestic and international political constraints on its foreign policy (Wan 1995: 93). As Yasutomo puts it, the AsDB serves as a “nonpolitical cloak” to “legitimize controversial policies, helping Japan to share the risks and the blame” (Yasutomo 1993: 339).

As another example, consider the African Development Bank (AfDB). Founded while many African countries were winning independence, the members wished to create an institution free from Western political influence. From 1963 until 1982, the membership of the AfDB included only African countries. With votes allocated according to the size of financial contributions to the institution, however, this also meant that the AfDB had no access to Western finance. In order to gain access to more funding, the AfDB eventually admitted Western governments who then contributed finance and also gained political influence over the institution. So, we can observe the AfDB both with and without Western political influence over the institution's formal operations.

Testing for politically-motivated lending patterns at the AfDB, Vreeland and Dreher (2014) find no evidence for the period before 1982. But after 1982, countries strategically important to the West receive larger loans from the AfDB than other countries. Specifically, African countries serving on the Security Council suddenly started receiving larger loans.

Vreeland and Dreher (2014) show that the United States, Japan, and Germany—all countries with power at the AfDB since 1982—have used the Security Council to send signals to their domestic publics about the appropriateness of forceful foreign policies. As discussed in the next sub-section, all three of these countries have domestic audiences who have proven sensitive to the approval of the Security Council. So, when these powers gained influence over the AfDB, they apparently used their power to favor African governments serving on the Security Council in return for political support of their foreign policy priorities, which, in turn, sent signals to their domestic publics.

The AfDB example reveals complicated dynamics across IOs. Government may use their privileged position in one organization to gain leverage over another. In this example, the United States uses its power at the AfDB to increase its leverage over the Security Council. Meantime, African governments can use their temporary position on the Security Council to cash in favors at the AfDB. The example also suggests a dilemma facing governments when leveraging influence over an international institution. Here, side-deals can undermine the independence and credibility of the Security Council. Governments may thus seek to obfuscate side-payments by operating at arm's length through another international institution (here, the AfDB). We return to this issue when discussing “dirty politics.”

For Comparativists, the main conclusions from this sub-section is that powerful countries can employ IOs to pursue domestically-motivated foreign policy goals. As Comparative Politics focuses on understanding the ideas, institutions, and interests driving the domestic motivations of politics, the field has much to teach scholars of International Relations.

The uses of independent international institutions in domestic politics

Comparativists also have much to learn from scholars of International Relations because IOs directly impact domestic politics. Interestingly, and in contrast to the previous discussion, governments can make this use of an IO when they do *not* enjoy a great deal of power over it. Indeed, sometimes an *independent* international institution can prove more useful to governments than one that they can directly influence. Specifically, international institutions can serve as independent third parties signaling credible-commitments.

Along with several colleagues, Rosendorff has shown that (1) entering into preferential trade agreements can signal credible free trade policies; (2) reporting economic data to international institutions can signal credible transparency; and (3) ratifying human rights agreements can signal credible “resolve” to remain in office.⁵

Regarding preferential trade agreements, participation enables governments to credibly signal their commitment to free trade policies. The argument is usually applied to democracies, where the government faces a protectionist lobby but would like to resist this pressure and pursue efficient economic policies that benefit the economy as a whole. The problem is that the protectionist lobbies are well informed about trade policy while the broader public is not. Mansfield *et al.* (2002) argue that an international trade agreement helps the government choose free trade because it provides necessary information to an otherwise uninformed electorate. The fact that other parties to the agreement can litigate cases of protectionism provides a credible signal to the domestic audience. Thus, participation in free trade institutions may help

governments win reelection for pursuing policies of free trade. The argument applies to the WTO as well as regional trade organizations.

Turning to transparency, the use of international institutions applies to a key issue in Comparative Politics: good governance. Adserà *et al.* (2003: 445), for example, argue that “the degree of citizen information curbs the opportunities politicians may have to engage in political corruption and management.” Hollyer *et al.* (2011) argue that democracies have stronger incentives to provide credible information to citizens than do non-democracies. The goal of such democrats is to avoid unfair eviction from office. The dilemma follows the standard principal-agent logic: The voter observes outcomes, not policies—and the outcome is a function of the effort supplied by the government and exogenous forces. How then can the government credibly signal to voters that it has provided high-level effort?

Once again, independent IOs provide a credible third-party signal. Democracies choose to provide ample data to IOs—for example, the World Bank—which, in turn, omits data considered “questionable” (Hollyer *et al.* forthcoming: 2). Thus, when individuals observe data provided by an independent IO, they can better assess whether their government has pursued sound economic policy or has shirked.

In a different policy-setting, the Security Council can play a similar role in convincing a skeptical domestic audience. As Chapman (2011) explains, voters may question an aggressive foreign policy as overzealously “hawkish,” but these citizens do not have access to top secret information. US presidents may be ready to use force, while the US public is more risk-averse, preferring to use force only for defensive purposes (Jentleson 1992; Perla 2011). This generates another standard principal-agent problem, where the agent (the US president) has better information than—but divergent preferences from—its principal (the voters).

A favorable vote from a Security Council member, who has security-clearance for privileged information and is known to be “dovish” on matters of international security, can convey a credible signal that the forceful policy is appropriate. To the extent that the US public perceives the Security Council as an independent third party, a resolution can thus act as a credible signal of the prudence of a specific military action.

The argument also holds for other countries, such as Japan.⁶ Japan’s so-called Peace Constitution renounces “war as a sovereign right of the nation and the threat or use of force as a means of settling international disputes” (Ueki 1993: 358; Dore 1997: 106). The Japanese political-right has argued, however, that Japan can use its military abroad if authorized by a Security Council resolution, while the political-left seeks Security Council approval because of its preference for multilateralism (Ueki 1993: 359–60; Green 2003: 197; Thompson 2006: 24; Chapman 2011: 78–79). Lim and Vreeland (2013: 42) thus highlight the multivocality of Security Council approval, which conveys “different signals to different constituencies.” Again, the use of an IO can send credible signals to domestic publics about the appropriateness of using its military abroad.

The argument that international institutions send “credible signals” to domestic audiences also applies to the protection of human rights—although in a more insidious way. Hollyer and Rosendorff (2011) argue that when dictatorships ratify the United Nations Convention Against Torture (CAT), they—ironically—send a credible signal of their ruthlessness.

Here, the uncertainty surrounds the type of dictatorship ruling a country: strong- or weak-resolve. All dictators may claim that their resolve is strong, but the public may rebel nonetheless in the hope that the dictatorship is actually the weak-resolve type. Weak-resolve dictatorships can indeed be overthrown through public unrest, but strong-resolve types cannot. An inefficiency results: strong-resolve dictatorships can signal their type only by brutally crushing the rebellion.

Hollyer and Rosendorff (2011) argue that strong-resolve dictatorships can take advantage of a specific sacrifice of sovereignty included in the CAT: “universal jurisdiction.” Universal jurisdiction implies that if a government commits torture, public officials may face prosecution for the crime by courts in other countries—this is what eventually happened to Pinochet of Chile, when a bench warrant by a Spanish judge placed him under house-arrest while visiting a hospital in London (he died before the case went to trial—Hawkins 2003).

The key to this story is that ratification of the CAT essentially commits the dictator to going to prison if he should ever fall from power because almost all dictatorships engage in some practice of torture during their tenure. As long as the dictator holds on to power, he is safe from prosecution. But if he ever falls from power, he may very well find himself in the same situation as did Pinochet. Weak-resolve dictators should thus not ratify the CAT. They know their type and recognize that their time in power is limited. So, they will practice torture as necessary to stay in office, but will likely flee the country someday.

Strong-resolve dictators have different incentives. Because of their confidence in their ability to maintain power, they can ratify the CAT. They expect to stay in power and thus have no fear of prosecution by foreign judges. The CAT thus allows a strong-resolve dictator to send a credible signal to the domestic audience of his confidence to survive in power and his willingness to do so by any means necessary. Interestingly, Hollyer and Rosendorff argue that human rights violations should decrease when a dictator ratifies the CAT—not because the government now respects human rights, but, ironically, because it clearly does *not*. The public now understands this fact unambiguously and fears the government so much that no one steps out of line.

As a final illustration of how international institutions can impact domestic politics, consider the role of international financial institutions in shaping the payoffs of alternative policies. IMF conditionality, for example, might provide credible leverage to push through unpopular policy changes in the face of an economic crisis (Putnam 1988; Vreeland 2003b). Bringing in the IMF changes the perceptions and payoffs of domestic actors. The loan itself provides an enticement for change, and failing to enact policy changes now stands as a rejection of the IMF, which can send negative signals to creditors and investors. Governments may thus use the leverage of an IO to coerce reticent interest groups so that they agree to raise taxes or cut expenditures.

Of course, often governments turn to the IMF because they need a loan, and the policy conditions appear forced on them. Usually, however, at least some domestic interest groups stand to gain from IMF reforms—while other groups lose. In order to make debt payments, protect the banking sector, and preserve the value of the national currency—which help society’s wealthy—the government may cut the size of the public sector and social expenditures on health and education (see Nooruddin and Simmons 2006; Nooruddin and Vreeland 2010). Accordingly, several studies have found that IMF programs exacerbate income inequality (Pastor 1987; Garuda 2000; Vreeland 2002a). Note that the IMF does not intend for this outcome, which often results from partial implementation of programs. Governments can prioritize which policy conditions to implement. As Stiglitz (2000: 551) has argued, governments can take advantage of informational asymmetries “both between themselves and the citizenry and between the international aid community and themselves.” As such, a government with no ostensible leverage over an IO can still use it to achieve domestic policy goals. In some cases, the government may even shift the blame for economic hardships to the IMF (Smith and Vreeland 2006).

This brings up the “dirty work” that IOs can perform for governments (Vaubel 1986: 48–51). IOs can benefit member-governments by obfuscating or laundering activities that look bad to the public (Abbott and Snidal 1998: 18–19; Yasutomo 1993: 339). This use of IOs applies to

both domestic and foreign policy. Domestically, a government may blame some policy outcomes on the conditionality of the IMF and the World Bank, or on the regulations governing trade through the World Trade Organization (WTO). Indeed, members of a pro-trade government may seek out the leverage of the WTO, hoping to lose a case so that it can report back to protectionist interest groups that their hands are tied (Allee and Huth 2006). In foreign policy, governments may use IOs to hide distasteful deals, like bribing a government. Pakistan, for example, received a generous loan package from the IMF with light conditionality in December 2001, when the United States needed military bases from which to stage its war efforts in neighboring Afghanistan (Vreeland 2002b). The perceived independence of IOs thus provides the additional benefit of political cover for distasteful policies.

The lesson for Comparativists is that myriad international institutions can impact domestic politics by sending credible signals about the appropriateness of policy options or by altering the payoffs from pursuing different courses of policy. The micro-foundations of the power dynamics of IOs can help reveal the mechanisms by which these institutions impact domestic politics.

Emerging markets, regionalism, and change

The rise of emerging market countries is changing power dynamics in IOs and leading to proposals for new institutions. The benefits of collaboration between scholars of International Relations and Comparative Politics thus continue to grow.

At present, the power-structure at most IOs reflects the order that emerged during the Cold War. The permanent members of the Security Council, for example, include only the allied powers from World War II, leaving out other important countries. The IMF and World Bank purport to assign votes according to economic importance but (at this writing) rank China in sixth place, behind the United States, Japan, Germany, France, and the United Kingdom. Many emerging market countries—from Brazil to South Korea—have supported reform of the voting system to allow for a greater voice. The old powers (notably the smaller Western European countries) are reluctant to give up the power that they currently have.

Lurking behind these international negotiations are domestic politics. Consider the United States: if it gives up too much power at the IMF and World Bank, isolationist forces in the US Congress will have a good excuse to say “no” the next time the president asks for an increase in contributions. Small Western European countries, too, have domestic politics to consider. Consider Switzerland, which holds a seat on the Executive Boards of the IMF and the World Bank. Joining the World Bank and the IMF in 1992 represented a major step for the historically-neutral Swiss, and the government justified the foreign entanglement to the skeptical electorate with an Executive Board position. This position also allows the government to advocate global policies favoring its important banking sector. Switzerland thus has a continued interest in retaining its seat (Vreeland 2011).

Yet emerging market countries also have domestic interest groups calling for a stronger voice in global institutions. The export sectors in Japan and China, for example, opposed the austerity imposed by the IMF on their trading partners during the East Asian financial crisis in the late 1990s (Lipsy 2003). This opposition eventually led to the founding of the Chang Mai Initiative. Turning to South American, the politicians on the left—long-opposed to the Washington Consensus policies promoted by the World Bank—have supported an alternative Banco del Sur (Desai and Vreeland 2011).

Power is thus on a tightrope. It used to make sense for the United States and the West to provide the lion’s share of resources at IOs and receive commensurate political control.

As emerging markets continue to grow, however, they will demand more voting power. Eventually, we may reach an impasse where Western governments will not have the domestic political capital to assent to the changes demanded by the developing world. Regional organizations represent a possible alternative. Asian countries have taken steps towards setting up a regional version of the IMF called the Chiang Mai Initiative Multilateralisation (see Lipsky 2003 on the background), and the BRICS (Brazil, Russia, India, China, and South Africa) have recently founded their New Development Bank.⁷ These efforts remain in their infancy, but stronger organizations could eventually emerge.

Indeed, the world's most sovereign IO is a regional one: the European Union. As Anderson (1999: 5) notes, "the contingent nature of sovereignty...is nowhere more apparent than in contemporary Europe." Even while battered by the 2008 Global Financial Crisis, its membership and powers have emerged unscathed, and in some policy areas, the union has taken on the status of a pseudo-state (McNamara 2008).

The relative success of cooperation among a narrow set of countries, even while global institutions falter, should not be surprising. The domestic gains from cooperation are highest when a set of countries already enjoy a high level of economic integration and economic integration is generally highest at the regional level. Busch (2007) shows that while a government may not wish to set a new precedent favoring free trade at the global level because it fears worldwide competition, it may prove willing to set the precedent among a smaller set of countries at the regional level. In general, the export sector should win from increasing levels of free trade, while the import-competing sector stands to lose. These competing interest groups will thus pressure their governments in opposite directions when it comes to participation in trade organizations. The losers from trade may fight hardest, however, against exposure to global trade, while regional trade may appear less threatening (Richardson 1993; Bohara *et al.* 2004). After all, countries tend to trade more with their neighbors to begin with, and domestic industries may have already anticipated greater integration.

Still, regional integration does generate losers, so regional organizations face opposition. Consider the loss of autonomous monetary policy for the members of the euro area. Whereas once, countries such as Germany and Greece could pursue their domestic agendas independently, now monetary policy is set by the European Central Bank. So, Greece must face the inflationary concerns of Germany, while Germany must contend with the unemployment concerns of Greece. The power asymmetry is obvious: Germany dwarfs Greece in economic and political power, and tends to get its way, but even Germany does not achieve all of its preferred policies. The ongoing financial crisis has given new impetus to the losers from regional integration to question the European project. Nevertheless, the euro area and, more broadly, the European Union continue to weather the storm. This was predictable. Back when the political-left took power in France in 1981, President Mitterand and the German Bundesbank similarly disagreed about whether monetary policy should be used to stimulate the economy (Oatley 2012: 263). At that point in history, it would have been relatively easy for countries to walk away because each had an independent currency. But the winners from regional integration prevailed. Currently, it would be very costly for euro-area countries to leave, and despite major setbacks, European integration has survived.

The reason that regional organizations stand to make more progress than global ones follows directly from the veto player theory of Comparative Politics: as the number of actors increases, the "winset" of possible agreements can only decrease (Tsebelis 2002). The broad membership of global organizations thus often leads to relatively shallow agreements, while the narrower set of countries at a regional level may reach deeper commitments.

Prospects for collaboration

With rising powers promising to change the dynamics of international cooperation at the global and regional levels, the opportunities for collaboration between scholars of international institutions and regional specialists may increase. As the power relations at IOs shift and the number of regional organizations grows, their interaction with domestic politics will change as well. China, for example, may eventually use international institutions to pursue domestic goals, generating new effects for less-powerful countries in Asia and around the world.

Future research should focus on the micro-foundations of politics both within IOs and at the domestic level. Research on IOs often addresses domestic politics at a superficial level, without careful attention to the preferences of actors who inhabit domestic institutions. Meantime, research in Comparative Politics addressing the impact of International Relations on a country may provide only a cursory treatment of power dynamics within IOs. When addressing the interaction of international and domestic institutions, future work would benefit from deeper collaboration between experts who can provide a more thorough understanding of politics at the international and domestic micro-levels.

This chapter has thus focused on what the field of Comparative Politics—with its emphasis on the ideas, interests, and institutions shaping domestic politics—has to contribute to and learn from the study of international institutions. On the one hand, when powerful countries seek to pursue foreign policy objectives through IOs, scholars of International Relations have much to learn from Comparativists about the domestic politics driving these objectives. On the other hand, when a government interacts with a relatively independent IO, the organization itself may play a major role in shaping domestic policies, and thus be worthy of the scholarly attention of the field of Comparative Politics.

Notes

- 1 This view recalls Przeworski's conception of democracy as an equilibrium (1991: 23) and Gandhi's cooperation and co-optation equilibria under dictatorship (2008: 87–90).
- 2 See Lake (1996, 1999) and Arend and Beck (1993).
- 3 See Article 12 and Northedge (1986: 53).
- 4 As the World Trade Organization requires unanimity, it might be considered at the left extreme. Yet, countries with the most at stake take the lead in negotiations about specific commodities (McGillivray 2000). So this organization does not readily fit into this scheme.
- 5 The seminal work—on trade—was co-authored by two of the leading scholars of International Relations, Edward D. Mansfield and Helen V. Milner (Mansfield *et al.* 2002). On transparency, see Rosendorff and Doces (2006) and Hollyer *et al.* (2011). On human rights and leader-resolve, see Hollyer and Rosendorff (2011).
- 6 For Germany, see Dreher *et al.* (2013).
- 7 See Desai and Vreeland (2014).

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Part III

The effects of comparative institutions

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VOTING BEHAVIOR AND POLITICAL INSTITUTIONS

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A common refrain in social science is that “institutions matter.” Institutions matter for, among other things, increasing a nation’s wealth, reducing corruption, and effective governance. And not least, political institutions matter for a citizen’s voting behavior. The political institutions within which a voter exercises his or her vote will structure the choices that are available to him or her. In turn, these choices act to shape the eventual voting decision. The interaction between these system-level characteristics and the voter’s own characteristics determines the voting decision. Voting is therefore not simply a personal choice, but one that is shaped by the context within which the choice is made (for reviews, see Anderson, 2007; Dalton and Anderson, 2011; Klingemann, 2009).

The institutional features of a political system have long been known to be a key influence on voter behavior. Voters have been observed to behave differently depending on whether the executive authority rests with a president or with a parliament; whether the electoral system is majoritarian or proportional; and whether the party system consists of two parties or more than two parties—to mention just three types of institutional variation (Duverger, 1954; Norris, 2004; Shugart and Carey, 1992). But understanding how and in what ways these institutional differences shape voting behavior has only become a prominent subfield of voting studies in the past two decades, as appropriate data sources and methods have become available.

The insight that systematic data collection is necessary to evaluate the impact of institutions is often attributed to the work of Stein Rokkan (1970) almost half a century ago. However, it was not until the 1990s that there was a move to measure systematically the impact of institutions on voting behavior. The first steps in this process were the establishment of a coordinated cross-national effort in data collection (especially through the Comparative Study of Electoral Systems [CSES] project—see Klingemann [2009]). Established in 1995, the CSES enables the systematic analysis of electoral behavior under globally varying institutional conditions. The project coordinates the operation of more than 50 national election studies across the world, thereby ensuring that information about citizens’ behavior and attitudes gathered in each country is comparable and can be matched with information about institutional arrangements. Combined with advances in methodology (and specifically multi-level modeling [see Kedar and Shively, 2005]), analysts have been able, for the first time, to measure the precise impact of institutions on voter behavior.

Since the range of political institutions is vast, and their potential impact on voting behavior substantial, some broad categorization is required. Accordingly, the chapter is divided into five sections. The first section examines the impact of electoral institutions on voting behavior, with a specific focus on electoral participation. The second section deals with election rules, such as thresholds and the degree of proportionality in the electoral system, and how they can shape behavior. The third section moves the focus of attention to system performance, which is defined by economic voting, but the logic applies equally well to other issues that concern voters. Partisanship and how it varies by the institutional arrangements of the country is the topic of the fourth section, while the fifth section examines the opportunities particular institutional arrangements open up for strategic and split ticket voting. The conclusion draws some broad conclusions about the total impact of institutions on voting behavior.

Electoral participation

The institutional factors that shape electoral participation are generally well known, although the relative magnitude of their effects on individual behavior and how they should be categorized are often a matter of dispute. This section examines the impact of voter and party registration rules—the classic “gate-keeping” function of the electoral system—on electoral choice, as well as other rules that serve to shape the act of voting. In different ways, each of the rules has an independent impact on citizens’ electoral choices.

Electoral laws. The laws that determine who is eligible to enroll to vote are the first institutional factor shaping electoral participation. There is some U.S. evidence, starting with Wolfinger and Rosenstone’s (1980) pioneering study, which shows that easier registration rules increase the numbers who enroll to vote; this, in turn, boosts turnout. However, U.S. registration rules tend to be different from those found in other countries, particularly in Europe. First, in the U.S. registration is an individual opt-in responsibility rather than an automatic process (Knee and Green, 2011: 314). Second, registration laws that have evolved in some U.S. states have been designed with the explicit purpose of dissuading blacks to register to vote. In any event, later studies, also examining the U.S., have suggested that the effects attributable to easier registration rules are relatively small (Knee and Green, 2011; Mitchell and Wlezien, 1995). The more limited international research that has been conducted has confirmed the importance of voter registration rules (Blais, 2000; Norris, 2004).

In addition to voter registration, the formal registration of political parties can indirectly influence electoral participation, by determining which parties are able to gain access to the ballot list. Party registration serves to regulate political funding and to control the party names (and sometimes the party emblems) that appear on ballot papers in order to avoid duplication or confusion for voters. In newly democratic countries party registration rules are often used to stifle opposition, or to ensure that one party does not gain a dominant position (Karvonen, 2007). Russia, for example, required a registered political party to have at least 10,000 members from 2001 to 2006, when it was increased to 50,000. Following mass protests in the wake of the December 2011 Duma election this requirement was reduced to 500 members. Studies in regions as diverse as Latin America (Birnie, 2004) and the Asia-Pacific (Reilly, 2006) confirm the importance of party registration rules.

The electoral system. A second institutional factor that can affect electoral participation is the electoral system itself. One consideration is whether voting is voluntary or compulsory, with studies showing that turnout is higher in countries that have compulsory rather than voluntary voting. In the first systematic analysis of the effects of compulsion on turnout, Jackman (1987; see also Blais and Dobrzynska, 1998; Franklin, 2004) estimated the increase in turnout due to

compulsion to be around 13 percentage points. However, the positive effect of compulsion tends to be limited to the established democracies that have compulsory voting—Australia and Belgium—while the effect is considerably less in other compulsory voting countries, particularly in Latin America (where it is called “mandatory voting”). The discrepancy appears to be caused by the strict enforcement of compulsory voting in Australia and Belgium, and the political culture that underpins compulsion; most other countries that operate compulsory voting have significantly weaker enforcement (Norris, 2002; Blais, 2007; Birch, 2009).

Another aspect of the electoral system is district magnitude, or the number of representatives that are elected from each electoral district. Larger districts with more candidates encourage greater competition between parties and candidates and therefore generate more mobilizing activity compared to smaller districts (Karp *et al.*, 2007). By contrast, smaller districts are often categorized as “safe” for one party and rarely change hands at any election; there is little incentive for non-incumbent parties to concentrate scarce resources on these seats and therefore turnout is lower. In many democracies, “safe” seats regularly make up the large majority of districts and as a result, the parties concentrate their activities on the minority of marginal districts that they have a chance of winning. However, part of this effect may be due to the electoral system itself, since proportional representation systems generally have larger districts than majoritarian or first-past-the-post systems.

One dimension of the electoral system that has received much attention in recent years is the integrity or fairness of the system. If voters have confidence in the system and in the processes that underpin it, they will be more likely to turn out to vote (Birch, 2010). In transitional democracies particularly, a belief that the electoral system is open to manipulation can undermine support for democracy and leave open the possibility of a return to “more orderly” and predictable authoritarian rule. Manipulation can take place directly, through electoral management bodies, or indirectly through biased electoral rules (Lindberg, 2009; Levitsky and Way, 2010). Studies show that public perceptions of electoral unfairness have a significant negative impact on views of democracy generally across a range of postcommunist societies (McAllister and White, 2015).

Type of legislature. The legislature is the third institutional factor that affects turnout. Jackman (1987) has identified countries with a single legislature as having higher turnout compared to bicameral countries. This proposition, according to Blais (2007), is based on the concentration of political authority, so “the more powerful the body that is being elected, the higher the turnout.” It should therefore also apply to unitary systems, with turnout being higher than in federal systems. Blais and Dobrzynska (1998) provide a more sophisticated measure of the principle, taking into account, among other things, direct and indirect elections and the presence or absence of subnational elections. They conclude that the salience of an institution for voters is indeed a significant influence on turnout.

The act of voting. The general rules that surround the act of voting and whether they make voting easy or difficult are a fourth factor that is known to affect turnout. The day on which polling takes place influences turnout (Franklin, 2004), with a national rest day (such as a Sunday, which is used in many European countries) producing higher rates of turnout compared to a regular work day (such as a Tuesday, as in the U.S., or a Thursday, as in the UK). There is also some evidence that the easy availability of postal or absentee ballots increases turnout, since voters will have flexibility in how they choose to cast their ballot. However, the effects of these various measures appear not to be large and as a consequence, the estimates are especially sensitive to the countries that are included in the analysis (Norris, 2004).

Minimum voting age. The age at which citizens are permitted to vote is a fifth factor influencing turnout. Since turnout is higher among older citizens, it follows that, other things being

equal, lowering the voting age will reduce turnout. In their study of 324 national elections across 91 countries, Blais and Dobrzynska (1998: 246) found that “everything else being equal, turnout is reduced by almost two points when the voting age is lowered one year.” Lowering the voting age from 21 to 18 years in the 1970s therefore reduced turnout by about 5 percentage points. Both Franklin (2004) and McAllister (2014), analyzing a smaller group of countries, estimate the decline in turnout due to the lowering of the voting age to 18 to be about 3 percentage points. Since the vast majority of democracies allow voting at 18 (among the established democracies, only Austria currently allows national voting at 16) the current voting age has few consequences for turnout. However, if the age was reduced further, to 16 as some parties and organizations advocate, turnout could be expected to decline.

Compulsory voting is known to reduce inequalities in voting—such as younger or lower socioeconomic status voters turning out less frequently than older voters or those in higher status groups (Gallego, 2010). A reduction in the observed patterns of inequalities in voting has also been demonstrated to have effects on redistributive public policies favoring the under-privileged (Hill, 2002). If certain groups are less likely to turn out to vote, governments have less incentive to address their concerns in framing policies (Griffin and Newman, 2005). Thus reducing inequalities in voting—in age, education or income, for example—can have measurable effects on government policies and an impact on whom they are directed.

This overview of the impact of institutional arrangements on electoral participation indicates that all have some tangible impact, but in general the effects vary between countries and are usually small. As Holmberg (2009: 167) puts it, “the electoral system matters, but not much.” Consequently, which countries are included in the study has a considerable impact on the estimates that are arrived at. Within the literature on electoral participation, the major factor that has been identified as impacting on turnout is whether the electoral system is proportional or majoritarian; this is examined in the next section, in the context of election rules.

Election rules

Election rules refer to a complex set of explicit regulations and principles that determine how votes are counted. This process ultimately determines who is elected. One of the earliest categorizations of election rules and their consequences was Duverger’s law (1954), which predicted that majoritarian electoral systems would produce two party systems while proportional election systems would produce multiparty systems. In each case, this outcome would have distinct implications for individual voting behavior. However, election rules also extend beyond the counting of votes to cover, for example, election thresholds.

Proportional versus majoritarian electoral systems. The proportionality of the electoral system is often identified as a major institutional feature that shapes voting behavior. There is some evidence that greater proportionality leads to higher turnout (Franklin, 1996; Radcliff and Davis, 2000), but as Blais (2007: 113–114) points out, the bulk of these studies are based on the established democracies and when new or emerging democracies are incorporated into the analysis, the results are less robust (Blais and Dobrzynska, 1998). Which countries are included in any study therefore seems to have a crucial effect on the results, in addition to the complicating factor, noted in the previous section, that PR systems tend to have larger districts compared to majoritarian ones. This means that electoral competition will generally be more intense in PR systems, but that higher district magnitude will not lead to higher levels of turnout.

Proportional representation systems are also divided into three groups, according to the form of the ballot. First, systems that use open ballot lists, where voters can choose between individual

candidates are the most candidate-centered systems. Second, systems that use semi-open ballot lists give voters the choice between voting for a pre-ordered party list, or for an individual candidate. Third, systems that use closed-list ballots force voters to choose between parties, not candidates and are therefore party- rather than candidate-centered. In general, systems that are more candidate-centered encourage candidates to cultivate personal votes and to provide effective constituency service (Shugart, 2001; Shugart *et al.*, 2005). By contrast, when candidate fortunes largely rest with the party, party service and party loyalty become paramount considerations for candidates. There is evidence to show that candidate-centered systems generate higher levels of voter satisfaction, net of other things (Farrell and McAllister, 2006; see also Anderson and Guillory, 1997).

The findings on the proportionality of the electoral system are on firmer ground with respect to how the electoral system shapes the public's views of the political system as a whole. Studies have shown that the more proportional the electoral system, the greater the number of competing political parties. This, in turn, generates a stronger sense among voters that their views are being adequately represented (Anderson, 2011; Karp and Banducci, 2008). By contrast, in two party systems many voters feel that their views are unduly constrained by the limited choice available. By producing a wider range of parties, then, PR systems provide more choice for voters and generate stronger satisfaction with the system as a whole (Anderson and Guillory, 1997; Norris, 2004). These popular feelings towards the political system can have significant effects on voting behavior, through stronger partisanship, more information about the available policy choices, and a higher level of mobilization by the parties.

Election thresholds. One specific consideration in election rules is whether or not a threshold is applied. Many countries have rules about an electoral threshold, so that a party is unable to secure representation until a minimum threshold is met. The purpose is to reduce the number of parties in the legislature and to improve governance, coalition formation, and accountability. For example, of the 34 countries that are members of the OECD, 19 have an election threshold; most of these 19 countries operate some form of proportional representation. The popularity of election thresholds rests in their simplicity and effectiveness; they are straightforward to implement and easy for voters to understand. During their transition to democracy, many of the postcommunist countries introduced thresholds, often with the intention of facilitating the return of noncommunist parties to power, and barring representation from small, extremist parties (Moraski and Loewenberg, 1999).

Thresholds can affect voter behavior in two ways. First, they have been shown to increase disproportionality in election outcomes and to contribute to the number of wasted votes (Anchar, 1997; Powell and Vanberg, 2000). Voters may therefore be discouraged from voting for a small party that looks unlikely to meet the required threshold and instead opt for a larger and more established party. Second, thresholds can act as a disincentive to new parties contesting elections and arguably limit the choices that are open to voters in an election. The ultimate effect may be to reduce representation. Thresholds may therefore act in a subtle way to nudge voter behavior towards support for major parties, and away from minor parties.

Election rules and their effect on the counting of votes has an obvious impact on voting behavior, by favoring the major parties. A majoritarian system will favor the major parties by giving them a disproportionate share of the seats in a legislature. While a PR system will result in a more equitable distribution of seats, the use of election thresholds in most of the countries that operate PR also biases the system towards the major parties, although perhaps not as strongly. The net effect in both cases is that some voters will perceive their vote as being wasted unless their choice is for a major party.

System performance

The performance of the political system—the ability to generate material benefits for citizens (such as physical security and economic prosperity) as well as non-material ones (such as confidence in institutions and political efficacy)—serves to shape public views of the system and ultimately determine individual voting behavior. Across the amorphous field of system performance and its political consequences, perhaps no subfield is matched by the breadth and depth of the research on economic voting. This focus forms the main part of this section, but the logic can apply equally well to any issue of concern to voters.

Economic voting. Institutions provide an important backdrop to economic voting by way of their impact on public policy. Electoral systems have been long found to have important implications for policy, with proportional systems leading to more redistribution and higher levels of public spending than plurality systems (Austen-Smith, 2000; Milesi-Ferretti *et al.*, 2002; Morelli, 2004). Others have observed the strong similarity between varieties of capitalism and the type of electoral system in the country (see, for example, Gourevitch and Shinn, 2005). However, the origins of these patterns have been a matter of debate. Rokkan (1970) and more recently Boix (1999) have argued that the adoption of PR systems was a conscious policy of the political right, while more recent work has cast doubt on this explanation and argued that the motivation lay in the right's support for consensus regulatory frameworks (Cusack *et al.*, 2007).

A consistent finding in the research is that voters will reward a government for good economic performance and punish a government for poor economic performance. But more than any other area, voters must see a direct line of accountability to government, and institutions help to shape whether that line of accountability is clear or opaque—what has been termed “clarity of accountability” (Powell and Whitten, 1993). In general, economic voting is stronger in countries where the clarity of accountability is transparent—such as two party systems where the major parties alternate in government—and weaker in countries with multiparty systems where coalition arrangements are the norm. This distinction tends to divide Westminster countries based on majoritarian electoral systems from their European counterparts which are based on proportional representation.

Accountability. In recent years the clarity of accountability argument has been subjected to considerable scrutiny, and it has been argued that changing economic and political conditions have been undermining the ability of voters to attribute reward or blame for government performance. The various arguments that clarity of accountability has been gradually eroded can be disaggregated into three, partially overlapping, components: path dependency; the growth of the welfare state; and economic globalization.

Path dependency—with each government being required to maintain a certain set of policies irrespective of their own policy priorities—implies that voters see little difference between governments. For example, interest groups, the bureaucracy, and the courts all play a role in the policy process, while past policies often institutionalize programs regardless of the party that occupies government. Thus, a range of factors may conspire to make it difficult or even impossible for a government to change policy direction. A wide range of studies have confirmed this conclusion. For example, Imbeau *et al.* (2001: 1; see also Schmidt, 1996) talk of “the average correlation between the party composition of the government and policy outputs is not significantly different from zero,” while Huber and Stephens (2001: 221) found “a sharp narrowing of political differences” on welfare state policies in established democracies during the 1980s.

A second, related, factor undermining clarity of accountability is the growth of the welfare state in the advanced democracies. One explanation for the weakening impact of economic performance on the vote is the development of sophisticated social welfare programs that

mitigate the political effects of poor economic performance. Thus, when unemployment rises, the provision of unemployment benefits helps to cushion the negative consequences for the incumbent government. Pacek and Radcliff (1995), for example, find that voters are less sensitive to the impact of a poorly performing economy in states with extensive social protection than in countries where the unemployed are expected to fend for themselves.

A third factor eroding clarity of accountability is globalization, and the view that the international economic environment is undermining the ability of governments to make independent economic policy. Hellwig and Samuels (2007) show that the impact of economic voting is less in countries that have more exposure to the globalized economy, so that a voter often finds it difficult to make retrospective judgments about his or her government's effectiveness. These effects are especially noticeable in medium or small-sized economies, where economic performance is seen to be determined by decisions in Beijing, Brussels, or Washington, rather than in the country's national capital (Hellwig, 2001). Economic voting in Australia, for example, is noticeably lower than in comparable economies, due in large part to the view that global economic forces wield more influence over economic conditions than the incumbent government (McAllister, 2011).

While most light has been shed on how economic voting is structured by political institutions, the same logic applies to other issues that voters might be concerned about, be it the environment, health, or education (Anderson, 2007; Kedar, 2009). In each case institutions will mediate citizens' views of the issue in question, and help to shape whether or not voters feel that the incumbent government can be rewarded or punished for their performance. For example, a national government cannot resolve the global warming problem, but it can be seen to be assisting or hindering a solution through national policies. Extending the logic further, institutions also help to mediate more general evaluations of system performance, such as confidence and trust in political institutions. To the extent that a clear line of accountability can be drawn between the performance of the institution and the government, the government stands to be rewarded or punished for its performance.

Partisanship

Few explanations for voting behavior have attained the status of party identification. First advanced in 1960 in *The American Voter* study, party identification rapidly became the dominant explanation for voting behavior in the U.S. and inspired a wave of research across the other established democracies. It gained a new lease of life with the collapse of communism, and scholars have examined the formation of partisanship in the new democracies of central and eastern Europe. These studies have generally found that partisanship is formed in these new democracies in much the same way as the established democracies, assuming the existence of similar institutional structures that foster open party competition and help candidates to forge a link with voters (Brader and Tucker, 2001, 2008; Dalton and Weldon, 2007; Whitefield, 2002).

While the roots of partisanship are attributed to group membership and to parental socialization, in recent years studies have moved to examine the contribution of institutional arrangements and political context to the formation, strength, and direction of party identification. As a result, we know a considerable amount not only about the individual-level characteristics that shape partisanship, but also about the system-level characteristics as well. In general, partisanship is more likely to form in countries that sustain "institutions that encourage retrospective clarity of responsibility" (Huber *et al.*, 2005: 366). This is more likely to occur in systems that have fewer legislative parties and stronger party discipline, so the choices that voters have to make are

more sharply defined. Voters will therefore adopt distinct policy preferences based on what the parties offer, and that in turn will encourage debate, competition, and the accumulation of information about the policy choices that are on offer (Bowler *et al.*, 1994).

The importance of the types of institutional context noted previously in shaping both the direction and strength of partisanship has also been shown to vary according to the cognitive skills of citizens. Huber *et al.* (2005) show that the impact of institutional context on partisanship has the strongest impact on individuals who have the fewest cognitive resources, measured by educational attainment. For these low-resource individuals, partisanship is a valuable heuristic for making political choices when other information is limited, so partisanship plays a greater role in low information environments or among those with limited political knowledge (Dalton *et al.*, 2011). This has particular implications in new or emerging democracies, especially where literacy may be low and/or the party system has yet to consolidate.

The type of electoral system has particular consequences for partisanship. In general, more proportional systems appear to lead to stronger partisanship and provide a stronger guide to voting. Tverdova (2011) shows that when the effect of candidates on voting is measured against the effect of partisanship, partisanship becomes a more important element in more proportional systems. This is at least partly a consequence of the larger number of parties in PR systems, so voters feel themselves more adequately represented. By contrast, in a two party system voters may feel that one party provides an inadequate representation of their views. Voters may therefore feel less inclined to identify with one or other party and if they do, to have weaker identification.

Other aspects of the electoral and party system also help to shape partisanship. The frequency of elections and whether the system is based on voluntary or compulsory voting are factors. Australia's internationally high levels of partisanship are often attributed to compulsory voting combined with a three-year national election cycle. The net effect is that the vast majority of Australian voters attend the polls regularly thus putting parties at the forefront of their minds (McAllister, 2011). The party system matters not just through the number of parties but in the expectations voters have of the post-election coalition arrangements that are usually made during the election campaign, and in the salience of policies (Curini and Hino, 2014). In multi-party systems where coalition governments are the norm, voters may alter how far they use their partisanship as a guide to voting depending on what coalition arrangements are likely to emerge after the election.

Partisanship is perhaps the most pervasive mass political view; even in countries where party identification has been in decline, a large majority of the electorate still identify with a political party. The fact that the direction and strength of partisanship is partly predicated on the design of political institutions confirms the importance of macro influences on individual voting behavior. It also suggests that where partisanship is in decline, institutional changes may be one mechanism for halting that decline.

Strategic and split ticket voting

Strategic voting (sometimes called "sophisticated" voting in the U.S. or "tactical" voting in Britain) occurs when a voter casts a ballot for a party other than the one he or she prefers. This event can occur when the voter believes that voting for a party other than his or her preferred party will help to bring about a particular outcome, such as blocking the election of a particular party. Strategic voting therefore combines two elements: a voter casting a ballot for a party or candidate that was not their most preferred; and a reasonable expectation about the outcome of the election (Blais *et al.*, 2001: 344; see also Cox, 1997). A strategic voter is usually distinguished from a sincere

voter, who votes for her preferred party regardless of the consequences, and a momentum voter, who votes for a party because she believes it is likely to win (Blais and Gschwend, 2011: 176).

While the normative reasons for strategic voting have long been understood and analyzed (see, for example, Cox, 1997), the empirical evidence for it has been elusive. The rise of third parties in Britain during the 1970s and 1980s provided much of the impetus for measuring strategic voting. Early attempts to measure strategic voting used respondents' self-reports (for example, Heath *et al.*, 1991) or inferences from aggregate election returns (for example, Johnston and Pattie, 1991). More recent approaches have included modeling the proximity of voters to the parties on a range of issues, and determining if voters deviate from voting for the party closest to them (Alvarez and Nagler, 2000; Blais *et al.*, 2005). In practice, these various methods tend to estimate the number of strategic voters in any one election at around 4 to 6 percent of all votes cast (Blais *et al.*, 2005).

The electoral system is usually identified as the major institutional factor that determines the level of strategic voting in any given system. In general, most studies see plurality systems as most susceptible to strategic voting, since voters have good local knowledge concerning the competing parties and candidates as well as accurate assessments of the likely election outcome. Accordingly, there has been considerable research on strategic voting in the Westminster democracies, notably the U.S. and Britain (see Alvarez *et al.*, 2006). More recently, European countries based on proportional electoral systems have been the subject of in-depth analysis, in addition to several comparative studies (see Blais and Gschwend, 2011; Gschwend, 2004). These studies aimed at proportional systems have concluded that strategic voting does exist, particularly when voters are concerned about what coalition government might be formed after an election (Kedar, 2009).

Isolating the common factors that may shape strategic voting in the various studies that have been conducted is problematic because of differences in country coverage, methodology, and the plethora of contextual differences that exist. However, several conclusions seem clear. First, the evidence does suggest that strategic voting is substantially conditioned by the electoral system, with majoritarian systems showing higher rates of strategic voting when compared to proportional systems. Second, the proportion of voters who vote strategically tends to be small, typically much less than 10 percent of the electorate, largely because the opportunity to vote strategically occurs only in a minority of districts. Third, there is good evidence that strategic voting has increased in recent years.

In addition to the option of voting strategically or sincerely, voters in some systems have the opportunity to vote for different legislatures in the same national election. A voter may therefore choose to vote for one party in one legislature and for another party in a different legislature. This phenomenon is known as split ticket voting and it has become a common feature in many electoral systems, particularly the U.S. (McAllister and Darcy, 1992). This is called "vertical" split ticketing, as the act occurs between different levels of government (Burden and Helmke, 2009). The alternative is "horizontal" split ticket voting, which occurs in parallel electoral systems, where voters can simultaneously cast a ballot in a constituency contest and a national party list (McAllister and White, 2000). The common feature between the two types of split ticket voting is that voters will be motivated by different things in the choices they make, and these motivations are a consequence of election rules.

The rise in the incidence of split ticket voting in many countries is usually attributed to declining popular trust in parties. Fiorina (1992) argues that this widespread suspicion of parties is reflected in a preference for divided government, so no single party has the capacity to govern unhindered and therefore potentially to dominate the electorate. Other research has endorsed the view that voters' motivations are tactical and positive rather than dysfunctional and negative.

Bowler and Denemark (1993) use aggregate data to argue that the electoral systems of the Australian upper and lower houses create very different structural opportunities for voters to cast their ballots tactically, and are unrelated to dealignment. In a comparison of Australia and the U.S., Bean and Wattenberg (1998) show that Australian split ticket voters are largely motivated by a desire to see power shared between parties, while this plays no role in shaping split ticket voting in the U.S. Studies in other countries have identified local factors as important, as in Brazil (Ames et al., 2009); the intervention of parties, as in Denmark (Elklit and Kjaer, 2009); and attempts to minimize policy risk, as in Mexico (Burden and Helmke, 2009).

The importance of these forms of voting is largely dependent on the institutional opportunities that exist to permit them to operate. Moreover, even when such opportunities exist, a minority of voters—and often a very small minority—choose to exercise them. They can, however, have a significant impact on government performance. For example, a government that fails to gain control of an upper house through split ticket voting among its supporters risks having its legislative program curtailed. Similarly, strategic voting can change the outcome of many constituency election contests, an outcome that is often unrelated to voting trends in national politics.

Conclusion

Institutions are important influences on citizens' voting behavior. This is hardly surprising; the "new institutionalism" of the 1980s and 1990s was premised on the assumption that institutions shape public norms, values, and beliefs which in turn serve to mould policy outcomes (see, for example, North, 1990). Each of the five topics covered here has presented evidence that confirms that political institutions do matter for voting behavior. The question remains, however, to what extent institutions matter in shaping political behavior compared to non-institutional factors, such as the voters' own characteristics. This raises two issues, one normative and one empirical.

Normatively, distinguishing between the impact of institutions and non-institutional factors on political behavior raises problems of endogeneity. If institutions are a product of the conditions under which they have emerged, then they cease to be independent agents. In other words, "conditions shape institutions and institutions only transmit the causal effects of these conditions" (Przeworski, 2004: 527). If this is the case, then it becomes almost impossible to evaluate the impact of institutions since they are the result of what we are trying to measure. Fortunately, it appears that this endogeneity problem has been over-stated; we know, for example, that particular institutions have specific effects on individual behavior. However, we can see the effects of endogeneity when an institution is implanted into one country and works as predicted, while the same institution implanted in another country results in total failure (Przeworski, 2004).

Empirically, much depends on which institutions and countries are included in the analysis. The CSES project mentioned earlier has done much to clarify this situation. We know, for example, that institutions have relatively little impact on patterns of voter abstention, but that the type and complexity of a party system will impact ideological voting and views of issues, among other things (Klingemann, 2009: 26). So what institutions are included for examination has important implications. The countries included in any sample will also have a major impact on the results, as already noted, since each differs in terms of institutional mix and, not least, in the age of their democracy. Moreover, as institutions change and evolve over the course of time, there may be a lag in the political effects of an institutional change.

Do institutions matter for voting behavior? According to Klingemann (2009: 26), the “results prove that they (mostly) do.” But as in any analysis, evaluations of their importance must be sensitive to the nuances. The study of institutional effects on voting behavior has made major progress in the past two decades. These advances have been predicated on advances in data collection and in methodology. The challenge of the next two decades will be to consolidate the findings of this research.

Note

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ETHNICITY AND ELECTIONS

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Introduction

Ethnicity, broadly defined as the fragmentation of a society along linguistic or religious cleavages, has been used by scholars to explain a large variety of questions ranging from inequality (Alesina and Glaeser 2004) or economic growth (Birnie and Waguespack 2011) to electoral success (Chandra 2007). However, a large bulk of the literature on ethnicity is devoted to explaining the relationship between models of institutional design and political stability (Elkins and Sides 2007; Fearon and Laitin 2003). The, as yet, unfinished academic debate on this question discusses whether ethnically accommodating institutions, like proportional representation electoral systems (PR), parliamentary systems or federalism reduces or not ethnic tensions (Lijphart 1977, 1991; Horowitz 1985).

The consequences of holding elections in divided societies are not yet sufficiently explored. Elections, as procedures regulated by institutions that appoint power holders (see Svolik, [Chapter 6](#)), can be both peaceful mechanisms to solve conflicts or events that trigger social unrest. Consider the following two examples. Ghana has been holding competitive elections since 1992 and two alternations in power have been observed to date. Ethnically speaking, roughly half of the country belongs to the majority Akan group while the remaining 50 percent belong to other different ethnic groups. In 2012, the presidential race was decided by an electoral difference less than 3 percent of the votes. The opposition leader, Akufo-Addo, challenged the presidential election results alleging electoral fraud. Akufo-Addo's allegation was first investigated by the Electoral Commission and later by the Ghanaian Supreme Court and in both cases the fraud allegation was rejected. As a result of such verdicts, Akufo-Addo accepted his defeat. In Kenya 2007, on the other hand, violence spread across the country when elections results were announced and supporters of opposition candidate Raila Odinga—mostly from the Luo and Kalenjin minorities—attacked supporters of the incumbent president Mwai Kibaki, from the Kikuyu ethnic group. The violence originated after Kibaki declared himself victor of an election that was perceived as fraudulent by the voters as well as by international organizations, such as the EU, who were monitoring this electoral process (Cheeseman 2008).² Other examples where ethnicity and fraudulent elections went hand in hand include Sri Lanka³ (2002), Rwanda⁴ (2003) and Ivory Coast⁵ (2010).

These two examples illustrate how elections, and the institutions that regulate them, can play a central role in explaining the political stability of ethnically divided societies. Sometimes institutions facilitate cooperation among groups, but only if they are able to create an atmosphere of trust (see Clark and Gandhi, [Chapter 3](#)). When institutions are biased towards a particular ethnicity or impose grievances to vulnerable groups, then coordination is harder and agitation can be expected. In this chapter, I broadly discuss this question by focusing on the role of elections. To do so, I first analyze the conditions that explain ethnic coordination and the consequences that can be observed in case of coordination or lack of it. I then move to discuss the role of institutions in explaining conflict. Finally, I discuss the role of elections as a mechanism to promote stability but also trigger conflict. In this section, I offer tentative hypotheses about the relationship between ethnicity and electoral fraud, a question still to be fully explored in the literature. The chapter concludes with some general reflections and further questions for research.

Ethnicity and cooperation

Are conflicts, particularly violent ones like civil wars, more likely to be observed in ethnically diverse societies than in homogenous ones? One could think heterogeneous societies are characterized, primarily, by divergent preferences, hence paving the way for conflict. The examples are numerous. Think, for example, about which language to use at school with respect for religious beliefs, or the conditions to form an ethnically-based political party. In democracies where policies are adopted following some majority rule, the contrast can be more striking. In homogenous societies, the policy space is normally decided along ideological lines. In heterogeneous societies, however, such policy space needs to incorporate an ethnic dimension. This makes preference convergence more complicated. In the case of non-democracies, the contrast between winners and losers can be even more pronounced if the autocrat belongs to a particular ethnic group that rivals other existing groups within the country. In these situations, autocrats could impose severe grievances on rival groups in order to consolidate the dominant position of their own ethnic group. In these scenarios, therefore, one should expect more tension and unrest as the level of ethnic fragmentation increases.

Lack of cooperation among different groups may be a relevant mechanism to understand the origin of conflict. Suppose a classic prisoner's dilemma (PD) game, where two groups can cooperate or defect. If groups cooperate, they both obtain an outcome that is Pareto optimum. If both groups defect, then, they obtain a sub-optimal outcome. When the game is played only once, each group is better off defecting than cooperating. The situation may change, however, if the game is played repeatedly. In this case, two types of outcomes may be observed. On the one hand, defecting may be an equilibrium if there are no conditions enforcing cooperation. On the other hand, however, cooperation may be imposed if groups are confident that no one will defect if the other cooperates. Such cooperation may be enforced by rules previously agreed upon by groups whereby defectors may be punished.

When cooperation is absent, the provision of public goods, for example, may be undermined. Alesina *et al.* (1999) show how cities' expenditures on public goods—like education, water sanitation or roads—negatively correlate with ethnic fragmentation. The more ethnically fragmented a city, the less likely it is to enjoy public goods. Alesina and his collaborators use three different datasets from US cities, metropolitan areas and counties to show empirical confirmation for these claims. To explain this finding, the authors elaborate an argument using a voter's utility on tax spending. Under a context of high ethnic fragmentation, voters are resistant to increase taxation if public spending aims to provide public goods to members from

ethnic minorities. When this happens, members of the majority group prefer a lower level of public goods. Two reasons explain this finding; first divergent views may exist about which public good to produce; second, the larger the number of groups using a particular public good, the lower the utility obtained by each group. These non-mutually exclusive reasons would explain why in this context, members of the largest ethnic group would prefer to support private initiatives.

In the US, the decreasing utility obtained by members of the white majority when asked to contribute to welfare-programs enjoyed mostly by the black minority constitute one dimension of what Alesina and Glaeser (2004) denominate the “American exceptionalism.” This particular effect of ethnic fragmentation plays an important role in the explanation provided by these scholars in understanding differences in income and poverty levels between Europe and the US. In fact, the difference in racial fractionalization alone explains half of the difference in levels of redistribution between Europe and the US. Again, their argument is based on the lack of cooperation between different racial groups.

How exactly does ethnic diversity undermine the provisions of public goods? Using experimental data from an urban slum in Kampala, Uganda, Habyarimana and his collaborators (2007) test three different mechanisms in order to answer this question. The first, a preference mechanism, is based on how distinct features of particular ethnic groups shape preferences for particular types of public goods. Different preferences would imply a “diversity of tastes,” which could fuel conflict and result in under-provision of public goods. The second mechanism, technology, is based on the conditions available for co-ethnics only to promote collective action. For example, homogenous groups may be significantly effective in providing public goods as they share a large pool of cultural resources like language and common standards of understanding. The third type of mechanism, strategy, is based on the idea that members of communities behave differently depending on mutual social interactions implying that cooperation exists only among co-ethnics. Public goods are produced when this mechanism is considered because of the capacity of the community to punish co-ethnics who do not cooperate.

Habyarimana *et al.* (2007) find no support for preference mechanisms. They find that providing public goods is not related to either sharing a preference for such particular good, or by having different values regarding welfare improvement between co-ethnics and members of other groups. What seems to explain the provision of common goods are, above all, the strategy and technology mechanisms. Under the observance of such rules enforcing cooperation, public goods are well provisioned. Here, cooperation can be understood in various forms. Firstly, co-ethnics cooperate if they know of effective institutions that credibly punish defection. Secondly, collective action in homogenous ethnic groups is effective when there are rules policing defection.

Creating conditions that increase cooperation between and within groups reduces the level of conflict. As Fearon and Laitin (1996) show using data from Africa, ethnic violence is, in fact, rare. This finding is also consistent with Fearon and Laitin (2003) where they analyze the causes of civil wars. Here, they conclude that not only is violence a rare phenomenon, but also that ethnicity does not account *per se* for a significant increase in the probability of observing such conflict. So, an interesting question remains about what explains peaceful cohabitation among different groups.

At the theoretical level, Fearon and Laitin (1996) develop two distinct explanations that would explain peaceful co-existence among different groups, which use the conditions of group cooperation as the core of these mechanisms. Using, again, a prisoner’s dilemma game, the emphasis remains on the conditions that promote only cooperation in a repeated PD game. Cooperation emerges in situations where trust exists, and this trust is based on the reputation of

actors. If actors build a reputation of respecting agreements, trust increases and hence conditions for cooperation are met.

Information plays a key role in building up trust. Cooperation between co-ethnics can be explained by accessing information about defectors. Ethnic groups are characterized by creating high levels of social networks, which decrease the cost of information (see Lauth, [Chapter 5](#)), and by using those channels, defectors can be identified and punished. In the case of interactions between different groups an asymmetry of such information exists where defectors of one group cannot be easily identified by members of the other group. So, how does cooperation emerge in this context of information asymmetry? The first mechanism is based on a spiral equilibrium. Suppose that a member of group A defects. When this happens, members of group B hold all members of group A accountable by punishing them. This reaction would produce an escalation of conflict resulting in the break-up of intergroup relations. Anticipating the expected loss of breaking up intergroup relations, is what would explain cooperation even in the case of individual defection.

A second mechanism that could explain inter-ethnic cooperation is based on an in-group policing equilibrium. The dynamics of this mechanism are based on the confidence that group A members have on group B's capacity to punish their own defectors. Since members of group A (or B) know that individual defecting behavior will be punished, they ignore transgressions and continue to cooperate.

To explain cooperation, or the lack of it, institutions may matter. Ethnicity may explain why public goods are undermined in the US but the nature of institutions may also play a role. In fact, a combination of high levels of ethnic fragmentation and majoritarian institutions seems to be a convincing mechanism to understand differences in redistribution (Alesina *et al.* 2001). Birnir and Waguespack (2011) show that institutions providing the inclusion of mobilized ethnic groups promote economic growth. Indeed, including ethnic groups in the policymaking process has several positive properties that foster growth. Firstly, citizens' preferences are better represented; and secondly, inclusion of ethnic groups increases the overall quality of policies precisely by having different preferences included. Finally, by increasing the number of veto players, policy stability increases.

The role of political institutions is, however, better perceived when explaining ethnic conflict. There is a rich and ongoing debate about how ethnic political accommodation can be used to understand conflict among ethnically diverse countries.

Ethnicity and political accommodation

In 1977, Arend Lijphart published his famous book *Democracy in Plural Societies* where he explained a fundamental question in empirical theory of democracy: how political stability would be achieved in ethnically divided societies. Lijphart's starting point was a reaction against the empirical and theoretical findings put forward by pluralist scholars like Lipset (1960) who explained why democracy in the US was never challenged. In a nutshell, Lipset argued that the success of US democracy was the combination of a society organized around crosscutting cleavages—people feel identified with different interests—and majoritarian institutions. Crosscutting cleavages generated political moderation and partisan division; majoritarian institutions, like the use of single-member districts using FPTP, facilitated alternation in power. The combined effect of these factors allowed political losers to have the expectation of becoming a political winner in a reasonable period of time. Hence, democracy became an equilibrium which explained its own survival (Przeworski 1991).

Lijphart challenged this view by arguing that in multi-ethnic societies neither cleavage would be crosscutting nor majoritarian institutions would generate alternation in power of rival

options. On the contrary, ethnically heterogeneous societies are characterized by having mutually reinforcing cleavages that increase the cost of creating cooperative mechanisms between different groups. Furthermore, in societies where a dominant group exists, majoritarian institutions exacerbate that superiority and ethnic minorities have little opportunity to participate effectively in the decision-making process. In a scenario like this, democracy would not be a centripetal mechanism as expected by Lipset but rather a centrifugal one.

The only way to reduce the risk of political instability in divided societies is, according to Lijphart, to adopt a consociational democracy. This model of democracy seeks “to share, diffuse, separate, divide, decentralize and limit power” (Lijphart 1977: 168) and it is articulated around four main features: grand coalition, autonomy, proportionality and mutual veto (Lijphart 2000). In terms of constitutional design, Lijphartian consociational democracies are characterized by using large electoral district magnitudes with proportional electoral formulas, parliamentary systems, a federalist territorial organisation, and grand-coalition decision-making arrangements. This power-sharing institutional setting would allow all groups to be equal partners in the decision-making process and that, Lijphart hypothesized, would increase the levels of political stability. The effects of consensus democracy in general, and of power-sharing institutions, in particular, have been empirically tested in relation to democratic outcomes and political stability.

Anderson and Guillory (1997) used different institutional arrangements to explain variation in the levels of satisfaction with democracy. After analyzing survey data from 11 European countries, they found that political losers—voters whose party did not win—were, on average, less satisfied than political winners—voters of the ruling party. However, institutions are important in that those political losers who lived in a consensus democracy were more satisfied with the way democracy worked than political losers living in a majoritarian democracy. For Anderson and Guillory (1997), power-sharing institutions such as PR and coalition governments, offered electoral losers the opportunity to more actively engage in politics. As a consequence, satisfaction with democracy for these political groups increased. A similar result is found by Ruiz-Rufino (2013) in relation with ethnic minorities from seven post-communist countries. In ethnically divided new democracies, institutions are also important to understand levels of satisfaction with democracy. Ethnic minorities living in countries with greater access to the decision-making processes have higher satisfaction with democracy than excluded minorities. In particular, if ethnic groups can articulate their preferences through a political party in a parliamentary system, then, the perceived level of satisfaction is increased. Electoral proportionality is also important but not as fully expected by Lijphart: ethnic minorities whose parties are small will be more satisfied with democracy under PR than under majoritarian electoral systems. However, as a political party wins support, then, satisfaction with democracy increases as proportionality decreases.

Lijphart's contribution to an empirical theory of democracy has been mostly tested, however, in relation with political stability and ethnic conflict. Saideman *et al.* (2002) use the Minorities at Risk dataset (MAR)⁶ to conclude that PR significantly reduces ethnic violence. A similar finding was also obtained by Cohen (1997) using a different sample. Reynal-Querol (2002) analyses 138 countries in the period between 1960 and 1995 to investigate the causes of ethnic civil wars. This analysis shows that consensus democracies reduce the spells of wars because these regimes offer institutional incentives to peacefully accommodate all ethnic groups. Finally, Alonso and Ruiz-Rufino (2007) argue that political institutions reduce the levels of ethnic conflict if, and only if, they incorporate the voice of ethnic minorities into the decision-making process. It is not enough to provide political representation if their voice in parliament is not effective. Ethnic minorities become moderate if their political parties actively participate in elaborating the different policies of the country. In other words, the combination of political representation with a parliamentary system reduces the probability that an ethnic minority will revolt against the state.

The main criticism of Lijphart's consensus democracy comes from Horowitz (1985, 1993, 2002). Donald Horowitz questions Lijphart's assumption that in multi-ethnic societies, stability is better achieved when ethnicity cleavages remain active rather than trying to diffuse them. Furthermore, for consociationalism to work, elites must play an active role in containing conflict. This implies a second assumption: the interest of all ethnic elites to reach agreements in order to pursue accommodation, but these premises do not accurately reflect the actual working of divided societies (Horowitz 1985). Why would leaders of majority groups cooperate in those scenarios where they can control all major political instances? The solution Horowitz offers is to establish power-sharing institutions where ethnic cleavages are not clustered. Accordingly, he rejects both PR and parliamentary systems as the optimum institutions to promote political stability in ethnically heterogeneous societies. PR is rejected on the grounds that it may increase intraethnic competition allowing the success of ethnic radical parties. In this sense, PR would have the unintended centrifugal effect of moving political competition away from the median voter to the extremes. Presidential systems are preferred to parliamentary systems because a single political figure chosen with the support of various groups is a more efficient way to accommodate groups than grand coalition cabinets resulting from parliamentary systems.⁷

A number of empirical studies have supported Horowitz's claims. Reilly (2002, 2001) has defended empirically and theoretically the virtues of the Alternative Vote (AV) in opposition to the use of PR. The main finding is that AV reduces the saliency of ethnicity by forcing selected candidates to get support from members of various ethnic groups. As a consequence a more moderate ethnic position is achieved as illustrated by the successful stories of Fiji and Papua New Guinea. The empirical evidence supporting presidential systems as an alternative to parliamentary systems is also mixed. Cheibub and Limongi (2002) found that the survival of regimes was not only dependent on whether a country was parliamentary or presidential but in how the whole decision-making process is designed. Saideman *et al.* (2002) do not find parliamentary/presidential systems statistically significant, although presidential systems seemed to reduce ethnic conflict more than parliamentary ones. Finally, Pippa Norris (2008) concludes that presidential systems are more associated with political instability than parliamentary democracies.

Federalism has also been seen by consociational scholars as an institutional mechanism that could ameliorate ethnic tensions. However, solid empirical evidence shows the opposite effect. The relationship between decentralization and ethnic conflict and secessionism has been broadly investigated by Dawn Brancati (2006, 2009). Her research concludes that there is a general effect of decentralization on reducing ethnic rebellions in decentralized countries. However, if regional parties are strong, then ethnic conflict may increase since these parties use decentralized political structures to reinforce ethnic cleavages and polarization.

This literature, however, overlooks the relationship between elections and political stability. It has been argued that elections are important since they are conducive to self-government (Przeworski 2010) but it has also been noticed that a relevant number of elections have been rigged in the last decades (Kelley 2012). The conditions to understand how elections, in a context of ethnic diversity, contribute to imposing peace or to triggering conflict are not fully clear. This question is the focus of the next section.

Ethnicity and elections

As discussed in the previous section, a pivotal issue within the Lijphart-Horowitz debate is how electoral rules account for variations in the level of conflict in divided societies. Elections, when they are competitive, seem to be effective mechanisms to solve conflicts in divided societies. Political competition and periodic elections maintain civil peace by allowing alternation in

power of rival groups. It is the expectation of winning in the future that prevents today's electoral losers from revolting (Przeworski 1991, 2005, 2011). One way to look at how elections serve to resolve conflict is by looking at the effect of electoral rules and also the conditions that establish the creation of political parties in ethnically divided societies. This is termed a procedural mechanism.

Using novel approaches, recent research explores the factors that explain why competitive elections increase political stability. John Huber (2012) investigates to what extent the use of PR actually favors ethnic politicization. According to Lijphart, PR promotes ethnic politicization since ethnic groups have the opportunity to vote for seat-winning ethnic political parties. To test this idea, Huber develops four measures of *ethnicization* based on two dimensions: polarization-fragmentation and group-based with party-based perspectives. The result is four indicators of ethnic electoral behavior used to test the effect of PR in 43 countries. In all different models, the effect of PR is negative. Or, alternatively, elections that use proportional rules decrease the level of ethnic politicization. There are two explanations to understand why this occurs.

First, in single-member districts, the probability of success of an ethnic party is small if the ethnic constituency is geographically dispersed. In this case, voters may coordinate and opt for one of the catch-all large parties, and by doing so, ethnic claims earn visibility. If an ethnic minority is, however, not geographically concentrated and is large enough then politicization occurs simply because of the success of the corresponding ethnic party. This logic does not seem to work under PR. According to Huber, PR allows the success of various ethnic parties calling at the same ethnic constituency. If this happens then the intensity of ethnic claims may diminish. So while proportional rules accommodate ethnic parties into the decision-making process, they also decrease the polarization of ethnic political behavior. These empirical findings are surprising. Using Huber's own words:

Ironically, this implies that if one accepts the Horowitz argument that the goal should be to depoliticise ethnicity in elections, one should adopt the electoral institutions advocated by Lijphart.

(Huber 2012:1000)

Some scholars have argued that ethnic politicization may destabilize democracy by producing an outbidding effect (Rabushka and Shepsle 1972; Horowitz 1985). Such an effect occurs when the activation of a particular ethnic group materializes in creating an ethnic party that in turn triggers the creation of other different ethnic parties. Given that different ethnic groups may have different preferences, an increase in the number of ethnic parties may endanger democratic stability. Under this scenario, elections may not serve to solve conflict but, contrarily, to increase it.

Kanchan Chandra (2005) shows how increasing the number of ethnic parties does not necessarily lead to an outbidding effect. On the contrary, it is precisely when the formation of different ethnic parties along different ethnic dimensions is allowed that conflict is minimized and political stability enhanced. Chandra illustrates her theory by studying ethnic politics in India and concludes that political institutions regulating party formation do matter. Restrictive institutions that only allow the formation of parties along one single ethnic dimension, such as language, may promote greater instability than institutions that allow the formation of parties along different dimensions.

There is, at least, a second mechanism linking elections with political stability: the competitive mechanism. In some contexts, voters may use competitive elections to maximize their level of welfare regardless of their ethnic affiliation. When this is the case, elections might work as

catalysts of bad policies and alternation in power would simply imply punishing incumbents for wrong-doings. Take again the case of Ghana. Since 1992, this Sub-Saharan country has held six competitive elections and has experienced two alternations in power. The largest ethnic group in Ghana, the Akan, represents about 49 percent of the population. The remaining 51 percent is split among several other groups including the influential Ewe minority group (13 percent). Despite having had ethnic tensions in the past, Ghana is now considered a well-functioning democracy in Africa where ethnic rivalries, if any, are resolved by ballot boxes.

In fact, ethnicity does not seem to be a key component in understanding voting behavior in Ghana. As Ichino and Nathan (2013) show, being loyal to one's own ethnic party is not always the case, especially in rural areas. If a member of a particular ethnic group lives in a rural area where a large majority belong to a different group, such a member is likely to vote for the party supporting the largest group rather than voting for their own party. This is so because voters favor the provision of public goods—such as roads—over voting along ethnic lines. In urban areas, however, this type of behavior is not observed. So, at least in Ghanaian rural areas, elections can work as mechanisms that enhance coordination among different groups and reduce ethnic tensions.

Ethnicity is, however, not completely irrelevant in Ghanaian electoral politics. On legal grounds, the 1992 Constitution of Ghana only recognizes those political parties that have a “national character” and where membership is not based on “ethnic, religious, regional or other sectional divisions” (art.55.4). In practice, however, it is assumed that the two major parties, the National Democratic Congress (NDC) and the New Patriotic Party (NPP), receive support from different, and historically rival, ethnic groups. While the NPP is supported by the largest ethnic group (Akan), the NDC electoral bases come from the Ewe minority. Such support is, however, not unconditional during election period. Hoffman and Long (2013) investigate the extent ethnicity was a key factor to explain party identification during the 2008 presidential elections. Using their own exit-poll survey, they concluded that ethnicity cannot explain voting behavior *per se*. To understand the electoral success of a Ghanaian party during the 2008 presidential election, one also needs to take into account its performance during office tenure. If such a party were successful in delivering essential services like health and education, then, voters rewarded it regardless of their ethnic identification.

The examples borrowed from Ghana, serve to show how elections work as substitutes of violence when they are credible and generate results that are accepted by all contending parties. When these conditions are met, ethnicity does not play a large role in destabilization.

This situation, however, may reverse if election outcomes are contested and not accepted by some of the parties. When this happens, political instability may erupt and ethnicity may fuel violence. This claim finds some empirical support. Using the NELDA dataset compiled by Hyde and Marinov (2012), the relationship between post-electoral violence and allegations of fraud can be explored. About 14 percent of the 3,000 elections analyzed by Hyde and Marinov resulted in some form of post-electoral conflict. Such conflict ranged from popular protests to riots and, in 60 percent of the cases, the government used some form of violence against demonstrators.

Is violent contestation most common in ethnically divided societies? [Figure 24.1](#) compares the NELDA indicators of post-electoral violence and fraud with ethnic fragmentation as calculated by Fearon (2003).⁸ Post-electoral violence was observed in 182 elections that occurred in countries with an ethnic fragmentation of at least 4. In other words, ethnic fragmentation was important in about 62 percent of the total number of elections where some form of violence was observed. But how exactly could ethnicity be related to a higher probability of rigging elections?

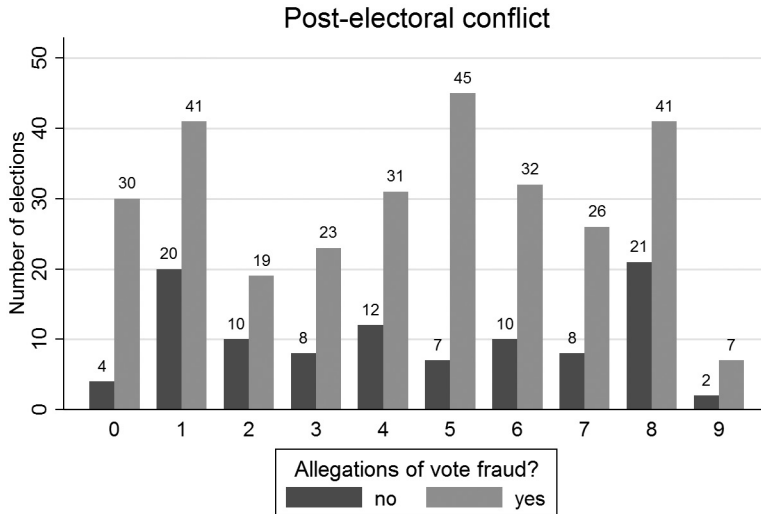


Figure 24.1 Post-electoral conflict, fraud and ethnicity

Sources: NELDA dataset; Fearon (2003)

Trusting elections in ethnically divided societies

If elections, when trusted by all parties, are important mechanisms to prevent conflict, then an interesting question is to see how trust can be achieved. One straightforward mechanism would be the use of transparent rules and procedures fully accepted by all contending parties. New research on electoral integrity shows that rules establishing clear procedures regarding the running and management of elections increase confidence in political institutions (Norris 2014). A second way of increasing trust is to invite third parties to monitor elections. Recent findings from experimental data show that the presence of international electoral monitors increases voters' confidence in elections (Brancati 2013). Electoral observers are, however, not a sufficient condition to expect clean elections. According to the data collected by Judith Kelley (2012) about a third of the elections that were monitored between 1970 and 2007 were considered somehow fraudulent. An immediate question follows this finding: why are some elections rigged while others are not?

Alberto Simpser (2013) theorizes about how some incumbents have incentives to deliberately engage in electoral misconduct. Incumbents with a large level of support would see electoral misconduct as a demonstration of power. This electoral *tour de force* would send a signal to political opponents about the real strength of the incumbent with the intention of discouraging opposition forces. Using historical data from Costa Rica, Lehoucq and Molina (2002) conclude that institutional rules as well as socio-economic conditions are essential explanations for electoral fraud. Using a comparative approach, Sarah Birch (2007) argues that electoral fraud is more likely to be observed in single-member districts (SMD) than in multi-member districts (MMD). Analyzing a sample of countries from Central and Eastern Europe, Birch's logic is developed along two complementing explanations. Firstly, electoral systems using SMD are advantageous to large parties while MMD is not. This bias towards large parties observed in SMD creates a window of opportunity to cheat for seat-maximizers' parties. Secondly, candidates in SMD play a relevant role in the election and, therefore, their capacity and incentives to engage in clientelistic behavior leading to fraud are higher than under MMD.

Electoral misconduct has important political consequences on the party system, turnout and the strategies of the opposition. As Donno and Roussias (2012) show there is a clear relation between electoral fraud and the number of competitors. Incumbents cheating during the pre-electoral period activate a deterrence mechanism which affects both parties and voters. The number of competing parties is significantly reduced and voters are discouraged from voting. Electoral turnout can also be explained by voters' perceptions on the quality of elections. Focusing on Mexico's PRI, Alberto Simpser (2014) shows that attendance at voting stations was higher when voters perceived that the PRI would allow a clean competition. Using data from the Comparative Study of Electoral Systems (CSES), Sarah Birch (2010) finds that when voters perceive that elections had some form of irregularity, their probability to vote decreased significantly.

Does ethnicity play any role in explaining electoral fraud? Are rigged elections more likely to be observed in countries where there are strong ethnic divisions? As shown in the previous section, elections may be a peaceful substitution for conflict but fraudulent elections may distort this mechanism and thus instability may emerge. In cases where ethnic divisions dominate social and political life, fraudulent elections may turn to violence as the case of Kenya illustrated at the beginning of this chapter. If so, then, the preceding sections open questions still unexplored in the literature: how does the political organization in multi-ethnic societies account for episodes of electoral fraud? Are elections rigged more often in countries where one minority dominates over the rest than in countries where several minorities share power? These are mostly empirical questions but there are theoretical reasons to build an argument around them. One possible way of approaching this question could be as follows.

Suppose a country where various ethnic groups must co-exist and one of those groups is clearly dominant with a majoritarian political system adopted. This, as previously discussed, would mean increasing the probability of having a significant part of the population excluded from the decision-making process and consequently also increasing the risk of social unrest and political instability. The leaders of the dominant group may be aware of such risk and may implement various institutional mechanisms to reduce tensions. For example, if dominated groups are geographically concentrated, one way to reduce tension could be to use single-member districts so that those minorities could elect their representative for that particular geographical zone. But it is not clear whether giving minorities access to elect a representative would reduce ethnic tensions in a system with majoritarian institutions. Representatives from "dominated" minorities would soon find their incapacity to participate in the decision-making process and frustration, as a prior step of unrest, would probably emerge. In anticipating this, the dominant group may decide instead to discourage other ethnic groups from further political participation and, following Simpser (2013), use every election to show their strength by engaging in fraudulent acts in districts where a seat could be challenged by different ethnic groups.

Suppose now that a power-sharing institutional setting exists in a country with no dominant ethnic group. In this case, electoral fraud should be harder to implement for various reasons. First, the decision-making process is controlled by various groups that effectively may check each others actions. Committing fraud requires a certain level of resources in order to conceal the illegal nature of the action. In a system where power is shared, separated and diffused among different ethnic groups such acts of secrecy may be difficult. Second, large districts using PR formulae may reduce the incentives to cheat in the elections. As the rich literature on electoral system has shown (Duverger 1954; Rae 1967; Taagepera and Shugart 1989), electoral systems that use SMD generate absolute winners and losers while systems using large district magnitude and PR formula produce relative winners and losers. This redistribute capacity of PR to allocate

seats may function as a mechanism to discourage ruling parties from rigging elections. Ruiz-Rufino (2014) shows that PR works in this direction when analyzing variations of pre-electoral harassment, a form of electoral misconduct. Focusing on the joint effect of proportionality and electoral competition, Ruiz-Rufino finds that only when proportionality is low and the electoral gap between rival parties is at its minimum, are incumbents more likely to engage in harassing the opposition.

Following this logic, electoral fraud should be more likely to be observed in multi-ethnic countries with electoral systems using low district magnitudes and where one of the groups has a politically dominant position, than in countries using PR and where no ethnic group dominates. A rough and impressionistic analysis of the data points in this direction. Figure 24.2 shows the mean district magnitude in democratic parliamentary elections that were declared acceptable by the international community after distinguishing between dominant and power-sharing ethnic groups.⁹

The top left and bottom right scenarios in Figure 24.2 illustrate the theoretical claims made. In both cases, irregular elections occur when on average district magnitudes are small. However, when one ethnic group is dominant and the other group—or groups—do not share political access, electoral fraud is observed 30 percent of the time. If there is no ethnic dominance and all groups share power, fraud is observed only 20 percent of the time. Also, under this particular ethnic power distribution, the average magnitude is much larger compared with a scenario of ethnic dominance. This finding is, naturally, not conclusive and deeper empirical analysis would be required to find confirmation for the explanations displayed. Further theoretical analysis is also required to understand scenarios where one ethnic dominant group co-exists with other groups that have power-sharing capacities. But in any case there are some reasons to think that

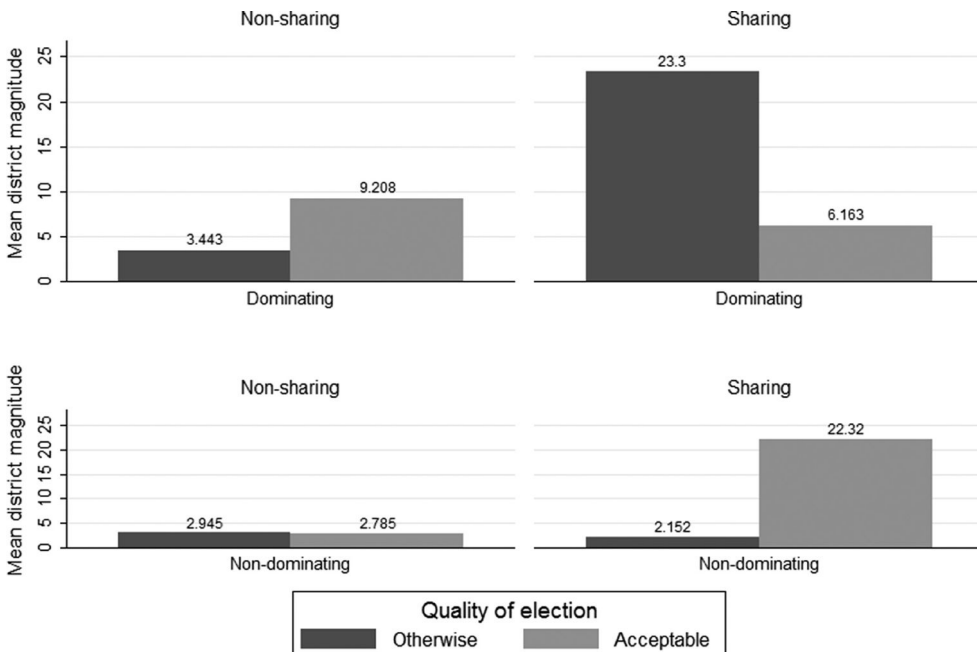


Figure 24.2 Ethnicity, electoral institutions and the quality of elections

Source: EPR dataset; DIEM dataset

ethnicity may play a role in explaining electoral fraud and, hence, expand our knowledge about when elections may be trusted.

Conclusion

This chapter shows how institutions play a central role in understanding political instability in ethnically divided societies. The interaction between the institutional design and the type of ethnic composition is crucial to explain cross-country variations in the level of conflict. Societies where institutions promoting ethnic coordination exist provide better public goods and social peace. The outcome of institutional blueprints to reduce ethnic tensions is, however, not yet clear and particular mechanisms need to be investigated to understand why some ethnic groups decide to revolt. Giving a voice in the parliament, for example, may not be sufficient if such a voice does not play a relevant role in the decision-making process.

If ethnic tensions are mostly derived after arrangements to decide who manages power and resources, focusing on elections may also be useful to understand political stability in countries with high levels of ethnic fragmentation. There are theoretical and empirical reasons to see how elections may be substitutes of bloody conflict. But elections can also trigger conflict if they are rigged and serve to impose the dominance of one ethnic group over another.

Several questions still remain unclear and require future research. For example, there is still not sufficient cross-country evidence on the effect of ballots on stability. As shown, there are theoretical reasons to believe that open ballots may force some level of ethnic coordination which, eventually, may reduce tension. The empirical evidence is, however, limited and the systematic effect of open ballots remains unclear. It is also not completely clear which mechanisms account for electoral fraud in ethnically divided societies. In this chapter, I have sketched some hypotheses based on the political relevance of ethnic groups but further theoretical and empirical research is still needed. Finally, if electoral observation missions really serve to reduce electoral fraud, then one question remains unanswered about whether these missions are more likely observed in ethnically fragmented than in homogenous societies.

Notes

- 1 I am grateful to Jen Gandhi, Christel Koop and Anna Gwiazda for their helpful comments and suggestions on previous versions of this chapter. I also thank Gemma Derrick for her patient reading and edition. All mistakes and errors are exclusively mine.
- 2 http://eeas.europa.eu/eucom/pdf/missions/kenya_2007_final_preliminary_statement.pdf
- 3 <http://aceproject.org/ero-en/regions/asia/LK/sri-lanka-final-report-parliamentary-elections-eu-1>
- 4 www.ipu.org/parline-e/reports/arc/2265_03.htm
- 5 www.eucom.eu/files/pressreleases/english/executive-summary-25012010_en.pdf
- 6 Most of the literature that analyses the relationship between ethnic conflict and institutions uses MAR as the main data source. The complete dataset can be accessed at www.cidcm.umd.edu/mar/. Robert T. Gurr's work (1993, 1994, 2000) focuses on discussing the nature of the data and the main criticism has come from Fearon and Laitin (2002). For a developed account on the MAR selection problem see Birnir *et al.* (2011).
- 7 A comprehensive summary of Horowitz's critiques to consociational theories can be found in Horowitz (2002). For a good discussion on the Lijphart-Horowitz debate see also Choudhry (2008).
- 8 The ethnic fractionalization index calculated by James Fearon ranges from 0 (complete homogeneity) to 1 (full heterogeneity). To ease interpretation of the graph, this index is recalculated in 10 categories where 0 indicates values between 0 and 0.9999 or 5 indicates values between 5 and 5.9999.
- 9 The data on the quality of elections comes from the DIEM dataset collected by Kelley (2012). This variable indicates whether an election, previously monitored by an independent organization, was declared fully acceptable by such organization or not. In case of not being acceptable, it does not necessarily imply

that there was fraud. It simply indicates that the procedural aspects of the elections were not fully in agreement with international standards. The variables indicating the type of ethnic relations come from the Ethnic Power Relation (EPR) dataset created by Cederman, Min, and Wimmer (see <http://hdl.handle.net/1902.1/11796>). This dataset identifies and classifies relevant ethnic groups in 155 countries in the period between 1946 and 2005. A key idea of the EPR dataset is to define what constitutes a politically relevant ethnic group. According to the authors, an ethnic group is politically relevant if “at least one significant political actor claims to represent the interest in the national political arena, or if members of an ethnic category are systematically and intentionally discriminated against in the domain of politics” (p. 2, “EPR Coding Rules” Cederman, Min, and Wimmer 2009).

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POLITICAL MOBILIZATION AND INSTITUTIONS

Erica Chenoweth

Mobilization is a process by which passive individuals become active participants in social life by pursuing popular collective action toward a defined set of claims, interests, or goals (Tilly 1978).¹ Mobilization can take various forms ranging from protests to civil resistance to revolution to armed rebellion. It can also occur in various degrees of organization, from more spontaneous contentious episodes such as riots to direct actions coordinated by social movement organizations.

Scholars from various disciplines—primarily sociology and political science—have taken up mobilization, contentious politics, social movements, and civil resistance as primary objects of study. To many, mobilization is a process that people undertake when other alternatives (e.g. political parties, legislative representation, legal representation, and the like) are either blocked, compromised, usurped by other interests, or otherwise unavailable. The so-called Color Revolutions, in which mass protests erupted in response to alleged electoral fraud in semi-democracies, epitomize this take on mobilization (Tucker 2007; Bunce and Wolchik 2011; Beissinger 2007). The Arab Spring, in which mass mobilization took place in many countries with few formal, legal channels for competitive political expression, provides another example. And in Europe, transnational, countermajoritarian institutions such as the European Central Bank or the European Commission have been important in generating public discontent and mobilization. In the latter case, citizens were organizing against agendas that they perceived as not legitimated through democratic processes. Thus one conventional view is that political mobilization occurs outside of formal institutions, and that mobilization is essentially a substitute for malfunctioning institutions (e.g. Özler 2013; Goodwin 2001). As Clemens argues, “Groups marginalized by existing political institutions have an incentive to develop alternative models of organization” (1993: 755). As such, we would expect the highest levels of mobilization to occur in times and places where widespread grievances prevail and few formal institutions exist through which to channel these grievances.

Yet one of the most interesting insights produced over the past five decades of scholarship is that mobilization is often quite common in countries with many formal, legal, viable institutions. Advanced democracies such as the United States, the United Kingdom, France, and Germany experience exceedingly high levels of mobilization—often in quite radical and disruptive forms—despite the existence of multiple, overlapping, and highly functional political, social, and economic institutions. Why do so many people pursue extra-institutional action when they have many institutional alternatives by which to express their grievances?

This puzzle has motivated a wealth of scholarship focusing on more specific institutional arrangements in attempts to better understand why mobilization occurs in spite of institutions—as well as why different institutional arrangements might produce varying levels of mobilization (for a useful overview of competing approaches, see Amenta and Ramsey 2010). The primary theoretical argument comes from political opportunity structure (POS) approaches, which argue that mobilization occurs where it can, and that institutional contexts either constrain or create space for popular collective action. Most of this scholarship has been preoccupied with the question of how institutions affect mobilization, placing institutions as the independent variable. A smaller body of scholarship also examines how mobilization has affected formal institutions—particularly party politics, electoral practices, and legal frameworks. And numerous scholars have looked at both movements within institutions and institutions within movements as fruitful objects of inquiry.

In this chapter, I identify 14 consensus propositions regarding links between institutions and mobilization. I present these fourteen findings as testable hypotheses because while they are often taken for granted, further empirical testing is required to evaluate their robustness across a wider number of cases and historical periods. I then describe three major barriers to identifying more general causal patterns, including (1) a lack of conceptual precision in defining relevant “institutions” as objects of study; (2) a lack of global data on protest and mobilization that could yield generalizable empirical findings; and (3) a historical tendency to overgeneralize findings from a small number of Western cases. I conclude the chapter by suggesting a few substantive and methodological practices that may advance our collective understanding of relationships between these concepts.

The effects of political institutions on mobilization

The dominant institutional perspective on political mobilization is the POS approach. Broadly speaking the POS approach argues that both grievances and resources are inadequate to explain patterns of mobilization, since the ability to mobilize is often facilitated or constrained by the political and institutional environment in which the grievances occur. McAdam operationalizes political opportunity using four key indicators: the openness of the formal institutions, apparent divisions within the elite, elite alignments in government, and the state’s willingness and ability to use repression (1996, 1999; see also Jung 2010: 27–8). Proponents of political mediation theory suggest that political party systems can affect the types of challengers that emerge, while the specific structures and policies of those states both help and hinder the challengers (Amenta and Zylan 1991: 250; Amenta 2006). In general, the broadest claims about institutional POS relate to differences across regime types, although scholars have also examined institutional variation within democracies (and, increasingly, autocracies) to see whether and why patterns of mobilization shift.

Regime type: democracy vs. autocracy

The most basic studies emphasize somewhat static structural attributes—such as regime type—to explain collective protest behavior. For example, POS approaches would expect a higher level of actual mobilization in democratic countries than in authoritarian regimes, since democracies are more open and tend to permit greater freedom of expression, assembly, and petition than authoritarian ones (Eisinger 1973). Indeed, Corcoran *et al.* (2011) indicate that political regime type (i.e. democracy vs. autocracy) strongly influences would-be activists’ likelihood of mobilizing in protest activities. Using cross-national data from the World Values Survey, they find that people who express self-efficacy—or one’s perceived ability to influence one’s environment through personal action—tend to engage in collective action behaviors more often than people

who express fatalism—or a sense of personal helplessness with regard to one's surroundings. In general, activists in democracies are more likely to see collective action as efficacious, whereas activists in less democratic societies are more likely to experience a sense of fatalism and, therefore, lower motivation to mobilize to pursue their interests. However, they also find that even citizens that express high levels of efficacy in less democratic countries are not as likely to engage in collective action as citizens in democratic systems. This may be because the personal risks to collective action are higher in societies where political institutions are more exclusive and repressive against political dissenters.

One of the most influential studies of protest patterns in democracies is Sidney Tarrow's 1989 work identifying different phases of the Italian protest cycle between 1965 and 1975. Using protest data gleaned from Italian newspapers, Tarrow identifies a prototypical protest in democratic regimes, which features relatively large and widespread yet disorganized collective action, the development of a more organized movement centralized by movement entrepreneurs, and subsequent repressive episodes and/or concessions. The movement then splits or fractures, participation declines, and the movement becomes more decentralized. Perhaps at this stage, a second generation of more radical members develops, calling for more radical behaviors as the different segments of the movement compete for a smaller market share of potential supporters.

Tarrow's analysis thus yields a variety of observable implications about contentious politics within democratic regimes. Importantly, however, Tarrow's study only evaluates a single case—Italy—which was a democracy throughout the period under study. Cross-national research, such as a recent study by Jung (2010), compares protest cycles across Western European countries while evaluating the impact of political opportunity over the course of the protest cycle. She finds that whereas political opportunities such as favorable government partisanship (especially right-wing parties in power) and increasing electoral competition can increase the likelihood of new social movement protest cycles, such factors do not well explain the decline of mobilization. Instead, she argues that protest cycles tend to end because of (1) the institutionalization of the social movement, or over-organization; and (2) internal competition within social movement organizations, which lead some members to radicalize and use violence, thus accelerating the demobilization process.

These insights yield two general propositions:

- 1 Democracies experience more mobilization than nondemocracies.
- 2 The interests represented among mobilizers in democracies are more diverse than those among mobilizers in nondemocracies.

Dichotomous distinctions between democracy and nondemocracy cannot explain the timing of waves of contentious politics, nor can they explain variation in mobilization patterns across democratic states. They also cannot explain mobilization in nondemocracies or hybrid regimes—a category of regimes attracting increasing interest since the end of the Cold War (Levitsky and Way 2010; Robertson 2010; Bunce and Wolchik 2011). These remaining puzzles have led scholars to evaluate the effects of political party configurations, electoral rules, and executive institutions on mobilization opportunities.

Political party configurations

Some recent scholarship has focused on the nature of movement demands as well as the composition of the government—particularly the configurations of political parties in power when the mobilization occurs (Goldstone 2003).

Within democracies, the partisan orientation of the polity parties in power appears to be crucial. In their study of variation in the ratification of the Equal Rights Amendment, for example, Soule and Olzak (2004) find that states were more likely to ratify the amendment in response to mobilization when the states were highly electorally competitive, had historically adopted civil rights legislation in the past, and possessed Democratic Party allies in the state legislature. They also find that public opinion mattered—that ratification was more likely to occur where public opinion polls revealed higher tolerance for progressive social and economic roles for women. Essentially, mobilization for the Equal Rights Amendment was only effective in states that were already predisposed to this legislation—especially because of the representation of political parties that were ideologically aligned with the movement’s goals.

Patterns of movement-allied party mobilization may vary by the institutional structure of the system. In the United States, leftist groups may mobilize more when Democrats control the Congress or the presidency because of high degrees of party fragmentation in the legislature. In Western Europe, on the other hand, leftist parties are most active when right-wing governments are in power. Kriesi, *et al.* (1995) argue that new social movements (comprising anti-war, environmental, gender, and LGBT-based movements, for example) are expected to be the most active when leftist political parties are in opposition. Even when leftist parties are in power, however, scholars expect some low levels of movement activity because extra-institutional mobilization is a feature of leftist political ideology, not just political opportunity *per se*.

In Western Europe, right-wing challengers, on the other hand, tend to mobilize the most when leftist parties are in power. Giugni *et al.* (2005) examine extreme right-wing protest in several European countries and find

a clear negative correlation between electoral strength and the proportion of protest actions: the lower share of protests occurred in France, which is also the country with the strongest extreme-right party and the highest in Germany, which does not have a strong party.

(Giugni *et al.* 2005: 157)

Giugni, *et al.* emphasize the “discursive opportunities” that political marginalization provides to would-be mobilizers. Exclusion from the political process creates discursive opportunities for the movement to occupy a unique position in the polity. As such, lack of electoral representation through ideologically similar parties may produce higher levels of protest. This suggests that when right-wing parties are in power, extreme right-wing groups will not mobilize. When they do, however, they will do so using radical repertoires (Giugni *et al.* 2005; Koopmans 1993).

Taken together, these results suggest the somewhat counter-intuitive finding that leftist parties in power often face the most protest with the greatest variety of contentious challengers, whereas right-wing parties in power tend to face a high level of new social movement protest only (see also Jung 2010). This yields a third consensus pattern:

- 3 Leftist parties tend to face the highest level of mobilization, comprising both moderate leftist and extreme right challengers.

In authoritarian regimes, there is more controversy regarding the role of formal opposition parties. To some, the presence of formal opposition parties within autocratic legislatures may reduce mobilization, since most opposition activity is represented through such channels and such parties protect their positions within government by discouraging contentious collective action outside of approved channels (Frantz and Kendall-Taylor 2014). Others suggest that the presence

of formal opposition groups helps to carve out and defend space for contentious politics, while increasing the capacity of challengers to engage in high-risk collective action (Bunce and Wolchik 2011; Robertson 2010). Often such mobilization is met with counter-mobilization, thereby producing cycles of upheaval within such regimes—particularly during or after elections (Bunce and Wolchik 2011; Robertson 2010; Tucker 2007). These observations point to two contradictory, testable hypotheses:

- 4 Autocracies with formal opposition parties face lower levels of mobilization than autocracies without formal opposition parties.
- 5 Autocracies without formal opposition parties face lower levels of mobilization than autocracies with formal opposition parties.

Such effects may vary considerably based on whether the autocracy is organized as a monarchy, military regime, personalist regime, or single-party regime (Geddes 1999; Geddes *et al.* 2014)—an area ripe for future research.

Electoral rules, legislative institutions, and executive institutions

The most comprehensive analysis examining the effects of electoral rules, party systems, and executive institutions on civil protest is a recent comparative study by Özler (2013). The key finding is that presidential systems are more prone to mobilization than parliamentary systems, that majoritarian systems are more likely to produce protest than proportional representation systems, and that political party fragmentation is associated with greater protest.

Presidential systems tend to feature more fragmented legislatures with less party discipline. As presidents attempt to form patronage relationships with individual legislators to advance their goals, citizens may resort to protest to recapture the attention of legislators. Parliamentary systems feature higher degrees of party discipline due to coalitional dynamics and the direct accountability of the executive to his or her coalition government. Thus citizens need not resort to short-term signaling such as mass protest to enforce accountability Özler (2013).

Majoritarian systems feature the highest levels of party fragmentation due to the fact that they typically produce two “catch-all” parties that aggregate many disparate interests. Thus multiple social cleavages overlap within each party, suggesting that voters can rarely select a party that meets all of their political, economic, or social demands. They may therefore press for these interests using collective protest. In proportional representation systems, on the other hand, voters can select political parties that align more closely with their particular interests, enhancing the degree of personal representation the voter perceives and lowering incentives to engage in costly collective action. Özler finds support for these propositions using cross-sectional data on 90 countries between 1978 and 2002.

The general relationships found by Özler (2013) do not hold in all cases. For example, in their study on changes in patterns of contentious politics over time in Switzerland, Hutter and Giugni (2009) identify Switzerland as a “weak state” in the sense that it has a “federal structure, proportional representation, multiparty coalition government with fairly undisciplined parties, weak public administration, and the presence of direct-democratic instruments” (430). They then argue that weak states such as Switzerland tend to facilitate social movement mobilization but that those movements tend to adopt a moderate “action repertoire” since the structure encourages institutionalized protest strategies rather than confrontational or extra-institutional ones (*ibid.*).

Of course, Hutter and Giugni’s concept of “state weakness” is slightly different from others’ conceptions. The latter identify Swiss institutions as weak because largely legitimate state

institutions have a light footprint, whereas more typical conceptions of state weakness focus on state capacity to internalize citizen demands, remain intact while managing crises, and quell ongoing dissent through limited uses of repression (Beissinger 2013; Hendrix 2010; Skocpol 1979). For example, Goldstone's (1991) seminal study on the origins of revolutions suggests that the simultaneous occurrence of financial crises, elite fractionalization, and population growth leads to mass mobilization, as state institutions fail to cope with increasing citizen demands and elite fractionalization signals to citizens that mobilization will be efficacious.

Still other studies argue that the institutional context affects social movements differently depending on their level of access to various stages of the legislative process and the tendency of legislators to select movement issues as worthy of attention. For example, in their study of the woman suffrage movement, King *et al.* (2005) find that mobilization can affect the likelihood of an issue appearing on the legislative agenda, but that at "each successive stage of the legislative process" there are "increasingly stringent rules" that limit the impact of social movement activity (1211). As a consequence, "legislators responded to suffragists by bringing the issue of woman suffrage to the legislative forum, but once suffrage bills reached the voting stage, differences in social movement tactics and organization did not have as great an impact" (1211).

This finding is related to Wisler and Giugni's (1996) institutional selection theory, which argues that political institutions tend to favor certain types of movements and constrain or exclude others. Movements that are seen as "pro-institutional" rather than "counter-institutional" may be more likely to successfully frame their issues to fit within existing discourses, allowing legislators to incorporate movement demands into the institutional process (85).

Nonetheless, in general, we can observe several consensus patterns:

- 6 Presidential systems experience more protest than parliamentary systems.
- 7 Majoritarian systems experience more protest than proportional representation systems.
- 8 Democracies with high levels of political party fragmentation experience higher levels of protest than democracies with lower levels of political party fragmentation.
- 9 States with low capacity experience higher levels of protest than states with high capacity.

Institutions and repression

A vast literature exists that evaluates the impact of repression and concessions on social movement behavior (e.g. Khawaja 1993; Lichbach 1995; DeNardo 1985; Rasler 1996; Davenport 2007; Moore 2000; Alimi 2009; Francisco 2009, 2010, among others). Few of these works specifically evaluate the ways that political institutions might alter incentives to engage in repression vs. concessions. Conventional wisdom suggests that in most democracies, restrictions on the executive lead to less repression and fewer violations of rights. Yet Davenport (2007) argues that the so-called "law of coercive responsiveness"—or the tendency of states to respond to mobilization with coercion—applies equally to democracies and nondemocracies. In fact, Della Porta (2006) and Davenport (2007) have argued that democracies can be exceedingly repressive toward social movements. Other scholars, however, have found that at least some features of democracy—particularly the requirement that officials maintain office through popular elections—may constrain the severity of repression (Carey 2006; Conrad and Moore 2010). Hence the consensus is that democracies may involve lower risks for those mobilizing with regard to repression, but risks remain in democracies nevertheless.

Insights from this literature suggest that mobilization in democracies is more likely to experience some level of accommodation, at least partially.

- 10 Although some level of repression is assured regardless of regime type, protest in democracies is more likely to be accommodated than protest in nondemocracies.

In turn, partial concessions are likely to satisfy many citizens, reducing their willingness to engage in costly collective action and thereby reducing the size of the movement (see also Jung 2010).

The effects of mobilization on institutions

Although most social movement studies are primarily concerned with the origins of mobilization, some have also examined how contentious politics have transformed into or otherwise affected conventional forms of political action, such as party politics, lobbying, union organizing, and legal action (Schneiberg and Lounsbury 2007: 652; Clemens 1993). In fact, Schneiberg and Soule (2005) see institutions as no more than political bargains that result from contested political processes resulting from mobilization, disruption, concession, and co-optation.

Despite the fact that grassroots organizing often features prominently in democratic elections, surprisingly little scholarship has focused on the impact of mobilization on elections. Yet Schneiberg and Lounsbury (2007) argue that social movements can often evoke controversy and debate, resulting in new items being placed on the agenda and new frames through which activists can press for reforms (654). Social mobilization has certainly directly affected the extension of the franchise to women and minorities, for example, which has directly affected the process and outcomes of elections. Neither the 19th Amendment to the United States Constitution, which extended suffrage to women, nor the 1965 Voting Rights Act can be fully understood without considering the impacts of the suffragists and the civil rights movements, respectively.

McAdam and Tarrow (2010) introduce six potential ways that social movement activity can influence elections. They find evidence that at least three of these—particularly the innovation of new collective action techniques, proactive movement mobilization, and movement/party polarization—were at play in the 2008 election of Barack Obama. McAdam and Tarrow (2010) suggest that social movements often provide key innovations to political parties seeking to challenge status quo politics. Grassroots organizing and the use of social media to communicate with supporters for get-out-the-vote drives, for example, may be a key linkage between mobilization and party politics. And Tucker (2007) argues that mobilization often occurs in response to electoral fraud, which can serve as a focusing event that allows people to overcome free-rider problems typically associated with collective action (see also Bunce and Wolchik 2011).

With regard to legislative decision-making, McAdam and Su (2002) examine the effects of anti-war mobilization on American congressional action. Relying on data sources from American newspapers, they find that protests that featured violence or property damage by demonstrators tended to increase anti-war voting but delayed the pace of congressional action. Large demonstrations, on the other hand, had the exact inverse outcome: whereas they tended to speed up congressional action, they also tended to lower the likelihood of anti-war votes (696). This finding is consistent with Gamson's (1990) seminal study, which finds that movements that employed more "violent" or disruptive tactics were more likely to achieve political success than movements that employed more passive or institutional tactics.

In its most extreme form, mobilization can actually lead to the disintegration of state institutions. Chenoweth and Stephan (2011) find that campaigns of civil resistance against dictatorships, against foreign military occupations, or for territorial secession were more likely to succeed when they used disciplined nonviolent action, whereas using violence actually reduced movement participation and delayed progress. Most of Chenoweth and Stephan's campaigns occurred

in nondemocratic regimes and involved highly disruptive (albeit nonviolent) methods of noncooperation, such as strikes, boycotts, etc. Most prior scholarship on the effectiveness of mobilization focuses solely on American politics—especially the American Congress in McAdam and Su (2002)—and solely on reformist goals—such as anti-war, anti-nuclear, civil rights, or labor disputes. The distinction between protest and campaigns of civil resistance is nontrivial because symbolic demonstrations may have lesser political effects than highly disruptive resistance campaigns.

In general, although there is some disagreement regarding the effects that mobilization has on specific policy outcomes, several authors have found that the post-communist regimes with the most “rebellious civil societies” also emerged as the most successful and robust democratic systems (Ekiert and Kubik 1998), and Tarrow (1989) argues that even the most disruptive episodes of contention ultimately strengthened Italian democracy. Chenoweth and Stephan (2011) similarly find that countries in which nonviolent mass campaigns have toppled existing regimes are much more likely to emerge as democracies than countries in which violent insurgencies have taken place. Existing scholarship generally agrees that mobilization enhances citizen empowerment, clarifies issues of public concern and crystalizes consensus around them, broadens associational life (Putnam 1994), demonstrates to elites that civilian movements have the capacity to disrupt the existing order in a way that makes elites more responsive to their demands, and provides a direct mechanism through which civilians can threaten to “correct” formal politics through extra-institutional channels.

11 Nonviolent mobilization tends to enhance the liberalization of democratic institutions.

Movements within institutions, institutions within movements

One interesting line of inquiry concerns the direct interaction between institutions and social movements, either by focusing on how social movements become active within existing institutional structures or looking at how social movements organize their own internal structures.

Movements within institutions

Take the example of social movements that exist within organizations. Various scholarship has shown that social movement “infiltration” of status quo-based institutions can be critical to the ultimate success of the movements. Indeed, one of the key insights from campaigns of civil resistance is that the ability of activists to produce loyalty shifts among regime insiders is a major source of success for these movements (Sharp 2005). This is because outsiders often face severe legitimacy dilemmas that make existing institutions resistant to change. Only when people within those institutions begin to align with the movement and its demands might the institution begin to consider reforms (Sharp 2005; Chenoweth and Stephan 2011).

Interactions between the movement and institutions can be more subtle but equally as effective in bringing about social change. For example, LGBT movement activities in the workplace are thought to be critical to the ultimate successes of the LGBT movement. Activists used their status as insiders to evoke frames like civil rights, fairness and equality, and corporate social responsibility to discuss these issues with colleagues and supervisors. As Schneiberg and Lounsbury describe, they also

used casual mentions of partners’ gendered names when sharing experiences of mundane activities and enacted non-stereotypical behavior to challenge stigma. They also

employed narratives of discrimination or inequality to highlight hypocrisies, evoking understandings that everyday routines produce injustice, and activating listeners' identities as non-prejudiced persons

(Schneiberg and Lounsbury 2007: 656–657)

This process normalized the claims that the LGBT movement was making accelerating changes in public opinion on these claims.

- 12 Movements that provoke loyalty shifts within existing institutions are more likely to succeed than movements that remain institutional “outsiders.”

Institutions within movements

Considerable interest also exists concerning how the organizational structure of social movements affects their behavior. One of the most pressing questions concerns the effects of organizational structure on the movement's selection of nonviolent or violent methods of contestation. Research by Asal *et al.* (2012) finds that ethno-political organizations in the Middle East are more likely to splinter if they possess a factional or competing leadership structure and use violence as a tactic. Wendy Pearlman's (2011) comparative research on the Palestinian national movements, the Northern Irish nationalist movement, and the anti-apartheid movement in South Africa suggests that movements may be more enduring and disciplined when they enjoy organizational cohesion as opposed to fragmentation. Her research reveals that social movements that possess leadership, internal institutions through which norms and rules are diffused and enforced within the movement, and a collective identity and vision of the future are more likely to remain unified and nonviolent than movements that lack these attributes. Her qualitative research design allows us to better understand the direction of causality with regard to fragmentation and the use of violence. She finds that organizational cohesion preceded movement decisions to use nonviolent methods, whereas decisions to use violence followed periods of organizational fragmentation.

Additionally, a consensus has emerged that internal competition among social movement organizations may be heightened in democratic countries relative to authoritarian regimes (Chenoweth 2013; Jung 2010; Tarrow 1989). Social movements can compete in authoritarian contexts, but the pressures to engage in tactical innovation and radicalization are often even higher in democracies. In democracies, movements must compete directly with rival organizations, political parties, and interest groups while appealing to a dwindling base of potential recruits and continually justifying the movement's existence even while political parties press for reforms that satisfy movement moderates.

- 13 Fragmented social movements are associated with a higher propensity to use violence.
- 14 Social movements are often more fragmented in democratic countries than in authoritarian countries, particularly in later phases of the protest cycle.

Why don't we know more about institutions and mobilization?

So far, I have identified 14 claims regarding links between institutions and mobilization that many researchers now take for granted. However, few of these propositions have been tested empirically over a wide number of cases and historical periods, meaning that they are somewhat provisional and contradictory. Progress in identifying general causal relationships has been

stymied by several trends in the field, including: (1) a lack of conceptual agreement in defining relevant institutions and mobilization as objects of study; (2) a lack of global data on protest and mobilization that could yield generalizable empirical findings; and (3) a tendency to overgeneralize findings from a small number of Western cases. I also identify some promising trends that may help the field achieve progress.

Conceptual ambiguity

Conceptual ambiguity about what constitutes an institution and what constitutes mobilization means that empirical findings must be interpreted with a great deal of conceptual precision and care.

On the one hand, definitions of “institutions” are often exceedingly broad. Within social movement studies, institutional analyses include everything from formal political institutions to economic class to civil society to the “deep linkages [that] help shape interests in society and provide (or deny) resources to contending social actors” (Houtzager and Kurtz 2000: 394). In fact, many social movement organizations are themselves institutions in a loose sense. On the other hand, rigid definitions of institutions, which limit the definition to formal, named institutions, are similarly problematic. Relevant institutions are seldom rigid and monolithic entities. Instead, they are known to be fluid, non-monolithic, and seldom (if ever) fully self-contained (e.g. Schneiberg and Lounsbury 2007: 656–657; Sharp 2005; Koopmans 2005). Indeed, one of the major sources of success for nonviolent mass campaigns in the twentieth century was the ability of participants to leverage relationships they had with people within state security institutions. Scholarship that appreciates relational links between social movement organizations and institutions is promising in this regard. This is true even in regard to relationships across and between the movements themselves. Transnational waves of contention, such as the 1989 Eastern European revolutions, the Color Revolutions, and the Arab Spring indicate processes in which social mobilization is replicated or emulated across borders, sometimes in fairly portable or “modular” form (Beissinger 2007; Bunce and Wolchik 2011). Focusing on such relational aspects is admittedly difficult (but not impossible) because such relationships are often opaque until after major upheavals—especially in authoritarian contexts. More focus on the relational attributes of social movements and institutions may help scholars to better understand complex outcomes, such as unforeseen mass defections from state institutions, sudden reforms, institutional concessions, or new episodes of mass contention.

Similar conceptual issues arise with regard to mobilization. General historical patterns that emerge from observing mass nonviolent campaigns for independence, for example, are not necessarily applicable to protest cycles in democracies. Many scholars treat mobilization as simply protest, whereas there are many varieties of mobilization, including nonviolent vs. violent mobilization, organized vs. spontaneous mobilization, intra-institutional mobilization vs. extra-institutional action, and grassroots collective action vs. formal opposition mobilization. There are also varying degrees of mobilization—from single protest events to full-scale revolution—that make comparing results across studies quite difficult. One fruitful way to proceed might be to focus on the number of participants engaged in collective action rather than on event counts.

Social movement scholars have long called for more inter-disciplinary discussion, particularly between political scientists and sociologists (McAdam *et al.* 2001). I reiterate this call here, primarily as a way to agree upon consistent nomenclature that scholars can use to delimit the scope of research claims. One of the barriers to progress is the failure to analyze comparable units of analysis, conflating protest, protest cycles, social movements, and campaigns of civil disobedience. In the literature, these concepts seem virtually interchangeable, but each is conceptually different.

Lack of global data

A second frustration for scholars interested in this topic is the lack of reliable cross-national time series data that can be used to evaluate more general relationships. As a consequence of this, even quantitative analyses are often limited to cross-sectional data, which can identify associations between variables but cannot establish the directionality of causal relationships between them.

Fortunately, protest data do exist for some countries during some time periods, and new techniques for obtaining data have yielded several global data sets as well (e.g. Salehyan *et al.* 2012; Chenoweth and Lewis 2013; Asal *et al.* 2008). The existence of multiple protest data sets from different regions or cases may tempt analysts to construct more universal measures of mobilization. Scholars must use caution when using these data, however, for several reasons.

First, because researchers do not use identical sources or inclusion criteria when collecting these data, so the existing data are seldom comparable across cases or times. Banks *et al.*, for example, are the only global data on protests, strikes, and riots from 1955 to 2012, but this research team has relied on a small sample of newspapers to glean protest and strike data. Moreover, these data appear as annual event counts, with the unit of analysis being the country-year. Salehyan *et al.*, (2012) on the other hand, identify events data in Africa from 1990 to 2011, with the unit of analysis being the event itself. The Nonviolent and Violent Campaigns and Outcomes (NAVCO) data set's unit of analysis is the campaign-year and includes both mass nonviolent and violent campaigns from 1946 to 2006; however, the campaigns are limited to maximalist campaigns of regime change, anti-occupation, or secession with at least 1,000 observed participants (Chenoweth and Lewis 2013). And an organizational data set, Minorities at Risk Organizational Behavior (MAROB), uses the organization-year as the unit of analysis but is limited to ethno-political organizations in the Middle East from 1980 to 2004 (Asal *et al.* 2008). Newer global data sets, such as the Integrated Crises Early Warning System (ICEWS), use automated coding and therefore may contain a high degree of overreporting errors; such data sets may be better viewed as trends data sets rather than as events data sets (Ward *et al.* 2013).

Second, those using data on protest events should be aware that these events are often collected from newspapers, which feature several important reporting biases. First, there is a well-documented violence bias, meaning that events in which protestors use violence or create property damage are more likely to be reported than events that are purely nonviolent. Second, mainstream media possesses a protest bias, meaning that they report on large protests and demonstrations more than they report on strikes, forms of mass noncooperation, and withdrawal of support. This misrepresents contentious politics as protest alone, whereas social movements are often using hundreds of methods of civil resistance at once (Sharp 2005). Newspapers also tend to have national elite biases, meaning that reporters tend to interview government officials at the national level rather than ordinary civilians at local levels, meaning that narrative frames about contentious politics are often shaped by the state's view. Finally, newspapers often feature an urban bias in their reporting, meaning that rural activities often go unnoticed and unreported. To complicate matters even more, newspaper data across democratic and authoritarian regimes is often difficult to compare because of deliberate concealment of protest events in authoritarian regimes.

Focus on Western democracies, a small-n bias, and generalizability

A third limitation in the existing literature is the fact that most consensus findings about the relationship between institutions and mobilization emerge from small-n case studies, most of which involve comparative studies of Western democracies. Indeed, many of the most important insights from social movement studies are based on research on the United States, which is a

natural laboratory for social movement studies because of the diversity of institutional arrangements across its 50 states. Although invaluable for theory-building, extending theory and empirical implications from a limited set of (relatively similar) cases is highly problematic (George and Bennett 2005).

In recent years, the exclusive focus on Western democracies as objects of inquiry has begun to change. The apparent increase in contentious politics practices within autocracies has led to more research on protest politics in hybrid regimes (Robertson 2010; Osa and Corduneanu-Huci 2003), emerging democracies (Ekiert and Kubik 1998), authoritarian regimes (Rasler 1996; Schock 2005; Kurzman 1996, 2004; Chenoweth and Stephan 2011), and highly repressive systems (Martin 2007; Alimi 2009). However, problems remain with collecting data on and specifying both the independent variable (Gandhi 2008) and the dependent variable under authoritarian conditions.

Nonetheless, many of the testable propositions identified in the literature necessitate more in-depth looks into authoritarian contexts. Improved understandings of contentious politics in authoritarian settings will allow scholarship to move toward comparative institutional analysis and away from blunt and static distinctions between democracies and nondemocracies. Mobilization in authoritarian settings may also enhance understandings of the important distinction between movements and counter-movements. Indeed, in authoritarian regimes, the study of counter-mobilization is perhaps just as important as the study of mobilization, particularly as states create or encourage counter-movements to preserve their own popular legitimacy (Robertson 2010).

Conclusion

Mobilization—especially protest—has numerous direct causes. In fact, there is considerable heterogeneity in the type, scale, and scope of mobilization that occurs within different institutions. Do certain institutions cause more mobilization than others? Does mobilization affect some institutions more than others? The existing literature offers some provisional patterns that help us to understand why mobilization occurs despite the presence of institutional alternatives. It seems clear that while institutions do not predetermine whether mobilization will occur, the current literature suggests that institutions may shape the form, ideology, scale, or scope of mobilization. The most important remaining challenge for researchers is to better specify the conditions under which institutions matter, while acknowledging that they are often static or, at best, exceedingly slow-changing (Koopmans 2005). Although barriers to progress in the field have been considerable, improved understanding about the general influence that political institutions have on mobilization will take place as scholars refine and expand existing data to test these propositions on a wider array of cases and time periods.

Note

- 1 In this chapter, I refer to mobilization, collective action, protest cycles, and social movements in the ways that the various authors discussed refer to them. As a consequence, these terms appear somewhat interchangeable. However, as I mention in the conclusion, more conceptual precision regarding these terms in both theory and applied research would advance collective knowledge about each of these phenomena.

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26

POLITICAL VIOLENCE

An institutional approach

Laia Balcells

Are institutions relevant for understanding political violence? The relationship between violence and institutions is as old as Hobbes (Levi and Menaldo, [Chapter 2](#)). In fact, if we assume that *political violence* and *order* are two sides of the same coin, we can argue that the fundamental questions that have inspired political theorists for centuries are necessarily related to political violence.¹

Among the literature on conflict and political violence, which has developed significantly during the last three decades, there have been approaches emphasizing its political side (e.g. Clausewitz 1832/1976; Schmitt 1976; Tilly 1992; Della Porta 1995; McAdam *et al.* 2001) as well as rationalist approaches highlighting its economic determinants (e.g. Collier 2000a; Keen 1998; Esteban and Ray 1994). Institutions have nevertheless been the focus of attention of many scholars, independently of the approach and the discipline. Indeed, even the work of prominent economists such as Daron Acemoglu has given a fundamental role to institutions in explaining outcomes such as coups, revolutions, and mass protests (Acemoglu and Robinson 2006; Acemoglu and Robinson 2012).²

This chapter starts off with a brief overview of the existing literature on political violence by reflecting on how institutions can account for the different varieties of political violence that we observe in contemporary world. It then focuses more specifically on the role of institutions in accounting for a predominant form of political violence: *civil wars*. The chapter concludes by highlighting several avenues of future research on the relationship between institutions and violent conflict.

Political violence and institutions

“Political violence” is an ambiguous concept and its definition involves some controversy (Della Porta 1995: 2) for it implies defining not only what is “political,” but also what is “violence.” In general, we can understand political violence as “repertoires of collective action that involve great physical force and cause damage to an adversary in order to impose political aims” (Della Porta and Tarrow 1986: 614, cited in Della Porta 1995: 2). Political violence includes phenomena that range from mass protest, coups, and terrorism to riots, pogroms, ethnic cleansing, and genocide (Kalyvas 2013).

Why should institutions be taken into account in the study of political violence? Institutions are systems of rules, beliefs, norms, and organizations that together generate a regularity of (social) behavior (Greif 2006, cited in Levi and Menaldo, [Chapter 2](#)). Hence, we can expect institutions to have a critical role in explaining violence with political aims because institutions themselves are very often likely to be the target of political violence. In addition, institutions also shape the opportunities and capacities for using violence: if institutions are such that citizens under their rule are satisfied with them and everyone perceives that there is inclusiveness, violent conflict should not be taking place. At the other end of the spectrum, if institutions are too repressive (i.e. in autocracies), even if people are likely to be dissatisfied, there are no opportunities for the organization and perpetration of political violence and therefore violent conflict should not occur (Hegre *et al.* 2001; Fearon and Laitin 2003; Vreeland 2008).³ Furthermore, electoral *politics* and *violence* can be conceived as strategic *substitutes* (Dunning 2011), and, in fact, some argue that elections function peacefully only when the electoral chances of competing parties reflect their military force “in the shadow” (Przeworski *et al.* 2014).⁴ Last but not least, institutions should be relevant for understanding political violence because they can have a crucial role in solving the commitment problems that generate inefficient conflict (Fearon 1995) and/or that impede the resolution of conflict (Walter 2001; Werner 1998; Wittman 2009; Powell 2006; Fearon 2014). This chapter provides an overview of all these different ways in which institutions relate to conflict.

Institutions and varieties of political violence

Despite the significant development in the scholarly study of conflict in the last thirty years, we still lack a comprehensive theory of political violence. Theories explaining low intensity forms of political violence are not fully able to explain civil wars or more intense forms of political violence (Sambanis 2004) despite the fact that civil wars “grow out of lower-level state dissident interactions and the key to understand them lies in identifying the escalatory process that leads from one form of contention to another” (Davenport 2013: 3). Even though they tend to be studied separately, different forms of political violence have important connections: for example, *popular protests* very often precede *coups* (in fact, they are their most consistent predictor, according to Belkin and Schofer 2003), *coups* sometimes precede *civil wars* (Kalyvas 2006; Fearon 2004; Balcells and Kalyvas 2014),⁵ and *terrorism* is very often part of *civil war* fighting (Sambanis 2008). In addition, *terrorism* and *contentious politics* might work as complements or as substitutes (Sánchez-Cuenca and Aguilar 2009), and civil wars sometimes escalate as a response to *state repression* (Della Porta 2014; Sambanis and Zinn 2005).⁶ Also, *criminal violence* has several important connections with all forms of political violence, to the point that some authors have even considered insurgencies as “quasi-criminal activities” (Collier 2000b). Despite all these links, bringing different forms of political violence under the same explanatory umbrella is undoubtedly challenging and that is probably why it has not yet been done successfully.⁷

Institutions and civil wars

In this chapter, we will focus on one particular form of political violence: *civil wars*. Kalyvas (2006) defines civil wars as armed combat within the boundaries of a recognized sovereign unit between parties subject to a common authority.⁸ Although civil wars imply the perpetration of political violence by groups in conflict, they are more than the mere aggregation of violent events. Civil wars “must involve an extended contest of arms to win state power” (Payne 2011: 1). Civil wars are usually fought over political and institutional issues, for example: demand for

self-determination, regime or leadership change. Hence, because they are *objects of violent contention*, institutions can be expected to be crucial in accounting for the incidence of civil wars.

The existing civil war literature has unsurprisingly taken institutions very seriously. This has been the case in the study of both ethnic and non-ethnic civil wars. Ethnic conflict scholars (e.g. Horowitz 1985; Reynal-Querol 2002; Cedermann *et al.* 2011; Cederman *et al.* 2010; Wimmer 2013) emphasize the role of institutions in preventing this type of violence.⁹ In his seminal work, Horowitz (1985) explains that the distribution of economic and political power between ethnic groups is crucial to understand ethnic conflict. Reynal-Querol (2002, 2004) highlights the importance of a political system's degree of inclusiveness in explaining ethnic and religious violence, and she does it by connecting the ethnic conflict literature with that of consociationalism (Lijphart 2004). In her theoretical model, institutions allowing the participation of ethnic minorities are crucial in the prevention of ethnic conflict.¹⁰ More recently, Cederman *et al.* (2010, 2011) have analyzed what they call ethnic "horizontal equality" and its effect on conflict. They distinguish between political and economic horizontal inequality. *Political* horizontal equality refers to equal access of ethnic groups to politics that occur at the center of the state. "Civil wars and armed conflicts are most likely in ethnocracies that violate the principles of ethnic self-rule" (Wimmer 2013: 6).¹¹ From this perspective, institutions ought to be not only inclusive but also flexible, so that inclusiveness is guaranteed across time.¹² *Economic* horizontal inequality, which captures wealth differences between groups in a society,¹³ also has an impact on ethnic conflict, with more unequal societies more prone to ethnic civil conflict (Cedermann *et al.* 2011). In this regard, redistributive institutions have an important role in civil war prevention.¹⁴

Beyond the study of ethnic conflict, authors such as Hegre *et al.* (2001) and Fearon and Laitin (2003) have also found an impact of institutional factors on civil wars. Specifically, they study the role of regime type, finding that "semidemocracies" (or so-called "anocracies") are more likely to experience internal conflict as compared to either democracies or autocracies.¹⁵ The rationale they bring forward is that these regimes generate exclusion while not repressing enough to impede the organization of insurgent violence. As Vreeland summarizes it:

Under pure dictatorship, opportunities to organize are too limited and the probability of successful collective action low. Pure democracy presents the possibility for peaceful collective action. Semidemocracies or "anocracies" are caught in the middle: they allow dissidents to organize, but nonviolent collective action may be too restrictive to be effective. In this context, violence is more likely to be seen as the most effective recourse for dissidents, and the state reacts with violence accordingly. An inverted U-shaped relationship between level of democracy and the probability of political violence is predicted.

(Vreeland 2008: 401)

Some authors have challenged these results due to measurement issues in the independent variable "anocracy" (Gleditsch and Ward 1997; Vreeland 2008).¹⁶

From a slightly different angle, Acemoglu and Robinson (2012) also argue that civil conflict is highly determined by the nature of institutions in place. According to them, violence is more likely where there are "extractive" institutions because individuals will want to extract a share of the pie in a context in which the state is expropriating any product that is being generated. In their framework, political violence is a characteristic of societies with extractive institutions, and not of societies with "inclusive" institutions.¹⁷ Carles Boix (2002, 2008), for his part, also conceives political violence as an instrument to change redistribution patterns, given existing institutions.¹⁸

Note that, in these approaches, institutions are conceived both as the *object of violent contention* and as *shaping the opportunities and capacities for using violence*. These two dimensions of institutions are highly intermingled because if institutions are such that they allow for inclusiveness and peaceful collective action (and thus they limit the opportunities for using violence) they are also less likely to be the object of contention. With regards to *institutions as a substitute for violence* (or violence as a substitute of peaceful interaction), they have been explored mostly in theoretical approaches to conflict. If war is politics by other means (paraphrasing Clausewitz 1832/1976) it makes sense to think of violence and democratic competition as strategic substitutes (as in Przeworski *et al.* 2014), and to try to understand the conditions under which rational actors will engage in violence instead of bargaining (as in Fearon 1995, 2014).¹⁹ Factors such as relative military capabilities between contesting actors, as well as commitment and informational problems are deemed as crucial for explaining conflict and violence. Even though there is a vast formal literature on the rational explanations of war (see, for example, Skaperdas 1992; Leventoglu and Slantchev 2007; Slantchev and Tarar 2011; Leventoglu and Tarar 2008; Esteban and Ray 1994), empirical explorations of these issues are still scarce (see, for example, Reed 2003; Chacón *et al.* 2011; Esteban *et al.* 2012).

Within the study of civil wars, a fundamental and yet quite underdeveloped line of research is one that bridges institutions and *wartime dynamics*. Can institutions explain violence during conflict? According to Posen (1993), war implies the collapse of the political center and generates fear among ethnic groups, which must take actions to reduce threats to their security. In his approach, violence is due to the absence of institutions that allow for the solution of commitment problems between groups. Fearon and Laitin (1996) challenge Posen's basic assumption and explain that there are two different institutional mechanisms that emerge to deal with problems of opportunism, which, according to them, is what generates inter-ethnic violence: *spiral regime* and *in-group policing regime*. Thus, according to these authors, in the absence of formal institutions, ethnic violence should not be the norm.²⁰ Stathis Kalyvas (2006) develops a strategic explanation of violence that emphasizes the tactical and strategic incentives of armed groups and civilians. Institutions are an important element to explain irregular warfare because state-building processes are a crucial tool to win the hearts and minds of the population (that is especially the case for insurgents).²¹ Roger Petersen (2001) gives particular relevance to local institutions (i.e. the community) in explaining violent rebellion against military occupation.²² Balcells (2011, 2014) explains that local level institutions and political leadership have a crucial role in the perpetration of different forms of violence against civilians during civil war.

Authors such as Metelits (2010), Mampilly (2011), and Arjona (2010) have recently explored institutions of governance during armed conflict, namely how institutions are shaped by war. While Metelits and Mampilly explore variation across civil wars and armed organizations, Arjona focuses on variation within civil wars and organizations. This author argues that during irregular wars there are different types of institutions that originate from the interactions between armed groups and civilians. Armed groups have an interest in becoming *stationary bandits* (Olson 1993) instead of just extracting resources from the civilian populations they control militarily; yet, these populations do not always agree to being governed by insurgent groups. The interaction between insurgents and local populations generates different "contracts" or forms of governance. Arjona argues that these local level contracts are highly conditional on the characteristics of prewar local institutions, which determine the incentives and the ability of local populations to influence the actions of the rebel groups.²³ Jeremy Weinstein (2006) also emphasizes the role of institutions in explaining violence dynamics during civil war: his focus, however, is the armed organization as an institution. Weinstein distinguishes between

“justice-oriented” and “greed-oriented” armed groups, which attract different types of individuals. The former are organizations that emerge when resources are limited and are characterized by strong social networks permitting recruitment, as well as cohesion and discipline. They achieve restraint in violence. Greed-oriented groups emerge in resource rich contexts, and they attract individuals that are motivated by greed. The latter generates organizational environments that do not allow for restraint of violence.²⁴ These organizations are those perpetrating the worst human rights violations, including sexual violence (Wood 2006; Cohen 2013).

There is a vast literature on *institutions as a form to resolve violence*. This literature is composed of both formal and empirical approaches. Based on the idea that groups in conflict face commitment and informational asymmetry problems (Fearon 1995), authors such as Werner (1998), Walter (2001), Wittman (2009), or Cunningham (2011)—among others—have explored empirically the conditions that make negotiated agreements more likely. Some authors have explored mediation institutions that are more likely to be successful at promoting peace (e.g. Kydd 2003; Beardsley 2012). Others, such as Regan (1996), Doyle and Sambanis (2006), Fortna (2008), or Toft (2009) have analyzed peacekeeping operations and their impact on domestic institutions for war termination and war recurrence. Brancati and Snyder (2012) have studied the effect of early elections on civil war recurrence, and other authors have explored the impact of postwar electoral institutions on the duration of peace (Cammatt and Malesky 2012). The main take away from this literature is that both international and domestic institutions have an impact on the likelihood of war termination as well as on war recurrence and peace duration.²⁵ Nonetheless, finding the optimal peacekeeping institutions is not an easy task as there are a lot of context-dependent factors that affect their likelihood of success.

Next steps

When in the late sixties Samuel Huntington referred to the role of institutions in explaining “disorder” in society, he probably established the foundations of institutional explanations of political violence to come afterwards, which are more sophisticated (empirically and theoretically). Today, from the large literature on conflict, we know that institutions matter, and they do so in multifaceted ways. Institutions can have diverging effects on political violence depending on the form of violence considered. Thus, there are a myriad of ways in which the study of institutions can potentially be bridged to political violence, and scholars of violence should try to establish these links in further research. Below, I list a set of additional avenues of research on this area.

First, as scholars of contentious politics such as McAdam *et al.* (2001), Della Porta (1995), or Davenport (2013) argue, we need to better understand cycles of protest, repression, and “action-reaction” escalation processes leading to civil wars. Yet, we also need to better theorize the impact of institutions on the different phases of escalation. One particular institution might have a significant impact in the early stages of a rebellion, but have no impact at later stages. This could be better explored both formally and empirically.

Second, research on violence dynamics has mostly focused on *lethal political violence*, but there is room for improvement in the understanding of other types of violence. On the one hand, we need to understand better *non-lethal* forms of *political violence* such as displacement (see, for example, Steele 2010), sexual violence (see Wood 2006; Cohen 2013), torture (Rejali 2007), and forms of violence that have been called “extra-lethal” (Fujii 2013). These forms of violence often take place in conjunction with lethal violence, and they act as complements (Hoover-Green 2011), but they have their own dynamics and determinants. On the other hand, we need

to understand better the role of institutions in *criminal violence*, as well as on what some authors have called “social violence” (Fox and Hoelscher 2012). Criminal violence does not comply with the characteristics associated with political violence, as their actors do not have clear political aims and they do not attempt to rule or govern. Yet, cases such as the recent conflict in Mexico (the so-called “war on drugs”) and the politicized character of some criminal organizations make increasingly obvious that the distinction between political and criminal violence is blurry.²⁶ Scholars should take criminal violence seriously and systematically compare its patterns with other forms of political violence.

Third, we have a lot of knowledge about the effects of institutions on conflict, but we still do not know much about the impact of conflict on institutions. Tilly’s seminal work (1992) argues that state structures as we know them appeared in the Middle Ages as a by-product of the efforts to organize the means of war. More recently, excellent work has been done on war and institutional change (e.g. Scheve and Stasavage 2009; Karaman and Pamuk 2013), as well as on the effect of ethnic war on partition and the creation of new states (see, for example, Kaufmann 1996; Sambanis and Schulhofer-Wohl 2009; Chapman and Roeder 2007; Etzioni 1992; Fearon 2004; Downes 2001). Yet, more should be done along the lines of establishing theoretical connections between all forms of political violence (that is, not only war) and institutional development.²⁷

Fourth, the relationship between international institutions and domestic conflict dynamics should be further researched. An extensive literature in International Relations has contributed to the understanding of the role of international institutions on international conflict (Goldstein 2012). Indeed, from the perspective of liberal institutionalism “international institutions are important actors in international politics, powerful enough to help push states away from war and to promote peace” (Mearsheimer 2013: 571).²⁸ Yet, international institutions such as the UN are thought of bringing not only international, but also domestic peace (Mearsheimer 2013: 571). According to Goldstein (2012), for example, the end of the Cold War led to increased peace within countries because of the role of international institutions. The post-Cold War has observed more civil war terminations, which have benefited from third party interventions (Walter 2001; Doyle and Sambanis 2006; Fortna 2008). In addition, international peacekeeping institutions, despite important weaknesses (Autesserre 2009), have also been seen as having a positive impact on postwar stability and non-recurrence (Fortna 2008; Toft 2009), as well as on civilian protection (Hultman *et al.* 2013). More research could be undertaken in order to understand the impact of international institutions on all forms of violence processes, ranging from non-lethal violence to mass displacement and genocide.

A final avenue of research lies in incorporating time and time changes in institutional explanations to violence. Indeed, most institutional approaches are somehow path-dependent. As a consequence, they manage to explain cross-sectional variation but they are not as proficient at explaining over-time variation in violent outcomes. Even though institutions are hard to change, they do seem to transform and evolve over time. We need better theories to account for institutional change,²⁹ and about the impact of this change on political violence. Bisin and Verdier (2013) have recently modeled the joint dynamics of culture and institutions. Similar approaches, emphasizing the determinants of institutional change over time, as well as the dynamic impact of institutions on political violence would constitute significant contributions to a literature that so far has been better at explaining static outcomes than dynamic ones. The latter is particularly relevant in the study of conflict because, as explained, civil wars and other forms of political violence often have an impact on institutions. Only by taking into account the bidirectional relationship between institutions and violence can we understand patterns of conflict change and continuity across time and space.

Notes

- 1 Samuel Huntington (1968) made this palpable when he established a clear dichotomy between “ordered” and “disordered” societies: political communities are ordered (i.e. well-functioning) when there is a good fit between social forces and institutions; in contrast, when there is a misfit, there is social disorder and therefore violence.
- 2 Yet, it must also be said that institutional approaches are becoming somewhat rare. This is partly due to the fact that scholars tend to focus more on the effect of specific variables (for example: natural resources, wealth, inequality, polarization) while abandoning comprehensive macro-level explanations of conflict and violence.
- 3 Note, however, that a prevalent form of political violence in autocracies is *state terror*, which is unilateral on the side of the state.
- 4 According to Przeworski (1991) “democracy is an equilibrium” when there is parity between competitors. In other words, democracy will be stable when the two competing blocs have a high chance of winning the election.
- 5 Given that “the use or threat of force” is a fundamental component of their definition, *coups* should be considered a form of political violence. The likelihood of coups is explained by economic factors (Londregan and Poole 1990), but also by institutional factors, including the democratic/autocratic nature of institutions.
- 6 Also, a fundamental question in the study of contentious politics has been which institutions turn terrorism into legitimate dissent (Lichbach 2005: 163).
- 7 McAdam, Tarrow, and Tilly, for example, have aimed at providing general theories explaining “contentious politics,” which they believe should account for events ranging from the 1789 revolution in France to the nationalist mobilization that led to the breakup of the Soviet Union. They argue that “different forms of contention—social movements, revolutions, strike waves, nationalism, democratization, and more—result from similar mechanisms and processes” (McAdam *et al.* 2001: 4). However, their theory does a better job at describing mechanisms than at identifying causal mechanisms.
- 8 “Civil war, as a form of armed conflict within a single political unit rather than a foreign war between two different polities, is one of the oldest forms of strife” (Payne 2011: 1).
- 9 Ethnic violence is a particular form of political violence, which can take the form of civil war or not. Brubaker and Laitin (1998) define ethnic violence as “violence perpetrated across ethnic lines, in which at least one party is not a state (or a representative of a state), and in which the putative ethnic difference is coded—by perpetrators, targets, influential third parties, or analysts—as having been integral rather than incidental to the violence” (428). Despite ethnic violence being perceived to be the norm, Fearon and Laitin (1996) explain that peaceful and ethnic cooperation between ethnic groups is more common than large-scale violence.
- 10 This author tests her predictions with different sets of variables capturing the inclusiveness of the political system, which she conceives as “the ability of a system to avoid political exclusion” (Reynal-Querol 2004: 446). In her approach, democracies can have different levels of political inclusiveness depending on the electoral system (proportional vs. majoritarian), and on the number of checks and balances, among other factors.
- 11 Wimmer (2013) has a comprehensive approach and considers that ethnic conflicts can take place both at the center, between included groups (what he calls *infigting*), and at the periphery, between included and excluded groups. While the former conflicts aim at changing existing state institutions, the latter aim to create a new state and therefore a new set of institutions.
- 12 Indeed, constitutional rigidities might contribute to conflict. Lebanon is one example of a country where consociationalism became controversial when the demographic distribution of Muslims and Christians in the country shifted in a way that the political system was no longer guaranteeing inclusiveness.
- 13 *Horizontal* economic inequality is different from *vertical* economic inequality. While the former measures *between-group* inequality, the latter captures *within-group* (and between-individuals) inequality (Cedermann *et al.* 2011).
- 14 A caveat: several authors have recently highlighted the potential endogeneity problems in the relationship between ethnicity and conflict: ethnic categories can be created and/or reinforced during conflict and therefore can be endogenous to the conflict itself (Kalyvas 2009; Kalyvas and Kocher 2007; Balcells 2012). In addition, ethnic categories are not totally exogenous to start with (Lieberman and Singh 2012, Posner 2005). Hence, it is not totally obvious that we should be focusing on the total number of ethnic

- groups in a country (and their degree of polarization and fractionalization) in order to explain mobilization along ethnic lines and, ultimately, ethnic violence.
- 15 There has also been a large literature linking regime type to *terrorism*, the main finding being that partial democracies and fractionalized democracies are the kind of regimes most likely to experience terrorism (Chenoweth 2013: 359).
- 16 Gleditsch and Ward (1997) and Vreeland (2008) warn against using the Polity index to measure *anocracy*. This index includes measures of political violence, which implies that, by using it, we are ultimately using political violence to predict political violence. Vreeland introduces a revised index that does not include the components defined with respect to political violence and finds that it has no significant effect on civil war.
- 17 Acemoglu and Robinson (2006) use settler mortality at times of colonization as an instrumental variable for “type of institutions.” High settler mortality determined the non-adoption of “neo-Europe” settlements, and the establishment of “extractive” institutions (as opposed to “inclusive” institutions, which were established in areas with low settler mortality). Note that they assume that early institutions have either persisted or highly determined current institutions in former colonies.
- 18 Boix (2002, 2008) has provided a comprehensive model for guerrilla conflict and civil wars, which is based on the nature of economic endowments and their distribution within society. His model accounts mostly for class-based political violence.
- 19 Note that violent conflict and electoral politics can also be viewed as “strategic complements—so that politicians use violence in part to advance electoral aims” (Dunning 2011: 328).
- 20 In Fearon and Laitin’s model (1996) inter-group violence only occurs under the spiral regime, in which the members of group A indiscriminately punish all members of groups B for opportunistic behavior by an individual in the latter group. Violence does not occur under the in-group policing regime, in which members of group A identify opportunists in the group and punish them.
- 21 In fact, Kalyvas defines his theory as one “of the informal institutions of civil war” (Kalyvas 2009).
- 22 Inspired by Jack Knight’s (1992) definition of social institutions Petersen argues that “the community is a set of social, familial, economic, and political interactions, indeed, a history of socially shared interactions, which produces information and expectations about how sanctions and rewards will operate during the period of rebellion” (Petersen 2001:19).
- 23 Perhaps one of the most interesting implications of this type of institutional approach is that it allows for the establishment of a clear link between behavior at the micro-level (of civilians and armed organizations) and macro-level patterns. For example, at a cross-national level, Balcells and Kalyvas (2014) identify different types of wars associated to different patterns of institution building by insurgents, and they observe significant correlations with war length and war outcomes. Irregular wars, where insurgents are more likely to build high quality institutions such as those described by Arjona (2010), are significantly longer than wars in which rebels avoid engaging with the population and establishing durable forms of governance.
- 24 Staniland (2012) argues that the nature of social networks mediates the relationship that Weinstein finds between economic endowments and nature of armed organizations.
- 25 From the aforementioned literature we learn, for example, that elections should not be instituted in early phases of peace in order not to destabilize countries (Brancati and Snyder 2012), that proportional electoral institutions, which are more inclusive, are overall beneficial for peacekeeping (Cammatt and Malesky 2012), or that security sector reform is overall desirable in order to make postwar countries more legitimate and therefore more stable (Toft 2009). Also, international peacekeeping operations should build on local politics in order to be functional and not counterproductive (Autesserre 2009).
- 26 Interestingly, Fox and Hoelscher find that political-institutional factors play a significant role in explaining interpersonal violence. They find that it peaks in “hybrid” political orders: “weakly institutionalized democracies are uniquely prone to social violence” (Fox and Hoelscher 2012: 432).
- 27 On the institutional impact of violence, we can find some isolated contributions in the study of social movements and contentious politics (e.g. McAdam *et al.* 2001; Della Porta 1995; Beissinger 2002) or terrorism (e.g. Lichbach 2005), but there still is plenty of room for more comprehensive approaches to this relationship.
- 28 As Mearsheimer (2013) puts it, from a “realist” perspective, institutions cannot do much to change states’ behavior because they are diplomatic instruments that the great powers employ selfishly. Put otherwise, institutions are irrelevant in realist approaches to international relations and international war because institutions will be beneficial for the great powers and, when not, the great powers will just ignore them.
- 29 For example, how do we go from Acemoglu and Robinson’s (2006, 2012) “extractive” to “inclusive” institutions? Or how do we go from Huntington’s (1968) “bad” institutions to “good” ones?

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THE INSTITUTIONAL CONTEXT OF TRANSITIONAL JUSTICE

Monika Nalepa

Introduction

Transitional justice (TJ) has typically been characterized as the large body of literature that discusses ways in which new democracies and states recovering from domestic conflict deal with their authoritarian and violent legacies. Transitional justice mechanisms fall into one of the following categories: (1) trials of former perpetrators for human rights violations, (2) reparations to victims, (3) legislative acts condemning the former regime, and (4) truth revelation procedures (Elster 2004).

The first category includes trials and lifting statutes of limitation, enabling courts to prosecute crimes for which statutes of limitation have expired. Examples of trials of former perpetrators include the Nuremberg Trials and the Tokyo Trials following World War II. Compensation ranges from official apologies through monetary compensation to the restitution of rights to property that was confiscated by the ancien régime. As an example of an apology, consider the apology Willy Brandt issued to Jews for the Holocaust. An example of reparations to former victims includes the Argentinean law of 2004 compensating children born in captivity during the period of the military dictatorship between 1976 and 1983. The third category covers acts proclaiming the criminality of the ancien régime and legislation expropriating former authoritarian parties of illegitimately acquired assets. An example of the former is legislation passed in the early nineties in Czechoslovakia condemning the Communist Party as a criminal organization. And examples of the latter include legislation confiscating all assets of the Polish United Workers' Party, the communist authoritarian party in Poland. Truth revelation procedures comprise truth commissions and lustration. The most famous truth commission is, by far, the Truth and Reconciliation Commission of South Africa, created following the negotiated transition from the Apartheid regime. "Lustration" describes the broad set of personnel policies that disqualify or restrict access to public office of members or collaborators of the former authoritarian regime (Appel 2005).

Since lustration is the procedure that this chapter focuses on to make a general argument about transitional justice institutions, I illustrate it with a couple of more detailed examples. The first is de-ba'athification, a 2006 policy of purging the state administration of former members of Saddam Hussein's authoritarian party. The second example, from post-World War II France is the policy of "épuration,"—banning from office former Vichy collaborators. Both these policies were intended to free the new state of vestiges of the authoritarian regime and to bring

about reconciliation between its former supporters and resisters. Yet when de-ba'athification disbarred 185 former Ba'athists—mostly Sunnis—running for seats to the new legislature, it disenfranchised an important ethnic group for its alleged relationship to the Ba'athists and prolonged the civil unrest in the region. France's "épuration" was similarly exploited in the strife for power between the Communists and Gaullists (Kritz 1995).¹

Transitional justice scholars have discussed a variety of mechanisms that are adopted for dealing with victims of authoritarian abuse, as well as members and collaborators of authoritarian enforcement apparatuses, including perpetrators of political violence (Huntington 1991; Huyse 1995; Linz and Stepan 1996). This scholarship has also explored which mechanisms are most conducive to peace and/or reconciliation (Laplante and Theidon 2007; Leebaw 2008; Lundy and McGovern 2008; Aolain and Campbell 2005; Rolston 2006), which are most agreeable with principles of rule of law (Allen 2001; Cassel 1998; Kaminski and Nalepa 2006; Krygier and Czarnota 2006; McEvoy 2007; Posner and Vermeule 2004; Sikkink and Walling 2007) and/or long term democratic stability (David 2003; Gibson 2007; King 2000; Sa'Adah 2006).

Most of the literature on transitional justice falls into one of the following two categories: it either addresses the normative question of how transitional justice mechanisms ought to be designed or explains how those that were adopted came about. Transitional justice research in the spirit of institutionalism is rare and critics have pointed out that this framework is inadequate for the study of transitional justice, because its adoption is more often the product of raw emotions (as diverse as revenge to forgiveness) rather than rational calculations (Elster 2004, Petersen 2002).

Yet contrary to these criticisms a rational choice perspective "does not require us to assume that institutions are chosen rationally." Instead, according to Avner Greif and Christopher Kingston, it "enables us to generate a theory with empirical refutable predictions about the institutions that prevail in a given situation." Contemporary rational choice scholars aspire to answer two kinds of questions: First, how are institutions selected (what Greif and Kingston refer to as the "institutions-as-rules" approach); and second, how institutions motivate people to behave in ways institutional designers would like them to (the "institutions-as-equilibria approach") (Greif and Kingston 2011, 13).² Hence, the "institutions-as-rules" approach focuses on explanations for who, when, and why adopted the institution in question is adopted, whereas "institutions-as-equilibria" focuses on the normative question of how institutions ought to be designed so that people are motivated to comply.

The absence of transitional justice research in the spirit of institutionalism is unfortunate, because institutionalist approaches to transitional justice could enhance this scholarship in at least three ways. First, explaining *who*, *when*, and *why* implements transitional justice institutions, irrespective of their desirability, would help us understand when transitional justice institutions are strategically viable. Second, examining the structure of incentives behind transitional justice mechanisms would allow designers to start verifying if normatively desirable mechanisms are feasible before they are put in place. Finally, combining the two approaches could indicate when transitional justice mechanisms that are likely to produce the normatively desirable outcomes are strategically attractive options for the actors who are empowered to introduce them. This third contribution is also consistent with Greif and Kingston's observation: "Despite their difference, the institutions-as-rules and institutions-as-equilibria approaches have much in common and are best viewed as complements rather than substitutes" (Greif and Kingston 2011: 15).

In short, TJ institutions are usually adopted in new democratic contexts after intensive bargaining between the executive and legislative branches of government and after they have been upheld by courts. Scholars focus on institutions-as-rules when they try to understand the adoption of transitional justice institutions, because institutions of separation of powers, rules organizing the work of legislatures, and executive-legislative relations and others are responsible for

conditioning the behavior of actors as they adopt a new institution. But when we are also interested in the extent that TJ institutions provide actors with incentives to comply with their intended normative goals (whether these be peace, reconciliation, rule of law, or democratic stability), this is also an account about institutions-as-equilibria. Scholars may combine the institutions-as-rules with the institutions-as-equilibria approach when they are explaining the durability of transitional justice institutions—the fact, that once adopted, actors do not have an incentive to change TJ institutions unless other institutional rules (or exogenous factors) change.

The remainder of this paper is organized as follows. In the next section, I explain how TJ institutions can be studied as rules of the game, focusing on when incentives push political actors to adopt them. In particular, I discuss how transitional justice institutions can be used strategically and/or preemptively and develop six hypotheses based on this theory. In the third section, I use an example from Poland to illustrate the transitional justice institutions-as-rules approach. In the “Transitional justice institutions as game equilibria” section, I explain how one can design, somewhat more lenient, institutions that will incentivize their targets to reveal information (incentive-compatible transitional justice) about the nature of past human rights violations that are no longer documented. I continue with the Polish example by showing how TJ institutions can motivate people to comply with normatively desirable institutions. The “Conclusion” summarizes how combining the two institutional approaches can be a fruitful exercise and concludes.

Transitional justice institutions-as-rules of the game

Lustration can affect public employees in any capacity, but as the Iraqi and French examples from the introduction illustrate, it has a potential for profoundly affecting electoral outcomes. Thus, electoral contenders can strategically exploit it.

Further investigations reveal that transitional justice continues to be used strategically in new democracies, even those that have successfully consolidated. In Romania just a few months prior to national elections, the Council for the Study of Securitate (CNSAS) started revealing names of politicians it suspected of collaboration with Romania’s communist secret police (Securitate). CNSAS’s work was curbed by a constitutional court decision. Within days of the decision, the government hastily passed a resolution empowering the CNSAS to continue working with the files even though it could no longer pass judgments on politicians in office (Bucharest News Agency 2008). CNSAS’s findings leaked to the media and destroyed the reputation of many politicians and candidates running for office. Similar anecdotal evidence can be cited from most post-communist countries (Nalepa 2010).³

These examples show that lustration can affect electoral outcomes by explicitly banning candidates from running for office or tarnishing their reputation to the point that they cannot effectively compete in elections. Thus, strategic politicians will exploit democratic institutions to their advantage and pursue the type of lustration that best serves their interests. If one believes that politicians act strategically to win and maintain office, one has to take into account the potential effects of lustration policies.

At the level of parties, politicians’ preferences regarding lustration are shaped by their beliefs about the extent to which their parties contain members and collaborators of the ancien régime. This is what I refer to as parties’ beliefs about their infiltration. Parties with many collaborators are hurt by lustration; parties with few collaborators or none stand to benefit from it. While some forms of collaboration are clandestine—such as being a secret informer of the authoritarian police—others—such as being a member of the authoritarian party—are open. Thus, in the case of some transitional justice procedures—for instance, decommunization—parties know how

lustration will affect them, but in the case of others, such as screening procedures revealing the identity of secret informers, political actors are initially uncertain as to how they will be affected by them. Because of such uncertainty, lustration laws are more likely to be adopted when parties that are not infiltrated by members and collaborators of the *ancien régime* (and know this to be the case) hold a majority in the legislature. In the case of post-communist Europe, where collaboration with the *ancien régime* tended to be clandestine, this means that contrary to both conventional wisdom and theories expressed in the transitional justice literature (Elster 1998; Halmai and Scheppele 1997; Holmes 1994; Huntington 1991; Huyse 1995), lustration laws are more likely to be adopted not in the immediate aftermath of the transition to democracy, but later—once successor-communist parties with high membership of open collaborators (former members of the authoritarian Communist Party and opposition parties infiltrated with clandestine collaborators)—have been replaced by new political parties that are less likely to harbor former collaborators in their ranks (Nalepa 2010). This consideration leads to the broadest strategic transitional justice hypothesis of this theory:

- 1 Lustration is likely to be adopted when either new political parties or parties who are not infiltrated with former collaborators win or are about to win significant representation in legislatures.

I define new parties as those that either have members who are too young to have been collaborators in the past or parties that have been recently formed by leaders after they have screened their membership for likely collaborators. Hypothesis 1 stipulates that parties who know they are not infiltrated with collaborators of the *ancien régime* have established a presence. In the case of new parties or lustration that screens out open collaborators (such as de-ba'athification, decommunization, or denazification) this is plausible. However, in the case of lustration that screens for secret informers, parties may be uncertain as to how lustration will affect them relative to parties they are competing against in elections. Thus, lustration that aims at uncovering clandestine collaborators may be delayed even if parties that are not infiltrated dominate the legislature. These parties will first have to learn that they are less infiltrated than their electoral competition before they engage in lustration. Assuming that this learning process is time-consuming, we have:

- 2 All else equal, lustration that aims at revealing secret informers of the political police occurs later in the transition than lustration vetting public offices of open collaborators of the *ancien régime*.

Hypotheses 1 and 2 specify when lustration laws will be adopted, but neither says anything about *who* will be proposing lustration laws and *what* kind of laws will be adopted. Studying politicians' strategic use of lustration (Nalepa 2008; 2010; 2012), I found that the presence of parties with fewer collaborators than their political competition increases the probability of adopting lustration in the close proximity of elections. However, parties that expect lustration to contribute directly to their electoral success are not always the ones that actually propose to adopt lustration. Agenda setting (i.e., closed as opposed to open rules of parliamentary decision-making; see Romer and Rosenthal 1979, 1978) in combination with electoral turnover can modify parties' incentives. Specifically, closed rules provide agenda setters that are more infiltrated than their electoral competition with incentives to preemptively adopt mild forms of lustration if they anticipate losing power to less infiltrated parties.

The implementation of lustration, as that of any transitional justice procedure adopted in the aftermath of a transition to democracy, is subject to the constraints of democratic processes.

Ironically, it is precisely the democratic features of post-authoritarian regimes that allow transitional justice institutions to be used strategically. The uncertainty of remaining in office associated with elections provides incumbents with incentives to tailor lustration laws to their advantage. But lustration, as any other bill, must be proposed for consideration in the legislature by the relevant agenda setter—a committee chairman, cabinet minister, or other member of the executive. Next, it has to be passed in a series of votes, often accompanied by intensive bargaining. Once a bill has made it out of the legislature, it is still subject to presidential vetoes and to judicial review by constitutional courts. Transitional justice mechanisms are, in particular, frequently subjected to constitutional review because of their potential for violating the principle of non-retroactivity (no punishment without crime). Typically, it is easy to assemble a coalition of political actors entitled to send the bill for constitutional review questioning its compatibility with this principle.

Given these institutional hurdles of the implementation process, a theory explaining lustration must take into account all stages of the legislative process: the proposal stage, the bargaining in the legislature, the bill's passage, the potential for an executive veto (as well as the legislature's capacity for override), and the motions of constitutional courts.

To develop these more specific hypotheses, I use the literature on legislative organization (Baron and Ferejohn 1989; Baron 2000; Gilligan and Krehbiel 1987; Weingast 1989) to distinguish between two legislative paths for adopting bills: closed and open rules of parliamentary procedure. Under closed rules, the proposer is different than the median in the legislature. The proposal, which is drafted in the appropriate committee or ministerial department, once on the floor of the legislature cannot be modified. The median has to "take it or leave it." In contrast, under open rule procedures, whatever the agenda setter proposes, can be amended upon reaching the floor of the legislature. As a result, the bill favored by the median voter in parliament is ultimately the proposal that gets passed. The role of the agenda setter is insignificant.

Suppose that rules of parliamentary decision-making are closed and an infiltrated party has proposal power but anticipates losing elections to a non-infiltrated party. Suppose furthermore, that the median in the legislature is located between the infiltrated and non-infiltrated parties. If the infiltrated party does not adopt any bill, then, following the turnover in proposal power (resulting from elections), it will suffer from a very stringent bill, as the median of the legislature may be ready to accept any transitional justice bill rather than put up with no lustration at all. The infiltrated party can prevent this punishment by implementing a mild bill, at the ideal point of the legislative median. If this bill is sufficient to appease the parliamentary median, it will prevail when the infiltrated party loses agenda setting power. [Figure 27.1](#) illustrates the game that underscores this logic.

This reasoning, assumes that both the infiltrated and non-infiltrated parties know the exact location of the median. Suppose however, as is typical in democratic electoral settings, there is uncertainty about the exact location of the median. This uncertainty complicates the infiltrated party's strategy of preemption. In previous work (Nalepa 2010), I formalized this argument into a game with incomplete information. A sketch of the changes made to the model and the main intuition behind the solution to it are described as follows.

Let us consider the more interesting case of closed rules of parliamentary decision-making. The players are: the successor-communists (infiltrated party), the anti-communists (non-infiltrated party)⁴ and two possible post-electoral medians: a left-of-center median and a right-of-center median. In terms of the figure below, both medians have ideal points between the infiltrated party and the non-infiltrated party, but the left-of-center median is located closer to the infiltrated party, while the right-of-center median is located closer to the non-infiltrated party. As before, the infiltrated party knows that after the elections, the non-infiltrated party will

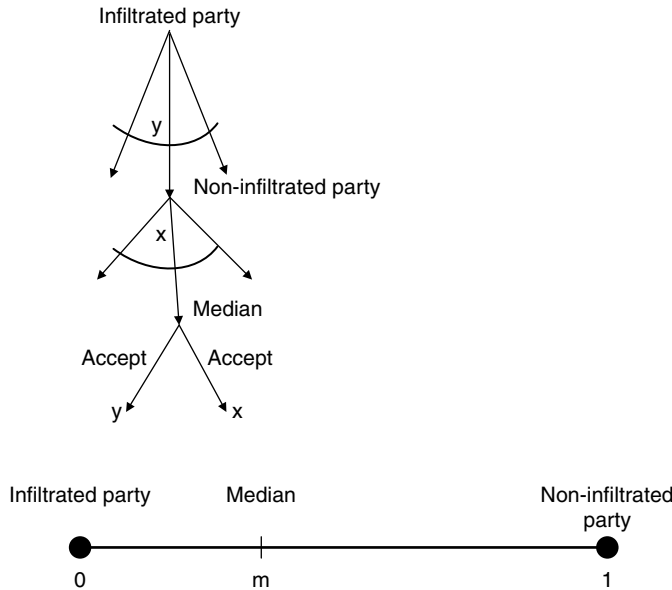


Figure 27.1 Strategic transitional justice under closed rules with certainty: the TJ space and game

have proposal power. They are uncertain however, whether the proposal must gain acceptance of the left-of-center or right-of-center median. The [first part](#) of the game takes place before the elections. The infiltrated party must adopt a bill (let's call it y) from the transitional justice space. The next stage is the elections. The uncertainty of who will be median is represented, as is typical in game theory, by a “move of Nature,” which determines whether the left-of-center or right-of-center party will be median. In the third stage, the new proposer (the right-of-center party) proposes a new bill (let's call it x) from the transitional justice space. In the fourth and final stage of the game, the median chosen in stage two chooses between x and y . The equilibrium outcome is very intuitive:⁵ if the infiltrated party believes the median is more likely to be left-of-center than right-of-center, it will adopt a lustration bill at the ideal point of that party. If its beliefs are to the contrary, it will adopt a lustration bill at the ideal point of the left-of-center party. In equilibrium, the original proposal prevails, that is, the non-infiltrated party cannot propose a competing bill that the median would prefer to the original proposal made by the infiltrated party. The intriguing empirical implication, expressed in hypothesis 3 below, is that sometimes it is rational for the infiltrated parties to propose and implement lustration:

- 3 Lustration bills are also likely to be proposed in legislatures with closed rules of procedure by agenda setters that are infiltrated with former collaborators, but who expect to lose proposal power to the parties that are not infiltrated with former collaborators; the lustration bills that are proposed under these circumstances are milder than the ideal points of the non-infiltrated parties.

In other words, if agenda setting power is about to be turned over to a political party with preferences on the opposite side of the legislative median as a result of the elections, the pre-electoral agenda setter is better off adopting a milder lustration bill (at the ideal point of the legislative median) than waiting for the extreme form of legislation to be adopted by the post-electoral agenda setter.

Note that if the roles of infiltrated and non-infiltrated parties are reversed, the symmetric result does not hold (i.e., parties that are not infiltrated with former collaborators, who expect to lose upcoming elections to infiltrated parties will not propose lustration that is to the left of their ideal point). The reason is that the more stringent lustration laws will lessen the electoral success of the infiltrated party by eliminating more of their candidates from the electoral competition. Hence we have the following hypothesis:

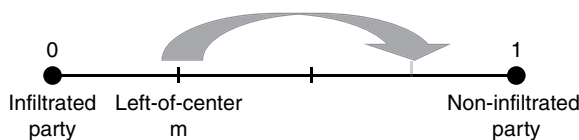
- 4 In legislatures dominated by parties that are not infiltrated with former collaborators stringent lustration laws will be proposed irrespective of the openness of rules of procedure and regardless of expectations about turnovers in power. However, in legislatures with open rules of procedure, the bills will be milder than in legislatures with closed rules of procedure.

The difference between the outcomes characterized in hypothesis 4 can be explained as follows. Open rules of procedure imply that the median's preferred policy prevails, thus the non-infiltrated parties cannot get the median to accept their preferred stringent bill in a "take-it-or-leave-it" fashion. However, under closed rules of procedure, the non-infiltrated party can force the median to make a choice between the no-lustration status quo and a stringent bill that is equidistant from the median with the status quo, but on the other side of the median's ideal point.

Note that hypotheses 1 through 4 specify predictions about when and what type of lustration is initiated for a no-lustration status quo. The final two hypotheses explore changes to lustration laws. Democratic elections entail uncertainty. Expectations of rational agenda setters may be intercepted by exogenous shocks, such as the flood in Poland in 1997 or the corruption scandal in Hungary in 1998 (Nalepa 2010). Rational expectations may lead to ex-post facto mistakes stemming from overly pessimistic or overly optimistic predictions. Consider first the pessimistic scenario: Suppose that the infiltrated party overestimates the median and adopts lustration at the right-of-center median's ideal point. Meantime, the median position goes to the right-of-center party. Since the infiltrated party implemented more stringent lustration than was necessary to appease the actual median, no amendments will take place. The non-infiltrated party will keep the gates on lustration closed, as nothing that it prefers to the original bill would be simultaneously acceptable to the median:

- 5 In legislatures with closed rules of procedure, where infiltrated parties adopted lustration prior to the elections, but the turnover in power was smaller than expected, the lustration law will not be amended.

Conversely, suppose that the infiltrated party underestimates the median and adopts lustration at the left-of-center party's ideal point, while the median position goes to the right-of-center party. The non-infiltrated party can now exploit the distance between the post-electoral lustration status quo and ideal point of the actual (right-of-center) median by proposing stringent lustration that is, approximately, as far from that median's ideal point as the left-of-center ideal point, but to the right of that median, as illustrated in [Figure 27.2](#):



[Figure 27.2](#) Equilibrium outcomes in the strategic transitional justice game

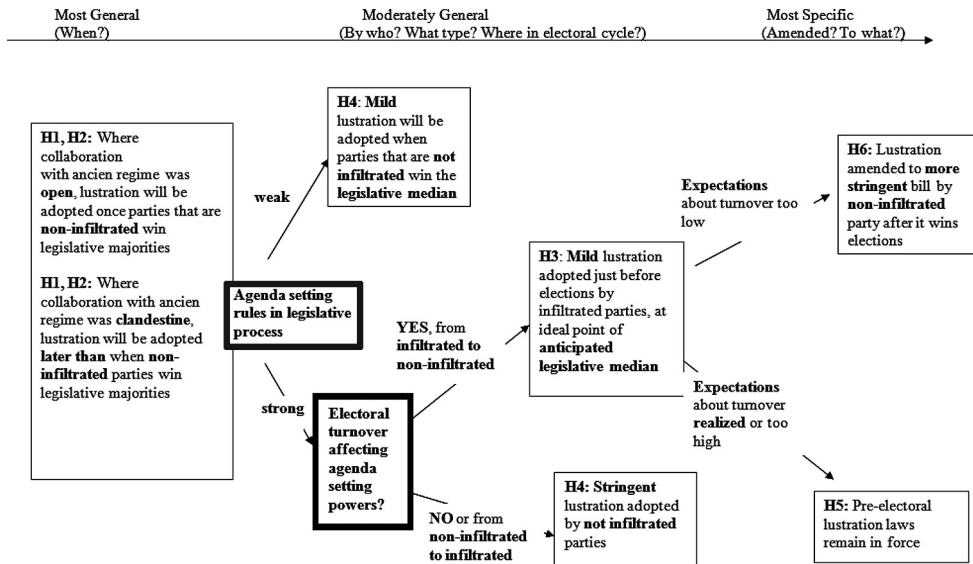


Figure 27.3 Hypotheses 1–6 from most general to most specific

Note: theoretical parameters requiring operationalization have been highlighted.

This strategic exploitation of the infiltrated parties' mistake by the party that is not infiltrated leads to the final hypothesis:

- 6 In legislatures with closed rules of procedure, where infiltrated parties adopted lustration prior to the elections, but the turnover in power was greater than expected, the lustration law will be amended.⁶

Figure 27.3 offers a graphic representation of all hypotheses and their relationship to one another.

Poland as a case study

In the previous section, the theory leading to hypothesis 3, stipulated that conditions of turnover in power may incentivize opponents of transitional justice to change the no-lustration status quo to a mild form of lustration in anticipation of losing power to elites preferring harsh lustration. This is exactly the background leading up to the passage of the Polish lustration law. By 1997, Poland, for reasons I explain elsewhere still had no lustration law in force (Nalepa 2010). The party holding a majority in the parliament at the time was the successor-communist Democratic Left Alliance (SLD), which was also doing very well at controlling the legislative agenda (Carroll and Nalepa forthcoming). Although formally, the rules of procedure were not amended to closed rule until late 1997, SLDs cohesive majority allowed it to act as a legislative cartel (Cox and McCubbins 2005). However, the next elections were scheduled for the fall of 1997 and the anti-communist right had for the first time succeeded at creating an electoral coalition (the Solidarity Election Action, AWS), with which it was expected to win in the upcoming elections, but not so overwhelmingly as to also capture the median. The median party in the legislature was expected to be either the slightly left-of-center Polish People's Party (PSL) or the slightly right-of-center Freedom Union (UW). In light of these circumstances, hypothesis 3 predicts that the

SLD, as the openly infiltrated (with communists) party would pass a mild lustration law in anticipation of power turning over to the non-infiltrated party (the AWS). This is indeed what happened in April of 1997. Supported with the votes of SLD and parties on the left, a mild and incentive-compatible form of lustration was passed in the Polish Sejm. For reasons outlined in hypothesis 6, the law was also amended, when instead of PSL, as the SLD had anticipated, the slightly more anti-communist UW became median.

Moreover, the Polish example suggests that conditions of turnover in power may incentivize transitional justice opponents to implement lenient (but possibly incentive-compatible) forms of these mechanisms in anticipation of losing power to elites preferring stringent forms of transitional justice. Somewhat surprisingly, we find that conditions characterizing transitional democracies, such as frequent turnovers in power, can actually facilitate the adoption of incentive-compatible (and normatively desirable) forms of transitional justice.

In the following section, I explain that the incentive-compatible mechanism of the Polish lustration law comes at a cost: the sanction for lying in an affidavit is a ban from office, but in equilibrium, the collaborator faces a milder sanction of only having his act of collaboration publicized.

Transitional justice institutions as game equilibria

After developing a theory in the spirit of institutions-as-rules of the game (Greif and Kingston 2011) and explaining who implements transitional justice, under what circumstances and of what kind, I devote this section to a discussion of transitional justice institutions-as-equilibria, with a particular focus on how to design incentive-compatible lustration mechanisms. The importance of aligning incentives in lustration mechanisms with the normative goals of transitional justice is particularly stark in light of three facts.

First, post-transitional states—which would have to serve as enforcers of rules—are notoriously weak and deficient of resources to ensure rule enforcement. Working for the police forces in former authoritarian states is neither a lucrative nor reputable job. Those with adequate training can obtain much more attractive positions working for private security agencies. Hence new democracies' enforcement apparatuses are notoriously weak. At the same time, organizing secret police archives so that they can serve the normative goals of lustration—that is removing from public office former spies and agents of the authoritarian regime—is an expensive process. Even if files had not been destroyed over the course of the transition, the authoritarian secret police would use and archive its evidence in a dramatically different way than is optimal from the point of view of a lustration process. For instance, Germany is one of the Central European countries that has been applauded for its implementation of lustration. But the BStU, commonly called the Gauck agency (after its first Federal Commissioner Joachim Gauck), spent a fortune on data recovery after in 1995 it began reassembling the shredded documents destroyed by the outgoing Stasi officers. By now archivists commissioned to the projects have reassembled 400 bags of shredded documents. Recently, the BStU has been developing a system for computer-assisted data recovery in order to convert the 15,000 bags of shredded documents into what is estimated to be 33 million pages.

Kilometers of files have been destroyed and this was even more true for regimes whose transition to democracy involved long and protracted negotiations, because those in their possession had the time, opportunity, and incentives to destroy as much as they could. This is the case because those in possession of compromising evidence that could be used in lustration proceedings are most likely the same people who helped collect it—they are the spies and officers of the secret police themselves who would also be implicated by a stringent lustration process. But even if the

lustration process were mild, why would the owners of materials compromising potential politicians want to just give up their evidence? Secret police files allegedly demonstrating that candidates for public office collaborated with the secret police can and have been used in new democracies to corrupt politicians with such “skeletons in their closet” (Stan 2008).

These facts have important consequences for lustration mechanisms that are expected to perform after being successfully implemented. A reliable mechanism must rely on minimal enforcement and in its design must correct for the scores of documents that were destroyed over the course of the transition.

The Polish lustration law of 2007, outlined next, satisfies these constraints by providing collaborators of the communist authoritarian regime with incentives to reveal information about their past, if they want to run for office.

The range of targets encompasses senior governmental officials chosen through national and local elections, members of the core executive, officials of the central judicial system, senior officials in the public healthcare and social welfare systems, editors-in-chief of the public media, administrators and faculty of public and private universities, high school headmasters, top personnel of the national postal service, and CEOs of companies where the state is the main shareholder. Targeted activities include officers and informers of the secret police, police units that were subject to the command of the secret police (such as riot police), faculty and administrators of police academies, members of the border guard patrol, officers working for military intelligence and counter intelligence, and employees of the communist Agency for Religious Beliefs and the Censorship Office (*Dziennik Ustaw* 2007). The sanction in the Polish lustration law is a combination of (i) a ban from holding office and/or (ii) exposing the truth about the past. All targeted persons must submit affidavits in which they have to attest to whether they engaged in the targeted activity. If the targeted person confirms collaboration, he is not banned from office, but the declaration is immediately published on the internet. As a result, voters or the appropriate nominating agency can withdraw support for the candidate if either believes the target’s past disqualifies him from the office in question. If a target’s affidavit denies collaboration, it is referred to a state prosecutor from the agency responsible for maintaining the archives of the former secret police in Poland. The prosecutor assesses the veracity of the target’s affidavit. If the prosecutor finds evidence that the affidavit was incorrect or incomplete, the candidate is accused of a lustration lie and tried by a lustration division of a regional court (*Dziennik Ustaw* 2007). If the target is found guilty, not only is the information about his collaboration published, but he is also banned from holding any public office for a period of 10 years.

To see the incentive-compatibility of the lustration mechanism, consider the decision-making calculus from the point of view of the ex-collaborator who is running for office, without knowing if evidence against him was preserved in the secret police archives or not. He may reveal the truth himself and face the certain outcome of voters or nominating agencies learning of his past; or he may withhold it and face the risk of being banned from office for 10 years if evidence of his collaboration was in fact preserved. With some probability, however—and this probability depends on how much evidence exactly was destroyed—he may be able to keep his collaboration secret. Although in the aftermath of democratic transitions, little can be done to change the probability with which evidence was destroyed, by making the sanction for lustration lies sufficiently harsh, lustration designers can ensure that even where evidence destruction was common, former collaborators will continue to self-reveal. Also, because the verification of lustration declarations is limited only to the affidavits that have nothing to declare, the burden associated with enforcing the lustration rule is reduced relative to a regime that would verify the past of all candidates running for office. In addition, the incentive-compatible lustration mechanism comes with the added bonus of being a non-retroactive form of transitional justice. Note that the

lustration court becomes involved because the public official in question lied in his affidavit, not because he was in the past a secret police collaborator. It is the act of dishonesty of the declaration, rather than its contents per se that are the subject of sanction. It is also worth pointing out, that although, in line with hypothesis 6, the law was amended to a more stringent form, the incentive-compatible element of truth revelation in exchange for a milder sanction was preserved.

Yet, it would be unfair to maintain that the incentive-compatible design of lustration comes at no cost at all. As long as the equilibrium of self-revelation is maintained, the only sanction former collaborators face is having the truth about their past publicized. Whether or not this sanction turns into a real penalty of losing office depends on the voters and the nominating agency. Once their interest in sanctioning former collaborators is depleted, so is the force of the lustration law. That is why such incentive-compatible mechanisms ought to be considered as mild forms of transitional justice.

Incentive-compatible lustration mechanisms have also been adopted in Estonia and Latvia. However, incentive-compatible transitional justice mechanisms are not limited to Eastern Europe.⁷ The South African Truth and Reconciliation Commission, which offered perpetrators amnesty in exchange for confessing their criminal acts, is an example of an incentive-compatible truth commission (Gibson 2004). Nor are they limited to truth revelation procedures, as indicated by the Colombian Justice and Peace Law of 2005, which offered former members of paramilitary groups participation in a reintegration program in exchange for surrendering their arms and providing testimony about the paramilitaries' chain of command (Daly 2012).

The purpose of the Polish example was to illustrate how combining the two approaches to institutional analysis—institutions-as-rules and institutions-as-equilibria—can be used in the context of understanding the adoption and operation of transitional justice mechanisms. The institutions-as-rules approach led us to conclude that moderate forms of transitional justice can be implemented by political elites who would prefer to avoid it altogether. The institutions-as-equilibria approach allowed us to characterize incentive-compatible forms of transitional justice using the example of lustration. At the same time, we explained that such incentive-compatible lustration laws are typically mild forms of transitional justice.

Conclusions

As I mentioned in the introduction, research on transitional justice lacks broad empirical tests. Among the few theories that have been tested to date are some of the specific hypotheses from the theory of strategic transitional justice outlined in section 2. These, however, have only been tested with data from a select group of countries in post-communist Europe.

Although there is no reason to believe that only East Europeans are capable of using lustration to promote their favorite political outcomes, no one to date has systematically investigated the strategic uses of transitional justice outside of Eastern Europe. But for all we know, strategic transitional justice could be a worldwide phenomenon. Since the nineteenth century, more than 150 authoritarian regimes have transitioned to democracy, 85 percent of which have transitioned after 1931. All these democratic regimes have been in a position to engage in the strategic use of transitional justice. However, as I explained, this strategic behavior has only been investigated for a select group of post-communist regimes.

Few scholars have even theorized about how former autocrats' anticipations of transitional justice may affect their behavior in nascent democracies. A few hypotheses about anticipating transitional justice have been formulated in the context of Latin American transitions (O'Donnell and Schmitter 1986; Geddes 1999), but their authors doubt that these explanations can be

extended to non-military regimes of the fourth wave of democratization, because single-party authoritarian regimes do not have access to the same coercive measures that outgoing military juntas do.

The lack of research on strategic uses of lustration cannot be attributed to the shortage of lustration procedures outside of post-communist Europe. Although they are not referred to as “lustration” (but “purge” or “vetting” or even “housecleaning”) such procedures are just as popular in other regimes. In a study devoted to post-conflict justice and impunity, Nalepa (2009) catalogues 55 lustration processes that have taken place within the last 70 years and this is a mere sampling.

Understanding the conditions under which transitional justice policies are adopted strategically plays a critical role in policy-making. While practitioners may know which transitional justice mechanisms are normatively best, ultimately, the type of mechanism that gets adopted is rarely best in some normative sense and is frequently the outcome of the interaction between strategic politicians constrained by democratic institutions. Studying transitional justice institutions-as-rules of the game on the one hand, and equilibria on the other, will help us understand which of the mechanisms under consideration are politically feasible, given the political and institutional constraints present. Without knowing what determines the feasibility of transitional justice mechanisms, speculating which mechanisms are normatively best is but an academic exercise with little practical consequence.

One final benefit of studying transitional justice in an institutional context is that this kind of analysis allows us to cut through the thicket of normative considerations accompanying regime transitions and see the similarity between lustration mechanisms and other processes used by states and organizations to screen their members for undesirable characteristics. The most obvious similarities that come to mind are transparency laws (such as Freedom of Information Laws) as well as other anti-corruption measures (Ang 2014). The dilemmas faced by politicians involved in corruption scandals who are concerned that information of this involvement may be disclosed to the electorate are very similar to those facing former collaborators who are running for office in post-authoritarian settings. Understanding how lustration works can help scholars studying transparency laws in conducting institutional analysis.

Notes

- 1 “Épuration” affected tens of thousands of people. Nearly 1,000 politicians, 6,000 teachers and 500 diplomats were vetted for possible collaboration with the Vichy regime. Such measures were not limited to positions in government, but were extended to the private sector as well. Separate purge committees were set up for writers, composers, artists, and the press, among others. The subsequent (1953) amnesty did not give civil servants a right to return to posts that they had lost in the purge, but made that possible, and restored the right to a pension. For an overview see (Macridis 1982).
- 2 For a similar distinction between the two ways scholars use formal theory to address problems in political science see Svolik, this volume.
- 3 Two, among many, examples illustrating how lustration laws undermine cabinet stability include Hungary in 2002 and Poland in 2006. In 2002, Hungarian Prime Minister, Peter Medgyessy, narrowly avoided the collapse of his newly created cabinet after an article in a Budapest daily revealed that he had worked as an undercover agent for the military counterintelligence (BBC International Monitoring 2002). In 2006, Polish Deputy PM, Zyta Gilowska, had to resign from office after being accused of collaborating with the secret police. Her resignation eventually brought down the entire cabinet (Easton 2006).
- 4 In a non post-communist setting the interpretation of infiltrated and non-infiltrated parties would be modified depending on which parties had collaborated with the ancien régime or occupying force. For instance, in post World War II Belgium, an example of a non-infiltrated party would be the Belgian Socialist Party, while an example of an infiltrated party would be the Christian Flemish National Union.

- 5 For readers familiar with game-theoretic terminology, this is the Subgame Perfect Equilibrium, which is the most appropriate equilibrium concept for extensive form games of this type.
- 6 As a conjecture, one could consider what happens if the agenda setting rules were weak. In such cases, we know from the Downsian model, that the preferences of the legislative median prevail. As a result, we cannot empirically distinguish strategic from non-strategic behavior. For this reason I refrain from formulating hypotheses that hold uniquely for legislatures with weak agenda setting powers.
- 7 Although examples of incentive-compatible lustration laws abound in Eastern Europe. In Estonia, the parliament adopted a law in February 1996 that required anyone who served as an informer or employee of foreign (i.e., Soviet and German) security intelligence bodies submit statements describing their activity to the Estonian security police. The names and statements would remain confidential, but the names of those who failed to confess within a year would be made public in the official State Gazette (Baltic News Service). The Latvian Parliament began discussing the passage of a similar law to the one in Estonia in May of 1993 in the context of the upcoming elections to the Saeima. The law was to mandate that each candidate running in the Saeima elections submit a statement that he has not been an official of the USSR or LSSR Committee for State Security [the KGB], the USSR Ministry of Defense, the security services, the army intelligence service, or the counter-espionage staff of Russia. The law was passed on January 15, 1994, just before the elections. Later, it was extended to elections to city, regional and district councils.

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THE INSTITUTIONAL COMPONENTS OF POLITICAL CORRUPTION

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Introduction

The misuse of public office for private or partisan gain—political corruption, in short—is as ancient as the art of politics itself. Its contemporary study began in the 1990s, with the public dissemination of various rankings on the degree of corruption in countries around the world. As the data depicted in [Figure 28.1](#) suggest, the rankings document that the main correlate of corruption is economic development. Poor countries are often highly corrupt; rich countries, almost never. Studies that analyze the rankings spotlight the great theoretical puzzle of political corruption: the establishment of democratic institutions has at best a modest and perhaps even no systematic impact on a country's cross-national corruption ranking. The coding by regime type depicted in [Figure 28.1](#) shows that poor democracies are as likely to be highly corrupt as poor autocracies. Why democracy does not conclusively and significantly reduce corrupt activities by elected officials remains an ongoing topic of controversy, and the main unsolved theoretical problem associated with this topic. It is not, however, the only unsolved issue related to corruption.

In this essay, in keeping with the purpose of the present volume, we review what we have learned in the last 25 years about how formal political institutions affect political corruption. We divide our remarks into three parts. First, we discuss the impact of basic political institutions in established democratic polities on corruption. We attend, in turn, to executive institutions, electoral institutions, federalism, and, finally, judicial institutions. Our discussion of institutional variation is limited to democratic polities since scholarly understanding of these institutions and how variations in them operate are confined chiefly to this regime type. We then discuss the inferential weaknesses in this line of work. The data available to measure corruption across countries does not permit research designs that allow convincing causal identification. A second section of this essay reviews the results of studies that stand on stronger inferential ground. We divide these into two separate topics: whether and when voters punish corrupt politicians at various levels of government and how political corruption affects the performance of government bureaucracies, especially as regards service delivery. In a third and final section of this essay, we return to the question posed earlier and discuss possible explanations for why the establishment of democratic political institutions appears to exert so little influence on the degree of political corruption.

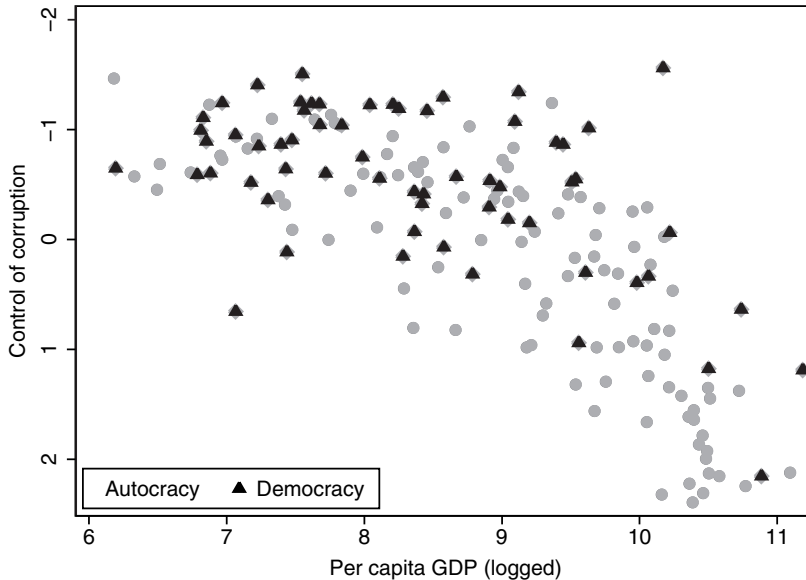


Figure 28.1 Scatterplot of corruption and per capita GDP (2012) for 183 countries by regime type (2008)

Sources: Control of corruption from World Bank, World Governance Indicators (Kaufmann *et al.*, 2010); GDP per capita from the World Bank, World Development Indicators (World Bank, 2013); regime type from Cheibub *et al.* (2010).

Our review explores three separate tensions in the institutional literature on corruption. First, there is tension between studies that use cross-national data and those using subnational data. These usually focus on different research questions, but when they study the same problem, produce divergent results. Second, reflecting a broader debate in the social sciences, there is tension between studies that use observational data, on the one hand, and studies that employ experimental or quasi-experimental research methods. Typically, the scope of the question that can be studied narrows as the research design moves from observational to experimental or quasi-experimental but the results are more believable. Third, there is a tension between institutional and non-institutional explanations of corruption.

Our own reading of the literature is that the most fruitful research in the next decade will be subnational, experimental or quasi-experimental, and non-institutional. But that, of course, remains to be seen.

Corruption in democratic polities

Can a change in political institutions reduce corruption? If a country in which corruption occurs at high frequency were to alter its electoral system, or the relations between the executive and legislative branches of government, would corruption fall as a consequence? These questions have fascinated political scientists and political economists in recent decades, especially because of a wave of reform to political institutions that has swept the democratic world. Between 1975 and 2010, 87 countries modified executive-legislative relations, for instance, while 19 countries adopted modifications of their electoral systems.² (Among the countries, we include those that transitioned from communism to democracy in eastern Europe and the former Soviet bloc.) In some of these cases, institutional reforms were designed in part with the explicit aim of curtailing corruption. Which reforms, if any, are likely to be successful in this regard?

Separating the effects of one type of national political institutional configuration from another is difficult, in part because institutions tend to cluster together. Countries whose electoral systems use proportional representation and not single-member majority systems, also tend to have prime ministers rather than presidents head their executive branch. For purposes of this review, nonetheless, we distinguish executive, electoral, federal, and judicial institutions. We treat each in turn and review what studies have shown about the effects on corruption of each separately. This is obviously highly artificial, since the effects of one institution on corruption may well hinge on the configuration of other institutions.

Executive institutions

The cross-national literature that has examined the impact of variations in executive institutions on corruption is divided between studies whose results support the argument that parliamentary regimes are less corrupt and those that argue precisely the reverse, namely, that presidential regimes are less corrupt. Panizza (2001) finds evidence in support of the former, showing that presidential systems tend to have lower institutional quality and speculating that under presidential regimes, there are greater incentives for rent-seeking behavior. Likewise, Gerring and Thacker (2004) and Lederman *et al.* (2005), in cross-national studies using 20 years of data, corroborate that presidential regimes exhibit more corruption than parliamentary systems. This is consistent as well with cross-national work by Kunicová and Rose-Ackerman (2005). The causal mechanism proposed for this pattern is that parliamentary systems—and in particular, the British “Westminster” system of parliamentary government—make it easy for voters to identify who is responsible for policy outcomes without having to sort through the overlapping jurisdictional problems of presidential systems. Further, according to Gerring and Thacker (2004: 315), “basic-level institutions that foster strong parties and effective governments, such as unitarism and parliamentarism, also help promote lower levels of political corruption.”

These results are, however, contravened by those reported by Persson and Tabellini (2003) and Persson *et al.* (2003), who contend that parliamentary systems are more corrupt than presidential regimes. The latter stream of work contends that presidential systems provide more accountability given the executive’s reliance on popular support, thus allowing for the punishment of corrupt politicians. In parliamentary systems, on the other hand, the executive is only indirectly appointed through national elections, weakening the accountability link between voters and the office of the prime minister. Persson and Tabellini (2003) also advance that the lack of strong checks and balances between legislative and executive branches in parliamentary regimes can further weaken political accountability and thereby present opportunities for increased corruption. This remains a lively and unresolved debate.

Electoral institutions

Like studies of executive institutions, the literature on the effects of electoral systems on corruption is divided. The first studies to investigate how electoral systems affected corruption argued that proportional representation was associated with more corruption than plurality electoral systems (Persson and Tabellini 2003). Along similar lines, Kunicová and Rose-Ackerman (2005) report higher corruption under closed-list than open-list PR, particularly in presidential systems. Their argument is that closed party lists provide disincentives for incumbents to perform well in office given their reduced visibility to voters. Using subnational surveys on public officials conducted in Bolivia, Brazil, and Chile, Gingerich (2013) also provides evidence of higher

corruption levels in closed-list than open-list PR systems, attributing this finding to their party-centric design, which encourages corruption in campaign and party financing.

These results are in contrast with theory developed in Carey and Shugart (1995), which argues that there will be a greater tendency for rent-seeking behavior in open-list systems where incumbents need to develop personal relationships with voters in order to win reelection than in their closed-list counterparts. This is corroborated empirically, especially when district magnitudes are large, in Chang and Golden (2007). The latter study, like all those referenced above, uses cross-national data, but it also adds a subnational component.

Further complicating the picture, Lederman *et al.* (2005) find no difference in levels of corruption between closed and open-list electoral systems once presidential, political stability, and press freedom are included as regression covariates. The best we can conclude from this ongoing debate is that cross-national results are not stable with changes in countries and years sampled or according to which other variables are included in the models.

Looking at more fine-grained measures of electoral institutions, James Alt and David Lassen (2003, 2008) analyze the institutional determinants of corruption across 45 states in the U.S. Among the leading causes of corruption in the U.S. are the presence of closed primaries, campaign spending restrictions, and the absence of direct voter initiatives, which can “increase the scope for political accountability by unbundling the voting decision” (Alt and Lassen 2003: 355). Interestingly, term limits do not seem to have an effect on corruption, in contrast to results reported in other work (e.g. Besley 2006). The studies that use cross-state U.S. data have strong internal validity in part because of the ability to hold macro-political factors fixed when considering subnational data. However, the findings are difficult to generalize given the many unique institutional aspects, such as the electoral college, the primary system, weak executive veto powers, and the exclusive use of single-member districts, of the U.S. among other presidential systems.

Federal institutions

Can federalism be regarded as an anti-corruption institutional framework? The federal as opposed to the unitary structure does provide additional checks and balances that could make it more difficult for corrupt actors to operate freely. Indeed, Weingast (1995) and Rose-Ackerman (1994) argue that federalism promotes local competition among law enforcement and oversight agencies, resulting in a more efficient and less corrupt political market. However, early empirical work on the consequences of federalism found the opposite effect. The first cross-national study to analyze the relationship between federalism and corruption is by Treisman (2000), who shows that federal countries experience higher levels of corruption than unitary countries. Gerring and Thacker (2004) and Goldsmith (1999) corroborate these results, finding that unitary systems foster lower levels of corruption due to the lack of veto points and the hierarchical framework of political institutions. Yet, more recent work by Treisman (2007) finds that the relationship between federalism and high corruption disappears when using updated corruption data. As Treisman (2007: 235) writes, “Federalism’s strong significance in regressions using TI’s 1996 and 1997 data probably reflected the limited number of countries available for those years.” In short, it is still unclear whether territorial sovereignty has an impact on corruption. While federalism is expected in theory to reduce corruption by increasing political competition and accountability, the empirical evidence from cross-national studies remains ambiguous.

Closely related is the study of political decentralization on corruption. While not formally classified as federal, countries with fiscal autonomy granted to subnational units are theorized to have more corruption at local levels. This follows from the logic that local autonomy in public

service provision may lead to capture by local elites (Fisman and Gatti, 2002). Using a formal model of capture in the delivery of service provisions, Bardhan and Mookherjee (2000) show that decentralization can lead to more corruption when compared to centralization. Importantly, the latter study also shows that decentralization is not necessarily corruption-promoting: local capture can be reduced by adopting institutional constraints on the revenue-raising capabilities of local governments through “user-fees” financing mechanisms.

Subnational research on federalism and corruption reveals interesting complexities. In another study, Bardhan and Mookherjee (2006) collected extensive original data to investigate transfers at multiple substate levels in one Indian state over two decades. They report that once government allocations reach villages, they are distributed as intended but that prior distributions to villages are characterized by inequitable elite capture. Although their concept of capture is looser than that of corruption, a clear implication of their argument is that corruption within federal systems will be greater when the actors involved in decision making are more restricted in number and when the decisions are less subject to public scrutiny. Perhaps more importantly, their work suggests that subtle and local decisions about how multi-tiered government institutions actually operate in practice affect their vulnerability to corruption. If this is correct, it helps explain the absence of consistent results in cross-national studies that attempt to ascertain whether federal or unitary systems are in general more corrupt. Bardhan and Mookherjee’s work suggests that it depends on fine-grained details of how institutions operate rather than on formal institutional structures.

Judicial institutions

While no one doubts that the rule of law is essential to clean and honest government, the problem in assessing the role of the judiciary in particular is largely one of measurement. Thus far, there are no measures for the independence and integrity of judicial institutions that are at once valid and clearly distinct from the outcomes that mark corruption in the first place. Some have argued that specific legal traditions affect corruption differently, and that continental legal systems, which are heavily prosecutorial and lack strong protections of the accused, are less effective in curbing corruption than the Anglo or German legal tradition (La Porta *et al.*, 1997, 1999; Treisman 2000). The results of these cross-national analyses are fragile, however, and because the legal institutions of many countries are difficult to classify and sometimes overlap (Keefer 2007; Gourevitch 2008), the sample of countries included tends to be restricted.

Splitting judicial independence into cases of *de jure* and *de facto* independence from other branches of government, van Aaken *et al.* (2010) provide cross-national evidence that an independent judiciary reduces corruption, but only when there is prosecutorial independence both in practice and in law. Maria Popova finds the opposite pattern using subnational data from Ukraine and Russia (Popova 2010) and Bulgaria (Popova 2012). Here, judicial independence does not reduce corruption, and in contexts of intense political competition (as is the case often in new or transition democracies) judicial independence appears to increase political corruption. The discrepancy in findings could be due in part to what Julio Ríos-Figueroa argues is the “u-shaped” relationship between judicial independence and corruption. When judiciaries are dependent on the current government, corruption is likely because executive and legislative behavior remains largely unchecked; when judges are independent, corruption could also emerge because independent judges may demand bribes for their verdicts (Ríos-Figueroa 2012). The so-called “sweet spot” is found in contexts where there is both some level of dependence and independence under an institutional system with broader checks and balances.

A unique assessment of the importance of judicial institutions in reining in corruption is McMillan and Zoido (2004), which analyzes the amounts paid by Peru's Fujimori government to various non-governmental actors to induce them to refrain from exposing or prosecuting corruption. Using the meticulous records kept by the office of the President of amounts paid in bribes, McMillan and Zoido document that the government paid larger bribes to the press than to judges. The authors interpret this as evidence that the government considered the press the main threat to its chronic illegal activities and as more important in this regard than the judiciary. Although restricted to a single country, the McMillan and Zoido (2004) study suggests that a free and aggressive press is likely to inhibit corruption more than an independent and active judiciary, although both are important.

Summary

Why do we find so many conflicting results across studies on the impact of basic political institutions on corruption? The only agreement in the literature is that freedom of the press—which we have not included in our review of formal political institutions—by exposing corruption, helps reduce it. There are two sets of reasons for the inconclusive results.

First, there is an inherently limited number of cases to consider: namely, the world's established and consolidated democratic countries, which currently number about one hundred. The estimation complexities of identifying the impact of a single institutional change in highly complex institutional, cultural, and geographic contexts are effectively overwhelming. Often, multiple changes occur simultaneously; or institutional change is endogenous to changes in public opinion, making it impossible to isolate the impact of the former; or a similar institutional change sweeps across many of the countries on a single continent in a short period of time. Identifying the effect of a single institution on corruption proves difficult because of the impossibility of holding everything else constant. As a result, the effects of institutions identified by cross-national studies are not estimated with enough precision to allow us to be confident that the impact is genuine.

A related reason for the inconclusive results in this domain of inquiry lies with the likely size of the effect we are trying to estimate. Overall, the magnitude of the effects of institutions on corruption is relatively small. Mainly, as we noted at the outset of this essay, economic development reduces corruption. When considering institutional variation, we are thus dealing with small magnitudes that cannot be precisely estimated. As a result, studies that utilize cross-national data to investigate the impact of institutional variations on corruption remain inconclusive and contested. Subnational studies, such as those that analyze institutional variations across the United States, lack external validity to make broader claims about institutional determinants of corruption.

Inference and research design

A deeper but not unrelated issue is highlighted by what has come to be called the credibility revolution in the empirical social sciences (Angrist and Pischke 2010). Small effects and imprecise estimates aside, the research designs used in the cross-sectional studies that we have referenced in the preceding section are not suitable for making causal inferences. This is because, to crib Angrist and Pischke (2010: 8), “they fail to isolate a source of variation in execution rates [political institutions] that is likely to reveal causal effects on homicide [corruption] rates.” More and more, scientific studies are shifting to the use of random assignment or to natural or quasi-experimental research designs in order to make valid causal statements.

Before we turn to examining some of these studies, we review more thoroughly the data used to study corruption on a cross-national basis. This sheds light on why it precludes the use of design-based inference.

Today's research into corruption was initiated by economists. They were chiefly interested in whether and to what extent corruption reduces economic growth (Mauro 1995). This concern was a direct result of the new institutional economics, inspired by the work of Ronald Coase, Douglass North, and others. The new institutional economics focused attention on how formal (and informal) political institutions affected entrepreneurship and economic growth. An interest in corruption, as well as related topics such as the rule of law, naturally followed.

The data first used to study corruption was proprietary, available for purchase from Political Risk Services, which since 1980 has produced the International Country Risk Guide (ICRG) for (currently) 140 nations. In 1995, a new international NGO, Transparency International (TI), launched its Corruption Perceptions Index (CPI), which in 2013 ranked 177 countries on the basis of results from multiple surveys, mainly of experts and international businesspersons. The aim of TI was to improve governance, and the organization's annual ranking of countries according to the degree of corruption was placed in the public domain as part of an effort to focus international attention on corruption. Finally, starting in 1996, the World Bank (WB) has provided a bi-annual modified CPI index, often referred to as KKZ, after its original authors, Daniel Kaufmann, Aart Kraay, and Pablo Zoido-Lobaton. The KKZ index weights country scores by the variability of the underlying surveys, but draws on essentially the same surveys as the CPI. The World Bank's intention was to weight survey results by how much they corresponded to results from other surveys.

All these indices produce similar results and, as we discuss momentarily, suffer similar flaws. The correlation coefficient between the WB indicator for corruption and the CPI index is 0.99 for 2012, for instance. The exact reasons for the strong relationships across data sources are difficult to pin down. The methodology used for assembling the surveys that TI uses has never been made fully public. This means that analysts are not able to examine whether changing the methods used alters the rankings. In addition, the underlying surveys change from year to year in various ways. This renders temporal analysis invalid, thereby precluding the use of differences-in-differences estimates that would generate causally valid estimates of the impact of changes in institutions on changes in the frequency of corruption. This is the main reason that the cross-national survey measures of corruption that are available are not susceptible to design-based casual research. Finally, all the indices reflect sample bias in favor of business-oriented perceptions (Arndt and Oman, 2006). Whether the indices capture the frequency of corrupt transactions that ordinary citizens, especially poor people, are embroiled in remains an open question. Citizen surveys tend to generate rather different patterns than the kinds of surveys used by TI and, by extension, the World Bank (Razafindrakoto and Roubaud, 2010). This suggests that the cross-national indices that are available reflect specific corruption markets rather than successfully measuring corruption generally.

A more recent source of cross-national data are penalties for convicted acts of corruption. Information gathered from violations of the Foreign Corrupt Practices Act (FCPA) provides one such measure. These data are based on published transcripts of FCPA violations that are released by the U.S. Department of Justice (DOJ) and the Securities and Exchange Commission. The output is the real dollar amount of penalties paid by firms for violations of the FCPA in a given country or countries. These values are then aggregated at the country level to create a cross-national corruption measure, which is shown in [Figure 28.2](#), along with the same map using an existing perceptions-based measure, the TI-CPI scores from Transparency International (Transparency International 2012). A virtue of this measure, unlike the survey-derived measure just discussed, is that it captures actual incidences of corrupt activities as opposed to perceptions

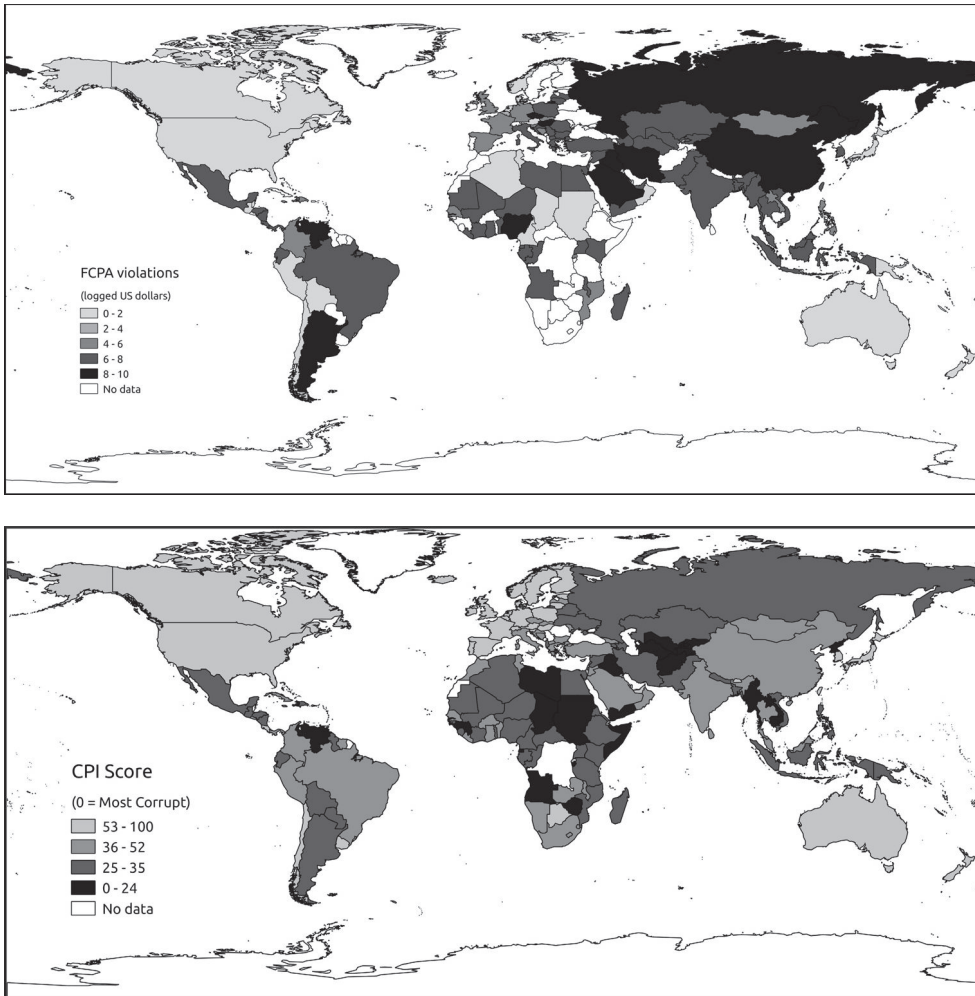


Figure 28.2 Geographical variation in FCPA violations across the world, 2006–12 (top map), and Transparency International Corruption Perceptions Index, 2012 (bottom map)

Notes: FCPA penalties data are measured in logged US dollars; TI-CPI data are measured on a 0–100 scale with 0 representing high levels of corruption and 100 representing low corruption.

Sources: United States Department of Justice (2012a, b); Transparency International (2012).

of corrupt behavior. Additionally, these data provide researchers the ability to focus on specific economic sectors rather than consider corruption as it is perceived at the aggregate level.

The only studies to have investigated corruption using the FCPA data, and data from other anti-corruption laws around the world, are to date still unpublished. The first, by Escresa and Picci (2014), uses violations of the FCPA, the OECD Anti-Bribery Convention, and the UK Anti-Bribery Act to construct a corruption index to capture variance in cross-border corruption cases. A second study disaggregates the FCPA data by economic sector to focus on the causes of corruption in the petroleum industry. It reports that nationalization increases the levels of bribery and extortion in the oil and gas sector (Mahdavi 2014).

One weakness of this measure is that the more likely a country is to be perceived as corrupt, the more likely it is to be examined for FCPA violations. It could be the case that perceptions of corruption are so strong as to influence judges and prosecutors around the world to pursue some cases rather than others. At worst, this would make these incidence-based measures no more helpful than the existing perceptions-based measures. Further research using this measure will have to adjudicate between the measure's conceptual accuracy and its potential endogeneity.

None of the cross-national indices currently available distinguish political corruption, or illegal activities for personal or partisan gain on the part of elected officials, from bureaucratic corruption, or illegal activities for personal or partisan gain on the part of bureaucratic officials. This distinction was formulated initially by Rose-Ackerman (1978). Bureaucratic corruption includes the small bribes taken on a daily basis by government agents who interact directly with the public: traffic police, customs agents, tax authorities, school officials, and inspectors and regulators of all sorts. Political corruption refers to illegal activities that are often at a much larger financial scale, including bribes for government contracts and construction. The distinction hinges on the nature of the agent involved in the illegal dealings and not on the scale, but they appear to vary together. Very large-scale corrupt dealings necessarily involve highly placed elected officials taking bribes from large corporations in exchange for government contracts. Bureaucratic corruption, by contrast, usually involves small amounts of money collected from large numbers of government clients. Data from cross-national surveys does not allow empirical distinction between political and bureaucratic corruption. The FCPA data are the first global cross-national data that we know of that specifically identifies political corruption.

As scholars have become more concerned with issues of research design and causality, they have turned increasingly to the use of subnational measures of corruption. Based on more precise data, the underlying validity of such measures is usually higher than that of the cross-national measures, although the obvious trade-off is that they are limited in geographic scope. Likewise, subnational studies often are more precisely concerned with either political or bureaucratic corruption. Finally, moving to the subnational level permits carefully constructed experimental or quasi-experimental research designs. We turn now to a discussion of their results.

Design-based studies of corruption

As the limitations of cross-national studies, both for the study of corruption and more generally in the field of comparative politics, became increasingly obvious, research has shifted to the subnational level. Many scholars have relinquished the attempt to estimate or explain the degree of corruption in countries around the world, turning instead to generating and then analyzing subnational data on corruption. Some of these studies also reverse the causal arrow: instead of seeking to explain variations in the frequency or extent of corruption, they seek to assess the impact of corruption on outcomes such as the performance of local government, the delivery of social services, or legislative and political selection and retention. This is also in line with a growing policy-relevant focus of contemporary work.

Research in this category derives largely from fortuitous or deliberately created data availability, which is inherently limited and usually highly country-specific. The latter means that much of what has been learned may be idiosyncratic to the setting, although in the absence of similar studies in other countries, it is difficult to be sure. The strength of these studies, however, is that they reflect much more serious attention to the issues of research design than have been stressed in the literature (Dunning 2012; Gerber and Green 2012).

Using experimental or quasi-experimental (natural) designs, analysts have explored two separate topics: political selection—whether, that is, politicians with records of political corruption

suffer electoral consequences—and service delivery and the relationship between bureaucratic and political corruption. We discuss each in turn.

A series of papers has exploited the availability of randomized financial audits of Brazilian municipalities to study the impact of public revelations of financial improprieties by mayors on their electoral fortunes (Ferraz and Finan 2008; Pereira *et al.*, 2009; Ferraz *et al.*, 2010). Auditing began in 2003 with the random selection of municipalities to be investigated. Local elections were held in the middle of the auditing process, thereby creating a sample of municipal elections distinguished by whether or not the public had recently received credible information about financial improprieties on the part of local government. This discontinuity allowed scholars to exploit the random assignment of municipalities as audited or non-audited and to compare the electoral fortunes of incumbent mayors facing audits with mayors who did not.

The results of the research show that mayors whose audits reveal them as having engaged in substantial financial abuse of office are significantly less likely to regain office. An even more pronounced effect is found for mayors in audited municipalities that have radio stations, which are as a channel for information dissemination. The finding has been interpreted to show that voters need only credible and relevant information about political corruption to eject corrupt incumbents from public office. Other local-level studies that also report that new information alters voting behavior or reduces local government corruption through similar mechanisms include Banerjee *et al.* (2009) and Reinikka and Svensson (2005). More formal work on the effects of information in reducing agency problems between voters and incumbents is provided in Besley *et al.* (2005). With more access to information on politicians, voters can identify corrupt leaders and hold them accountable at reelection.

This view contrasts sharply with analyses of voter responses to revelations of corruption at the national level, where study after study (all using observational data and less inferentially valid research designs) report that voters almost never eject corrupt politicians from office even when new and credible information emerges (Peters and Welch 1980; Welch and Hibbing 1997; Reed 1999; Chang *et al.*, 2010). The reasons for the discrepancy are not precisely understood. Are the different results due to the use of experimental compared with observational data? This would seem the first place to look, but the differences in results do not seem obviously related to that. Do voters respond differently to corruption revelations about local elected officials than about national level politicians? Evidence in support of this comes from the only study that to our knowledge compares electoral outcomes for politicians at different levels of government—in this case, municipal, regional, and national—in a single time frame, all charged with similar criminal offenses. Analyzing (non-experimental) Italian data from the 1990s, Asquer (2014) reports that incriminated mayors and local-level politicians are repudiated by voters at much higher rates than national legislators—even though the national legislators are often accused of more serious crimes.

Our own hypothesis for this turns on the extensive literature about differences in the voting population in local as opposed to national elections (Bechtel 2012; Jackson 1997; Patterson and Caldeira 1983). Turnout is consistently lower in local elections, whose electorate is more interested in and informed about politics. Being more informed and more selective in their political choices, the electorate that votes in local elections is also more likely to use information about political malfeasance to eject politicians from office. Some evidence from the U.S. exists that supports the idea that the electorate is not homogeneous in its responses to corruption revelations, and that more informed and aware members of the electorate are more likely to repudiate politicians charged with wrongdoing (Klašnja 2014).

Given the current state of the evidence, however, other interpretations may still be correct. Perhaps the informational resources of the subelectorate that participates in local elections are not

relevant and it is the type of elected office that is important. Voters could respond in systematically different ways to revelations about incumbents holding executive office, such as mayors or presidents, than to revelations about legislators. This might be because ejecting a single legislator from office may not reduce corruption if enough other legislators are engaged in corrupt practices. Electing a new president, by contrast, can more easily carry thorough-going consequences.

Considerable uncertainty thus surrounds the literature on voter responses to revelations of corruption by elected officials. We have little direct information on how much voters value probity on the part of elected or appointed officials or when they are willing to relax selection criteria. The precise mechanisms that encourage voters, or a small subset of voters, to use information on legal transgressions or malfeasance by elected officials as the basis for their vote choice remain open for additional research.

Another debate that uses subnational data concerns the performance of government bureaucrats. Particularly telling is a series of studies of absenteeism by civil servants, particularly teachers and medical professionals (Chaudhury *et al.*, 2006; Khemani 2007; Björkman and Svensson 2009). Compiling results of parallel research undertaken in Bangladesh, Ecuador, India, Indonesia, Peru, and Uganda, scholars report that on average, 19 percent of teachers and 35 percent of health workers are absent from work at any single time. Even when present, many educational and medical professionals are found to be doing something other than working when unannounced enumerators visit their work sites. An almost unique aspect of this work is that it draws on results from different country settings, showing that it is possible to replicate and cumulate micro-level research. Indeed, as far as we are aware, these are the only studies related to corruption that have been performed in multiple national settings with the explicit aim of making separate assessments on a country-by-country basis of the degree of corruption in particular spheres.

A study of the quality of government (Rothstein 2011) details mechanisms linking corruption in the medical sector and the performance of the health bureaucracy. Because of information asymmetries between doctor and patient, government healthcare professionals easily influence the cost and necessity of treatments and procedures. Combined with the complexity of the industry, this makes the health sector particularly prone to corruption. This in turn ultimately damages aggregate health outcomes. These findings are supported by cross-national evidence on corruption and health outcomes, such as life expectancy, infant mortality, and World Health Organization survey measures of healthcare quality.

Recent experimental research conducted in Pakistan has extended this line of work, exploring explicitly the political determinants of misallocation in the health sector (Callen *et al.* 2013). The new study shows that absences (in this case, of medical personnel) are most common among those who owe their appointments to the patronage of a local politician. This has important implications. It demonstrates that bureaucratic corruption is a function of deliberate choice by elected politicians rather than a phenomenon that elected officials are unable to prevent. This corroborates earlier studies that theorized along similar lines (Golden 2003). Bureaucratic corruption does not occur because of agency slippage in the control of bureaucrats by politicians but instead because allowing bureaucrats to engage in corruption is useful for politicians, perhaps in the construction of their electoral coalitions. To understand variations in the frequency of bureaucratic corruption thus requires a theory of the electoral incentives governing strategies of bureaucratic slippage, something that is a long way off.

Summary

Even with more attention to issues of research design and more precise data, uncertainty about the causes and the consequences of corruption continues because of the difficulties in making

cross-national comparisons based on subnational data. There could be contextual effects that are not easily captured by comparing different studies—for instance, although corruption may diminish a Brazilian voter's evaluation of political incumbents, revealing information on corruption may improve an Indian voter's image of elected officials. Here we see an emerging opportunity for future research that captures salient differences across existing subnational work, perhaps along the lines of meta-analysis by Lau *et al.* (1999) of experiments and observational studies in political communication and media effects or the exemplary meta-analysis by Hsiang *et al.* (2013) of climate change and violence.

Democracy, development, and corruption

In democratic polities, public authorities are subject to recall by voters. The electoral sanction imposes basic accountability on elected officials. It follows—at least in principle—that democratic polities exhibit outcomes that better reflect the preferences of voters than occurs in non-democratic settings. Yet it has proven surprisingly difficult to document this empirically. On a whole series of dimensions that ought to be highly pertinent to voters, researchers have had difficulty producing robust and compelling evidence that democracies perform better than non-democracies. The literature shows contrasting and fragile results about whether public health, education, or other measures of welfare are superior in democracies than non-democracies (Lake and Baum 2001; Mulligan *et al.* 2004; Ross 2006). Likewise, cross-national research finds that corruption is only weakly and inconsistently reduced in settings marked by free and competitive elections. Treisman (2000), for example, reports that democracy reduces corruption only in the long term, when a country has been democratic for more than 50 years. This suggests that it takes three generations for voters to use the electoral mechanism to hold elected authorities accountable, which is a long learning curve. A more recent study (Treisman 2007) shows that the “democracy effect” is primarily due to higher press freedom in consolidated democracies rather than voter learning. And when using polity measures that capture competitive elections and strong executive constraints, the relationship between democracy and corruption disappears. A study by Montinola and Jackman (2002) argues that democracy may have a non-linear effect, whereby hybrid or transition democracies are marked by higher corruption than either full autocracies or democracies, though these results only appear statistically significant with the Freedom House measure of democracy.

Most of the difficulty comes from countries at lower levels of development. Wealthy countries are disproportionately democratic. They also perform well on all the outcomes we would expect, including corruption. Poorer countries, by contrast, are highly heterogeneous, and poverty tends to swamp institutional differences. In [Table 28.1](#), we show the average corruption scores in 2012 based on the KKZ measure used in [Figure 28.1](#), broken down by income group and regime type. Not only is there more balance between regime types among the poorest countries of the world, but corruption is statistically equivalent across poor autocracies and poor democracies. Indeed, the poorest democracies appear just as corrupt as the poorest autocracies, while in richer countries democracy is the norm (39 of the 48 high income states are democratic) and corruption is relatively low. Interestingly, even without statistical controls or multivariate regression, the data indicate that middle income democracies are more corrupt than high income autocracies, consistent with economic development as the main determinant of corruption. Why might this be the case?

Some recent theoretical headway on this exceptionally challenging question comes in two recent papers. Fearon (2011) provides an investigation into mechanisms underpinning effective electoral accountability in fragile democracies. The paper sets out the problem of understanding

Table 28.1 Corruption by regime type and income group (2012)

Income group	KKZ Corruption (2012)		
	Autocracy	Democracy	Difference
Low income			
	– 0.90	– 0.80	0.10
	(0.09)	(0.09)	(0.14)
Number of cases	24	15	
Middle income			
	– 0.57	– 0.16	0.41***
	(0.10)	(0.09)	(0.13)
Number of cases	39	56	
High income			
	0.43	1.25	0.82***
	(0.35)	(0.12)	(0.32)
Number of cases	9	39	

Note: ***p < 0.01

Sources: Regime type from Cheibub *et al.* (2010) where democracy is defined as a “system in which incumbents lose elections and leave office when the rules so dictate.” Income group from the World Bank, World Development Indicators (World Bank 2013). Countries with less than \$1,035 of per capita GNI are classified as low income and countries with more than \$12,616 of per capita GNI are classified as high income. Control of corruption from World Bank, World Governance Indicators (Kaufmann *et al.*, 2010).

why outcomes such as rent-seeking may be identical across authoritarian and democratic polities: why, in other words, universal suffrage and regular elections do not necessarily produce accountability. In Fearon’s model, coordinated political opposition, in the form of an organized political party, may play an important role in making democratic elections credible, thereby reducing shirking by leaders. Bidner and Francois (2013) build on Fearon’s paper to argue that democratic political institutions only operate as expected when rulers act according to norms of accountability: that is, when citizens are willing to punish transgressions by rulers.

Each of these two papers provides an intuition about different factors that may reduce corruption even in impoverished but democratic settings. From Fearon comes the idea that elections operate more effectively to enforce accountability where political contests are organized; hence, political parties may be relevant in containing corruption. As far as we are aware, little research into how party systems affect corruption exists. From Bidner and Francois comes attention to the development of norms, and in particular the diffusion of the norm that transgressions by government will be punished by voters. Each of these theories represents a major analytic challenge for future empirical progress in the study of political corruption.

Conclusions

Institutionalist approaches are surprisingly ineffective in understanding corruption. Democratic elections permit but do not enforce accountability. We speculate that they limit corruption and

rent-seeking only when underpinned by popular expectations of how political leaders should behave and when these expectations are organized into permanent political rivalry and monitoring in the form of political parties. Thus far, scholarly work has yet to show that democracy reduces corruption. Instead, we find that in the poor nations of the world, where corruption is often very high, democratic political institutions are inadequate to establish and coordinate expectations that reduce corruption. Within democracies, even wealthy ones, it is similarly unclear which specific configurations of formal institutions explain variations in corruption outcomes. Economic development, finally, is the main driver of the reduction of corruption that characterizes the West, but we have little understanding of how, when, or why this occurs during the process of modernization.

The literature we have reviewed shows more agreement about the consequences of corruption on institutions. Corruption is uniformly seen to carry negative effects on bureaucratic performance and related outcomes, particularly in the health and education sectors. Here the challenge is to devise research that can assess quantitatively the consequences of corruption. What are the costs of corruption on years of schooling, literacy, and lifetime income? What are its costs for health-adjusted life expectancy? Assessing the costs of corruption is not only of academic interest but also likely to be important in reducing corruption itself.

Notes

- 1 For comments on an earlier version, we are grateful to Jennifer Gandhi and Rubén Ruiz-Rufino, as well as to Lucio Picci. Financial support to Miriam Golden during work on this project was provided by the Academic Senate of the University of California at Los Angeles.
- 2 This calculation is based on data from the Database on Political Institutions, managed by Philip Keefer (Beck *et al.*, 2001). A change in executive-legislative relations is coded for any shift between presidential, parliamentary, or hybrid regime. A change in electoral systems is coded for any shift from PR to plurality or vice-versa.

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POLITICAL INSTITUTIONS AND ECONOMIC DEVELOPMENT

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Why are some countries rich and others poor? Part of the variation in economic development across countries can be explained by accumulation of human and physical capital and investments in innovation. However, substantial variation in economic development across countries remains after accounting for these factors. A growing body of literature emphasizes political institutions as the “deeper” determinants of economic development (e.g. Rodrik 2003, Helpman 2008). Political institutions encourage or discourage the rate of accumulation and innovation by influencing policy decision-making and by enabling (or not) politicians to credibly commit to policies.

This chapter focuses on three determinants of the substantial variation in economic development across countries: (1) the ability of a government to credibly commit to not behave opportunistically; (2) the ability of a government to protect private property rights and enforce contracts; and (3) the ability of a government to provide public goods and infrastructure. The chapter also surveys the different historical factors that have been proposed as explanations for why institutions differ across countries.

Why are these three factors determinants of economic development? Credibility makes governments reliable by limiting the incentives for politicians to change policy opportunistically tomorrow. Settings of low government credibility can lead to misallocation of public funds and distorted public policy. Private property rights and contract enforcement promote investment, specialization and market exchange. Private actors will not make optimal accumulation and investment decisions without adequate legal guarantees; that is, in the face of risks of expropriation by the government, or by other private actors. Markets do not develop without a last resort guarantor of market transactions. Lastly, public goods foster economic development by providing physical and organizational infrastructure.

While many scholars agree that credibility, legal protections, and physical and organizational infrastructure are key determinants of economic development, there is less agreement on the specific political institutions that promote and maintain credibility, protect transactions and organize collective action. The next three sections discuss the major political institutions that have been proposed as sources of variation in government credibility, legal protection and enforcement, and public good provision. Emphasis is given to (i) the underlying mechanisms linking institutions and development, and (ii) empirical tests validating the proposed relations. Section 4 surveys various historical explanations for the variation in political institutions across countries. The last section concludes by discussing the challenges for future research.

Which political institutions matter for credibility

There is scholarly agreement that institutions that foster credibility in the government's ability to keep its promises promote adequate private investment and public policy-making.¹ Which political institutions foster credible commitments? Recently, emphasis has been put on the role of institutional checks and balances, universal franchise and competitive elections.

Institutional checks and balances

Institutional checks and balances enhance government credibility by limiting the ability of any one political actor to act unilaterally. One such form of institution is a legislative assembly, e.g a parliament. For North and Weingast (1989), a legislative assembly limits the extent to which a ruler can spend and borrow unchecked, enhancing thus the rulers' credibility to commit to sovereign obligations (see also Hoffman and Norberg 1994; Dincecco 2011). Similarly, Boix (2003) argues that legislatures constrain autocrats and can thus promote private investment. Besley and Kudamatsu (2008) argue that the presence of a selectorate able to replace the leader, without losing power itself, can encourage good policy.²

More generally, in this argument what limits the government is the presence of a veto point, the legislative assembly, which can prevent opportunistic policy changes. Also, some argue, the greater the number of veto points, the greater the likelihood that the government is obliged to keep its promises: ex post changes in regulation and policy become even more difficult (Levy and Spiller 1994). Stasavage (2002, 2003) shows, however, that limiting the government by means of veto points is neither a necessary nor a sufficient condition for an increase in the government's ability to commit.³ We must also consider who controls different veto points. A legislative assembly can guarantee the government will pay its debts, say, as long as creditors are well represented within the legislature. Furthermore, with more veto players, more credibility is guaranteed on average as long as preferences over policies are independently distributed across veto players (Stasavage 2002: 161). But if one powerful group, say landowners, control all veto points, government commitment cannot be guaranteed for a policy not in the interest of landowners. If, however, we allow for more than one policy dimension and disciplined political parties, cross-issue bargains between members of the same political party may guarantee commitment even if the majority in the assembly prefers a change in policy.⁴

Gehlbach and Keefer (2012) emphasize another important reason for why institutions such as legislatures are able to foster credibility. By allowing supporters to organize collectively, a leader can enhance his ability to commit to not behave opportunistically. Some autocrats permit institutional arrangements that give their supporters capacity for collective action. These collectively organized groups are able to impose sanctions on the leader through, for example, fund-raising armed resistance, disrupting policy implementation, or withholding resources in the event that any member of the group was expropriated (Gehlbach and Keefer 2012: 622–23). In fact, the argument in North and Weingast (1989) is that in seventeenth century England the legislature allowed the nobility to act collectively against the king.

Another alternative is to delegate bureaucratic functions to the private actors that require guarantees. In doing so, a government ties its own hands by giving private actors the ability to withhold revenue, or otherwise punish the government, if politicians behave opportunistically (Jones 1994; Sargent and Velde 1995; Stasavage 2002). Hénisz (2000b), for example, argues that investors can reduce the impact of institutional country risks by creating joint partnerships, and thus coordinating with other investors.

Relatedly, some have argued that providing independence to certain offices within the government can isolate policy makers from political pressures and thus allow for the implementation of optimal policy. Central bank independence, for example, grants credibility to monetary policy.⁵ Governments unable to commit to controlling the money supply are less likely to hold down inflation (Keefer and Stasavage 2003). Recent empirical evidence, however, has shown that legally mandated central bank independence provides only a weak protection against political pressures whenever the career incentives of central bankers are guided by private banks and the government. Adolph (2013: [ch. 3](#)) shows the financial sector and bureaucratic experience of central bankers affect inflation as much as measures of central bank independence. Also, investments in the capacity to collect taxes can enhance the government's commitment to low inflation. By increasing fiscal reliance on taxation, and correspondingly reducing fiscal reliance on other sources of revenue, the government enhances its ability to commit to avoid seignorage, rent seeking, and corruption (Cukierman *et al.* 1992; Aisen and Veiga 2008).

A variety of measures of institutional checks and balances have been proposed in the literature, and typically have been shown to robustly predict economic growth. Henisz (2000a) constructs an indicator of the number of veto players in a country by considering whether the executive controls the other veto players: the legislature, the judiciary, and state governments (federalism). He then weights the indicator according to the fractionalization, or heterogeneity, of the different veto players' policy preferences. He finds that the measure is a robust predictor of economic growth, and that it predicts measures of security of property rights. Keefer and Knack (2002) link the presence of multiple veto players with measures of credit worthiness. Their analysis finds supporting evidence for a significant positive effect of checks and balances on the credit ratings of a country.

Because checks and balances, universal franchise, and competitive elections are typically analyzed jointly in the empirical literature, I postpone further review of the evidence until the following subsection.

Universal franchise and competitive elections

Competitive elections and universal franchise can also make governments credible by giving those not part of the political elite the means to punish opportunistic behavior. Acemoglu and Robinson (2001, 2006) argue that an expansion of the franchise enables the rich to credibly commit to redistribute to the poor. By making the poor the median voter, an expansion of the franchise allows the rich to credibly promise tax transfers and property rights protection to the (previously) disenfranchised to prevent strikes, riots, or a revolution. According to Acemoglu and Robinson, only in societies with intermediate levels of inequality do the demands of the poor and the low redistributive costs to the elite combine to support an extension of the franchise. Boix (2003) similarly understands democracy as a redistributive struggle between the rich and the poor, but advances a linear relationship between inequality and democratization. This view of democracy, however, leaves some room for debate.⁶

On theoretical grounds, it is not clear why the rich need to provide universal franchise to avoid a threat of revolution. Alternative institutional arrangements, such as limited franchise, can provide security to the median citizen while at the same time safeguarding the elite's control of assets (Keefer 2004: 265). The United States during the nineteenth century, for example, authorized a popular vote for one legislative chamber, while the other chamber remained insulated from the electorate. In this way, the median voter had some ability to constrain the government but institutions still gave the elite veto power to protect their privileges.

In addition, if franchise extension is to increase the government's commitment to redistribution, it should be more likely in unequal societies. Empirical assessments of the link between inequality and franchise extension provide conflicting results, however. Using measures of land inequality, both Boix (2003) and Ansell and Samuels (2010) find a negative relation between inequality and democratization. Yet using income inequality as measure, Ansell and Samuels (2010) find a positive relation between inequality and democratization. Przeworski *et al.* (2000), and others, have failed to find a relationship between different measures of inequality and regime type (Muller 1988; Houle 2009). In part, these conflicting results make manifest the difficulty of measuring inequality and of identifying the causal relationship between inequality and democracy.⁷ All the same, the evidence seems to suggest democratic countries do not have on average less inequality, casting doubt on the argument that democratic institutions function as a means to commit governments to redistribution.⁸

Competitive elections may still make governments more accountable and, in so doing, promote productivity-enhancing investments. Through elections citizens not part of the political elite are able to punish politicians that do not follow through on their campaign promises or otherwise misbehave. The empirical literature linking regime type and economic development, however, provides contradictory evidence.⁹

Rodrik and Wacziarg (2005) find that transitions from authoritarian to democratic regimes do increase income growth per capita. Similarly, Giavazzi and Tabellini (2005) find a positive feedback between economic and political reform, yet the sequence of reforms matters. In groundbreaking work, Przeworski *et al.* (2000) find that higher income countries are more likely to remain democracies. However, they find no significant difference in growth rates between democratic and autocratic regimes. Glaeser *et al.* (2004) show that more developed countries are those that pursue good economic policies, often under dictatorships; democratization may come later. According to Gehlbach and Keefer (2012), countries without formal checks and balances and lacking competitive elections also manage to attract private investment: 40 percent of countries without competitive elections retained more private investment than the median country with competitive elections.

Authoritarian regimes, it seems, are also able to credibly commit under some circumstances. Gehlbach and Keefer (2012) argue that institutional arrangements that provide supporters effective capacity for collective action, grant credibility to autocrats. Two conditions are necessary: (1) information transmission within the group, and (2) coordinated behavior.¹⁰ If, in contrast, a ruler creates competing organizations that report directly to the ruler, information transmission is debilitated and focal points suppressed (e.g. the Red Guard under Mao). Similarly, by exercising tight control over the agenda and group meetings, a leader limits independent initiative and thus hinders coordination among supporters.

Gehlbach and Keefer (2012) provide examples of this type of institutional arrangement under autocracies. Competitive legislative elections provide credibility as long as the support bases of legislators are independent of the autocrat. This arrangement allows the support group of the legislator to act collectively and impose sanctions on the autocrat through the legislator.¹¹ Other institutional arrangements that facilitate collective action are disciplined ruling parties in nondemocracies (e.g. the PRI in Mexico; see Haber *et al.* 2003 and Magaloni 2006) and clear rules for leadership change.

Some scholars argue that long horizons and stable rule provide incentives for authoritarian rulers to not behave opportunistically. According to Olson (1993), stability gives autocrats incentives to promote investment and economic growth even if they are predatory: the larger the pie, the more they can get. McMillan (2003) argues that the communist government under Mao faced no challenges and so was motivated to not renege on contracts.¹² Even autocrats with

long horizons have turned out to be predatory despots, however, so stable rule seems not to be a sufficient condition to ensure an authoritarian ruler is benevolent and promotes economic success.

Although part of the lack of robustness in the empirical evidence is likely due to uncertainty and imperfections in political markets across countries, it is also clear that we need to better understand the conditions under which specific political institutions have different effects on credibility. The wide variation in development outcomes across autocracies suggests we need a more detailed understanding of the institutional variation within dictatorships. This may shed light on other political institutional arrangements (such as those suggested by Gehlbach and Keefer 2012) with the potential to increase government credibility. A greater understanding of the trade-offs and complementarities between public and private institutions is also critical to increase our knowledge of the institutional sources of government credibility.¹³

Property rights and contract enforcement

An important strand of the literature argues that variation across states in their legal institutions helps explain why some are better able than others to commit to protect property rights and enforce the rule of law between private agents, and between private agents and the government.¹⁴ Well-defined and protected property rights incentivize innovative entrepreneurship rather than predation, and diminish the need to protect against predation (Baumol 1990). Contract enforcement prevents parties from failing to honor their contracts. Asymmetric information and hold-up problems can provide incentives for contract renegeing. The parties will choose not to renege only if the cost from doing so is high. Repeated interactions and geographic proximity facilitate punishments in bilateral arrangements and thus reduce the need for formal legal institutions.¹⁵ As markets grow and the geographic location of exchange expands, however, information asymmetries increase together with the probability of renegeing. Formal institutions enforced by a third party (i.e. the government) are then necessary to facilitate exchange and growth (Greif *et al.* 1994; Greif 2006; Brown *et al.* 2004).

Economic historians have been the first to document the importance of formal private property rights and contract enforcement for markets and development. Market-supporting legal institutions have been critical for the rise of capitalism and international trade (North and Thomas 1973) and for the development of the corporation (Rosenberg and Birdzell 1986). Greif (2006) argues that a functioning market economy failed to develop in China due to the absence of formal commercial arrangements.¹⁶

A growing empirical literature provides sharpened measures and channels of causality. Weak legal systems lead to slower firm growth and to pyramidal structures within firms, both with negative implications for innovation and growth. In Mexico, firms in states with a low measure of efficiency of judges (based on the quality, impartiality, and resources of judges, along with other dimensions) are smaller than in states with a high measure (Laeven and Woodruff 2007). Larger firms are more efficient and are able to expand their markets. Khanna and Palepu (2000) document that weak property right protection resulted in pyramidal structures. A pyramidal structure of firms relies more on intra-group resource allocation than free standing firms, reducing aggregate allocative efficiency (Almeida and Wolfenzon 2005), hampering competition,¹⁷ and limiting externalities from innovation (Morck *et al.* 2005).

Formal entry and exit barriers also impact the allocation of resources. Entry barriers in the form of high registration costs hinder the entry of new firms, undermining innovation and competition (Berkowitz and Jackson 2006; Klapper *et al.* 2006; Fisman and Saria-Allende 2010; Bruhn 2011) and induce informality (Djankov *et al.* 2002).¹⁸ Exit barriers in the form

of impartial and quick resolution of insolvencies appear to contribute to a more efficient firm selection: Gine and Love (2010) find that insolvency reform improved the allocation of assets in the Colombian economy. The evidence regarding creditor-friendly insolvency regimes is mixed, however. There is evidence that industries that depend on innovation in countries with stronger creditor rights see fewer patents (Acharya and Subramanian 2009).

Financial sector development has also been associated with legal protections and enforcement.¹⁹ Countries with secure creditor and minority shareholder rights have larger credit and stock markets (La Porta *et al.* 1997), higher intermediation efficiency (Laeven and Majnoni 2005), and higher corporate valuations in the stock exchange (La Porta *et al.* 2002; Caprio *et al.* 2007). Djankov *et al.* (2007) find that the extent to which credit information is shared has an effect on the depth of the financial sector. Also, legal procedures and trial duration can limit access to credit markets. The introduction of new tribunals to resolve large-claim disputes in India resulted in lower interest rates for borrowers (Visaria 2009), while the duration of trials in India is negatively associated with farmers' access to credit markets (Chemin 2009a). Levine and Zervos (1998) and Levine (1998) find that various measures of financial development contribute to economic growth.

In addition, recent work has pointed to the importance of the political process in understanding how legal contracts are written and enforced. Pagano and Volpin (2005) argue that parties in proportional electoral systems favor low investor protection and high labor protection because obtaining a majority of votes is indispensable when competing with other parties. In contrast, in majoritarian electoral systems parties favor high investor and low labor protection because parties need to win more districts (and so the pivotal group becomes important). The authors also provide empirical evidence for the OECD countries in support of their argument. However, many of the changes in corporate governance after the 1930s took place under authoritarian regimes; an explanation based on electoral systems is therefore not sufficient (Musacchio 2010).

For Gourevitch and Shinn (2005), conflicts between firm owners, managers, and labor shape the regulation to protect shareholders. Shareholder protections are stronger and ownership less concentrated in countries in which owners of firms and labor are in dispute with managers. In contrast, when there is greater coordination between these players, ownership is more concentrated and there is higher government intervention.²⁰

In addition to the specific indicators of investor protection mentioned, broader measures of institutional quality have also been linked to development. Besley and Persson (2011), for example, show that an index of property rights protection and an index of contract enforcement are both positively correlated with income.²¹ These broad indices raise two concerns. First, the measures are constructed using expert surveys, which can be biased by the perceptions of each expert. Also, expert surveys may reflect outcomes rather than specific rules or institutional arrangements; importantly, if they reflect economic development outcomes, establishing a link between the index measures and development is problematic.²²

Second, legal institutions impact development, but economic outcomes also influence the evolution of institutions. Concerns about reverse causation and simultaneity are especially important when using broad measures of institutional quality because identifying the various mechanisms that could be at play is tricky. The recent literature has therefore concentrated on identifying the exogenous component of measures of institutional quality in order to address the endogeneity problem. Pioneers in the literature are Hall and Jones (1999), Knack and Keefer (1997), and Mauro (1995). More recently, Acemoglu and Johnson (2005) examine separately the effect of property rights and contract enforcement institutions. As a proxy for property rights, they use risk of government expropriation and constraints on the executive. For contract enforcement

institutions, they use the indicators in Djankov *et al.* (2002, 2003). To handle the endogeneity problems of these measures, Acemoglu and Johnson (2005) further use their settler mortality variable (see Acemoglu *et al.* 2001) as an instrument for property rights institutions, and legal origin as an instrument for contract enforcement institutions. They find strong evidence for the importance of property rights institutions on income per capita. However, once the effect of property rights is accounted for, contract enforcement institutions do not appear to relate to income per capita.²³

In sum, the literature documents the importance of formal private property rights and third-party enforcement for understanding economic development. Also, the literature provides an important contribution towards identifying the mechanisms through which specific legal institutions impact development. Our understanding of the effect of the political process and of political institutions on legal arrangements, however, remains limited. More generally, we need to understand why some countries have more effective institutions than others in order to venture policy changes. More on this in the next sections.

Public good provision and organizational infrastructure

Decision-making rules within legislatures have an impact on regulation and policies. These rules determine who sets the agenda, who has veto power, and how transitions in and out of office take place. The literature typically focuses on clusters of these rules when analyzing the impact of these political rules on policy.

One strand of the literature contrasts presidential and parliamentary systems. Persson and Tabellini (2000) argue that in parliamentary systems the institutional framework gives rise to high public good provision, and correspondingly high rent seeking and high targeted transfers, because the legislature is able to propose a high tax rate. In contrast, under presidential systems, the tax committee sets low taxes, leading to a low public good provision, low rent seeking, and low targeted transfers. The variation is a result of the different abilities to veto proposals between the tax and the spending committees in legislatures.²⁴

The empirical evidence confirms that presidential systems spend less than parliamentary systems (e.g. Persson and Tabellini 2000). However, the evidence is less robust regarding how the allocation of government spending differs across the two systems (Persson and Tabellini 1999). Presidential and parliamentary systems differ among many other dimensions, likely a reason why the findings are inconsistent (Keefer 2005). In addition, Persson and Tabellini (2003) demonstrate that the effect of parliamentarianism and presidentialism on economic policies depends on whether democracies are well established or weak. In well-established democracies, presidential regimes are more growth oriented than parliamentary regimes, while in weak democracies parliamentary regimes are more growth oriented than presidential regimes.

The literature has also focused on the effect of systems of representation on policies. Persson and Tabellini (2000) show that majoritarian systems provide fewer public goods and more targeted goods to specific constituencies than a system of proportional representation. Each system generates different electoral incentives. In proportional systems, representation is determined by the overall distribution of vote shares; in majoritarian systems what matters is the distribution of votes across districts (see also Milesi-Ferretti *et al.* 2002).

Persson and Tabellini (2003) further find that having small districts hinders economic development under majoritarian systems.²⁵ Small districts encourage politicians to satisfy a small rather than large constituency. Further, voters in small districts are able to influence policy more than voters in large districts (Samuels and Snyder 2001). For instance, by giving the same representation in Congress (two senators) to California (the largest state in the U.S.) and to the

smallest U.S. states, voters from smaller states receive more representation. This gives them more ability to influence legislation and in some cases a larger share of non-discretionary redistributive transfers (Lee 1998). Majoritarian rules, however, seem to deter corruption (Persson *et al.* 2003).

In sum, electoral and political rules seem to matter. However, the direction of the effect of different rules on public good provision and growth-promoting policies is not clear. Different countries have different combinations of electoral and political rules. To understand the overall effect we need to better understand the offsetting effects of individual rules.

History, institutions, and economic development

Economic historians have provided the seminal contributions highlighting the importance of institutional differences across societies to explain development. These early contributions also provide theories and evidence about the determinants of those institutional differences. For instance, North and Weingast (1989), mentioned previously, argue that parliamentary institutions provided the grounds for democratic institutions. Greif's (1994) seminal work on the development of market institutions highlights cultural differences. These studies are analytic and qualitative in nature and focus on specific regions and time periods. Yet, the studies provide convincing support for the notion that history and institutions matter.

Building on this work, a growing empirical literature has emerged seeking to identify the causal effect of historic events and institutions on current outcomes. These studies exploit rich datasets and employ careful identification strategies. The literature highlights events such as the onset of colonialism and environmental factors such as soil suitability for certain crops as explanations for the divergent institutional paths of different regions.

Next, I first discuss two factors that have been posited as explanations for institutional variation: factor endowments and legal origins. Second, I explore the mechanisms that have been proposed to link historic institutions to current institutions, and the empirical literature that links historic events and historic institutions to development outcomes.

Resource endowments

The colonial experience has been extensively analyzed to study the determinants of current institutional variation across countries. One of the main factors proposed in the literature is the endowment of resources at contact. The underlying argument is that initial endowment differences in the colonized regions led to the implementation of policies that resulted in "good" or "bad" colonial institutions. The effect of these institutions has persisted and shaped contemporary institutions.

Engerman and Sokoloff (1997, 2002) argue that in regions with the potential for obtaining economically profitable exports, such as soils with the potential for growing large-scale crops (sugar, tobacco, and cotton) or with mineral deposits, colonial practices (e.g. forced labor) led to substantial inequality in wealth and political power. The colonial elite in those regions was able to set up political institutions to perpetuate inequality and grant privileges to themselves (e.g. voting rights). In contrast, in regions with the potential for growing small-scale crops the organization of production resulted in a more equal distribution of wealth and power.²⁶

Engerman and Sokoloff's argument has been subsequently tested empirically in the literature. Various studies have found a negative relationship between colonial slavery and various measures of economic development (e.g. Mitchener and McLean 2003; Nunn 2008). Nunn (2008) also finds a positive relationship between slavery and landholdings inequality; however, he

shows that large landholdings are not correlated with current levels of economic development. The latter result challenges the channel of causality proposed by Engerman and Sokoloff: although slavery is correlated with poor economic development, the relationship does not seem to be driven by large-scale plantation slavery.²⁷ As we will see, we still need to learn more about the channels through which institutions change and persist.

The argument in Acemoglu *et al.* (2001, 2002) is similar to Engerman and Sokoloff's yet emphasizes different factor endowments. Acemoglu *et al.* (2001) argue that environmental conditions that threatened the survival of Europeans, such as environments with potential for malaria and other diseases, discouraged European settlement. Lack of extensive settlement prompted colonists to maximize short-term extraction by relying on forced labor and trade monopolies that benefited the colonial elite. These practices resulted in political institutions that facilitated state extraction and did not foster market development. In contrast, regions where Europeans settled in large numbers, resulted in private property and political institutions that limited the government (Acemoglu *et al.* 2001: 1375).

Acemoglu *et al.* (2002) complement and expand the argument in Acemoglu *et al.* (2001) by incorporating the levels of indigenous population density and prosperity in the regions at contact. In regions sparsely populated by the natives and less prosperous the colonists set up what the authors call *property rights institutions*: "a cluster of (political, economic, and social) institutions ensuring that a broad cross section of society has effective property rights." In contrast, under *extractive institutions*, "the majority of the population faces a high risk of expropriation and holdup by the government, the ruling elite, or other agents" (Acemoglu *et al.* 2002: 1262). Colonists set up extractive institutions in regions where extraction was profitable: those with prosperous native economies and abundant indigenous labor.²⁸

Although a strand of the literature on the effect of resources and geography argues that these have a direct impact on productivity,²⁹ the studies detailed previously defend that the effect of natural endowments is indirect, through their effect on institutional development rather than directly on productivity and growth.³⁰ Other scholars propose alternative mechanisms of transmission. Europeans brought with them other traits that could also explain colonial variation, such as their culture (Landes 1998), human capital (Glaeser *et al.* 2004), and economic ideology (Mahoney 2010). More work focusing on specific institutional elements is needed to better disentangle the effects of geography, institutions, and other transmission variables.

In this direction, Arias and Girod (2014) study the variation in early types of colonial forced labor and argue that two conditions influenced colonists' choice of forced labor: natural resources exploitable in large scale (e.g. minerals) and pre-colonial political organization of labor (labeled *hierarchy* by the authors). In territories with high pre-colonial hierarchy, indigenous forced labor was more likely regardless of natural resources. Foreign forced labor (e.g. African slavery), in contrast, was more likely in territories with low pre-colonial hierarchy *and* abundant natural resource potential. In high-hierarchy territories, co-optation of native leaders facilitated colonist control of indigenous forced labor. However, in regions lacking such organization colonists faced high costs of forcing indigenous peoples to work; doing so was only profitable if resources could be exploited and exported. Original data covering 439 subnational territories in the Americas provides support for the authors' hypotheses. By demonstrating that indigenous forced labor depended on factors other than foreign forced labor, the authors suggest that both pre-existing institutions and natural resources can account for institutional development during colonial rule.

Other institutional variables have been highlighted in the literature as determinants of contemporary institutional variation and economic development; the next subsections explore the literature.

Legal origin

The legal origin of a country seems to be the common denominator of differences across states in the institutions that support markets. The most prevailing legal origins in the world are the English common law and the French civil law. Relevant differences between the two are the reliance on professional judges, a legal code, and written records in civil-law countries, while common-law countries rely on lay judges, broader legal principles, and oral arguments (Glaeser and Shleifer 2002).

La Porta *et al.* (1998) show that common-law countries have stronger laws protecting investor rights and better law enforcement than do civil-law countries. La Porta *et al.* (1999) further show that common-law countries have better quality of business regulation and the top marginal tax rate relative to countries with civil-law origins. Common-law countries have also been found to have courts more efficient at resolving disputes (Djankov *et al.* 2003) and lower costs of establishing a new firm (Djankov *et al.* 2002). Civil-law countries require more procedures to start up a firm and more days to execute those procedures than common-law countries.³¹

Legal origin seems to also explain variation in indicators of judicial independence. La Porta *et al.* (2004) find that in countries with a common-law tradition, judges are stronger and more independent than in countries with a civil-code tradition. Berkowitz and Clay (2005; 2006) find that in U.S. states colonized by countries with civil-law traditions (France or Spain) judges were granted independence later, had access to fewer resources, and have lower-quality courts than those colonized by the English. Others find that a common-law origin gives countries today more flexibility and adaptability in contract enforcement (e.g. Djankov *et al.* 2003). This difference in adaptability appears to explain country variation in financial sector development and firms' reported financial constraints, according to Beck *et al.* (2005).

Musacchio (2010) finds, however, that using alternate measures of financial development for 1900 and 1913 (stock market capitalization to GDP, private credit to GDP, number of companies traded per million people, and bond market capitalization) there is no evidence of significant differences in financial development across common and civil-law countries. Further, Musacchio finds convergence in creditor rights across common-law and civil-law bankruptcy laws circa 1900. Roe and Siegel (2011) also find, after controlling for political instability, no effects of legal origins on measures of financial development.

In sum, legal origin has been linked to various measures of legal institutions,³² yet the lack of robustness in the empirical relationships suggests that we need to better understand the channels of causality. For instance, Berkowitz *et al.* (2002) argue that the process of transplantation of legal institutions plays an important role in determining the functioning of the legal system. Further, much of the literature seems to assume time-invariant legal traditions. Some studies analyze the time variation in legal institutions and suggest that legal institutions vary with the underlying political conditions (e.g. Pagano and Volpin 2005).

Historic institutions and economic development

Institutions impact development, but economic outcomes also influence the evolution of institutions. This endogeneity between political institutions and economic development is the major challenge of the literature. By studying the historic origins of institutions and emphasizing causal effects, a burgeoning strand of the literature is convincingly demonstrating that history and historic institutions have an impact on current economic outcomes. This literature developed from the previously examined work of Engerman and Sokoloff (1997; 2002), Acemoglu *et al.* (2001), and La Porta *et al.* (1998).³³

To identify the causal effect of institutions—extractive versus property rights—on current economic development, Acemoglu *et al.* (2001, 2002) employ the mortality of potential European

settlers (discussed previously) as an instrument for contemporary institutions. They use as proxies for current institutions broad indices of the risk of government expropriation and constraints on the executive. The underlying identification assumption is that settler mortality has no effect on current per capita income, other than through institutional development. The argument posits the persistence of colonial institutions as the mechanism linking history to contemporary development.³⁴ They find a strong positive effect of current institutions on per capita income.³⁵

Subsequent studies have focused on more specific historic institutions. Banerjee and Iyer (2005) focus on land revenue collection institutions in colonial India, contrasting the districts where land taxes were collected by British officials (non-landlord system) to those where collection was in the hands of native landlords (landlord system). The authors convincingly show that post-independence levels of education, health, and agricultural investment were higher in landlord than in non-landlord districts.³⁶ Dell (2010) studies the *mita*, a forced-labor institution inherited from the pre-colonial era and used by Spanish colonists in Peru and Bolivia until 1812. She finds that today former *mita* districts have political institutions that concentrate wealth and power and provide few public goods, while political institutions in non-*mita* districts result in higher public good provision. The average level of contemporary household consumption in former *mita* districts is 32 percent lower than in non-*mita* districts.

Jha (2013) examines the origin of institutions promoting religious tolerance and supporting markets across cities in contemporary India. He argues that towns where overseas trade with the Middle East flourished in early medieval times, created institutions supporting cooperation between Hindus and Muslims, and those institutions have persisted. Jha is able to identify the causal effect of historic medieval trade on current outcomes by using medieval natural harbors as exogenous determinants of cities with high overseas medieval trade. Nunn and Wantchekon (2011) study the effect of the slave trade on mistrust in Africa. They find a negative correlation between the number of slaves taken from an individual's ethnic group during the slave trade and the individual's reported trust in others. To confirm whether the relation is causal, they instrument slave exports with the ethnic group's historic distance from the coast.

A limitation of these studies is that they fail to identify the channel or mechanism through which institutions evolve or persist.³⁷ Identifying empirical relationships may not be enough. We need to better identify explicit arguments for why institutions do or do not change. If there are underlying conditions leading to different political institutions, those conditions may be the cause of variations in outcomes, and not the institutions themselves.

Some recent work is starting to shed light on precise mechanisms of institutional persistence and change. To explain institutional persistence, Nunn (2007) argues that African governments govern within the same equilibrium that emerged when colonists built extractive institutions. Acemoglu and Robinson (2008) argue that political power allows elites to preserve the economic institutions that serve their narrow interests. Arias and Girod (2014) document that certain forms of pre-colonial institutional elements (high hierarchy) are more self-enforcing, and therefore persistent, than others (low hierarchy).³⁸ Spolaore and Wacziarg (2013) show that the persistence of institutions appears to be at the level of populations, not locations. If so, inter-generationally transmitted traits may constitute barriers to the diffusion of technology and institutional knowledge across locations.

Challenges for the future

Despite the conflicting evidence and at times contradictory theoretical arguments, the consensus seems to be that political institutions matter. We have identified that institutions guaranteeing private property and contract enforcement are necessary for market development; that government

credibility is important to incentivize investment and innovation, although we need to learn more about the specific institutional arrangements that increase credibility; and that political decision-making has an impact on public good provision. In addition, the historic literature has contributed to our understanding of the determinants of institutions, and has convincingly shown that history and historic institutions have an effect on current development outcomes.

To make further progress, we need a richer understanding of the mechanisms through which specific institutions affect various dimensions of economic outcomes (e.g. investment, innovation, labor-capital conflicts, coordination) and of the channels through which long-term variables affect current institutions and outcomes. This is of foremost importance to be able to derive policy implications and assess the impact of current and future policies.

In this direction, country-level studies focusing on specific institutions at specific times and places can provide institutional richness and context-specificity that allow for a better understanding of the mechanisms of causality.³⁹ Rodrik (2003) argues that the success of specific institutions depends on context-specific characteristics that cannot be easily replicated across countries. Fafchamps (2004) suggests different policy prescriptions for three different African countries after undertaking an individual game-theoretic analysis for each. De Soto (2004) emphasizes the importance of building on local knowledge to understand how institutions work in different locations.

Further, a theory of institutional change and persistence is necessary if we want to undertake institutional engineering. Some argue that the opportunities for institutional change must be taken advantage of quickly; gradualism prolongs the costs that need to be borne by some in society as a result of reform. Others favor caution; they underscore that drastic reform can increase the risk of political opposition, risking failure. However, Heybey and Murrell (1999) conclude that a country's initial conditions are more important than whether reform is rapid or gradual. Lacking theories and empirical evidence for why institutions do or do not change, we can not know whether what explains variation in development outcomes is the underlying conditions that determine political institutions, the process of institutional reform or, rather, the institutions themselves.

Future work needs to engage with the mechanisms through which specific political institutions impact various dimensions of economic development. Without a detailed specification of particular institutional arrangements and a better understanding of the mechanisms that lead to the creation, persistence, and change of specific institutions our knowledge of the role of institutions in economic development will remain limited.

Notes

- 1 Credibility is distinct from other effects of institutional arrangements, namely stability or predictability. A stable policy outcome need not be credible. A policy is stable when the set of policies preferred by the median voter (or median veto player) to the status quo is small. Credibility, in contrast, refers to the incentives of politicians to change policy opportunistically tomorrow for a given reliance on government promises today. See Tsebelis (1995, 2002) for a thorough examination of the stability effects of different institutional arrangements. Bardhan, (2005: [ch. 4](#)) offers a detailed discussion of the concept of credibility.
- 2 Gandhi and Przeworski (2006) and Gandhi (2008) also emphasize the positive effect of legislatures on improving economic performance. However, their argument is not about the role of legislatures in enhancing credibility. Rather, they suggest legislatures allow for co-optation of the opposition.
- 3 Ertman (1997) and Cox (2011) have also expanded North and Weingast's argument and included qualifications.
- 4 See also Aldrich (1995), Huber (1996) and Diermeier and Feddersen (1998).

- 5 Some argue that while institutional constraints can significantly reduce discretionary behavior, these institutions come at a cost in flexibility. The inability to change the status quo in response to external shocks, or to correct policy mistakes, must be weighted against the benefits of constraining the executive. See, for instance, Walsh (2010) and Alesina (1995). The literature seems to assume that, on average, the benefits outweigh the costs.
- 6 Both Boix (2003) and Acemoglu and Robinson (2006) echo Meltzer and Richard (1981), for whom the key source of political conflict is taxation.
- 7 Alquist and Wibbels (2012) use a novel empirical strategy to identify exogenous shocks to a country's distribution of income across factors of production. Using data covering 130 years and more than 100 countries, and accounting for spatial dependence of regime types, they find no evidence that domestic inequality relates to regime outcomes. Their measure of inequality is the 90/10 income ratio.
- 8 There is a large literature on inequality and regime type. See, for instance, Beramendi and Anderson (2008).
- 9 The literature for the most part measures democracy as a composite index of the extent to which a country has checks and balances, competitive elections, and universal franchise.
- 10 In this sense, the argument is similar to that in Greif (1993) and Greif *et al.* (1994).
- 11 Boix and Slovik (2013) also emphasize the informational role of legislatures.
- 12 Qian (2003) and Rodrik *et al.* (2004) argue, in contrast, that it was the local governments that had incentives to ensure the prosperity of the new enterprises.
- 13 See, for instance, Allen and Qian (2009).
- 14 Greif (2005) distinguishes between contract enforcement institutions and coercion-constraining institutions; the former regulate interactions among private parties while the latter regulate interactions between private parties and the government. This section discusses both.
- 15 There is a large literature on the role of reputation in supporting transactions through informal bilateral and multilateral arrangements. See for instance: Greif (2006) and Dixit (2007).
- 16 Smith (1776) and Hayek (1960) already pointed to the importance of the protection of property rights.
- 17 Chemin (2009b) shows that better judicial training for judges resulted in new firm entry in the real sector in Pakistan, for instance.
- 18 Friedman *et al.* (2000) show that more general measures of property rights protection and contract enforcement explain variation in informality across countries as well.
- 19 See Beck and Levine (2005) for a review.
- 20 Roe (2003) also highlights the importance of labor groups in the political system and in the corporation.
- 21 Besley and Persson (2011) also show a strong positive correlation between fiscal capacity, measured by the ratio of tax revenue to GDP, and their legal measures.
- 22 See Beck (2010) for a thorough description of legal institutions and discussion of the measures used in the literature.
- 23 Brunt (2007) finds a similar result in South Africa: improvements in agricultural productivity and output in the early nineteenth century resulted from changes in the definition of property rights but not from changes in contract enforcement.
- 24 See also Cox and McCubbins (2001) and Diermeier and Feddersen (1998).
- 25 Persson and Tabellini (1999) also find that countries with low district magnitudes provide fewer broad public goods (e.g. education).
- 26 Bruhn and Gallego (2012) provide a related argument emphasizing the effect of "good" and "bad" colonial economic activities on contemporary political representation and ethnic composition.
- 27 Acemoglu *et al.* (2009) also find a relationship counter to the Engerman and Sokoloff argument. They analyze Cundinamarca, Colombia, and show that land inequality in the nineteenth century is positively related to current measures of primary- and secondary-school enrollment rates. In addition, they find that economic inequality and political inequality are negatively correlated (see also Galor *et al.* 2009; Dell 2010).
- 28 There is a large related literature on resource windfalls that also links natural resources to variation in current political institutions. Resource wealth stimulates corruption among politicians (Mauro 1995; Ales and Di Tella 1999; Vicente 2010; Brollo *et al.* 2013), rent seeking (Ross 2001a, b), and conflict (Fearon 2005). By driving resources away from productive activities, a windfall of natural resources erodes property rights and democratic institutions. Hodler (2006) finds that the erosion of property rights is exacerbated in the presence of many rival factions and substantial resource revenues. There are, however, countries that have benefited from natural resources wealth (e.g. Norway). Recent literature

- suggests that the effect of a positive resource shock depends on the institutional quality of the country (Mehlum *et al.* 2006; Collier and Hoeffler 2009; Bhattacharyya and Hodler 2010). For a review of the literature on natural resources and economic development see Van der Ploeg (2011).
- 29 See, for instance, Sachs (2000, 2001).
 - 30 See also Rodrik (2003); Rodrik *et al.* (2004). But see Michalopoulos and Papaioannou (2013) for recent evidence finding little effect of national institutions on economic performance in Africa.
 - 31 See also Rajan and Zingales (2003); Beck *et al.* (2003a, b); Djankov *et al.* (2007).
 - 32 Legal origin has also been linked to broader societal characteristics; for an overview see La Porta *et al.* (2008).
 - 33 For a careful review of these articles and the literature see Nunn (2009).
 - 34 In doing so, the argument implies a reversal of fortunes because prosperous regions received “extractive” institutions, which persisted and eventually hindered investment and capital accumulation, resulting in poor institutions today, and thus poor economic development. The property rights institutions implanted in previously less prosperous regions persisted, allowing for higher levels of investment and economic development today.
 - 35 Albouy (2006) reassesses the empirical strategy in Acemoglu, Johnson and Robinson by providing a critical examination of their settler mortality measure. See Acemoglu *et al.* (2012) for a response.
 - 36 Berger (2009) also exploits differences between regions in the form of taxation institutions to show a persistent effect of colonial institutions in Nigeria. Gennaioli and Rainer (2007) find a positive correlation between a measure of the level of complexity of pre-colonial African states and contemporary public good provision.
 - 37 Banerjee and Iyer (2005: 1208–9) provide a tentative explanation for why colonial revenue collection systems in India had an impact on political institutions after independence. Greater cooperation among residents of districts with landlord systems may have led to responsive local governments that invested more in infrastructure and development. In contrast, non-landlord districts inherited land conflicts that translated in local governments spending more on land reform rather than on development.
 - 38 See also Greif and Laitin (2004) and Mahoney and Thelen (2010).
 - 39 See, for instance, Greif (2006).

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