THE ECONOMIC EFFECTS OF CONSTITUTIONS

What do the data say?*

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To Our Mothers

Preface

This book is intended for the scholar or graduate student who wants to learn about a new topic of research: the effects of constitutional rules on economic policymaking and performance. We draw on existing knowledge in several fields: economics, political science, and statistics. In particular, the book builds on theoretical work from the last few years, and it forms a natural sequel to our previous book, *Political Economics*: *Explaining Economic Policy*, published by MIT Press in the year 2000. While the previous volume focused mainly on theory, the purpose of this new book is uncompromisingly empirical. Taking the existing theoretical work in comparative politics and political economics as a point of departure, we ask which theoretical results are supported and contradicted by the data, and try to identify new empirical patterns for a next round of theory.

The empirical results we present in the book go beyond those in our recent articles and working papers on the same general topic. But there are other reasons why the entire thing is greater than the sum of its parts. We take advantage of the book format to present a more thorough discussion of measurement and methodology than is possible in a single paper. In the end, the empirical picture stands out quite clearly and convincingly, when considering a number of related issues with a similar methodology.

Our decision to embark on the empirical research program resulting in the book was taken when one of us (Tabellini) gave the Munich Lectures, hosted by CES, in November 1999. At that point, we had produced several theoretical studies of constitutional rules and economic policy, but we had only started to look at the data and our empirical results were still preliminary. The comments received from the Munich audience, and in particular from Hans-Werner Sinn and Vito Tanzi, were an essential input and inspiration for the research that followed. The warm hospitality and the outstanding atmosphere of excitement and enthusiasm at CES made those lectures a particularly memorable event.

Another event that helped focus our minds, when this project was further under way, was the Walras-Bowley Lecture, given by one of us (Persson) at the Econometric Society World Congress in Seattle, August 2000. On this occasion, as well, we obtained important feedback that led to major improvements in our research.

After having completed a first full draft, in May 2002, we had the opportunity to present overviews of the manuscript to different audiences in Uppsala, Princeton, Harvard, the European Science Days in Steyr, Austria, and the Yrjö Jahnsson Foundation, in Helsinki.

At these presentations, and at numerous seminars on the underlying research papers, many colleagues made insightful comments that improved the quality of our research. Here, we particularly want to thank our colleagues who generously gave up their time for reading and commenting on the first draft: Jim Alt, Tim Besley, Robin Burgess, Jon Faust, Jeff Frieden, Emanuel Kohlscheen, Per Molander, Olof Petersson, Per Pettersson-Lidbom, Gérard Roland, Ludger Schuknecht, Rolf Strauch, David Strömberg, Jakob Svensson, and three anony-

Contents

Chapter 1

Introduction and overview

Since the 1990s, constitutional reforms are the subject of heated debate in many democracies. Debate has already led to a number of important reforms. Among the industrial countries, Italy left its former reliance on full proportional representation (PR), introducing a first-past-the-post system for 75% of the seats in the National Assembly. Similarly, New Zealand introduced a mixed PR-plurality system, but from the opposite point of departure – the traditional British system of appointing all lawmakers by plurality rule in one-member constituencies. Japan also renounced its special form of plurality voting in favor of a mixed system. In Latin America, Bolivia, Ecuador and Venezuela undertook large-scale electoral reform in the 1990s, as did Fiji and the Philippines.

Other reforms are still under debate. In the UK, discussions about switching to a mixed or proportional electoral system have resurfaced. In Italy, key political leaders consider proposals of injecting elements of presidentialism or semi-presidentialism into the current parliamentary regime, while in France some commentators would like to go the other way, towards more parliamentarism. Alternative ideas of how to address inefficient decision-making and the "democratic deficit" in the European Union, involve constitutional reforms introducing clearer principles of either parliamentary or presidential democracy at the European level.

These constitutional discussions often concern the alleged effects of constitutional reforms on economic policy and economic performance.[1] Is it

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[1] The contributions in Shugart and Wattenberg (2001) discuss the motives behind, and the political consequences of, reform in these and other countries adopting mixed electoral

true that a move towards more majoritarian elections would stifle corruption among politicians, as presumed by the vast majority of Italians who approved the electoral reform? Would it also reduce the propensity of Italian governments to run budget deficits? If the UK were to abandon its current first-past-the-post system in favor of proportional elections, would this change the size of overall government spending or the welfare state? Can we really blame the poor and volatile economic performance of many countries in Latin America on their presidential form of government? More generally, what are the economic effects of constitutional reforms? Knowing the answers to these types of questions is important not only for established democracies contemplating reform, but also for new democracies designing their constitutions more from scratch.

The goal of this book is to contribute to the body of empirical knowledge about these very difficult, yet fundamental, issues.

1.1   Constitutions and policy: a missing link

Surprising as it may seem, social scientists have not really addressed the question of constitutional effects on economic policy and economic performance, until very recently. In fact, some observers have even gone as far as deeming it impossible to predict the consequences of constitutional reforms (Elster and Slagstad, 1988). But this is clearly an extreme position. Analyzing the effects of alternative constitutions is a main research topic in political science, as exemplified by the contributions of Sartori (1994), Bingham Powell (1982), Lijphart (1984), to name just a few. Yet, despite this long and honored tradition, little is known empirically about the economic effects of alternative constitutions.

To see why, consider the stylized view of the democratic policymaking process in *Figure 1.1.* Citizens and groups in society have conflicting preferences over economic policy. Political institutions aggregate these preferences into specific political outcomes and these in turn induce public-policy decisions in the economic domain (the arrows on the right in the figure). Public policies interact with markets and influence the prices of different goods, employment and remunerations in different sectors of the economy, and these market outcomes feed back into policy preferences (the arrows on the left). In this view of the interaction between politics and economics, the formal

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systems in the 1990s.

rules of the constitution influence political decisions over economic policy, given some distribution of (primitive) preferences over economic outcomes in the population. Our goal is to learn more about the effects of these formal constitutional rules on specific economic policies.

Figure 1.1 about here

The box on the right-hand side of *Figure 1.1* is the domain of traditional comparative politics. Political scientists in this field of research have spent decades working on the fundamental features of constitutions and their political effects. Apart from a few recent exceptions mentioned below and in Chapter 2, however, this research does not reach beyond political phenomena: how different electoral systems affect the number of parties or the incidence of coalition governments, how the form of government affects the frequency of government crises and political instability, and so on. In terms of *Figure 1.1*, the political-science research on constitutions has remained within the confines of the box to the right, dealing with the link between constitutional rules and political outcomes. Yet, the conclusions often point squarely in the continuation of this arrow, i.e., towards an investigation of systematic policy consequences. For example, the comparative politics literature portrays the choice between majoritarian and proportional elections, as a trade-off between accountability and representation.[2] It is plausible that this choice will be reflected in observable economic-policy consequences: better accountability might show up in less corruption, and broader representation in more comprehensive social-insurance programs. A few political scientists have recently asked the empirical "so-what" question of how constitutional rules influence economic policy. Largely based on simple correlations in relatively small data sets of developed democracies, these studies have not come up with clear-cut evidence of a mapping from electoral rules, or forms of government, to policy outcomes.[3]

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[2] The recent book by Bingham Powell (2000), for example, makes this point very clearly and thoroughly.

[3] Lijphart (1999) asks a so-what question about some macroeconomic outcomes, including budget deficits, in different democracies classified largely by their electoral rules. Using mainly bivariate correlations in a sample of 36 countries, he finds few systematic effects. Castles (1998) studies possible determinants of economic policy, including the size of government and the welfare state in 21 developed OECD democracies. One of the determinants is an institutional indicator, mixing five different constitutional provisions,

It is not fair to say that all research in political science has remained inside the box on the right hand side of the figure. Another substantial political-science literature relates economic policy to political outcomes, such as party structure or political instability. But these political outcomes are typically taken as the starting point of the analysis and they are not explicitly linked to specific constitutional features. This can be illustrated as a study of the arrow from "Political outcomes" to "Policy decisions" in *Figure 1.1.* Since the political outcomes are indeed systematically related to the constitutional rules we study in this book (electoral rules, e.g., help shape the party structure), this research is also relevant for our main question and we discuss it further in Chapter 2.

The box on the left-hand side of *Figure 1.1* is the domain of traditional economics. Economists in the field of political economics have tried to escape from this box, devoting their attention to the other issues illustrated in *Figure 1.1.* They have asked how economic policy interacts with markets to shape the policy preferences of specific individuals and groups, and how the distribution of those preferences in turn induce economic policy outcomes and performance. Until very recently, however, this literature portrayed the aggregation of policy preferences in simple games of electoral competition, or lobbying, devoid of institutional detail.[4] Thus, the literature on political economics mainly focused the attention on the remaining parts of *Figure 1.1*, while treating the box on the right-hand side as a black box. As a result, this research as well has generated few predictions, let alone empirical tests, about how constitutional features influence economic policy outcomes.[5] Once more, asking this so-what question is a logical next step.

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including the rules for elections and the form of government (see Chapter 2). Castles finds little effect of this indicator, once more, mostly on the basis of bivariate analysis.

[4]Recent textbook treatments of this literature can be found in Drazen (2000a), Grossman and Helpman (2001) and Persson and Tabellini (2000a). We also refer to some of the relevant contributions in Chapter 2.

[5]This statement is misleading when it comes to the constitutional rules regulating the degree of decentralization to lower levels of government, and some specific rules, such as budgetary procedures, both of which have been the subject of extensive and influential empirical and theoretical work by economists. The traditional literature on Public Choice concentrated precisely on issues of constitutional economics (cf. Buchanan and Tullock, 1962, Brennan and Buchanan, 1980, Mueller 1996). But this literature is mostly normative and did not lead to a careful empirical analysis of the economic effects of alternative constitutional features, with the main exception of a few interesting papers on referenda (e.g., Pommerehne and Frey, 1978).

To sum up, the question about the constitutional effects on economic policy is an example of interesting research topics falling in between existing disciplines and research traditions. The main motivation for writing this book is precisely to fill that void between the stools of economics and political science.

1.2 Which constitutional rules and policies?

The general question of constitutional effects on economic policies is still far too wide for a single book. We narrow it down by just considering a few constitutional features and areas of policy, and by almost exclusively focusing on empirical evidence rather than theoretical modeling. Thus, we limit our attention to two broad aspects of the constitution: the rules for elections and the form of government. On the policy side, we consider different aspects of fiscal policy, political rents taking the form of corruption and abuse of power, and structural policies fostering economic development. Moreover, we focus exclusively on the direct (or reduced-form) link between constitutions and policies, neglecting the intermediate causal effects of the constitution on political outcomes, and from these onto economic policies.

Why these specific constitutional provisions and policies? An obvious reason is that a small recent theoretical literature has dealt precisely with the link between some of them. This literature has generated a number of specific predictions, which suits our empirical purpose. In that sense, we are looking for the key under the street-lamp. But our theoretical street-lamp shines on pretty interesting pieces of ground.

First, electoral rules and legislative rules associated with different forms of government are the most fundamental constitutional rules in modern representative democracies. Voters delegate policy choices to political representatives in general elections, but how well their policy preferences get represented and whether they manage to "throw the rascals out" hinge on the rules for election as well as the rules for approving and executing legislation. Politicians make policy choices, but their specific electoral incentives and powers to propose, amend, veto and enact economic polices hinge on the rules for election, legislation and execution. Electoral rules and forms of government are also the constitutional features that have attracted most attention from researchers in comparative politics. We thus have a solid body of work to rely upon when it comes to measuring and identifying the critical aspects of

these political institutions in existing democracies.

Second, the chosen areas of policy and performance display a great deal of variation in observed outcomes. If we look across countries in the late 1990s, total government spending as a fraction of GDP stood around 60 % in Sweden, and well above 50% in many countries of continental Europe, but around 35% in Japan, Switzerland, and the US. We also see striking variations in the composition of spending: transfers are high in Europe, but low in Latin America; among the 15 members of the European Union, spending on the unemployed in the 1990s ranged from 2% of total spending (Italy) to 17% (Ireland). Perceptions of corruption and ineffectiveness in the provision of government services are generally higher in Africa and Latin America than in the OECD, but still differ a great deal among countries at comparable levels of economic development. Output per worker and total factor productivity vary enormously across countries, reflecting the wide gaps in living standards not only across the world, but also within the same continents.

Looking instead across time in the last 40 years, we see some common patterns in the data. Average government spending, in a large group of countries, grew by about 10% of GDP from the early 1960s to the mid 1980s, to stabilize around a new higher level towards the end of the century. Budget deficits were, on average, below 2% of GDP in the early 1960s and the late 1990s, but reached 5% of GDP in the early 1980s. In spite of such common trends, however, we observe substantial differences in the time paths of individual countries.

As we shall see later in the book, considerable differences remain, even as we take into account the level of development, population structure, and many other observable country characteristics. Hence, it interesting and plausible to explore whether some of the remaining variation can be attributed to different political systems. This is what we do in the rest of the book.

But we are not just interested in finding nice correlations in the data. Our ultimate goal is to draw conclusions about the causal effects of the constitution on specific policy outcomes. In the end, we would like to answer questions like the following. If the UK were to switch its electoral rule from majoritarian to proportional, what would this do to the size of its welfare state, or its budget deficit? If Argentina were to abandon its presidential regime in favor of a parliamentary form of government, would this help the adoption of sound policies towards economic development? That is, we would like to answer questions about hypothetical counterfactual experiments of

constitutional reform.

It goes without saying that this is a very ambitious goal. Drawing inference about causal effects from cross-country comparisons is a treacherous exercise and much of the book revolves around the question of how to draw robust inference about causation from observed patterns in the data. But we are not groping in the dark. A large and sophisticated econometric literature has dealt with exactly this difficulty, how to use observed correlations for inference about causation. So far, the main applications of this econometric literature have been in applied microeconomics. One of the contributions of this book is to bring these techniques into the field of comparative politics, in an attempt to discover some economic effects of political constitutions.

1.3 Overview of the book

We finish this brief introductory chapter by sketching the broad plan of our campaign. Chapter 2 provides a starting point by describing a small and recent theoretical literature by economists on the link between constitutions and policy outcomes. As the book focuses on empirical evidence, we keep this discussion brief and non-formal, mainly summarizing the testable predictions of the theory. The chapter also comments on other non-formalized, but related, ideas in the political-science and economics literatures, as well as some possible extensions of existing theory. It ends with a list of empirical questions, some taking the form of well-defined testable hypotheses, others really amounting to quests for systematic patterns in the data. This list sets the agenda for the empirical work to follow.

Given our big-picture questions, the most interesting constitutional variation is observed among different countries. We have assembled two different cross-country data sets for our purpose. One takes the form of a pure cross section, measuring average outcomes in the 1990s for 85 democracies. The other has a panel structure, measuring annual outcomes from 1960 to 1998 for 60 democracies. Chapter 3 presents the bulk of these data. Specifically, it describes our measures of the size and composition of government spending, budget deficits, political rents, structural policies and productivity – an ultimate measure of economic performance. This chapter also introduces our data on many other cross-country characteristics which we need to hold constant in the empirical work to follow. We show how these characteristics are correlated with the policy and performance outcomes, both across countries

and time.

Chapter 4 describes our empirical measures of electoral rules and forms of government. As the theory in Chapter 2 refers to collective decision-making in democratic societies, we first describe how to restrict our two data sets to countries and years of democratic governance. We then introduce an overall classification of electoral rules into majoritarian, mixed and proportional, as well some continuous measures of the finer details of these rules. Similarly, we provide an empirical classification of countries into presidential and parliamentary forms of government. Discussing the history of current constitutional rules, we find deep constitutional reforms to be a very rare phenomenon among democracies. We also uncover a systematic, non-random selection into different constitutional rules, based on observable historical, geographical and cultural country characteristics.

The rarity of constitutional reforms implies that any direct constitutional effect on policy must be estimated from the cross-sectional variation in the data. But the non-random selection means that we risk confounding the causal effects of constitutions with other, fixed country characteristics. Chapter 5 discusses the statistical pitfalls in estimating the causal effect of constitutional reforms from cross-country data under these circumstances and introduces a number of econometric methods that might help us circumvent them. While the discussion is cast in the context of our particular problem, this is mainly a methodological chapter. Some of the traditional methods we discuss (such as linear regression, instrumental variables, and adjustment for selection bias) are probably well known to many of our readers. Other, quasi-experimental methods (such as propensity-score matching) are more new.

Chapter 6 presents a first set of empirical results. Here, we apply the econometric methods from the previous chapter and estimate constitutional effects on fiscal policy, exploiting the cross-sectional variation in the data. For most of our policy measures, we obtain constitutional effects robust to the specification and estimation method. Presidential regimes have smaller governments than parliamentary regimes. Majoritarian elections induce smaller governments, less welfare-state spending and smaller deficits than do proportional elections. Many of the effects expected from theory also appear to exist in practice. Moreover, some of them are not only statistically significant but quantitatively very important.

Chapter 7 presents another set of results, on the constitutional effects on political rents, growth-promoting policies and productivity, once more esti-

mated from the cross-sectional variation in the data. Lower barriers to entry for new candidates or parties (measured by the number of legislators elected in each district) and more direct individual accountability of political candidates to voters both lead to less corruption and greater effectiveness in the provision of government services, while the crude classifications of electoral rules and forms of government is less important. The same detailed features of electoral rules promote better growth-promoting policies and higher productivity. Finally, parliamentary forms of government and older democracies seem to have better growth-promoting policies, but we also uncover some subtle interactions between the form of government and the overall quality of democratic institutions. As in Chapter 6, these effects are both statistically and economically significant.

Chapter 8 exploits the time variation in our panel data on fiscal policy. Due to the inertia in constitutional features, we cannot use institutional reforms to estimate any direct constitutional effects. We thus pose a somewhat different question, focusing on the interaction between (mainly fixed) constitutions and time-varying policies. Are different constitutional rules systematically associated with different responses to important economic and political events? We discover that the cyclical adjustment of spending and taxes differs crucially, depending on the form of government. Presidential democracies have a slower growth of government spending than parliamentary democracies until the early 1980s, with less inertia and less cyclical response of spending. Proportional and parliamentary democracies alone display a ratchet effect in spending, with government outlays as a percentage of GDP rising in recessions, but not reverting in booms. All countries cut taxes in election years, but other aspects of electoral cycles are highly dependent on the constitution. Presidential regimes postpone fiscal contractions until after the elections, while parliamentary regimes do not; welfare-state programs are expanded in the proximity of elections, but only in democracies with proportional elections.

Finally, Chapter 9 takes stock of our findings. While most of the results are clearly consistent with theory, others are not, and we speculate on the reasons for success and failure. Several of our estimates uncover new, and sometimes unexpected, patterns in the data. These results suggest further extensions of existing theory, as well as additional measurement to create new data sets. Based on our discoveries, we argue that the next round of work in the comparative politics of policy making should be both theoretical and empirical. In that endeavour, it should attempt to integrate the policy-

making incentives emphasized by economists with the political mechanisms emphasized by political scientists regarding party structure and government formation.

Figure 1.1
The democratic policymaking process
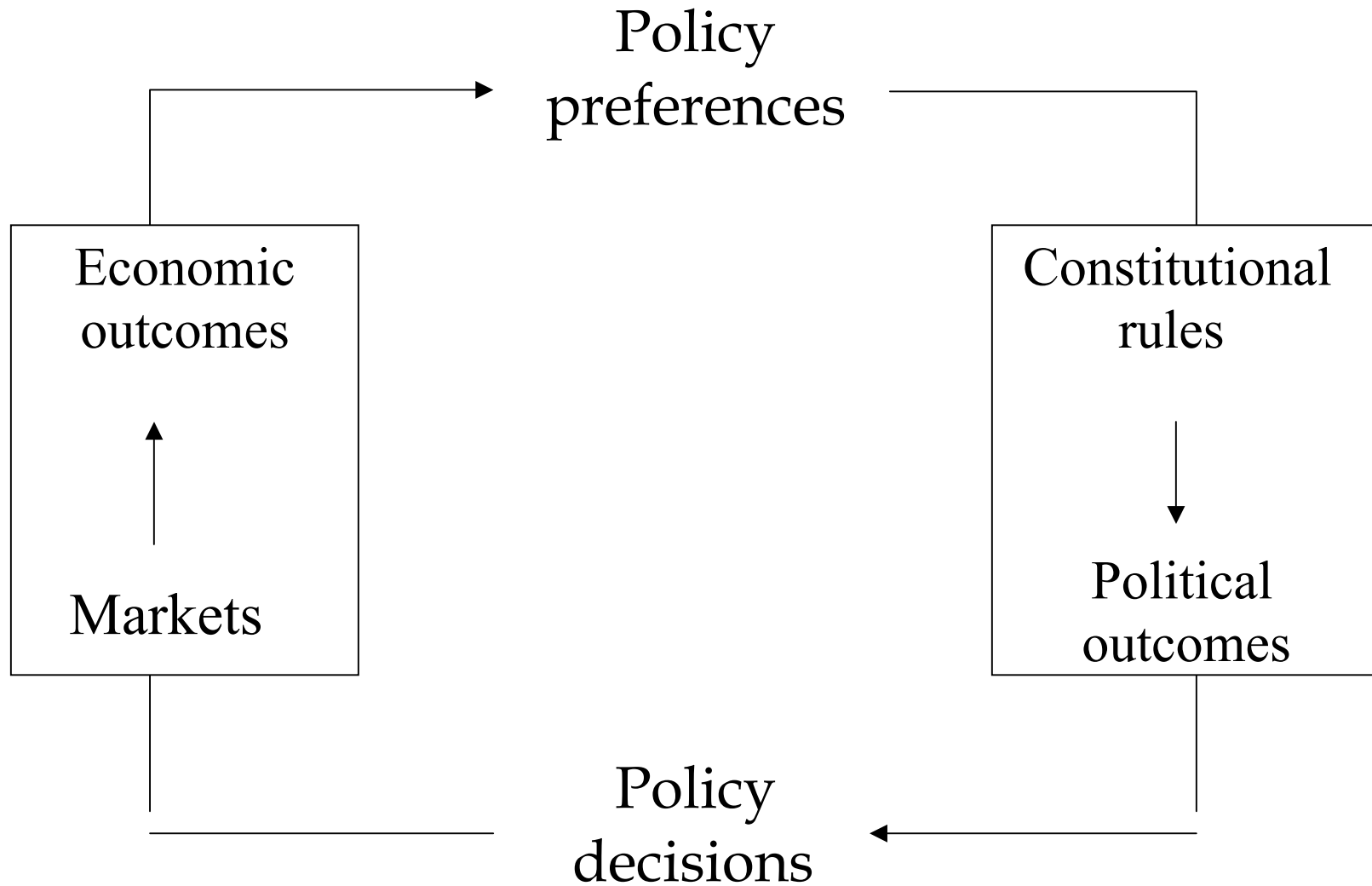
Chapter 2

What does theory say?

2.1 Introduction

Economic policymaking generates conflicts in different dimensions: among different groups of voters, among different groups of politicians, and between voters and politicians. The basic idea in the literature discussed in this chapter is that the resolution of these conflicts – and, therefore, the policy outcomes we observe – hinges on the political institutions in place.

At a general level, this idea is familiar to economists and has an analogy in microeconomic theory. Markets generate conflicts of interest between consumers and producers over price and product quality, and among different producers over profit. How these are then resolved depends on market institutions. Equilibrium prices, qualities and profits hinge on regulations, determining the barriers to entry and the scope for competition between producers. They also hinge on legislation, determining how easily consumers can hold producers accountable for bad product quality or collusive pricing behavior. The basic idea in the literature on political institutions and economic policy is similar.

Political institutions is a label that has been attached to a wide range of different phenomena: from written constitutions, via organizations like political parties or trade unions, all the way to existing social norms. In this book, we only investigate formal rules, as laid down by explicit constitutional provisions. Moreover, as anticipated in Chapter 1, we concentrate on two fundamental aspects of the constitution: electoral rules and forms of government. The former determine how the voters' preferences are aggregated

17

and how the powers to make decisions over economic policy are acquired by political representatives; the latter determine how these powers can be exercised once in office, and how conflicts among elected representatives can be resolved.

Three distinct, but related, lines of research have compared alternative electoral rules and forms of government and their consequences. The oldest and most established tradition is comparative politics, one of the main fields in political science. Researchers in comparative politics have focused on the *political* consequences of alternative constitutions, for instance the number of parties, the emergence of political extremism, and the frequency of political crisis. A basic insight of this line of research is that alternative constitutional features entail different combinations of two desirable attributes of a political system: accountability and representativeness. Accountability means that it is possible for the voters to identify who is responsible for policy decisions and to oust officeholders whose performance they find deficient. Representativeness means that policy decisions reflect the preferences of a large spectrum of voters. The tradeoff between accountability and representativeness is very stark in the case of electoral rules: plurality rule is geared towards holding politicians accountable and PR (proportional representation) towards representing different voters in the legislative process. But a similar tradeoff also emerges in the evaluation of alternative forms of government, even though the distinctions are then more subtle. A presidential regime leans towards accountability, because it concentrates the executive powers in a single office directly accountable to voters and provides checks and balances by a clear separation of executive and legislative prerogatives. A parliamentary regime instead leans towards representativeness, since the government represents and must hold together a possibly heterogeneous coalition in the legislature. Research in comparative politics is so extensive and well known that we do not attempt to summarize its main contributions.[1]

A second, very recent, line of research has exploited the insights of the comparative politics tradition, to ask how electoral rules and forms of government shape *economic policy outcomes*. If alternative constitutional features have relevant implications for accountability and representativeness, this is likely to be reflected in the economic policy decisions emanating from the po-

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[1]Classics within the political science literature on comparative politics from the last two decades include Powell jr. (1982, 2000), Lijphart (1984, 1999), Taagepera and Shugart (1989), Shugart and Carey (1992), and Cox (1997); see Myerson (1999) for a discussion of the theoretical literature on the consequences of different electoral rules.

litical process (for instance, in the extent of political corruption and abuse of power, or in the size and scope of redistributive programs). This recent line of research uses the analytical tools of economics and formally models the political process as a delegation game between voters and politicians. It asks how alternative rules of the game embedded in alternative constitutional features shape the incentives of rational players and, ultimately, the equilibrium policy outcome. This theoretical literature generates strong predictions regarding the causal effect of the constitution on economic policy, but typically neglects its effect on the political phenomena studied by political scientists. The primary goal of this chapter is to summarize the main predictions of this theoretical line of research. In Section 2, we begin by describing its general approach. Next, we describe the specific predictions of the theory, first regarding alternative electoral rules (Section 3), then regarding alternative forms of government (Section 4).

Finally, a third group of related studies in political science and economics have taken an intermediate approach, linking economic policy outcomes not to the constitution, but to other political phenomena such as the number and type of political parties, and the incidence of minority, coalition or divided governments. This line of research is mainly empirical and does not attempt to study the whole chain of causation, from the constitution to political phenomena to economic policy outcomes, just focusing on the last link. But since the party structure and the types of governing coalitions are known to be influenced by the constitution, these studies are also relevant for our task. Section 5 briefly mentions some of the relevant contributions, with no pretence of completeness. The results of this line of research provide additional motivation for some specific hypotheses we wish to test, and suggest a number of more exploratory empirical questions.

In Section 6, we take stock of the main ideas in the chapter. This section also sets the agenda for the empirical work in the book by listing the specific hypotheses we wish to test, as well as the open questions we wish to confront with the data. Section 7 briefly describes how the remaining chapters of the book try to make progress on this agenda.

2.2 A common approach

Political institutions aggregate conflicting interests into public policies. As we are interested in conflicts with an economic origin, we focus on economic

policy in general, though most of the specific applications in the literature deal with government spending. It is useful to distinguish between three types of economic policy on the basis of the beneficiary. Economic policy can provide benefits to: (i) *many citizens*, (ii) *a narrow group of citizens*, and (iii) *virtually no citizens, but a specific group of politicians.*

Each of these types of policy induces a specific kind of economic conflict. Broad programs in the form of general public goods like defense, or broad redistributive programs like social insurance or pensions, are examples of type (i) policies, that provide benefits to many individuals. Because of their broad nature and universalistic design, these programs cannot easily be tailored to the specific demands of well defined groups of citizens. Hence, they are evaluated in a similar fashion by large groups of beneficiaries. Many of the entitlement programs typical of the modern welfare state belong to this category. Local public goods or specific redistributive programs, like agricultural support, or transfers to government enterprises, are examples of type (ii) policies, only benefiting narrow groups of citizens. This kind of spending is referred to as "pork barrel" and often, though not always, reflects discretionary policy decisions. Such narrow programs can much more easily be targeted to groups in specific geographic areas.[2]

The third type of economic policy generates *rents* to politicians. These can take various forms, depending on the specific economic circumstances: literally, they are salaries for public officials or the financing of political parties. Less literally, one can consider various forms of corruption and waste as ultimately providing rents for politicians. While broadly or narrowly targeted programs induce conflicts among voters, rents for politicians are at the core of the political agency problem, pitting voters at large against politicians (or other government officials). Voters are unanimous in their desire to limit the rents extracted by politicians, but may lack the necessary means to achieve this. The resources appropriated in this way are probably small in most modern democracies, compared to the overall size of tax revenues. But since they directly benefit the agents in charge of policy decisions, the political struggle to appropriate such "crumbs" can nevertheless induce a strong influence on other policy decisions. Moreover, in developing democracies – particularly at lower levels of development – the direct extraction of resources

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[2]Naturally, the distinction is not as crystal clear in reality. For example, social security programs may include early retirement provisions that could be targeted to workers in occupations or sectors predominating in specific geographical areas.

by powerful political leaders can be quantitatively significant, as revealed by well-publicized examples in Africa, Asia and Latin America.

This discussion suggests a general approach to modeling the outcome of policymaking. How these three conflicts are resolved and thus, what economic policy we observe, hinge on the constitutional rules in place. In this approach, economic policy is the equilibrium outcome of a delegation game, where the interaction between rational voters and politicians is formally modeled as a game on extensive form. The voters – the multiple principals – elect political representatives – the agents – who, in turn, set a policy to further their own objectives. The principals have some leeway over their agents because they can offer them election, or re-election. But these rewards are mostly implicit, not explicit, so that the constitution becomes an "incomplete contract", leaving the politicians with some power in the form of residual control rights. The crucial aspects of constitutions are those setting the rules of this delegation game: namely, electoral rules and rules for government formation and dissolution.

This approach to the politics of policymaking forces the theorist to be precise about the rules of the game. It is then quite natural to ask what are the effects of changing these rules, letting alternative rules of the game represent alternative constitutional provisions. Thus, comparative politics becomes a natural, almost inevitable, item in this research program.

We now survey a number of recent theoretical studies which all apply a comparative-politics approach of this type, with the purpose of extracting their testable predictions. As the focus of the book is decidedly empirical, we keep the description of theory brief, emphasize the main ideas and the intuition behind the results, and do not attempt to reproduce any of the formal arguments.[3]

2.3 Electoral rules

We begin with recent studies of alternative rules for electing the legislature. All these studies focus on different aspects of fiscal policy (and, in particular, of government spending), but the general idea generalizes to other economic policies. Legislative elections around the world differ in several dimensions. The political-science literature discusses these dimensions in great detail, but

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[3]Many of the ideas are described in greater detail in Persson and Tabellini (2000a, chs. 8-10).

commonly emphasizes three of their features: *district magnitude,* the *electoral formula,* and *the ballot structure.* District magnitude simply determines the number of legislators (given the size of the legislature) acquiring a seat in a typical voting district. One polar case is that all legislators are elected in districts with a single seat, the other that they are all elected in a single, all-encompassing district. The electoral formula determines how votes are translated into seats. Under plurality rule, only the winners of the highest vote shares get seats in a given district, whereas proportional representation (PR) awards seats in proportion to the vote share. The ballot structure, finally, determines how citizens cast their vote, choosing between different individual candidates or different party lists. As discussed further below (and in Chapter 4), these three features are strongly correlated among real-world electoral systems.[4]

2.3.1   District magnitude

A series of related papers compares two-parties electoral competition in single member districts vs a single national district, under plurality rule. The winner sets policy (the so-called winner-takes-all assumption). All these papers predict that district magnitude influences the *composition and allocation of spending* promised during the electoral campaign.[5]

Persson and Tabellini (1999, 2000a, Ch. 8) use a probabilistic-voting model with two parties, where the election outcome is uncertain when the two parties design their electoral platforms ahead of the elections. Economic policy outcomes are determined by the commitments to these platforms. Larger districts diffuse electoral competition, inducing parties to seek support from broad coalitions in the population, and from the whole population in the extreme case when the whole legislature is elected in a single district. Smaller districts instead steer electoral competition towards narrower, geographical constituencies, which are thus the primary beneficiaries of the electoral promises of both candidates. Specifically, when districts are small

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[4]Cox (1997), Blais and Masicotte (1996) and Grofman and Lijphart (1986) give recent overviews of the electoral systems across the democracies in the world.

[5]Given the simple framework of two-party competition and the assumption of the winner takes it all, the distinction between district magnitude and electoral formula is hard to draw. In a single national district, plurality rule and a strictly proportional electoral formula are equivalent. Thus, these papers can also be considered as comparing strictly majoritarian to strictly proportional elections in a simple framework.

and not altogether homogeneous in the composition of voters, each party is typically a certain winner in some "safe" districts, and electoral competition becomes concentrated to the remaining pivotal districts. Both candidates thus have strong incentives to target their policies towards voters in these districts. Clearly, broad programs are more effective in seeking broad support and targeted programs in seeking narrow support. Elections with larger districts should thus be more biased towards non-targeted programs, such as general public goods or broad transfer programs. In a study of the US electoral college, Strömberg (2002) studies a more general version of the same kind of model. Focusing on the allocation of electoral campaign spending, he predicts that both candidates should spend more in the pivotal electoral districts, which is consistent with data from recent US presidential elections.

Under the winner-takes-all assumption and plurality rule, district magnitude has a second effect, which reinforces the previous prediction. Under these assumptions, votes for a party not obtaining plurality are completely lost, and a small district magnitude reduces the minimal coalition of voters needed to win the election. With single-member districts and plurality, e.g., a party only needs 25 % of the national vote to win (50 % in 50 % of the districts). With a single national district, by contrast, it needs 50% of the national vote. Politicians are thus induced to internalize the policy benefits for a larger segment of the population, which gives them stronger incentives to select policy programs with broad-based benefits under PR than under plurality rule. Lizzeri and Persico (2001) make this point in a general model of electoral competition, where voters are forward looking and two political candidates commit to electoral promises of how to split a given budget between national public goods and transfers, which can be targeted to any coalition of voters. The equilibrium has more public-good provision under a single national district than under several single member districts. Persson and Tabellini (2000a, Ch. 9) reach the same conclusion in a model where policy choices are instead made by an incumbent, once in office. Voters follow a retrospective strategy re-electing incumbents whose policy choices give them high enough utility. Once more, the equilibrium has more public-goods provision with a single national district.[6]

Milesi-Ferretti, Perotti and Rostagno (2002) obtain a similar prediction in

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[6]As discussed in the previous footnote, these papers can also be considered as contrasting a strictly majoritarian with a strictly proportional electoral system, where both district magnitude and the electoral formula are changed at the same time. Thus, the effect on the composition of spending can also be seen as resulting from the electoral formula.

a model where the policy is instead decided upon after elections in bargaining among the politicians elected to the legislature. Voters understand the working of the post-election legislative bargaining and elect their representatives strategically. As a result, legislators mainly represent socio-economic groups when districts are large, while they mainly represent groups in specific geographic locations when districts are small. This way, smaller districts again become associated with the targeting of narrow geographical groups, whereas larger districts become associated with broad programs benefiting voters across many districts. Milesi-Ferretti et al (2002) also obtain the result that larger districts should be associated with larger *overall spending*, while Persson and Tabellini (2000a, Ch. 8) find the effect on overall spending to be ambiguous.

District magnitude is also likely to influence *rent extraction*, with larger districts reducing the rents extracted by politicians. One mechanism is analyzed by Myerson (1993). In his model, parties (or equivalently, candidates) differ along two dimensions: their intrinsic honesty and their ideology. Voters prefer honest candidates, but disagree on ideology. Dishonest incumbents may still cling to power if voters sharing the same ideological preferences cannot find a good substitute candidate. With large districts, an honest candidate is available, for all ideological positions, and dishonest candidates have no chance of being elected in equilibrium. With single-member districts, the equilibrium can be very different. Even if honest candidates run for office for all possible ideological types, only one candidate can win the election. Voters may strategically vote for a dishonest, but ideologically preferred, candidate: if they expect other voters of the same ideology to do the same, switching to the honest candidate entails a risk of giving the victory to a candidate on the other side of the ideological scale. Small districts and strategic voting thus raise the barriers to entry in the electoral system, and make it more difficult to oust dishonest incumbents from office.

In Myerson's model, voting behavior is endogenous to the electoral rule, whereas dishonesty is an exogenous feature of candidates. Ferejohn (1986) instead endogenizes the behavior of incumbents, by letting them choose a level of effort, given that voters hold incumbents accountable for their performance through a retrospective-voting rule. But Ferejohn's model can easily be reformulated such that rent extraction is equivalent to exerting little effort.[7] In Ferejohn's model, electoral defeat is less fearsome, the higher is

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[7]See Persson, Roland and Tabellini (2000).

the probability that an ousted incumbent will return to office in the future. While Ferejohn treats this probability as an exogenous parameter, he points out that it is likely to be negatively related to the number of parties, or the number of candidates. Given the strong empirical relation between district magnitude and the number of candidates, we obtain the same prediction as above: larger districts should be associated with less extraction of rents.

2.3.2 Electoral formula

Lizzeri and Persico (2001) also contrast how alternative electoral formulas influence the *composition of government spending*. They interpret a proportional electoral rule as one where both candidates maximize the vote share (since the spoils of office are proportionally divided among candidates to the share of the vote). Plurality rule is instead associated with the winner take all assumption (since the spoils go to the winner). Here, the prediction turns out to be ambiguous: proportionality is associated with more public goods and less targeted redistribution, compared to plurality rule, only if the public good is very desirable for the voters; otherwise the opposite might occur. The intuition is that if the public good is very desirable, reducing it implies a large drop in the vote share of the non pivotal voters. Under plurality rule, candidates disregard this cost. But if they also care about vote shares, they internalize it, which leads them to provide more public good at the expense of targeted redistribution.

Austen-Smith (2000) suggests another mechanism whereby the electoral formula may shape the *overall level of taxation and spending*. His model takes the party structure as exogenous, but makes the empirically plausible assumption that fewer parties are represented under plurality rule (two parties) than under PR (three parties). Policy in the form of redistributive taxation is decided in post-election legislative bargaining. Under plurality rule, the winner-takes-all property results in the party commanding a majority making single-handed policy decisions, but under PR, no party commands a majority and successful parties must form a coalition to set policy. The interaction between elections, redistributive taxation, and the endogenous formation of economic groups typically produces politico-economic equilibria with higher taxation under PR than under plurality.

2.3.3   Ballot structure

Whereas voters typically cast their ballot among individual candidates under plurality rule, they cast it among party lists under proportional representation. Such lists may dilute the incentives for individual incumbents to perform well. Persson and Tabellini (2000a, Ch. 9) examine the policy consequences of this difference in a model where individual politicians can extract personal rents in the policy process. But they also have career concerns, which they can enhance by building a reputation for their competency among imperfectly informed voters. Politicians thus face countervailing incentives: current rent extraction has a direct benefit, at the cost of a worse reputation. In this model, voting on party lists is associated with more rent extraction than voting on individuals, because the career-concern (re-election) motive becomes a weaker counterweight to the rent-extraction motive for politicians when they are collectively, rather than individually, accountable.

Earlier non-formalized work in political science has expressed related ideas, even though it has only been implicitly, or tangentially, concerned with economic policy outcomes. One good example is Carey and Shugart (1995), who discuss the incentives for politicians to act so as to cultivate a "personal vote" in different electoral systems. They use this criterion to classify real-world systems on the basis of ballot structure and other features (including district magnitude, distinctions between open and closed lists in PR-systems, etc.; see Chapter 4, below).

2.3.4   Empirical predictions

We thus have several predictions. On the composition of spending, large districts and PR both pull in the direction of broad programs, whereas small districts and plurality pull in the direction of programs narrowly targeted at small constituencies. These reinforcing effects are important when using the data, due to the strong correlation in district size and electoral formulas across real-world electoral systems. Some systems can be described as majoritarian, combining small voting districts with plurality rule (cf. elections to the UK parliament or the US Congress, where whoever collects most votes in a district obtains the *single* seat). As we have seen, both features favor narrow programs. Other electoral rules are instead decidedly proportional, combining large electoral districts with PR (cf. Dutch or Israeli elections, where parties obtain seats in proportion to their vote shares in a single na-

tional voting district), both favoring broad programs. While we find some intermediate systems, most countries fall quite unambiguously into this crude classification (see further the discussion in Chapter 4).

Some models, albeit not all, predict that majoritarian systems should overall be associated with smaller government spending and taxes. In Milesi-Ferretti, Perotti and Rostagno (2002), the reason is a smaller district size, while in Austen Smith (2002), the reason is plurality rule.

When it comes to rents for politicians, the predictions are more subtle. Majoritarian systems have higher barriers to entry than proportional systems, due to their smaller districts, which should permit more rent extraction. But they also have more direct accountability due to their use of voting for individuals (under plurality rule), which should restrict rent extraction. The overall effect is ambiguous, depending on which of these two features is quantitatively more important. Ideally, empirical work should identify the separate consequences of these different features of electoral rules.

Finally, some of these ideas might have relevant implications for electoral cycles in spending and taxes. As noted in Section 1 above, and as emphasized by political scientists, accountability is greater under majoritarian elections, in particular under plurality rule. Thus, elected officials might have stronger incentives to please their voters (or at least to appear to) in the imminence of elections under majoritarian than under proportional electoral rule. A reasonable conjecture is thus that electoral cycles in spending or taxes are more pronounced in majoritarian (plurality and individual-centered) systems.[8]

2.4 Forms of government

Recent theory has devoted less effort to the rules for legislation than to those for elections. But it has clarified the consequences of two aspects of the legislative regime inherent in different forms of government. These concern the powers over legislation: to make, amend, or veto policy proposals. One is the allocation of these powers to different offices: is there an effective separation of powers across different politicians and offices, or is there a single office vested with several different powers? The other aspect is how these powers are maintained over time: in particular, is the executive subject to a *confidence requirement* of continued support from a majority in the legislative

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[8]Indeed, commenting on the career concern model quoted above, Persson and Tabellini (2000a) formulate this conjecture with regard to the effect of the ballot structure.

assembly? With some provisos noted below, and further discussed in Chapter 4, these two aspects of legislative rules can be associated with the two predominant forms of government, namely presidential and parliamentary democracies.[9]

2.4.1   Separation of powers

Many presidential regimes have a stronger separation of powers – between the president and congress, but also between congressional committees holding important proposal (agenda-setting) powers in different spheres of policy (think about the US) – than many parliamentary regimes, where the proposal powers over legislation are instead concentrated in the hands of the government. This statement is a stark simplification, as the separation of legislative powers also differs a great deal within each of these forms of government, depending on more detailed constitutional arrangements (see further Chapter 4). Still, it is a useful starting point.

Why should separation of powers be of importance for economic policy? A classical argument, already formulated in a clear fashion by James Madison more than 200 years ago, holds that checks and balances between different offices constrain politicians from abusing their powers. Persson, Roland, and Tabellini (1997, 2000) formally demonstrate this old point. In both settings, incumbent politicians set policy under alternative assumptions about legislative bargaining, designed to capture some basic distinctions between different forms of government. The incumbents are held accountable by retrospective voters. Because of the greater concentration of powers in parliamentary regimes, it is easier for politicians to collude with each other at the voters' expense; in equilibrium, weaker electoral accountability results in higher rents and higher taxes than in presidential regimes, where more pronounced checks and balances help the voters hold the politicians more accountable for abusing their powers by diverting tax money for private gain.

2.4.2   Confidence requirement

The second crucial distinction between presidential and parliamentary democracies, indeed one of the defining distinctions between these forms of govern-

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[9]Lijphart (1984) contains a useful discussion of different forms of government. Shugart and Carey (1992) provide an exhaustive treatment of presidential regimes in the world, with a thorough discussion of separation of powers as well as executive survival.

ment, is the presence or absence of a confidence requirement.[10] Presidential regimes lack this rule: once appointed (typically in a direct election), the executive can hold on to her powers without the support of a majority in the legislature. According to the main principle of parliamentarism cabinets in parliamentary regimes instead need the continuous confidence of a majority in the legislature to maintain their powers throughout an entire election period. (How to make this classification in practice is discussed in Chapter 4.)

The confidence requirement is important for the working of the legislative process. Parties supporting a parliamentary executive hold valuable powers, which they risk losing in a government crisis. Therefore, a confidence requirement creates strong incentives to maintain party discipline, as noted, among others, by Shugart and Carey (1992) and as formally modeled by Huber (1996). But as analyzed in detail by Diermeier and Feddersen (1998), the incentives to hold legislative *cum* executive majorities together extend from members of the same party to coalitions of parties. To use the jargon of the literature, the confidence requirement creates "legislative cohesion", namely stable majorities supporting the cabinet and voting together on policy proposals. The absence of a confidence requirement, by contrast, fosters unstable coalitions and less discipline within the majority.

Building on this idea of legislative cohesion, Persson, Roland and Tabellini (2000) derive two additional predictions in their model, where incumbent legislators elected by retrospective voters in different districts set policy in alternative arrangements for legislative bargaining. In arrangements modeled on parliamentary regimes, a stable majority of legislators tends to pursue the joint interest of its voters. Spending thus optimally becomes directed towards broad programs benefiting a majority of voters, such as broad social transfer programs or general public goods. In presidential regimes, the (relative) lack of such a majority instead tends to pit the interests of different minorities against each other for different issues on the legislative agenda. As a result, the allocation of spending targets powerful minorities among the constituencies of powerful officeholders, e.g., heads of congressional committees in the US.

Moreover, in parliamentary regimes, the stable majority of incumbent legislators, as well as the majority of voters backing them, become "resid-

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[10]Another distinction is often made on the basis of the executive: presidential regimes having an individual executive, and parliamentary regimes having a collective executive.

ual claimants" on additional revenue; they can keep the benefits of spending within the majority, putting part of the costs on the excluded minority. Both majorities thus favor high taxes and high spending. In presidential regimes, on the other hand, no such residual claimants on revenue exist, and the majority of taxpayers and legislators therefore resist high spending. On the basis of this mechanism, and the other mechanisms described above, Persson, Roland and Tabellini (2000) unambiguously predict larger governments (higher taxes and overall spending) in parliamentary regimes than in presidential regimes.

2.4.3   Empirical predictions

In summary, several predictions emanate from the theoretical research on how policy outcomes are affected by the legislative rules enshrined in different forms of government. By the separation of powers argument, presidential regimes should be associated with less rent extraction and lower taxation than parliamentary regimes. By the confidence requirement argument, they should also be associated with more targeted programs at the expense of broad spending programs. Overall, we should find parliamentary regimes to have larger governments than presidential regimes.

2.5   Related ideas

The research surveyed in the previous sections tries to model the direct effects of constitutional rules on policy outcomes through the policymaking incentives of political candidates or incumbents, leaving out prospective indirect effects through intervening political outcomes. This may be an important omission, as we have very good reasons to expect such indirect effects to exist.

Specifically, many contributions by political scientists stress the implications of electoral rules and government regimes for party structure and the type of government. As mentioned above, proportional elections entail lower barriers to entry for political parties and we do observe that larger districts and a proportional electoral formula go hand in hand with a larger number of parties (see Rae, 1967, Taagepera and Shugart, 1989, and Lijphart 1990). Related to this, parliamentary regimes with majoritarian electoral rules are much more likely to produce single-party majority governments, whereas

coalition and minority governments are more likely under PR (Taagepera and Shugart, 1989, Strom, 1990, and Powell 2000). Moreover, presidential regimes are sometimes associated with a divided government, with presidents and congressional majorities coming from different parties, while this is ruled out in parliamentary regimes (Shugart and Carey, 1992).

These political outcomes may, in turn, have systematic effects on economic policymaking, thus creating an indirect link between the constitutional rules and economic policy outcomes of interest. Indeed, the general idea that party structures and types of government shape economic policy reappears in many studies. All the specific ideas may not have been fleshed out with the same analytical rigor as in the recent theoretical literature. And some conclusions are derived from observed empirical correlations, rather than coherent theoretical models. But some of these studies suggest the same reduced-form predictions as the above-mentioned hypotheses.

In particular, some studies of the so-called common-pool problem in fiscal policy suggest this problem to be more pervasive under coalition governments. The common-pool problem refers to a situation where the benefits of government spending are concentrated to relatively narrow groups of beneficiaries, while the costs of raising revenues are shared among all taxpayers. In this situation, all groups have an incentive to push for more of the spending from which they benefit, since they only internalize part of the cost. The equilibrium is likely to result in aggregate over-spending. Since the distortion in the incentives is greater, the larger is the number of groups (or equivalently, the smaller is the group size), Kontopoulos and Perotti (1999) argue that government spending might increase in the number of coalition parties, and provide evidence consistent with this hypothesis. Scartascini and Crain (2001) reach similar empirical results. Because coalition governments are more common under proportional electoral rules, we obtain an indirect positive association of the size of government with proportional electoral rules, i.e., the same ultimate conclusion as the studies cited in Section 3.

Scholars in political sociology have investigated determinants of the welfare-state programs and spending, including constitutional determinants. The broad study by Huber, Ragin and Stephens (1993) is of particular interest. They argue that presidentialism, as well as majoritarian elections, produce dispersed political power and multiple points of influence on policy and that this will hamper welfare-state expansion; an argument similar to that in the formal models discussed above. Moreover, they show that a constitutional index including these, and other, features has a strong negative influence on

welfare-state expenditures, when a number of other economic and social variables are held constant, in a data set encompassing 17 developed democracies over 30+ years.[11] More recently, similar arguments appear in Swank (2002) and the contributions in Pierson (2001).

Related studies suggest further questions that can be posed to the data. In their review of the extensive work on government budget deficits, Alesina and Perotti (1995), drawing on work by Velasco (1999), argue that coalition governments face a more severe dynamic common-pool problem which makes them more prone to run deficits. Hallerberg and von Hagen (1998, 1999) explicitly link the severity of the common-pool problem to electoral systems and forms of governments and argue that the appropriate reforms of the budget process differ across these constitutional rules. These arguments are supported by empirical evidence from European and Latin American data sets.

As coalition governments have more veto players, these could be subject to a more serious status-quo bias in the face of adverse shocks (Roubini and Sachs, 1989, and Alesina and Drazen, 1991). Government crises are a priori more likely and empirically more frequent under proportional elections (due to the greater incidence of minority and coalition governments). Such crises could lead to greater policy myopia and larger budget deficits (Alesina and Tabellini, 1990 and Grilli, Masciandaro and Tabellini, 1991). These ideas are related to those in Tsebelis' (1995, 1999, 2002) studies, where a larger number of veto players (and a larger ideological distance between them) tends to "lock in" economic policy and its ability to handle outside shocks. In Tsebelis' conception, proportional elections often lead to multiple partisan veto players in government and thus, to more policy myopia, even though the electoral rule is not the primitive in his analysis.

Finally, large swings in the ideological preferences of governments as a result of the elections are less likely in systems where coalition governments are the norm. Alesina, Roubini and Cohen (1997) suggest that coalition governments (and thus, proportional elections) correlate with less pronounced "partisan" cycles after elections, and Franzese (2002) provides further evidence on this.

These studies suggest that we should expect to find greater budget deficits under proportional than under majoritarian elections (at least among par-

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[11]More precisely, their index has five parts, namely indicators for federalism, bicameralism, referenda, presidentialism and majoritarian elections.

liamentary regimes). They also suggest another question: is the adjustment of spending or taxes to economic shocks conditional on the electoral rule? Finally, they put forward an additional argument, beyond that in Section 3, i.e. why electoral cycles might depend on the electoral rule.

The same empirical questions could be posed about policy outcomes under different forms of government. No formal analysis of which we are aware has tried to contrast the size of the budget deficit or the reaction of policy to economic shocks in presidential and parliamentary regimes. A priori, the comparison could go either way. On the one hand, a more effective separation of powers under presidential regimes, might imply a greater status-quo bias in policymaking, to the extent that it increases the number of veto players above that in parliamentary regimes. Indeed, some authors have tried to explain the occurrence of budget deficits and the adjustment to shocks in the US states as the result of a divided government, where governors and majorities in state congresses are controlled by different parties (Alt and Lowry, 1994). The common criticism among political scientists against Latin American presidential regimes for being commonly dead-locked and ineffective, can be read in the same way. On the other hand, the fixed term of office and the greater durability of the executive in presidential regimes could reduce policy myopia, relative to parliamentary regimes.

Moreover, we do not know of any formal analysis trying to predict the relative size of electoral budget cycles under different forms of government. But if the strength of the electoral cycle depends on electoral accountability, the more pronounced separation of powers and the individual nature of the executive in presidential regimes, the strength and nature of such cycles may well be systematically associated with the form of government.

The above discussion has centered on fiscal policy, broadly defined to include rents for politicians, which certainly reflects the orientation of the literature. But it is not difficult to think of plausible extensions into other areas of economic policymaking, such as regulatory policy or trade policy. The same mechanisms within presidential regimes and under majoritarian elections that bias policy decisions towards spending programs targeted at narrow groups may bias policy decisions towards boosting the incomes of geographically concentrated special interests by, say, tariff protectionism or regulation of entry. Thus, it is plausible to conjecture that such structural policies also differ systematically across political systems, though this is still an open research agenda for both theoretical and empirical analysis.

Economists or not, at the end of the day, we are not only interested in

government policies per se, but also in their overall effect on more fundamental economic and social performance measures. Asking precise questions about how different political systems perform in these final dimensions is certainly much too difficult at our present state of knowledge. Even if we knew the precise effects of specific constitutional forms on different policy outcomes, these policies interact in a complicated way, and most probably affect performance in different directions. For instance, the greater accountability of majoritarian elections might discourage the corruption of elected officials but, at the same time, less representativeness and the associated weaker incentives for public good provision might have an ambiguous net effect on economic development or private investment. Moreover, separating the influence of the political system from that of other features of society is a Sisyphean task. Thus, existing theoretical research or background empirical and historical knowledge does not enable us to entertain any precise prior hypothesis about the likely causal effects of alternative constitutional features on measures of economic performance, such as labor productivity or economic growth.[12] Nevertheless, it is tempting to take a look at the relation between political systems and economic performance, and try to clarify at least some of the policy links whereby the political system shapes economic performance. Once more, such an exercise should not be seen as the testing of specific theories; but it could serve as a suggestive exploration of the data.

2.6   What questions do we pose to the data?

Given the discussion in this chapter, some of the empirical questions we will pose take the form of specific hypotheses, whereas others really amount to a search for systematic patterns in the data. We also confine ourselves to posing questions on reduced form – is there a link from constitutional rules to policy outcomes? – without trying to discern whether the effects we might find are direct or indirect, running through political outcomes such as party structures. As discussed in Chapter 9, identifying the precise channels of constitutional influence is certainly a very important task. But we have to start somewhere and thus leave this task for future work, including a substantial investment in new data.

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[12]However, Talbott and Roll (2002) provide interesting and convincing evidence of democracy being unambiguously good for economic performance. See also Barro (2000??) and Alesina, Ozler, Roubini and Swagel (1996).

After this qualification, let us summarize the main empirical questions we would like to address with the help of *Table 2.1*. At the top of the table, we encounter the theoretical predictions from Sections 3 and 4. According to theory, presidential regimes should have smaller governments than parliamentary regimes and also less spending on broad government programs vs. targeted programs (these predicted spending differences between presidential and parliamentary regimes are indicated by minus signs in the two upper cells of the right-hand column). Presidential regimes are also predicted to have less rent extraction than parliamentary regimes. Under majoritarian elections, we should observe less spending on broad programs than under proportional elections (this spending difference between majoritarian and proportional elections is indicated by the minus sign in the second-row of the left-hand column). Several, but not all, models predict that the electoral rule also shapes the size of government, with proportional elections associated with larger governments (thus, we enter a minus sign/question mark in the first row of the column for majoritarian vs. proportional policy outcomes). Given the correlation between district size and the electoral formula mentioned above, the different predictions of electoral rules and spending in the literature typically reinforce each other. In the case of rent extraction, however, the district size and ballot structure commonly encountered in majoritarian elections pull in opposite directions: while smaller districts imply larger rents, voting on individuals implies smaller rents (hence, the minus and plus signs in the electoral rules column). All of these are cross-sectional predictions in that they have been explicitly derived by comparing equilibria in static game-theoretic models.

Table 2.1 about here

Moving down the table, we reach the more open-ended, cross-sectional questions formulated in Section 5. Here, we are interested in the effect of the two constitutional provisions on the budget balance (surplus or deficit). While several ideas suggest that majoritarian elections should be associated with smaller deficits, the prospective impact of the form of government is more uncertain. In the light of the discussion in the previous section, we would also like to know whether structural policies promoting economic growth and efficiency systematically take on different orientations under certain electoral rules or forms of government. We also want to explore whether different political systems entail systematic differences in economic perfor-

mance but, once more, we have no precise prior hypothesis.

At the bottom of the table, we find some predictions and questions, which are more squarely in the time-series domain. One plausible conjecture has been recalled above: the stronger incentive for politicians to perform should generate more pronounced electoral cycles under majoritarian elections than under proportional elections. We have no theoretical prior as to the strength of electoral cycles under different forms of government. Similarly, we would like to empirically investigate whether the reaction of fiscal policy to economic shocks differs across political systems, but we have few strong priors of what to expect.

2.7   The empirical agenda

The empirical questions summarized in *Table 2.1* set the agenda for the remainder of the book. A first task is to make operational the different aspects of policy and performance entering the left-most part of the table. How do we measure the composition of government programs, rent extraction or economic performance in practice? What shocks do we consider and over what period? And so on. We have collected data for a large number of countries, both for the most recent decade and for a longer period going back to 1960. In Chapter 3, we describe these measures of observed policy outcomes. Because the existing theoretical models – at best – deliver *ceteris paribus* predictions about the effect of constitutional features, we must also take into account a number of other country characteristics that may shape the outcomes we seek to explain. We thus introduce these other variables and show how they influence policy outcomes.

Chapter 4 tackles another crucial question: how to classify and measure real-world constitutional features? As the underlying theory deals with democratic decision-making, the chapter starts by defining what we mean by a democracy in practice. It then moves on to classify electoral rules and forms of government into the broad categories of *Table 2.1* – and also to develop continuous measures of some detailed features of electoral rules. Ideally, we would like our measures to be consistent with the distinctions between different electoral and legislative rules made in the underlying game-theoretic models. Because of the rich variation in actual constitutions, however, this requires taking a number of specific decisions. In the chapter, we show that constitutional rules have a great deal of inertia: the broad features of electoral

and legislative rules – as we measure them – are very rarely reformed. This is both a blessing and a curse. It implies that we cannot hope to draw inferences about direct constitutional effects by observing the consequences of reform. Instead, we must rely on cross-country comparisons. But it also means that the chain of causation is likely to go from institutions to policies and not vice versa. Moreover, we take a first look at the data by constitutional group, and find strong evidence that the selection of different constitutional rules is certainly not random, relative to geography, history and other country characteristics. A better understanding of these non-random patterns of constitution selection is also one of the important goals of Chapter 4.

Chapter 5 is devoted to some non-trivial methodological issues. How exactly do we define a causal "constitutional effect"? And how can we estimate it in a reliable way, given the aforementioned non-random selection and inertia of constitutional rules? We propose a number of econometric methods designed to address different statistical pitfalls but requiring different assumptions for identifying a constitutional effect. These assumptions are scrutinized in the context of our data. The methodological discussion in this chapter takes us into statistical territory, parts of which may be familiar to many readers, while others may not.

In Chapter 6, we apply the resulting battery of statistical methods to draw inference from the cross-country variation in fiscal policies and institutions. The theoretical predictions summarized in the rows of *Table 2.1* regarding the constitutional effect on the size of government, the composition of government and budget deficits, are taken to the data and tested with a variety of alternative statistical methods. Despite the different estimation methods, many of the results are surprisingly stable.

In Chapter 7, we go on to the predictions regarding the constitutional effects on rent extraction and the open questions regarding structural policies and economic performance, once again relying on the cross-sectional variation of the data. In this chapter, as in the prior one, we find robust results partly in line with theory. Here, the finer measures of district magnitude and the ballot structure play a more important role, than the crude classification into majoritarian and proportional electoral rules.

Chapter 8 returns to fiscal policy, but exploits its variation over time. Here, we explore the open issues with question marks in the lower part of *Table 2.1*. Do countries with different constitutions adjust when hit by common or idiosyncratic economic shocks? Does fiscal policy behave in a particular way immediately before or after the elections? If so, are these electoral cycles

different when conditioned on the electoral rule or the form of government? We find the answer to all these questions to be in the affirmative, and uncover a number of new stylized facts.

Finally, Chapter 9 starts with a brief discussion of what we can learn from the empirical evidence in the preceding chapters with regard to each cell in the table. It ends by asking where research should go next.

Table 2.1
Constitutions and economic policy
Theoretical predictions and open questions

Policy outcome	Electoral rules	Form of government
	Majoritarian vs.proportional	Presidential vs. parliamentary
Overall size of government	– / ?	–
Composition: broad vs. narrow programs	–	–
Rent extraction	+ / –	–
Government deficits	– / ?	?
Structural policies/economic performance	?	?
Adjustment to shocks	?	?
Electoral cycles	+/?	?

Chapter 3

Policy measures and their determinants

3.1 Introduction

Now that we know what questions to explore regarding the constitutional effects on different policies, it is time to turn to the data. In this chapter, we describe the measures of performance and economic policies we seek to explain on the basis of alternative constitutional features. Data on the constitutions are discussed in the next chapter. We start the chapter with fiscal policy: the size of government, the composition of government spending and the budget deficit, measuring these outcomes in alternative ways. Then, we proceed to proxies for rent extraction by politicians: perceptions of the incidence of corruption and of (in)effectiveness in government services. Finally, we turn to composite measures of growth-promoting policies, such as the protection of property rights or other structural policies, and their impact on long-run indicators of economic performance, namely labor productivity and total factor productivity.

Because it is hard to find interesting variations in electoral rules or forms of government at the sub-national level, we focus on comparisons among nation states. We are interested in the variation of policy across both time and place. It is then convenient to keep the data in two different data sets, corresponding to the two dimensions of policy variation. Since the theory underlying the predictions outlined in Chapter 2 refers to democratic decision-making, these data sets are restricted to democracies. The next chapter

explains in detail our (sometimes quite generous) criteria for including countries in the two sets of democracies.

To study the cross-sectional variation of policy, we design a data set including 85 countries that can be considered as democracies in the 1990s. For these countries and a large number of variables, we take an average of the yearly outcomes over the 1990-1998 period, referring to the resulting data set as the nineties' cross section, or the 85-country cross section. It then forms the main basis for our empirical work on constitutional rules and policy outcomes across countries in Chapters 6 and 7. This data set is used to analyze all measures of performance and policy outcomes: fiscal policy, rent extraction, growth-promoting policies and productivity.

To study the time variation of policy, we design an alternative data set with 60 countries where data are available for a sufficiently long period. Here, annual observations are kept in panel format for each of the years 1960-98, though data are missing for many variables and countries for some of these years. We refer to this data set as the 1960-1998 panel or the 60-country panel. It is mainly used in our empirical work on constitutional rules and policy outcomes across time in Chapter 8. This work focuses on the time variation of fiscal policy, since the other policy variables are not available for a sufficiently long time interval.

Naturally, the policy and performance outcomes we study reflect many other economic, social, cultural, geographical and historical factors, besides any influence of constitutional rules. Both data sets therefore include a variety of such auxiliary determinants of policies and performance. In this chapter, we also describe these determinants (control variables). Rather than a mere listing of the variables, however, we introduce them in their context as explanatory variables for specific policies. Thus, for each of our main policy measures, we show how they correlate with a number of prospective determinants across countries and time, suggested by theory or – more often – earlier empirical work. This provides an opportunity to briefly review some of the main findings of earlier related empirical studies, with no pretence of completeness. In the course of this discussion, we thus present the estimates of linear regressions relating each policy measure to alternative sets of specific determinants. The constitutional variables of interest are omitted; they are defined in Chapter 4 and their effect on policy and performance is studied in Chapters 6 through 8. Our statistical analysis in this chapter is very simple: the regression results are not displayed for the purpose of statistical inference or hypothesis testing, but as an economical view of describing the patterns

in the data.

We begin with fiscal policy (Section 2), turning to rent extraction (Section 3), to finish with productivity and productivity-enhancing policies (Section 4). The text only provides a broad summary discussion of our empirical measures and their sources. More precise descriptions are relegated to the Data Appendix at the end of the book.

3.2   Fiscal policy

3.2.1   Size of government

Two alternative measures of the size of government appear in both our data sets. Our primary measure is central government spending (inclusive of social security) as a percentage of GDP (*CGEXP*), but we also consider central government revenues (*CGREV*) as a percentage of GDP. For most OECD countries and many countries in Latin America, data on central government spending and revenues are available for all years in our 1960-98 panel. For many developing countries, the availability is limited to the period from the 1970s and onwards, however. The statistical source for all these variables is the IMF (the IFS database).

The size of government varies a great deal, both across time and place. In the 1990s cross section, the mean value of central government spending is 29.8% of GDP, with a standard deviation of 10.4%. The range is more than 40% of GDP, from 9.7% (in Guatemala) to 51.2% (in the Netherlands). *Figure 3.1* shows the same measure for the entire 1960-98 panel (about 2000 observations in total). Once more, government expenditure in a typical year ranges from below 10 percent of GDP to well above 50 percent.[1] We also see how the distribution drifts upwards over time, reflecting growth in the average size of government – the curve in the graph – by about 8% of GDP from the 1960s to the late 1990s. Most of this growth takes place in the 1970s and 1980s.

Figure 3.1 about here

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[1]In drawing the figure, we have censored the observations where *CGEXP* exceeds 60% of GDP to obtain clearer graphics. The censored observations apply to the years of war or unrest in Israel and Nicaragua.

A natural question is why central, rather than general, governments (the latter also include local and regional governments)? The main reason is data availability and comparability.[2] Data on general government spending are available in the GFS database of the IMF from the early 1970s and onwards, but only for 41 of the democracies in our sample. Moreover, even for these countries, the definition of the relevant government entities or the precise definition of government outlays and revenues are often not comparable across countries or time. Central government data are more reliable, however. For countries where data on general governments are available, the correlation coefficient between the size of central and general governments is very high (about 0.9). Moreover, centralization of spending (measured as the ratio between central and general government spending) is not correlated with the constitutional variables of interest (the electoral rule and the form of government) defined in the next chapter. Thus, we are quite confident that focusing on central rather than general governments does not bias our inferences. Nevertheless, we always include an indicator variable for federal political structures (called *FEDERAL*) in our cross-country analysis. The source of this variable is Adserà, Boix and Payne (2001) who, in turn, relied on data from Downes (2000).

Several other basic country characteristics are likely to correlate systematically with the size of government. One idea originating in Wagner's Law (Wagner, 1893) is that government spending goes up with income. To measure differences in the level of development, we use (the log of) each country's real per capita income (*LYP*), taken from the Penn World Tables and the World Bank. We also use a binary indicator variable for OECD membership in the early 1990s[3] (*OECD*).

Another relevant characteristic – particularly given our interest in constitutional effects – is the quality of democratic institutions. We measure this feature by an index produced by Freedom House (*GASTIL*) for the 85-country cross section, and a similar variable compiled by Eckstein and Gurr (1975) (*POLITY_GT*), re-scaled by us and expressed in the same units as the Freedom House index for the 60-country panel. Both variables run on

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[2]Strictly speaking, the theory reviewed in Chapter 2 concerns decisions taken by central government politicians. These are likely to more easily control all levels of government in unitary than in federal states.

[3]As we mainly treat OECD membership as a (binary) indicator of development, we include all OECD members in the early 1990s, except Turkey which had a considerably lower GDP per capita than other OECD member states.

a scale from 1 to 7, higher values indicating worse democracies. These are described in detail in the next chapter.

Most empirical work on the size of government finds strong correlations between the demographic composition of the population and government spending, older populations being associated with higher spending. To measure these aspects, we use two variables: the percentages of the population between 15 and 64 years of age (*PROP1564*), and those above 65 (*PROP65*). Earlier empirical work, starting with Cameron (1978), has found more open economies to have larger governments. This might reflect increased demand for social insurance in more open and hence, more risky, economies, as suggested by Rodrik (1998), but also readily available tax bases on exports and imports, often exploited in developing countries (cf. Goode 1984). Here, we use a measure of a country's openness (*TRADE*), defined as exports plus imports over GDP. The last three variables are all extracted from the World Development Indicators of the World Bank.

Variation across countries How do these variables correlate with the size of government across countries? Column 1 of *Table 3.1* shows the results of a multiple linear regression of central government expenditures on the seven country features described above. The sample consists of the 80 countries in the nineties' cross section, where all variables are available. These seven variables explain about 60% of the variation in the dependent variable. As expected, a large share of old people is strongly associated with government spending – the elasticity is even above unity so that an additional 1% of 65+ inhabitants (at the expense of 1% less 15-year olds in the population) raises spending by more than 1% of GDP. As expected, central government spending is also lower in federal states, almost 5% of GDP. More open countries seem to have larger governments; the effect is statistically significant but relatively small: it takes a 20% increase in the trade share to raise government spending by 1% of GDP. Similarly, better democracies have larger governments: moving from the status of "semi-free" (a *GASTIL* score of 3.5, see Chapter 4) to "free" (a score of 1.5), is associated with a 4% higher spending share. In this specification, neither the level of income nor OECD membership significantly affects the size of government, ceteris paribus.

Table 3.1 about here

There are strong reasons to believe that geography and history might also

correlate systematically with government spending. To capture the geographical aspects, we use four dummy variables for continental location. They refer to countries in Africa ($AFRICA$), eastern and southern Asia ($ASIAE$) (other than Japan which is included in the OECD group), and southern and central America, including the Caribbean ($LAAM$). Taking into account the OECD group, the default group of countries thus consists of non-OECD countries in Europe and the Middle East. Among historical aspects, colonial history may be particularly important. We partition all former colonies in our sample into three groups: British, Spanish-Portuguese, and Other colonial origin. We then define three binary (0,1) indicator variables for these groups (called COL_UK, COL_ESP, COL_OTH). Since the influence of colonial heritage is likely to fade with time, we weigh these (0,1) indicators by the fraction of time elapsed since their independence, giving more weight to colonial history in young independent states. Colonial history dating back more than 250 years receives no weight at all. The result is three truncated, but continuous, measures of colonial origin, adjusted for the time elapsed since independence, and called: COL_UKA, COL_ESPA and COL_OTHA.[4]

Column 2 of the table adds these continental and colonial variables to the specification in column 1. As expected, the auxiliary variables add explanatory power; the regression now explains 65% of the cross-country variation in the data. We see that Latin American and Asian countries have smaller governments, ceteris paribus. None of the colonial variables significantly affect the size of government. As concerns earlier co-variates, the share of old people, federalism and the quality of democracy retain their significant influence. Column 3 reports the results from the same specification, except that government revenue ($CGREV$) replaces government expenditure ($CGEXP$) as the dependent variable. The results are quite similar, except that the positive effect of openness is more precisely estimated.

The controls included in columns 1 and 2 (together with the constitutional variables discussed in the next chapter) constitute the core specifications we use in Chapter 6 when estimating the constitutional effect on fiscal policy from cross-country data. These variables are either selected on a priori grounds (for instance, the level of per capita income), or because they are found to have a strong and robust correlation with the size of government. Nevertheless, we have also tried to expand the specification with a

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[4]Thus, for instance, the variable COL_UKA is defined as: COL_UK * (250 - years of independence)/250.

number of other covariates. The results of some of these alternative specifications are reported in columns 4-6, where the dependent variable is the size of government spending.

Alesina and Wacziarg (1998) have suggested that government spending is perhaps influenced by country size (which determines the scope of economies of scale or the heterogeneity of voters' preferences), and not by openness to international trade per se. Naturally, country size and openness are strongly negatively correlated, and when both variables are included in our regressions, none of them turns out to be statistically significant. In column 4, we replace openness with the logarithm of population ($LPOP$) and a measure of ethnic and linguistic fractionalization ($AVELF$), taking higher values for more fractionalized countries. This variable is described below with reference to the determinants of corruption. Both estimated coefficients have the expected sign (negative and positive, respectively), but none of them is statistically significant.

For countries at low levels of development, the administrative and deadweight costs of taxation might limit the size of government. But a large mining sector can provide a cheap source of government revenues, either directly, or indirectly through the income of the corporations operating in that sector (Goode 1984). Data on the output of the mining sector are available from the UN national accounts statistics for 75 countries in our sample, even though not always comparable across countries or time. When mining as a ratio of GDP ($MINING\_GDP$) is added to the regression in column 5, its estimated coefficient is positive and statistically significant; a finding generally robust to alternative specifications and estimation methods. Nevertheless, to avoid shrinking the sample size and given the lower reliability of these data, we do not include this variable in our default specification. Mining is not systematically correlated with the constitutional variables of interest, and its omission or inclusion does not have an impact on the results reported in Chapter 6 on the influence of the constitution on the size of government.

Finally, several median-voter models (starting with Meltzer and Richards, 1981) suggest that more income inequality raises government spending. To capture this aspect, we use the Gini coefficient ($GINI\_8090$) sampled around 1980 and 1990, and available for about 60 countries in our sample (the source is Deninger and Squire, 1996). Its estimated coefficient in column 6 is not statistically significant, a result which is very robust. Note that in this specification, mining loses its explanatory power.

Comparing these alternative specifications, a few results stand out as the most robust. A federal structure ($FEDERAL$) and location in Latin America ($LAAM$) or Asia ($ASIAE$) are associated with a smaller central government, while an older population ($PROP65$) is always associated with a larger government. Instead, the other control variables included in our default specification, such as per capita income, openness, quality of democracy, or being an OECD country, do not have a stable estimated coefficient as we vary the specification. This might reflect collinearity with some of the other included regressors. In the light of our strong priors and the findings of earlier empirical work, we always include them in our default specification, however.

Variation across time   Next, we turn to the variation in the size of government over time. In this case, we rely on the 1960-98 panel and the results are displayed in *Table 3.2.* In column 1, we report on a regression including "country fixed effects". This means that we add a dummy variable for each country to the right hand-side of the regression. Another way of understanding this specification is to consider the dependent variable as the deviation of each country's government expenditure in a given year from its mean over the entire sample period. In this formulation, country-specific variables remaining constant over time can only contribute to explain mean expenditures. Thus, we must exclude from the regression the indicator variables for federalism, OECD-membership, continental location and colonial history that were used above in studying the cross-country variation. A fixed-effect specification has the advantage that we also hold constant any *unobserved* (omitted) country-specific (time-invariant) determinants of the size of government. Put differently, the effect of any regressor on the size of government is fully identified from its variation over time, and not at all from its variation over countries – the sole basis for identification in *Table 3.1.* Note that unlike the cross-sectional regressions, the quality of democracy is measured by the variable *POLITY_GT,* available over the entire 1960-98 period (and also comparable over time). Higher values of this variable continue to denote worse democracies.

Table 3.2 about here

The results show that the share of old people, openness, and a better democracy, continue to be positively related to spending, thereby confirming

the results from the cross-sectional regressions of *Table 3.1.* The level of income now has a positive estimated coefficient, as expected from Wagner's Law.

This sample of nearly 2000 observations includes all the available data in our panel. For some of these countries and years, the political system can not be described as democratic, however, due to the rule of military juntas or other restrictions of democratic rights. This may be of little importance here, but not in Chapter 8, where we test for the predicted effect of the constitutional form in well-functioning democracies. As further discussed in Chapter 4, we therefore restrict the sample to years of democratic rule. Column 2 shows the same specification as column 1 under this restriction, which means dropping about 350 observations. Most country panels are still quite long: their average length is 26.2 years (out of the 39 from 1960 to 1998). Income, openness, and demographics retain their earlier sign and significance pattern. But the quality of democracy now exerts a positive and non-significant influence on the size of government. This change in signs and the lower precision in the coefficient strongly suggest that the estimate in column 1 captures a threshold effect, namely growth of government in connection with transitions from dictatorship to democracy (of about 1% of GDP according to the parameter estimate). But marginal changes in the quality of democracy among established democracies have no significant effect on spending.

While this finding is thought-provoking, our interest in this book is not the effect of democracy, but that of different democratic constitutions. Since a number of observations are missing for the variable $POLITY\_GT$, which plays only a small role in the sample of democracies, the rest of the table shows the results when this variable is not included among the regressors. The specifications reported in the remaining columns 3-6 (and the corresponding samples) are those used in the detailed analysis of Chapter 8 below.

When interpreting the results in columns 1 and 2, it is important to keep in mind that income has a strong upward trend in most countries over this time period, as has the share of old people and openness. As we saw above, there is also an upward trend in the average size of government, so that the estimated effect of these variables might well be spurious. To rule out this possibility, we use the specification in column 3, which is identical to column 2, with one important difference (besides the omission of the quality of democracy). We now also add "year fixed effects" – i.e., a set of year

dummies – to the right-hand side of the regression. In the same way as country dummies pick up the country averages over all years, these year dummies pick up the year averages over all countries. Including the year-dummies, we clean the estimates of the impact of jointly trending variables. Now, the demographic variables and openness retain their expected signs, but the estimate of income becomes much smaller and turns statistically insignificant. The likely explanation is that the strong effects in columns 1 and 2 at least partly reflect upward trends in both income and spending (including or omitting $POLITY\_GT$ makes no difference).

A measure of the cumulative growth of government over a certain period of time can be obtained by taking the difference between the estimated coefficients on the last and the first year dummy of the period (which is preferable to using the simple year averages plotted in *Figure 3.1*, as the country fixed effects take care of the potential problem of countries with different (average) sizes of governments entering and exiting the panel at different times). This measure suggests a cumulative rise in government spending of about 12% of GDP in the twenty-five year period from the early 1960s to a peak in the early 1980s, and a subsequent fall-back of about 3% of GDP from that peak until the late 1990s.

The size of the government is likely to change relatively slowly over time, and with a great deal of inertia. This has, so far, not been taken into account in the estimates. A simple way of capturing these dynamics is to add the lagged (one-year) size of government ($LCGEXP$) to the right-hand side of the regression, which we do in column 4.[5] As the estimates show, there is indeed a strong positive inertia in expenditures: a coefficient of 0.8 means that 80% of a change in spending in a given year remains in the next year. The other right-hand side variables generally retain their earlier signs, but lose statistical significance, especially income and openness. Furthermore, the point estimates are smaller in absolute value, which is natural as the specification now allows the adjustment to a shock to spread out over time. The fit of the regression increases considerably, so that we explain over 80% of the variation in the dependent variable, as opposed to 50% in the previous columns.

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[5]The addition of the lagged dependent variable to a panel regression can create statistical problems with biased estimates, particularly when the panel is short. As stated above, we have more than 26 years for the average country panel, so that this problem is likely to be relatively small in our case. Chapter 8 includes a more extensive discussion of the prospective methodological problems and possible ways around them.

The year dummies capture the total effects of *common* unobserved economic and political shocks to the countries in our panel. But a few of the most salient common shocks over this period may also be observable. An obvious example is the oil shocks hitting the world in the seventies and eighties. To gauge these common shocks, we use the price of oil in US dollars. Because this variable is common for all countries in the sample, we remove the fixed year effects to avoid perfect colinearity. As the effect of an increase in oil prices is likely to be quite different for oil exporters and importers, we interact the oil price with dummy variables for oil exporters and importers (which allow a country's net export status to shift over time), thus creating the variables OIL_IM and OIL_EX.

We also add to the regression idiosyncratic economic shocks, in the form of a country-specific business cycle. Specifically, we take the (log) difference between real GDP in the country and its trend, when the latter is computed with the so-called Hodrick-Prescott filter. With that definition, we can interpret the resulting variable ($YGAP$) as the deviation of aggregate output from its trend value in percent, a measure sometimes called the output gap. *Figure.3.2* displays the frequency distribution of these output gaps, pooling together all observations in our default sample (in the figure, each data point has been approximated by its closest integer). Quite a few output gaps take on extremely large positive and negative values. To avoid drawing inference from such extreme outliers, we drop all output gaps exceeding 5% in absolute value from the sample.

Column 5 of *Table 3.2* shows the results when these additional variables, OIL_IM, OIL_EX and $YGAP$, are added to the previous specification. All of these are strongly significant with the expected sign. For oil importers, a rise in the oil price and a negative output gap raises spending as a fraction of GDP. Thus, the ratio of government spending to GDP is counter-cyclical (meaning that government spending does not move in proportion to income during the business cycle, or even in the opposite direction). A negative output gap on the order of 3% of GDP is associated with a higher spending level of about $\frac{1}{3}$ % of GDP, whereas an oil-price hike of 10\$ raises the ratio of government spending to GDP by about $\frac{1}{2}$ a percentage point. Oil exporters raise their spending more than oil importers, thereby suggesting a direct effect on spending via government income. Demographics now regains some of its explanatory power (perhaps because the oil price does not purge the effects of common trends as effectively as the time dummies).

Finally, column 6 relies on the same specification, but with government

revenue rather than expenditures as the dependent variable. The results are essentially the same for most variables, except that output gaps are no longer statistically significant and the estimated coefficients on oil prices are smaller. This makes intuitive sense (at least for oil importers): tax revenue in nominal terms is likely to be more sensitive to the state of the economy than government spending, implying a smaller reaction when scaled to GDP.

Figure 3.2 about here

3.2.2 Composition of government

When discussing the composition of government spending, we focus on welfare-state programs as a percentage of GDP. We measure the size of these programs by the level of social security and welfare spending by central government (SSW), which includes programs such as pensions and unemployment insurance. The source of this variable is the GFS database of the IMF. In the 1990s cross section, data on SSW are available for 72 countries out of 85. In the 1960-1998 panel, we only miss data on SSW for a few countries.

We use this variable to test the predictions sketched in Chapter 2 of (geographically) targeted vs. non-targeted spending under different constitutional rules. In advanced industrial countries, it is certainly the case that broad social transfer programs like pensions and unemployment insurance cannot be finely targeted towards narrow geographical constituencies, whereas spending on goods and services can. Hence, SSW measures the size of broad redistributive programs likely to benefit large groups in the population, as opposed to narrow geographical constituencies. Whether the interpretation of this variable also applies to developing countries is less evident: in such countries, the size of social welfare spending is generally very small, and often directed towards urban residents.

Like the size of government, welfare-state spending varies a great deal over time and place. The mean value of SSW in the 1990s cross section is 8.1% of GDP and its standard deviation 6.6%. The maximum value (for Sweden) is 22.4% and the minimum (for Bangladesh) 0.1%. The distribution in a given year of the 1960-1998 panel has a similar range. From the early 1970s to the 1990s, the average level of welfare spending across countries rose by about 2.5% of GDP .

The basic determinants of social transfers are likely to coincide with those of overall spending. Column 1 of *Table 3.3* thus shows the result from the same basic regression in the 85-country cross section as column 1 of *Table 3.1,* but now with *SSW* as the dependent variable. These seven left-hand side variables in this basic specification explain almost 80% of the cross-country variation in social transfers. As the table shows, the coefficients have the same sign as the size regression, but only one of them is significantly different from zero. While the share of old people still exerts a strong influence, openness and federalism no longer appear as important determinants when it comes to this aspect of policy. Column 2 reports the results when adding geography and history in the form of continental and colonial indicator variables which, once more, are not statistically different from zero. There are few changes in the other coefficients.

For the overall size of government, we have experimented with a few alternative specifications, but we do not report the results in *Table 3.3.* Not surprisingly, the size of the mining sector has no explanatory power in this case, nor do income inequality, population size or the degree of heterogeneity. Basically, we can explain about 80% of the cross country variation in social security and welfare spending on the basis of just a few variables; but the only one systematically estimated to have a statistically significant effect is the share of elderly in the population.

Table 3.3 about here

The two remaining columns in *Table 3.3* show estimation results from the 60-country panel. We first rely on a specification with the basic time-varying regressors, fixed country effects, lagged welfare spending (*LSSW*), oil prices (separately for oil exporters and importers) and country-specific output gaps. In column 3, the sample is only restricted by data availability. Column 4 adds the restrictions to years of democratic governance, output gaps less than 5% in absolute value and drops the quality of democracy variable (which overlaps with the sample and the specification used in Chapter 8). As in the case of overall spending, we find strong inertia in welfare spending. A higher share of old people is correlated with higher spending, as expected. More trade is now associated with less welfare spending, in contrast to what might be expected from the argument in Rodrik (1998), although the point estimate is small. Output gaps, but not oil shocks, have a significant and negative effect on welfare spending, particularly in the restricted sample excluding

the exceptional output gaps. According to the estimates in column 4, an income fall of 3% below the trend in the average country is associated with higher transfers by about $\frac{1}{4}$% of GDP in the same year, followed by further increases in subsequent years (due to the high positive coefficient on lagged transfers). The quality of democracy is estimated to only raise spending when the data include significant democratic transitions (column 3) (in the sample of better democracies, the variable *POLITY_GT* is never significant even if included). Furthermore, the share of old people has a less pronounced effect in the more restrictive sample (column 4).

3.2.3 Budget surplus

Our final fiscal policy outcome is the government budget balance. We measure this by the size of the budget surplus of the central government (*SPL*), once more in percent of GDP. The source is the IFS database of the IMF. Data are available for 75 countries in our nineties' cross section. The average country in this sample runs a deficit amounting to 2.2% of GDP; i.e., the mean for *SPL* is negative. While the standard deviation is 3.5%, the range in the sample runs from whopping deficits – the highest being 11.4% of GDP (in Greece) – to surpluses – the highest being 12.4% of GDP (in Singapore). In the first decade of the 1960-98 panel, there is less variation across time. But the 1970s and 1980s see the average country going more heavily into deficit, while dispersion grows across countries. In the 1990s, there is instead a general trend towards fiscal consolidation. When we take averages over the whole 1960-98 period for the 60 countries for which data are available, the mean deficit is 2.9% of GDP, with a standard deviation of 2.4%. Israel is the country with the largest average deficit (about 11% of GDP), while Botswana has the largest average surplus (about 4% of GDP) throughout this period.

Table 3.4 presents the results of a set of cross sectional and panel regressions, with the surplus as the dependent variable. The specification is the same as for the previous fiscal policy regressions. Here, we only explain quite a small part of the cross country or time variation, suggesting that there may be relevant omitted variables. In particular, our specification neglects variables measuring the availability of funds to specific sovereign borrowers. Some governments may be more risky borrowers than others, and they may face borrowing constraints; but none of our included variables controls for that.

The cross-sectional estimates in columns 1, 2 and 3 suggest that richer countries have better fiscal balances than poor ones. The same is true for countries more dependent on international trade. But these results only appear in the 1990s data, and not in the longer sample. African and Latin American countries appear to have smaller deficits than countries belonging to the OECD, though the estimated coefficients are not statistically different from zero. This might reflect borrowing constraints in international financial markets, rather than a lower propensity to borrow. But here, it is important to add the ceteris-paribus qualifier: an income difference of two standard deviations corresponds to a larger average deficit of 3% of GDP, according to the point estimates in columns 1 and 2. Previous British colonies have worse budget outcomes (compared to non-colonialized countries), but the difference is only statistically significant in the longer time average.

A country cannot keep running large budget deficits forever, without becoming insolvent. If a theory makes predictions about tendencies to run budget deficits under specific political systems, these predictions apply to the stock of government debt in the steady state, not to the budget deficit itself. This suggests that, in studying the determinants of budget deficits in a cross section of countries, we must avoid focusing on too short a time period (particularly if it is a period of budgetary consolidation, like the 1990s). For this reason, when we estimate the constitutional effect on the budget balance from cross-country variation in Chapter 6, we only report results from data averaged over the whole period 1960-98 (corresponding to column 3 in Table 3.4). The results for the 1990s are quite similar, though.

Table 3.4 about here

Columns 4 and 5 display the panel estimates. Column 4 refers to the full sample, while column 5 is restricted to that used in Chapter 8 (democracies only, extreme output gaps dropped), and the same specification (the quality of democracy variable omitted). Country fixed effects are always included, while year fixed effects are not. Deficits, like other fiscal instruments, have a strong positive inertia: the lagged deficit has a precisely estimated positive coefficient taking on the same value in these and other specifications. Openness to trade retains its positive influence from the cross-sectional regressions in all specifications. For demographics, we obtain a more plausible result than in the cross-sectional estimates, namely a larger number of working-age people and a smaller number of old people improve the surplus (although the

latter not significantly so). The effects of oil prices are also sizeable among oil exporters, though not among importers: an oil price hike of $ 17 – the change in *OIL* in the second oil crisis in 1979 – reduces the surplus to GDP ratio by almost 1 percentage point $(0.05 \cdot 17)$ in the same year. This suggests that a considerable part of the higher government revenues is spent, though presumably, the estimated coefficient also captures some of the rise in GDP associated with a higher oil price (cf. also *Table 3.2*). Positive output gaps do increase the budget surplus, as expected, but only when extreme gaps are dropped (column 5).

3.3 Rent extraction

3.3.1 Measuring corruption

An empirical counterpart to rent extraction by politicians is not easily available in a large cross section of countries. Given the theory reviewed in Chapter 2, an ideal measure would focus on illegal political rents. Clearly, real-world abuse of a higher political office can take the form of outright corruption and, more generally, misgovernance. We use three different measures in the empirical work to follow; two of which refer to corruption, the third to effectiveness in the provision of government services.

As Tanzi (1998) observes, it is difficult to define corruption in the abstract. Moreover, as corruption is generally illegal, violators try to keep it secret. Cultural and legal differences across countries make it hard to investigate corruption without taking country-specific features into account. Good proxies for political corruption should thus offer reliable information on the unlawful abuse of political power, as well as a strong level of comparability across different countries.

The Corruption Perceptions Index (CPI) goes some way towards meeting these requirements.[6] Produced by Transparency International, an organization disseminating and compiling information about world-wide corruption, this index measures the "perceptions of the degree of corruption as seen by business people, risk analysts and the general public". Each score ranges from 0 (perfectly clean) to 10 (highly corrupt). It is computed as the simple average of a number of different surveys, assessing each country's performance in

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[6] A number of recent empirical studies of corruption have employed this index, including Fisman and Gatti (1999), Treisman (2000) and Wei (1997a and 1997b).

a given year. The yearly score thus includes information from many sources. For example, the 1998 score is based on 12 surveys from 7 different institutions, and the 1999 score on 14 surveys from 10 sources.   As discussed at length in Lambsdorff (1998), the results of these surveys are highly positively correlated: the pair-wise correlation coefficient among different surveys on average exceeds 0.8, thereby suggesting that the independent surveys really measure some common features. Dispersion across the surveys in the ranking for an individual country is an indicator of measurement error in the average score constituting the CPI. For this reason, we typically weigh observations with the (inverse of the) standard deviation among the different surveys available for each country.

We take the average of these yearly country scores from 1995 to 2000 for the countries in the nineties cross section. This variable, called *CPI9500*, is one of our measures of corruption. It is available for 72 countries, with a mean of 4.8 and a standard deviation of 2.4. The lowest recorded value is 0.3 (for Denmark) and the highest 8.3 (for Honduras and Paraguay).

An alternative corruption measure is based on a similar collection of surveys presented and discussed in Kaufman et al (1999). Here, the original surveys apply to 1997 and 1998. The observed survey results are combined into different clusters of governance indicators by a statistical, unobserved-components procedure. We use their sixth cluster called "Graft". According to the authors, this particular cluster captures the success of a society in developing an environment where fair and predictable rules form the basis of economic and social interactions. Perceptions of corruption also play a central role here. The original surveys range from -2.5 to 2.5, with higher values corresponding to less corruption. We thus invert and re-scale this measure, which we also call *GRAFT*, to the same 0-10 scale as *CPI9500.* In this case as well, we will often weight the observations by the standard deviation of the original surveys.

While *GRAFT* is based on a shorter time interval, and is less focused on "grand political corruption" than *CPI9500*, it has the advantage of being available for 81 of the countries in our cross section except three. It has a mean of 4.2, a standard deviation of 1.9, a minimum of 0.7 (for Denmark), and a maximum of 6.9 (for Paraguay). Notwithstanding the a priori differences, it is strongly correlated with *CPI9500* (the simple correlation coefficient is 0.97).

Another cluster of governance indicators presented by Kaufman et al (1999) instead focuses on surveys of government effectiveness (once more re-

ferring to 1997-1998). Here, the purpose is to combine perceptions of the quality of public-service provision, the quality of the bureaucracy, the competence of civil servants and their independence from political pressures. These scores are also re-coded on the same 0-10 scale as the other measures, with higher values meaning lower effectiveness, thereby producing the variable *GOVEF.* Like *GRAFT,* it is available for 81 countries. *GOVEF* has the same average as *GRAFT* (4.2), a slightly lower standard deviation (1.7), and ranges from 0.8 (for Singapore) to 7.3 (for Zimbabwe). While supposedly measuring other aspects of government performance, it is still highly correlated with the corruption measures (the correlation is 0.91 with *CPI9500* and 0.95 with *GRAFT*). In the next subsection, we refer to a number of empirical studies that relied on these measures of corruption.

We have not included panel data on corruption. The only available data are those produced by ICRG from the mid 1980s and onwards – a fairly short period for such slowly moving variables as individual perceptions. Moreover, several of the determinants of corruption emphasized in our analysis are either time invariant or not readily available over such a time interval. Nevertheless, Persson, Tabellini and Trebbi (2002) have also analyzed these panel data, and their results reinforce those of the cross-sectional estimates reported in Chapter 7.

3.3.2 Determinants of corruption

Earlier empirical work based on cross-country data has identified a number of economic, social, cultural, historical and geographical variables associated with the incidence of corruption. We do not attempt at an exhaustive review of that literature here, but refer the reader to the discussion in recent studies by Treisman (2000) and Persson, Tabellini and Trebbi (2002) and the references in these papers. Based on these studies, we select a number of variables for our basic empirical specification.

Some of these variables have already appeared in our discussion of fiscal policy above. Thus, a country's economic and political developments are likely to correlate with the rent extraction by politicians. As in the case of fiscal policy, we measure these aspects by our democracy index (*GASTIL*), the level of income per capita (*LYP*) and the indicator for OECD membership (*OECD*). Because earlier work has shown openness to trade and a decentralized political structure to be negatively correlated with corruption (see Ades and di Tella, 1999, and Fisman and Gatti, 1999, respectively), we

include our measure of openness (*TRADE*) and our indicator for federalism (*FEDERAL*) in the basic specification.

Based on the existing literature, we also include some other country characteristics, one of which is population size, measured in millions and expressed in natural logarithms (*LPOP*). Several recent studies have found a higher fractionalization of the population in the linguistic or ethnic dimension to be a significant determinant of misgovernance (see e.g., Mauro, 1995 and La Porta et al 1999). We use one widely available measure for linguistic and ethnic fractionalization, which is itself put together as an average of five different indexes (*AVELF*). This measure goes from 0 to 1, with higher values corresponding to more fractionalization. It is also likely that a more educated population will suffer less from rent extraction by politicians. To allow for this possibility, we use a comprehensive measure of the country's level of education (*EDUGER*), measuring primary and secondary school enrollment in percent of the relevant age group in the population (the source is Unesco). Several authors have also found religious beliefs to be significantly associated with more or less corruption (see e.g., Treisman, 2000). To allow for this possibility, we use the population shares with a Protestant or Catholic religious tradition as measured in the 1980s (*PROT80* and *CATHO80*), which vary continuously between 0 and 1, and a dummy variable for Confucian dominance (*CONFU*).

The first three columns of *Table 3.5* show the results when each of our three measures of rent extraction are regressed on the eleven variables mentioned in the above discussion. As mentioned above, the estimation is by Weighted Least Squares, using the inverse of the standard deviation in the dependent variable as a weight. Together, these basic co-variates explain about 80-85% of the variation in the dependent variables. Once the effect of these observable determinants has been removed, the unexplained range of variation in corruption (i.e., of the estimated residuals of the regressions) is generally + 1 or -1 around the mean, with outlier countries reaching up to +2.5 or -2.5 around the mean. Most of the right-hand side variables have an estimated coefficient with the same sign in the three columns. Despite a great deal of colinearity amongst these regressors in our 85-country sample, some of them clearly stand out as more important and statistically significant determinants of rent extraction. As expected, higher-income countries have less corruption and more effective governments. According to these point estimates, all of them around unity, it takes a higher income level by about two standard deviations ($2 \cdot 0{,}96 = 1{,}92$) to reduce corruption or ineffective-

ness by one standard deviation (see above). Membership in the OECD has a significant negative effect on rent extraction of roughly the same magnitude. More open economies also seem to have less corruption: the coefficient on *TRADE* may appear small at first sight, but the standard deviation of this variable is 47.7. The evidence on religion is more mixed, but when it comes to corruption (columns 1 and 2), the estimates suggest that more Protestants and fewer Catholics are helpful in restricting corruption. Finally, and contrary to Treisman (2000) who found federal countries to be more corrupt, federalism does not appear to be a significant determinant of rent extraction.

Table 3.5 about here

In column 4 of this table, we add geography and history to the specification, with *GRAFT* as the dependent variable. More precisely, we add the indicators for continental location and the (discounted) indicators for colonial history. Most of the results from column 1 are unaffected, except that Protestant rather than Catholic religion now has a significant influence. Furthermore, the inclusion of the continental indicators makes OECD membership barely insignificant. The continental indicators themselves are never statistically significant (recall that the default group is the democracies of Central and Eastern Europe and the Middle East). When it comes to colonial history, being a former British Colony has a negative effect on corruption.

An alternative aspect of institutional history concerns the history of national legal systems. Here, we use a set of legal origin indicator variables taken from La Porta et al. (1998). These authors extensively analyzed the impact of these indicators on various measures of government efficiency, while Treisman (2000) studied their effect on corruption, attempting to separate the legal framework as such from colonial influences on a country's "legal culture" (expectations of the efficiency of the legal system as a whole). The indicators classify the origin of legal systems into five different categories: Anglo-Saxon common law, French civil law, German civil law, Scandinavian law and Socialist law. We use the first four of these categories, creating four dummy variables: *LEGOR_UK*, *LEGOR_FR*, *LEGOR_GE*, and *LEGOR_SC*. The default is thus the countries with a socialist legal origin.

Column 5 reports on a regression identical to that in column 4, except that we substitute the legal-origin variables for the colonial-origin variables (the two sets are strongly correlated). As the table shows, Anglo-Saxon and Scandinavian legal origins have the strongest negative effects on corruption,

relative to the default. Not surprisingly, Anglo-Saxon legal origin seems to pick up the same features as British colonial origin (when including both variables at the same time, each has a negative sign but neither is significant), but the effect of legal origin seems more important. Scandinavian legal origin and a large share of Protestants apparently capture similar country characteristics – and low perceived corruption levels in Scandinavia – as the share of Protestants becomes insignificant, once we add the legal-origin dummies. For the rest, the results are not considerably affected.

3.4 Productivity and policy

The ultimate measure of the lasting success of economic policy is its impact on the level of economic development. As discussed in Chapter 2, it is natural to ask whether and how different constitutional rules influence economic development.

When studying economic development, it is useful to distinguish between two different types of questions. One concerns the aggregate accumulation of knowledge that can *potentially* be applied to the productive process at any given moment in time. What determines shifts in the knowledge frontier over time? This question is crucial for understanding why the US or other leading industrial countries keep growing over time, and why they are so much richer now than 50 or one 100 years ago. But this does not take us very far if our goal is to understand differences in the level of development across countries at the same point in time.

The second type of question concerns how different countries actually apply already available knowledge to their productive processes. Why do some countries only apply a fraction of existing technologies to the production of goods? And why are other countries so much more efficient in exploiting innovations and incorporating knowledge at the frontier? This second type of question is crucial to understanding differences in international income.

Recent contributions by economists have emphasized that institutions and structural economic policies determine the incentives of firms and individuals to adopt efficient productive techniques and hence, these are the main factors explaining differences in the level of development among countries (see, in particular, Hall and Jones, 1997 and Parente and Prescott, 2000). In this section, we describe two summary measures of development that will be more thoroughly studied in Chapter 7. We also introduce some observable

features of economic policies and institutions/regulations that seem to promote efficient productive techniques. Finally, we discuss additional historical and geographical variables that have been found to explain the adoption of good policies and institutions. As these variables are only available at one point in time, they are included in the 85-country cross section, but not in the 60-country panel. An influential paper by Hall and Jones (1999) is the source of these data, and the inspiration for our empirical analysis of these issues.[7]

3.4.1 Measuring productivity and growth promoting policies

Economic development and economic performance can be measured in many different ways and from many different angles. Hall and Jones (1999) have compiled data on two measures of productivity for a large sample of countries. The most comprehensive measure is labor productivity, i.e., output per worker, and is called $LOGYL$. As a rough correction for differences in the availability of natural resources across countries, this measure is computed by removing the output of the mining sector from the total value added. The second measure is total factor productivity (also in logs), called $LOGA$, which is computed as a residual, after imputing a fraction of output per worker to both physical and human capital. Thus, labor productivity measures the amount of aggregate output produced by an average worker (net of the output produced in the mining sector), whereas total-factor productivity measures the average efficiency with which labor is used, after taking into account its average education and the average capital per worker. Both variables are measured as logarithms of levels and refer to 1988. Since they are expressed in common international prices and refer to the same point in time, cross-country comparisons are possible.

Output per worker is available for 75 countries in our 85-country cross

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[7]The idea that institutions are the key to understanding economic development has a long and honored tradition among historians, political scientists and economists; see, for instance, North (1981), Mokyr (1990) and Engerman and Sokoloff (2000). But the contributions by Hall and Jones (1997, 1999) have spurred a recent wave of empirical and theoretical research, including, in particular, Parente and Prescott (2000), Acemoglou, Johnson and Robinsion (2001), Easterly (2002), Easterly and Levine (2002), and Acemoglou, Aghion and Zilibotti (2002). Reviewing this rapidly growing literature is beyond the scope of this chapter.

section, while total-factor productivity is available for 74 countries. Both measures display considerable variation: the output per worker (in logs) varies from 6.95 for Malawi to 10.48 for the US, meaning that a typical US worker produces about 3.5 times more output than a worker in Malawi. The mean of this variable is 9.23, and its standard deviation is 0.90. Total factor productivity displays about the same range of variation: from 6.28 (in Zambia) to 9.01 (in Italy), with a mean of 8.18 and a standard deviation of 0.61. In fact, these two measures are highly correlated: their correlation coefficient is 0.87. Thus, differences in total factor productivity seem to be a major reason behind cross-country differences in output per worker, with differences in education and capital per worker playing an additional role. Parente and Prescott (2000) stress the crucial role played by total factor productivity in explaining international income differences. But the high correlation between output per worker and total factor productivity could also reflect measurement error in computing total factor productivity.

Hall and Jones (1999) showed that the cross-sectional variation in output per worker and total factor productivity can largely be explained by two policy and institutional variables. One (called $YRSOPEN$ and originally compiled by Sachs and Werner 1995) measures the number of years a country has been open to international trade during the period 1950-94. The other (called $GADP$) measures perceptions of economic and institutional environments encouraging the production of output rather than its diversion. Diversion can take on various forms, such as theft, corruption, litigation and expropriation. This variable is similar to the perceptions of corruption described in the previous section; it has been compiled by Knack and Keefer (1995) using ICRG data. It is measured over the period 1986-95 and consists of a simple average of five indicators; two of which relate to the role of the government in protecting property rights against private diversion (law and order, and bureaucratic quality); the other three to the role of the government itself as a source of diversion: corruption, risk of expropriation and government repudiation. Both variables vary from 0 to 1, with higher values indicating better policies (more protection of property rights or lower barriers to trade). The mean and standard deviations of the anti-diversion policy indicator ($GADP$) are 0.69 and 0.20; those of openness to trade ($YRSOPEN$), 0.47 and 0.35. According to both indicators, Bangladesh has the worst policies and Switzerland the best. Not surprisingly, the indicator of anti-diversion policies is highly correlated with the indicator of corruption described in the previous section: the correlation coefficient between $GADP$ and $GRAFT$ is

-0.87 (recall that higher values of $GRAFT$ denote more corruption). The correlation coefficient between $GADP$ and $YRSOPEN$ is smaller, namely 0.64.

More recent empirical contributions have used similar indicators of institutional environments, encouraging productive economic activities, as opposed to rent seeking or appropriation of output produced by others. Acemoglou, Johnson and Robinson (2001) focus on protection against the risk of expropriation (one of the components of the $GADP$ indicator), also originally compiled by Knack and Keefer (1995). Easterly and Levine (2002) rely on the broader indicator estimated by Kaufman, Kraay and Zoido-Labaton (1999). Their variable measuring a "good" institutional environment aggregates over 300 indicators ranging from ratings of country experts to survey results, and measuring absence of corruption, protection of property rights and respect for the rule of law, light regulatory burden, government effectiveness in the provision of public services, political stability and freedom.[8] All these aggregate indicators (or their components) are highly correlated, and measure similar features of an economic and institutional environment. It is not very clear what formal features of political and economic institutions are responsible for these perceptions, which is the main limit of this type of empirical analysis. Yet, as we will see, the underlying features measured by these indicators seem to play an important role in fostering economic development.

3.4.2 Determinants of productivity and growth promoting policies

Naturally, neither of these policy measures can be taken as exogenous to economic development: it is likely that they influence as well as are influenced by the level of development. One of Hall and Jones' (1999) main ideas was to suggest that some observable historical and geographic features of a country exclusively influence productivity through their impact on the policy and institutional environments as measured by $GADP$ and $YRSOPEN$. That is, Hall and Jones (1999) propose a number of "instruments" that can be used to isolate exogenous variation in these two policy variables, and thus estimate their effect on productivity (readers not familiar with instrumental-variable

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[8]The variables $GRAFT$ and $GOVEF$ discussed in the previous section are components of this broader index by Kaufman, Kraay and Zoido-Labaton (1999).

estimation are referred to Chapter 5, and the references mentioned therein, for a detailed discussion of such techniques in a cross-sectional setting).

The instruments proposed by Hall and Jones (1999) are four. The first two are direct measures of cultural influence: the fractions of the population speaking English as their mother tongue (called $ENGFRAC$), or speaking one of the five primary European languages (including English) as their mother tongue (called $EURFRAC$). The sources are Hunter (1992) and Gunnemark (1991). Naturally, the fraction of individuals with English as their mother tongue is much higher among former British colonies, with a mean of 0.29, vs. a mean of 0.04 in the rest of the sample. But, contrary to what might be expected, the percentage of English-speaking is not just another way of measuring colonial origin: the correlation coefficient between $ENGFRAC$ and British colonial origin is only 0.38. Thus, colonial origin and the diffusion of English as a mother tongue measure somewhat different aspects of a country's history.

The third variable measures geographic location: distance from the equator, measured as the absolute value of latitude and re-scaled to lie between 0 and 1 (called $LAT01$). This variable also measures cultural and historical influences. Countries closer to the equator provided a more inhospitable environment for the first settlers from Western Europe. These regions of the world were thus colonized later and, as argued by Acemoglou, Johnson and Robinson (2001), were mainly used by the West to exploit natural resources rather than as settlements for its migrants.[9] Engerman and Sokoloff (2000) also show that in these tropical regions, agricultural production mainly took the form of large plantations, where slave labor was the main factor of production. Whatever the specific argument, the distance from the equator proxies for different patterns and influences of Western colonization.

The fourth and last variable is a composite measure: the (log) predicted trade share of the economy based on a gravity model of international trade by Frankel and Romer (1996), relying on a country's population and geographic features (called $FRANKROM$). The predicted trade share measures the physical endowments and the geographic location of the country.

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[9]Indeed, the mortality rate of European soldiers in former colonies in the early 19th century is much higher, the closer they are to the equator. The data on settlers' mortality collected by Acemoglou, Johnson and Robinson (2001) are only available for 36 of the countries in our dataset. Among these, the correlation coefficient between the logarithm of settlers' mortality and the variable $LAT01$ is - 0.48. Countries with higher settlers' mortality are also much younger democracies in our sample.

Some of these variables are also used as instruments for the constitution in our work; we show (in Chapter 4) that they are correlated with several of our constitutional measures.

Table 3.6 reproduces some of the findings of Hall and Jones (1999) in our own 85-country cross section. Columns 1 to 4 present linear regression estimates of a reduced form, where the four instruments, latitude ($LAT01$), the fraction of the population whose mother tongue is English or another European language ($ENGFRAC$ and $EURFRAC$), and the gravity measure ($FRANKROM$) are used to explain the two productivity measures and the two policy indicators. In our data set, these four regressors explain over 50% of the variation in output per worker ($LOGYL$) and antidiversion policies ($GADP$), but they explain a smaller part of total factor productivity ($LOGA$), and almost no part of the trade policy indicator ($YRSOPEN$).

Latitude is a very important variable, in that both productivity and policies improve with the distance from the equator. Acemoglou, Johnson and Robinson (2001) show that in their data set, this largely reflects the correlation of latitude with settlers' mortality: once the latter is also included in their regressions, latitude tends to become statistically insignificant. The same occurs in our sample of countries: when adding settlers' mortality to the regressions reported in columns 1-4 of *Table 3.6*, the effect of latitude vanishes. Since the overlap of our data set with that of Acemoglou, Johnson and Robinson (2001) is limited to 36 countries, we do not report these results, however.

Among the language variables, more English speakers are associated with better policies (with a significant effect on antidiversion policies), while more speakers of another European language are associated with higher productivity, but not with better policies. Finally, the gravity indicator is almost never statistically significant.

Table 3.6 about here

Columns 5 and 6 instead show estimates of the impact of the two policy and institutional indicators on productivity, under the restriction that the four instruments only indirectly affect productivity through policy. Thus, we estimate by two-stage least squares (see Chapter 5 for a discussion of this estimation method). In the first stage, the endogenous institutional and policy variables ($GADP$ and $YRSOPEN$) are regressed on the four instruments – the same regressions as in columns 3 and 4. In the second stage,

productivity is regressed on these institutional and policy variables – the results displayed in columns 5 and 6. Anti-diversion policies ($GADP$) have a strong and significant effect on output per worker, with the expected sign. Trade policies also enter with an expected (positive) sign, but the estimated coefficient is (weakly) statistically significant only in the case of total factor productivity. Note that the fit of the total factor productivity regression is quite low, thereby suggesting that the dependent variable might be measured with considerable error.[10] Finally, the over-identifying restriction for the validity of the four instruments (once more, see Chapter 5 for further discussion) is rejected in this sample: according to the data, at least some of the instruments appear to exert a direct influence on productivity, over and above their impact on the two policy indicators. Since Hall and Jones (1999) could not reject this over-identifying restriction, this indicates that their results are fragile to the sample of countries.

Some of the variables introduced in earlier sections to explain fiscal policy or rent extraction might also have an effect on productivity, either directly or indirectly through the Hall and Jones policy indicators. To explore these possibilities, *Table 3.7* extends the reduced and structural forms of *Table 3.6,* allowing for a less parsimonious specification. As in the previous table, columns 1 to 4 display results from a reduced-form estimation of productivity and policies. Besides the four instruments used by Hall and Jones (1999), we have added our measure of federalism ($FEDERAL$), our set of colonial-origin dummy variables, and our dummy variables for geographic location.[11]

Several interesting results emerge. On the one hand, geography remains relevant: the distance from the equator (measured by $LAT01$) retains its explanatory power, though the coefficient is less precisely estimated. But other measures of geography now become highly significant, with Latin America

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[10]Hall and Jones (1999) had constrained $GADP$ and $YRSOPEN$ to enter the productivity equation with the same coefficient, but as shown in *Table 3.6,* this constraint is strongly rejected in our sample. Since there is no a priori reason to impose such a constraint, we let the two policy variables enter with different coefficients.

[11]We have not added quality of democracy as an explanatory variable, for it would be endogenous in this setting . But we return to this issue in Chapter 7, where we also include the age of democracy in each country among the regressors. We have also experimented with some other regressors used in earlier sections, such as the indicators of religious beliefs and population size, but they do not seem to have robust and general effects on the dependent variables of *Table 3.6* (except for a hard to interpret negative and significant estimated coefficient of the share of protestants ($PROT80$) in the reduced form for total factor productivity).

and Africa being associated with lower productivity and worse policies. The relevance of geography is also confirmed by the predicted share of trade from the gravity model ($FRANKROM$), which is now always statistically significant with a positive estimated coefficient. History and culture, on the other hand, seem less important: the estimated coefficients on language variables and colonial origin are generally not statistically different from zero, except for $EURFRAC$ which has a positive effect on productivity and institutions. This lack of significance might be due to some of these variables measuring similar historical heritages, however. Finally, political centralization also plays an important role, with federal countries having higher productivity and better institutions and policies. The fit of all regressions naturally improves, even though we still explain a very small part of the variation in the trade policy variable ($YRSOPEN$).

Table 3.7 about here

Columns 5 and 6 of *Table 3.7* report the two-stage least squares estimates. The first stage coincides with columns 3 and 4. In the second stage, productivity is regressed on the two policy indicators ($GADP$ and $YRSOPEN$), but also on colonial origin other than Britain and Spain (COL_OTHA) and the dummy variable for Latin America ($LAAM$). The choice of these additional control variables has been made with the criterion of not violating the over-identifying restrictions on the remaining instruments. Thus, with this second-stage specification, the instruments for the two institutional and policy indicators are the four Hall and Jones (1999) instrumental variables ($LAT01$, $ENGFRAC$, $EURFRAC$ and $FRANKROM$), plus federalism, the dummy variables for Africa and Asia, and British and Spanish colonial origin. As shown in columns 5 and 6, with this second-stage specification, we can no longer reject the over-identifying restrictions at the 10% confidence level. The results of the previous specification are confirmed: both institutional and policy variables are highly statistically significant and better policies and institutions are associated with much higher levels of productivity. Antidiversion policies ($GADP$) have an exceptionally strong effect on the output per worker. The estimated coefficient of 4.26 implies that the different values of anti-diversion policies between, say, Switzerland and Spain, can account for twice the distance in output per worker between these two countries. If Spain could improve its institutional environment, cutting the

distance to Swiss institutions and policies by half, it would thus have the same output per worker as Switzerland.

Some of the basic insights of Hall and Jones (1999) and the subsequent related literature are thus confirmed in our data set: two indicators of anti-diversion and trade polices appear to be strongly associated with economic performance. Countries with a better protection of property rights and less corruption (higher values of $GADP$) and more open access to international trade (higher values of $YRSOPEN$) also have a higher productivity of labor.

But the identifying assumptions of Hall and Jones (1999) are rejected. This is important, because it implies that other country characteristics, prox-ied by colonial origin and geographic location, also shape productivity over and above their impact on two central policy variables. Moreover, the spe-cific constitutional and political determinants of these good economic and institutional environments remain rather mysterious. What is the $GADP$ variable really measuring, and why does it lead to higher levels of produc-tivity and more efficient methods of production? One of the goals of the empirical analysis in Chapter 7 is to shed some further light on what specific features of the political constitution – if any – might lead to the adoption of better economic and regulatory policies and hence, to a stronger economic performance.

Figure 3.1
Size of Government 1960-98
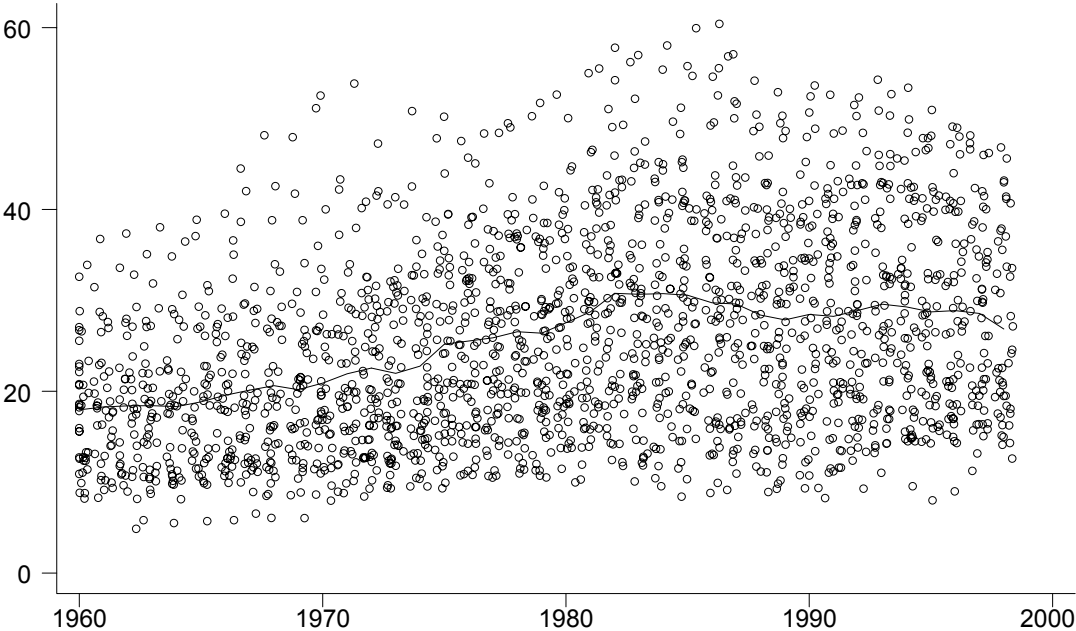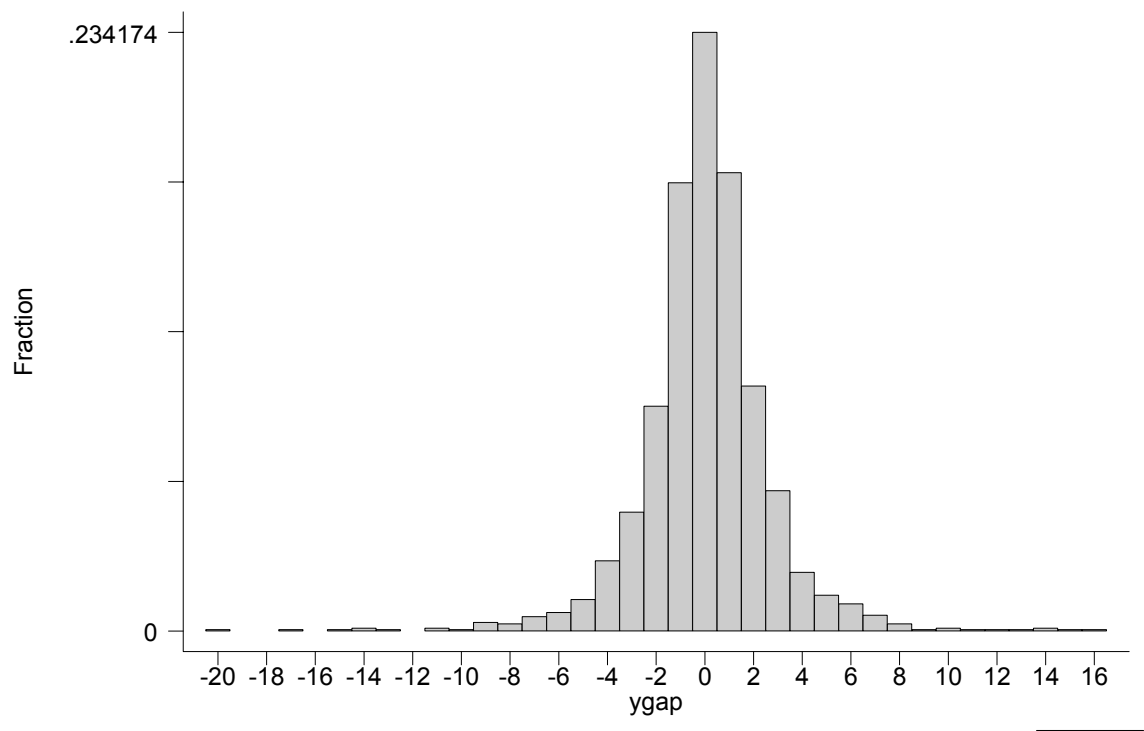
Table 3.1
Size of government and its determinants
Cross section estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	CGEXP	CGEXP	CGREV	CGEXP	CGEXP	CGEXP
GASTIL	-2.08	-2.27	-1.82	-1.19	-1.68	-0.49
	(1.24)*	(1.18)*	(1.29)	(1.14)	(1.16)	(1.02)
LYP	-1.64	0.05	2.75	-0.50	-1.10	1.15
	(2.15)	(2.10)	(1.80)	(2.14)	(2.23)	(2.10)
TRADE	0.05	0.03	0.06		0.04	0.03
	(0.02)**	(0.02)	(0.02)***		(0.02)	(0.02)
PROP1564	-0.32	-0.30	-0.34	-0.14	-0.18	-0.74
	(0.34)	(0.36)	(0.32)	(0.34)	(0.38)	(0.36)**
PROP65	1.65	1.10	1.06	1.66	1.24	2.39
	(0.43)***	(0.42)**	(0.41)**	(0.43)***	(0.45)***	(0.57)***
FEDERAL	-4.56	-4.78	-4.76	-4.79	-4.07	-3.54
	(2.25)**	(2.54)*	(2.50)*	(2.75)*	(2.82)	(2.95)
OECD	-0.21	-3.71	-4.07	-0.97	-1.98	-10.21
	(3.67)	(3.99)	(4.19)	(3.88)	(3.88)	(4.20)**
AFRICA		-2.83	4.26		-3.81	-5.49
		(4.57)	(5.19)		(4.24)	(4.89)
ASIAE		-7.05	-2.56		-7.08	-8.47
		(3.16)**	(3.27)		(3.03)**	(3.49)**
LAAM		-9.01	-4.66		-8.06	-12.22
		(3.13)***	(3.96)		(2.81)***	(3.60)***
COL_ESPA		0.53	0.87		0.36	6.89
		(5.55)	(4.90)		(4.76)	(5.05)
COL_UKA		2.59	0.22		1.90	2.29
		(3.13)	(2.68)		(3.03)	(2.37)
COL_OTHA		-1.11	-0.55		-2.22	-5.11
		(3.03)	(3.08)		(2.89)	(3.15)
LPOP				-0.88		
				(0.59)		
AVELF				3.96		
				(4.15)		
MINING_GDP					0.28	0.17
					(0.09)***	(0.12)
GINI_8090						0.15
						(0.16)
Obs.	80	80	76	80	75	63
Adj. R2	0.59	0.65	0.64	0.57	0.68	0.75

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.2
Size of government and its determinants
Panel estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*	*CGEXP*	*CGREV*
POLITY_GT	-0.26	0.35				
	(0.10)**	(0.34)				
LYP	3.52	5.94	1.14	0.57	0.86	0.55
	(0.79)***	(0.90)***	(0.93)	(0.57)	(0.53)	(0.48)
TRADE	0.05	0.05	0.06	-0.00	0.00	0.01
	(0.01)***	(0.01)***	(0.01)***	(0.01)	(0.01)	(0.01)*
PROP1564	-0.12	-0.18	-0.38	-0.04	-0.12	-0.04
	(0.06)*	(0.07)***	(0.08)***	(0.05)	(0.04)***	(0.03)
PROP65	2.36	1.99	1.45	0.21	0.19	0.17
	(0.14)***	(0.15)***	(0.18)***	(0.11)*	(0.09)**	(0.08)**
LCGEXP				0.80	0.79	
				(0.02)***	(0.01)***	
OIL_IM					0.05	0.04
					(0.02)**	(0.02)**
OIL_EX					0.07	0.03
					(0.01)***	(0.01)***
YGAP					-0.11	-0.04
					(0.04)***	(0.03)
LCGREV						0.83
						(0.01)***
Country effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	No	No	Yes	Yes	No	No
Sample	Full	Democratic	Democratic	Democratic	Democratic |yshock|<5	Democratic |yshock|<5
Obs.	1941	1609	1594	1550	1452	1405
Countries	60	60	60	60	60	59
R2	0.35	0.39	0.49	0.82	0.83	0.83

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
R2 refers to within-R2

Table 3.3
Welfare spending and its determinants
Cross section and panel estimates

	(1)	(2)	(3)	(4)
Dep. var.	*SSW*	*SSW*	*SSW*	*SSW*
GASTIL/ POLITY_GT	-0.67	-0.62	-0.07	
	(0.46)	(0.56)	(0.03)***	
LYP	0.18	0.33	0.18	0.34
	(0.76)	(0.95)	(0.24)	(0.28)
TRADE	0.01	0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.00)***	(0.00)***
PROP1564	-0.16	-0.12	-0.02	-0.02
	(0.13)	(0.14)	(0.02)	(0.02)
PROP65	1.34	1.29	0.09	0.05
	(0.15)***	(0.27)***	(0.04)**	(0.04)
FEDERAL	-0.15	-0.58		
	(1.19)	(1.23)		
OECD	-1.78	-2.05		
	(1.81)	(2.11)		
AFRICA		0.66		
		(2.00)		
ASIAE		-0.99		
		(1.87)		
LAAM		-0.47		
		(2.23)		
COL_ESPA		3.39		
		(3.16)		
COL_UKA		-1.43		
		(1.73)		
COL_OTHA		-1.72		
		(1.30)		
LSSW			0.80	0.80
			(0.02)***	(0.02)***
OIL_IM			0.01	0.01
			(0.00)	(0.01)
OIL_EX			0.01	0.00
			(0.00)*	(0.00)
YGAP			-0.03	-0.08
			(0.01)***	(0.01)***
Country effects			Yes	Yes
Year effects			No	No
Sample			Full	Democratic |yshock|<5
Obs.	69	69	1092	890
Countries			58	56
Adj. R2	0.79	0.80	0.75	0.77

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Adj. R2 in panel regressions (columns 3-4) refers to within-R2

Table 3.4
Government surpluses and their determinants
Cross section and panel estimates

	(1)	(2)	(3)	(4)	(5)
Dep. var.	*SPL*	*SPL*	*SPL*	*SPL*	*SPL*
GASTIL/ POLITY_GT	-0.01	0.06	-1.67	0.10	
	(0.68)	(0.70)	(0.96)*	(0.05)**	
LYP	1.69	1.66	0.03	-0.50	-0.37
	(0.80)**	(0.75)**	(0.92)	(0.39)	(0.49)
TRADE	0.03	0.03	0.01	0.02	0.01
	(0.01)**	(0.01)***	(0.02)	(0.00)***	(0.01)**
PROP1564	-0.11	-0.08	0.01	0.11	0.12
	(0.14)	(0.14)	(0.10)	(0.03)***	(0.04)***
PROP65	-0.16	-0.12	-0.14	0.05	0.05
	(0.14)	(0.18)	(0.13)	(0.06)	(0.08)
FEDERAL	-0.02	0.18	0.41		
	(0.86)	(0.87)	(0.70)		
OECD	-2.02	-1.31	-0.57		
	(1.46)	(1.69)	(1.58)		
AFRICA		2.69	4.32		
		(2.27)	(2.58)		
ASIAE		1.06	2.50		
		(1.59)	(1.56)		
LAAM		1.83	1.37		
		(1.79)	(1.27)		
COL_ESPA		1.19	-1.18		
		(2.49)	(2.24)		
COL_UKA		-2.27	-4.35		
		(1.71)	(1.41)***		
COL_OTHA		0.75	-1.53		
		(1.47)	(1.76)		
LDFT_SPL				0.71	0.71
				(0.02)***	(0.02)***
OIL_IM				-0.00	0.01
				(0.02)	(0.02)
OIL_EX				-0.05	-0.04
				(0.01)***	(0.01)***
YGAP				-0.00	0.06
				(0.02)	(0.03)*
Country effects				Yes	Yes
Year effects				No	No
Sample	1990s	1990s	1960-90s	Full	Democratic
	Broad	Broad	Broad		|yshock|<5
Obs.	72	72	60	1832	1427
Countries	72	72	60	60	60
Adj. R2	0.30	0.38	0.32	0.57	0.54

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Adj. R2 in panel regressions (columns 3-4) refers to within-R2

Table 3.5
Rent extraction and its determinants
Cross section estimates

	(1)	(2)	(3)	(4)	(5)
Dep. var.	GRAFT	CPI9500	GOVEF	GRAFT	GRAFT
GASTIL	0.12	-0.11	0.17	0.15	0.18
	(0.16)	(0.21)	(0.17)	(0.17)	(0.17)
LYP	-0.87	-1.06	-0.93	-0.97	-0.79
	(0.25)***	(0.30)***	(0.25)***	(0.24)***	(0.27)***
TRADE	-0.01	-0.00	-0.01	-0.01	-0.01
	(0.00)**	(0.00)	(0.00)**	(0.00)*	(0.00)**
FEDERAL	0.05	-0.04	0.28	0.15	0.06
	(0.31)	(0.39)	(0.32)	(0.31)	(0.35)
OECD	-1.41	-2.19	-1.29	-0.92	-0.55
	(0.36)***	(0.51)***	(0.38)***	(0.46)*	(0.58)
LPOP	0.05	0.21	-0.09	0.01	0.01
	(0.11)	(0.15)	(0.11)	(0.12)	(0.12)
EDUGER	-0.01	-0.02	-0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
AVELF	-0.40	-0.94	-0.78	0.71	0.47
	(0.51)	(0.64)	(0.53)	(0.63)	(0.62)
PROT80	-0.01	-0.01	-0.01	-0.01	-0.00
	(0.01)	(0.01)**	(0.01)	(0.01)*	(0.01)
CATHO80	0.01	0.01	-0.00	0.00	0.00
	(0.00)**	(0.00)*	(0.00)	(0.00)	(0.00)
CONFU	0.48	0.18	0.06	0.53	0.67
	(0.47)	(0.71)	(0.51)	(0.52)	(0.60)
AFRICA				-0.39	0.05
				(0.51)	(0.66)
ASIAE				-0.09	0.39
				(0.53)	(0.67)
LAAM				0.77	0.83
				(0.50)	(0.64)
COL_ESPA				-1.36	
				(1.08)	
COL_UKA				-0.75	
				(0.41)*	
COL_OTHA				0.46	
				(0.39)	
LEGOR_UK					-1.37
					(0.60)**
LEGOR_FR					-0.60
					(0.61)
LEGOR_GE					-0.98
					(0.74)
LEGOR_SC					-1.52
					(0.89)*
Obs.	78	68	78	78	78
Adj. R2	0.83	0.87	0.79	0.86	0.86

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Estimation in all columns by WLS, using [1/std(Dep. var.)] as the weight

Table 3.6
Productivity and growth promoting policies
Hall and Jones (1999) variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	LOGYL	LOGA	GADP	YRSOPEN	LOGYL	LOGA
LAT01	3.08	1.48	0.74	0.53		
	(0.42)***	(0.31)***	(0.09)***	(0.21)**		
ENGFRAC	0.25	-0.02	0.12	0.12		
	(0.24)	(0.19)	(0.05)**	(0.15)		
EURFRAC	0.70	0.59	0.03	0.01		
	(0.17)***	(0.13)***	(0.04)	(0.15)		
FRANKROM	0.11	0.10	0.02	0.07		
	(0.09)	(0.07)	(0.02)	(0.06)		
GADP					3.10	0.79
					(0.88)***	(0.74)
YRSOPEN					1.19	1.40
					(0.89)	(0.80)*
Chi2: over-id					9.22***	7.69**
Estimation	OLS	OLS	OLS	OLS	2SLS	2SLS
Obs.	74	73	75	75	73	73
Adj. R2	0.54	0.35	0.55	0.00	0.59	0.24

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Chi2: over-id refers to the statistic for testing the over-identifying restriction that the instruments included in first-stage regressions in columns 3 and 4 do not enter the second-stage regressions in columns 5 and 6.

Table 3.7
Productivity and growth promoting policies
Other determinants

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	LOGYL	LOGA	GADP	YRSOPEN	LOGYL	LOGA
GADP					4.26 (0.71)***	1.72 (0.61)***
YRSOPEN					0.93 (0.47)*	1.11 (0.43)**
LAT01	1.00 (0.59)*	0.31 (0.45)	0.37 (0.15)**	-0.01 (0.52)		
ENGFRAC	0.35 (0.25)	0.05 (0.21)	0.08 (0.05)*	0.37 (0.26)		
EURFRAC	0.45 (0.24)*	0.52 (0.21)**	0.10 (0.05)**	0.05 (0.17)		
FRANKROM	0.20 (0.09)**	0.18 (0.07)**	0.04 (0.02)	0.21 (0.07)***		
FEDERAL	0.46 (0.16)***	0.27 (0.13)**	0.09 (0.03)***	0.07 (0.12)		
AFRICA	-1.25 (0.33)***	-0.62 (0.28)**	-0.11 (0.07)	-0.01 (0.33)		
ASIAE	-0.48 (0.30)	0.05 (0.24)	-0.06 (0.08)	0.60 (0.55)		
LAAM	-0.59 (0.24)**	-0.27 (0.21)	-0.21 (0.05)***	-0.25 (0.21)	0.92 (0.18)***	0.58 (0.18)***
COL_ESPA	-0.15 (0.52)	-0.70 (0.41)*	-0.11 (0.15)	-0.46 (0.44)		
COL_UKA	-0.24 (0.21)	-0.32 (0.18)*	-0.03 (0.06)	-0.63 (0.39)		
COL_OTHA	0.34 (0.29)	0.10 (0.24)	0.00 (0.07)	-0.63 (0.36)*	0.52 (0.25)**	0.33 (0.24)
Chi2: over-id					6.43	4.28
Estimate	OLS	OLS	OLS	OLS	2SLS	2SLS
Obs.	74	73	75	75	73	73
Adj. R2	0.69	0.48	0.65	0.19	0.69	0.37

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Chi2: over-id refers to the statistic for testing the over-identifying restriction that the instruments included in first-stage regressions in columns 3 and 4 do not enter the second-stage regressions in columns 5 and 6.

Chapter 4

Electoral rules and forms of government

4.1 Introduction

The theory surveyed in Chapter 2 attempts to explain how economic policy outcomes are influenced by electoral rules and forms of government. The empirical policy measures introduced in Chapter 3 are chosen to correspond as closely as possible to those appearing in the theoretical models. In this chapter, we outline the second piece necessary for laying the empirical puzzle: we describe how to measure and classify observed constitutions in a way consistent with the theoretical constructs.

As already mentioned, it is hard to find enough interesting variation in electoral rules and government regimes at the sub-national level, so we focus on constitutions at the nation-state level. We limit ourselves to democracies, because the theories we take as our starting point predict how alternative democratic institutions modify the incentives of voters and politicians. Thus, we must first face a primitive question: what do we mean by a democracy? More practically: how do we exclude countries that cannot be regarded as democracies from the two data sets introduced in Chapter 3? That question is addressed in Section 2 of this chapter.

In Section 3, we discuss how to measure the various characteristics of electoral rules in practice. This leads up to a simple classification of the countries in our samples into majoritarian, mixed and proportional electoral rules, as well as two continuous variables measuring the finer details of the

electoral system. In addition, we give a short account of the history and the recent reforms of electoral systems. In Section 4, we turn to the legislative rules implied by different forms of government, and provide a classification of countries into parliamentary and presidential regimes, also briefly discussing their historical origin.

A major feature of our data is that these constitutional measures are not randomly distributed across countries. In Section 5, we describe the observed correlations between constitutional rules and other variables, including a number of the socio-economic characteristics included in the previous chapter. We also take a first look at the relation between the constitution and the various policy and performance measures introduced in Chapter 3. At this point, we note some intriguing differences, but caution the reader not to jump to premature conclusions as to how constitutions might influence policy. As in the case of Chapter 3, additional details on our data and our sources, beyond those provided in the text, can be found in the Data Appendix at the end of the book.

4.2   Which countries and years?

Our empirical investigations rely on cross-country data, either in pure cross-section or panel format. In view of this, we have an obvious interest in maximizing the number of prospective observations. Given the scarcity of reliable outcome measures, particularly for the period from 1960 and onwards, our minimal requirement for a democracy is quite generous. In the coming chapters, however, we check carefully whether our results are sensitive to the chosen definition of democracy.

We primarily obtain data on democratic governance from two sources. One is the well-known surveys made and published by Freedom House since 1972, the other is the Polity IV data set, first described by Eckstein and Gurr (1975). Freedom House covers a large group of countries, but does not go far back in time. The Polity IV data go back to 1800, are more carefully drafted, and previous codings are updated to take account of subsequent changes in definitions. This data set does not cover the very small countries in our sample, however. For this reason, we rely on the Freedom House data set to select the 1990s cross section, our sample of contemporary democracies. But we mainly rely on the Polity IV data set to select the 1960-98 panel and use it exclusively to extract historical information. Where they overlap, the two

data sets lead to a similar classification of countries into democracies and non-democracies – see below.

4.2.1 Defining democracies

To define a democracy in the 1990s cross section of countries, we rely on the annual surveys published by Freedom House. The so-called Gastil indexes of political rights and civil liberties vary on a discrete scale from 1 to 7, with low values associated with better democratic institutions. According to each index, countries scoring 1 or 2 are "free", countries scoring from 3 to 5 are "semi-free", while those scoring 6 or 7 are "non-free". To arrive at these rankings, Freedom House uses the answers to a number of questions on a specific checklist. For political rights, this list involves items such as rulers being elected in free and competitive elections, a role for the opposition, freedom of organization; for civil liberties, it includes freedom of expression and assembly, rule of law, and so on.[1] We call the resulting variable $GASTIL$. A drawback of these measures is that changes in definitions in any given year do not change the codings of previous years, which makes the surveys less useful for comparisons over time. In a few cases detailed below, we also need to compute a measure comparable to $GASTIL$ for the period 1960-70, for which Freedom House data are not available. We then rely on the comparable measure compiled by Bollen (1990), available every five years and also going back to that period (we re-scale Bollen's measure onto a scale from 1 to 7, equivalent to that of the Freedom House data). Thus, the source for the variable $GASTIL$ in our data set is Freedom House when available, and Bollen rescaled for the period 1960-70.

To include a country in the 1990s cross section, we require a $GASTIL$ score lower than an average of 5 for the 1990-98 period. This rule permits 85 countries, which we call our *broad* sample. This sample includes some shady countries, however, such as Belarus and Zimbabwe (which also experienced significant cuts in democratic rights at the end of, and after, the sample period). For this reason, we always check whether the results of the empirical analysis are robust to imposing a stricter definition of democracy. Specifically, we define a *narrow* sample by only including those countries with an average $GASTIL$ score lower than 3.5. This cuts 20 countries from the data

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[1]A more precise description of the methodology can be found at: http://www.freedomhouse.org/research/freeworld/2000/.

set, leaving us with 65 prospective observations (subject to the availability of other variables).

For the 1960-98 panel, we instead mainly rely on the Polity IV data set. This represents the fourth wave of the well-known and encompassing historical study of democratic development initiated and first described by Eckstein and Gurr (1975). For each country, the data go back to as early as 1800, or the creation of an independent nation. This data set covers all independent nations with a population exceeding half a million people (both criteria refer to 1998). Specifically, we use the most encompassing POLITY index, which assigns an integer score ranging from -10 to +10 to each country and year, with higher values associated with better democracies. This index in itself constitutes the difference between two separate indexes (DEMOC and AUTOC in the original source). The former is an institutional measure of democracy with values from 0 to 10, based on the competitiveness and openness in selecting the executive, political participation, and constraints on the chief executive, whereas the latter scores autocratic limitations from 0 to 10 in the same dimensions of democratic rights. We adopt the original name, $POLITY,$ for this index in our two data sets. When the index is reconstructed, any changes in definitions are imposed on the entire historical data set, so as to allow comparability over time.[2]

We therefore rely on this index to define years and countries of democratic rule in our 1960-98 panel, with the following slight modification. For five small countries, Bahamas, Barbados, Belize, Malta and St. Vincent, the $POLITY$ index is missing. We use the $GASTIL$ scores whenever available to amend the series, creating the modified index $POLITY\_GT.$ (Specifically, having rescaled the variable $POLITY$ to make it comparable with $GASTIL$, we regress it on $GASTIL$ and use the predicted values from this regression to replace the missing observations; since we need to go back in time, the $GASTIL$ variable here includes the observations obtained from Bollen (1990)). We then restrict our panel to only include those countries and years with values of $POLITY\_GT$ below or equal to 3.666 (corresponding to positive values of $POLITY$). This rule permits a total of 60 countries in the panel, but some of these enter in some years only. As an example, the rule temporarily excludes countries like Turkey (intermittently in the 1970s and 1980s), Argentina (until 1972 and between 1976 and 1982) and Chile

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[2]More information about the Polity IV data can be downloaded from the web site: http://www.bsos.umd.edu/cidcm/polity/#data.

(between 1974 and 1988). Throughout, we treat these censored observations as randomly missing and we do not attempt to model this aspect of sample selection. We also perform some sensitivity analysis and occasionally restrict the sample with a stricter definition of democracy, corresponding to *POLITY* scores above 5 (the suggested boundary for a stable democracy).

Table 4.1 lists the sample of 85 countries included in the 1990s cross section, and the average quality of democracy in the 1990s as measured by *GASTIL* and *POLITY*. The 60 countries that also belong to the 1960-98 panel are indicated in the last column of the table.

Table 4.1 about here

The two alternative measures imply similar, albeit not identical, classifications of democracies and non-democracies for the nineties cross section where both indicators are available. An alternative criterion for inclusion in our 1990s cross section, consistent with our criterion for the 1960-98 panel, would be to insist on positive average values of *POLITY*. The resulting classification is somewhat stricter than the broad Gastil rule described above; it defines a set of 77 countries, where all observations have a *GASTIL* value below 4.3. The countries (above 0.5 million inhabitants) excluded by this Polity IV rule but included in our broad sample of democracies are Belarus, Gambia, Ghana, Malaysia, Peru, Singapore, Uganda, and Zimbabwe. Some sensitivity analysis suggests that the results reported in Chapter 6 through 8 are robust to this small modification of our default sample.

4.2.2 Dating democracies

The countries in our sample also differ in another important dimension, namely *how long* they have been democracies. This could be of empirical importance: mature democracies might adopt systematically different policies than young ones. For example, while welfare-state programs may be predominantly associated with democracies, it may take considerable time to decide on and build up programs such as public pension systems. Such a link is also suggested by the panel estimates in Chapter 3, showing that the quality of democracy only (positively) affects overall and welfare-state spending when democratic transitions are included in the sample. Alternatively, older democracies might have a better system of checks and balances to fight corruption and abuse of power.

We then date the birth of democracy by defining the variable DEM_AGE as the first year of a string with uninterrupted positive yearly $POLITY$ values until the first observation in 1990, given that the country is also an independent nation. In defining this variable, we do not regard foreign occupation during World War II as an interruption of democracy, as it was imposed externally rather than being the result of a coup. For the five smaller countries not included in the Polity IV data set, we rely on the modified $POLITY\_GT$ variable to identify the date when a country first became a democracy (all these smaller countries are young nations whose independence is dated after 1960, and they can typically be classified as democracies, already from the year of independence).

The resulting birth-of-democracy dates are listed in *Table 4.1.* They reflect historical waves of democratization that have swept over the world.[3] Some countries are very old democracies, going back well into the 19th century. Apart from these, a number of European states – such as the Nordic and Benelux countries – obtained a stable democracy status by extending the franchise, dismantling weighted votes, and undertaking other reforms, in the twenty years from the turn of the 19th century until the aftermath of World War I. Another set – including Germany, Italy and other countries relapsing into dictatorship in the interwar period – consolidated their democracies after World War II. Some former European colonies became stable democracies in the sixties and seventies. Finally, many former Latin American and Communist dictatorships became democracies through reforms in the last two decades of the 20th century.

The historical information discussed in this section will be exploited in the empirical analysis, when we require instrumental variables for the constitution (i.e., variables correlated with constitutional features but not with unobserved determinants of policy outcomes, see Chapter 5). We also include the age of democracy as an explanatory variable in the empirical analysis of Chapters 6 and 7 (based on $DEM\_AGE,$ we define $AGE = (2000 - DEM\_AGE)/200$, so that AGE varies between 0 and 1). As suggested above, we expect the age of democracy to be correlated with measures of performance, such as corruption and government spending. The age of democracy might influence performance in non linear ways, and the defini-

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[3]A classic work on different waves of democratization is Samuel Huntington's book from more than a decade ago (Huntington, 1991).

tion of the variable AGE may be too constraining.[4] For this reason, in some specifications we check the robustmess of our results when we include both a linear and a squared term in the variable AGE.

4.3 Electoral rules

Our theoretical discussion in Chapter 2 involved three aspects of electoral rules: (i) how many legislators get elected in each district (district magnitude), (ii) how vote shares are converted into seat shares (the electoral formula), and (iii) how voters cast their ballot on the spectrum from single individuals to party lists (ballot structure). While these aspects are analytically distinct, they are correlated across countries in the real world, as already noted in Chapter 2. In particular, electoral formulas and ballot structures, (ii) and (iii), are closely correlated: plurality rule is typically associated with voting over alternative individual candidates, whereas proportional representation (PR) is typically implemented by a system of party lists. But also district magnitudes and electoral formulas, (i) and (ii), co-vary systematically. The single-most common form of legislative elections in the world is the traditional UK first-past-the-post system, combining single-member constituencies with plurality rule in the elections to the lower house of the Parliament. On the other side of the spectrum, the 120 members of the Israel Knesset and the 150 members of the Dutch lower house are elected in single national districts where legislative seats are awarded by PR. Moreover, in full PR-systems with smaller primary voting districts, the regular seats are often combined with "adjustment seats" awarded in secondary – most often national – districts, so as to obtain a closer relation between overall national vote shares and seat shares in the legislature.

But the correlations are certainly not perfect. In particular, we find a number of "mixed" electoral systems. A well-known example is Germany, where voters have two ballots, electing half the 656 members of the Bundestag by plurality in single-seat electoral districts, and the other half by PR at a national level, so as to achieve proportionality between the national vote and seat shares.[5] Furthermore, a few PR-systems, such as the Irish one (see

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[4]For instance, Treisman (2001) reports that democracies older than 45 years have significantly less corruption compared to younger democracies.

[5]656 is the minimal number of seats in the German lower house. The actual number is often higher due to the so-called Überhangsmandate used to achieve an outcome closer

further below), do not rely on party lists.[6]

According to the theory in Chapter 2, the correlated features may either pull in the same or in different directions, depending on the performance measure. For the composition of fiscal policy, larger voting districts and the associated greater reliance on PR both pull the outcome in the direction of broad, rather than targeted, policies. But in the case of rent extraction, larger districts pull towards less rents, whereas the associated greater reliance on party-list oriented ballots pulls towards more rents. This motivates us to compile different measures of electoral rules.

4.3.1 Basic measures of electoral rules

We always classify the rules for electing the *lower house.* In the event of reforms, we date them by the year in which the first election took place under the new electoral rule, irrespective of when the reform was passed (more on this below).

Our most basic measure is a simple classification of the *electoral formula* into "majoritarian", "mixed" or "proportional" electoral rules, resulting in two binary indicator (dummy) variables, MAJ and $MIXED$. More precisely, countries electing their lower house exclusively by plurality rule in the most recent election are coded as $MAJ = 1$, whereas those relying on other (mixed or proportional) rules are coded $MAJ = 0$. The alternative indicator variable $MIXED$ is also defined on the basis of the electoral formula, taking on a value of 1 only in the electoral systems relying on a mixture of plurality rule and PR and a value of zero in pure plurality or PR systems. These two binary variables are relatively easy to collect and we have an entry for all countries and years in the 1960-98 panel, as well as in the nineties cross section. Another reason for relying on binary variables is that a binary measure is required by some of the statistical methods used to allow for a systematic (non-random) selection into different constitutions (to be discussed in Chapter 5). *Table 4.2* displays the values of the MAJ and $MIXED$ indicators for the 85 countries in our nineties' cross section.

Table 4.2 about here

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to full proportionality.

[6]See, for instance, Blais and Mascotte (1996) and Cox (1997) for recent and extensive overviews of the electoral systems in the world.

For a few countries in this table, MAJ and $MIXED$ take on values strictly in between 0 and 1. Because the entries in the table are computed as an average from 1990 to 1998, these are the countries that undertook sufficiently substantial electoral reforms in the last decade to change their classification according to our indicators. Four of these countries – Japan (in 1994), New Zealand, the Philippines, and Ukraine (all three in 1996) – went from a system where every lower-house legislator is individually elected by plurality rule to a German-style mixed system where some, but not all, legislators are instead elected via party lists and PR.[7] Only Fiji (in 1994) went in the opposite direction, replacing a mixed-member system with an exclusive reliance on plurality rule (and only single-member districts from the 1998 election). If we also add the four countries that changed their status according to the $MIXED$ measure, the movement towards "middle-of-the-road" mixed-member systems becomes even more apparent. Bolivia (1996), Ecuador (1996), Italy (1994) and Venezuela (1993) all replaced full PR with mixed-member systems. In addition, following the fall of communism, some new democracies, like Russia and Hungary, introduced a mixed-member system already in their first free elections.[8]

The nineties is, however, an exceptional decade in terms of the frequency of electoral reform, at least when it comes to the basic features of electoral systems. For instance, in our panel data set, the 1980s shows only two electoral reforms: Cyprus going from plurality rule ($MAJ = 1$) to PR ($MAJ = 0$) in 1981, and a brief experiment where France temporarily replaced plurality rule with PR during 1985-86. Moreover, in the 1960s and 1970s, we register no electoral reform sufficiently important to change the coding according to MAJ. All this stability reflects an inertia of electoral systems sometimes called an "iron law" by political scientists. We will return to this stability in several places throughout the book, as it has important consequences for how to design a convincing empirical strategy for identifying

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[7]The pre-reform Japanese system was based on the Single Non-Transferable Vote. Unlike other multi-seat plurality elections – where each voter in a district has as many votes as the number of seats – Japanese voters had only one vote in districts with three to five seats going to the candidates with the highest number of votes. Although subject to some dispute, most political scientists include the pre-reform Japan system among the plurality-rule systems.

[8]See Shugart (2001) and the collection of studies in Shugart and Wattenberg (2001) for a discussion of the forces behind the reforms and the political consequences of reform in these countries.

the causal effect of electoral rules on policy outcomes.

In the 85-country cross section, our observations do not refer to a particular year, but are averages over the period 1990-98. As mentioned above, some of our statistical tools (to be discussed in the next chapter) require that we measure the constitution as a binary (0 or 1) variable. The question then arises of whether to treat the countries that underwent a reform in the 1990s as 0's or 1's. Our rule is to measure the constitution according to *the earlier* part of the sample, on the argument that it takes some time before constitutional reform changes such slowly moving variables as the size of government or the average perception of corruption. Thus, Italy and Japan, which both had elections under the new rules in 1994, are coded as proportional and majoritarian, respectively, since that was the prevailing rule until their 1994 elections. Using an alternative timing convention produces similar empirical results.

In the remainder of this chapter and in the empirical analyses to follow, we always use and refer to the values of MAJ, $MIXED$ (and the regime indicator $PRES$ to be defined in the next section) as strictly equal to 0 or 1, unless otherwise noted, constructed with the dating convention stated above (namely, we code the constitution before the reform).

In this classification, 52 countries have proportional elections, while 33 have majoritarian elections. Only 9 countries in the cross-sectional sample have mixed electoral systems, all the others are either strictly majoritarian or strictly proportional. The few observations in the mixed group make it difficult to empirically estimate differences between mixed systems and either strictly majoritarian or strictly proportional ones. For this reason, much of the empirical analysis in later chapters focuses on the effects of the MAJ indicator, namely on differences between strictly majoritarian countries on the one hand, and mixed plus proportional countries on the other. In general, as noted below, the data do not reject the hypothesis that mixed and proportional countries can be lumped together as far as their policy outcomes are concerned. But with only 9 countries, the alternative hypothesis of important differences between proportional and mixed systems may be hard to refute.

4.3.2   Dating of electoral rules

It is not surprising that history has played an important role in shaping the electoral rules observed today. Some of the historical and cultural cir-

cumstances that shaped the electoral systems are peculiar to each individual country[9], while other determinants may be common for all countries in the data set. The various forces shaping constitutional rules – such as experience by other democracies, prevalent political and judicial doctrines, academic thinking – may shift systematically over time. These time-varying historical determinants may be very hard to gauge in terms of observable variables (although we make some attempts in this direction in Chapters 5-6). Due to the stability of electoral rules, however, they are likely to show up in systematically different distributions of electoral rules, if we compare constitutions dating back to different historical epochs.

To look for such patterns in the data, we date the origin of the electoral rules as classified by the variable MAJ. Specifically, we proceed as follows. First, we obtain the earliest possible date of the electoral rule for a particular country by checking two separate conditions: (i) when it became an independent nation and (ii) what is the value of DEM_AGE; i.e., from which year democracy has been uninterrupted until 1998, as defined by the $POLITY$ rule described above. We then take the later date of (i) and (ii). If the MAJ classification in that year is the same as today, without intervening changes, the initial year gives the age of the electoral rule. If the electoral rule has been reformed in the interim, the most recent reform date – provided that it changed the value of MAJ – gives the age of the electoral rule. When repeating this procedure for each country in our data set, we obtain a new variable, called $YEARELE$. Its value for the each country in our nineties' cross section is displayed in *Table 4.2*.

Table 4.2 about here

For the few countries pursuing electoral reforms in the 1990s, we display two values in *Table 4.2*: the original date of the current electoral rule (in brackets), as well the original data of the pre-reform rule. For example, Japan's previous plurality-rule system (based on the Single Non-Transferable Vote) originated in 1952, but was reformed in 1994 in favor of a mixed-member system, so both years enter the table.

Does the distribution of current electoral rules vary with age? To answer

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[9]Lijphart (1984b) and, more recently, Colomer (2001), provides useful discussions of the history behind past and present electoral rules in a number of countries. Boix (1999) and earlier Rokkan (1970) suggest strategic-choice theories based on the balance of power between existing and prospective political forces at the time of democratization.

this question, we consider four broad time periods, suggested by the discussion in Section 2 of this chapter: before 1920, 1921-1950, 1951-80, and after 1981. The frequency of majoritarian rules ($MAJ = 1$) does indeed appear to be systematically related to the age of the electoral rule.[10] While the overall frequency of majoritarian electoral rules in our nineties cross section is slightly above one third (33 countries out of 85), it is much lower (one seventh) in the 1921-50 period, but much higher (one half) in the 1951-80 period. We exploit this pattern in the empirical work to follow, by constructing three dummy variables corresponding to the periods 1921-50, 1951-80 and after 1981, which take a value of 1 if the current electoral rule originated in the respective period, and 0 otherwise. Other factors have certainly played an important role for the selection of electoral rules, but a pure timing effect as captured by these dummy variables generally retains predictive value, even as we hold constant other geographical and cultural variables.

4.3.3   Continuous measures of electoral rules

The binary variable MAJ is based on the electoral formula (aspect (ii) above), but correlated with district magnitude (aspect (i) above). This may be sufficient for investigating the predicted constitutional effect on fiscal policy, as the theory tells us that these aspects of the electoral system pull policy in the same direction. But this variable may be too blunt when the theoretical predictions are more subtle, as in the case of rent extraction. Therefore, we have also constructed two continuous measures; one measuring district magnitude, the other ballot structure (aspects (i) and (iii) above).

As several related measures in the political-science literature, $MAGN$ gauges the average size of voting districts, in terms of the number of legislative seats. Specifically, let $DISTRICTS$ be the number of districts, primary as well as secondary (and tertiary if applicable), and $SEATS$ the number of seats in the lower house. Then, we define $MAGN$ by:

$$MAGN = \frac{DISTRICTS}{SEATS} \ .$$

Thus, our measure is the *inverse* of district magnitude as commonly defined by political scientists; it ranges between 0 and 1, taking a value of 1 in a

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[10] As noted above, for the countries that underwent a reform in the 1990s, we code MAJ according to the existing rule before the reform.

UK-style system with single-member districts and a value slightly above 0 in an Israel-style system with a single national district, where all legislators are elected. By construction, the endpoints of its range coincide with the two possible values of MAJ, which simplifies the interpretation of the empirical results.

To check whether our results are robust, we also rely on an alternative measure of district magnitude collected and discussed by Seddon et al (2001). The variable SDM is defined as traditional measures of district magnitude (i.e., as $\frac{SEATS}{DISTRICTS}$), except that district magnitude is now a weighted average, where the weight on each district magnitude in a country is the share of legislators running in districts of that size. Seddon et al (2001) argue that this measure better reflects differences across electoral systems in the incentives for the typical legislator of appealing to a narrow constituency.

To gauge the ballot structure, we define another measure called $PIND$. As $MAGN$ (and SDM), this variable will mostly be used to investigate rent extraction (corruption) by politicians. In line with the theoretical career-concern model discussed in Chapter 2, we focus on the incidence of voting for individuals rather than party lists to capture the notion of individual rather than collective accountability. Specifically, our measure is defined by

$$PIND = 1 - \frac{LIST}{SEATS} \ ,$$

where $LIST$ denotes the number of lower-house legislators elected through party lists. We thus measure the proportion of legislators elected via a vote on individuals (as opposed to party lists). Like our continuous measure of district magnitude, this measure of ballot structure ranges between 0 and 1, taking the value of 1 in a plurality system with single-member constituencies and a value of 0 in a pure (list-based) PR-system.

The political-science literature often subdivides party-list systems into three types: closed-list, open-list (or preference), and panachage.[11] Closed-lists systems are the most common and do not allow voters to express a preference for individual candidates. Some open-list systems, as in Italy before the 1993 reform, allow voters to express a preference among candidates on a list, but the party list is still the default option for the voter. The latter is also true for the panachage practiced in Switzerland, which allows

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[11]Carey and Shugart (1995) provide a clarifying discussion. They also classify different electoral and list systems, on the basis of the likely incentives for individual politicians to cultivate a personal vote.

voters to express preferences across parties. A few open list-systems, such as those in Brazil and Finland, instead oblige voters to cast a single vote for an individual on one of the party lists. While these list systems introduce some intra-party competition, they share the fundamental property of other list systems that the allocation of seats is based on the pooled vote for the whole party list. In this sense, individual politicians are collectively accountable and face a free-rider problem when it comes to their individual performance, as in the career-concerns model of electoral accountability. For this reason, we set $LIST = SEATS$ in all these systems.[12]

In other PR systems, however, no pooling takes place at the party level, so that the election of individual politicians depends on their ability to attract votes, independent of the vote share of the party as a whole. This is the case of the Dáil Eireann in Ireland, which relies on the Single Transferable Vote, where voters are obliged to rank-order individual candidates. The same electoral formula is used in Malta. In these cases, we set $LIST = 0$.

Once more, we use an alternative variable compiled by Seddon et al. (2001) to check for robustness. This variable is called $SPROPN$ and measures the share of legislators elected in national (secondary or tertiary) districts rather than sub-national (primary) districts. As the forces of collective rather than individual accountability – the theoretical concepts stressed in Chapter 2 – may be at their largest for a politician running on a national party list, we sometimes use $SPROPN$ as an alternative to $PIND$ (naturally expecting the opposite sign in the estimated coefficient).

Information on current electoral arrangements for most countries is readily available from public sources, such as the Inter Parliamentary Union or the International Institute for Democracy and Electoral Assistance. But the detailed information required for coding $MAGN$ and $PIND$ is less easily available, as we move back in time. In many cases, we must consult national sources so as to obtain reliable data. Moreover, these variables will be of most use when investigating the constitutional determinants of rent extraction by politicians; and, as discussed in Chapter 3, reliable corruption data are mostly available only for the nineties. For these reasons, we have limited the data collection to this later period, so that the continuously measured variables only enter the 85-country cross section. *Table 4.2* displays

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[12]The precise party-list allocation formulas for distributing seats within each district (D'Hondt, modified St. Laguë, LR-Hare, etc.) do not immediately affect the individual candidate's career concern, and therefore, we do not distinguish between them.

the values of these two variables.

4.4 Forms of government

The theoretical studies surveyed in Chapter 2 highlight two features of the legislative rules entailed in different forms of government. One is the separation of powers in the legislative process between different political offices and different groups of legislators, producing a more effective accountability of politicians towards voters. The other is the presence of a confidence requirement that makes the executive accountable to the legislature, thereby producing greater incentives for legislative cohesion, i.e., to form stable majority coalitions of legislators, which support the government and vote together on economic policy decisions to avoid triggering a costly government crisis.

Our prototype of a presidential regime has a directly elected president fully in charge of the executive, with the executive not being accountable to the legislature for its survival, and with a clear separation of powers, not only between the president and congress, but also between congressional committees holding proposal (agenda-setting) powers in different spheres of policy. Conversely, in our prototype of a parliamentary regime, the executive is not directly elected but formed out of the majority of the legislature. Thus, it needs the continued confidence of a majority in the parliament to maintain those powers throughout an entire election period, and has considerable powers to initiate legislation.

Several real world constitutions correspond closely to these prototypes. The US is one example of a presidential regime, but not the only one. Most countries with an elected president do not have a confidence requirement, and the executive can hold onto its powers without the support of a congress majority. Likewise, in many real world parliamentary regimes, government formation must be approved by parliament, which can also dismiss it by a vote of no-confidence; and legislative proposals by the government get preferential treatment in the agenda of the assembly.

Nevertheless, even more than in our classification of electoral rules, some observed constitutions cannot easily be assigned to one model or the other. First, when it comes to the confidence requirement, the political-science literature emphasizes that semi-presidential regimes, such as France, combine an elected president with considerable executive powers and an important role for the government held accountable by the legislature (Duverger, 1980).

Within the group of semi-presidential countries, a further distinction between premier-presidential and president-parliamentary regimes is sometimes made on the basis of who holds the government accountable (Shugart and Carey, 1992) and who controls its formation. Moreover, among parliamentary states, the precise constitutional mechanisms for accountability towards the legislature, and thus the incentives for maintaining stable coalitions, also vary considerably. For example, the German rule of a constructive vote of confidence (any coalition voting the government out of office must come up with a new coalition), makes it more difficult to break up the government (Diermeier and Merlo, 2000) and thus weakens the incentives for legislative cohesion.

Second, when it comes to separation of powers, Shugart and Carey (1992) identify important differences in the relative powers of the president and the legislature, even among clear-cut presidential regimes. In Argentina and the US, e.g., the president has relatively weak legislative powers, mainly deriving from his ability of vetoing legislative bills as a whole (package veto). Other presidents, like in Brazil, have more extensive legislative powers, including line-item veto rights, restrictions on congressional rights of amending bills, and the possibility to legislate by decree. As stressed by Strom (1990), parliamentary countries also vary considerably in the extent to which agenda-setting powers are concentrated in the government vs. vested in parliamentary committees. For instance, UK and French cabinets clearly dominate the parliament, but Belgian and Danish governments must live with relatively powerful parliamentary committees, which also grant the opposition an influence (see Strom, 1990, the contributions in Döring, 1995, and Powell, 1989, 2000).

4.4.1 A basic measure of forms of government

As the above discussion indicates, it would be interesting to develop detailed measures of the two constitutional features discussed above. One would be based on different constitutional rules introducing separation of legislative powers, promoting electoral accountability. The other would be based on the constitutional provisions, such as different confidence requirements and rules for government formation, promoting legislative cohesion in the form of stable majorities. Fragmentary measures along these lines for subsets of the countries in our data sets do exist in the political-science literature (see the work mentioned above and the sources cited therein). But building detailed measures with a comprehensive coverage of the countries in our data sets is

a daunting task well beyond the scope of this study.

We therefore limit ourselves to the less ambitious task of making a crude classification of constitutions into presidential and parliamentary regimes. Thus, we introduce a binary variable called $PRES$, taking on values of either 1 or 0. Because data on the separation of powers are less readily available – and perhaps more disputable – we take our starting point in the other feature suggested by the theory, namely the existence of a government subject to a confidence requirement. If this feature is absent, we call the country presidential ($PRES = 1$), if present, we call it parliamentary ($PRES = 0$).

In most cases, the classification is straightforward, even though sometimes leading to results somewhat different from the popular conception. Thus, using the confidence requirement as a decisive criterion leads us to classify one or two countries without a popularly elected president as presidential. This is the case with Switzerland, where a coalition government is appointed by each newly elected assembly, but cannot be revoked before the next election. Similarly, in some circumstances, the Bolivian Congress – rather than the voters – elects the President who, in turn, forms the executive and is not subject to a censure vote from the legislature.

The situation is less clear-cut for some of the semi-presidential countries, however, where both the president and the legislative assembly have some control over the appointment and/or dismissal of the executive. In these cases, we classify a regime as parliamentary or presidential depending on whether such control primarily rests with the president or the legislative assembly.[13] Specifically, suppose that the legislative assembly has the right to censure the government, but shares this right with the president and plays no role in government formation. Then, the right of censure is less likely to be exercised by the assembly and the incentives to maintain stable legislative coalitions seem correspondingly weaker. We therefore code such regimes as presidential. This applies to Colombia, Ecuador and Peru. Instead, suppose that the legislative assembly has an exclusive right of censure and that the president does not have an exclusive and predominant role in government formation. Then, the right of censure can be used in the assembly as a credible threat to hold the coalition together and the incentives for legislative cohesion seem much stronger. According to this criterion, France and Por-

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[13]In principle, and according to some literature, the classification of these countries might also change over time, depending on whether the president and the assembly belong to the same party. But we abstain from such complications in what follows.

tugal are coded as parliamentary, since the legislature has an exclusive and unrestricted right of censure and the president nominates the government (or has an influence over it), but parliament must approve it. Finally, Finland and Iceland fall somewhere in between, since the assembly has an exclusive and unrestricted right of censure – as in France – but the president has a full right of appointment without requiring parliamentary approval – as in Peru. Since the right of censure is likely to be strategically more important than the right of appointment, we classify these regimes as parliamentary. The information underlying the classification of these borderline cases is extracted from Shugart and Carey (1992, Ch. 8).[14]

We have collected the $PRES$ indicator annually both for the countries in the nineties' cross section and those in the longer panel. The values for the 85 countries in our nineties' cross section are listed in *Table 4.2*. Given our rules for classification, we have a total of 33 presidential and 52 parliamentary regimes.

4.4.2 Dating of forms of government

Consistent with constitutional inertia in the broad features of political institutions, we observe almost no change in these classifications over time. Bangladesh shifts from a parliamentary to a presidential regime in 1991 (although it only enters the data set before 1991 according to the broad Gastil rule, but not according to the Polity rule). No other change in the regime classification is observed in the panel for the earlier decades, except a brief experiment with parliamentary rule in Brazil in the years 1961-63.

Like in the case of electoral rules, we want to know how the distribution across presidential and parliamentary regimes depends on the age of their constitutional provision. To that end, we determine the original date of the present regime classification (according to $PRES$) for each country. The procedure is completely analogous to that used in dating the present electoral rule classification (according to MAJ). Thus, we create another variable, $YEARREG$, the values of which are displayed in *Table 4.2*.

Here, we do find a monotonic development over time, if we divide history

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[14]More precisely, we use the classification of non-legislative powers in Table 8.2 of Shugart and Carey (1982). The borderline semi-presidential cases are those assigned a score of 0 in the column for Censure. Regimes scoring a sum of 8 in the two columns for Cabinet Formation and Dismissal are coded as presidential, those scoring a sum of 4 or less, as parliamentary (no country has a score between 4 and 8).

into the same broad time periods as in the previous section. A total of 13 present regimes have their constitutional origin in the period up to 1920. Only two of these – Switzerland and the US – are presidential. Similarly, two out of nine stable regimes from the 1921-50 period are presidential, namely Costa Rica and Sri Lanka. But the 1951-80 period produced seven stable presidential regimes out of a total of 23. The two most recent decades have meant a further increase in relative frequency: more than half, 22 out of 41, democratic regimes born since 1981 are presidential.

Naturally, forces other than the vogue of each historical time period may explain the higher frequency of presidentialism at later birth dates. For example, while the early birth dates of stable political regimes are associated with countries located in the old world, later birth dates are increasingly associated with countries in the new world, where the influence of European cultural and political traditions is presumably smaller. Furthermore, observers such as Linz (1987) claim presidential regimes to be more prone to military coups and other breakdowns of democracy than parliamentary regimes, which would bias the outcome towards a higher frequency of older (surviving) parliamentary regimes. This claim is far from settled, however (it is disputed by Shugart and Carey, 1992, e.g.). Be that as it may, we shall see that a pure time effect remains even as we hold constant country characteristics such as continental location and colonial history, as well as the age of the democracy (as measured by AGE defined above).

Comparing the values of $YEARELE$ and $YEARREG$ in *Table 4.2*, we see that only six countries have an electoral rule and a form of government dating back to different periods. Everywhere else, both constitutional features date back to the same period, oftenmost the broad period that gave birth to the democratic state – another confirmation that fundamental constitutional features, such as those captured by our classifications, are stable and rarely change. To reduce the number of variables measuring the historical aspects of the constitution, we summarize the origin of *both* constitutional features, that is, the electoral rule and form of government, by means of a single categorization. Specifically, we create three indicator variables, corresponding to the three periods mentioned above (1921-50, 1951-80, post 1981); these indicator variables take a value of 1 if *either* the current form of government *or* the current electoral rule (as measured by $PRES$ and MAJ) originate in the relevant sub-period, and a value of zero otherwise. Chapter 5 explains the statistical reasons for reducing the number of these historical-constitutional variables.

4.5   Our political atlas

It is convenient to summarize the simple classifications we have made of political institutions with a map. *Figure 4.1* thus illustrates the values of our indicators MAJ and $PRES$ as coded in 1998, the last year of data in the computation of the cross-sectional average for the 1990s. The colored portions of the map represent the 85 countries in that data set. Striped areas indicate presidential regimes ($PRES = 1$) and solid areas parliamentary regimes ($PRES = 0$), notwithstanding the shade. A darker shade indicates majoritarian elections ($MAJ = 1$) and a lighter shade proportional elections ($MAJ = 0$), notwithstanding the pattern. As revealed by the map, the least common system is the US-style (gray striped) combination of a presidential regime with majoritarian elections, with only 11 countries. But each of the other three combinations is well represented in the sample: 22 countries are proportional and presidential, 23 majoritarian and parliamentary, while 30 are proportional and parliamentary.

Figure 4.1 about here

Even a cursory look at the map indicates that different constitutions do not appear to have been randomly selected. The electoral rule does not exhibit a particular pattern in terms of development, but most Anglo-Saxon countries and previous UK colonies are majoritarian, while most of Europe and South America are proportional. Presidential regimes are largely confined to non-OECD countries (the only presidential regimes in the OECD are the US and Switzerland). Moreover, presidential regimes are overrepresented in the Americas, though the nineties cross section also includes several parliamentary Caribbean countries. Other presidential regimes are found in Africa and Asia and in former Spanish and Portuguese colonies.

Table 4.3 confirms this visual impression by reporting the fractions of majoritarian and presidential political systems by colonial origin, continental location and level of development. All former Spanish colonies are now presidential regimes, while over 70% of the former UK colonies have majoritarian electoral rules. Perhaps more surprisingly, over 70% of the Asian and African countries in our sample have majoritarian elections. Many (but not all !) countries in Latin America are presidential regimes. And OECD countries are prevalently parliamentary-proportional democracies.

Table 4.3 about here

Exploiting these and other historical and geographic variables, we can indeed explain a considerable fraction of the cross-country variance in constitutional rules. *Table 4.4* reports the results of probit regressions on our two main constitutional indicators (MAJ and $PRES$), under two specifications. One is more parsimonious, including British colonial origin, an indicator for Latin America, the three dummy variables defined above corresponding to the birth of the current constitutional features, plus the age of democracy.[15] The second is more comprehensive, adding another measure of geography ($LAT01$), and two indicators of cultural heritage, namely the fraction of the population whose mother tongue is English ($ENGFRAC$) or a European language ($EURFRAC$) – these variables are only available for a smaller number of countries. *Table 4.5* shows that historical and cultural variables predominantly explain the electoral rule, while geographic variables tend to explain the regime type. As expected, British colonial origin and English mother tongue always significantly contribute to predicting majoritarian electoral rules (and in the case of $ENGFRAC$ also parliamentary governments), while presidential regimes are more likely to be found in Latin America and close to the equator. This last result, that countries closer to the equator are more likely to be presidential, might seem surprising. A possible interpretation is that closeness to the equator is associated with a later wave of colonialization by the West and hence, a weaker influence of the predominant form of government in Europe.

Note that the three dummy variables dating the origin of the current constitution remain statistically significant (jointly and in some cases even individually) in all specifications, even after controlling for the other geographical and historical or cultural variables. We exploit this result in later chapters, when trying to isolate exogenous variation in (finding instruments for) constitutional rules.

Table 4.4 about here

This non-random pattern of constitutional rules in our data sets raises a fundamental question. Can we really treat the constitution as exogenous in the empirical analysis of policy performance? Concern for this question is a major theme in the empirical analysis of subsequent chapters. But before

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[15]The other colonial origin indicators and continental dummy variables were not included, otherwise, we could perfectly have predicted the constitutional state of several countries.

addressing this issue, let us see how the constitution correlates with policy outcomes and other variables that are, a priori, likely to influence these outcomes.

4.6 Constitutions, performance and co-variates: a first look

4.6.1 Constitutions and outcomes

In this section, we take a provisional look at how the policy and performance measures introduced in Chapter 3 vary across constitutions. *Table 4.5* shows the mean and standard deviation of fiscal policy, rent extraction, and productivity outcomes in our nineties cross section, grouped by the binary indicators of the regime ($PRES$) and the electoral rule (MAJ and $MIXED$) introduced in this chapter.

The first three columns of the table split this cross-sectional sample according to the electoral rule. Columns 1-3 report the mean values of each policy outcome by the electoral rule. Columns 4-6 report the *p*-values for equal-means tests across electoral systems, comparing majoritarian vs. mixed, mixed vs. proportional, and majoritarian vs. proportional, respectively. Notice that these tests should just be interpreted as a convenient way of describing the data, and should definitely *not* be given any causal interpretation (see further below).

Majoritarian elections ($MAJ = 1$) are associated with a smaller overall size of government ($CGEXP$), smaller welfare spending (SSW), and larger budget surpluses (smaller deficits) (SPL) than proportional and mixed systems. The differences between majoritarian and proportional countries are large (and statistically significant): 5% of GDP for the two spending variables, and almost 2% of GDP for the budget deficit. $MIXED$ electoral systems are in between the two extremes as far as the two spending variables are concerned and have an even larger deficit than proportional countries. Given the small number of countries with mixed electoral rules, the standard errors are large, however, so we cannot reject that they have the same mean for all fiscal policy variables as either proportional or majoritarian countries. Rent extraction ($GRAFT$) and the indicator of anti-diversion policies ($GADP$) do not seem to vary systematically with the electoral rule, however. Nevertheless, labor and total factor productivity ($LOGYL$ and $LOGA$, respectively)

are correlated with electoral rules: both measures of productivity are lower in majoritarian countries, though the difference is not very large. In this case, mixed systems are very similar to proportional countries.

The last three columns of *Table 4.5* split the nineties' cross section by form of government. Parliamentary regimes ($PRES = 0$) have much larger governments than presidential regimes ($PRES = 1$), the difference is as large as 11% of GDP. The same is true for welfare spending (5% of GDP). Moreover, parliamentary regimes are less corrupt ($GRAFT$ is lower, which corresponds to the perception of a less widespread abuse of power) and they have policies conducive to growth ($GADP$ is higher, which corresponds to better policies). These differences are also large. Finally, parliamentary regimes are associated with higher values of labor and total factor productivity. Only budget deficits do not seem correlated with the form of government.

Table 4.5 about here

As discussed in Chapter 3, we are also interested in the *time variation* of the fiscal policy measures. *Table 4.6* gives the results for the full 60-country panel, and its breakdown according to forms of government and electoral rules. It then displays the average values of fiscal policy outcomes in each group of countries, for sub-periods of five years between 1960 and 1998.

Panel (a) considers the size of government, displaying both the average value of central government spending ($CGEXP$) in each five-year period, as well as its cumulative change ($DCGEXP$) over the period, expressed as percentages of GDP. Since the number of countries varies over time according to data availability, the time variation in the size of government is best captured by the change in the columns. In the early 1960s, parliamentary countries already have larger governments than presidential countries, whereas governments in proportional and majoritarian countries have about the same size. As already discussed in Chapter 3, government spending increases in all countries in the 1970s and the first half of the 1980s. The growth of government is especially rapid in parliamentary countries, but also in proportional countries. (A further breakdown into four constitutional groups shows that the most rapid growth indeed takes place in the proportional-parliamentary sub-group.) Moreover, the acceleration starts earlier (already in the mid 1960s) in the parliamentary and proportional groups, and later (in the mid 1970s) in the presidential and majoritarian groups. The late 1980s and the 1990s are periods of government retrenchment everywhere. All in all, parliamentary

governments grow about twice as much as presidential governments in the entire sample period.

Similar patterns are displayed by social security and welfare spending, in panel (b) – here, the table only starts in 1970 due to data availability. Here, both the proportional and parliamentary groups have much larger welfare states initially than the other groups. And these differences grow over time. It is somewhat harder to identify a common time pattern, though welfare spending keeps growing until the mid 1990s in almost all groups.

Finally, panel (c) considers the budget surpluses (also as a percentage of GDP). All groups look very similar until the mid 1970s. In the 1970s, the budget deficit grows everywhere, but the increase is more pronounced among proportional and parliamentary countries. These groups continue to have larger deficits in the 1980s and 1990s.

Table 4.6 about here

Altogether, *Table 4.6* shows the relation between the constitution and government fiscal policy to be changing over time. Some important differences in fiscal policy between constitutional groups are already apparent in the 1960s. But something special occurs in the 1970s and 1980s, which has a different effect on constitutional groups and this has lasting consequences well into the 1990s. This pattern suggests that, to fully understand the constitutional effects on fiscal policy, we also need to pay attention to the time variation in the data.

Both *Tables 4.5* and *4.6* reveal important similarities between majoritarian electoral rules and presidential forms of government, and some stark differences relative to proportional and parliamentary countries. Majoritarian and presidential countries have smaller governments, smaller welfare states, smaller deficits and lower productivities. Parliamentary regimes are also less corrupt and their policies are more conducive to growth.

It is tempting to relate these performance patterns to the theoretical predictions put forward in Chapter 2, a temptation which should be strongly resisted, however. If the selection of countries into different constitutional rules were entirely random, we could use such unconditional comparisons for inference. For if this were the case, we might trust that other country characteristics would not systematically influence our policy and performance measures. But given the correlations between the constitution and other cultural, historical, or geographical variables, discussed in Section 5, this is not

an assumption we can seriously entertain. Inference about the causal effect of the constitution on policy outcomes requires additional assumptions and more sophisticated statistical techniques. These issues will be addressed in the next three chapters. But before that, we will complete the provisional discussion of the data by considering other differences between our constitutional groups.

4.6.2   Constitutions and other co-variates

In Chapter 3, we showed policy outcomes to be systematically correlated with several economic and social characteristics, such as per capita income, demographics and openness to international trade. How different are the countries in our constitutional groups when it comes to those characteristics? *Table 4.7* displays means and standard deviations of some prominent policy determinants, by political regimes and electoral rules.

Starting with the split by electoral rules (the first six columns), we find some stark differences. Majoritarian electoral rules are clearly found in poorer countries (LYP), in worse democracies ($GASTIL$), and societies with more Catholics ($CATHO80$) and younger populations ($PROP65$) – only the differences between majoritarian and pure proportional countries tend to be statistically significant. As indicated by the results in Chapter 3, the younger populations might explain the smaller governments and welfare states of majoritarian countries found in *Tables 4.5* and *4.6.* But openness to international trade ($TRADE$) is not correlated with the electoral rule, contrary to the widespread expectation that more open economies prefer PR because they need stability to survive on world markets, as argued by Rogowski (1987) and others. While there is a positive correlation between PR and openness among the 24 developed OECD democracies studied by Rogowski, no apparent correlation is present in our more extensive data set and if anything, the correlation seems to go the other way. Note also that we do not find differences in the age of democracy (AGE) across electoral rules.

Continuing with the split according to the form of government (the last two columns of *Table 4.7*), we see that countries with parliamentary regimes are richer, more open to international trade, and have a larger percentage of old people than countries with presidential regimes (all differences are statistically significant). As Chapter 3 showed, these factors all tend to correlate with larger governments. The higher spending in parliamentary regimes revealed in *Table 4.6* might just reflect these socio-economic differences. On

average, parliamentary regimes also have smaller proportions of Catholics and larger proportions of Protestants; moreover, they are older and better democracies. These features are all expected to correlate with lower corruption (again recall Chapter 3), which might account for the lower unconditional level of corruption in parliamentary regimes.

Table 4.7 about here

These cautionary remarks – and earlier remarks on non-random constitution selection – remind us of the common danger in social-science research of attributing causal interpretations to simple correlations. As a minimum requirement, we should be very careful in holding constant other determinants of the outcomes we study when testing the constitutional effects our theory might suggest. The next chapter contains a more systematic discussion of the assumptions necessary to draw causal inference from cross-country data, in the presence of non-random selection.

Figure 4.1
Electoral rules and forms of government 1998

■ MAJ=1 PRES=1	▦ MAJ=0 PRES=1	▤ MAJ=1 PRES=0	▥ MAJ=0 PRES=0
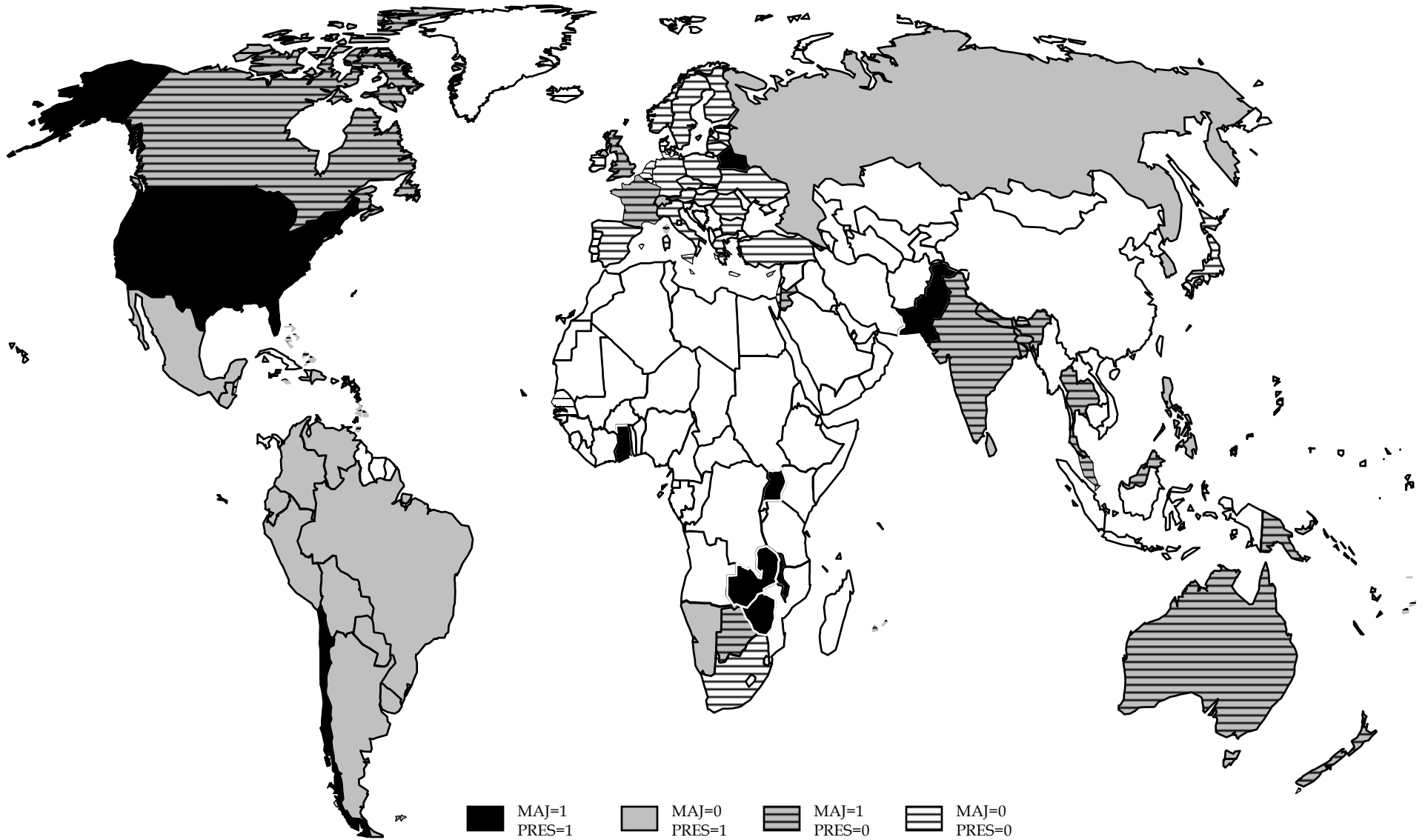
Table 4.1
Age and quality of democracies

Country	GASTIL	POLITY	DEM_AGE	1960-98 PANEL ?
Argentina	2.3	7.0	1983	Yes
Australia	1.0	10.0	1901	Yes
Austria	1.0	10.0	1946	Yes
Bahamas	1.7	.	1973	Yes
Bangladesh	3.2	4.8	1991	No
Barbados	1.0	.	1966	Yes
Belarus	4.9	0.9	1991	No
Belgium	1.2	10.0	1853	Yes
Belize	1.1	.	1981	Yes
Bolivia	2.4	9.0	1982	Yes
Botswana	1.9	9.0	1966	Yes
Brazil	3.0	8.0	1985	Yes
Bulgaria	2.4	8.0	1990	No
Canada	1.0	10.0	1867	Yes
Chile	2.1	8.0	1989	Yes
Colombia	3.5	8.0	1957	Yes
Costa Rica	1.3	10.0	1841	Yes
Cyprus (G)	1.0	10.0	1960	Yes
Czech Republic	1.7	10.0	1918	No
Denmark	1.0	10.0	1915	Yes
Dominican Republic	2.9	6.4	1978	Yes
Ecuador	2.6	8.9	1979	Yes
El Salvador	3.0	6.9	1984	Yes
Estonia	2.1	6.0	1991	No
Fiji	3.8	5.0	1990	Yes
Finland	1.0	10.0	1917	Yes
France	1.5	9.0	1946	Yes
Gambia	4.4	0.2	1965	Yes
Germany	1.5	10.0	1949	Yes
Ghana	4.3	-0.6	1996	No
Greece	1.8	10.0	1975	Yes
Guatemala	4.0	4.7	1986	Yes
Honduras	2.7	6.0	1982	Yes
Hungary	1.7	10.0	1990	No
Iceland	1.0	10.0	1944	Yes
India	3.3	8.4	1950	Yes
Ireland	1.1	10.0	1921	Yes
Israel	2.0	9.0	1948	Yes
Italy	1.4	10.0	1948	Yes
Jamaica	2.3	9.3	1959	No
Japan	1.6	10.0	1868	Yes
Latvia	2.3	8.0	1991	No
Luxembourg	1.0	10.0	1879	Yes

Malawi	4.2	0.0	1994	No
Malaysia	4.6	10.0	1957	Yes
Malta	1.0	.	1964	Yes
Mauritius	1.6	10.0	1968	Yes
Mexico	3.8	2.7	1994	Yes
Namibia	2.4	8.0	1990	No
Nepal	3.3	5.0	1990	Yes
Netherlands	1.0	10.0	1917	Yes
New Zeland	1.0	10.0	1857	Yes
Nicaragua	3.4	6.9	1990	Yes
Norway	1.0	10.0	1898	Yes
Pakistan	4.3	7.8	1988	No
Papua New Guina	2.8	10.0	1975	Yes
Paraguay	3.3	5.8	1989	Yes
Peru	4.4	2.1	1980	Yes
Philippines	2.9	7.9	1987	Yes
Poland	1.8	8.1	1989	No
Portugal	1.1	10.0	1976	Yes
Romania	3.6	6.0	1990	No
Russia	3.5	3.3	1992	No
Senegal	4.0	-1.0	1975	No
Singapore	4.7	-2.0	1965	No
Slovak Republic	2.5	7.2	1993	No
South Africa	2.9	7.9	1910	No
South Korea	2.2	6.2	1988	No
Spain	1.3	10.0	1978	Yes
Sri Lanka	4.2	5.0	1948	Yes
St. Vincent&Granada	1.4	.	1978	Yes
Sweden	1.0	10.0	1917	Yes
Switzerland	1.0	10.0	1848	Yes
Taiwan	3.0	5.8	1992	No
Thailand	3.4	7.2	1992	Yes
Trinidad&Tobago	1.3	9.0	1962	Yes
Turkey	4.1	8.1	1983	Yes
USA	1.0	10.0	1800	Yes
Uganda	4.9	-4.3	1994	No
UK	1.5	10.0	1837	Yes
Ukraine	3.4	6.4	1991	No
Uruguay	1.7	10.0	1985	Yes
Venezuela	2.6	8.2	1958	Yes
Zambia	3.8	2.7	1991	No
Zimbabwe	4.9	-6.0	1989	No

Table 4.2
Electoral rules and forms of government

Country	MAJ	MIXED	MAGN	PIND	YEARELE	PRES	YEARREG
Argentina	0.00	0.00	0.09	0.00	1983	1	1983
Australia	1.00	0.00	1.00	1.00	1901	0	1901
Austria	0.00	0.00	0.05	0.00	1945	0	1945
Bahamas	1.00	0.00	1.00	1.00	1973	0	1973
Bangladesh	1.00	0.00	0.91	1.00	1991	0	1991
Barbados	1.00	0.00	1.00	1.00	1966	0	1966
Belarus	1.00	0.00	1.00	1.00	1991	1	1991
Belgium	0.00	0.00	0.14	0.00	1899	0	1853
Belize	1.00	0.00	1.00	1.00	1981	0	1981
Bolivia	0.00	0.22	0.07	0.12	1982 (1996)	1	1982
Botswana	1.00	0.00	1.00	1.00	1966	0	1966
Brazil	0.00	0.00	0.05	0.00	1988	1	1988
Bulgaria	0.00	0.00	0.13	0.00	1991	0	1991
Canada	1.00	0.00	1.00	1.00	1867	0	1867
Chile	1.00	0.00	0.50	1.00	1989	1	1989
Colombia	0.00	0.00	0.21	0.00	1957	1	1957
Costa Rica	0.00	0.00	0.12	0.00	1953	1	1949
Cyprus (G)	0.00	0.00	0.11	0.00	1981	1	1960
Czech Republic	0.00	0.00	0.04	0.00	1993	0	1993
Denmark	0.00	0.00	0.11	0.00	1920	0	1915
Dominican Republic	0.00	0.00	0.25	0.00	1966	1	1978
Ecuador	0.00	0.33	0.26	0.00	1979 (1996)	1	1979
El Salvador	0.00	0.00	0.18	0.00	1984	1	1984
Estonia	0.00	0.00	0.11	0.00	1992	0	1992
Fiji	0.56	0.44	0.64	0.79	1990 (1994)	0	1990
Finland	0.00	0.00	0.09	0.01	1917	0	1917
France	1.00	0.00	1.00	1.00	1986	0	1958
Gambia	1.00	0.00	0.98	1.00	1965	1	1965
Germany	0.00	1.00	0.52	0.50	1949	0	1949
Ghana	1.00	0.00	1.00	1.00	1992	1	1992
Greece	0.00	0.00	0.19	0.00	1975	0	1975
Guatemala	0.00	0.00	0.24	0.00	1985	1	1985
Honduras	0.00	0.00	0.14	0.00	1982	1	1982
Hungary	0.00	1.00	0.51	0.46	1990	0	1990
Iceland	0.00	0.00	0.13	0.00	1944	0	1944
India	1.00	0.00	1.00	1.00	1950	0	1950
Ireland	0.00	0.00	0.25	0.00	1937	0	1937
Israel	0.00	0.00	0.01	0.00	1948	0	1948
Italy	0.00	0.56	0.46	0.42	1945 (1994)	0	1945
Jamaica	1.00	0.00	1.00	1.00	1962	0	1962
Japan	0.33	0.67	0.38	0.87	1952 (1994)	0	1952
Latvia	0.00	0.00	0.05	0.00	1991	0	1991

Luxembourg	0.00	0.00	0.07	0.00	1918	0	1879
Malawi	1.00	0.00	1.00	1.00	1994	1	1994
Malaysia	1.00	0.00	1.00	1.00	1957	0	1957
Malta	0.00	0.00	0.20	0.00	1964	0	1964
Mauritius	1.00	0.00	0.33	1.00	1968	0	1968
Mexico	0.00	1.00	0.60	0.60	1994	1	1994
Namibia	0.00	0.00	0.17	0.00	1990	1	1990
Nepal	1.00	0.00	1.00	1.00	1990	0	1990
Netherlands	0.00	0.00	0.12	0.00	1917	0	1917
New Zealand	0.67	0.33	0.85	0.85	1906 (1996)	0	1906
Nicaragua	0.00	0.00	0.13	0.00	1990	1	1990
Norway	0.00	0.00	0.12	0.00	1919	0	1898
Pakistan	1.00	0.00	1.00	1.00	1988	1	1988
Papua New Guina	1.00	0.00	1.00	1.00	1975	0	1975
Paraguay	0.00	0.00	0.23	0.00	1992	1	1992
Peru	0.00	0.00	0.01	0.00	1979	1	1979
Philippines	0.89	0.11	0.98	0.98	1987 (1996)	1	1987
Poland	0.00	0.00	0.11	0.00	1989	0	1989
Portugal	0.00	0.00	0.09	0.00	1976	0	1976
Romania	0.00	0.00	0.12	0.00	1989	0	1989
Russia	0.00	1.00	0.50	0.50	1992	1	1992
Senegal	0.00	1.00	0.25	0.43	1975	0	1975
Singapore	1.00	0.00	0.30	1.00	1965	0	1965
Slovak Republic	0.00	0.00	0.03	0.00	1993	0	1993
South Africa	0.00	1.00	0.02	0.00	1994	0	1994
South Korea	0.00	1.00	0.78	0.78	1988	1	1988
Spain	0.00	0.00	0.15	0.01	1978	0	1978
Sri Lanka	0.00	0.00	0.10	0.00	1978	1	1948
St. Vincent&Granada	1.00	0.00	1.00	1.00	1978	0	1978
Sweden	0.00	0.00	0.08	0.00	1917	0	1917
Switzerland	0.00	0.00	0.13	0.03	1918	1	1874
Taiwan	0.00	1.00	.		1992	0	1992
Thailand	1.00	0.00	0.42	1.00	1978	0	1992
Trinidad&Tobago	1.00	0.00	1.00	1.00	1962	0	1962
Turkey	0.00	0.00	0.19	0.00	1982	0	1982
USA	1.00	0.00	1.00	1.00	1800	1	1800
Uganda	1.00	0.00	1.00	1.00	1994	1	1994
UK	1.00	0.00	1.00	1.00	1837	0	1837
Ukraine	0.80	0.20	0.90	0.90	1991 (1996)	0	1991
Uruguay	0.00	0.00	0.11	0.00	1985	1	1985
Venezuela	0.00	0.67	0.45	0.33	1958 (1993)	1	1958
Zambia	1.00	0.00	1.00	1.00	1991	1	1991
Zimbabwe	1.00	0.00	1.00	1.00	1989	1	1989

Table 4.3
Constitutional rules across the world

	COL_UK	COL_ES	LAAM	ASIAE	AFRICA	OECD
MAJ	0.73	0.13	0.30	0.69	0.73	0.30
PRES	0.33	1.00	0.74	0.31	0.64	0.09

Fractions of majoritarian (*MAJ*) and presidential (*PRES*) constitutions in each group.

Table 4.4
Determinants of constitutional rules
Probit estimates

	(1)	(2)	(3)	(4)
Dep. var.	*PRES*	*PRES*	*MAJ*	*MAJ*
CON2150	0.52	0.15	-1.72	-1.38
	(0.63)	(0.72)	(0.69)**	(0.82)*.
CON5180	0.59	-0.04	0.10	0.13
	(0.58)	(0.63)	(0.62)	(0.68)
CON81	1.83	1.52	0.03	0.23
	(0.62)***	(0.73)**	(0.72)	(0.72)
AGE	1.61	3.83	0.74	0.14
	(1.15)	(1.51)**	(1.27)	(1.48)
COL_UKA	-0.08	-0.05	2.15	1.02
	(0.45)	(0.67)	(0.45)***	(0.62)
LAAM	1.51	1.61	-0.38	-1.96
	(0.40)***	(0.63)***	(0.36)	(0.80)**
LAT01		-5.15		-4.19
		(1.79)***		(1.57)***
ENGFRAC		-3.26		2.62
		(1.02)***		(0.90)***
EURFRAC		0.71		0.74
		(0.61)		(0.72)
Sample	90s, broad	90s, broad	90s, broad	90s, broad
Obs.	85	78	85	78
Pseudo R2	0.26	0.51	0.31	0.50

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4.5
Policy outcomes and constitutions
Variation across countries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. var.	MAJ	MIXED	PROP	p(MAJ,MIX)	p(PROP,MIX)	p(MAJ,PROP)	PRES	PARL	p(PRES,PARL)
CGEXP	25.6	27.1	31.4	0.676	0.323	0.016	22.2	33.3	0.000
	(8.2)	(11.6)	(11.3)				(7.2)	(10.0)	
SSW	4.7	7.1	10.7	0.330	0.195	0.000	4.8	9.9	0.002
	(5.4)	(6.8)	(6.5)				(4.6)	(7.0)	
DFT_SPL	-1.1	-3.0	-2.8	0.229	0.821	0.053	-2.0	-2.3	0.708
	(4.3)	(2.7)	(2.8)				(2.7)	(3.9)	
GRAFT	4.3	4.3	4.0	0.959	0.687	0.594	5.3	3.4	0.000
	(1.9)	(1.4)	(2.0)				(1.5)	(1.8)	
GADP	0.7	0.7	0.7	0.559	0.780	0.649	0.6	0.8	0.000
	(0.2)	(0.1)	(0.2)				(0.2)	(0.2)	
LOGYL	8.9	9.3	9.5	0.295	0.543	0.007	8.8	9.5	0.000
	(1.1)	(0.7)	(0.7)				(0.9)	(0.8)	
LOGA	8.0	8.2	8.3	0.359	0.622	0.024	7.9	8.3	0.003
	(0.8)	(0.4)	(0.5)				(0.6)	(0.5)	

$p(X,Y)$ is the probability of falsely rejecting equal means in groups X and Y, under the maintained hypothesis of equal variances.
Standard deviations in parenthesis

Table 4.6
Fiscal policy outcomes and constitutions
Variation over time

(a) Size of government

	Full Sample		Presidential		Parliamentary		Majoritarian		Proportional	
.	CGEXP	DCGEXP	CGEXP	DCGEXP	CGEXP	DCGEXP	CGEXP	DCGEXP	CGEXP	DCGEXP
1960-64	18.3	0.7	14.6	-0.4	20.5	1.4	19.2	1.5	18.7	0.5
1965-69	20.0	1.8	15.7	1.0	22.7	2.4	21.0	0.6	20.4	2.4
1970-74	22.2	3.0	16.7	1.1	25.8	4.3	22.1	1.5	22.9	3.6
1975-79	26.0	3.6	18.3	2.1	30.6	4.5	26.3	3.0	26.3	4.0
1980-84	29.8	4.1	21.7	4.1	34.4	4.0	28.6	3.5	30.6	4.5
1985-89	29.3	-2.4	20.7	-3.0	33.9	-2.0	27.3	-1.5	30.2	-2.9
1990-94	29.1	1.2	20.0	1.5	34.4	1.0	27.5	-0.3	29.9	1.9
1995-98	28.5	-0.8	20.3	-0.1	33.4	-1.2	25.9	0.2	29.9	-1.4
All years	25.8	11.9	18.7	7.4	30.1	14.7	25.4	8.6	26.5	13.3
Countries	60	60	22	22	38	38	21	21	39	39

(b) Social security and welfare spending

	Full Sample		Presidential		Parliamentary		Majoritarian		Proportional	
	SSW	DSSW	SSW	DSSW	SSW	DSSW	SSW	DSSW	SSW	DSSW
1970-74	5.9	0.9	3.7	-1.1	7.2	2.2	3.8	-0.9	6.8	1.6
1975-79	6.4	1.0	3.6	0.1	8.2	1.6	4.7	0.8	7.6	1.1
1980-84	8.0	0.7	4.9	0.3	9.4	0.9	5.6	1.0	9.6	0.5
1985-89	7.7	-0.1	3.8	-0.6	9.4	0.1	5.1	-0.5	9.0	0.1
1990-94	8.1	1.5	4.9	1.7	9.8	1.3	5.4	0.6	9.5	1.9
1995-98	8.1	-0.5	6.0	0.0	9.2	-0.7	4.6	-0.2	9.9	-0.6
All years	7.4	5.2	4.4	1.8	9.0	7.0	5.0	2.7	8.8	6.5
Countries	50	49	18	17	32	32	18	17	32	32

Countries denotes the number of countries in 1990-94 (this number changes over time). *DCGEXP* and *DSSW* denote the average cumulative changes of *CGEXP* and *SSW* in the rows for different subperiods, as a % of GDP, and the average cumulative change over the whole time period in the All years row.

Table 4.6
Fiscal policy outcomes and constitutions
Variation over time (continued)

(c) Budget surplus

	Full Sample	Presidential	Parliamentary	Majoritarian	Proportional
	SPL	*SPL*	*SPL*	*SPL*	*SPL*
1960-64	-1.5	-1.2	-1.7	-1.9	-1.4
1965-69	-2.0	-1.7	-2.3	-1.9	-2.2
1970-74	-2.4	-2.1	-2.7	-3.0	-2.3
1975-79	-3.7	-2.6	-4.4	-3.5	-3.9
1980-84	-5.0	-4.3	-5.4	-4.2	-5.5
1985-89	-3.2	-3.7	-2.9	-1.7	-3.9
1990-94	-2.8	-1.7	-3.4	-1.5	-3.4
1995-98	-2.0	-1.3	-2.4	-0.8	-2.6
All years	-3.0	-2.4	-3.3	-2.5	-3.3
Countries	60	22	38	21	39

Countries denotes the number of countries in 1990-94 (this number changes over time)

Table 4.7
Country characteristics and constitutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	MAJ	MIXED	PROP	p(MAJ,MIX)	p(PROP,MIX)	p(MAJ,PROP)	PRED	PARL	p(PRES,PARL)
LYP	8.1	8.5	8.6	0.395	0.792	0.058	7.9	8.7	0.000
	(1.1)	(0.9)	(0.8)				(0.9)	(0.9)	
TRADE	85.2	67.3	78.8	0.386	0.415	0.580	62.5	89.1	0.011
	(59.6)	(24.3)	(40.2)				(27.5)	(54.2)	
PROP65	6.6	7.7	9.9	0.563	0.234	0.003	5.6	10.3	0.000
	(4.4)	(5.1)	(4.8)				(3.5)	(4.8)	
CATHO80	23.0	23.9	55.7	0.924	0.039	0.000	57.8	29.8	0.001
	(24.5)	(32.0)	(42.4)				(39.2)	(34.5)	
PROT80	16.7	18.1	18.3	0.847	0.980	0.791	9.9	22.3	0.028
	(18.5)	(19.0)	(31.7)				(15.9)	(29.2)	
AGE	0.2	0.1	0.2	0.280	0.194	0.926	0.2	0.3	0.056
	(0.2)	(0.1)	(0.2)				(0.2)	(0.2)	
GASTIL	2.8	2.9	2.1	0.752	0.043	0.027	3.1	2.0	0.000
	(1.4)	(1.0)	(1.1)				(1.2)	(1.1)	

$p(X,Y)$ is the probability of falsely rejecting equal means across groups X and Y, under the maintained hypothesis of equal variances.
Standard errors in parenthesis

Chapter 5

Cross-sectional inference: Pitfalls and methods

5.1 Introduction

As we have seen in Chapter 4, the broad features of electoral rules and forms of government are very stable over time. Because of this stability, we do not have enough "constitutional experiments" to safely isolate the causal effect of constitutional reforms from variation in performance over time. Instead, we must infer the causal effects of constitutions from cross-country comparisons, a task undertaken in the next two chapters. But the road to secure inference from cross-country data is riddled with statistical difficulties. We therefore devote the present chapter to a discussion of the most important pitfalls and methods that may provide ways around them. Readers mainly interested in the substantive results, and willing to trust our choice of methodology, can probably skip parts of this chapter. Similarly, econometrically skilled readers could skim the material already familiar to them. To get a sense of where we are going, the econometrically uninterested or proficient should read this introduction, however. They should also read the final parts of Sections 4.1, 4.2 and 5, which speak most directly to the practical implementation of the methods to be used in Chapters 6 and 7. More extensive discussions of the econometric methodology introduced in this chapter can be found in Angrist and Krueger (1999), (2001), Heckman, Lalonde and Smith (1999), Ichino (2001) and Wooldridge (2002, Ch. 18).

Which questions on constitutions and policy outcomes do we pose to the

data? We are not interested in correlations per se, but in what they reveal about underlying causation. For the policy and performance measures introduced in Chapter 3, we would thus like to answer counterfactual questions like: "Suppose we pick a country at random in our sample and, going back in history, change its constitution. How would this alter its current performance?" In Section 2 of this chapter, we show how to pose such questions in a precise way and discuss a fundamental difficulty in providing an answer.

At the end of Chapter 4, we pointed out that constitutional features are correlated with country characteristics that also determine policy outcomes, and stressed the importance of holding constant these common determinants in cross-country comparisons. But confounding constitutions with other unobserved socio-economic determinants of performance is certainly not the only pitfall in separating correlation from causation. It is just one instance of a general statistical phenomenon known as "simultaneity", namely that our inference becomes biased if the variation in constitutional rules used to explain performance is related to the random (unexplained) component of performance. Simultaneity problems can take the form of *reverse causation*, different forms of *selection bias*, and *measurement error*.

Direct reverse causation, i.e., a causal link from policy outcomes to constitutions, is probably not a major concern in our context. If it were, we would probably not observe so much stability of political institutions over very long time periods, despite pretty large changes in policy. The constitutional stability highlighted in Chapter 4 indicates that it may be correct to treat electoral rules and political regimes as given by history, and not plagued by reverse causation from outcomes to constitutions. In that way, stability may be a blessing.

Historically predetermined constitutional rules certainly do not rule out problems of selection bias, however. As Chapter 4 made eminently clear, constitutional choices do not appear to be have been random. It is quite possible that countries self-selected into constitutions on the basis of cultural treats and historical experience that also shape long-run collective preferences and thus influence policy and performance even today. For instance, Botswana's history as a UK colony may have fostered the selection of a first-past-the post electoral system, as well as a tradition (by African standards) of resistance to corruption; and this might bias our inference towards finding a negative link between majoritarian elections and corruption. This is why it is crucial to hold constant prospective determinants of constitutional choices that may also influence policy performance. The Botswana example – and more gener-

ally the evidence from the equal-means tests and probit estimates in Chapter 4 (Section 5) – suggests that is important to control for measures like colonial history and continental location. This is what we try to achieve when estimating the constitutional effect via simple linear regression analysis with many right-hand-side variables. While this way of approaching the data is the most common empirical strategy in economics and, probably, in political science, it does rely on very strong underlying assumptions. In Section 3 of the chapter, we clarify and discuss these *identifying* assumptions.

Specifically, holding constant observable constitutional determinants may not be enough. Suppose, for example, that Sweden's history of equal opportunity, broad education and wide-spread ownership of land (relative to other countries) fostered a common culture of equality, thereby promoting the selection of a proportional electoral system as well as a preference for a welfare state. Because such underlying determinants might be very hard to identify or measure, we may encounter a problem of selection on *unobservables*. Although notoriously difficult, social scientists have developed some methods for dealing with the selection-bias problem: isolating sources of truly exogenous variation in the constitution by instrumental variables, or correcting the estimation of the constitutional effect by an adjustment for systematic self-selection. Instrumental-variable estimation is also a classical method for dealing with one of the other sources of simultaneity bias, namely measurement error in the causal variable of interest. Section 4 introduces and discusses these statistical methods in the context of our problem and explains how they are used in Chapters 6 and 7.

A final concern is that our attempts to estimate a causal effect of the constitution involve "comparing the incomparable", a critique frequently waged by some political scientists against statistical work in comparative politics. Suppose the effect of a political reform, say going from a parliamentary to a presidential regime, depends on culture, geography and history. For example, the same constitutional reform might have different effects on policy outcomes in, say, Latin America vs. Europe, or in good vs. bad democracies. Then, we may indeed run into problems, even if the most relevant constitutional determinants are fully observed and held constant. In the wake of such interaction effects, or non-linearities, the fact that presidential regimes are (much) more common in Latin America and in worse democracies makes it dangerous to extrapolate from their experience to good parliamentary democracies in Europe. To handle this problem of selection on *observables,* we need a method that is robust to functional form and handles systematic selection

by focusing on appropriate "local" comparisons. Non-parametric matching methods with these properties are introduced in Section 5, where we also explain how they will be applied to our problem in subsequent chapters.

In most of the chapter, we simplify the formal treatment by considering the estimation of a single constitutional effect at the time. Section 6 briefly deals with the extensions to multiple constitutional features.

5.2 The question

5.2.1 Primitives

For the sake of the argument, assume that there is only one constitutional rule, S, which can only take on two values, $S = 0, 1$. At any point in time, country i belongs to one of these constitutional states, denoted by the indicator S_i. Thus, country i could be Botswana and S could be the electoral rule, as measured by the binary variable *MAJ,* in which case we would have $S_i = 1$.

Suppose that the constitution selection by country i can be described by the index model

$$S_i = \begin{cases} 1 & \text{as} \;\; G(\mathbf{W}_i) + \eta_i \geq 0 \\ 0 & \text{as} \;\; G(\mathbf{W}_i) + \eta_i < 0 \end{cases}, \tag{5.1}$$

where $\mathbf{W}$ is a set of *observed* variables influencing the observed choice of constitution. Members of $\mathbf{W}$ would involve variables, such as those discussed in Chapter 4, describing continental location, colonial history, culture and the pure timing of constitutional choice. Other *unobserved* country-specific factors are summarized by the random variable η_i. Throughout, we assume that η and $\mathbf{W}$ are uncorrelated.

Let Y_i^S denote the *potential* policy outcome or performance of country i in constitutional state S. Thus, Y could be corruption, with Y_i^1 and Y_i^0 and denoting corruption in Botswana under majoritarian and proportional elections, respectively. Potential performance is not observed, as each country can only have one constitution at a given moment in time. We only observe *actual* performance in one of the two constitutional states, Y_i:

$$Y_i = S_i Y_i^1 + (1 - S_i) Y_i^0 . \tag{5.2}$$

We observe corruption in Botswana under its actual majoritarian electoral rule, but not under its *counterfactual* proportional rule. This distinction be-

tween actual and potential performance is crucial to the statistical problems to be discussed in this chapter, so we return to it below.

The stochastic process determining potential performance in constitutional state S and country i s:

$$Y_i^S = F^S(\mathbf{X}_i) + \varepsilon_i^S, \quad S = 0, 1 , \tag{5.3}$$

where $\mathbf{X}$ is a vector of observed variables, say the educational attainment and predominant religion in the population, $F^S(\cdot)$ is a function that is allowed to depend on the constitutional state and ε^S is a random variable capturing the effect of all the unobserved determinants of performance. The observed determinants $\mathbf{X}$ could interact with the constitution in influencing policy or performance in many ways. But, by assumption, the observable variable $\mathbf{X}$ itself is not causally affected by the constitutional state.[1]

Importantly, the unobserved determinant of performance, ε^S, is allowed to depend on the constitutional state. For instance, corruption could depend on unobservable social norms, but the effect of social norms on corruption is also influenced by the constitution, so that $\varepsilon_i^1 \neq \varepsilon_i^0$. In this case, the effect of the constitution on performance can differ across countries even if they have identical observables. In the special case in which $\varepsilon_i^1 = \varepsilon_i^0$ for all i, the influence of the constitution on performance is instead homogenous for countries with similar observables $\mathbf{X}$, since it does not interact with ε_i. Even if we allow ε^1 and ε^0 to differ, we always assume that both have a mean of zero in the *full population* of countries. We also assume that ε^S is uncorrelated with $\mathbf{X}$, for $S = 0, 1$.

With this notation, each country in our sample is fully described by a realization of the vector $(\mathbf{W}, \mathbf{X}, \eta, \varepsilon^1, \varepsilon^0)$. These random variables (some of them unobserved) are the primitive objects that define our population of countries. Through equations (5.1) and (5.3), these primitives then determine a realization of (S, Y^1, Y^0), and together with (5.2) they define a vector of observables $(S, Y, \mathbf{X}, \mathbf{W})$. Below, we discuss what restrictions are needed on the joint distributions of these random variables for unbiased inference from

---

[1]The assumption of no effect of S on $\mathbf{X}$ is admittedly very strong. It is more plausible for some variables entering our actual $\mathbf{X}$ vector than for others. If the assumption fails, the estimated effect of S on performance that we discuss below has the interpretation of a direct effect on performance (a partial derivative), holding constant the values of $\mathbf{X}$, rather than a reduced form effect (a total derivative). But in this case, other estimation problems arise on top of those discussed in this chapter – see Heckman et al. (1999).

observable data. But before doing that, we must define more precisely the parameter we want to estimate.

5.2.2 The parameter of interest

Suppose that we pick a country at random with characteristics $\mathbf{X}$, and switch its constitutional state from $S = 0$ to $S = 1$. The expected effect of constitutional reform in this particular country is now given by the (conditional) expectation:

$$\alpha(\mathbf{X}) = \mathsf{E}(Y^1 - Y^0 \mid \mathbf{X}) , \tag{5.4}$$

where the expectations operator E refers to potential performance, Y^S, $S = 0, 1$.[2]

Naturally, it would be very interesting to estimate the effect for many different values of $\mathbf{X}$. But given the rich set of relevant determinants in $\mathbf{X}$ and the relative scarcity of observed democracies, we simply do not have enough data for such conditional estimation. What can be more realistically estimated is the *average* value of constitutional reform on performance for all countries in our sample. Thus, we define our parameter of interest α, as the *average* value of $\alpha(\mathbf{X})$ in our population, namely:

$$\alpha = E\left\{\mathsf{E}(Y^1 - Y^0 \mid \mathbf{X})\right\} = \mathsf{E}(Y^1 - Y^0) . \tag{5.5}$$

In (5.5), the outer expectations operator E is taken over the actual unconditional distribution of $\mathbf{X}$ in our sample, and the second equality follows from the law of iterated expectations. Throughout the chapter, we refer to α as the *constitutional effect*. It tells us the expected effect of constitutional reform on performance for a country drawn at random in the population of countries. In the corruption example above, this is the average (or expected) effect on corruption of an electoral reform, switching from proportional to majoritarian elections.[3]

---

[2]In terms of our primitives, a constitutional reform can be thought of as a hypothetical experiment in which we change the realization of the unobserved determinant of the constitution, η, so that the constitutional state switches from $S = 0$ to $S = 1$.

[3]Readers familiar with the program evaluation literature will recognize this expression as the *average treatment effect*. See Heckman et al (1999) for a discussion of this and other statistical definitions of a causal effect in program evaluation. Wooldridge (2002, Ch.18) provides an advanced textbook treatment of different approaches to estimating average treatment effects.

Given our assumption that ε^S has zero mean in the population, $S = 0, 1$, by (5.3) and (5.5) we can rewrite the constitutional effect as:

$$\alpha = E[F^1(\mathbf{X}) - F^0(\mathbf{X})] \ ,$$

where again the expectations operator is taken over the distribution of $\mathbf{X}$ in the whole sample of countries. This formulation makes clear that our question ultimately concerns how the function $F^S(\cdot)$ determining performance varies with the constitution. But since we don't know $F^S(\cdot)$, we have to estimate it from observable data. This is the problem we turn to next.

5.2.3 Estimation

As already noted, we only observe *actual* performance, not *potential* performance. Yet, we are interested in the determinants of potential performance: the function $F^S(\cdot)$. We would like to know how changing the electoral rule would affect corruption in, say, Botswana. But we only observe Botswana under majoritarian electoral rule, not under an hypothetical proportional rule.

The consequences of this "missing-data problem" can best be seen by rewriting the expression in (5.5) as

$$\begin{aligned} \alpha \ &= \ P \cdot [\mathsf{E}(Y^1 \mid S = 1) - \ \mathsf{E}(Y^0 \mid S = 1)] \qquad (5.6) \\ +(1 - P) \cdot [&\mathsf{E}(Y^1 \quad | \quad S = 0) - \mathsf{E}(Y^0 \mid S = 0)] \ , \end{aligned}$$

where P is the probability of observing a country with $S = 1$ in the sample. The first bracketed term in (5.6) is the effect of constitutional reform in countries currently in state $S = 1$, while the second bracketed term is the effect for countries currently in state $S = 0$. In the above example, $S = 1$ denotes majoritarian electoral rule, while $S = 0$ denotes proportional electoral rule. Equation (5.6) says that the effect of electoral reform on the whole sample is the weighted average of the effect of electoral reform on the two groups of countries, those currently under majoritarian rule (the first term) and those under proportional rule (the second term), each weighted by its relative frequency.[4] Clearly, we can easily estimate the factual outcomes

---

[4]The first effect is also known as the "average effect of treatment *on the treated*" in the program evaluation literature, whereas the second term is called the "average effect of treatment *on the controls (untreated)* ". Note that these two effects are *not* necessarily

$\mathsf{E}(Y^1 \mid S = 1)$ and $\mathsf{E}(Y^0 \mid S = 0)$ from, say, the sample mean of observed corruption under each electoral rule. But how should we proceed with the unobserved counterfactuals $\mathsf{E}(Y^1 \mid S = 0)$ and $\mathsf{E}(Y^0 \mid S = 1)$? This difficult question is sometimes referred to as the fundamental problem of causal inference.

In a world where the constitution were randomly assigned to countries, the problem would have a simple solution. Random selection would imply that constitutional rules, S, were independent of outcomes Y; moreover, it would balance the distribution of $\mathbf{X}$ across the two constitutional groups. As a result, we could safely set $\mathsf{E}(Y^1 \mid S = 0) = \mathsf{E}(Y^1 \mid S = 1) = \mathsf{E}(Y^1)$, thereby replacing unobservable counterfactual performance with observable actual performance, as the two would be (close to) equal. Similarly, we could set $\mathsf{E}(Y^0 \mid S = 1) = \mathsf{E}(Y^0 \mid S = 0) = \mathsf{E}(Y^0)$. For example, we could comfortably assume that the average *potential* corruption in the whole population under, say, majoritarian rule could be measured by the average *actual* corruption in *currently majoritarian* countries. Making these substitutions in (5.6), we would thus simply compute the constitutional effect as $\alpha = \mathsf{E}(Y^1 \mid S = 1) - \mathsf{E}(Y^0 \mid S = 0)$. The constitutional effect on corruption would be evaluated as the observed difference in average corruption between majoritarian and proportional countries.

But as highlighted in Chapter 4, this is not the real world. Constitution selection is certainly not random, so we need to make additional assumptions in order to evaluate the unobservable counterfactuals. At a general level, these assumptions can be described with reference to the model in equations (5.1)-(5.3), defining constitution selection and performance. One set of assumptions concerns the *unobserved determinants* of outcomes and constitutional choices, as captured by the joint distribution of the random components η and $\varepsilon^S, S = 0, 1$.[5] Another set of assumptions concern the *functional forms* of $G(\cdot)$ and $F^S(\cdot)$, $S = 0, 1$, and their exact specification

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symmetric if the selection of the constitution is not random and not independent of the outcome. The average treatment effect is thus a weighted average of the average effect of treatment on the treated and the average effect of treatment on the controls.

[5]In fact, as noted by Wooldridge (2002), the assumptions about the unobserved determinants of constitution selection and performance needed to identify and estimate the constitutional effect (i.e., the average treatment effect) can be stated in terms of the means of Y^S, conditional on S and $\mathbf{X}$, without imposing any kind of model on the joint distributions of our primitive variables ($\mathbf{X}$, $\mathbf{W}$, η, ε^1, ε^0). Of course, these assumptions imply specific conditions on the underlying joint distribution of the primitive variables, and in some cases we will explicitly spell out such conditions.

(i.e., which variables are excluded from one equation but included in the other). As we explain below, when constitutional selection is non-random, making accurate assumptions regarding these distributions and functional forms is crucial to obtain unbiased estimates of the constitutional effect, α. The different estimation methods described in the following three sections implicitly trade off less restrictive assumptions in one of these dimensions against more restrictive assumptions in the other.

5.3 Simple linear regressions

Linear regression analysis is routinely applied in empirical work by most economists and many political scientists. Does it give reliable results if applied to our problem? This section discusses a set of identifying assumptions guaranteeing an unbiased estimate of the causal effect of constitutional reform on performance.

5.3.1 Conditional independence

A standard and convenient assumption is *conditional independence.* Loosely speaking, this assumption says that selection of the constitution is random, once we have controlled for the vector of observable variables in $\mathbf{X}$. Specifically, suppose that the variables entering $\mathbf{W}$ in the index model of selection (5.1) are a subset of the variables in $\mathbf{X}$ influencing performance in (5.3). Then, conditional independence is satisfied if the random terms in these relations, η and ε^S, are uncorrelated. We can also state the assumption in a different way, namely as *recursivity* of the model consisting of (5.1) and (5.3).

In Chapter 4, we saw that a number of observable variables likely to influence performance are indeed correlated with electoral rules and forms of government. This is the case for colonial history and continental location, but also for levels of income, openness and the quality of democracy. The critical assumption is thus that when these observables have been taken into account, the unexplained influence on constitution selection is not systematically related to the unexplained influence on potential performance. As a result, the assumption is also known as *ignorability* or as *selection on observables.*

More precisely, we assume:

$$
\begin{aligned}
\mathsf{E}(Y^1 \mid \mathbf{X}, S = 0) &= \mathsf{E}(Y^1 \mid \mathbf{X}, S = 1) = \mathsf{E}(Y^1 \mid \mathbf{X}) \qquad (5.7)\\
\mathsf{E}(Y^0 \mid \mathbf{X}, S = 1) &= \mathsf{E}(Y^0 \mid \mathbf{X}, S = 0) = \mathsf{E}(Y^0 \mid \mathbf{X}) \ .
\end{aligned}
$$

This assumption is sometimes called conditional *mean* independence, to emphasize that it is slightly weaker than conditional independence. It says that, once we have conditioned on $\mathbf{X}$, expected *potential* performance in state S, Y^S, is the same for all countries irrespective of their *actual* constitutional state. Conditional mean independence is implied by orthogonality of ε^S and η : if ε^S and η are uncorrelated, then $\mathsf{E}(\varepsilon^1 \mid S = 0) = \mathsf{E}(\varepsilon^1 \mid S = 1) = 0$ and taking expectations of (5.3) we satisfy (5.7).[6]

This assumption allows us to replace the unobservable counterfactuals in each constitutional state entering into (5.6) by an estimate obtained from the actual performance in each state. That estimate should take into account the observable variables in $\mathbf{X}$ and thus, all variables $\mathbf{W}$ systematically correlated with constitutional rules.

5.3.2 Linearity

But how exactly should we separate the constitutional effect from that of other determinants of performance, i.e., how should we control for $\mathbf{X}$? A particular concern is that the constitutional effect could interact with other determinants of performance in subtle ways. For instance, the electoral rule could be a more important determinant of corruption in more developed democracies and economies. As some developing countries have more dubious democratic institutions, the influence of electoral rules might be less important than implicit or unwritten norms, when comparing with more developed countries. Or else, the effect of income inequality on the size of government might depend on the electoral rule. Since the Meltzer-Richards (1981) model relies on the median-voter theorem, it may be more applicable in countries with first-past-the post elections, as plurality rule in single-member districts promotes two-party systems, where the logic of two-candidate electoral competition for the median voter is more likely to apply.

---

[6] We could also get by with the weaker assumption $\mathsf{E}(\varepsilon^1 \mid S = 0) = \mathsf{E}(\varepsilon^1 \mid S = 1)$, possibly different from 0, according to which ε^1 (and ε^0) would be correlated with η in the same way across states.

Formally, such interactions would show up as non-linearities in $F^S(\mathbf{X})$. If we knew the precise functional form $F^S(\mathbf{X})$, through which $\mathbf{X}$ affects the performance in each state, this would not be a problem. But we do not, so again we need additional assumptions. The most parsimonious assumption is that the constitution only has a *direct* effect on performance, which is always the same irrespective of the values taken by the variables in $\mathbf{X}$. In other words, we assume away any interaction effect between the constitution and the conditioning variables. This is indeed our assumption when estimating the constitutional effect in a linear regression.

More precisely, suppose that the data generating process $F^S(\mathbf{X})$ determining performance in state S is linear with constant coefficients, except for a constitution-dependent intercept. Thus, equations (5.3) take the form:

$$
\begin{aligned}
Y_i^1 &= F^1(\mathbf{X}_i) + \varepsilon_i^1 = \alpha^1 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i^1 \\
Y_i^0 &= F^0(\mathbf{X}_i) + \varepsilon_i^0 = \alpha^0 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i^0 \ .
\end{aligned}
\tag{5.8}
$$

Under this assumption, the constitutional effect is just:

$$
\alpha = E\left\{\mathsf{E}(Y^1 - Y^0 \mid \mathbf{X})\right\} = \alpha^1 - \alpha^0 \ .
\tag{5.9}
$$

A less restrictive formulation would allow some of the slope coefficients $\boldsymbol{\beta}$ to differ across the constitutional states in (5.8). Denoting these coefficients by $\boldsymbol{\beta}^S$, we would then define the constitutional effect as

$$
\alpha = E\left\{\mathsf{E}(Y^1 - Y^0 \mid \mathbf{X})\right\} = \alpha^1 - \alpha^0 + (\boldsymbol{\beta}^1 - \boldsymbol{\beta}^0)E(\mathbf{X}) \ .
\tag{5.10}
$$

In the regression analysis conducted in Chapters 6 and 7, we will mainly impose the linearity assumption behind (5.9), but experiment somewhat with non-linear specifications.

5.3.3 Ordinary Least Squares

The simplest linearity assumption plus conditional independence allow us to estimate the constitutional effect α as the coefficient on S in a linear regression of Y on $\mathbf{X}$ and S. We now explain why. Recall that we observe $Y_i = S_i Y_i^1 + (1 - S_i)Y_i^0$. Exploiting linearity, we can replace Y_i^1 and Y_i^0 by the corresponding expressions in (5.8) to get:

$$
\begin{aligned}
Y_i &= S_i(\alpha^1 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i) + (1 - S_i)(\alpha^0 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i) = \\
&= \alpha^0 + \boldsymbol{\beta}\mathbf{X}_i + S_i(\alpha^1 - \alpha^0) + e_i \ ,
\end{aligned}
\tag{5.11}
$$

where the error term e_i in the second line is defined by $e_i = \varepsilon_i^0 + S_i(\varepsilon_i^1 - \varepsilon_i^0)$.

Equation (5.11) looks very familiar. It is tempting to jump to the conclusion that we can easily uncover the constitutional effect α as the estimated coefficient $\widehat{\alpha}$ on the binary variable S in an OLS regression. But the error term of this equation is highly non-standard, since it has a component switching on and off with S. To see what could go wrong, consider the special case with $\alpha^0 = 0$ and no conditioning variables $\mathbf{X}$, such that $Y_i = S_i\alpha + e_i$. The probability limit of the OLS estimate is then:[7]

$$\text{plim } (\widehat{\alpha}) = \frac{\text{Cov}(Y, S)}{\text{Var}(S)} = \alpha + \frac{\text{Cov}(e, S)}{\text{Var}(S)} = \alpha + \mathsf{E}(\varepsilon^1 \mid S = 1) - \mathsf{E}(\varepsilon^0 \mid S = 0) \ . \tag{5.12}$$

This is where the assumption of conditional independence is essential. By (5.7), $\mathsf{E}(\varepsilon^1 \mid S = 1) = \mathsf{E}(\varepsilon^1 \mid S = 0) = 0$ and likewise for ε^0. Both the last two terms in the right-most expression of (5.12) are thus equal to zero, which guarantees an unbiased OLS-estimate of α.

To appreciate which possible sources of bias in OLS estimates we are ruling out by the conditional-independence assumption, rewrite the last two terms on the right-most side of (5.12) as:

$$\left[\mathsf{E}(\varepsilon^0 \mid S = 1) - \mathsf{E}(\varepsilon^0 \mid S = 0)\right] + \mathsf{E}(\varepsilon^1 - \varepsilon^0 \mid S = 1) \ . \tag{5.13}$$

Consider the first two terms inside the square brackets. These would be non-zero if there were non-zero correlation between ε^0 and S, a version of the common problem of omitted variables. To pick up the earlier example, this problem would arise if, in a corruption regression, we leave out some determinants, such as colonial history, also likely to have influenced the selection

---

[7]To derive the last equality in (5.12), note that

$$\text{Cov}(e, S) = \mathsf{E}(eS) - \mathsf{E}(e)\mathsf{E}(S) =$$
$$P\mathsf{E}(\varepsilon^1 \mid S = 1) - P^2\mathsf{E}(\varepsilon^1 - \varepsilon^0 \mid S = 1)$$

and that
$$\text{Var}(S) = \mathsf{E}(S^2) - [\mathsf{E}(S)]^2 = P(1 - P).$$

Moreover, $\mathsf{E}(\varepsilon^0) = 0$ implies

$$P\mathsf{E}(\varepsilon^0 \mid S = 1) = -(1 - P)\mathsf{E}(\varepsilon^0 \mid S = 0).$$

Using these expressions and simplifying, we obtain (5.12). The same expression for the OLS bias could be derived if other conditioning variables $\mathbf{X}$ entered the regression.

of the electoral rule. Notice that the direction of this source of bias has the same sign as the correlation between the unobserved determinants of performance and constitution selection, ε^0 and η. In the example, former British colonies are more likely to have majoritarian elections. If they also have less corruption, and colonial history is not included in the corruption regression, the correlation between ε^0 and η is negative. As former British colonies tend to have less corruption (lower ε^0) and are more likely to be majoritarian (high η), we are more likely to observe majoritarian rule ($MAJ = 1$) where corruption is low; conversely, we are more likely to observe proportional rule ($MAJ = 0$) where corruption is high. Not observing the random determinants of corruption (colonial history), we mistakenly attribute the smaller corruption under majoritarian rule to a causal effect, when in fact it is due to the selection of the constitution on unobservables. Hence, the difference inside the square brackets in (5.13) is a negative number, and our estimate of α is biased downwards.

Next, consider the last term in (5.13). This second prospective source of bias is more subtle. It arises if constitutional choices are systematically related to the *heterogeneous* component of the effect of constitutional reform, $\varepsilon_i^1 - \varepsilon_i^0$. This could happen if there were reverse causation: constitution selection is driven by the desire to improve performance in the particular dimension measured by Y. As already discussed in Chapter 2, the political-science literature suggests that the choice of majoritarian ($S = 1$) rather than proportional elections ($S = 0$) may foster better accountability at the expense of less wide-spread representation. Suppose, therefore, that countries where the accountability effect on corruption is particularly strong (i.e., $\varepsilon_i^1 - \varepsilon_i^0$ is negative) choose majoritarian rule, whereas those where it is weak (i.e., $\varepsilon_i^1 - \varepsilon_i^0$ is positive) choose proportional rule. Such choices would imply a negative value of $\mathsf{E}(\varepsilon^1 - \varepsilon^0 \mid S = 1)$, which would once more bias our OLS estimate of α towards finding a negative effect of majoritarian elections on corruption.

Both biases are ruled out under the conditional independence assumptions. The only remaining non-standard feature is that we are estimating a "random coefficient" model: even though the error term e is uncorrelated with S, the country-specific heterogeneity in the constitutional effect remains, unless we assume that $\varepsilon_i^1 = \varepsilon_i^0$ for all i. In other words, we have a heteroscedastic error term and should take that into account when computing standard errors.

In Chapters 6 and 7, we frequently regress performance on a constitu-

tional dummy variable and other controls $\mathbf{X}$ to estimate our parameter of interest. Those estimates rely on the assumptions of linearity and conditional independence. Clearly, the latter assumption becomes more credible if the performance regression includes a large number of variables in $\mathbf{X}$, likely to be correlated with constitutional origin. The parsimonious assumption on functional form makes such a strategy feasible.

5.4 Relaxing conditional independence

While common and convenient, the conditional-independence assumption is very strong. In Chapter 4, we saw that countries in different constitutional groups differ systematically in the observable variables known (from Chapter 3) to influence policy outcomes. But how do we know that we have controlled for all such common determinants? It could very well be that some unobserved determinants of policy also differ systematically across constitutional groups.

   If the conditional-independence assumption is violated, we have seen that OLS-estimates of the constitutional effect become biased. Clearly, this is an instance of a well-known problem in econometrics, known as *selection bias.* In the context of our application, we present two ways of dealing with this problem. One relies on finding instrumental variables isolating some truly exogenous variation in constitutional rules. The other way relies on adjusting our estimates of the constitutional effect for "self-selection", i.e., for any remaining correlation between selection and performance.

5.4.1 Instrumental variables

How can instrumental variables solve the selection-bias problem?[8] That is, how can some truly exogenous variation in constitutional rules be isolated and used in the estimation? Suppose we find a variable – an instrument – Z, which is correlated with the constitutional state S, but not with the error term e in

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[8]Most modern mainstream econometrics texts, such as Green (2000), Ruud (2000) and Wooldridge (2002), provide a general treatment of instrumental variables, while Stock (1999) and Angrist and Krueger (2001) give easily accessible introductions. For the specific problems of using instrumental variables to estimate average treatment effects under conditions of self-selection, see Wooldridge, (2002, ch. 18), Heckman et al. (1999), and Angrist and Krueger (1999).

(5.11). Formally, we thus require $\text{Cov}(Z, S) \neq 0$, but $\text{Cov}(Z, e) = 0$. Under these conditions, we can find a consistent estimate of the coefficient on the constitutional dummy variable in (5.11) and hence, of the true constitutional effect, α. To see the main idea, consider the simple special case in the previous section with $\alpha^0 = 0$ and no conditioning variables $\mathbf{X}$, such that $Y_i = S_i \alpha + e_i$. Then, we have: $\text{Cov}(Z, Y) = \alpha \text{Cov}(Z, S) + \text{Cov}(Z, e)$. Now, if $\text{Cov}(Z, S) \neq 0$ and $\text{Cov}(Z, e) = 0$, we can uncover the true value of the constitutional effect as $\alpha = \frac{\text{Cov}(Z,Y)}{\text{Cov}(Z,S)}$ (given that we can consistently estimate the two covariances in this ratio in our sample).

A set of valid instruments, $\mathbf{Z}$, must thus satisfy two requirements. First – and corresponding to $\text{Cov}(Z, S) \neq 0$ above – they must be *relevant.* That is, they should help predict the constitutional state once we control for $\mathbf{W}$, the subset of controls $\mathbf{X}$ influencing both performance and the constitution. In other words, the instruments should enter the selection process of the constitutional rule:

$$S_i = \begin{cases} 1 & \text{as } G(\mathbf{W}_i, \mathbf{Z}_i) + \eta_i \geq 0 \\ 0 & \text{as } G(\mathbf{W}_i, \mathbf{Z}_i) + \eta_i < 0 \end{cases}. \tag{5.14}$$

We may readily check this requirement, e.g., by estimating a linear probability model for (5.14), as we do below, and testing whether the partial correlation between S and $\mathbf{Z}$ is equal to zero.

Second – and corresponding to $\text{Cov}(Z, e) = 0$ above – the instruments must be *exogenous,* that is, they should be uncorrelated with the error term e in (5.11). This requirement is more tricky in our context, as e is not a primitive object, but given by $e = \varepsilon^0 + S(\varepsilon^1 - \varepsilon^0)$. Therefore, an exogenous instrument must satisfy two distinct conditions, corresponding to the two possible sources of bias in the OLS estimates, captured by the two terms discussed in connection with (5.13).[9] The first is standard: $\text{Cov}(\varepsilon^0, \mathbf{Z}) = 0$, namely, the instruments must not help predict the unobserved component of performance in constitutional state 0. The second condition is:

$$\text{Cov}(S(\varepsilon^1 - \varepsilon^0), \mathbf{Z})) = \text{Prob } (S = 1 \mid \mathbf{Z}) \cdot \mathsf{E}[(\varepsilon^1 - \varepsilon^0) \mid \mathbf{Z}, S = 1] = 0 .$$

As discussed in the previous section, the term $(\varepsilon^1 - \varepsilon^0)$ is the country-specific, unobserved change in performance associated with constitutional reform from

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[9]We rule out the remote possibility that the two terms in (5.13) sum to zero even though each of them is non-zero.

$S = 0$ to $S = 1$. Conditional on being in state $S = 1$, this change must be uncorrelated with the instruments. Phrased differently, when controlling for $\mathbf{Z}$, the remaining random component of constitution selection must become uncorrelated with $\varepsilon^1 - \varepsilon^0$. This second requirement could be violated even if the first one is met.

If we have more instruments in $\mathbf{Z}$ than the number of constitutional features S, the model is overidentified and we can test for the exogeneity of the *additional* instruments. Note that the test is valid only under the nul hypothesis that *at least one* of the instruments in $\mathbf{Z}$ is uncorrelated with the error term e in the performance equation, however. Hence, a rejection of the overidentifying restrictions implies that some of the instruments are not valid. But we cannot interpret a failure to reject the overidentifying restrictions as a test of the validity of all instruments. It might be the case that we fail to reject and yet no instrument is valid. The assumption of at least one exogenous instrument is non-testable.

Even if the suggested instruments are exogenous, a possible problem in our application is that of *weak instruments.* This refers to the common situation where the instruments $\mathbf{Z}$, although exogenous, are relatively weakly correlated with the constitution S, given the variables in $\mathbf{W}$. As the correlation between $\mathbf{Z}$ and S becomes weaker, the partitioning of S into exogenous and endogenous components becomes more arbitrary, and when the correlation goes to zero, the bias in the instrumental-variables estimates approaches the OLS selection bias.[10] Clearly, this problem might become worse if the vector $\mathbf{W}$ contains many variables and these are correlated with the instruments, $\mathbf{Z}$. A possible remedy is to choose a parsimonious formulation of the selection equation, excluding most variables from $\mathbf{W}$, so as to preserve a strong correlation between $\mathbf{Z}$ and S. This would not be a good idea if the problem motivating the instrumental-variable estimation were one of reverse causation. In that case, we must set $\mathbf{W} = \mathbf{X}$, i.e., the first stage should include all exogenous variables in the outcome relation. Our main problem is not reverse causation, but omitted variables, however. In this case, the model is recursive and using a parsimonious first-stage relation with few variables beyond the instruments $\mathbf{Z}$ is fine. For more on this point, see Wiggins (2000).

Concretely, our estimates of the constitutional effect on outcomes in

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[10]Staiger and Stock (1997) also show that the ratio between the finite sample bias of IV and OLS estimators can be estimated by $1/F$, where F is the F-statistic on the excluded instruments in the first stage.

Chapters 6 and 7 rely on the method of two-stage least squares. In a first stage, we estimate a model such as (5.14) to decompose the variation in S into an "exogenous" and an "endogenous" component. In a second stage, the exogenous variation in S (namely the projection of S on the instruments) is exploited to estimate the constitutional effect. Because the dependent variable in the first stage takes values between 0 and 1, such as our electoral dummy MAJ, this procedure amounts to imposing the so-called linear probability model. That is, the first stage effectively estimates the probability of a particular country i having a majoritarian electoral rule as a linear function of its characteristics $(\mathbf{W}_i, \mathbf{Z}_i)$. The assumption of a linear first-stage model is more robust to functional form specification, as compared to a probit or logit first-stage model (Angrist and Krueger, 2001).

In Chapters 6 and 7, we use this technique to isolate exogenous variation in electoral rules and forms of government when estimating the constitutional effect on fiscal policy, rent extraction and productivity. Unless noted otherwise, we always use six instruments for our two binary constitutional variables ($PRES$ and MAJ). The first three are the indicator variables for the historical periods when the current electoral rules and political regimes were adopted (1921-50, 1951-80, post 1981). We also include three other measures of geography or cultural heritage already discussed in Chapter 4, namely the distance from the equator ($LAT01$), and the percentage of the population whose mother tongue is English ($ENGFRAC$) or a European language ($EURFRAC$). To diminish the problem of weak instruments, we typically restrict the first-stage regression to these six instruments plus the age of the democracy (AGE), thus omitting all other controls in $\mathbf{X}$.[11]

Are these instruments exogenous? For the three timing variables for constitutional origin, we think the likely answer is yes. There is little reason to expect the pure timing of constitutional adoption to have a systematic effect on fiscal performance, corruption or productivity. To allow the age of democracy to exercise an effect on constitutional choices (constitutional reforms are often adopted at the verge of democratization), we include this

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[11]As anticipated in Chapter 4, the problem of weak instruments is addressed by defining the dummy variables dating the origin of the constitutional feature so that each dummy variable takes a value of 1, if *either PRES or MAJ* originated in the relevant time period. This is only relevant for six countries: all other countries have the same origin for both *PRES* and *MAJ* anyway. This definition allows us to reduce the number of instruments relative to the number of endogenous variables which, in turn, reduces the likely bias of IV estimators in the presence of weak instruments (see Angrist and Krueger, 2001).

variable (measured by AGE) among the first-stage regressors on top of our instruments. This way, the three timing variables should really pick up the pure effect of history on constitutional selection (rather than the birth date of the democracy). Naturally, more distantly adopted electoral rules or political regimes might be correlated with older, and perhaps stronger, democracies which might have systematically different policies. For these reasons, however, variables such as the age and quality of democracy also enter in the vector $\mathbf{X}$, and are thus held constant in the second-stage outcome regressions. It is plausible that the remaining unexplained portion of the performance is uncorrelated with our timing dummies.

The case is different for the other three instruments ($LAT01$, $ENGFRAC$ and $EURFRAC$). As Hall and Jones (1999) and, more recently, Acemoglu, Johnson and Robinson (2001), we would like to argue that they reflect the depth of European cultural influence. Acemoglu et al. show latitude to be strongly correlated with the incidence of tropical disease among early European conquerors and, therefore, their propensity of exploitation as opposed to settlement. This way, geography might have influenced subsequent constitutional choices, with less influence on territories closer to the equator. The current fractions of English and European speakers in a country are likely to reflect the historical penetration of British and (continental) European culture on society, more generally, and constitutional choices, more specifically. Admittedly, these variables could be correlated with other unobserved historical determinants of fiscal policy or corruption. To diminish the correlation with the second-stage error term, we try to include variables such as continental location or colonial origin in the second-stage performance relation. Moreover, as we are confident about the exogeneity of the time dummies for constitutional adoption, we can test the validity of the additional instruments by exploiting the over-identifying restrictions. When this is done systematically in Chapters 6 and 7, we typically do not reject the hypothesis that the additional instruments are exogenous.

Are these six instruments relevant, in the sense of being correlated with the constitutional state? Above, we have tried to argue that there are strong a priori arguments to expect this to be the case. In practice, we have already seen in Chapter 4 that the relative frequencies of alternative electoral rules and political regimes do indeed differ across time periods of adoption, and that the other instruments are strongly correlated with constitution selection. Columns 1 and 2 of *Table 5.1* report the estimates of a linear regression of MAJ and $PRES$ on the six instruments, plus the age of the democracy

(AGE) for the sample of countries in our nineties cross section where these seven variables are available. The results are very similar for other subsamples, defined by the availability of our different performance measures. Overall, we explain 40-50% of the variation in $PRES$ and MAJ.

Table 5.1 about here

As the table shows, the cultural influence variables ($LAT01$, $ENGFRAC$ and $EURFRAC$) have a great deal of explanatory power with regard to both constitutional features, and the signs of their coefficients conform with prior beliefs. Indeed, their explanatory power is a strong reason for using them as instruments along with the three indicator variables dating the origin of the constitution. If we only use the timing variables, these explain relatively little of the variation in $PRES$ and MAJ. In the regression for presidential regimes displayed in the table, none of the three constitutional timing variables is significant in isolation, but an F-test comfortably rejects the hypothesis that they do not jointly belong to the regression. Their partial correlation with majoritarian elections is considerably weaker, however, and the F-test cannot reject the hypothesis that the coefficients on the timing variables are zero.[12]

When interpreting the results in the next chapter, we should thus bear in mind that the instrumental-variable estimates of the constitutional effects of majoritarian elections may be less reliable. The major sources of variation in the first-stage regression are the cultural influence variables ($LAT01$, $ENGFRAC$ and $EURFRAC$) and we are less certain that this variation is truly exogenous to outcomes. This problem is smaller when estimating the constitutional effect of presidential regimes, where the timing variables for constitutional origin play a more important role in the first stage.

5.4.2 Adjusting for selection

A second way around the presence of selection bias is to first estimate the bias in (5.13) and then correct our estimates of the constitutional effect.[13] To simplify the exposition of this method, we assume that $\varepsilon_i^1 = \varepsilon_i^0 = \varepsilon_i$: the

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[12]Specifically, a regression of $PRES$ on the three timing variables and a constant has an R^2 of 0.124 and an F-statistic with a p-value of 0.012, whereas the same regression for MAJ has an R^2 of 0.065 and an F-statistic with a p-value as high as 0.135.

[13]Maddala (1983) is the classic reference on econometrics with so-called limited-dependent variables. It includes an exhaustive discussion of estimation techniques to address prospective selection-bias problems in a variety of models.

unexplained part of performance is common across constitutional states and denoted by ε, that is, we abstract from the country-specific, heterogenous part of the constitutional effect. (With a heterogenous constitutional effect, a similar estimator to the one below can be developed under additional assumptions, as discussed in Wooldridge, 2002, Sect. 18.4).

Maintaining the linearity assumption in the outcome relation, the equation to be estimated (5.11), can be rewritten as a so-called switching regression model:

$$
\begin{aligned}
Y_i &= \alpha^1 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i &\text{if}\quad S_i = 1 \qquad (5.15)\\
Y_i &= \alpha^0 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i &\text{if}\quad S_i = 0 \ .
\end{aligned}
$$

Inferring the constitutional effect from the estimated coefficient of S in an OLS regression (5.11) is equivalent to estimating the constitutional effect from: $\hat{\alpha} = \mathsf{E}(Y \mid \mathbf{X}, S = 1) - \mathsf{E}(Y \mid \mathbf{X}, S = 0)$. This is almost like estimating the two equations in (5.15) separately, and then subtracting the two estimated intercepts. If conditional independence is violated, however, the terms $\mathsf{E}(\varepsilon \mid S = 1)$ and $\mathsf{E}(\varepsilon \mid S = 0)$ are not zero. Just as before, this biases the OLS estimate, which converges to:

$$
\text{plim} (\hat{\alpha}) = \alpha + [\mathsf{E}(\varepsilon \mid S = 1) - \mathsf{E}(\varepsilon \mid S = 0)] \ . \qquad (5.16)
$$

The last term in (5.16) constitutes the selection bias already discussed in the previous section. Heckman (1974, 1976a, 1979) pioneered the development of methods for dealing with this problem.[14] These methods rely on an assumption about functional form. Specifically, suppose that the unobserved determinants of performance, ε, and constitution selection, η, are jointly normally distributed, with correlation coefficient ρ and standard errors σ_ε and σ_η, respectively. Following the argument in Maddala (1983), these assumptions imply:

$$
\mathsf{E}(\varepsilon \mid \mathbf{W}, \mathbf{Z}, S = 1) = \mathsf{E}(\varepsilon \mid \eta > -G(\mathbf{W}, \mathbf{Z})) = \rho\sigma_\varepsilon M^1(G(\mathbf{W}, \mathbf{Z})) \ . \quad (5.17)
$$

---

[14]In fact, a problem quite similar to ours of identifying a true constitutional effect appears in another early paper by Heckman (Heckman, 1976b). Landes (1968) had analyzed how the existence of fair employment laws affected the status of blacks across US states. Relying on methods like those in this section, Heckman argued that such estimates can fail if the possibility of selection bias is not taken into account (states where blacks are better treated could be more likely to have fair employment laws, or the demand for such laws could be higher in states where they are treated badly).

The first equality follows from (5.14). The second follows from the formula for the conditional mean of a truncated bivariate normal, where $M^1(G(\mathbf{W}, \mathbf{Z})) = \phi(G(\mathbf{W}, \mathbf{Z}))/ \Phi(G(\mathbf{W}, \mathbf{Z}))$ is the ratio between the density, ϕ, and the cumulative, Φ, of a standard normal distribution evaluated at the point $G(\mathbf{W}, \mathbf{Z})$, an expression also called the (inverse) Mills ratio. Similarly,

$$\mathsf{E}(\varepsilon \mid \mathbf{W}, \mathbf{Z}, S = 0) = \mathsf{E}(\varepsilon \mid \eta < -G(\mathbf{W}, \mathbf{Z})) = \rho\sigma_\varepsilon M^0(G(\mathbf{W}, \mathbf{Z})), \quad (5.18)$$

where $M^0(G(\mathbf{W}, \mathbf{Z})) = \phi(G(\mathbf{W}, \mathbf{Z}))/[1 - \Phi(G(\mathbf{W}, \mathbf{Z}))]$.

If we knew the value taken by the right-hand-side of (5.17) and (5.18), we could correct for the selection bias in (5.16) and obtain an unbiased estimate of the true constitutional effect. Expressions $M^S(\cdot)$ are known functions of $G(\mathbf{W}, \mathbf{Z})$, while parameters ρ, σ_ε and those of the function $G(\cdot)$ are unknown. These parameters are identified, however, and can be jointly or sequentially estimated from the constitution selection equation (5.14), and the performance equations (5.15). The Heckman-style adjustment procedure amounts to precisely this kind of correction.

In Chapters 6 and 7, we follow this approach. We estimate a probit model for the constitution-selection equation (5.14), with a linear specification of $G(\cdot)$. From these estimates, we can retrieve consistently estimated values of the two Mills ratios for each country in the sample $M_i^S, S = 0, 1$. We estimate the parameters ρ, σ_ε, α^S, $\boldsymbol{\beta}$ by (5.15) augmenting each equation by the estimated Mills ratios according to (5.17) and (5.18).[15] The constitutional effect α is then just the difference between the estimates of α^1 and α^0. Note that this procedure also enables us to test the nul hypothesis of conditional independence, namely that the correlation coefficient ρ is zero. The estimation can either be done by maximum likelihood, or else by a two-step procedure, where the Probit selection equation is estimated in the first step and the (augmented) outcome relation in the second.

This procedure has drawbacks, however. The estimates and tests statistics are very sensitive to the distributional assumptions regarding ε and η,

---

[15]These parameters can be separately estimated from the two regimes in (5.15) by rewriting each regime S as:

$$Y_i = \alpha^S + \boldsymbol{\beta}\mathbf{X}_i + \rho\sigma_\varepsilon \hat{M}_i^S + v_i,$$

where $\hat{M}_i^S = M^S(\hat{G}(\mathbf{W}_i, \mathbf{Z}_i))$ is the Mills ratio estimated in the first step. Since the Mills ratios have been consistently estimated, the error term v_i now has a zero mean and is uncorrelated with the included variables.

and to the assumed linearity of the performance equation.[16]   The reason
is that the outcome relation now includes a specific and highly non-linear
function of the variables $\mathbf{W}$ which, in turn, is a subset of the controls $\mathbf{X}$
influencing constitution selection. This critique applies most forcefully when
we have no valid instrument (the set of variables $\mathbf{Z}$ is empty). Identification
of α is then *only* achieved through a functional-form assumption. Specifi-
cally, the non-linearity of the subset $\mathbf{W}$ of variables in the second step only
reflects the Mills ratio, and not the performance equation which is instead
assumed to be linear. A set of valid instruments $\mathbf{Z}$ makes the identification
more robust, as the instruments are excluded from the outcome regression.
Nevertheless, if the normality assumption for η and the linearity assumption
for the outcome regression fail, our correction for selection bias could be off
– possibly way off – and we could falsely reject the nul hypothesis of zero
correlation between ε and η (see Maddala, 1983 for an extensive discussion).

Another drawback of the above procedure is that it fails to address the
possibility of a heterogenous treatment effect - the second source of bias due
to $\varepsilon_i^1 \neq \varepsilon_i^0$. While the adjustment for selection could be extended to this more
general case, we believe that our data set is too small for such an extension
to be meaningful.[17]

A final issue when applying the adjustment for selection in Chapters 6
and 7, is how to specify the probit for constitution selection. Our specifi-
cation reflects some concern for the above-mentioned fragility to functional
form. To obtain more robust identification, we always include the six instru-
mental variables (corresponding to $\mathbf{Z}$) discussed in the previous subsection.
Otherwise, we choose a parsimonious specification (few variables in $\mathbf{W}$), only
adding the age of democracy (AGE), and two other variables correlated with
both constitutional features, namely British colonial origin and a dummy
variable for Latin America. Columns 3 and 4 of *Table 5.1* show the coeffi-
cient estimates of these Probit regressions. Indeed, these columns coincide

---

[16]In fact, the critical assumptions are that the error term η of the constitution selection
equation is normal, and that the mean of ε conditional on η is a linear function of η; both
assumptions are satisfied if η and ε are bivariate normal.

[17]More precisely, we would allow for separate distributions for ε_i^1 and ε_i^0 . Imposing the
assumption of trivariate normality, we would allow for separate correlation coefficients ρ^0
and ρ^1 between these errors and η. These would enter separately in the expressions for
(5.17) and (5.18), and be estimated along with the other parameters in an augmented
outcome relation much as in the procedure explained above. (See Wooldridge (2002), Sect
18.4).

with one of the specifications already displayed in *Table 4.5*, and are only reproduced here for convenience. The general sign and significance picture is the same as for the linear probability model in columns 1 and 2. But the inclusion of British colonial origin and Latin American location strengthens the relation between the timing variables and constitutional outcomes.

5.5 Relaxing linearity

Imposing linearity in applied econometrics is so common that the assumption almost seems innocuous. But is it? As argued above, there are many a priori reasons to expect that the constitutional effect on performance is not only direct, but the result of an interaction with many other variables, such as demographics, or economic development. We can still disregard these non-linearities and approximate the performance equation by a linear regression. But a linear approximation of a non-linear model is only reliable locally, in a neighborhood of the point where the approximation is taken. Once we move away from that point, the approximation can become very bad, and the assumption of linearity very restrictive.

In our context, we rely on the linearity assumption as an approximation of two possibly non-linear relations: the function $Y^1 = F^1(\mathbf{X}) + \varepsilon^1$, determining performance as a function of controls $\mathbf{X}$ in constitutional state $S = 1$; and the same performance equation, $Y^0 = F^0(\mathbf{X}) + \varepsilon^0$, in constitutional state $S = 0$. As explained above, a common maintained assumption in regression analysis is that F^1 and F^0 in (5.8) only differ by an intercept. This assumption may be innocuous if we approximate F^1 and F^0 in the neighborhood *of the same point*. Unfortunately, this may not be the case in our application. Already in Chapter 4, we saw that the variables in $\mathbf{X}$ have very different distributions for the different constitutional states. Recall the tests in *Table 4.7*, where we rejected equality of means between presidential and parliamentary regimes for 7 covariates out of 7, and between majoritarian and proportional countries for 4 covariates out of 7. In other words, presidential and parliamentary countries (or majoritarian and proportional countries) also differ in several other dimensions.

The importance of the linearity assumption can also be stated in terms perhaps more familiar to political scientists (see also the discussion in King and Zeng, 2001). At a given moment in time, we only observe the policy performance of a given country in one constitutional state. But we still seek

the answer to a counterfactual question: how would performance change in a country of our sample, drawn at random in the event of constitutional reform? For this purpose, we compare the performance of countries currently in different constitutional states. We try to draw inferences about counterfactuals – would the most corrupt countries in Western Europe and Latin America, namely Belgium and Paraguay, be less corrupt if they had majoritarian rather than proportional elections? But this can only be done by observing the performance in countries ruled by other constitutions. Thus, if the counterfactual of interest is very far from what we observe – if Belgium and Paraguay differ from currently majoritarian countries in many respects – then our inference is fragile to the functional-form assumption. As the data reveal, the counterfactual of interest can indeed be quite far from what we observe: on average, majoritarian and proportional countries do differ in some respects, while presidential and parliamentary regimes differ even more.

In these circumstances, linearity cannot just be regarded as a convenient local approximation; it is really a binding and important functional form assumption. How can it be relaxed?

5.5.1 Matching estimators

The central idea in matching is to approach the evaluation of causal effects as one would in a controlled experiment. If we are willing to make a conditional independence assumption, we can largely recreate the conditions of a randomized experiment, even though we only have access to observational data. We start by splitting the observations into two groups, often called "treated" and "controls", as in an experiment. Here, that terminology is less useful, however, as the assignment of treatment and control labels would be quite arbitrary. Anyway, the countries are split into two groups according to their constitution ($S = 1$ or $S = 0$), say majoritarian vs. proportional electoral rule. The crucial point is that by conditional independence, constitution selection is random and uncorrelated with performance, once we control for $\mathbf{X}$. Consider countries with the same characteristics, $\mathbf{X}$. Some of these have the constitution $S = 1$, others $S = 0$. The constitutional effect on performance for this group of countries is:

$$
\begin{aligned}
\alpha(\mathbf{X}) &= \mathsf{E}(Y^1 - Y^0 \mid \mathbf{X}) = \\
\mathsf{E}(Y^1 &\mid S = 1, \mathbf{X}) - \mathsf{E}(Y^0 \mid S = 0, \mathbf{X}) \,,
\end{aligned} \tag{5.19}
$$

where the second equality follows from conditional independence, (5.7). The average constitutional effect for the whole sample is then just $\alpha = E\left\{\alpha(\mathbf{X})\right\}$, where the expectation is now taken over the $\mathbf{X}'$s.

In other words, if we are willing to assume conditional independence and consider countries with similar conditioning variables $\mathbf{X}$, the counterfactual distribution of performance is the same as the observed distribution of performance. This enables us to derive the right-most side of (5.19) so that it contains no counterfactual. The unobservable counterfactual outcome for a specific country is estimated from the actual outcomes among countries with similar observable attributes.

Once more, the basic idea is that we should compare the performance of similar countries, because their selection into different constitutions is largely random, as in an experiment. Thus, for each country with a particular constitutional rule, we try to find its "twin" or "set of close relatives" with the alternative rule. In the above example, we try to find countries with majoritarian elections as similar as possible to Belgium and Paraguay. Practically, the computation of $\alpha = E\left\{\alpha(\mathbf{X})\right\}$ could be done by splitting the sample into different groups, each defined by countries with similar values of $\mathbf{X}$. A separate estimate of the constitutional effect is then computed within each group, and the overall constitutional effect is a weighted average of the constitutional effects for all groups.

Note that this argument did not impose any functional form assumption for the performance equation. In fact, we can estimate the constitutional effect non-parametrically by comparing (weighted) mean outcomes. This is the central difference between the method of matching and a linear regression. Matching allows us to draw inferences from *local* comparisons only: as we compare countries with similar values of $\mathbf{X}$, we do not rely on counterfactuals very different from the factuals observed. Relaxing the functional form assumption comes at the price of reduced efficiency in our estimates. Compared to linear regressions, we should thus expect matching estimates of the constitutional effect to be associated with larger standard errors.

5.5.2   Propensity scores

There is a difficulty in this matching methodology, however, which is easily seen in our application. We have already stressed that countries differ in many attributes that may correlate with observed policy outcomes as well as

observed constitutional states; i.e., the relevant dimension of $\mathbf{X}$ is high. Comparing similar countries under different constitutional rules would therefore rapidly exhaust the available data. An important result due to Rosenbaum and Rubin (1983) provides a way out, however. It implies that comparing countries with the same *probability of selecting* a specific constitutional rule, given the relevant controls $\mathbf{X}$, is equivalent to comparing countries with similar values of $\mathbf{X}$.

Specifically, let

$$p_i = p(\mathbf{X}_i) = \text{Prob}[S_i = 1 \mid \mathbf{X}_i]$$

be the conditional probability that country i is in the constitutional state $S_i = 1$, given the vector of controls, $\mathbf{X}_i$. This conditional probability is also called the *propensity score*. Assume the propensity score to be bounded away from 0 and 1 for all countries, the so-called *common-support* condition:

$$0 < p(\mathbf{X}_i) < 1, \text{ all } \mathbf{X}_i.$$

Rosenbaum and Rubin (1983) show that conditioning on vector $\mathbf{X}$ is equivalent to conditioning on the scalar p, in the sense that conditional independence, (5.7), implies:

$$\mathsf{E}(Y^0 \mid S = 0, p(\mathbf{X})) = \mathsf{E}(Y^0 \mid S = 1, p(\mathbf{X})), \tag{5.20}$$

and similarly for Y^1. That is, for countries with similar propensity scores, constitution selection is random and uncorrelated with the potential outcomes (Y^1, Y^0). Hence, we can replace the unobserved counterfactual $\mathsf{E}(Y^0 \mid S = 1, p(\mathbf{X}))$ with its observed counterpart $\mathsf{E}(Y^0 \mid S = 0, p(\mathbf{X}))$.

This result has an important practical implication: when applying the method of matching, we can match countries with similar propensity scores, rather than similar values of $\mathbf{X}$. The curse of dimensionality is reduced as the one-dimensional propensity score p becomes a sufficient statistic for the full-dimensional vector, $\mathbf{X}$.

Repeating the argument in the previous subsection, the constitutional effect for countries with propensity score $p(\mathbf{X})$ is:

$$\alpha(p(\mathbf{X})) = \mathsf{E}(Y^1 - Y^0 \mid p(\mathbf{X})) , \tag{5.21}$$

while the effect for the whole population is

$$\alpha = \mathsf{E}(Y^1 - Y^0) = E\left\{\alpha(p(\mathbf{X}))\right\} , \tag{5.22}$$

where the expectations operator E is taken over the distribution of $p(\mathbf{X})$. (We return to the evaluation of this expression below). As in direct matching, the method forces us to draw inference from *local* comparisons of similar countries. But now we have a simple metric, the propensity score, for measuring similarity. For our purpose, two countries are similar and comparable if they have similar conditional probabilities of being in the same constitutional state, S.

But what does "similar propensity scores" mean in practice? If two countries are too distant, we can no longer perform local comparisons appealing to (5.21). Here, Rosenbaum and Rubin (1983) prove a second result that is very useful. Under the common support assumption, and *conditional on the propensity score*, the observable covariates $\mathbf{X}$ are uncorrelated with the constitutional state, S. Countries with the same propensity score $p(\mathbf{X})$ should thus have the same distribution of $\mathbf{X}$, irrespective of their constitutional state. This result, known as the *balancing property*, suggests a practical test. We could rank countries in terms of their estimated propensity scores and partition this ranking into different "strata". Within each stratum, the distribution of covariates $\mathbf{X}$ should be the same for all countries, irrespective of their constitution. If this version of the balancing property is rejected, either the partition into strata is too coarse and should be refined, or something is wrong with the propensity score.

The latter possibility brings us to the next point. The entire discussion above presupposes that we know the propensity score. But we do not. The estimation of the propensity score thus becomes a crucial step in the methodology. This could be done by a simple probit or logit, as in Chapter 4. But which variables should we include in $\mathbf{X}$? There are two concerns.

First, and crucially, we must respect the conditional-independence assumption. The appropriate specification will thus vary with the particular measure of performance we are investigating. It will also differ from the specification of the selection equation in the Heckman procedure, discussed in the previous section. There, we worry about correlation between the variables $\mathbf{W}$ included in the probit regression (5.1), and the error term η of that same regression. Thus, we should not omit any variables really driving selection. To get robust identification in the second stage, we should also include some variables (instruments) uncorrelated with performance. Here, we worry about conditional independence. Thus, we should not omit any variables really driving performance, and try to include in $\mathbf{X}$ all variables correlated with performance, conditional on the constitutional state. This

speaks in favor of an inclusive logit/probit specification.[18]

The second concern in estimating the propensity score is the common-support condition. If we explain constitutional choice "too well", we shrink the region of overlapping propensity scores between countries belonging to different constitutional groups: for some $S_i = 1$ countries, the estimated propensity score can be very close to 1, for some $S_i = 0$ countries, it can be very close to 0. Matching becomes difficult for these extreme observations, because there are no comparable cases (i.e., no countries in the opposite constitutional state). Preserving enough randomness in the propensity scores thus speaks for a parsimonious logit/probit specification.

5.5.3 Implementation

In Chapters 6 and 7, we experiment with different specifications when estimating the propensity score. For example, when estimating the constitutional effect of the electoral rule on the size of government, we estimate the probability of majoritarian election as a function of four socio-economic covariates – the level of income (LYP), the proportion of old people ($PROP65$), the quality of democracy ($GASTIL$), and the indicator for federal states ($FEDERAL$) – plus the indicators for previous British colonies and Latin American location, all factors likely to correlate with the size of government. *Table 5.2a* lists the 83 countries for which these variables are available in the nineties' cross-section, as well as their actual value for the electoral-rule indicator (MAJ). The countries are ranked by their estimated propensity scores, which are also listed in the table. Notice that the countries with *low* estimated probabilities of majoritarian elections are mostly located in continental Europe and Latin America, regions where elections are indeed most often conducted by proportional rule. In contrast, countries with higher scores are more often previous British colonies and, as we move down the ranking, more often poor countries with young populations, not located in Latin America. We use *Table 5.2a* in the discussion below, but for completeness, *Table 5.2b* shows a similar listing of countries in the order of their propensity scores for the form of government indicator ($PRES$) estimated by

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[18]The contrast between the specification of the propensity score equation and that of the first stage of the instrumental variable estimation is even starker. In the instrumental variable estimation, we want to avoid correlation between the instruments included in the first stage and the error term of the second stage. Here, we instead want to avoid correlation between the error terms of the two equations.

a probit over the same six variables.

Table 5.2 about here

Now that we have an estimate of the propensity score, p, how do we impose the common support condition in practice? To be on the safe side, we define the *estimated* common support as the interval between the minimum estimated p_i among the $S = 1$ countries, and the maximum estimated p_i among the $S = 0$ countries. All observations outside this estimated common support are discarded as non-comparable in terms of observable attributes. In *Table 5.2a*, for example, we discard the six proportional countries (MAJ $= 0$) at the very top of the table, which all have a score lower than the UK, the actual majoritarian country with the lowest estimated probability (about 0.08) of being majoritarian. In the same way, we discard the seven majoritarian countries ($MAJ = 1$) at the very bottom of the table, which all have a higher score than Fiji, the actual proportional country with the highest estimated probability (about 0.85) of being majoritarian. This procedure reduces an already small sample, but it has the advantage of excluding outliers, as we drop countries that may be anomalous in their social and economic conditions. It reinforces the idea that matching estimation relies on inference from local comparisons among similar countries.[19]

Another important question in the practical implementation is how well propensity-score matching eliminates observable differences among countries. In other word, does the balancing property hold up empirically? To check this, we follow the approach suggested in the previous subsection for a given estimate of the propensity score. Consider the propensity score for majoritarian elections, estimated by the probit formulation underlying *Table 5.2a* and the three strata defined in that table, namely countries with low ($p < 0.33$), medium ($0.33 < p < 0.67$) and high ($p > 0.67$) estimated scores, given that they belong to the estimated common support. We test whether the means of a number of covariates are equal in the groups of majoritarian (MAJ $= 1$) and proportional ($MAJ = 0$) countries in each of these three strata. The upper part of *Table 5.3* shows the results of such equal-means tests for a total of nine variables. The first six all enter into the estimation of the propensity score ($LYP, PROP65, GASTIL, FEDERAL, COL\_UKA$ and

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[19]When imposing the common support condition for the form of government in the specification used for fiscal policy, we are forced to discard a large number of presidential regimes in Latin America – cf. *Table 5.2b*.

$LAAM$), but the other three, openness to trade ($TRADE$) and the shares of Protestants and Catholics in the population ($PROT$80 and $CATHO$80) do not. Column 1 shows that, for the full sample, we reject equal means for 3 of these 9 variables at the 5% level (and for 5 of 9 at the 10 % level). What about the three strata defined by the estimated propensity scores? Here we reject equal means in no case out of 27 (9 variables in 3 strata) at the 5 % level. Admittedly, we have fewer observations in each stratum than in the full population, so that a statistical rejection of equal means is more difficult. But the ranking based on the propensity score appears successful in balancing the distribution of observables, and even at the 10% significance level, we only reject 2 cases out of 27. Interestingly, the balancing property appears to extend also to those variables not actually included in the estimation of the propensity score, giving some hope that other – and genuinely unobservable – characteristics may also be balanced out by the matching procedure.

Table 5.3 about here

The lower half of *Table 5.3* shows the results when we use the same stratification and test procedure, given the estimated propensity scores for presidential regimes displayed in *Table 5.2b*. As column 1 shows, the observable differences between presidential and parliamentary countries are very pronounced in the full sample: we clearly reject equal means at the 5% level for 7 covariates out of 9. Once we go to the strata, however, the covariates seem more balanced. We now reject equal means at the 5% level only in 3 cases out of 27 (and in 5 out of 27 at the 10% level). Once again, this might be due to a lack of degrees of freedom in some strata, but the balancing of the distribution extends to the variables not included in the estimation of the propensity score.

Now that we have a metric (the propensity score) that appears to capture similarities, and a sample of reasonably comparable countries (those on the common support), the question is exactly how we should compare the performance among similar countries. There are many possible ways of doing this and each method of comparison corresponds to a specific matching estimator.

A simple method is *stratification.* Countries are ranked on the basis of their estimated propensity scores, and then grouped into different strata, indexed by q. In our applications for electoral rules, we use same three strata as those defined in *Table 5.2a*, namely low (from the UK score to 0.33),

intermediate (0.33 - 0.67) and high (from 0.67 to the Fiji score) propensity scores. Naturally, the proportion of actual majoritarian countries is lower in the bottom stratum, $q = 1$ (7/42 countries) than in the top stratum, $q = 3$ (12/18 countries). Within each stratum q, we then compute the difference in average performance between $S = 1$ and $S = 0$ countries, $\alpha(q)$, as in (5.21). The overall constitutional effect is the weighted average of the $\alpha(q)$ across strata, with weights given by the fraction of countries in each stratum

$$\alpha = \sum_{q=1}^{3} \alpha(q) \frac{N_q}{N},$$

where N is the number of countries on the common support and N_q is the full number of countries in stratum q (counting both $S = 1$ and $S = 0$ countries).

While easily computed, this estimator has the drawback that, in small samples, it can be sensitive to the precise definition of the strata. Hence, we also rely on two other estimators. To explain their logic, it is useful to exploit the law of iterated expectations and re-write equations (5.21) and (5.22) as:

$$\begin{aligned} \alpha \;=\; & P \cdot E\left\{\mathsf{E}(Y^1 \mid p(\mathbf{X}), S = 1) - \mathsf{E}(Y^0 \mid p(\mathbf{X}), S = 1)\right\} && (5.23) \\ & +(1 - P) \cdot E\left\{\mathsf{E}(Y^1 \mid p(\mathbf{X}), S = 0) - \mathsf{E}(Y^0 \mid p(\mathbf{X}), S = 0)\right\} \;. \end{aligned}$$

As in the similar expression (5.6), $P = \mathrm{Prob}(S = 1)$ denotes the (unconditional) probability of observing the constitutional state $S = 1$ in a country drawn at random. The first term in (5.23) is the effect of constitutional reform in countries currently in state 1. We need to replace the unobservable counterfactual $\mathsf{E}(Y^0 \mid p(\mathbf{X}), S = 1)$. As above, conditional independence allows us to use the observed expression $\mathsf{E}(Y^0 \mid p(\mathbf{X}), S = 0)$, if it is computed from countries in the opposite state ($S = 0$), sufficiently similar in terms of $p(\mathbf{X})$. The same applies to the second term in (5.23), capturing the effect of constitutional reform in the $S = 0$ countries.

The *nearest neighbor* method defines "sufficiently similar" in a simple and intuitive way. For each country with $S = 1$, we just find its "closest twin" in the opposite state: the $S = 0$ country with the closest estimated value of p. Close countries can be used several times, if they happen to be the closest match for several $S = 1$ countries. This will raise the standard errors, but is preferable in terms of reduced bias. Countries currently in $S = 0$ that are not the closest twin to any $S = 1$ country are discarded as incomparable. This allows us to compute an estimate of the constitutional effect for countries

currently in $S = 1$, simply as the average difference in performance between these matched countries. To compute the constitutional effect among the countries currently in state $S = 0$ – the second term in (5.23) – we proceed in reverse. For each country with $S = 0$, we find its closest twin in the opposite state: the $S = 1$ country with the closest estimated value of p. Again, such closest countries can be used several times, while distant matches are discarded. The overall constitutional effect, α, is the weighted average of the constitutional effects for countries currently in states $S = 1$ and $S = 0$, as in (5.23), with weights P and $1 - P$, respectively. Weights P and $1 - P$ are estimated by the *relative frequency* in our sample of countries in states $S = 1$ and $S = 0$, respectively.

In our earlier example, S refers to the electoral rule (as classified by MAJ), which is the best match for the two proportional countries discussed above, Belgium and Paraguay, in nearest-neighbor matching? With the estimated propensity scores in *Table 5.2a*, France is the best match for Belgium, whereas Chile is the best match for Paraguay; in both cases, majoritarian countries with similar observable characteristics. As the table shows, France is also the best match for other proportional Western European countries such as Spain and Portugal, while Chile is the nearest match for several other proportional Latin American countries such as Nicaragua and Ecuador. Admittedly, not all matches suggested by the table are equally intuitive.

The nearest neighbor estimator is intuitively appealing. In a small sample, however, it could be quite fragile: small changes in the specification of the propensity score could change the ranking of countries, thereby switching which observations are more heavily used as close matches. This may imply large swings in the weights assigned to different countries as we change the estimated propensity scores.

To achieve more robustness, we also rely on a third method, namely *kernel-based* matching. The logic is quite similar to that of the nearest-neighbor method. The overall constitutional effect can be expressed as the weighted average of the constitutional effect in the $S = 1$ and $S = 0$ countries, once more with weights given by P and $1 - P$. But here, the match for any particular $S = 1$ country is a weighted average of all $S = 0$ countries within a certain propensity-score distance, with weights declining in that distance, and conversely, when matching the $S = 0$ countries. Specifically, we use a radius of 0.25 (also imposing that countries belong to the estimated common support). In the example of *Table 5.1*, this means that proportional Belgium is matched against seven majoritarian countries. The closest countries like

France and the UK obtain a high weight, whereas the more distant ones within the radius, such as Thailand and the US, obtain a low weight.

5.6 Multiple constitutional states

Above, we have treated the case with only one constitutional feature, measured by a binary variable, $S = 0, 1$. But we are interested in two aspects of the constitution, electoral rule and the form of government. Under the assumption that the constitutional effects of these two features are additive, some of the methods illustrated in this chapter extend directly and without additional assumptions to the case of two constitutional features. The case of OLS is straightforward, and just requires the inclusion of both constitutional dummy variables, MAJ and $PRES$, in the same regression. Similarly, when estimating by instrumental variables, we treat both the electoral rule (as measured by MAJ) and the form of government (as measured by $PRES$) as two endogenous variables appearing in the same performance equation and jointly apply an instrumental-variable estimation to them. Finally, when estimating by matching with the propensity score, we do this separately with one constitutional dimension at a time, and no loss of generality (because of the additivity assumption).

To apply the simple Heckman procedure to two binary variables, however, we need additional assumptions besides additivity. In Chapters 6 and 7, we adjust for selecting one constitutional dimension at a time; the other constitutional dimension is treated as a control generally included in the performance equation, but not in the selection equation. For instance, when estimating the effect of the electoral rule (as measured by MAJ), we include the indicator variable for the form of government ($PRES$) in the performance equation and treat it as one of the control variables, without also adjusting for its endogenous selection. Thus, besides additivity, we here also impose the assumption that the second constitutional feature ($PRES$ in the example above) is randomly assigned to countries; and vice versa, when estimating the effect of the form of government, we impose the assumption that the electoral rule is random.

Absent additivity, we really have four groups of countries, not just two. Can the methods discussed above be generalized if that is the case? In the case of linear regressions and IV estimation, discussed in Sections 3 and 4, the extension is relatively straightforward. We just define three indictor variables

– say, *PROPRES*, *MAJPAR*, and *MAJPRES*, in obvious notation – rather than two, and proceed basically as indicated above, with the proviso that we should be careful in drawing inference from small groups (we only have eleven countries that are presidential and majoritarian, while the other three groups contain about the same number of countries). The Heckman adjustment in Section 4 can, in principle, be extended to deal with self-selection into more than one state. We will not pursue this extension here, however, mainly due to lack of data. For the same reason, we will not extend the matching analysis presented in Section 5 to multiple constitutional states, although this can also be done (see Persson and Tabellini, 2002 for details).

Table 5.1
Constitution selection
OLS and Probit estimates

	(1)	(2)	(3)	(4)
Dep. var.	*PRES*	*MAJ*	*PRES*	*MAJ*
CON2150	-0.04	-0.16	0.15	-1.38
	(0.14)	(0.16)	(0.72)	(0.82)*
CON5180	-0.12	0.07	-0.04	0.13
	(0.18)	(0.24)	(0.63)	(0.68)
CON81	0.27	0.06	1.52	0.23
	(0.20)	(0.25)	(0.73)**	(0.72)
LAT01	-1.37	-0.88	-5.15	-4.19
	(0.33)***	(0.39)**	(1.79)***	(1.57)***
ENGFRAC	-0.69	0.92	-3.26	2.62
	(0.12)***	(0.12)***	(1.02)***	(0.90)***
EURFRAC	0.42	-0.35	0.71	0.74
	(0.11)***	(0.13)**	(0.61)	(0.72)
AGE	0.54	0.20	3.83	0.14
	(0.31)*	(0.29)	(1.51)**	(1.48)
COL_UKA			-0.05	1.02
			(0.67)	(0.62)
LAAM			1.61	-1.96
			(0.63)**	(0.80)**
Sample	90s, broad	90s, broad	90s, broad	90s, broad
Estimation	OLS	OLS	Probit	Probit
F: all *CON*= 0	3.66**	0.52		
Obs.	78	78	78	78
R2	0.48	0.40	0.51	0.50

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
F: all *CON*=0 refers to the F-statistic for the test that the coefficients on *CON2150, CON5130* and *CON81* are all zero in columns 1 and 2
R2 (unadjusted) for OLS, pseudo R2 for Probit.

Table 5.2
Estimated propensity scores

(a) Majoritarian elections

Country	PSCORE	MAJ	Country	PSCORE	MAJ
Uruguay	**0.052**	**0**	Nepal	0.337	1
Sweden	**0.070**	**0**	South Korea	0.355	0
Greece	**0.073**	**0**	Bangladesh	0.371	1
Bulgaria	**0.075**	**0**	Philippines	0.377	1
Italy	**0.077**	**0**	Namibia	0.419	0
UK	0.078	1	Barbados	0.496	1
Romania	0.083	0	New Zeland	0.568	1
Peru	0.084	0	Jamaica	0.582	1
Belgium	0.090	0	Ireland	0.617	0
Norway	0.090	0	Canada	0.641	1
France	0.093	1	Singapore	0.659	1
Spain	0.095	0	Israel	0.673	0
Latvia	0.101	0	Sri Lanka	0.674	0
Portugal	0.104	0	Trinidad&Tobago	0.694	1
Denmark	0.105	0	Australia	0.735	1
Hungary	0.106	0	South Africa	0.757	0
Japan	0.108	1	Cyprus (G)	0.759	0
Colombia	0.112	0	Malta	0.760	0
Estonia	0.114	0	Bahamas	0.763	1
Guatemala	0.115	0	Pakistan	0.781	1
Czech Republic	0.126	0	Uganda	0.790	1
Luxembourg	0.127	0	Gambia	0.794	1
Chile	0.128	1	Ghana	0.797	1
Argentina	0.132	0	Zimbabwe	0.808	1
Finland	0.132	0	Belize	0.812	1
Paraguay	0.133	0	Fiji	0.828	0
Slovak Republic	0.141	0	**Malawi**	**0.831**	**1**
Nicaragua	0.148	0	**St. Vincent&Granada**	**0.856**	**1**
Dominican Republic	0.152	0	**Zambia**	**0.856**	**1**
Netherlands	0.153	0	**Malaysia**	**0.857**	**1**
Ecuador	0.157	0	**Mauritius**	**0.873**	**1**
Germany	0.160	0	**India**	**0.886**	**1**
Russia	0.161	0	**Papua New Guina**	**0.904**	**1**
Poland	0.177	0	**Botswana**	**0.924**	**1**
Bolivia	0.181	0			
Honduras	0.185	0			
Mexico	0.194	0			
Austria	0.199	0			
Iceland	0.212	0			
Switzerland	0.214	0			
Turkey	0.220	0			
Brazil	0.230	0			
Costa Rica	0.240	0			
El Salvador	0.258	0			
Thailand	0.264	1			
Venezuela	0.292	0			
USA	0.297	1			
Senegal	0.320	0			

PSCORE is the predicted value of a logit regression of MAJ on LYP, PROP65, FEDERAL, GASTIL, LAAM, COL_UKA
Boldface observations are discarded to impose common support.

(b) Presidential regimes

Country	PSCORE	PRES	Country	PSCORE	PRES
Cyprus (G)	0.017	1	St. Vincent&Granada	0.481	0
New Zeland	0.017	0	Turkey	0.519	0
Malta	0.018	0	Uruguay	0.532	1
Ireland	0.018	0	Zimbabwe	0.541	1
Sweden	0.024	0	Ghana	0.545	1
Norway	0.024	0	Jamaica	0.559	0
Luxembourg	0.027	0	Zambia	0.567	1
Denmark	0.029	0	Gambia	0.576	1
Israel	0.029	0	Philippines	0.582	1
Belgium	0.031	0	Bangladesh	0.632	0
Finland	0.036	0	Malawi	0.640	1
Italy	0.036	0	Malaysia	0.653	0
UK	0.037	0	Nepal	0.699	0
Netherlands	0.038	0	Uganda	0.699	1
France	0.039	0	Chile	0.708	1
Japan	0.042	0	Costa Rica	0.746	1
Mauritius	0.045	0	India	0.769	0
Spain	0.046	0	Senegal	0.784	0
Iceland	0.047	0	**Russia**	**0.836**	**1**
Portugal	0.047	0	**Ecuador**	**0.866**	**1**
Greece	0.072	0	**El Salvador**	**0.868**	**1**
Australia	0.096	0	**Colombia**	**0.895**	**1**
Hungary	0.100	0	**Dominican Republic**	**0.901**	**1**
Singapore	0.104	0	**Bolivia**	**0.903**	**1**
Canada	0.107	0	**Paraguay**	**0.925**	**1**
Bulgaria	0.132	0	**Argentina**	**0.931**	**1**
Czech Republic	0.137	0	**Honduras**	**0.933**	**1**
Botswana	0.150	0	**Guatemala**	**0.946**	**1**
Barbados	0.153	0	**Peru**	**0.948**	**1**
Poland	0.153	0	**Nicaragua**	**0.954**	**1**
Germany	0.163	0	**Venezuela**	**0.959**	**1**
Switzerland	0.169	1	**Brazil**	**0.975**	**1**
USA	0.182	1	**Mexico**	**0.978**	**1**
Austria	0.182	0			
South Korea	0.185	1			
Slovak Republic	0.191	0			
Latvia	0.197	0			
Fiji	0.208	0			
Estonia	0.212	0			
South Africa	0.225	0			
Trinidad&Tobago	0.249	0			
Papua New Guina	0.250	0			
Bahamas	0.252	0			
Sri Lanka	0.305	1			
Belize	0.337	0			
Thailand	0.425	0			
Namibia	0.430	1			
Romania	0.454	0			
Pakistan	0.477	1			

PSCORE is the predicted value of a logit regression of *PRES* on *LYP, PROP65, FEDERAL, GASTIL, LAAM, COL_UKA*
Boldface observations are discarded to impose common support.

Table 5.3
Balancing property
Equal-means tests for different constitutional groups

	Whole sample	p < 0.33	0.33 < p < 0.67	0.67 < p
MAJ=1 vs. MAJ=0				
LYP	0.04	0.04	0.62	0.21
PROP65	0.01	0.32	0.90	0.04
GASTIL	0.08	0.33	0.55	0.37
FEDERAL	0.93	0.79	0.57	0.48
COL_UKA	0.00	0.69	0.42	0.35
LAAM	0.34	0.27	0.39	0.17
TRADE	0.44	0.13	0.93	0.31
PROT80	0.94	0.56	0.75	0.37
CATHO80	0.00	0.11	0.46	0.83
PRES=1 vs. PRES=0				
LYP	0.00	0.87	0.01	0.54
PROP65	0.00	0.34	0.39	0.86
GASTIL	0.00	0.59	0.22	0.71
FEDERAL	0.22	0.07	0.30	0.27
COL_UKA	0.44	0.88	0.56	0.83
LAAM	0.00	0.53	0.23	0.22
TRADE	0.01	0.33	0.34	0.40
PROT80	0.03	0.65	0.60	0.22
CATHO80	0.00	0.28	0.24	0.02

Probabilities of falsely rejecting the hypothesis of equal means across constitutional groups under the hypothesis of equal variances.
Strata defined on the common support of propensity scores, p, estimated by logit regressions including: *LYP, PROP65, GASTIL, FEDERAL, COL_UKA, LAAM.*

Chapter 6

Fiscal Policy: Variation across countries

6.1 Introduction

Armed with the methods introduced in the last chapter, we now proceed to estimating the constitutional effects on policy outcomes from cross-country comparisons. We mostly use the data from the 85 democracies in our nineties cross-section. But when the data so permit, we also check the robustness of the results in cross sections based on our longer panel going back to 1960. In this chapter, we study fiscal policy, namely the size and composition of government spending and the budget surplus. The next chapter studies constitutional effects on political rents and productivity.

Our goal is to estimate the effect of constitutional reforms, that is, changing the form of government or the electoral rule. The theories reviewed in Chapter 2 suggest specific hypotheses about the effect of such reforms on fiscal policy. Switching from a parliamentary to a presidential form of government is expected to reduce the size of government and, in particular, spending programs with many beneficiaries (such as general public goods and broad welfare programs). The reason is that under a presidential regime, the majority of voters are not residual claimants on additional tax revenues. Spending is directed towards powerful minorities (rather than towards programs benefiting many), and voters/tax payers can exploit the checks and balances of a presidential regime to keep down overall government spending. No specific prediction has been formulated with regard to the budget deficit.

Switching from proportional to majoritarian electoral rule is also expected to have significant effects on fiscal policy. The predicted effects are similar to those of a switch to presidentialism, though the reasons are different. Many theories predict that plurality rule and small electoral districts (i.e., majoritarian elections) induce spending targeted to small, but pivotal, geographic constituencies. Proportional elections instead induce political parties to seek consensus in broad groups of the population and hence, naturally lead to programs with many beneficiaries. Some theories also predict that majoritarian elections make it easier to limit both the size of government and the size of budget deficits. One reason (though not the only one) is related to the party structure: majoritarian elections reduce the number of parties and the occurrence of coalition governments which, in turn, helps resolving the common pool problems that might be at the root of excessively large governments and deficits.

Throughout the chapter, we do not attempt to discriminate among different theories, nor do we consider the detailed mechanisms through which the constitution has an impact on policy outcomes. Instead, we estimate a variety of reduced forms, where the constitution is allowed to have a direct effect on the policy outcome of interest. Thus, we seek to quantify these constitutional effects, motivated by a set of theoretical priors, but without testing one specific model against another. Since this exercise is repeated for a variety of policy outcomes – in this chapter, as well as in the two following – our empirical results, in the end, paint a comprehensive picture which can be fruitfully contrasted with the theoretical priors. However, we defer this general discussion to the last chapter of the book, after having seen all the results.

As discussed in Chapter 5, the effect of changing the form of government and the electoral rule is estimated by the coefficients of the two binary indicator variables defined in Chapter 4: $PRES$ and MAJ, respectively. The estimated coefficient of $PRES$ measures the constitutional effect of switching from a parliamentary to a presidential system, holding constant the electoral rule and under the assumption that the electoral rule itself is of no importance for this comparison (i.e., under the additivity assumption that the effect of changing the form of government is the same whatever the rule for electing the legislative assembly). Similarly, the estimated coefficient on the variable MAJ measures the effect of switching from proportional or mixed to majoritarian electoral rule for the legislative assembly, holding constant the form of government and under the assumption that the effect is the same

irrespective of the form of government.[1] In some cases, we relax the additivity assumption and allow interactions between constitutional reforms. The constitutional effects estimated under these more general assumptions correspond to the estimated coefficients of the dummy variables $MAJPRES$, $PROPRES$ and $MAJPAR$, with proportional-parliamentary democracies as the default group. The estimated coefficient of $MAJPRES$ thus measures the effect of changing both the electoral rule and the form of government at the same time.

Our empirical findings are discussed in different sections, each referring to a specific fiscal policy outcome. In each section and for each policy outcome, we first estimate the constitutional effects by ordinary (linear) least squares. Then, we relax the conditional-independence assumption, using instrumental variables and Heckman's procedure. Finally, we relax the linearity assumption and estimate the constitutional effect non-parametrically, using propensity-score matching.

Section 2 considers the size of government as measured by overall spending or revenue. We find presidential and majoritarian countries to have a smaller size of government, as expected. The effect of presidentialism is slightly larger and more robust. Both constitutional effects are weaker in the earlier time period and stronger in the nineties cross section, thereby suggesting that presidential regimes and majoritarian elections have led to smaller governments because they have dampened their growth in the postwar period.

In Section 3, we evaluate the size of broad welfare-state programs. In this case as well, we confirm our priors: presidential regimes and majoritarian elections have a negative effect on the size of welfare-state programs, but these effects are weaker than for the overall size of government. Relaxing conditional independence and linearity suggests that the negative effect of majoritarian elections is the more stable result.

Section 4 extends the analysis of constitutional effects to the budget balance. We find one very stable result across time periods and estimation

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[1] As discussed in Chapter 4, majoritarian here refers to strictly majoritarian, while the alternative state ($MAJ = 0$) aggregates strictly proportional and mixed electoral rules. Allowing for a finer partition of the electoral rule, using our indicator for mixed electoral rules ($MIXED$), does not alter the results reported below while the estimated coefficient of $MIXED$ is never statistically significant. This failure to discriminate between mixed and proportional systems might also reflect the relative scarcity of the mixed electoral systems in our sample.

methods, confirming an earlier empirical finding in the literature: majoritarian elections promote smaller deficits (or larger surpluses). The form of government has no stable causal effect on the propensity to run unbalanced budgets, even though unconditionally, presidential regimes have larger surpluses than parliamentary regimes.

Section 5 concludes by summarizing what we have learned from the evidence. Overall, these empirical results are remarkably in line with the theoretical priors, particularly in the case of the electoral rule.

6.2   Size of government

Does the constitution influence the size of government? To answer this question, we measure the size of government by *central* government spending and revenue in percent of GDP (the variables *CGEXP* and *CGREV* introduced in Chapter 3). As discussed in Chapter 3, data on general government spending do not exist or are much less reliable. Nevertheless, when we apply the same methods to the smaller sample of countries where some data on general government are available, the results are similar to those reported below. Moreover, a dummy variable for federal countries ($FEDERAL$) is always included in our basic set of control variables.

6.2.1   OLS estimates

We start by assuming conditional independence and linearity and estimate the constitutional effects by OLS. The results are reported in *Table 6.1*. The most parsimonious specification, in column 1, relies on our nineties' cross-section. It holds constant variables that previous studies or a priori reasoning suggest to be correlated with the size of government spending. As discussed in Chapters 3 and 4, we take these variables to be per capita income (LYH), openness ($TRADE$), two demographic measures ($PROP1654$ and $PROP65$), the age and quality of democracy (AGE and $GASTIL$) and dummy variables for federal and OECD countries ($FEDERAL$ and $OECD$). Being a presidential regime reduces the size of government by 6% of GDP. The point estimate is not only highly statistically significant, but also economically and politically so. Majoritarian elections also appear to produce smaller governments, but here the effect is smaller, about 3% of GDP, and less precisely estimated.

Table 6.1 about here

The next column adds our indicator variables for geographical location (Africa, Asia and Latin America) and colonial origin (UK, Spanish, and Other, all discounted to the present from the date of independence). As discussed in Chapters 4 and 5, these indicator variables are correlated with constitution selection. Hence, the conditional independence assumption is more credible in this more comprehensive specification. Only the dummy variable for Latin America is significantly different from zero. But the estimated constitutional effect of presidential regimes is remarkably stable, the estimate dropping just slightly and maintaining about the same level of precision. The estimated effect of majoritarian elections now exceeds 5% of GDP. These results are quite robust to more parsimonious specifications of the continental dummy variables and the colonial origin variables, dropping one set of dummies but not the other, and to adding other controls such as income inequality, a dummy variable for former socialist countries (not statistically significant), or the age of democracy (AGE) entered both linearly and squared (to allow for different function forms through which the age of democracy influences policy outcomes).

In column 3, we break down the constitutional variables into the finer partition ($MAJPRES$, $MAJPAR$ and $PROPRES$). The effects of the two constitutional features indeed appear to be additive, so that introducing both a presidential form of government and majoritarian electoral rules in a proportional-parliamentary country would reduce the size of government by a whopping 10% of GDP.

In column 4, we measure the size of government by revenue instead of spending (the variable $CGREV$ rather than $CGEXP$). The effect of presidential regimes is the same as before, but that of majoritarian elections is weaker. Later in the chapter, we shall see that the difference between government revenue and spending in majoritarian countries has a counterpart in our results for government deficits (which are consistently smaller in majoritarian countries).

The 80 countries in our broad sample for the nineties (where all variables are available) include some dubious democracies. In weak democracies, the formal constitution might play a less important role as compared to other informal practices and norms (we will return to a direct test of this idea, below). Moreover, some of the weaker democracies tend to be presidential regimes, which might introduce a systematic bias. Column 5 thus restricts the

estimation to the better democracies in a narrower sample already discussed in Chapter 4 (62 countries for which $GASTIL$ is smaller than 3.5). The effect of presidential regimes now appears to be even stronger, whereas that of majoritarian elections remains stable and significant.

What happens when the average size of government is computed for a longer time period than the nineties? Column 6 reports on the same specification as column 2, when the dependent variable is the average outcome across the 60 countries in our panel, starting in 1960. The effect of both presidentialism and majoritarian elections is still negative, but neither estimate is significantly different from zero. The weaker results do reflect the different time period, rather than the different sampling of countries. To show this, column 7 in the table returns to the nineties' cross section, restricting the sample to those countries included in the longer panel. These results strongly suggest that the differences observed in the nineties' data largely result from a faster *growth* of government over the last forty years in countries with parliamentary regimes and proportional elections. We will return to this important theme in Chapter 8.

We have also searched for interaction effects between the constitution and our covariates. In particular, we have tested whether the share of old people, the quality of democracy and income inequality have the same effect on spending under different constitutions. We can reject the nul in the case of income inequality: larger inequality seems to produce a larger government – as expected from a simple median voter model – but only under majoritarian elections and presidential regimes. These estimates are fragile to the sample and how inequality is measured, however, and thus, they are not emphasized here.

In summary, under the assumptions of conditional independence and linearity, the negative constitutional effect of presidential regimes is large (between -5% and -8% of GDP) and robust to the specification. The electoral rule also has an effect on the size of government, associated with strictly majoritarian countries (a negative effect ranging from -3% to -6% of GDP). Both effects conform to prior expectations. They are stronger in the later period, thereby suggesting that the constitution has influenced post-war growth in the size of government.

6.2.2 IV and Heckman estimates

How robust are the above estimates of the constitutional effects when we try to relax conditional independence? The short answer is that they are quite robust.

Consider first the Heckman procedure. As discussed in Chapters 4 and 5, in the first stage, we estimate by probit a constitution selection equation specified as follows. One set of variables measures the date of origin of the current constitution: the three discretely measured indicators of constitutional origin ($CON20, CON2150, CON5180$) and the continuously measured age of democracy (AGE). (Recall that the three indicator variables capture the origin of the current constitution *or* the date of becoming a democracy, whatever came last.) A second set of variables measures the cultural influence of the West, and Great Britain in particular. These are the distance from the equator ($LAT01$) – to measure different penetrations of colonization by the West – and the fraction of the population whose mother tongue is English ($ENGFRAC$) or a European language ($EURFRAC$). Since many countries in Latin America tend to be presidential systems with proportional legislative elections, we also include a dummy variable for Latin America ($LAAM$). Finally, given the importance of British heritage to explain the electoral rule, and since the fraction of the population speaking English is not highly correlated with colonial origin, we also include a variable for UK colonial origin (COL_UKA). These variables have considerable explanatory power for both the form of government and the electoral rule – see *Table 5.1* in Chapter 5.

The policy outcome equation (the second stage) is specified with the usual set of regressors. To minimize the necessary adjustment for the correlation between unobserved determinants of constitution selection and performance, we also include dummy variables for colonial origin and continental location.[2]

The second-stage estimates for the Heckman procedure are reported in columns 1 and 2 of *Table 6.2*. The estimated constitutional effects remain negative and strongly significant. Allowing for an endogenous selection of majoritarian elections (column 2), the estimated correlation coefficient between the random parts of constitution selection and performance (rho in the table) is practically zero. Thus, the estimate is similar to the OLS estimates. When we allow for endogenous selection of presidential regimes (column 1),

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[2]As noted in Chapter 5, we apply the Heckman correction to one constitutional dimension at a time, treating the other dimension as random.

the correlation coefficient is instead positive and high: 0.64. Thus, the OLS estimates are likely to be upward-biased, and the Heckman correction produces an even larger negative estimate of the constitutional effect. These results are quite robust to alternative specifications of the first-stage equation for constitution selection.

Table 6.2 about here

Next, consider instrumental-variable estimation. Here, we exploit the crucial exclusion restriction that some variables entering the first stage do not influence fiscal policy, except through their effect on the constitution, once we hold constat other determinants of policy.

We start with a parsimonious specification for both the first and the second-stage regression. The second-stage regressors include our standard controls, but no continental and colonial indicator variables. The first stage is kept as in the Heckman estimation, except that we drop the dummy variable for Latin America ($LAAM$) and the variable for UK colonial origin (COL_UKA), i.e., the same specification as reported in *Table 5.1* of Chapter 5. Thus, the identifying assumption is that the constitutional dating variables ($CON21$, $CON2150$, $CON5180$), the language variables ($ENGFRAC$ and $EURFRAC$) and latitude ($LAT01$) are all uncorrelated with the remaining unobserved determinants of fiscal policy. The constitutional effects on the size of government are reported in column 3 of *Table 6.2*.[3] The point estimates are similar to and – if anything – larger in absolute value than the OLS estimates of *Table 6.1*. They also closely correspond to those obtained with the Heckman correction in columns 1 and 2.

Our identifying assumption says that any omitted variable is not correlated with our instruments. For instance, if colonial origin or being in Latin America influences the size of government, their effect would appear in the residual of the second-stage equation (because they are omitted in column 3). This would not bias the IV estimates, however, as long as our instruments are not correlated with colonial origin or continental location. We consider

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[3]Thus, among the second stage regressors, only AGE also enters in the first stage. This parsimonious first-stage specification is chosen to avoid excessively weak instruments. Imposing the restriction that only AGE plus the six instruments enter in the first stage, we estimate the first stage by OLS, run the second stage on the predicted values of MAJ and $PRES$, and correct the second stage residuals as discussed by Maddala (1977, ch 11) and Wiggins (2000). The point estimates are very similar (or stronger) if all second-stage controls are added to the first-stage regression.

this a reasonable assumption in the case of the three dating variables, while we are less certain about the remaining three instruments. If we assume the first three instruments to be valid, however, the validity of the remaining three can be tested via the implied over-identifying restrictions. As shown in column 3, we cannot reject the over-identifying assumptions, which reassures us that the estimates are consistent, despite the omission of colonial origin and continental location.

Nevertheless, the power of the over-identification test might be low, since the dating variables are only weakly correlated with constitution selection. Indeed, if we re-specify the first stage by omitting the more dubious instruments ($LAT01$, $ENGFRAC$ and $EURFRAC$), the fit of the first stage becomes sufficiently weak for the estimated constitutional effects to be statistically insignificant (though the point estimate remains negative and, in the case of majoritarian elections, it is even larger in absolute value). For this reason, column 4 reports the results when we add the most likely culprits to the second stage, namely the variables for British colonial origin and Latin American location. The constitutional effect of presidential regimes now drops towards its OLS estimate, but with a larger standard error, while the point estimate for majoritarian elections increases in absolute value, but remains statistically insignificant.[4] One interpretation of these results is as follows. A parsimonious first stage only leaves a small share of the variation in constitutional arrangements explained by the first-stage regressors. This variation is insufficient to exert a significant influence on the size of government, once we have also included all the dummy variables in the second stage (since adding auxiliary controls keeps removing variation from the size of government).

6.2.3 Matching estimates

How robust are the results when we relax the assumption of linearity (but maintain conditional independence), and estimate the constitutional effects non-parametrically with matching methods? As discussed at length in the previous chapter, these quasi-experimental methods involve pairing up coun-

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[4]The results are very similar if the first-stage regression associated with the estimates in column 4 is expanded to also include the dummy variable for Latin America. Adding all the colonial origin and continental variables to the second stage, the standard errors grow even further.

tries with different actual constitutional rules, but similar estimated probabilities – propensity scores – of having selected a particular rule.

The first step is thus to estimate these propensity scores for electoral rules and government regimes, respectively. We have experimented with different estimation methods for the selection equation: probit vs. logit. The differences are minor and we only display the results for the logit estimates.[5] We have also tried different specifications of the variables entering these logits. As explained in Chapter 5, our concern here is very different from the first-stage of the Heckman adjustment for self-selection. To respect conditional independence, we should include the most important determinants of the size of government, also correlated with constitutional selection. At the same time, we should preserve some randomness in the selection process: if we explain constitutional selection too well, the common support becomes empty and the basis for matching is lost. We only report results for the nineties cross section, as we want to check whether the main results hold when relaxing the strong functional-form assumptions.

We report the results for two different logit specifications. Both include four potentially important determinants of the size of government, namely the log of per capita income (LYP), the share of old people ($PROP65$), the quality of democracy ($GASTIL$), and the presence of a federal system ($FEDERAL$). In one specification, we also include the indicators for previous British colonies and Latin American location, which correlate both with the size of government and constitutional selection (adding other indicators, such as Spanish or Portuguese colonial origin is not feasible as we start perfectly predicting some constitutional outcomes). The second specification instead adds the share of English and European language speaking people in the population ($ENGFRAC$ and $EURFRAC$), as well as latitude ($LAT01$).

Table 6.3 displays the results. For each method of matching, we report the estimates obtained under both logit specifications. The underlying standard errors have been estimated by a bootstrapping procedure. The Kernel estimators (reported in columns 1 and 2) are the most reliable in a small sample such as ours, while the nearest matching is the least reliable. As explained in Chapter 5, in a small sample, measurement error or slight changes in the logit specification affect the ranking of countries based on the propen-

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[5]Persson and Tabellini (2002) also report estimates of constitutional effects on the size of government with these same non parametric methods and a similar, but not identical, specification of the first (constitution selection) stage. The results are similar to those reported below.

sity score. With the nearest matching estimator, this can have large impacts on our estimates, while the Kernel estimator is more robust. With the first logit specification, and when imposing the common support restriction, we typically discard 10 to 15 observations. The second logit formulation explains the constitution particularly well, and we end up losing more observations, particularly for presidential regimes where we are left with only 40 countries in the sample of the common support of estimated propensity scores (see Chapter 5).

Table 6.3 about here

Despite these changes in the sample of countries and the estimation methods, the estimates reported in *Table 6.3* confirm the main message of the previous subsections. Given the sample, the results are most directly comparable to those in columns 1 and 2 of *Table 6.1* and *Table 6.2*. According to the more reliable estimates in columns 1-4 of *Table 6.3*, presidentialism reduces the size of government by between 6% and 8% of GDP, while majoritarian elections reduce it by between 4% and 6% of GDP. The nearest neighbor estimators dampen the effect of presidentialism and increase that of majoritarian elections. The standard errors of these estimates are larger than those of the OLS estimates, but that is to be expected as we are trading off less specification bias against higher standard errors in this non-parametric estimation. The most precise estimates are found by the Kernel estimator, which is intuitive as this method is the least sensitive to individual observations.

All in all, allowing for non-linear constitutional effects does not change the conclusions we draw from these data.

6.2.4   Summary

The three sets of results paint a very consistent picture. If we are willing to assume conditional independence, given a large set of covariates, both constitutional effects are negative for the nineties' cross section. Presidential regimes and majoritarian elections each cut the size of government by about 5% of GDP, perhaps more in the case of presidentialism. These results are robust to relaxing the linearity assumption. Relaxing conditional independence does not change the estimated effect of majoritarian elections, whereas the effect of presidential regimes appears to be even larger. The results for

presidential regimes conform with our theoretical prior, obtained from the work discussed in Chapter 2. In the case of majoritarian elections, our prior was more fuzzy, but the empirical results lead us to revise it.

6.3 Composition of government

Do the constitutional effects extend to welfare-state spending? As discussed in Chapters 2 and 3, pensions and unemployment insurance are normally paid out in broad expenditure programs with many beneficiaries in the population at large. This makes geographical targeting much harder than for other types of discretionary spending, particularly in more developed countries. This is why, based on the theory, we expect the size of welfare spending to be smaller in presidential regimes and under majoritarian electoral rule. In this section, we investigate whether our main constitutional variables (*PRES* and *MAJ*) have direct or indirect effects on welfare-state spending, relying on the same battery of methods as for the size of government.

6.3.1 OLS estimates

Table 6.4 reports on a variety of linear regression estimates. We hold constant the same variables as in the standard specification for the size of government. On the whole, the estimated constitutional effects are smaller than for the overall size of government. But the data reveal important interactions between the constitution and other variables also influencing welfare spending.

Column 1 refers to the full sample of countries in the nineties cross-section (*SSW,* our measure of social transfers, is available for a dozen less countries than *CGEXP,* our measure of the size of government). Both presidential regimes and majoritarian elections appear to reduce welfare-state spending by about 2% of GDP, quite a large number. But neither effect is statistically significant (*p*-values of 0.14 and 0.11). The results are similar in other (non-reported) specifications, such as when we drop the dummies for continents and colonial origin, add income inequality, or the age of democracy squared.

The absence of a strong constitutional effect may seem puzzling, given that the size of welfare-state spending is (unconditionally) much smaller in presidential and majoritarian countries – cf. *Table 4.5* in Chapter 4. The key socio-economic covariate driving the result is the proportion of elderly in the population (as measured by *PROP*65). When this variable is held constant,

the estimated constitutional effects are about -2% of GDP in magnitude, but statistically insignificant; when the variable is omitted, these effects are much larger in absolute value and significant. In other words, presidential and majoritarian countries do have smaller welfare spending on average but, in part, this reflects their younger populations.

In column 2, the constitution is further subdivided into four separate groups. As expected, switching both the electoral rule and the form of government is estimated to have the strongest effect (the point estimate of the dummy variable $MAJPRES$ has the largest point estimate in absolute value). But only the estimated coefficient of the dummy variable $PROPRES$ (corresponding to a change in the form of government in proportional countries) is statistically significant.

Table 6.4 about here

As discussed in Chapter 5, constitutional features may shape policy outcomes with different strength at different stages of democracy. If such interactions exist, they may be particularly important here, as welfare-state spending may be precisely triggered by broad political participation. Columns 3-5 of *Table 6.4* show that the quality and age of a democracy indeed interact with alternative constitutional features.

In column 3, we confine the sample of countries to better democracies (56 countries where $GASTIL$ is, on average, lower than 3.5 in the 1990s). Now, the estimated effect of a presidential regime is much stronger (over - 4% of GDP) and significant, as predicted. The effect of majoritarian elections is also stronger, but remains imprecisely estimated.[6]

Columns 4 and 5 return to the full sample of democracies, but interact the electoral rule and the form of government with the age and quality of democracy (measured by AGE and $GASTIL$). Variables $PRES\_OLD$ and MAJ_OLD in column 4 are defined as the product of $PRES$ and AGE, and MAJ and AGE, respectively; the suffix OLD reminds us that higher values of AGE correspond to older democracies. Similarly, the variables $PRES\_BAD$ and MAJ_BAD in column 5 are defined as the product of

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[6]These results on the electoral rule are weaker than the findings by Milesi-Ferretti, Perotti and Rostagno (2002), who estimate a negative and significant effect of less proportional electoral rules on social transfers in the OECD countries from 1960 to 1995 (they neglect the form of government). Restricting the regressions for the longer cross section to the 23 OECD countries in our sample (including the same covariates except the continental and colonial-origin dummies), we obtain an insignificant effect close to zero, however.

PRES and *GASTIL* and *MAJ* and *GASTIL,* respectively, where the suffix *BAD* reminds us that higher values of *GASTIL* correspond to worse democracies.

The estimates yield two results. First, and confirming the results in column 3, presidentialism and majoritarian elections restrain welfare spending only among older and better democracies (i.e., those with higher values of *AGE* and lower values of *GASTIL*). We infer this from the negative estimated coefficients of *PRES_OLD* and *MAJ_OLD* in column 4, and the negative estimated coefficients of *PRES* and *MAJ* in column 5, together with the positive and significant estimated coefficients of *PRES_BAD* and *MAJ_BAD*.

The significant estimated coefficients of variables *AGE* and *GASTIL* in columns 4 and 5, respectively, also suggest a second inference. Older and better democracies (higher values of *AGE* and lower values of *GASTIL*) have significantly higher welfare-state spending only if they are parliamentary and proportional (the default constitutional state).[7] This finding is consistent with the common view among political scientists that proportional elections and parliamentary systems allow for a better representation of disadvantaged groups, that is, the likely beneficiaries of welfare-state spending. In other words, these political institutions might better aggregate the policy preferences of disadvantaged groups into an actual influence on policy. As democracies become older and allow for greater opportunities of political participation, the size of the welfare state increases. This effect of democratization is only present in proportional and parliamentary democracies, however, and not among presidential and majoritarian democracies.[8]

What do the estimates tell us about the overall constitutional effect of presidential regimes or majoritarian elections, under the maintained assumption of conditional independence? In Chapter 5, we defined the constitutional effect as the average effect of constitutional reform in a country drawn at random, a definition that might also include interaction terms. Recalling the

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[7]Summing the coefficients of *AGE* and *PRES_OLD*, we obtain a point estimate of 1.06 with a standard error of 2.04 (transformed to take the linear combination of estimated coefficients into account). Thus, we cannot reject the nul hypothesis that the effect of *AGE* on welfare spending is zero among presidential regimes. The same results are obtained for the set of majoritarian (parliamentary) countries, or for the effects of *GASTIL*.

[8]These interactions between the quality and age of democracy and the constitution are only present when the dependent variable is welfare spending, and not in the case of the overall size of government. This further supports the interpretation proposed in the text.

definition in equation (5.9), in column 5 we should add the estimated intercept (the coefficient on *PRES* or *MAJ*) to the estimated interaction effect (the coefficient on *PRES_BAD* or *MAJ_BAD)* times the average quality of democracy (the average value of *GASTIL*) - or equivalently for the age of democracy in column 4. These calculations for the estimates in columns 4 and 5 produce a point estimate close to that in column 1 of *Table 6.4,* i.e., both presidential regimes and majoritarian elections have lower welfare-state spending by about 2 % of GDP.

Finally, in column 6 of *Table 6.4* we interact our constitutional variables with income inequality (measured by the Gini coefficient in the 1980s and 1990s). One a priori reason for this to be interesting has already been mentioned. The central prediction from the simple median-voter model – that inequality boosts redistributive transfer payments – is most relevant when elections have fewer candidates, which is more likely under majoritarian elections or presidential forms of government. More inequality (higher values of *GINI*) affects welfare spending in opposite directions under different forms of government. The significant negative coefficient on inequality (*GINI*) shows that more *inequality* is associated with a smaller welfare state in parliamentary democracies, contrary to expectations and irrespective of the electoral rule (the estimated coefficient of *MAJ_GIN* is close to zero). Since inequality is measured in the 1980s and 1990s, while these welfare programs have existed in their current form for a longer time span, this might also reflect some reverse causation (larger transfers might reduce inequality). In presidential regimes, inequality is instead associated with higher welfare spending (the sum of the coefficients on *GINI* and *PRES_GIN* is positive and significant). Reverse causation might be less of a problem in Latin America (the home of many presidential regimes), since those welfare programs are more recent and more likely to target urban workers rather than the poor in the countryside.

Do the conditional results on the quality of democracy and income inequality reflect different mechanisms, or are they two sides of the same coin? After all, bad democracies are more likely to have higher income inequality (the simple correlation in our nineties cross section is 0.38). Furthermore, both variables are strongly correlated with presidential regimes (correlation coefficients around 0.5). To answer this questions, we have allowed both interactions to appear simultaneously in the same specification. The results – not reported – are surprisingly stable, despite the relatively few degrees of freedom.

We have also estimated the same set of equations appearing in *Table 6.4* in the longer panel of 60 countries for which data are also available from the early 1970s and onwards (unlike for the overall size of government, social security and other transfer data are not available for the 1960s). All results reported in *Table 6.4* are very similar, including the interaction effects, suggesting that they are not a peculiarity of the 1990s.

6.3.2 IV and Heckman estimates

Next, we relax the conditional-independence assumption, using instrumental variables and the Heckman two-step procedure in the broad nineties cross section, where we have the largest number of countries. Despite the interaction results just reported, we retain the restriction of a linear model with constant slope coefficients. It would just be too demanding on the data to also allow for endogenous constitution selection in this more complex specification; moreover, we do not have reliable instruments for the quality of democracy (as measured by $GASTIL$) or income inequality. Thus, the estimates reported in *Table 6.5* should be compared to the OLS estimates in *Table 6.4*, column 1.

The specification of the first- and second-stage regressions for both the Heckman and the instrumental variables estimates is identical to those for the size of government.[9] In particular, when we estimate by instrumental variables, we report two specifications for the second-stage estimates, one inclusive of the dummy variables for British colonial origin and Latin America (column 4), the other not (column 3). Our previous concerns about the validity of the instruments remain, but are not repeated. Now, the over-identifying restrictions can indeed be rejected at the 10% level for the more parsimonious second-stage specifications (column 3) and at the 5% level for the less parsimonious ones. Recalling the interaction effects identified in the previous subsection, the fragility of the Heckman correction to possible functional form mis-specification is also an issue.

Table 6.5 about here

Despite these concerns, the pattern of the constitutional effects is consistent across the estimates reported in *Table 6.5*, while somewhat different

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[9]Except in the first stage of the Heckman estimation when MAJ is treated as endogenous, where we drop the variable $CON2150$ to avoid a perfect prediction of 9 observations.

from the OLS estimates in *Table 6.4* The presidential effect is not significantly different from zero. In the Heckman estimates, it is about the same size as in the OLS regressions, namely about -2% of GDP (cf. column 1), consistent with the finding that the estimated correlation coefficient between the unobserved determinants of constitution selection and performance (rho) is close to 0. In the instrumental variable estimates, it is practically zero. Overall, relaxing the conditional independence weakens the estimated effect of presidentialism.

The effect of majoritarian elections, on the other hand, is reinforced. The estimated coefficient of MAJ is now negative and statistically significant according to both procedures. Column 2 of *Table 6.5* suggests errors with a strong positive correlation (a rho of +0.47), implying an upward bias in the OLS estimate of the constitutional effect in *Table 6.4.* When the bias is corrected, the constitutional effect of majoritarian elections becomes negative and statistically significant (column 2), a result confirmed by each of the instrumental-variable estimates (columns 3 and 4).

The consistency of these results under two different estimation methods is an indication that accounting for deviations from conditional independence might be important. Once this has been done, there is stronger evidence that majoritarian elections induce a smaller welfare state, while the form of government appears to be less important.

6.3.3 Matching estimates

Finally, we turn to the matching methods and relax the assumption that the welfare-state relation is linear in the covariates. In light of the interactions reported in the OLS regressions, this extension seems quite important for assessing the robustness of our inferences. Once more, we proceed as for the overall size of government by estimating two alternative logit specifications of the propensity score – the same as already discussed for the size of government. The second specification entails a larger loss of countries, particularly when we estimate the presidential effect.

Table 6.6 about here

Table 6.6 displays the results for these specifications and our three matching methods. As noted in the previous section, the Kernel estimators are the most reliable in such a small sample. Despite the different estimators and

first-stage specifications for the propensity scores, most estimates are quite similar to those reported in *Tables 6.4* and *6.5,* if not larger in absolute value: both presidential regimes and majoritarian elections have a negative effect on welfare-state spending of about -2-3% of GDP, although the estimates are rarely statistically significant. As higher standard errors are to be expected, these consistently negative and stable estimates strengthen our belief that both constitutional effects are indeed negative, despite the interaction effects discussed in connection with the OLS regressions.

6.3.4 Summary

Our findings suggest interesting constitutional effects on welfare-state spending. Majoritarian elections cut welfare spending, as predicted by theory, and by as much as 2-3% of GDP. For presidential regimes the evidence points in the same direction, although the estimates are somewhat less robust. Furthermore, there are interaction effects. Both constitutional effects are much stronger among better and older democracies. Moreover, better democracies have larger welfare states, but only if they are proportional-parliamentary. Selection bias seems to be a more severe problem here than for the size of government. Correcting for this reinforces the negative constitutional effect of majoritarian elections, but weakens the effect of the form of government.

6.4 Budget surplus

Is there a constitutional effect on government deficits? Earlier informal work and empirical results suggest that proportional electoral rules may be conducive to government debts and deficits, since they are often associated with unstable governments and coalition governments. Is this also the case in the broad data sets used in this book? To investigate this question in a reduced-form manner, we apply the same approach as in the two earlier sections of the chapter, using the government surplus as a percentage of GDP (SPL) as our dependent variable.

As noted in Chapters 3 and 4, a country cannot keep running a budget deficit forever. The 1990s stand out as a somewhat special decade in this respect. Many countries began running large budget deficits in the 1970s and 1980s, while the 1990s was a period of budgetary consolidation, particu-

larly for some of the countries having accumulated large debts in the earlier decades. To avoid basing our conclusions on data from a decade when several countries were trying to recover from large public debts, we only study the 60-country panel for which we can take averages for the whole period 1960-1998. Most of the results reported below are very similar for the nineties' cross section, however.

6.4.1 OLS estimates

Columns 1-4 of *Table 6.7* report the OLS estimates. Column 1 runs the same specification as our basic regressions in the two previous sections. Countries with majoritarian elections have larger surpluses (smaller deficits) than those with proportional elections – the effect is precisely estimated and quite large, about 2% of GDP. Note that this regression includes a set of continental dummies on top of our standard controls, so that the results do not reflect, say, larger deficits and a greater incidence of proportional countries in Latin America. There is no significant effect of the government regime.

Table 6.7 about here

To address the possibility that larger or smaller deficits simply reflect initial debt levels, in column 2 (and in all remaining columns in the Table), we also control for the level of debt in the first year when deficit data become available. The estimated coefficient on initial debt (not reported) is negative and highly significant, meaning that a larger initial debt indeed leads to a smaller surplus, presumably because of higher interest payments.[10] The majoritarian effect only drops marginally, however, and remains significant. Similarly, the result is robust to excluding the worst democracies (column 4).

Column 3 reports on the results from the finer disaggregation into four constitutional states. Clearly, the main result in the other columns derives from differences within the group of parliamentary countries. This gives some indirect support to the idea that coalition or minority governments – which are much more common under proportional elections – may suffer from a

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[10]Note, however, that in such cross-country regressions, the initial debt variable could be negatively correlated with the error term, thereby leading to a possible downward bias in the estimated debt coefficient (the dependent variable is the surplus).

status-quo bias or a dynamic common pool problem and find it harder to get their fiscal house in order, as compared to majority governments.

Finally, as already noted in Chapter 3, we do not succeed in explaining any considerable fraction of the cross-country variation in the surplus: the adjusted R^2 is low, between 20 and 30%, despite the inclusion of dummy variables for colonial origin and continental location. Other important determinants of budget deficits are unaccounted for by our standard controls. Nevertheless, the results are stable to alternative specifications or the sample of the 1990s. Experiments with various interaction effects do not yield any stable results, but do not change the effect of majoritarian elections reported above.

6.4.2 Heckman estimates

The last two columns of *Table 6.7* relax conditional dependence. Instrumental-variable estimation is problematic in the longer period 1960-1998: the variables dating constitutional origin are no longer reliable instruments because the sample includes some constitutional reforms and countries becoming democracies for the first time (recall from Chapter 4 that the constitutional-origin instruments reflect the year when the constitutional feature was first selected, or the year when the country became a "democracy", whatever came last). Hence, these instruments should be more carefully redefined for the longer period, which we have not done. Since we are only left with the less reliable instruments ($LAT01$, $ENGFRAC$ and $EURFRAC$), we do not report on instrumental variable results. For the same reason, the first stage of the Heckman model does not rely on the variables dating constitutional origin (the auxiliary instruments are less of a problem here, as we can still achieve identification through the functional form assumption, as discussed in Chapter 5).

Consider the constitutional effect of majoritarian elections (column 6 of *Table 6.7*). The OLS estimates placed this around 2% of GDP (lower deficits under majoritarian elections). The Heckman procedure estimates the correlation coefficient between the unobserved parts of the electoral rule and performance to be 0.28. This implies a small positive small bias in the OLS estimates; when this is corrected, the constitutional effect of majoritarian elections drop towards 1% of GDP and becomes statistically insignificant. A similar result is obtained for presidentialism (column 5), where the constitutional effect is now estimated to be very close to zero.

Overall, relaxing conditional independence suggests weaker constitutional effects. This result partly depends on the sample, however. In the cross section of the 1990s (not reported), the constitutional effect of majoritarian elections remains about 2% and is statistically significant also according to the Heckman estimates

6.4.3 Matching estimates

As in earlier sections, we complement the parametric estimates of the constitutional effect with non-parametric estimates obtained by matching methods. Once again, the results rely on two logit specifications of the propensity score estimates. These are identical to the specifications for government size, except that we also include the initial debt level (*CCG_NET_0*). *Table 6.8* shows the results for these two specifications and our three matching methods. In the case of presidential regimes, the second (more comprehensive) logit specification predicts too well in our smaller sample of 60 countries, and the remaining observations on the common support are too few for reliable inference. Hence, for this second specification, we only report the estimates of the constitutional effect for the electoral rule.

Table 6.8 about here

The estimates are most directly comparable to columns 2, 5 and 6 of *Table 6.7*. Once again, majoritarian elections seem to promote larger surpluses (smaller deficits). The effect is estimated to be between 1% and 2% of GDP, though it is seldom statistically significant; as already noted, this is not surprising with this non-parametric method. The estimated effect of presidential regimes fluctuates between being positive and negative, and is never statistically significant.

6.4.4 Summary

All in all, our finding that majoritarian elections cause smaller government deficits is quite robust to statistical pitfalls. The effect is also economically large: about 2 % of GDP. No robust effect seems to be present when we compare presidential versus parliamentary forms of government.

6.5   Concluding remarks

What have we learned about the differences between alternative electoral rules and alternative forms of government and their impact on fiscal policy?

One important conclusion is that electoral rules exert a strong influence on fiscal policy. Majoritarian elections induce smaller governments, smaller welfare states and smaller deficits. These estimated constitutional effects are not only statistically significant and robust. They are also quantitatively relevant. For a country drawn at random from our sample – and over a sufficiently long period to neglect transitory effects – a constitutional reform from proportional to majoritarian elections reduces the size of central government spending by 4-5% of GDP, the size of welfare and social security programs by 2-3% of GDP, and the budget deficit by 1-2% of GDP.

These findings are remarkably consistent with the qualitative predictions of existing theory. As discussed in Chapter 2, there is not a single unified theoretical model of how electoral rules shape fiscal policy. Different authors have emphasized different aspects and implications. But several existing models predict that broad programs with many beneficiaries are larger under proportional elections, and some also predict that proportional elections are associated with larger overall spending and less disciplined fiscal and financial policies. The cross-country evidence uncovered here suggests these theoretical ideas to be on the right track. We have not attempted to discriminate among alternative theories, however, nor have we sought to identify the precise channel through which the electoral rule shapes fiscal policy. Does the constitutional effect operate through the electoral incentives in two-party electoral competition, as some recent theories have suggested? Or is the electoral rule of importance because it influences the party structure and thus, the incidence of coalition governments or the average duration of governments? Discriminating among these alternative hypotheses is the next important step in this research program.

Turning to the form of government, our central empirical result is that presidential regimes create considerably smaller governments than parliamentary regimes. A negative constitutional effect on welfare spending is also present, but it is less robust. Once more, these constitutional effects are quantitatively large and about the same size as for the electoral rule. A reform from parliamentary to presidential regime would shrink the size of overall spending by about 5% of GDP, and the size of welfare programs by about 2% of GDP. The effect on welfare spending is less precisely estimated,

however, perhaps because many presidential regimes have younger populations and it is difficult to separate the effect of the constitution from that of demographics in cross-country comparisons. These fiscal policy effects are in line with our theoretical priors, though here the theory of how the form of government shapes fiscal policy is less advanced than for the electoral rule. No effect of the form of government on budget deficits is apparent from the data, nor did we expect to find one a priori.

The robustness of some of these findings is remarkable, given the variety of estimation techniques employed in this chapter. Cross-country comparisons are often associated with ambiguous and fragile inference. We expected this to be particularly true in our case, given the non-random pattern of constitutional forms and the extensive differences among countries belonging to different constitutional groups. Nevertheless, when it comes to the broad features of fiscal policy investigated in this chapter, the constitutional effects do seem robust to the most common econometric pitfalls in cross-sectional analysis. One reason might be that the unconditional differences in fiscal policy across constitutional groups are indeed very large. As pointed out in Chapter 4, governments in parliamentary countries are about 11% of GDP larger than in presidential countries; the unconditional difference between proportional and majoritarian countries amounts to about 5% of GDP. The unconditional means in the other fiscal policy variables are also very different across constitutional groups. It is difficult to explain away such large differences on the basis of omitted variables, mis-specified functional forms, or other possible econometric pitfalls.

Finally, these cross-country comparisons have revealed some interesting interactions between the formal constitutional rules and the stages of democracy. The effect of the constitution on welfare spending is stronger in older and better democracies. Conversely, older and better democracies are associated with larger welfare states, but only under parliamentary-proportional constitutions. This effect of the stage of democracy on policy outcomes, and its interaction with different constitutional rules, is a theme recurring also in the next chapter, where we focus on political rents and economic development. Although plausible ex post, we did not expect these findings on the basis of existing theory. They deserve more attention in future research.

Table 6.1
Size of government and constitutions
Simple regression estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.	*CGEXP*	*CGEXP*	*CGEXP*	*CGREV*	*CGEXP*	*CGEXP*	*CGEXP*
PRES	-6.08	-5.29		-5.17	-8.29	-3.46	-7.49
	(1.97)***	(1.92)***		(2.44)**	(2.72)***	(3.88)	(2.72)***
MAJ	-3.29	-5.74		-3.03	-5.59	-2.93	-4.81
	(1.73)*	(1.95)***		(1.85)	(2.68)**	(3.09)	(2.75)*
PROPRES			-7.08				
			(2.70)**				
MAJPAR			-7.30				
			(3.02)**				
MAJPRES			-10.36				
			(2.70)***				
Continent	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Colonies	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, narrow	60-90s, broad	90s,obs as(6)
Obs.	80	80	80	76	62	60	60
Adj. R2	0.58	0.63	0.63	0.58	0.60	0.54	0.63

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include standard controls: *LYP, GASTIL, AGE TRADE, PROP65, PROP1564, FEDERAL, OECD*
Narrow sample corresponds to countries where *GASTIL* is less than 3.5

Table 6.2
Size of government and constitutions
Heckman and Instrumental Variables estimates

	(1)	(2)	(3)	(4)
Dep. var.	CGEXP	CGEXP	CGEXP	CGEXP
PRES	-10.50	-5.37	-8.65	-4.50
	(3.98)***	(2.19)**	(3.63)**	(3.89)
MAJ	-5.69	-4.92	-3.90	-5.12
	(1.86)***	(2.57)*	(3.46)	(3.61)
Conts & Cols	Yes	Yes	No	COL_UKA, LAAM
Sample	90s, broad	90s, broad	90s, broad	90s, broad
Endogenous	PRES	MAJ	PRES	PRES
selection			MAJ	MAJ
Estimation	Heckman	Heckman	2SLS	2SLS
	2-step	2-step		
rho	0.64	-0.02		
Chi-2: over-id			4.64	3.61
Adj. R2			0.59	0.60
Obs.	75	75	75	75

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Critical value of chi-2(4,0.05) = 9.49
Always included in second-stage specification (cols 1-4): *AGE, LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, OECD*
First-stage specification of Heckman (cols 1-2) includes: *CON2150, CON5180, CON81, AGE, ENGFRAC, EURFRAC, LAT01, LAAM*
First-stage specification of 2SLS (cols 3-4) includes: *CON2150, CON5180, CON81, AGE, ENGFRAC, EURFRAC, LAT01*

Table 6.3
Size of government and constitutions
Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	CGEXP	CGEXP	CGEXP	CGEXP	CGEXP	CGEXP
PRES	-7.30	-7.91	-5.87	-7.92	-2.54	-4.00
	(2.30)***	(2.90)***	(4.93)	(5.11)	(2.30)	(3.45)
MAJ	-5.76	-6.55	-4.87	-4.08	-6.59	-8.81
	(2.94)*	(2.82)**	(3.65)	(4.16)	(3.06)**	(3.15)***
Estimation	Kernel	Kernel	Strat	Strat	Nearest	Nearest
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Logit	1	2	1	2	1	2
Specif.						
Obs. on	65 PRES	40 PRES	65 PRES	40 PRES	65 PRES	40 PRES
common	67 MAJ	57 MAJ	67 MAJ	57 MAJ	67 MAJ	57 MAJ
support						

Standard errors in parentheses obtained by bootstrapping
* significant at 10%; ** significant at 5%; *** significant at 1%
Kernel, Stratification and Nearest-neighbor estimators described in Chapter 5.5
Logit specifications underlying the propensity score estimates:
1: *LYP, PROP65, GASTIL, FEDERAL, COL_UKA, LAAM*
2: *LYP, PROP65, GASTIL, FEDERAL, ENGFRAC, EURFRAC, LAT01*

Table 6.4
Welfare spending and constitutions
Simple regression estimates

	(1)	(2)	(3)	(5)	(4)	(6)
Dep. var.	SSW	SSW	SSW	SSW	SSW	SSW
PRES	-1.89 (1.27)		-4.42 (1.84)**	0.22 (1.58)	-8.65 (2.94)***	-22.15 (6.74)***
MAJ	-2.01 (1.25)		-2.44 (1.88)	0.32 (1.60)	-4.96 (2.70)*	-4.28 (5.41)
PROPRES		-2.74 (1.58)*				
MAJPAR		-2.72 (1.71)				
MAJPRES		-3.51 (2.31)				
PRES_OLD				-8.54 (3.81)**		
MAJ_OLD				-7.69 (2.99)**		
AGE	1.14 (2.60)	1.51 (2.86)	1.16 (3.60)	9.60 (4.14)**	3.62 (3.09)	-0.84 (2.90)
PRES_BAD					2.67 (1.09)**	
MAJ_BAD					1.50 (0.86)*	
GASTIL	-0.55 (0.57)	-0.61 (0.56)	-1.10 (1.50)	-0.67 (0.58)	-2.43 (0.96)**	-1.39 (0.75)*
PRES_GIN						0.57 (0.16)***
MAJ_GIN						0.06 (0.13)
GINI_8090						-0.33 (0.12)***
Continents and Colonies	Yes	Yes	Yes	Yes	Yes	Yes
Sample	90s, broad	90s, broad	90s, narrow	90s, broad	90s, broad	90s, broad
Obs.	69	69	56	69	69	58
Adj. R2	0.75	0.75	0.73	0.78	0.78	0.81

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include standard controls:
LYP, PROP65, PROP1564, GASTIL, AGE, TRADE, FEDERAL, OECD
Narrow sample corresponds to countries where *GASTIL* is less than 3.5

Table 6.5
Welfare spending and constitutions
Heckman and Instrumental Variables estimates

	(1)	(2)	(3)	(4)
Dep var.	*SSW*	*SSW*	*SSW*	*SSW*
PRES	-1.99	-1.62	0.30	-0.39
	(2.06)	(1.37)	(1.96)	(2.34)
MAJ	-1.76	-3.21	-3.63	-4.13
	(1.13)	(1.64)**	(1.82)**	(2.12)*
Continents and Colonies	Yes	Yes	No	*COL_UKA LAAM*
Sample	90s, broad	90s broad	90s, broad	90s, broad
Endogenous	*PRES*	*MAJ*	*PRES*	*PRES*
Selection			*MAJ*	*MAJ*
Estimation	Heckman 2-step	Heckman 2-step	2SLS	2SLS
Rho	0.08	0.47		
Chi-2: over-id			5.73*	9.81**
Obs.	64	64	64	64
Adj. R2			0.78	0.78

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Critical value of chi2 (4,0.05) = 9.49
Always included in *SSW* equation: *AGE, LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, OECD*
2SLS first-stage specification includes: *CON2150, CON5180, CON81, AGE, EURFRAC, ENGFRAC, LAT01*
Heckman first-step probit specification includes: *CON2150, CON5180, CON81, LAT01, ENGFRAC, EURFRAC, AGE, COL_UKA, LAAM* (*CON2150* dropped from probit for *MAJ* to avoid perfect predictions)

Table 6.6
Welfare spending and constitutions
Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.	*SSW*	*SWW*	*SSW*	*SSW*	*SSW*	*SSW*
PRES	-3.75	-3.11	-3.15	-1.83	-0.45	-2.02
	(2.43)	(1.89)	(3.38)	(2.78)	(1.77)	(1.54)
MAJ	-3.29	-4.62	-1.84	-1.89	-2.47	-3.70
	(1.74)*	(1.61)***	(1.92)	(2.09)	(1.96)	(2.01)*
Estimation	Kernel	Kernel	Strat	Strat	Nearest	Nearest
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Logit	1	2	1	2	1	2
Specif.						
Obs. on	56 *PRES*	35 *PRES*	56 *PRES*	35 *PRES*	56 *PRES*	35 *PRES*
common	58 *MAJ*	50 *MAJ*	58 *MAJ*	50 *MAJ*	58 *MAJ*	50 *MAJ*
support						

Standard errors in parentheses obtained by bootstrapping
* significant at 10%; ** significant at 5%; *** significant at 1%
Kernel, Stratification and Nearest-neighbor estimators described in Chapter 5.5
Logit specifications underlying the propensity score estimates:
1: *LYP, PROP65, GASTIL, FEDERAL, COL_UKA, LAAM*
2: *LYP, PROP65, GASTIL, FEDERAL, ENGFRAC, EURFRAC, LAT01*

Table 6.7
Government surplus and constitutions
Simple regressions and Heckman estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.	SPL	SPL	SPL	SPL	SPL	SPL
PRES	1.31	1.00		1.05	0.18	1.01
	(1.24)	(1.26)		(1.34)	(1.69)	(.99)
MAJ	2.11	1.79		2.06	1.65	1.19
	(0.68)***	(0.71)**		(0.79)**	(0.75)**	(1.13)
PROPRES			2.36			
			(1.21)*			
MAJPAR			2.81			
			(0.91)***			
MAJPRES			2.51			
			(1.74)			
Endogenous selection					PRES	MAJ
Estimation	OLS	OLS	OLS	OLS	Heckman 2-step	Heckman 2-step
rho					0.31	0.28
Continents and Colonies	Yes	Yes	Yes	Yes	Yes	Yes
Sample	60-90s, broad	60-90s, broad	60-90s, broad	60-90s, narrow	60-90s, broad	60-90s, broad
Obs.	60	59	59	53	59	59
Adj. R2	0.17	0.28	0.30	0.31		

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include standard controls: *AGE, LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, OECD*, in columns 2-6 initial debt is also included.
Heckman first-step probit specifications (columns 5-6) include: *LAT01, ENGFRAC, EURFRAC, AGE, COL_UKA, LAAM*
Narrow sample corresponds to countries where *GASTIL* is less than 3.5

Table 6.8
Budget surplus and constitutions
Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*SPL*	*SPL*	*SPL*	*SPL*	*SPL*	*SPL*
PRES	-0.21		-0.26		0.20	
	(1.17)		(1.43)		(1.01)	
MAJ	1.16	0.78	1.80	1.12	2.11	1.91
	(0.56)**	(0.71)	(0.76)**	(1.28)	(0.82)**	(0.90)**
Estimation	Kernel	Kernel	Strat	Strat	Nearest	Nearest
Sample	60-90s, broad	60-90s, broad	60-90s, broad	60-90s, broad	60-90s, broad	60-90s, broad
Logit Specif.	1	2	1	2	1	2
Obs. on common support	37 *PRES* 55 *MAJ*	44 *MAJ*	37 *PRES* 55 *MAJ*	44 *MAJ*	37 *PRES* 55 *MAJ*	44 *MAJ*

Standard errors in parentheses obtained by bootstrapping
* significant at 10%; ** significant at 5%; *** significant at 1%
Kernel, Stratification and Nearest-neighbor estimators described in Chapter 5.5
Logit specifications underlying the propensity score estimates:
1: *LYP, PROP65, GASTIL, FEDERAL, COL_UKA, LAAM, CCG_NET_0*
2: *LYP, PROP65, GASTIL, FEDERAL, ENGFRAC, EURFRAC, LAT01, CCG_NET_0*

Chapter 7

Political rents and productivity: Variation across countries

7.1 Introduction

In this chapter, we study the effect of the constitution on the size of political rents, on policies promoting economic development and economic development itself. As explained in Chapter 3, political rents are measured by perceptions of corruption and the abuse of power by public officials, and by perceptions of (in)effectiveness in the provision of public services. Economic development is measured by output per worker or total factor productivity, and policies towards development by a broad policy indicator of the protection of property rights. Since these variables are only available in the second half of the nineties or, in the case of labor productivity and policies towards development, in the mid 1980s, we confine the analysis to comparisons in our nineties cross section.[1] Throughout, we employ the same battery of estimation methods as in the previous chapter's study of fiscal policy.

The theoretical work reviewed in Chapter 2 identifies several channels through which the constitution can influence the incidence of corruption and the abuse of power by public officials. Presidential regimes often have more separation of powers. Moreover, the executive is directly accountable to

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[1] Our analysis of perceptions of corruption and the electoral rule draws on Persson, Tabellini and Trebbi (2002). That paper also studies a short panel of data on perceptions of corruption for which yearly data are available from the mid 1980s until the late 1990s (the source is ICRG). The results of the panel-data study confirm the cross-sectional findings reported in this chapter.

153

voters and the dilution of responsibility often plaguing coalition governments is not an issue. On these grounds, the theory suggests lower rent extraction and less corruption under presidential regimes, as compared to parliamentary forms of government.

When it comes to the electoral rule, the predictions are more subtle. A few papers on rent extraction have focused on the simple distinction between strictly majoritarian and purely proportional electoral systems. Since the election outcome is generally more sensitive to the incumbent's performance in the former than in the latter, the prediction is that majoritarian elections are more effective in deterring political rents. But other theoretical studies have emphasized two details of electoral rules: the ballot structure and the number of legislators elected in each district (i.e. district magnitude). Electing politicians from party lists (rather than individually) weakens their incentives for good behavior, because it creates a free rider problem and a more indirect chain of delegation from voters to parties to politicians. Thus, ballots where voters directly choose individual incumbents are predicted to reduce the incidence of corruption, relative to those where citizens vote on party lists. Smaller electoral districts raise higher barriers to entry, which is predicted to increase corruption by reducing the choice set of voters. With small districts, a smaller number of parties (or ideological types) are represented in the legislature leaving voters with fewer alternatives to corrupt politicians or parties. What do these detailed predictions imply for the simple distinction between majoritarian and proportional elections? The answer is ambiguous, since the two effects tend to offset each other: proportional electoral systems typically combine large districts (which decreases corruption) and party-list ballots (which increases corruption), while the opposite is true under majoritarian elections (single-member districts with plurality rule).

In Section 2 of this chapter, we seek to discriminate between these different hypotheses. Presidential regimes are found to have less corruption, as expected, but this is a fragile result: it only appears among better democracies or if we relax conditional independence. Turning to the electoral rule, we find that what is of importance are the details, not the raw distinction between majoritarian and proportional systems. Large districts and voting over individuals both reduce corruption, as expected. But corruption is roughly the same when we compare across our broad classification into proportional vs. majoritarian systems. Evidently, the opposite effects of the ballot structure and district magnitude offset each other, with no robust net

effect.

The rest of the chapter tries to identify the effect of the constitution on two ultimate measures of good economic performance, namely labor and total factor productivity. As discussed in Chapter 3, previous studies have found productivity to be well explained by a broad policy indicator of protection of property rights and anti-diversion policies (the variable $GADP$). Here, we ask whether electoral rules or forms of government have an effect on productivity, directly or through this broad policy indicator.

A priori, the constitution could influence productivity and policies towards development in several ways. Naturally, one channel is corruption. Whenever corruption redistributes rents from producers to politicians, it hurts economic development. Hence, any constitutional feature with an impact on corruption is also likely to influence productivity. This suggests that presidentialism, large electoral districts and ballots for individuals should be associated with better economic policies and higher productivity. There are other possible offsetting channels, however. Small governments and low tax distortions in presidential and majoritarian political systems could induce high productivity, by high investment. But, as discussed in Chapters 2 and 6, the same constitutional features could also lead to targeted redistribution and low public-good provision. Such policies are likely to involve poor general protection of property rights, and distortions in the allocation of economic activity, with negative effects on productivity. Overall, the constitutional effects on productivity and policies towards development have an ambiguous sign. Hence, our empirical work in this chapter is really more of a preliminary search for interesting patterns in the data, than a test of specific hypotheses.

In that search, we also ask what is the link between productivity and the age of democracy (as measured by AGE). This variable could also influence performance in two opposite ways. On the one hand, older democratic institutions might perform better, as citizens will have learned to use them effectively in fighting government abuse and corruption. Indeed, a recent empirical "event study" by Roll and Talbott (2002) shows economic growth taking off, once a country becomes democratic. On the other hand, according to Olson (1982), older and more stable democracies are more easily captured by organized special interests, which might hurt their long-run economic performance.

Our empirical results on economic performance are reported in Section 3. Majoritarian elections at large do not have a robust causal effect on policy or productivity, but the finer details of the electoral system do: voting over indi-

viduals and large district magnitude promote productivity-enhancing policy in much the same way as they deter rent extraction. We also find parliamentary regimes and older democracies to select better policies towards economic development and for this reason, they have higher productivity. But our results also indicate that this negative effect of presidentialism on productivity is mainly due to the worse democracies in our sample. Combining these results with those on corruption, we tentatively conclude that presidentialism reduces rent extraction under better democratic conditions, while it hurts economic performance under worse democratic conditions.

Section 4 summarizes and contains some concluding remarks.

7.2 Political rents

As discussed in Chapter 3, we gauge political rents by three alternative measures concerning perceptions of rent extraction. Two of these refer to the perception of corruption by public officials ($GRAFT$ and $CPI9500$), the third to (in)effectiveness in the provision of government services ($GOVEF$). As we have most observations for $GRAFT$, and this variable is probably most closely related to the theoretical construct of political rents, we mainly focus on this indicator. But we show that the results also extend to the other measures. Our goal in this section is to describe how alternative constitutional features influence these perceptions of rent extraction.

The form of government is measured by our binary indicator for presidentialism, $PRES$. The simple distinction between majoritarian and proportional or mixed electoral systems is measured by the binary variable MAJ. In this chapter, we also measure more detailed aspects of the electoral rule, however. The continuous variable $MAGN$ is a measure of *inverse* average district magnitude (see Chapter 4). It captures the barriers to entry in the electoral race, as it assigns higher values to fewer candidates elected per district, and we expect it to induce more corruption. The continuous variable $PIND$ is a measure of the percentage of legislators *not* elected from party lists (again, Chapter 4 gives an exact definition). Thus, it is predicted to induce less corruption..

We follow the same empirical strategy as in the previous chapter: first we estimate some simple regressions, then we relax conditional independence and finally we relax linearity. The more general estimation methods only apply to the effects of the binary constitutional variables (MAJ and $PRES$),

however. Hence the analysis of how the details of the electoral rule influence rent extraction is confined to the simple linear regressions reported in the next subsection.

7.2.1   OLS estimates

Table 7.1 reports on the results of simple regressions, estimated under the assumptions of linearity and conditional independence. To help reduce the noise from measurement error, the estimation method is always weighted least squares, where the weights are given by the (inverse) standard deviation of the dependent variable (see Chapter 3 for more details). Estimating by OLS and correcting the standard errors for heteroscedasticty, produce very similar results. Throughout, we hold constant a dozen variables that other studies have found to influence the perception of corruption, such as per capita income, religious beliefs, education, and so on (see Chapter 3 for discussion and references, and the notes to *Table 7.1* for a complete list of the controls). We also hold constant continental location and colonial origin. Controlling for legal origin instead of colonial origin leads to similar or stronger estimated constitutional effects, meaning that we report the least favorable specification for the theory.

Consider first the effect of the form of government. In column 1, rent extraction is measured by $GRAFT$, while the constitutional features are measured by the binary variable for presidentialism ($PRES$) and the continuous measures for the electoral rule ($PIND$ and $MAGN$). Presidentialism has a negative estimated effect on rent extraction, as expected. But its estimated coefficient is only significant at the 10% level, and small in absolute value (recall that all our measures of corruption vary from 0 to 10). Neglecting columns 2 and 3 for a moment, the estimated coefficient of presidentialism is insignificant in all the other columns of *Table 7.1*, where rent extraction is measured by the other two perception variables, $CPI9500$ and $GOVEF$, or the electoral rule is measured by other indicators such as *PROPN, PDM* or *MAJ.*

A possible interpretation of this inconclusive finding on the form of government is that our measure of presidentialism does not square well with the theory. As discussed in Chapter 4, our distinction between different forms of government relies on the confidence requirement, and not on the separation of powers. Yet, according to the theories reviewed in Chapter 2, presidential governments mainly reduce political rents because of their greater separation

of powers than parliamentary governments.

But columns 2 and 3 suggest another possibility: an interaction between the form of government and the quality of democracy. Presidential regimes are often found in worse and younger democracies, where the formal constitutional rules might be less important and the stronger checks and balances associated with presidentialism might not exert their full effect. Indeed, as shown in column 2, presidentialism has a negative and significant effect on corruption, once we restrict the sample to better democracies. In column 3, we interact the $PRES$ indicator with the quality of democracy (as measured by $GASTIL$) in the full sample. The estimated coefficient of presidentialism increases further and acquires a stronger statistical significance, but its effect is dampened in worse democracies (recall that higher values of $GASTIL$ correspond to worse democracies).

A third possibility is a combination of the other two. It may be that separation of powers is lacking precisely in the worst democracies. Indeed, as discussed in Chapter 4, the $GASTIL$ measure partly reflects whether there are effective checks on the behavior of the executive.

Table 7.1 about here

Next, consider the effect of electoral rules. Here, the data strongly support the idea of the details of the electoral rule being important determinants of rent extraction. As shown in the first two columns of *Table 7.1*, inverse district magnitude and ballots with individuals are statistically significant with the expected sign: more individual voting (higher values of $PIND$) reduces corruption, while higher barriers to entry associated with smaller districts (higher values of $MAGN$) increase corruption. This result is robust to the sample of better democracies (i.e. those with a $GASTIL$ score smaller than 3.5, in column 2). Moreover, the estimated coefficients of $PIND$ and $MAGN$ are large (both variables are defined so that they lie between 0 and 1) and their standardized beta coefficients are, by far, the largest of all the regressors. For example, switching from a system where all legislators are elected on party lists ($PIND = 0$), to one where all are elected as individuals ($PIND = 1$), is estimated to reduce the perceptions of corruption by well over 20% (2 points out of 10) in the sample of good democracies, which is about twice the effect of *not* being a Latin American country. The estimated effect of inverse district magnitude (also taking positive values below 1) is even larger, though it is somewhat less stable to the specification. Omitting

the dummy variables for continental location and colonial origin does not importantly affect the coefficient of $PIND$, though the coefficient of $MAGN$ becomes somewhat smaller and remains statistically significant only at the 10% level. Finally, these variables are not only individually, but also jointly, significant. Given the high correlation between these two variables and their opposite effect on corruption, this is a further sign that we are not just picking up a statistical artifact.

Being a survey of surveys, the dependent variable is clearly measured with error. This is the rationale for our WLS estimation, attaching lower weights to observations where the different components of the perception index are more divergent. In columns 4-6 of *Table 7.1,* we carry out additional sensitivity analyses, with alternative measures for our dependent and independent variables. Columns 4 and 5 report on the same specification as in column 1, but with either *GOVEF* or *CPI9500* as the dependent variable. The results are even stronger when we measure corruption by $CPI9500$ and almost as strong when we instead consider *GOVEF,* measuring ineffectiveness in government (recall from the previous section that we have re-scaled all these measures to run on a scale from 0 to 10). Column 6 replaces our own two measures of the electoral system by the alternatives from the data set constructed by Seddon et al (2001) and defined in Chapter 4. Recall that *PDM* is their measure of district size, defined so that higher values mean larger districts, not smaller as with our variable *MAGN.* Similarly*, PPROPN,* their measure of legislators elected at the national level, is an inverted measure of individual accountability, and not a direct measure as our *PIND* variable. Thus, the expected sign of these two variables is the opposite relative to *PIND* and *MAGN.* As shown in column 6, the main results hold up equally well with these alternative measures.

Overall, these simple regressions strongly suggest that the details of the electoral rules influence corruption, as expected. Countries predominantly voting over individuals tend to have less corruption than those predominantly voting over parties. Countries with smaller electoral districts also tend to have more corruption. According to these results, a comprehensive electoral reform, going from a Dutch-style electoral system with party lists in a single national constituency to a UK-style system with first past the post in one-member districts (i.e., moving both *MAGN* and *PIND* from approximately 0 to 1), would have two counteracting effects on corruption, producing a net result close to zero. A better reform from the viewpoint of reducing rent extraction would be to switch to plurality rule voting for individuals, but

keeping districts with more than one member as in Chile (two-member districts and $MAGN = 0.5$) or Mauritius (three-member districts and $MAGN = 0.33$). Indeed, these countries, especially Chile, turn out to have very low corruption levels as compared to neighboring countries.

Another way of asking what are the effects of a comprehensive electoral reform from proportional to majoritarian elections is to infer the constitutional effect of the estimated coefficient of our binary indicator for the electoral rule, MAJ. This question is of independent interest, since according to some models reviewed in Chapter 2, majoritarian electoral systems enhance electoral accountability and thus, deter corruption. The result is shown in column 7 of *Table 7.1*. The estimated MAJ coefficient is negative, but small and statistically insignificant. The estimated MAJ coefficient increases somewhat in absolute value, and becomes marginally significant if we do not control for colonial origin (results not reported). As noted in Chapter 4, majoritarian electoral rules are often found in former British colonies, and it is difficult to tell the influence of these two variables apart (when we control for continents or legal origin, the constitutional effect remains negative and statistically significant, so it is really colonial origin that makes a difference). But to interpret the estimate of a regression that does not control for British colonial origin as a causal constitutional effect, we would need to assume colonial origin not to have any effect on perceptions of corruption, which is not very plausible.

7.2.2 IV and Heckman estimates

We only attempt to relax conditional independence for the binary constitutional indicators (MAJ and $PRES$). In principle, the continuous measures of the electoral rules (such as $PIND$ and $MAGN$) might also be correlated with the random component of rent extraction, which would bias the OLS estimates. But the Heckman procedure cannot be applied to continuous variables. Instrumental-variable estimates are also problematic, because our instruments for constitutional origin are unlikely to be appropriate. The finer measures of the electoral system change more frequently than the simpler classification into majoritarian and presidential constitutions, so it would be more difficult to date them back to specific historical periods. Thus, in this section, we only apply instrumental variable estimation and the Heckman procedure only to the binary variables MAJ and $PRES$.

With our standard specification (colonies and continents included in the

second stage), the two-step Heckman procedures yield estimates of the correlation coefficient (rho) of $+1$ or -1, thereby suggesting a perfect correlation between the error terms between constitution selection and performance. As this is implausible, we instead preform the Heckman correction with a maximum-likelihood estimator. To achieve convergence of the maximization algorithm, however, we must impose more parsimonious first-step and second-step specifications for both constitutional variables, as compared to the specification adopted in Chapter 6. Specifically, when estimating the first-step (probit) regressions, we drop the indicator variables for constitutional origin ($CON2150$, $CON5180$ and $CON81$); the remainder of the specification is as in Chapter 6 (see also the discussion in Chapter 5). In the second step, we only include the dummy variables for UK colonial origin and Latin America, besides all the standard controls, thus omitting the other continental and colonial origin indicators.

Instrumental-variable estimation is also performed in a slightly different way, compared to Chapter 6. Our instruments are still the same as those discussed in Chapter 5 and used in Chapter 6: the three indicators for constitutional origin ($CON2150$, $CON5180$ and $CON81$), latitude ($LAT01$) and the fractions of the population whose mother tongue is English or a European language ($ENGFRAC$ and $EURFRAC$). But here, we move in the opposite direction and adopt a less parsimonious first stage specification than in Chapter 6: we now run the first-stage regression of the 2SLS estimates on the full set of the six instruments plus all controls entering the second-stage regression (see *Table 7.2* for a complete list). Several of the second-stage controls now measure historical and social variables, such as religious beliefs or ethnic fractionalization, which could also influence constitution selection. Excluding such controls from the first stage, if they belong there, might bias the 2SLS estimates of the constitutional effect. Furthermore, adding the full set of controls to the first stage now *increases* the explanatory power of the dummy variables dating constitutional origin, and thus reduces our concern for weak instruments.[2] For both these reasons, the inference is more reliable with a less parsimonious first-stage specification.

Table 7.2 about here

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[2]The F tests of the nul hypothesis that all instruments dating constitutional origin have a zero coefficient in the first-stage regressions for presidentialism and majoritarian elections yield the test statistics $F = 3.06$ ** and $F = 2.26$* , respectively.

Consider the constitutional effect for presidentialism. The Heckman procedure (column 1) produces a positive and highly significant estimate ($rho = 0.57$) of the correlation between selection of a presidential regime and corruption. Correcting the upward bias in the OLS estimates, the constitutional effect is a reduction in corruption/rent extraction by about 1 point (out of 10), a statistically significant and non-trivial effect. The 2SLS estimates (column 3) yield the same result, namely a large and statistically significant effect of presidentialism on corruption. The fact that both estimators produce similar results, despite the different identification assumptions, suggests that a violation of conditional independence could indeed bias the OLS estimates towards zero. As shown by columns 4 and 5, there is a negative effect when we replace $GRAFT$ by the two alternative measures of rent extraction.

For majoritarian elections, we reach the opposite conclusion. The estimated correlation coefficient in column 2 is negative ($rho = -0.47$), though imprecisely estimated, and the constitutional effect is now positive, although insignificant (i.e., a sign reversal relative to the OLS estimates). Once more, the instrumental variable estimation in columns 3-5 reinforces this conclusion.

Thus, addressing conditional independence does make a difference in the case of corruption. The OLS estimates suggested no effect (or a negative but small and fragile effect) of government regimes and majoritarian elections on corruption. The conclusion regarding the form of government is reversed when allowing for conditional independence. Presidential regimes are found to reduce corruption, while for majoritarian elections, the inference of no constitutional effect is reinforced.

7.2.3   Matching estimates

We end with the non-parametric estimates, re-imposing the conditional independence assumption. Because the matching methodology requires a binary variable, we only report results for the two simple constitutional indicators (MAJ and $PRES$). Columns 1 to 3 of *Table 6.14* show the constitutional effects on rent extraction measured by $GRAFT$, according to our three matching estimators. The specification of the propensity score includes a basic set of six covariates (LYP, $GASTIL$, $AVELF$, $PROT80$, COL_UKA and $LAAM$). In columns 4 and 5, $GRAFT$ is replaced by the two alternative measures ($CPI9500$ and $GOVEF$), for the same logit specification and the Kernel estimator. Column 6, finally, maintains $GRAFT$ as the dependent

variable, but relies on a different propensity-score specification.

Table 7.3 about here

The results can be stated briefly. The presidential effect now becomes positive, though always statistically insignificant and small. The effect of majoritarian elections is always negative, but never significant. Neither estimate is very stable, and in other (non reported) specifications, the sign of both effects changes, though always remaining small and insignificant. Overall, these estimates suggest that neither constitutional feature has a robust effect on corruption. Our conjecture that the linear OLS estimates of the presidential effect were hiding a potentially relevant interaction between the form of government and the quality of democracy does not seem supported by this more general estimation method which allows for non-linear functional forms. Note, however, that we do impose conditional independence.

7.2.4 Summary

The overall picture emerging from this section is multi-dimensional. Presidential regimes do not have a stable effect on political rents under the maintained assumption of conditional independence (required by OLS and matching), except in better democracies. Relaxing conditional independence seems empirically important and produces a negative constitutional effect, however.

What about the electoral rule? Here, the central empirical result is that the devil is in the details. Larger electoral districts seem to cut rent extraction, as do elections where voters cast their ballots for individual politicians rather than party lists. From the perspective of a radical reform from proportional to majoritarian elections, these two aspects of the electoral system tend to offset each other, with no net effect on corruption; a result confirmed by the estimates associated with our binary indicator for plurality rule.

7.3 Productivity

In this section, we search for constitutional effects on two ultimate measures of economic performance, namely labor productivity, i.e., output per worker ($LOGYL$), and total factor productivity ($LOGA$). The main difference between these two is that labor productivity largely reflects underlying capital

intensity and thus previous capital accumulation, whereas total factor productivity does not. We first estimate a *direct* constitutional effect on these two variables, by a reduced form similar to that in the existing literature on cross-country productivity differences. It is important to probe further beyond any reduced-form findings, however. Specifically, do our constitutional variables explain a broad policy indicator of protection of property rights and anti-diversion policies ($GADP$) that previous studies have found to be an important determinant of productivity? (See Chapter 3 and Hall and Jones (1999) for a precise definition and discussion.) Do such *indirect* constitutional effects on productivity operate through the policies studied earlier in this chapter and the previous one, namely the size and composition of government spending or corruption? Are higher productivity and better economic policies related to the age of democracy. Are the estimates robust to endogenous selection of the constitution, and non-linearities in the outcome relation?

7.3.1   Reduced-form estimates

We begin by estimating a simple reduced form by OLS, with the two productivity measures as our dependent variables. The underlying specification is the same as in Chapter 3 which, in turn, follows Hall and Jones (1999) closely. Thus, we control for latitude ($LAT01$), the fractions of the population speaking English or a European language ($ENGFRAC$ and $EURFRAC$), a measure of comparative advantage in international trade ($FRANKROM$) and our indicator for federalism ($FEDERAL$). But now, we also add our usual constitutional variables plus the age of democracy (AGE) to the regressors. We always hold constant continental location and colonial origin to lend more credibility to the conditional-independence assumption (omitting these indicator variables, we obtain stronger estimated constitutional effects, with the same signs as those described below).

Columns 1 and 2 of *Table 7.4* show that both presidential regimes and majoritarian elections have a negative coefficient: according to these reduced-form estimates, both constitutional features harm economic performance. The effect on total factor productivity is smaller and not statistically significant, suggesting that the negative effects might operate through disincentives for capital deepening (i.e., investments in physical or human capital). Total factor productivity is also harder to explain – the regression in column 2 explains about 50% of the variation in productivity, as opposed to about 70%

in column 1 – probably because of larger measurement error. To gauge the size of the constitutional effect, recall that labor productivity is expressed in logs and ranges from a maximum of about 10.5 for the US to a minimum of about 7 for Malawi. According to the estimates in column 1, switching from parliamentarism to presidentialism or from proportional to majoritarian elections reduces labor productivity by about 0.3: a non-trivial effect close to the difference between the US and the UK, or between Spain and Greece, in the mid 1980s.

Columns 3 and 4 decompose the effect of the electoral rule in the same two dimensions as in the previous section, namely the fractions of legislators elected with an individual vote ($PIND$) and (the inverse of) district magnitude ($MAGN$). Our previous results for rent extraction lead us to expect positive and negative effects on productivity, respectively, from these variables. These signs are indeed what we find. Moreover, the estimated coefficients are statistically significant and quite large. In these regressions, however, the effect of the form of government seems to vanish.

Table 7.4 about here

The constitutional effect on labor productivity of presidential regimes and majoritarian elections is sensitive to the sample of countries: a significant estimate is obtained in the broader sample, but not among the better democracies (column 5). This fragility does not extend to the finer measures of the electoral system ($PIND$ and $MAGN$), however, which remain statistically significant when we restrict the sample to the better democracies (column 6). In the case of total-factor productivity, none of the constitutional variables is statistically significant in the narrow sample, and the fit of the regression is generally rather poor.

As shown in all columns of the table, the age of democracy (AGE) is strongly correlated with economic performance. Older democracies are more productive, and the effect is statistically significant for all measures of productivity, all specifications, and almost all samples.

These reduced-form estimates indicate some intriguing constitutional effects on productivity, over and beyond the historical, geographical and cultural variables held constant in these regressions. Presidential and majoritarian countries seem to have lower productivity, particularly in worse democracies, and the specific form of the electoral system also seems to be of importance. To gain more insights into the channels of these constitutional effects,

we need to estimate a more structural model, which maps our constitutional measures into observable policies, and these policies into productivity. The next subsection attempts to make some progress on this non-trivial task.

7.3.2 Structural-form estimates

Do the constitutional effects on productivity operate exclusively through the comprehensive policy indicator of anti-diversion policies ($GADP$)? This question consists of two parts: (i) is there a constitutional effect on this broad policy indicator? (ii) are there direct constitutional effects not going through this indicator, thus reflecting some other policy channels?[3]

Column 1 of *Table 7.5* addresses sub-question (i) by OLS estimation (retaining the assumption of conditional independence) and a specification following the reduced form of *Table 7.4*. Thus, we control for the age of democracy, our indicators for federalism, colonial origin and continental location, plus the four Hall-Jones variables mentioned above. Recall that higher values of the policy indicator ($GADP$) amount to better policies, and that the values range from about 0.3 (for Bangladesh) to 1 (for Switzerland). As expected from the reduced-form estimates, parliamentary regimes have better policies with quite a substantial effect. The age of democracy is also statistically significant, with older democracies having much better policies. But now, the broad form of the electoral rule does not seem to be of importance.

Table 7.5 about here

Next, we ask whether the anti-diversion policy indicator has an effect on productivity. The estimation is by 2SLS, where policy is endogenized in the first stage with the specification underlying column 1. The second-stage productivity equations reported in columns 2 and 3 still include the colonial-origin and continental variables, but nothing else. Thus, the instruments for $GADP$ are the same as those used by Hall and Jones (1999), plus our four constitutional variables: the dummy variables for presidentialism, majoritarian elections, federalism, and the age of democracy. The identifying

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[3]As discussed in Chapter 3, Hall and Jones (1999) argue that two policy variables could account for cross-country differences in productivity. One is the indicator of anti-diversion policies ($GADP$), the other an indicator of commercial policy ($YRSOPEN$). The effect of commercial policy on productivity is not robust, however, and disappears when we include dummy variables for continents and colonial origin. We omit it from the analysis of this chapter, since it is almost never statistically significant.

assumption is that these instruments only affect productivity through the policy indicator $GADP$. According to the results in columns 2 and 3, policy has a positive and significant effect on both productivity measures. The coefficients are roughly the same as those obtained in Hall and Jones (1999) and Chapter 3, with a larger impact on labor productivity than on total factor productivity. Together with the first-stage estimates, this suggests that parliamentary regimes and older democracies have higher productivity, because they promote better policies, as summarized by the $GADP$ indicator.

As can be seen from columns 2 and 3, however, the over-identification assumptions of the instruments are almost rejected at the 10% level of significance. Some of the variables entering in the first-stage regressions of column 1 may thus have a direct effect on productivity, not captured by our policy indicator. It turns out that three instruments are responsible for this behavior. The main culprit is the dummy variable for majoritarian elections (MAJ), but the fractions of the population whose mother tongue is English or a European language ($ENGFRAC$ and $EURFRAC$) also play some role. Adding these three variables to the second-stage regressions reported in columns 4 and 5, the test statistic for the over-identifying restrictions stays very comfortably within the acceptance range. Moreover, the direct effect of majoritarian elections on productivity is negative and, in the case of labor productivity, significantly different from zero. Majoritarian elections thus seem to hurt productivity through some other policy channel, not captured by the indicator of anti-diversion policies.

To shed some further light on the role played by the electoral system, we re-specify the first-stage equation for the policy indicator, $GADP$, replacing the binary indicator for majoritarian elections with our two continuous measures. Both influence policy choice as expected: more individually elected legislators lead to better policies ($PIND$ has a positive coefficient in column 6), as do larger districts ($MAGN$ has a negative coefficient). Thus, the electoral system seems to influence policy choices, but only through its finer details – a result entirely in line with the rent-extraction results in the previous section. Nonetheless, the second-stage regressions reported in column 7 reveal that even with this alternative first-stage specification, there is still a direct effect of majoritarian elections on productivity.

In light of these last results, it is natural to ask whether the policies discussed in the previous section and the previous chapter – rent extraction, or the size and composition of government spending – affect economic performance, and whether this could explain the direct impact of majoritarian

elections on productivity. The short answer is: probably not. None of the other policy measures is significant in the second-stage regression for labor productivity, as long as the comprehensive policy indicator $GADP$ is included, while the coefficient on $GADP$ is basically immune to the inclusion of these other policies.

But the long answer may be worth spelling out. The other policy measures do have an impact on productivity, if we omit anti-diversion policies ($GADP$) from the productivity regression.[4] First, as might be expected, corruption (measured by $GRAFT$) has a negative effect on labor productivity. But there is still a direct effect of majoritarian elections, which is even stronger and more precisely estimated than that in *Table 7.5*. Moreover, the measures of corruption and anti-diversion policies are highly correlated (the correlation coefficient of -0.87), and thus probably measure similar aspects of policy making. Second, the size of government ($CGEXP$) and welfare spending (SSW) also appear as determinants of productivity, with a positive and significant estimated coefficient. This effect is particularly robust for welfare spending. It is difficult to see why welfare spending should improve productivity. The fragility of the result to the inclusion of the policy indicator $GADP$ also suggests that we should play it down. But when both anti-diversion policies and welfare spending are included as determinants of productivity, the direct effect of majoritarian elections on productivity disappears, and the over-identifying restrictions can no longer be rejected. This suggests a possible interpretation. According to the theories in Chapter 2, majoritarian regimes have less welfare spending but also less public-good provision, and the latter hurts productivity. If welfare spending and public goods provision are indeed positively correlated across countries, evidence of a direct constitutional effect of majoritarian elections on productivity should be dampened when we also control for welfare spending.

Before claiming too much, however, we should follow the approach of the earlier sections and ask whether the results are robust to relaxing conditional independence.

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[4]Naturally, these variables are treated as endogenous, like the indicator $GADP$. In the case of the fiscal policy variables, the first-stage regressors include the same specification for $GADP$, plus the proportion of elderly in the population ($PROP65$).

7.3.3   Endogenous selection

Relaxing conditional independence is somewhat more difficult here than in earlier sections, as we have added another level of relations: endogenously selected institutions influence policies which, in turn, influence productivity. We therefore break our estimation problem into three parts.

First, we want to estimate the constitutional effect on the policy indicator $GADP$, allowing for endogenous constitution selection. In the previous section we used six instruments for the constitution: the three dummy variables dating constitutional origin, plus three of Hall and Jones's (1999) variables – latitude and the fractions of the population with English or a European language as their mother tongue. As discussed in Chapters 4 and 5, the last three instruments have more power in explaining constitution selection. But here, these variables have a direct impact on the policy indicator $GADP$, and perhaps also on productivity, so we cannot credibly assume that they only affect outcomes through their effect on constitution selection. In fact, this restriction is strongly rejected by the data. We are thus left with the three weaker instruments dating constitutional origin. But the data also reject the over-identifying assumption that these three dummy variables affect constitution selection, but not policy.[5] Whatever the reason for this rejection, we lack reliable instruments correlated with constitution selection, but not with policy.

We can still relax conditional independence by the Heckman procedure, however, basing our identification entirely on the functional form assumption. The results are shown in column 1 of *Table 7.6*. The first-stage probit for constitution selection is the same as in the previous section. The second-stage regression for anti-diversion policies controls for the age of democracy, federalism, the four Hall-Jones variables, continental origin and British colonial origin (other colonial-origin variables are omitted to facilitate convergence of the maximum-likelihood estimation). Since majoritarian elections seem to have no influence on anti-diversion policies according to the OLS estimates, we omit its indicator (MAJ).[6] The previous results (column 1 of *Table 7.5*) continue to hold. Conditional independence cannot be rejected (the esti-

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[5]The first-stage for constitution selection is specified exactly as in earlier sections, while the test of the over-identifying restrictions is performed on the residuals of the $GADP$ equation.

[6]The results are identical if the dummy variable MAJ is also included in the $GADP$ equation, but treated as exogenous, and the estimated MAJ coefficient does not differ significantly from zero. Moreover, estimating the effect of majoritarian elections on $GADP$

mated value of rho is almost zero), presidentialism still has a negative and significant effect on anti-diversion policies, and older democracies have better policies.

Table 7.6 about here

Second, we want to estimate the effect of policy on productivity, thereby allowing both constitution selection and policy choices to be endogenous. Columns 2 and 3 of *Table 7.6* perform this estimation for labor and total factor productivity. Predicted antidiversion policies ($GADP$) are generated from the two-step Heckman procedure described above. They are used as a regressor in the productivity equations, where the standard errors are corrected, taking into account that $GADP$ is a generated regressor. The results are very similar to the 2SLS estimates reported in *Table 7.5*, columns 2 and 3, confirming that the prior estimates are robust to endogenously selected forms of government. Combining the results in columns 1-3 of *Table 7.6*, we can safely conclude that parliamentary regimes are good for productivity, because they promote better anti-diversion policies.

Third, we would also like to know if the direct negative effect of majoritarian elections on productivity is robust to relaxing conditional independence. This is a very difficult question: to address it in full, we would need to allow for a joint endogenous selection of government regimes and electoral rules. Instead, we take the same kind of short-cut as in the previous section, allowing the selection of only one constitutional dimension at a time. More precisely, we first estimate the equation for antidiversion policies with Heckman's two-step procedure as in column 1 of *Table 7.6*, allowing for endogenous selection of the form of government but imposing the restriction that the electoral rule does not enter this equation. Then, we once more apply the two-step procedure, estimating our productivity equation with the predicted value of $GADP$ as a regressor, and allowing for endogenous selection of the electoral rule. In the latter estimation, we neglect the fact that $GADP$ is a generated regressor and do not correct the estimated standard errors.[7]

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with the Heckman procedure (and treating presidentialism as exogenous) still leads to an estimated MAJ coefficient not significantly different from zero.

[7]We can still test the nul hypothesis that $GADP$ does not enter the productivity equation without correcting the standard errors, because under the nul, the standard errors are correctly estimated. But this does not apply to the other estimated coefficients and, in particular, to those of MAJ.

For a comparison with the 2SLS estimates in columns 4 and 5 of *Table 7.5* , we also add the fractions of the population speaking English or a European language to the productivity equation (recall that testing the over-identifying restrictions suggested these variables to have a direct impact on productivity). The results appear in columns 4 and 5 of *Table 7.6*. Now, the direct constitutional effect of majoritarian elections on productivity vanishes: the estimated coefficient is positive (rather than negative as in *Table 7.5*), but not significantly different from zero. This is consistent with the estimated correlation coefficient (rho), which is negative and large, suggesting that the earlier estimates were indeed downward biased.

Thus, once we allow for endogenous constitution selection, we are led to the conclusion that the indirect negative effect of presidentialism on productivity, through worse antidiversion policies, is robust. But the direct negative effect of majoritarian elections on productivity is not. A broad reform from proportional to majoritarian electoral rules would not have a robust effect on productivity, either directly or indirectly through better anti-diversion policies.

7.3.4   Matching estimates

Finally, we report two sets of non-parametric estimates: the constitutional effect on anti-diversion policy ($GADP$) and the reduced-form constitutional effect on productivity ($LOGYL$ and $LOGA$). Our different findings for samples of good and bad democracies (cf. *Table 7.4*) suggest that non-linearities may be important. The matching is based on propensity scores estimated by a logit specification including the four Hall and Jones variables ($LAT01$, $EURFRAC$, $ENGFRAC$, $FRANKROM$). Given the strong effect of the age of democracy on productivity and antidiversion policies reported in the previous subsection, this variable (AGE) is always included in the logit regressions as well.

Table 7.7 about here

Table 7.7 shows the main results. Columns 1-3 confirm that presidentialism leads to significantly worse anti-diversion policies. In fact, the estimated effect is even larger than the OLS estimate reported in *Table 7.5* (column 1). As in the linear estimates, the electoral rule has no effect on anti-diversion policies.

Columns 4-6 report the estimated effects on output per worker, which correspond to the reduced-form OLS estimates in *Table 7.4*. Both presidential regimes and majoritarian elections are associated with lower labor productivity, for all our three matching methods. The point estimates are slightly higher than before and, as usual, the standard errors are larger. The more reliable Kernel estimator produces statistically significant effects for both the form of government and the electoral rule. The estimates for total factor productivity (not shown) are similar, although less pronounced.

Overall, maintaining the assumption of conditional independence but relaxing linearity reinforce the earlier conclusions from the linear regressions. Proportional and parliamentary regimes have higher productivity. The effect of the form of government operates through antidiversion policies, while the effect of the electoral rule is direct (i.e., operates through some other policy channels).

7.3.5   Summary

Sorting out the causal relations between institutions, policies and productivity is not straightforward. Nevertheless, the results in this section can be summarized as follows.

The form of government and the age of democracy have strong constitutional effects. In particular, parliamentary regimes and older democracies pursue better anti-diversion policies (as measured by $GADP$) promoting productivity. These results are robust to endogenous selection of government regimes and possible non-linearities, although the negative effect of presidentialism seems confined to worse democracies.

Once more, the details of the electoral rule are of great importance, in a way consistent with the earlier results on rent extraction: larger electoral districts and more direct voting over individuals promote better policies. A radical reform from proportional to majoritarian elections has no effect on anti-diversion policies; it does have a negative direct effect on productivity, but this effect is not robust. Specifically, under the assumption of conditional independence, majoritarian elections are associated with lower productivity, but the effect disappears when this assumption is relaxed.

7.4 Concluding remarks

Some of the primary goals of any democratic constitution are to limit the abuse of power by political leaders, to protect private property rights, and thus to promote economic development. In this chapter, we have seen that some constitutional features are more effective in achieving these goals than others.

One robust lesson is that the fine details of the electoral rule are more important than the crude distinction between majoritarian and proportional elections. Direct individual accountability reduces corruption and is associated with policies more respectful of property rights. But small electoral districts are associated with more corruption and worse policies towards economic development, in line with the idea that barriers to entry are higher in single-member districts. Since these two dimensions of the electoral rule tend to co-vary, the net effect of our binary election indicator of corruption and growth promoting policies is ambiguous.

A second lesson is that the effects of the form of government interact in subtle ways with the overall quality of democratic institutions. Under good and well-established democratic traditions, corruption is lower under presidentialism than under a parliamentary government, which is what we expected, given existing theories. But in worse democracies, the positive effect of presidentialism seems to be lost. On the contrary, in these worse democratic environments, presidentialism is associated with less protection of property rights and overall, worse policies towards economic development. As a result, presidentialism exerts a negative effect on productivity. These results may partly be due to our definition of presidential government, which is based on the confidence requirement neglecting the separation of power dimension, and partly due to our quality of democracy measure picking up constraints on executive power. But it may not be too surprising that institutions vesting a great deal of power in the executive branch of government fare well only or mainly in countries with strong democratic traditions.

A robust empirical result is that older democracies are more productive with economic policies more favorable to growth. Olson's (1982) conjecture that older democracies are more easily captured by organized special interests and hence, perform worse, is not supported by our data.

In many ways, the empirical findings in this chapter are more preliminary than those in the previous chapter on fiscal policy. One problem is that the measures of performance we have tried to explain (perceptions of corruption,

perceptions of anti-diversion policies) are measured with larger error and more loosely related to theory than in the case of fiscal policy.

A second problem, particularly in our analysis of productivity, is that theory offers little guidance on the variables to hold constant and the primary mechanisms through which the constitution affects economic development. There are many possible channels of influence, some of which are likely to produce offsetting effects. For instance, in the previous chapter, we saw that presidentialism leads to a smaller government and less taxation, which is probably good for economic performance, but also to less universalistic programs and less public-good provision, which might have the opposite effect. Given these possibilities and the lack of a well-specified theory, drawing inferences from the data is much more difficult.

Finally, we have neglected a third important issue throughout this chapter: the reverse link from economic development to the quality of democratic institutions.[8] This link could partly account for our finding that older democracies have better economic policies. Sorting out these difficult issues, with better measurement and more precisely formulated theoretical hypothesis, is a difficult but challenging task for future research.

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[8]See, however, the recent paper by Kaufmann and Kraay (2002) which finds the feedback effect from development to corruption to be weak or even positive.

Table 7.1
Political rents and constitutions
Simple regression estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.	*GRAFT*	*GRAFT*	*GRAFT*	*CPI9500*	*GOVEF*	*GRAFT*	*GRAFT*
PRES	-0.52	-0.79	-1.41	-0.27	-0.30	-0.04	-0.28
	(0.30)*	(0.38)**	(0.68)**	(0.43)	(0.35)	(0.30)	(0.32)
PRES_BAD			0.35				
			(0.24)				
MAJ							-0.14
							(0.31)
PIND	-2.12	-2.88	-2.10	-2.88	-2.01		
	(0.76)***	(0.85)***	(0.75)***	(1.02)***	(0.87)**		
MAGN	2.72	3.53	2.61	3.39	2.14		
	(0.87)***	(0.95)***	(0.86)***	(1.14)***	(1.01)**		
PPROPN						1.25	
						(0.47)**	
PDM						-0.01	
						(0.00)**	
Continents and Colonies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	90s, broad	90s, narrow	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Obs.	78	59	78	68	78	72	78
Adj. R2	0.84	0.87	0.84	0.88	0.75	0.87	0.81

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Estimation: by weighted least squares. Weights for dep. var. are given by 1/std(dep. var.)
All regressions include the following controls: *GASTIL, AGE, LYP, LPOP, EDUGER, TRADE, OECD, FEDERAL, AVELF, PROT80, CATHO80, CONFU*
Narrow sample consists of countries where *GASTIL* is less than 3.5

Table 7.2
Political rents and constitutions
Instrumental variable and Heckman estimates

	(1)	(2)	(3)	(4)	(5)
Dep. var.	GRAFT	GRAFT	GRAFT	GOVEF	CPI9500
PRES	-1.28	-0.50	-1.89	-1.47	-2.16
	(0.44)***	(0.28)*	(0.83)**	(0.83)*	(1.32)
MAJ	-0.18	0.30	0.31	-0.26	0.46
	(0.26)	(0.66)	(0.61)	(0.64)	(0.98)
Endogenous selection	PRES	MAJ	PRES MAJ	PRES MAJ	PRES MAJ
Estimation	Heckman ML	Heckman ML	2SLS	2SLS	2SLS
rho	0.57***	-0.49			
	(0.17)	(0.58)			
Chi2: over-id			3.08	2.97	2.49
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Obs.	73	73	73	73	63
Adj. R2			0.75	0.68	0.75

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Always included in performance equations: GASTIL, AGE, LYP, LPOP, EDUGER,
TRADE, OECD, FEDERAL, AVELF, PROT80, CATHO80, CONFU, LAAM, COL_UKA
2SLS first-stage specification includes: CON2150, CON5180, CON81, LAT01, ENGFRAC, EURFRAC, plus all controls in performance equations
Chi2: over-id refers to the test statistic for the over-identifying restriction that the instruments in the first-stage regressions underlying columns 1 and 2 do not enter the performance equations. Critical value of chi-2(4,0.05) is 9.49
Heckman probit specification includes: LAAM, COL_UKA, LAT01, ENGFRAC, EURFRAC, AGE

Table 7.3
Political rents and constitutions
Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*GRAFT*	*GRAFT*	*GRAFT*	*CPI9500*	*GOVEF*	*GRAFT*
PRES	0.52	0.06	0.02	0.19	0.63	0.73
	(0.44)	(1.94)	(0.41)	(0.63)	(0.47)	(0.59)
MAJ	-0.23	-0.46	-0.25	-0.39	-0.23	-0.26
	(0.49)	(0.54)	(0.38)	(0.74)	(0.48)	(0.43)
Estimation	Kernel	Strat	Nearest	Kernel	Kernel	Kernel
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Logit spec	1	1	1	1	1	2
Obs. on common support	64 *PRES* 69 *MAJ*	64 *PRES* 69 *MAJ*	64 *PRES* 69 *MAJ*	46 *PRES* 57 *MAJ*	64 *PRES* 69 *MAJ*	48 *PRES* 58 *MAJ*

Standard errors in parentheses obtained by bootstrapping
Kernel, Stratification and Nearest-neighbor estimators described in Section 5.5
Logit specifications underlying estimated propensity scores:
1: LYP, GASTIL, AVELF, PROT80, COL_UKA, LAAM
2: LYP, GASTIL, AVELF, PROT80, ENGFRAC, EURFRAC, LAT01

Table 7.4
Productivity and constitutions
Reduced-form estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*LOGYL*	*LOGA*	*LOGYL*	*LOGA*	*LOGYL*	*LOGYL*
PRES	-0.29	-0.21	-0.09	-0.09	-0.08	0.08
	(0.16)*	(0.15)	(0.17)	(0.14)	(0.18)	(0.20)
MAJ	-0.29	-0.15			-0.13	
	(0.15)*	(0.11)			(0.20)	
PIND			0.78	0.47		0.60
			(0.28)***	(0.29)		(0.25)**
MAGN			-1.18	-0.74		-0.62
			(0.34)***	(0.36)**		(0.35)*
AGE	1.05	0.68	0.83	0.54	0.51	0.42
	(0.38)***	(0.34)**	(0.35)**	(0.32)	(0.26)*	(0.24)*
Continents and Colonies	Yes	Yes	Yes	Yes	Yes	Yes
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, narrow	90s, narrow
Obs.	74	73	73	72	56	55
Adj. R2	0.73	0.50	0.76	0.52	0.69	0.73

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls always included: *FEDERAL, LAT01, ENGFRAC, EURFRAC, FRANKROM*
Narrow sample consists of countries where *GASTIL* is less than 3.5

Table 7.5
Productivity and constitutions
Structural-form estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.	*GADP*	*LOGYL*	*LOGA*	*LOGYL*	*LOGA*	*GADP*	*LOGYL*
GADP		3.51 (0.50)***	2.35 (0.58)***	3.24 (0.54)***	2.00 (0.58)***		3.65 (0.55)***
PRES	-0.10 (0.03)***					-0.06 (0.03)*	
MAJ	0.02 (0.04)			-0.38 (0.13)***	-0.22 (0.15)		-0.40 (0.13)***
AGE	0.33 (0.06)***					0.32 (0.05)***	
PIND						0.21 (0.07)***	
MAGN						-0.20 (0.09)**	
Continents and Colonies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimation	OLS	2SLS	2SLS	2SLS	2SLS	OLS	2SLS
Chi2: over-id		10.51	7.46	1.04	1.95		4.65
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Obs.	73	73	73	73	73	72	72
Adj. R2	0.74	0.81	0.49	0.83	0.53	0.79	0.83

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Other first-stage regressors (columns 1 and 5*): LAT01, ENGFRAC, EURFRAC, FRANKROM, FEDERAL*
Other second-stage regressors (columns 4, 5 and 7 only): *ENGFRAC, EURFRAC*
Chi2: over-id refers to the test statistic for the overidentifying restriction that the instruments in the first-stage regressions do not enter the performance equations in columns 2-5 and 7. The critical value (at the 10% level) in the specification underlying columns 2 and 3 is 10.64.

Table 7.6
Productivity and constitutions
Endogenous selection

	(1)	(2)	(3)	(4)	(5)
Dep. var.	*GADP*	*LOGYL*	*LOGA*	*LOGYL*	*LOGA*
GADP		3.56	2.37	3.48	2.35
		(0.56)***	(0.65)***	(0.68)***	(0.62)***
PRES	-0.10				
	(0.06)*				
AGE	0.33				
	(0.07)***				
MAJ				0.14	0.22
				(0.37)	(0.34)
Continents and Colonies	col_uka & conts	Yes	Yes	col_uka & conts	col_uka & conts
Other controls	1	2	2	3	3
Endogenous selection	*PRES*	*PRES*	*PRES*	*MAJ*	*MAJ*
Estimation	Heckman 2-step	generated regressors	generated regressors	Heckman 2-step & gener. reg.	Heckman 2-step & gener. reg.
rho	0.01			-0.67	-0.67
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Obs.	73	73	73	73	73
Adj. R2		0.74	0.52		

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls included:
1: *FEDERAL AGE LAT01 ENGFRAC EURFRAC FRANKROM*
2: *NONE*
3: *ENGFRAC EURFRAC*
First-stage probit specifications for selection in Heckman always include:
CON2150, CON5180, CON81, LAT01, ENGFRAC, EURFRAC, AGE, COL_UKA, LAAM
Precise specifications underlying columns 2-5 described in the text.

Table 7.7
Productivity and constitutions
Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*GADP*	*GADP*	*GADP*	*LOGYL*	*LOGYL*	*LOGYL*
PRES	-0.16	-0.13	-0.14	-0.63	-0.53	-0.32
	(0.05)***	(0.07)*	(0.05)***	(0.29)**	(0.76)	(0.22)
MAJ	-0.02	0.04	0.03	-0.61	-0.30	-0.32
	(0.05)	(0.10)	(0.07)	(0.23)***	(0.77)	(0.32)
Estimation	Kernel	Strat	Nearest	Kernel	Strat	Nearest
Sample	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad	90s, broad
Obs. on	42 *PRES*	42 *PRES*	42 *PRES*	43 *PRES*	43 *PRES*	43 *PRES*
common	61 MAJ	61 *MAJ*	61 *MAJ*	61 *MAJ*	61 *MAJ*	61 *MAJ*
support						

Standard errors in parentheses obtained by bootstrapping
* significant at 10%; ** significant at 5%; *** significant at 1%
Kernel, Stratification and Nearest-neighbor estimators described in Section 5.5
Logit specification underlying estimated propensity score includes: *AGE, LAT01, ENGFRAC, EURFRAC, FRANKROM*

Chapter 8

Fiscal policy: Variation across time

8.1 Introduction

While the previous two chapters relied exclusively on cross-country variation, we now turn to the time variation in the data. Since we want to go as far back in time as possible, we confine the analysis to fiscal policy in the 60-country panel, where we have data since the 1960s or 1970s. Throughout the chapter, we limit the analysis to a few variables: overall (central) government spending or revenue, welfare-state spending, and the budget surplus, all expressed as a percentage of GDP.[1] As described in Chapter 3, these policy measures are available from the 1960s for most OECD countries and many countries in Latin America, and from the 1970s for most of the remaining countries (though welfare spending is only available since the 1970s for all countries). Thus, we study an unbalanced panel, with considerable variation in the length of the time series available for different countries, but with quite long time series for the average country.

To repeat a point made earlier in the book, deep constitutional reforms are so rare that they cannot be meaningfully exploited for statistical inference. Instead, we exploit the interaction between the constitution and other time-varying variables. In Chapter 3, we showed that fiscal policy exhibits considerable time variation. This variation, to some degree, shows up in un-

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[1]The work in this chapter builds heavily on an earlier study by Persson and Tabellini (2000b), but extends their analysis both methodologically and substantively.

explained common time trends. In the 1970s and the first part of the 1980s, the size of government, welfare-state spending and budget deficits thus increased everywhere, a common time variation which is difficult to attribute to observable determinants of policy. Moreover, fiscal policy fluctuates over time in response to observable shocks and events, like income fluctuations and elections. In this chapter, we ask whether these patterns are common across different constitutional groups, or whether they take different forms in presidential versus parliamentary regimes, or under proportional versus majoritarian elections.

In most of Chapter 6, our investigation of fiscal policy was guided by specific predictions derived from the theory summarized in Chapter 2. But, to date, the formal modeling has typically dealt with static environments and does not entail predictions on the interaction between institutions and other events. Hence, our goal in this chapter is somewhat different and more modest. Rather than testing specific hypotheses, we aim at establishing some stylized facts that can be used in the next stage of theorizing. We also aim at somewhat better understanding what mechanisms might underlie the constitutional differences in fiscal policy uncovered in Chapter 6. In particular, what has led to the larger overall size of government and welfare spending in proportional-parliamentary countries? As noted in Chapter 6, these differences among constitutional groups were more pronounced in the 1990s than in the earlier part of the postwar period. Thus, they must at least partly be related to the dynamics of spending between the early 1960s and the late 1990s.

Compared to Chapters 6 and 7, we pay less attention to the endogeneity (selection) of the constitution. The reason is twofold. For one, it is difficult. As we will be estimating dynamic interaction effects between the constitution and other variables, allowing for endogenous constitution selection would raise a number of new econometric subtleties. For another, selection bias is arguably less of a concern here. We always allow for country fixed effects picking up any time-invariant but country-specific unobserved determinants of fiscal policy, such as any *direct* effects of the constitution itself, or of history, geography or culture. We instead focus on *indirect* constitutional effects, captured by interactions between the constitution and other variables. The possibility that historical or cultural determinants of the constitution would also influence these interactions seems more remote than the likelihood of a direct influence on fiscal policy. In Section 2 of the chapter, we present our empirical methodology, clarifying the questions we pose to the data and

the estimation strategy.

Section 3 then considers the response of fiscal policy to common un-explained events and compares policy persistence in different constitutional groups. In presidential regimes, spending (and particularly welfare spending) displays a more dampened response to whatever common events led to the expansion of government spending from the 1960s to the mid 1980s. Fiscal policy variables are also less persistent in presidential than in parliamentary regimes. Moreover, majoritarian electoral rules have a dampening effect on persistence and the reaction to common events, but it is weaker and less robust than for presidentialism.

In Section 4, we turn to the cyclical behavior of fiscal policy. Here, we find a second mechanism that can partly account for a more rapid growth of government in the postwar period in proportional-parliamentary countries. This group of countries (and only this group) displays a ratchet effect in government spending, with an expansion of the size of government and welfare programs during economic downturns that is not undone in subsequent upturns. We also encounter some evidence of a procyclical fiscal policy in presidential regimes.

Finally, Section 5 contrasts electoral cycles under different constitutions. All types of governments are found to cut taxes during election years. Presidential regimes also postpone fiscal adjustments until after the election. Governments in majoritarian countries do not only cut taxes, but also spending, in the election year, while governments in proportional countries raise welfare-state spending on both sides of the election. Section 6 concludes and summarizes the chapter.

8.2 Methodology

Does the constitution influence how fiscal policy reacts to economic or political events, or time-varying determinants of policy? This is the general question addressed in this chapter. In this section, we discuss how to formulate it more precisely and how to structure our strategy of estimation.

8.2.1 The question

Retaining the same notation as in Chapter 5, let Y_{it}^S denote the *potential* policy outcome in country i, year t and constitutional state S, and let $\mathbf{X}_{it}$ be

a vector of time-varying controls (i.e., of policy determinants) - throughout, boldface letters denote vectors or matrices. The constitution is measured by a *time-invariant* dummy variable, $S_i = 1, 0$, reflecting the distinction between majoritarian and proportional electoral rules, or between presidential and parliamentary forms of government. As in previous chapters, we only observe actual policy outcomes, $Y_{it} = S_i Y_{it}^1 + (1 - S_i) Y_{it}^0$.

Suppose that potential policy outcomes are determined by the following stochastic process, which is a reformulation of (5.8) in Chapter 5, allowing for time variation and interaction effects:

$$Y_{it}^S = \alpha_i^S + \lambda^S Y_{it-1} + \boldsymbol{\beta}^S \mathbf{X}_{it} + \varepsilon_{it}^S \ . \tag{8.1}$$

Here, α_i^S captures the effect of *all* time-invariant policy determinants, including the constitution, colonial history and geography, λ^S and $\boldsymbol{\beta}^S$ are unknown coefficients and ε_{it}^S is an unobserved error term uncorrelated with the controls $\mathbf{X}_{it}$. As suggested by the results in Chapter 3, we assume some persistence: potential policy outcomes in the current period depend on actual policy outcomes in the previous period. But now, the serial-correlation parameter λ^S is allowed to depend on the constitutional state. We can rewrite (8.1) in terms of observed policy outcomes:

$$Y_{it} = \alpha_i + \lambda^0 Y_{it-1} + S_i(\lambda^1 - \lambda^0)Y_{it-1} + \boldsymbol{\beta}^0 \mathbf{X}_{it} + S_i(\boldsymbol{\beta}^1 - \boldsymbol{\beta}^0)\mathbf{X}_{it} + e_{it} \ , \tag{8.2}$$

where $\alpha_i = \alpha_i^0 + S_i(\alpha_i^1 - \alpha_i^0)$ and $e_{it} = \varepsilon_{it}^0 + S_i(\varepsilon_{it}^1 - \varepsilon_{it}^0)$. The previous two chapters sought to determine the direct constitutional effect: how the intercept α_i^S varies with S for a country drawn at random. We can now formally see an obvious point made earlier – since both S_i and α_i^S are time invariant, this direct effect can only be estimated from the cross-country variation in the data, as done in Chapters 6 and 7.[2] In this chapter, our goal is instead to quantify differences in coefficients λ^S and $\boldsymbol{\beta}^S$ across constitutional groups, exploiting both time and country variation. The differences $(\lambda^1 - \lambda^0)$ or $(\boldsymbol{\beta}^1 - \boldsymbol{\beta}^0)$ capture what might be called "indirect constitutional effects" on fiscal policy, namely interactions between the constitution and other policy

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[2]The intercept α_i^S can be written as: $\alpha_i^S = \alpha^S + \boldsymbol{\gamma} \mathbf{R}_i$, where $\mathbf{R}_i$ is a vector of constant policy determinants, such as colonial origin or geography, $\boldsymbol{\gamma}$ a vector of unknown parameters and α^S a coefficient reflecting the direct constitutional effects on policy. If $\mathbf{R}_i$ is observed, parameters $\boldsymbol{\gamma}$ and α^S can be identified separately, but only by exploiting the cross country variation in the data, which is what we did in Chapters 6 and 7.

determinants.[3] If such differences are zero, the reaction of fiscal policy to events or other time-varying variables is not systematically related to the constitution. Our general purpose is thus to identify relevant interactions between the constitution and other policy determinants.

Specifically, we focus on three sets of interactions. In Section 3, we ask whether the constitution modifies the influence of *unobserved* determinants of policy that are *common* across countries. An example of such common unobserved events would be the worldwide rise of left-wing ideologies in the late 1960s and early 1970s, and more conservative political movements in the mid 1980s. These common events are unobserved, however, or – at least – very hard to measure for the econometrician. We therefore capture their effect through time-dummy variables, asking whether the estimated coefficients differ across constitutional groups. In Section 4, we focus on cyclical fluctuations, asking whether deviations of GDP from its long-run trend have an impact on fiscal policy that depends on the constitution. We also ask whether there is a different response to positive and negative output gaps. Finally, in Section 5, we turn to electoral cycles, measuring the election dates by means of indictor variables for election or post-election years. We begin by looking for unconditional electoral cycles in fiscal policy, but focus our search on fiscal-policy behavior in the proximity of elections systematically related to the constitution.

8.2.2 Estimation

Throughout this chapter, we take the selection-bias problems taking center stage in Chapters 5 through 7 more lightly; most often we thus assume that $\varepsilon_{it}^1 = \varepsilon_{it}^0 = e_{it}$ for all i's and t's, and that this error term is uncorrelated with the constitutional state, S_i. In this case, equation (8.2) is reduced to a standard dynamic panel (dynamic because it contains a lagged dependent variable), where the parameters of interest can be estimated with a variety of techniques, depending on the assumed properties of the error term – see e.g., Hsiao (1986), Baltagi (1995), or Wooldridge (2002) for overviews.

It is useful to decompose the error term e_{it} in (8.2) into three components:

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[3]These indirect effects are similar to the state-dependent slope coefficients discussed at the end of Section 5.3, and estimated for welfare spending in Section 6.3, although these were identified from the cross-sectional variation, rather than the time-variation in policy.

$e_{it} = \eta_i + v_t + u_{it}$, one varying across countries (η_i), one varying only across time (v_t) and one varying across both countries and time (u_{it}). A general equation to be estimated can then be written as:

$$Y_{it} = \lambda^0 Y_{it-1} + S_i(\lambda^1 - \lambda^0)Y_{it-1} + \boldsymbol{\beta}^0 \mathbf{X}_{it} + S_i(\boldsymbol{\beta}^1 - \boldsymbol{\beta}^0)\mathbf{X}_{it} + \alpha_i^* + v_t + u_{it} \ , \ (8.3)$$

where $\alpha_i^* = \alpha_i + \eta_i$ captures all (observed and unobserved) country-specific and time-invariant policy determinants, including any direct constitutional effect. Such a decomposition was already used when obtaining our basic estimates in Chapter 3.

First, consider the time-specific component of the error term, v_t. We deal with this component in two ways, depending on the specification. In one specification, all controls $\mathbf{X}_{it}$ vary across both countries and time; we then always include year-specific indicator variables (a set of time dummies) as additional regressors, thus removing the yearly mean from all observations. Even if v_t is random, this procedure ensures that the time component of the error term does not (asymptotically) bias our estimates of $\lambda^{\tilde{S}}$ and $\boldsymbol{\beta}^S$. Moreover, as discussed in Chapter 3, the estimated coefficients of the time-dummy variables are of independent interest, since they capture the effects of unobserved determinants of policy outcomes common to all countries, such as common ideological trends. A second specification instead includes the dollar price of oil as a regressor (allowed to enter differently for oil exporters and importers). Since this variable is common for all countries, we cannot separately estimate its coefficient and those of the year-indicator variables. In this specification, we thus omit the year dummy variables, imposing the restriction that $v_t = 0$ for all t, essentially assuming that the oil price is the only time-varying policy determinant common for all countries. If that assumption is violated, the estimated coefficient of the oil price might be biased, because of an omitted-variable problem: this bias would reflect the correlation of the oil price with the omitted common policy determinants.

Next, consider the country-specific component of the error term, α_i^*. Once more, we deal with this component in two ways. Our preferred specification is to estimate (8.3) in levels. In this case, we always include country fixed effects (i.e., country-specific indicator variables), thus removing the country means from all observations. If the coefficients on the lagged dependent variables were zero $(\lambda^1 = \lambda^0 = 0)$, this method would remove any bias due to this component of the error term as the number of countries increases. But if, as plausible, $\lambda^1, \lambda^0 > 0$, an asymptotic bias remains in our estimate

of λ^S, even as the number of countries tends to infinity. The reason is that the initial condition, Y_{i0}, is correlated with the component α_i^* of the error term, which creates a correlation of order $1/T$ between the lagged dependent variable (in the deviation from country means) and the remaining component of the error term, u_{it}.[4] The direction of the bias in our estimate of λ^S has the opposite sign of the true λ^S; if, as likely, $\lambda^S > 0$, we thus tend to underestimate the persistence. Note, however, that this bias becomes smaller as the length of the panel, T, increases. When policy corresponds to the size of government or the budget surplus, the average country panel in our 60-country data set is 26 years, and the bias is probably negligible. In the case of welfare-state spending, we have 16 years per country on average, and the bias problem could be more relevant. A second way of dealing with this component of the error term is to estimate equation (8.3) in first differences. This removes the α_i^* component, but introduces a moving-average component in the remainder of the error term, $u_{it} - u_{it-1}$. To cope with this possible pattern of serial correlation, when (8.3) is specified in first differences, we impose the restriction that $\lambda^S = 0$ and estimate by GLS, allowing for country-specific autocorrelation coefficients in the residuals.[5]

The remaining component of the error term, u_{it}, does not pose any specific challenges beyond the usual pitfalls that it may be correlated with the controls, $\mathbf{X}_{it}$, and the lagged dependent variable, Y_{it}, due to omitted variables, reverse causation, selection bias or serial correlation. Some of these issues are discussed below in the context where they arise.

8.3   Unobserved common events

As already discussed in Chapters 3 and 4, several of our fiscal-policy measures display a similar qualitative development over time in most countries. A plausible conjecture is that these trends reflect some common economic and political events, such as the worldwide rise of left-wing ideologies in the late 1960s and 1970s, the turn to the right in the mid 1980s, or the productivity

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[4] See, for instance, Hsiao (1986, ch.4), or Baltagi (1995, ch.8).

[5] When estimating in first differences, we have also used the Arellano-Bond (1991) GMM-method, which uses earlier lags as instruments for the lagged dependent variable. This method is sensitive to the choice of instruments and can be biased in small samples, especially when the number of panels is low. The results when applying the Arellano-Bond estimator are similar to those reported below, but the over-identifying restrictions for validity of the instruments are always rejected. Hence, we do not report those estimates.

slowdown and the oil shocks in the 1970s and 1980s. Our goal in this section is to find out whether and how the impact of such common events on fiscal policy depends on the constitution. Our main interest is the constitution, so we do not seek to identify and measure the common events. Instead, we treat them as unobserved and proxy for them by a set of year-specific indicator variables, focusing on the interaction between this set and the constitution. This method was suggested by Blanchard and Wolfers (2000) to study how labor-market institutions influence the reaction of unemployment to common unobservable shocks, and was also used by Milesi-Ferretti, Perotti and Rostagno (2002) to compare the reaction of fiscal policy under different electoral systems in the OECD countries.

Let us rewrite equation (8.3) slightly, to get:

$$Y_{it} = \lambda^0 Y_{it-1} + S_i(\lambda^1 - \lambda^0)Y_{it-1} + \boldsymbol{\beta}\mathbf{X}_{it} + (1 + \gamma S_i)\boldsymbol{\delta}\mathbf{Q}_t + \alpha_i^* + u_{it} \; , \quad (8.4)$$

where we have assumed that all observable controls $\mathbf{X}_{it}$ have the same vector of coefficients, irrespective of the constitution, and $\mathbf{Q}_t$ is the time-t value of a vector $\mathbf{Q}$ of year indicators (i.e., a set of dummies, one of which takes the value of 1 in year t, while the others take the value of 0). Our interest is in the γ-coefficients (one for each constitutional rule). If these are zero, the unobserved common events have the same impact in all countries, irrespective of the constitution; conversely, if γ is different from zero, the policy impact of unobserved common events systematically depends on the constitution. A positive value of γ implies that the constitutional feature measured by $S_i = 1$ inflates the impact of common events relative to the default constitutional feature $S_i = 0$, while a negative value implies a dampening effect. Note that the time-varying component of the error term, v_t, has been dropped from (8.4), since its effect is now fully captured by the vector of time dummy variables. The country-specific component, α_i^*, is still included, however.

Given the form of (8.4), we estimate the parameters of interest by nonlinear least squares, also including country-dummy variables.[6] Throughout this section, the vector of controls, $\mathbf{X}_{it}$, always includes the variables introduced in Chapter 3 and used in Chapter 6, namely per capita income (LYP), demographics (the two variables $PROP65$ and $PROP1564$) and openness ($TRADE$). All these variables vary across both countries and time. But we omit time-invariant variables, such as the indicators for federalism, OECD-membership, geography or colonial origin, because their effects on policy are

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[6]In the estimation, we use a set of time dummies from 1961 to 1998, plus an intercept.

already subsumed in the country fixed effect, together with the direct effect of the constitution.

8.3.1 Size of government

Table 8.1 considers the size of government spending and reports the estimates of the coefficient γ for presidential regimes and majoritarian elections (in the rows for the indicators $PRES$ and MAJ). Since both constitutional dummy variables are included in the same regression, the default group consists of proportional and parliamentary countries. Thus, the vector of estimated coefficients $\boldsymbol{\delta} = (\delta_t)$ (one per year, not reported in the table), reflects the impact of the vector of unobserved common events $\mathbf{Q}$ in this default group. The estimated coefficient of presidentialism ($PRES$) in *Table 8.1* captures the difference between presidential-proportional and parliamentary-proportional countries, or alternatively (due to additivity) between majoritarian-presidential and majoritarian-parliamentary countries. The estimated coefficient of majoritarian elections (MAJ) in *Table 8.1* instead captures, say, the difference between majoritarian-parliamentary and proportional-parliamentary countries.

In column 1, we impose the restriction that $\lambda^0 = \lambda^1 = 0$, thereby excluding the lagged dependent variable from the regression. This specification thus forces all the dynamics to be captured either by the included controls, or the time dummies. Since the controls included in $\mathbf{X}_{it}$ exhibit a limited time variation, we attribute a large fraction of the dynamics in government spending to the unobserved common events. The estimated values of γ for $PRES$ and MAJ in column 1 are both negative and highly significant. The estimated coefficient of -0.59 for presidential regimes can be interpreted as follows: An unobserved event in period t raising government spending by 1% of GDP in proportional-parliamentary countries (formally, a year when $\delta_t - \delta_{t-1} = 1$), only raises spending by about 0.4% of GDP ($\approx 1 - 0.59$) in presidential-proportional countries. This is a very large difference. The dampening effect of majoritarian elections is smaller, with a coefficient of –0.37, but also highly relevant.

Table 8.1 about here

Figure 8.1 depicts the estimated effect of unobserved common events in our four groups of countries, when their point of departure is normalized to

the same level in 1960. The uppermost line (with diamonds) refers to the default group of proportional-parliamentary countries (in each year, the line depicts the estimated coefficient δ_t, pre-multiplying the dummy variable of that year in the regression of column 1). The squared, triangular and circular shapes indicate presidential-proportional, majoritarian-parliamentary, and majoritarian-presidential countries, respectively (i.e., each point on these lines depicts the same estimated coefficient δ_t, multiplied by the relevant $(1 + \gamma)$ expression).[7] Until the early 1980s, spending in these constitutional groups reacted in a very different way to whatever generated the common upward movement in spending. But from the early 1980s and onwards, the time pattern of spending looks much more similar in all groups of democracies.

These time patterns across constitutional groups might seem surprising. The early 1980s coincide with the rise of conservative governments in several countries. It is natural to conjecture that different constitutions would imply different reactions of spending to such an ideological swing to the right. But this is not what we observe. The time trend of government spending stabilizes at about the same time in all countries in the mid 1980s (except in the group of majoritarian-presidential countries where the time trend was already absent even before the 1980s). But the common slope of these time paths is really imposed through the constraint in our specification that the differences across constitutional groups remain constant over time. To relax this constraint, we re-specify equation (8.4), allowing the γ-coefficients interacted with each of MAJ and $PRES$ to take on different values before 1982 (the period of an average upward trend) and after 1982 (without an average upward trend). But the estimates for these constitution-dependent γ coefficients (not shown) are very similar across the two sub-periods, both for the electoral rule and the form of government. The interaction between the constitution and the common unobserved events is thus the same before and after the 1980s. In the early half of the sample, however, something drove up government spending everywhere, although much more so in parliamentary and proportional countries. The differences in the size of government across constitutional groups observed today – and documented in Chapter 6 – thus, to a large extent, seem due to events in the period between 1960 and 1980.

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[7]The levels of these curves have all been normalized to zero in 1960. This is to illustrate the relative growth paths of government spending in the four constitutional groups during the last 40 years, but not the relative levels of these paths (to illustrate the latter, we would also have to take account of the average estimated fixed effect in each group).

Figure 8.1 about here

As government spending is highly persistent over time, it would be a mistake to attribute all unexplained variation in spending in a particular year to unobserved common events in that same year. Some of the observed variation could simply reflect a delayed response to previous events. To allow for such persistence in government spending, the specification in column 2 adds the lagged dependent variable ($LCGEXP$) to the regression, while still retaining the restriction that its coefficient is common across constitutions (i.e., we assume that $\lambda^1 = \lambda^0$). The common events captured by the time-dummy variables now play a smaller role: their estimated coefficients $\boldsymbol{\delta}$ are much smaller than in the regression of column 1, and precisely estimated. The estimated constitutional effects associated with the year effects also change somewhat: while the estimate of γ for presidential regimes remains quite stable and highly significant, the estimated effect of majoritarian elections drops to –0.23 and is now only significantly different from zero at the 10% level.

In column 3, we also allow the coefficient for the lagged dependent variable to take on a different value across constitutional groups. (The estimated coefficient in the $PRES * LCGEXP$ row of column 3 corresponds to the difference in persistence between presidential-proportional and parliamentary-proportional countries, $\lambda^1 - \lambda^0$, and similarly for the estimated coefficient of *MAJ*LCGEXP.*) Proportional and parliamentary democracies display more persistence: the estimate of λ^0 rises from 0.8 in column 2 to 0.85 in column 3, while presidential regimes and (to a smaller extent) countries with majoritarian elections display less persistence. Both constitutional effects on persistence are statistically significant, though the effect of presidentialism is larger. But the evidence for a constitutional interaction with the unobserved common events is now much weaker: the estimate of the γ coefficient for presidentialism is smaller and no longer statistically different from zero; in the case of majoritarian elections, the estimated value of γ is close to zero.

Finally, column 4 asks whether these results are robust if the degree of persistence is allowed to also vary with colonial origin or geographic location. In terms of equation (8.4), we are thus worrying about non-random selection in the form of a correlation between the error term u_{it} and the constitutional indicators, S_i. In the light of the results reported in column 3 and to simplify the estimation problem, we remove the non-linear interaction between the constitution and the time-dummy variables, constraining the γ coefficients

to zero. We thus estimate with linear fixed effects, including both time-dummy and country-dummy variables. Presidential regimes continue to have significantly less persistence with an identical point estimate, even though we interact the lagged dependent variable with a dummy variable for Latin America ($LAAM*LCGEXP$) – the continent where presidential regimes are most over-represented. The effect of the electoral rule on persistence is not as robust, however; it disappears when interacting the lagged dependent variable with UK colonial origin ($COLUK*LCGEXP$) – the historical origin where majoritarian elections are most over-represented.

From these results, we infer that presidential democracies indeed have less persistent dynamics in overall spending than parliamentary democracies. This reflects less inertia in spending, as well as a more dampened reaction to common unobserved events. Majoritarian democracies also have less persistent dynamics, but look much more similar to proportional democracies.

In other words, government spending increased in many countries from the postwar period until the mid 1980s. It increased most in parliamentary countries, because the (generally upward) movements in spending had a larger permanent component, and because the reaction of spending to unobserved common events was greater in this group.

8.3.2   Welfare spending

We already know from Chapters 4 and 6 that proportional and parliamentary democracies have larger welfare states than other constitutional groups. As welfare-state spending typically stems from entitlement programs, it is likely to be highly persistent. Thus, if we find a higher persistence of total government spending in proportional or parliamentary democracies, it is natural to attribute this to their larger welfare states. With this motivation in mind, we now turn to the interaction between the constitution and the dynamics of welfare-state spending, repeating the analysis of the previous subsection. Note that our panel is shorter in this case, as data on welfare spending are only available from the early 1970s and onwards for most countries.

Table 8.2 contains the results when we estimate the effect of unobserved common events with social security and welfare spending in percent of GDP (SSW) as the dependent variable. In column 1, we do not include the lagged dependent variable. The year-specific indicator variables proxying for common events now span over the period 1973 to 1998. The estimated coefficients on these variables peak in the early 1990s and remain roughly constant there-

after. At the peak, the difference between the estimated coefficient and the coefficient of the 1973 year dummy is about 5. This result suggests that unobserved common events account for a rise in welfare spending of about 5% of GDP in the default group of proportional-parliamentary countries throughout this period. But the impact on the other constitutional groups is much smaller, as revealed by the estimated γ coefficients: –0.52 for presidential regimes and –0.17 for majoritarian elections. These estimates are quite similar to those for total government spending, reported in column 1 of *Table 8.1*. In words, the unobserved common events that raised welfare spending by 5% of GDP in the default group only raised it by 4% of GDP in majoritarian-parliamentary countries, by 2.5% of GDP in proportional-presidential countries, and by 1.5% of GDP in majoritarian-presidential countries.

Table 8.2 about here

When we add the lagged dependent variable ($LSSW$) in column 2, the estimated interaction between the time dummy variables and presidentialism drops to –0.33 and remains significantly different from zero, while the interaction term with majoritarian elections goes up to –0.37. These estimated interaction effects remain stable around the same values (and stay significantly different from zero), when we allow the coefficient on the lagged dependent variable to vary across constitutional groups in column 3. Contrary to the findings for total government spending, we cannot reject the hypothesis that the lagged dependent variable has the same coefficient, irrespective of the constitution. This last result remains true even if we interact the lagged dependent variable with the constitution as well as with dummy variables for Latin America and British colonial origin (results not shown). As a final piece of sensitivity analysis, we let the non-linear response to the unobserved events depend not only on the constitutional indicators, but also on the indicators for Latin America and British colonial origin.[8] In column 4, the dampening effect of presidential regimes remains at its previous level, while the dampening effect of majoritarian elections appears less robust.

Summarizing, the data reveal important indirect constitutional effects on welfare spending. These effects are similar to those uncovered for total

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[8]In terms of equation (8.4), we thus allow for four γ coefficients: one each for presidential regimes ($PRES$), majoritarian elections (MAJ), Latin-American location ($LAAM$), and British colonial origin ($COLUK$).

government spending, but with some subtle differences. The dynamics of both total spending and welfare spending are more dampened in presidential than in parliamentary regimes. But while total spending is less persistent in presidential countries, the constitution does not seem to affect the persistence of welfare spending. Instead, the constitutional effect on welfare spending stems from a different reaction to common unobserved events: the common events increasing welfare spending in parliamentary countries had a much smaller impact in presidential regimes. Finally, the electoral rule also affects the dynamics of welfare spending, with majoritarian countries reacting less to unobserved common events, although this finding is somewhat less robust. Naturally, the different constitutional effects could, to some degree, reflect the different time periods: 1973-1998 for welfare spending, and 1961-1998 for total government spending.

8.3.3   Budget surplus

As already noted in Chapters 3, 4 and 6, most countries have, on average, been running deficits, i.e., negative values of our dependent variable (SPL). For this variable, yearly data are available for the full sample period of 1960-1998 in many countries. Column 1 of *Table 8.3* shows the results when we estimate the response to common events in the form of time-dummy variables, but without the lagged dependent variable. In the default group of proportional and parliamentary countries, unobserved common events increased the deficit by a whopping 8% of GDP from 1961 to a peak reached in the early 1980s (the coefficient on the year indicators is close to 0 in the early 1960s and about –8 in the early 1980s). From then onwards, unobserved common events reduce the deficit in the default group by 4-5% of GDP, with a gradual and monotonic decline continuing until 1998. But in presidential democracies, common unobserved events had a considerably more muted impact than in parliamentary democracies: as shown in column 1, the estimated γ coefficient for presidentialism is –0.44. The electoral rule, in contrast, seems to have a weaker and less precisely estimated effect on the response to unobserved common events.

Table 8.3 about here

Columns 2 and 3 of the table show these results to be robust to including the lagged dependent variable, with or without the constraint of com-

mon coefficients across constitutional groups. A further result now emerges, however: budget deficits in presidential regimes also display less persistence. Instead, the electoral rule does not affect the degree of persistence.

Finally, columns 4 and 5 show the results of further sensitivity analysis. We also include the interaction of the indicator variables for Latin America and UK colonial origin with the common events (column 4) and the lagged dependent variables (column 5). These specifications demand a great deal from the data, perhaps too much. Anyway, they show differences in persistence between presidential and parliamentary countries to be robust, while the dampening effect of presidentialism is not.

8.3.4   Summary

This section uncovers several indirect constitutional effects on fiscal-policy dynamics. Presidential countries in particular stand out as quite different from the others.

Overall government spending grew everywhere as a result of some common, but unobserved, events from the early 1960s to the early 1980s. Whatever the cause of this common rise, it had a much larger impact among proportional and parliamentary democracies. Generally, presidential regimes were much less affected with some dampening also in majoritarian democracies. Similarly, common events raised welfare spending in all countries before the 1990s, while a majoritarian electoral rule and a presidential form of government dampened this pattern, with a particularly strong effect of presidentialism. The differences in these time trends are large enough to quantitatively account for a good part of the constitutional effects documented in the cross-sectional analysis of Chapter 6.

A second related finding concerns the degree of persistence: overall spending is less persistent in presidential than in parliamentary countries. Since the persistence of welfare-state spending does not differ across constitutions, other components of government spending (such as public employment and health spending) must be more persistent in parliamentary democracies.

Finally, budget deficits in presidential countries display a unique time pattern: they are less persistent and respond less to the common events that raised deficits world-wide in the 1970s and 1980s.

8.4   Output gaps

Part of the time variation in fiscal policy reflects the response to changes in other variables, such as shocks to aggregate output and income. These responses might be the result of automatic stabilizers – for given tax schedules or remuneration rates in entitlements programs – or of deliberate policy decisions triggered by the business cycle. In this section, we focus on such cyclical fluctuations in fiscal policy and their interaction with the constitution. As in the previous section, we are not led by sharp theoretical priors, but seek to describe the systematic patterns in the data. Nevertheless, the findings in Chapter 6 of direct constitutional effects on all aspects of fiscal policy lead us to expect that the cyclical policy response might also be systematically influenced by the constitution.

Throughout this section, we estimate the following version of equation (8.3):

$$Y_{it} = \lambda^0 Y_{it-1} + S_i(\lambda^1 - \lambda^0)Y_{it-1} + \phi^0 YGAP_{it} + S_i(\phi^1 - \phi^0)YGAP_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \alpha_i^* + u_{it} \; .$$
$$(8.5)$$

The variable $YGAP$ is the output gap, the percentage deviation of income from a country-specific trend, as defined in Chapter 3. We want to know whether the effect of this variable on fiscal policy depends on the constitutional state (i.e., whether the coefficients ϕ^1 and ϕ^0 are the same). The other controls in $\mathbf{X}$ are the same as in the previous section (the two population variables, openness to trade and per-capita income). As in Chapter 3, we also include the price of oil (OIL) as a proxy for economic shocks common to most countries, while allowing for a different effect in oil-exporting and oil-importing countries. The constitution is measured by our two indicators for majoritarian elections and presidential regimes (MAJ and $PRES$), with proportional and parliamentary countries as the default group ($MAJ = PRES = 0$).

In principle, all controls in $\mathbf{X}$ could interact with the constitution and their $\boldsymbol{\beta}$ coefficients could vary with the constitutional state. In practice, this does not occur, however: for most variables and most specifications, we cannot reject the nul hypothesis that the $\boldsymbol{\beta}$ coefficients are the same, irrespective of the constitutional state. While the results vary across specification and estimation methods, we find no robust and clear pattern of interactions with the constitution. Therefore, we impose the constraint that all controls in $\mathbf{X}$ have the same $\boldsymbol{\beta}$ coefficients irrespective of the constitution, and exclusively

focus on the output gap.

To take account of the country-specific component of the error term, α_i^*, we estimate equation (8.5) in levels with country fixed effects. We also check that the results are robust to estimating in first differences and allowing for country-specific autocorrelation in the error term. Since one of the regressors (the oil price) is common to all countries, we drop the year dummies to avoid colinearity.

As noted in Chapter 3, the output gaps take on very large values (as large as 10% or more) for some observations. To avoid basing our inference on a few outlying observations, we restrict the sample to observations where the gaps are strictly less than 5% in absolute value. (Including the full sample with the outlying observations for output gaps strengthens the results reported below.) Finally, we ignore a possibly important estimation problem: a component of the output gap could be endogenous and reflect an exogenous variation in fiscal policy itself. This might bias the estimated coefficient ϕ downwards when the dependent variable is government revenue or the budget surplus, and upwards when the dependent variable is government spending. The bias is unlikely to affect the inference about constitutional interactions, however, unless the endogenous component of output varies with the constitution.

8.4.1 Size of government

We begin with overall government spending ($CGEXP$). Consider the first three columns of *Table 8.4*. Column 1 estimates equation (8.5) for the full sample of democracies, excluding the output gaps exceeding 5% in absolute value. Columns 2 and 3 restrict the sample to smaller output gaps (less than 3% in absolute value), and better democracies (the variable $POLITY\_GT$ less than 1.1). The results are very similar across all samples. First, we confirm the finding in the previous section that government spending is much more persistent in parliamentary than in presidential democracies, whereas the indirect constitutional effect of the electoral rule on persistence is more frail.

Second, the contemporaneous response of government spending (in percent of GDP) to output gaps varies with the constitution. In the default group of proportional and parliamentary countries, the estimated coefficient of output gaps ($YGAP$) is consistently negative with a value of about –0.2, meaning that a 5% drop in real income induces a rise in the spending ratio of nearly 1 percentage point. Since spending is highly serially correlated, this

effect persists over time. But in presidential regimes, the spending to GDP ratio reacts in a different way to output gaps (*PRES*YGAP* has a coefficient significantly different from zero). In these countries, government spending as a share of GDP is acyclical (the sum of the coefficients of *YGAP* and *PRES*YGAP* is not significantly different from zero). Note, however, that this constitutional effect is much weaker in good democracies (the variable $PRES*YGAP$ is not statistically significant in column 3). In majoritarian countries, the estimated contemporaneous impact of income fluctuations is smaller than in proportional countries, but the difference is neither robust nor statistically significant.

Table 8.4 about here

The estimated policy responses to output gaps under different constitutions are depicted in *Figure 7.2.* This figure pushes the results somewhat by portraying the spending responses in the four constitutional subgroups in the wake of a one-year positive 1% output gap, according to the point estimates in column 1 of *Table 8.4*.[9] The labeling of the groups is the same as in *Figure 8.1* above. While a positive boost to income has virtually no effect in presidential-proportional countries (marked by squares), it leads to a marked and protracted drop in the spending to GDP ratio in proportional-parliamentary countries (marked by diamonds), a small drop in majoritarian-parliamentary countries (marked by triangles), and a small hike in majoritarian-presidential countries (marked by circles).

Figure 8.2 about here

To gain a better understanding, column 4 of the table disaggregates output gaps into positive (*POSYG*) and negative (*NEGYG),* still interacting them with our two constitutional dummy variables. *Figure 8.3* depicts the responses of spending to positive and negative deviations of income from the trend. These figures reveal an interesting asymmetry in that the main action is associated with negative rather than positive output gaps. In proportional-parliamentary countries, only negative output gaps significantly change the spending ratio, and the estimated coefficient is much larger in absolute value

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[9]Note that here, we are neglecting possible delayed effects of fiscal policy on the *YSHOCK* variable itself. To take those fully into account, we would need to estimate a panel VAR.

than for positive output gaps. This asymmetry suggests a ratchet effect: a negative drop in income induces a lasting expansion in the size of government, which is not undone when income grows above its potential. But this ratchet effect is not present in proportional-presidential countries, and – if anything – appears to have the reverse sign in majoritarian-presidential countries (though the difference between proportional and majoritarian elections is not statistically significant).

Figure 8.3 a and b about here

Columns 5 and 6 of *Table 8.4* assess the robustness of these results to alternative specifications and estimation methods. In both columns, we try to address the non-random pattern of constitution selection by interacting the output gap not only with our two constitutional dummy variables, but also with the dummy variables for Latin American location ($LAAM * YGAP$) and British colonial origin ($COLUK * YGAP$). Thus, we allow the effect of output gaps to vary not only with the constitution, but also with history and geography. We estimate both in levels (column 5) and in differences with country-specific serial correlation in the residuals (column 6). Each set of estimates should be compared with those in column 1. In column 5, the estimated γ coefficient on the output gap for presidential democracies remains large (0.13), but is no longer statistically significant. In column 6, however, the contrast between parliamentary and presidential countries reappears strongly and with statistical significance, even though the interaction of output gaps with the Latin American and the British colonial-origin dummy variables is also significant. In both specifications, the indirect effect of different electoral rule vanishes, however, and is picked up by the colonial-origin variable.

How can these constitutional effects be explained? The larger cyclical response of the spending to GDP ratio in proportional-parliamentary democracies could reflect their larger welfare states: the outlays of such entitlement programs are fixed in cash terms, or might even be inversely related to income. But the presence of a ratchet effect only among proportional-parliamentary countries is harder to explain, and suggests that the constitution might also have a direct effect on the discretionary policy reaction to exogenous events. One possibility is related to the theoretical discussion in Chapter 2 and the empirical findings in Chapter 6. If proportional elections and parliamentary regimes indeed both have a bias towards larger overall

spending, politicians in those systems may be less prepared to cut spending when the economy is doing badly. Another explanation may lie in the incidence of coalition governments. As discussed in Chapters 2 and 4, such governments are more common in proportional and parliamentary countries. And they may induce a greater status-quo bias – particularly in bad times – due to the difficulties in bargaining, highlighted by economists such as Alesina and Drazen (1991) and political scientists as Tsebelis (2002).

Yet another possibility is that some democracies are more likely to face binding borrowing constraints. If presidential democracies are more likely to experience political crises, as some political scientists hold (cf. Chapter 2), they may also have more frequent debt or currency crises. Borrowing constraints would impart a procyclical bias to fiscal policy: governments must cut spending or raise revenues when hit by a recession or by a financial crisis, since they cannot let the deficit absorb the shock. Indeed, many presidential regimes are located in Latin America or Africa, where financial crises have been more frequent, and earlier studies have shown fiscal policy in Latin America to be more pro-cyclical than elsewhere – see, in particular, Gavin and Perotti (1997). The estimates in columns 5 and 6 of *Table 8.4* are consistent with this notion; yet, we find that the indirect effect of presidential regimes remains in those columns. Whatever its interpretation, the asymmetric ratcheting upwards of government spending contributes to the differential size of governments in different political systems uncovered by the cross-sectional analysis.

To shed further light on these alternative interpretations, we now turn to the analysis of the cyclical response of welfare-state spending and budget deficits.

8.4.2   Welfare spending

In this subsection, we consider similar regressions for welfare-state spending. Columns 1 and 2 of *Table 8.5* thus report the estimated response to output gaps smaller than 5% (column 1) and 3% in absolute value (column 2), in the default sample of democracies. Column 3 restricts the sample to better democracies ($POLITY\_GT < 1.1$). The results are similar to those for overall government spending, although there are some discrepancies. Like total spending, welfare spending is most counter-cyclical among proportional-parliamentary democracies, and least counter-cyclical among majoritarian-presidential ones, with the other two groups in between. The electoral rule

also plays a role, however, with majoritarian countries responding significantly less than proportional countries (particularly when the sample is restricted to good democracies). Moreover, in proportional-parliamentary countries, the cyclical response of welfare spending to output gaps is somewhat smaller than that of total government spending – cf. the first three columns in *Table 8.4* - meaning that other components of spending are also strongly countercyclical. But the difference between presidential and parliamentary governments is less marked: while *total* government spending as a fraction of GDP is constant over the cycle in presidential democracies, welfare spending remains somewhat counter-cyclical even in presidential countries. Finally, in contrast to total government spending, inertia in welfare spending is never affected by the constitution (thus confirming what we had already found in the non-linear estimation in Section 3).

Table 8.5 about here

Column 4 of the table decomposes output gaps into positive and negative ones. Once more, there is a ratchet effect in proportional-parliamentary countries, with positive gaps having no effect on welfare spending relative to GDP, but negative gaps expanding the welfare state. As for total government spending, the ratchet is eliminated in proportional-presidential democracies. Here, the electoral rule also makes a significant difference (like in columns 1-3 of *Table 8.5* but unlike column 4 of *Table 8.4*), however. Once more, the estimated coefficients reported in *Table 8.5* are somewhat smaller than those in *Table 8.4*. This suggests that the ratchet effect mainly concerns the welfare state, but that other spending items must also exhibit an asymmetric response to output gaps.

Finally, columns 5 and 6 add the interaction of the output gap with the dummy variables for Latin American location and British colonial origin, estimating in levels and first differences. The results for levels are fragile and the constitutional interactions lose statistical significance. But they reappear as significant in the results for first differences. In both cases, Latin American location and British colonial origin make the response to output gaps less counter-cyclical.

These estimates do shed some light on the possible interpretations offered at the end of the previous subsection. They suggest that the larger welfare states in proportional-parliamentary democracies indeed make automatic stabilizers more important in this constitutional group. But the

somewhat different results obtained for total spending and welfare spending suggest that the welfare state is not the whole story, and that other spending items also respond differently to the cycle under different constitutions.

8.4.3   Budget surplus and government revenue

If a deviation of income from trend expands government spending, the expansion can be financed by taxation or borrowing. In this section, we try to infer whether this choice also depends on the constitution. *Table 8.6* thus estimates the cyclical response of the budget surplus and government revenue, both scaled to GDP. To save space, we only report the results for the default sample of democracies and output gaps not exceeding 5% in absolute value.

Columns 1 and 2 of *Table 8.6* are estimated by seemingly unrelated regressions (SUR), in levels and with fixed country effects, for government spending, government revenue and budget surplus. The results for spending are not reported, since they are similar to those in *Table 8.4,* while those for revenues are reported in columns 5 and 6.[10] Column 1 reports the results for output gaps in the basic specification, while column 2 decomposes the output gaps into different signs. In columns 3 and 4, we add output gaps interacted with the dummy variables for Latin America and British colonial origin to allow for non-random constitution selection, estimating in levels with country fixed effects, and in differences by GLS.

Table 8.6 about here

What do we find? First of all, budget deficits are less persistent in presidential regimes, thereby confirming the findings of the non-linear estimation in *Table* 8.3. As in that table, persistence is not significantly related to the electoral rule. Second, the constitution also affects the cyclical response of the budget surplus. In the default group of proportional-parliamentary democracies, the budget surplus increases in booms and shrinks in recessions, as expected. The cyclical response of the surplus in this constitutional group of countries is particularly evident and large in columns 3 and 4, where we also

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[10]In *Table 8.4,* we estimated the spending equation in isolation, rather than by SUR. The reason is that we have more observations on spending than on tax revenue and surpluses; hence the joint estimation by SUR increases efficiency for the last two dependent variables, but implies a loss of observations for government spending.

interact output gaps with Latin America and British colonial origin. But the cyclical response mainly seems to emanate from recessions (column 2), thus conforming to the earlier findings of asymmetric spending responses to positive and negative output gaps. Majoritarian-parliamentary countries behave in the same qualitative way as proportional-parliamentary countries, but the pro-cyclical response of the surplus is more accentuated, particularly when controlling for British colonial origin (columns 3 and 4). In proportional-presidential countries, we instead find an acyclical response of the surplus, consistent with the acyclical spending response found in *Table 8.4.* The absence of a systematically cyclical surplus in presidential democracies is due to large, negative responses to positive output gaps (column 4): in a boom, presidential governments shrink the surplus (or expand the budget deficit). A possible interpretation was already mentioned above: presidential regimes may face binding borrowing constraints that are relaxed in good economic times. Another possibility is reverse causation; an expansionary fiscal policy leading to a boom, rather than vice versa.

The last two columns of *Table 8.6* estimate the cyclical response of government revenue. Here, we do not detect many constitutional interactions, except for a strong response of government revenues to negative output gaps in majoritarian countries. The positive estimated coefficients of output gaps interacted with majoritarian electoral rule show that majoritarian countries are alone in cutting taxes during recessions, perhaps because they engage in Keynesian stabilization policies.

8.4.4   Summary

The cyclical response of fiscal policy is indeed affected by the constitution. Proportional-parliamentary countries display strong ratchet effects in total and welfare spending: spending in percent of GDP increases in cyclical troughs, but does not fall in booms. This ratchet effect is absent in presidential regimes, where spending relative to GDP varies much less over the business cycle, whatever the electoral rule. Larger welfare states account for some of these patterns, but not for all. In other words, the ratchet effect in parliamentary-proportional countries extends also to other spending items.

Finally, the cyclical pattern of the budget surplus (and, to some extent, overall spending) suggests that presidential democracies – and not only those found in Latin America – pursue fiscal policies where the budget deficit strongly expands in economic upturns. Such a procyclicity is not

found in parliamentary democracies, however. On the contrary, majoritarian-parliamentary countries appear to cut taxes during recessions.

8.5   Elections

Not only economic, but also political, events are likely to induce variations in fiscal policy. Elections of the legislature and the executive are recurrent political events in any democracy. Naturally, executive elections are only separately held in systems with a popularly elected president. In this section, we study the behavior of fiscal policy in the proximity of elections, again trying to identify interactions with the constitution.

A sizable empirical literature deals with electoral policy cycles. Most of it has focused on monetary policy in OECD countries, however, with somewhat inconclusive results. Empirical work on fiscal policy is more recent and less systematic, and many studies rely on data sets from a small number of political jurisdictions. Recent research suggests that politicians systematically manipulate fiscal policy *before* elections. Moreover, some studies find these electoral cycles to be more pronounced in developing countries ruled by worse democratic institutions, or affected by other constitutional provisions. Little is known about the systematic pattern of fiscal policy *after* elections, as existing research on post-election cycles has almost exclusively focused on "partisan" (i.e., left or right) cycles.[11]

Why is it reasonable to expect the nature of electoral cycles to vary with the constitution? In Chapter 2, we discussed the career-concern model of electoral cycles due to Persson and Tabellini (2000a, Ch. 9), where majoritarian elections are associated with stronger individual accountability – and

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[11]Among the more recent studies on international data, Shi and Svensson (2001) analyze a large panel of developed and developing countries, focusing on how electoral cycles interact with voters' access to information and incumbents' access to rents. Schucknecht (1996) and Block (2000) study different samples of developing countries, as does Gonzalez (1999) who also focuses on the interaction with the quality of democratic institutions. Among the papers using regional data, Besley and Case (1995) and Lowry,. Alt and Ferree (1998) focus on the US states, the former asking whether cycles are stronger when governors are not up against a term limit and the latter conditioning on the form of election and the party in power. Pettersson-Lidbom (2002) studies a panel of almost 300 Swedish municipalities. All these papers find evidence of pre-election cycles in fiscal policy. Alesina, Roubini and Cohen (1997), Drazen (2000a), (2000b) and Persson and Tabellini (2000a) review the theoretical and empirical literature.

therefore lower taxes and wasteful spending – than proportional elections, where politicians are more collectively accountable. Sharper incentives under majoritarian elections should result in larger tax and spending fluctuations around the elections. Moreover, we have emphasized the prediction that proportional electoral rules give politicians stronger incentives to garner votes via broad policy programs, such as welfare-state spending. It is not far fetched to expect these incentives to be at their strongest at election time, resulting in different electoral cycles in the composition of spending, depending on the electoral rule.

When it comes to the form of government, we have stressed how policy-making incentives differ in presidential and parliamentary democracies, both for the size and composition of government spending. Once more, it is reasonable to expect these effects to show up more strongly at election time. Another difference between presidential and parliamentary forms of government is the individual vs. collective nature of the executive. By analogy with the above career-concern argument that individual political accountability gives stronger incentives than collective accountability, we might expect stronger electoral cycles under presidential regimes.[12]

Based on the above motivation, we search for evidence of different electoral cycles in fiscal variables under different electoral rules and forms of government. To carry out this search, we adapt the empirical methodology used in the previous section. As we want to find evidence of electoral cycles, it is important to allow for reasonably rich dynamics in the policy variables. As we have seen in earlier sections, all our fiscal instruments display a great deal of inertia. Therefore, we always include the lagged dependent variable on the right-hand side of our regressions. Since fiscal instruments tend to be highly cyclical, we also include our measure of cyclical deviations from trend ($YGAP$). On top of this, we allow the dynamics to differ across constitutional groups; specifically, we include interaction terms between both constitutional indicators (MAJ and $PRES$) and the lagged dependent variable, as well as the output gap, in the regression. This is important to avoid confounding different general policy dynamics with different electoral cycles in different constitutional groups. A natural starting point is thus equation (8.5) in the previous section, where the lagged dependent variable and the output gaps

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[12]Lowry, Alt and Ferree (1998) make a similar point when arguing – and empirically showing – that voters respond more vigorously to policy in gubernatorial elections than in legislative elections in the US states.

are allowed to have different coefficients across constitutional groups, but the other variables included in the control vector $\mathbf{X}$ are constrained to have the same coefficients. Throughout the section, we make the estimation in levels, adding country fixed effects. To better separate the effect of elections from other common events in a given year, we replace the oil price with a vector of time dummy variables constrained to have the same coefficients in all countries.

To search for constitution-dependent electoral cycles, we obviously need information on election dates. In parliamentary democracies, elections of the legislature and the executive coincide. In presidential democracies, the executive is elected separately, but the legislature is almost always elected in the same year (in our sample, only about ten presidential elections do not coincide with elections of the legislature). Nevertheless, in presidential regimes, there are also many "mid-term" legislative elections in between the years of simultaneous presidential and legislative elections. Our prior is that the incentives created by these mid-term elections are weaker than those when both the president and the legislature are elected. Indeed, this is what the data suggest: when estimating electoral-cycle models for our different policy instruments, we never find mid-term elections to be significant determinants of policy. In the following, we therefore limit the attention to the years of presidential elections.[13] That is, in all regimes, we code the year when the executive is elected. The resulting variable (labeled $ELEX$) is thus equal to 1 in the years of presidential elections in presidential countries and in the years of legislative elections (for the lower house) in parliamentary countries; in all other years, it is equal to zero. To study fiscal policy behavior both before and after elections, we also use the one-year lags of the executive election dates (labeled $LELEX$).

A prospective econometric problem is that some election dates may not be exogenous. This is less important in presidential regimes, where elections are typically held on a fixed schedule with, say, four or six years in between elections. The concern is greater for parliamentary democracies, where the election date often reflects tactical choices of incumbents or government crises. Specifically, endogenous election dates may be correlated with the economic

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[13]Another reason for leaving out the mid-term elections is more pragmatic, namely that we want to study both pre-election and post-election years. In some countries, this poses problems with too much crowding. If presidential elections are held every four years and legislative elections every second year, e.g., each year would either be a pre-election or a post-election year.

cycle: incumbent governments calling early elections when the economy is doing well, or government crises – and new elections – erupting when it is doing badly. This may bias our estimates of electoral cycles, as our policy instruments are expressed as percentages of GDP. But these prospective problems are addressed by our inclusion of income shocks ($YGAP$) among the controls, both alone and interacted with the constitutional indicators. These variables should account for any regime-specific correlation between the policy variable of interest and the election date induced by the economic cycle. This, in turn, should reduce any simultaneity bias from an error term correlated with election dates.

In the next subsection, we start out by constraining the coefficients of the electoral dummy variables to be the same for all countries, irrespective of their constitution, and characterize the nature of unconditional electoral cycles in fiscal policy. We then allow their coefficients to differ with the electoral rule, thereby contrasting majoritarian and proportional elections. Subsequently, we study electoral cycles, conditional on the form of government, contrasting presidential and parliamentary countries. A final subsection digs deeper for the roots of the results, by disaggregating the electoral variables into a full, four-way classification of constitutional groups.

8.5.1 Unconditional electoral cycles

We start with the results when all constitutional groups are constrained to respond to the election date in the same way. As mentioned above, we report the results for elections to the executive (the $ELEX$ and $LELEX$ indicators). Our broadest sample includes more than 500 executive elections, but that number is somewhat reduced, depending on data availability for the policy variables (especially welfare-state spending), and whether we restrict the sample to better democracies. The results corresponding to legislative elections are very similar. As already mentioned, the similarity is likely to reflect the coincidence of these two functions of elections in all parliamentary regimes, the coincidence of electoral dates in many presidential regimes and the lesser importance of mid-term elections.

Table 8.7 shows the results for all fiscal policy variables studied in this chapter, namely overall spending ($CGEXP$), overall revenue ($CGREV$), budget surplus (SPL) and welfare spending (SSW). For each policy variable, we report the results from two different samples, corresponding to our most and least generous definitions of democracy ($POLITY\_GT$ less than 3.7

and 1.1, respectively, see Chapter 4).

Table 8.7 about here

A number of regularities stand out. There is no significant effect on overall spending in the election year. But the estimated coefficient of lagged elections ($LELEX$) on spending is about – 0.3 in both samples (columns 1-2); it is statistically significant, except in the sample of better democracies. Thus, on average, spending is reduced by 0.3% of GDP in the year after the elections. It appears that incumbent executives procrastinate over painful cuts in spending until the year after the election – alternatively, newly elected executives carry out necessary fiscal adjustments early on in their term. Second, taxes are cut by about 0.4% of GDP during an election year. Revenues are also raised after the elections, adding further evidence that painful adjustments are postponed; but a significant post-election tax hike is only present in the better democracies (columns 3-4). Third, the budget surplus improves in the year after the election by about the same order of magnitude. It also deteriorates in the election year, but this pre-election effect is small and not statistically significant (columns 5-6). Finally, no electoral cycle is evident in social-security and welfare spending (columns 7-8). Contrary to the findings of earlier studies, we find no systematic evidence of worse democracies having larger electoral cycles.

These findings are broadly in line with our priors and the predictions of the literature on electoral cycles. According to existing models, both opportunistic and rent-seeking incumbents want to appear competent in the eyes of imperfectly informed voters just before the elections, and they do this by manipulating policy in the election year. Government revenues do indeed fall in an election year, as predicted by both opportunistic and agency models of cycles. But government spending does not change in an average election year, the data are thus silent on the point where the two models deliver different predictions. Instead, spending cuts are postponed until after the elections. The latter effect seems to dominate the government budget balance, since the surplus also improves after the elections. One interpretation of these findings is that tax revenue is easier to manipulate in a discretionary way, while aggregate government spending is more rigid, so that its timing is harder to fine tune; in the wake of unpleasant spending cuts, politicians

procrastinate and do not impose them until after the elections.[14]   Another possible explanation is that these unconditional results conceal systematic differences across different political systems. We now turn to this possibility.

8.5.2   Proportional vs. majoritarian democracies

Are the electoral cycles similar under proportional and majoritarian elections? To answer this question, split the two earlier indicator variables for election years (current and lagged) into four, two for proportional and two for majoritarian electoral systems. For example, the EL_MAJ variable is defined as $MAJ * ELEX$, while the EL_PRO variable is defined as $(1 - MAJ) * ELEX$, and similarly for the lagged election variables. *Table 8.8* reports the results when we use these new indicators to estimate the same regression package as in the previous section. The table also reports the F-statistic for a test of the hypothesis that the coefficients on the current (lagged) election indicators are equal across electoral rules.

Different electoral rules do indeed seem to induce quite different electoral cycles. Starting with the aggregate variables, we find that the election-year tax cuts identified in the previous subsection seem to be common to both types of elections (columns 3-4). But the estimated tax cuts in majoritarian countries are more aggressive, amounting to about 0.6% of GDP. In proportional countries, the tax cuts are smaller and not as precisely estimated. But we cannot reject the hypothesis that the policy shifts are the same in majoritarian and proportional countries.

Table 8.8 about here

Majoritarian countries cut spending during election years – though the estimated coefficients are smaller and less precisely estimated than those of the tax cuts (columns 1-2). Here, the election has no effect in proportional countries (if anything spending goes up), and the difference between majoritarian

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[14]The finding of tax cuts in an election year is also in line with the empirical research quoted earlier in this section. But the existing literature typically only estimated the coefficient of a single election dummy variable, not distinguishing between pre-election and post-election cycles (or imposing the restriction that the coefficients are the same but with opposite signs). Thus, to the best of our knowledge, the finding that painful fiscal adjustments tend to be delayed until after the election is new.

and proportional countries is (marginally) significant. The post-election cycle with spending and deficit cuts estimated in the previous subsection, is not perceptibly different across electoral rules, even though the coefficients are more precisely estimated (and only reach statistical significance) in proportional countries (columns 5-6).

The results for welfare-state spending (columns 7-8) are starker. Proportional elections are associated with hikes in welfare-state spending: transfers increase by 0.2% of GDP in the election year, and by almost as much in the post-election year. If anything, this component of spending falls under majoritarian elections, and the difference across electoral rules is highly significant for the pre-election cycle. These results contrast sharply with the cycle in aggregate fiscal variables.

How can these findings be interpreted? On the one hand, majoritarian elections do induce more pronounced cycles in aggregate fiscal policy compared to proportional elections. This is in line with the general idea discussed in Chapter 2 that electoral accountability and incentives to perform well are stronger under plurality rule. Specifically, the pre-election tax *cum* spending cuts in majoritarian countries are consistent with agency models of political cycles, such as Besley and Case (1995) and Persson and Tabellini (2000a). Interestingly, our results for majoritarian countries are similar to Besley and Case's (1995) findings of pre-election tax and spending cuts in US-state executive elections. If anything, the pre-election cycle estimated in proportional countries is more consistent with an opportunistic/traditional political business cycle, a la Rogoff (1990). On the other hand, expansions in welfare-state spending in the proximity of elections are only observed in proportional countries. This finding is thus consistent with the theoretical hypothesis in Chapter 2 that proportional electoral rules induce politicians to seek support among broad coalitions of voters, while majoritarian electoral rules instead induce them to target spending to smaller (geographical) groups, once we assume that these incentives are particularly strong around elections.

Overall, the results in this subsection rhyme well with another general idea from comparative-politics research in political science (also mentioned in the introductory chapter), namely that majoritarian elections is mainly a vehicle for promoting accountability, while proportional elections are mainly a vehicle for promoting representation.

8.5.3   Parliamentary vs. presidential democracies

We next turn to differences in electoral cycles among democracies with different forms of government. In analogy with the approach in the previous subsection, we create four different indicator variables, interacting the election dates with the regime indicator: $EL\_PRE = PRES * ELEX$, $EL\_PAR = (1 - PRES) * ELEX$, and analogously for the lagged election dates. Using these new indicators in the estimation for our four fiscal instruments generates the results displayed in *Table 8.9*.

The results strongly suggest that the post-election cycle in overall government spending, taxes and the surplus identified above is predominantly due to the presidential countries. Governments in presidential regimes cut spending considerably just after the election, by about 0.8% of GDP. They also postpone tax hikes by the same magnitudes, with correspondingly large effects on the surplus, which improves by about 0.75 % of GDP after a typical presidential election. Some post-election spending and deficit adjustments also appear to take place among parliamentary regimes, but these effects are smaller and not statistically significant. The post-election differences between the two regime types are strongest (and highly significant) for taxes and overall spending.

Table 8.9 about here

As already suggested by the split according to electoral rules, systematic pre-election tax cuts are common for all countries. They are stronger and more precisely estimated in the parliamentary regimes, however, where the estimates suggest tax cuts of about 0.5% of GDP in an average election year. The results for welfare-state spending do not indicate pronounced effects anywhere, except perhaps among the better democracies where parliamentary governments raise this component of spending after elections, while presidential governments seem to cut it along with aggregate spending.

The post-election cycles in presidential countries are intriguing and existing theory does not suggest a straightforward explanation. One difference between these two regimes is that the election dates in presidential regimes are generally fixed, while they are subject to choice in most parliamentary countries (Norway and Sweden are among the few exceptions). As mentioned above, however, we deal with the potential simultaneity problem by includ-

ing income shocks in our econometric specification. The difference between the regimes is thus not likely to be a statistical artifact.

The different rules for legislative bargaining discussed in Chapter 2 may provide an interpretation of the post-election cycle. Presidential regimes tend to have more decision makers with proposal and veto rights than parliamentary regimes – for instance, in many countries both the president and the legislature must approve the budget. The possibility of fiscal deadlock might accordingly be more serious, particularly in the case of divided government, i.e., when the president and congress belong to different parties, or when the congress does not have a well-defined majority party. Each decision maker may be able to veto painful adjustments before elections, but none may have the strength to pass deliberate fiscal expansions or tax cuts. In parliamentary democracies, instead, the same majority typically controls the executive and approves the budget, and is thus better able to fine tune fiscal policy to its electoral concerns.[15] Testing this explanation would require careful data collection and coding of the partisan identity of presidents and legislative majorities.

But this is not the only plausible interpretation. Another possibility, also consistent with other results in this chapter, is that presidential countries are more likely to face binding government borrowing constraints. In Section 4, we saw that presidential countries tend to have an acyclical or procyclical, rather than a countercyclical, fiscal policy. If governments in presidential countries do face tighter borrowing constraints, they may also have to undertake more painful fiscal adjustments than parliamentary democracies. It might be optimal to postpone such painful adjustments until after the elections. Indeed, empirical research by Frieden and Stein (2001) has found robust evidence of exchange-rate devaluations tending to be postponed until after the presidential elections in many Latin American countries, where presidential regimes are over-represented. The results in the next section constitute other indirect evidence in favor of this interpretation.

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[15]This reasoning is similar to the idea in the literature on US state fiscal policy that legislative institutions – such as a governor's line-item veto – have more bite on taxes, spending and deficits in situations of divided government, an idea that has received some empirical support. See Besley and Case (2002) for an extensive survey of this literature.

8.5.4   A four-way constitutional split

So far, we have chosen to look for system-dependent electoral cycles in par-simonious specifications, where we only condition on one constitutional dif-ference at a time. While the tests for different cycles are valid under the nul hypothesis of no differences, the reader may legitimately ask whether both specifications can be true at the same time, to the extent that we find differences across constitutional features. The answer is probably in the neg-ative: even under our implicit assumption that any constitutional differences are additive, the estimates will still be biased if the frequency of the omitted constitutional feature, say the form of government, differs across the included feature, say the electoral rule. The likely culprit here is that our sample in-cludes few elections in presidential countries with majoritarian electoral rules. For a total of 518 election dates in our panel, only 24 are thus associated with these constitutional features, whereas the other three types are much better represented (for presidential countries with proportional elections, we have 131 elections, while for parliamentary-majoritarian and parliamentary-proportional, we have 139 and 213 elections, respectively). This means that our estimates of the cycle under majoritarian elections above may be biased in the direction of the cycle found for parliamentary countries (if different from the presidential cycle). Conversely, estimates of the cycle in presidential countries may be biased in the direction of the cycle found for proportional elections (if different from the majoritarian cycle).

To address this issue and further understand the source of our results, we condition the electoral-cycle estimates on four separate constitutional groups (labeled EL_MAJPRE, and so on, in the obvious notation). *Table 8.10* shows the estimates of pre-and post-election cycles in these four groups.

Table 8.10 about here

Key findings in *Table 8.8* are the unique pre-election spending cuts and stronger pre-election tax cuts under majoritarian elections. Are these driven by the higher frequency of parliamentary countries and the regime differ-ences found in *Table 8.9*, as the above discussion suggests might be the case? The results in *Table 8.10* indicate the answer to be no (see the upper part of columns 1-4). The coefficients show that election-year spending and tax cuts are present in both the presidential (called EL_MAJPRE) and the parliamentary (called EL_MAJPAR) subgroups of majoritarian countries.

Moreover, the cuts are larger among the majoritarian-presidential democracies for all specifications. A more balanced sample (with more presidential countries) would thus have produced even larger estimates (in absolute value) in *Table 8.8*.

Another key result in *Table 8.8* is the finding of electoral cycles in welfare-state spending being uniquely associated with proportional electoral rules. The estimates in *Table 8.10* (columns 7-8) show the results for the pre-election cycle to reflect hikes in the parliamentary and presidential subgroups alike. But post-election hikes in welfare spending are exclusively found among proportional-parliamentary countries, a group including many of the European welfare states.

The key finding in *Table 8.9* is the uniqueness of the post-election fiscal adjustment to presidential democracies. Here, the results in *Table 8.10* (the lower part of columns 1-6) do indeed suggest that the results are driven by the higher frequency of proportional-presidential than majoritarian-presidential democracies. While the post-election fiscal adjustments go in the same direction in both these groups, they are always larger in the proportional-presidential subgroup. Since this group is predominant in Latin-America, the results give some indirect support for an interpretation in terms of borrowing constraints, as offered at the end of the previous subsection.

8.5.5   Summary

We have uncovered strong constitutional effects on the presence and nature of electoral cycles in fiscal policy. True, governments in all countries appear to cut taxes in the election year. But only presidential regimes postpone unpopular fiscal-policy adjustments until after the elections. Only governments in majoritarian countries cut spending during election years. And only proportional democracies raise welfare spending around the time of the election, with further commitments for the post-election year.

8.6   Concluding remarks

In Chapter 6, we exploited the variation in fiscal policy across countries to draw inferences about constitutional effects. There, we found presidential and majoritarian systems to have smaller governments, as compared to parliamentary and proportional systems; moreover, majoritarian elections

induce smaller welfare states and budget deficits than proportional elections. This chapter has exploited the variation in fiscal policy across time. Our findings here shed further light on the earlier conclusions, and the mechanisms through which the constitution might shape fiscal policy.

Proportional *cum* parliamentary democracies differ from other constitutional groups in several respects. First, fiscal policy is much more persistent in this group than in the others. Second, this is the only group of countries where we find a ratchet effect on spending: downturns lead to a lasting expansion of outlays and welfare spending in proportion to GDP, which is not undone during upturns. Third, in this group of countries, welfare-state programs expand more in the proximity of elections than in other years. Fourth, the difference in the size of government between this group and the others grew particularly large in the period up to the early 1980s (the early 1990s in the case of welfare spending), in response to some unobserved events leading to a generalized increase of government outlays everywhere. These features of proportional-parliamentary systems all contribute to explain why they have much larger governments than other constitutional groups in the 1990s.

Presidential regimes also stand out in some important respects. The procyclical response of fiscal policy and the procrastination over postponing painful fiscal adjustments are peculiar to this group, A possible explanation is that presidential regimes are more likely to face tight borrowing constraints. Fiscal policy is also least persistent among presidential countries. Countries with majoritarian elections share some of these features, although not to the same extent; they are also unique in cutting not only taxes, but also overall spending during election years.

In many ways, the findings in this chapter are more preliminary than those reported in Chapter 6 (and 7). Much more remains to be done to exploit the observed variation in the data. The dynamic interaction between fiscal policy and the business cycle could be more carefully studied, also allowing for a (contemporaneous or delayed) impact of policy on the state of the economy, as in a panel VAR. The findings on electoral cycles suggest that it may be worth digging deeper into the institutional details by studying e.g., the effect of time limits for presidential terms, or the specific rules for breaking up the government and calling new elections in parliamentary regimes. Interesting sources of time variation in the data that might interact with the constitution have not been exploited in this chapter, such as swings of the executive from the left to the right, or changes in the quality of a democracy. For such empirical efforts to be fruitful, the existing theory should be extended so as

to generate more precise empirical predictions. We leave it to future research to pursue these interesting questions.

Figure 8.1
Unobserved common events and the size of government
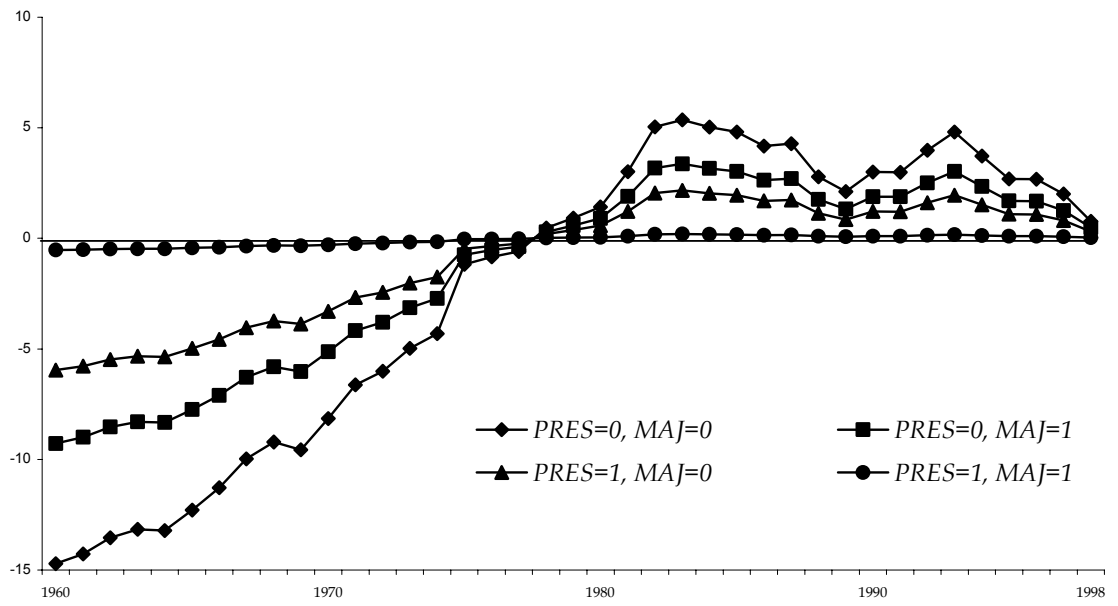
Figure 8.2
Response of government spending to a +1% output gap
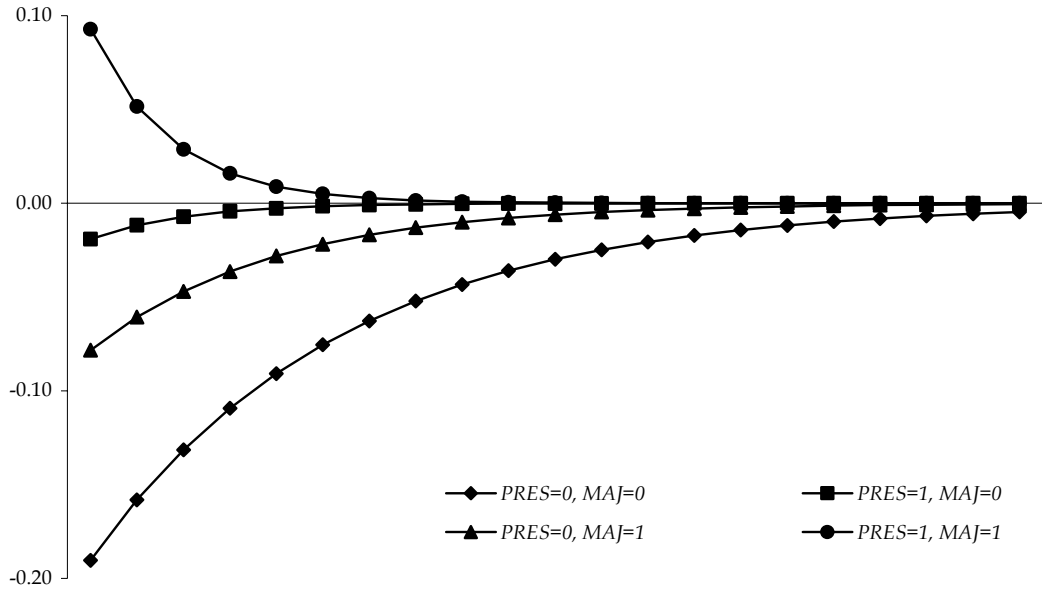
Figure 8.3a
Response of government spending to a positive 1% output gap
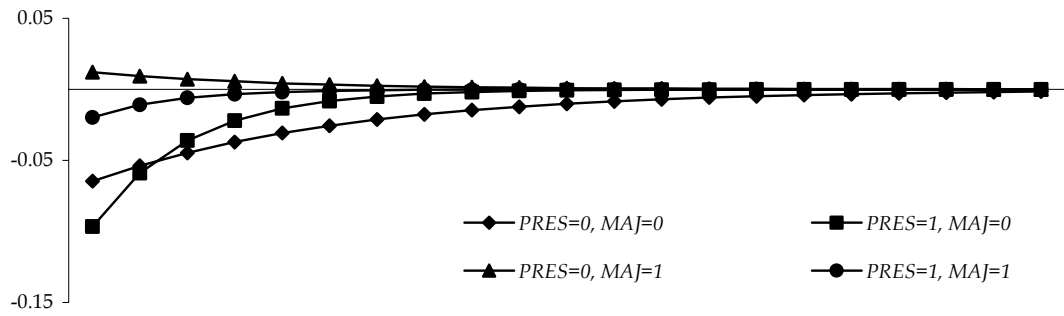
Figure 8.3b
Response of government spending to a negative -1% output gap
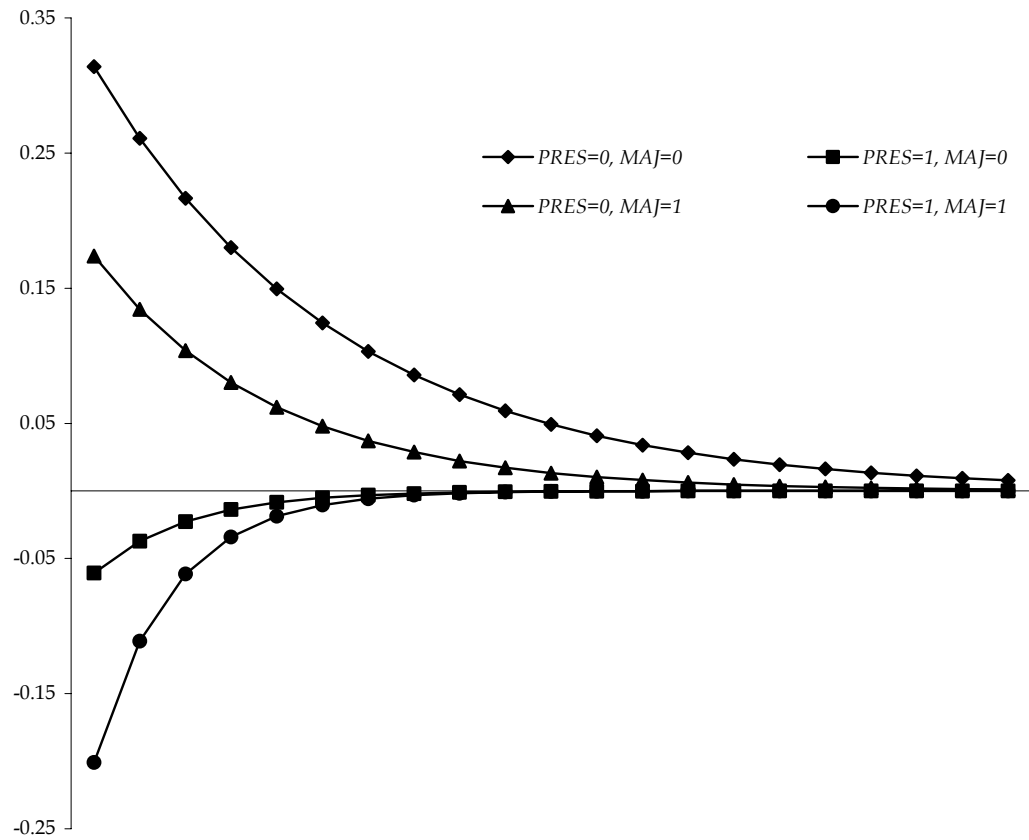
Table 8.1
Unobserved common events and the size of government

	(1)	(2)	(3)	(4)
Dep. var.	CGEXP	CGEXP	CGEXP	CGEXP
LCGEXP		0.79	0.84	0.86
		(0.02)***	(0.02)***	(0.02)***
PRES*LCGEXP			-0.19	-0.19
			(0.04)***	(0.05)***
MAJ*LCGEXP			-0.09	-0.04
			(0.03)***	(0.02)
PRES	-0.59	-0.42	-0.19	
	(0.04)***	(0.13)***	(0.18)	
MAJ	-0.37	-0.23	0.03	
	(0.04)***	(0.12)*	(0.18)	
LAAM*LCGEXP				0.01
				(0.04)
COLUK*LCGEXP				-0.05
				(0.03)*
Estimation	NLS FE	NLS FE	NLS FE	OLS FE
Obs.	1594	1550	1550	1550
Adj. R2	0.86	0.95	0.95	0.82

Standard errors in brackets
* significant at 10%, ** significant at 5%, *** significant at 1%
Other controls always included: *TRADE, LYP, PROP65, PROP1564*, country fixed effects
In column (4), Adj. R2 refers to within-R2

Table 8.2
Unobserved common events and welfare spending

	(1)	(2)	(3)	(4)
Dep. var.	*SSW*	*SSW*	*SSW*	*SSW*
LSSW		0.82	0.81	0.81
		(0.02)***	(0.02)***	(0.02)***
*PRES*LSSW*			0.04	
			(0.04)	
*MAJ*LSSW*			-0.01	
			(0.03)	
PRES	-0.52	-0.33	-0.36	-0.45
	(0.05)***	(0.18)**	(0.17)**	(0.16)***
MAJ	-0.17	-0.37	-0.35	-0.05
	(0.05)***	(0.15)**	(0.16)**	(0.13)
LAAM				-0.13
				(0.18)
COLUK				-0.03
				(0.00)***
Estimation	*NLS FE*	*NLS FE*	*NLS FE*	*NLS FE*
Obs.	1000	942	942	942
Adj. R2	0.96	0.99	0.99	0.99

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls always included, *TRADE; YP, PROP65, PROP1564*, country fixed effects

Table 8.3
Unobserved common events and the budget surplus

	(1)	(2)	(3)	(4)	(5)
Dep. var.	SPL	SPL	SPL	SPL	SPL
LSPL		0.70	0.77	0.70	0.79
		(0.02)***	(0.03)***	(0.02)***	(0.03)***
PRES*LSPL			-0.29		-0.18
			(0.05)***		(0.06)***
MAJ*LSPL			-0.03		0.01
			(0.04)		(0.04)
PRES	-0.44	-0.53	-0.40	-0.08	
	(0.09)***	(0.19)***	(0.21)*	(0.39)	
MAJ	-0.17	0.33	0.48	0.23	
	(0.09)*	(0.23)	(0.27)*	(0.31)	
LAAM				-0.25	
				(0.41)	
COLUK				0.04	
				(0.01)**	
LAAM*LSPL					-0.19
					(0.06)***
COLUK*LSPL					-0.03
					(0.04)
Estimation	NLS FE	NLS FE	NLS FE	NLS FE	OLS FE
Obs.	1561	1515	1515	1474	1515
Adj. R2	0.45	0.72	0.73	0.72	0.58

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls always included: *TRADE, LYP, PROP65, PROP1564*, country fixed effects
Adj. R2 in column 5 refers to within-R2

Table 8.4
Cyclical response of government spending to output gaps

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	CGEXP	CGEXP	CGEXP	CGEXP	CGEXP	DCGEXP
LCGEXP	0.83	0.83	0.83	0.83	0.83	
	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	
PRES*LCGEXP	-0.22	-0.22	-0.30	-0.22	-0.22	
	(0.04)***	(0.04)***	(0.04)***	(0.03)***	(0.04)***	
MAJ*LCGEXP	-0.06	-0.04	-0.05	-0.06	-0.06	
	(0.02)**	(0.03)	(0.02)*	(0.02)**	(0.02)**	
YGAP	-0.19	-0.17	-0.19		-0.27	-0.33
	(0.06)***	(0.08)**	(0.06)***		(0.07)***	(0.04)***
PRES*YGAP	0.17	0.21	0.11		0.13	0.31
	(0.08)**	(0.12)*	(0.10)		(0.11)	(0.05)***
MAJ*YGAP	0.11	0.02	0.12		0.03	0.02
	(0.08)	(0.11)	(0.09)		(0.09)	(0.04)
POSYG				-0.06		
				(0.11)		
PRES*POSYG				-0.03		
				(0.16)		
MAJ*POSYG				0.08		
				(0.15)		
NEGYG				-0.31		
				(0.11)***		
PRES*NEGYG				0.37		
				(0.16)**		
MAJ*NEGYG				0.14		
				(0.15)		
LAAM*YGAP					0.15	0.12
					(0.10)	(0.04)***
COLUK*YGAP					0.18	0.19
					(0.09)**	(0.05)***
Sample	|yshock| < 5	|yshock| < 3	|yshock| < 5, narrow	|yshock| < 5	|yshock| < 5	|yshock| < 5
Estimation	CTRY FE	CTRY FE	CTRY FE	CTRY FE	CTRY FE	DIFF, GLS
Obs.	1452	1283	1201	1452	1452	1448
Countries	60	60	54	60	60	59
Adj. R2	0.83	0.83	0.84	0.83	0.83	

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls always included: *LYP, TRADE, PROP1564, PROP65, OIL_IM, OIL_EX*
Narrow sample corresponds to countries where *POLITY_GT* is less than 1.1
Adj. R2 refers to within-R2

Table 8.5
Cyclical response of welfare spending to output gaps

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	*SSW*	*SSW*	*SSW*	*SSW*	*SSW*	*DSSW*
LSSW	0.79	0.75	0.79	0.80	0.79	
	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	
*PRES*LSSW*	0.03	0.05	0.02	0.03	0.03	
	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	
*MAJ*LSSW*	-0.03	-0.01	-0.03	-0.03	-0.03	
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
YGAP	-0.13	-0.13	-0.15		-0.16	-0.11
	(0.02)***	(0.03)***	(0.02)***		(0.03)***	(0.01)***
*PRES*YGAP*	0.07	0.11	0.05		0.06	0.07
	(0.03)**	(0.04)**	(0.04)		(0.04)	(0.01)***
*MAJ*YGAP*	0.07	0.03	0.09		0.04	0.03
	(0.03)**	(0.04)	(0.03)***		(0.03)	(0.01)***
POSYG				-0.05		
				(0.04)		
*PRES*POSYG*				-0.02		
				(0.06)		
*MAJ*POSYG*				0.02		
				(0.05)		
NEGYG				-0.20		
				(0.04)***		
*PRES*NEGYG*				0.15		
				(0.06)**		
*MAJ*NEGYG*				0.12		
				(0.06)**		
*LAAM*YGAP*					0.05	0.05
					(0.04)	(0.01)***
*COLUK*YGAP*					0.07	0.07
					(0.03)**	(0.01)***
Sample	|yshock| < 5	|yshock| < 3	|yshock| < 5, narrow	|yshock| < 5	|yshock| < 5	|yshock| < 5
Estimation	*CTRY FE*	*CTRY FE*	*CTRY FE*	*CTRY FE*	*CTRY FE*	*DIFF, GLS*
Obs.	890	779	752	890	890	830
Countries	56	56	49	56	56	55
Adj. R2	0.77	0.77	0.78	0.77	0.77	

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Other controls always included: *LYP, TRADE, PROP1564, PROP65, OIL_IM, OIL_EX*, country fixed effects
Narrow sample corresponds to countries where *POLITY_GT* is less than 1.1
Adj. R2 refers to within-R2

Table 8.6
Cyclical response of budget surplus and government revenue to output gaps

	(1)	(2)	(3)	(4)	(5	(6)
Dep. var.	*SPL*	*SPL*	*SPL*	*DSPL*	*CGREV*	*CGREV*
LDEPVAR	0.72	0.73	0.75		0.81	0.80
	(0.02)***	(0.02)***	(0.03)***		(0.01)***	(0.01)***
*PRES*LDEPVAR*	-0.32	-0.33	-0.34		-0.21	-0.20
	(0.04)***	(0.04)***	(0.05)***		(0.03)***	(0.03)***
*MAJ*LDEPVAR*	0.05	0.05	0.03		-0.03	-0.02
	(0.03)	(0.03)	(0.04)		(0.02)	(0.02)
YGAP	0.08		0.21	0.20	-0.11	
	(0.05)		(0.06)***	(0.04)***	(0.05)**	
*PRES*YGAP*	-0.13		-0.11	-0.16	0.02	
	(0.07)*		(0.10)	(0.04)***	(0.07)	
*MAJ*YGAP*	0.05		0.19	0.14	0.13	
	(0.07)		(0.08)**	(0.04)***	(0.07)**	
POSYG		-0.03				-0.02
		(0.10)				(0.10)
*PRES*POSYG*		-0.21				-0.05
		(0.15)				(0.14)
*MAJ*POSYG*		0.09				0.00
		(0.13)				(0.12)
NEGYG		0.18				-0.19
		(0.10)*				(0.09)**
*PRES*NEGYG*		-0.03				0.08
		(0.15)				(0.14)
*MAJ*NEGYG*		0.03				0.27
		(0.14)				(0.13)**
*LAAM*YGAP*			-0.18	-0.01		
			(0.09)**	(0.04)		
*COLUK*YGAP*			-0.32	-0.25		
			(0.08)***	(0.05)***		
Sample	|yshock| < 5	|yshock| < 5	|yshock| < 5	|yshock| < 5	|yshock| < 5	|yshock| < 5
Estimation	*SUR, CTRY FE*	*SUR, CTRY FE*	*CTRY FE*	*DIFF, GLS*	*SUR, CTRY FE*	*SUR, CTRY FE*
Obs.	1352	1352	1427	1422	1352	1352
Countries	59	59	60	59	59	59
Adj. R2	0.74	0.74	0.56		0.96	0.96

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
SUR run on system of 3 equations (*CGREV, DFT_SPL* and (not shown) *CGEXP*)
Other controls always included*: LYP, TRADE, PROP1564, PROP65, OIL_IM, OIL_EX*, country fixed effects
Adj. R2 is unadjusted for columns 1-2 and 5-6, within-R2 for column 3

Table 8.7
Electoral cycles in fiscal policy
Executive elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	*CGEXP*	*CGEXP*	*CGREV*	*CGREV*	*SPL*	*SPL*	*SSW*	*SSW*
ELEX	-0.01	0.03	-0.40	-0.40	-0.19	-0.16	0.07	0.07
	(0.16)	(0.18)	(0.14)***	(0.16)**	(0.14)	(0.16)	(0.06)	(0.07)
LELEX	-0.31	-0.26	0.20	0.29	0.38	0.38	0.05	0.06
	(0.16)**	(0.18)	(0.14)	(0.16)*	(0.14)***	(0.15)**	(0.06)	(0.07)
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Obs.	1521	1248	1472	1210	1495	1217	931	785
Countries	60	55	59	55	60	55	56	49
Adj. R2	0.83	0.84	0.83	0.84	0.58	0.61	0.80	0.81

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include fixed country and year effects and the following covariates: *LYP*; *TRADE*;
PROP1564; PROP65; YGAP, alone and interacted with *MAJ* and *PRES*; lagged dependent variable,
alone and interacted with *PRES* and *MAJ*
Narrow sample corresponds to countries and years where *POLITY_GT* is less than 1.1
Adj. R2 refers to within R2

Table 8.8
Electoral cycles in fiscal policy
Alternative electoral rules

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	CGEXP	CGEXP	CGREV	CGREV	SPL	SPL	SSW	SSW
EL_MAJ	-0.40	-0.42	-0.57	-0.52	-0.20	-0.15	-0.11	-0.17
	(0.28)	(0.31)	(0.24)**	(0.27)*	(0.25)	(0.27)	(0.10)	(0.11)
EL_PRO	0.20	0.27	-0.30	-0.33	-0.18	-0.16	0.17	0.21
	(0.20)	(0.22)	(0.18)*	(0.20)*	(0.18)	(0.19)	(0.08)**	(0.08)**
LEL_MAJ	-0.21	-0.15	0.14	0.28	0.27	0.37	-0.05	-0.07
	(0.28)	(0.31)	(0.24)	(0.27)	(0.25)	(0.26)	(0.10)	(0.11)
LEL_PRO	-0.36	-0.32	0.23	0.29	0.44	0.40	0.11	0.14
	(0.20)*	(0.22)	(0.17)	(0.20)	(0.18)**	(0.19)**	(0.08)	(0.09)
F: MAJ=PRO	3.05*	3.19*	0.80	0.35	0.00	0.00	4.98**	6.88***
F: LMAJ=LPRO	0.20	0.21	0.09	0.00	0.33	0.01	1.62	2.13
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Obs.	1521	1248	1472	1210	1495	1217	931	785
Countries	60	55	59	55	60	55	56	49
Adj. R2	0.83	0.84	0.83	0.84	0.58	0.61	0.80	0.81

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include fixed country and year effects and the following covariates: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *YGAP*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable, alone and interacted with *MAJ* and *PRES*.
F: MAJ=PRO refers to the test statistic for equal coefficients on *EL_MAJ* and *EL_PRO*
Narrow sample corresponds to countries and years where *POLITY_GT* is less than 1.1
Adj. R2 refers to within-R2

Table 8.9
Electoral cycles in fiscal policy
Alternative forms of government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	*CGEXP*	*CGEXP*	*CGREV*	*CGREV*	*SPL*	*SPL*	*SSW*	*SSW*
EL_PRE	-0.23	-0.17	-0.26	-0.03	-0.21	-0.09	0.07	0.08
	(0.32)	(0.43)	(0.28)	(0.37)	(0.28)	(0.36)	(0.13)	(0.16)
EL_PAR	0.08	0.08	-0.45	-0.48	-0.18	-0.18	0.07	0.07
	(0.19)	(0.20)	(0.16)***	(0.18)***	(0.17)	(0.17)	(0.07)	(0.08)
LEL_PRE	-0.76	-0.93	0.53	1.01	0.69	0.82	-0.10	-0.17
	(0.32)**	(0.41)**	(0.28)*	(0.36)***	(0.28)**	(0.35)**	(0.12)	(0.16)
LEL_PAR	-0.14	-0.10	0.08	0.12	0.27	0.28	0.10	0.12
	(0.19)	(0.20)	(0.16)	(0.17)	(0.17)	(0.17)	(0.07)	(0.08)
F: PRE=PAR	0.69	0.27	0.37	1.19	0.01	0.06	0.00	0.00
F: LPRE=LPAR	2.75*	3.21*	1.88	4.84**	1.69	1.88	1.90	2.72*
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Obs.	1521	1248	1472	1210	1495	1217	931	785
Countries	60	55	59	55	60	55	56	49
Adj. R2	0.83	0.84	0.83	0.84	0.58	0.61	0.80	0.81

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include fixed country and year effects and the following covariates: *LYP*; *TRADE*; *PROP1564*; *PROP65*;
YGAP, alone and interacted with *MAJ* and *PRES*; lagged dependent variable, alone and interacted with *MAJ* and *PRES*.
F: PRE=PAR refers to the test statistic for equal coefficients on *EL_PRE* and *EL_PAR*
Narrow sample corresponds to countries and years where *POLITY_GT* is less than 1.1
Adj. R2 refers to within R2

Table 8.10
Electoral cycles in fiscal policy
Alternative constitutional groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	*CGEXP*	*CGEXP*	*CGREV*	*CGREV*	*SPL*	*SPL*	*SSW*	*SSW*
EL_MAJPRE	-1.04	-0.82	-1.11	-0.73	0.19	0.22	0.00	-0.08
	(0.70)	(0.80)	(0.59)*	(0.68)	(0.61)	(0.68)	(0.26)	(0.28)
EL_PROPRE	0.00	0.10	-0.00	0.26	-0.33	-0.21	0.10	0.15
	(0.36)	(0.50)	(0.32)	(0.45)	(0.31)	(0.43)	(0.15)	(0.19)
EL_MAJPAR	-0.27	-0.34	-0.48	-0.50	-0.28	-0.23	-0.13	-0.18
	(0.31)	(0.33)	(0.26)*	(0.29)*	(0.27)	(0.29)	(0.11)	(0.12)
EL_PROPAR	0.30	0.31	-0.44	-0.48	-0.12	-0.16	0.20	0.22
	(0.24)	(0.25)	(0.21)**	(0.22)**	(0.22)	(0.22)	(0.09)**	(0.09)**
LEL_MAJPRE	-0.32	-0.27	0.27	0.47	0.50	0.82	-0.09	-0.07
	(0.71)	(0.78)	(0.59)	(0.68)	(0.63)	(0.67)	(0.27)	(0.28)
LEL_PROPRE	-0.87	-1.22	0.61	1.24	0.74	0.83	-0.09	-0.21
	(0.36)**	(0.50)**	(0.32)*	(0.44)***	(0.31)**	(0.43)*	(0.14)	(0.19)
LEL_MAJPAR	-0.18	-0.12	0.11	0.23	0.22	0.28	-0.03	-0.07
	(0.31)	(0.34)	(0.26)	(0.29)	(0.27)	(0.29)	(0.11)	(0.12)
LEL_PROPAR	-0.12	-0.09	0.06	0.05	0.31	0.28	0.19	0.22
	(0.24)	(0.25)	(0.21)	(0.22)	(0.21)	(0.21)	(0.09)**	(0.09)**
Sample	Broad	Narrow	Broad	Narrow	Broad	Narrow	Broad	Narrow
Obs.	1521	1248	1472	1210	1493	1215	931	785
Countries	60	55	59	55	60	55	56	49
Adj. R2	0.83	0.85	0.83	0.84	0.58	0.61	0.80	0.81

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include fixed country and year effects and the following covariates: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *YGAP*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable, alone and interacted with *PRES* and *MAJ*.
Narrow sample corresponds to countries and years where *POLITY_GT* is less than 1.1
Adj. R2 refers to within R2

Chapter 9

What have we learned?

It is time to take stock of our findings. We start by summarizing the empirical results uncovered in Chapters 6 through 8. The summary provokes a discussion of what lessons one may learn from these results, first for the electoral rule, then for the form of government. Which theoretical ideas are supported by the data? Which ought to be re-formulated? What are the main puzzles? On the basis of this discussion, we then close the chapter and the book by outlining some directions for future research.

9.1 Theoretical priors and empirical results

In Chapter 2, we recapitulated the priors from existing theory in a list of questions to be posed to the data. That list becomes a useful checklist when accounting for the results of our empirical investigation. The columns in *Table 9.1* headed "Theory" thus reproduce the contents of *Table 2.1* and summarizes the predicted constitutional effects of changing the electoral rule from proportional to majoritarian, or the form of government from parliamentary to presidential. The columns headed "Data", show a bold attempt of succinctly summing up our empirical findings. Here, a "0" means that no significant constitutional effect was found, while a "+" or a "–" indicates the qualitative direction of a statistically significant effect. Naturally, distilling the many dimensions and edges of our quantitative findings into this simple scheme makes it necessary to cut some corners. But as the theory only provides qualitative predictions, the table provides a useful perspective on the mapping from priors to posteriors suggested by the data.

9.1.1   Electoral rules

One of the central findings in this book is the strong constitutional effect of electoral rules on fiscal policy. Existing theoretical arguments, emphasizing different aspects of electoral rules, predict that majoritarian elections induce smaller welfare states than proportional elections; some, but not all, theories also predict smaller governments and smaller deficits. In the data, we find that welfare states are indeed smaller in majoritarian countries; so are overall government spending and deficits, which sharpens our fuzzy theoretical priors. According to the cross-sectional evidence in Chapter 6, a switch from proportional to majoritarian elections reduces overall government spending by almost 5% of GDP, welfare spending by 2-3% of GDP, and budget deficits by about 2% of GDP. Advocates of the opposite switch in the UK, from majoritarian to proportional, should take a careful note. The electoral rule emerges from this research as one of the primary determinants of fiscal policy in modern democracies. According to our results, electoral reform in the UK would make its a public sector more similar in size to that in continental Europe.

A related finding in Chapter 8 concerns the response of spending to common unobserved events: the worldwide growth of welfare-state spending and total government spending in the 1970s and 1980s was much more pronounced in proportional than in majoritarian countries. The cumulative effect of the different growth profiles amounts to almost 5% of GDP for total government spending and about 2% of GDP for welfare spending, numbers remarkably similar to the cross-sectional effect. The panel data analysis does not point to a large impact of electoral rules on the cyclical reaction of fiscal policy, nor on its degree of persistence – but there, we did not have any meaningful theoretical priors.

Some theoretical models also suggest a stronger electoral cycle under majoritarian elections, as politicians face sharper individual incentives to please their constituencies than under proportional elections. In Chapter 8, we do find that electoral cycles vary with the electoral rule in a subtle pattern. The findings do not contradict our theoretical priors, but they contain some unexpected elements. On the one hand, majoritarian countries alone cut not only taxes, but also spending, ahead of the elections, by as much as 0.5% of GDP. An interpretation of this finding is that incumbent governments under majoritarian rule want to appear less wasteful in the eyes of voters, as suggested by agency theories of politics. On the other hand, proportional countries

alone expand welfare programs in election years, by something like 0.2% of GDP (about 2.5% of program size in the average country of our sample). A possible interpretation is that incumbent governments under proportional rule have strong incentives to seek re-election support from broad coalitions of voters, and that these incentives are at their peak just before elections. As we observe an additional expansion of these programs in the post-election year, some garnering of votes may take the form of promises in electoral platforms, rather than expansions before elections.

In the case of political rents and corruption, we expected the fine details of electoral rules to influence outcomes, but not necessarily the coarse distinction between majoritarian and proportional rule. Our empirical findings in Chapter 7 are in line with the theoretical predictions. Direct individual accountability via the ballot structure reduces both corruption and government ineffectiveness, as expected. Small electoral districts do the opposite, in line with the idea that barriers to entry are higher in single-member districts. Both effects are statistically robust and quantitatively significant. Since these two dimensions of the electoral rule co-vary, the net effect of a comprehensive reform towards majoritarian elections on rent extraction is ambiguous, even though the effect of individual accountability seems to (weakly) prevail in the data.

When it comes to growth-promoting policies and productivity, we did not have much of a theoretical prior. The empirically estimated constitutional effects in Chapter 7 are similar to those for rent extraction. Larger electoral districts and more direct individual accountability both promote higher productivity through policies that better protect private property rights. But the crude classification into majoritarian vs. proportional elections has no robust effect on these variables. While there is some evidence of a directive negative effect of the majoritarian elections on labor productivity, this constitutional effect is not robust to selection bias.

Table 9.1 about here

These findings support a general idea in the political-science literature on comparative politics: the design of electoral rules entails a trade-off between accountability and representation. Aspects of this general idea also appear in recent theoretical studies in political economics. Majoritarian elections and, in particular, plurality rule, make the electoral outcome more sensitive to marginal changes in the distribution of votes. On the one hand, this

creates stronger incentives for politicians not to use their office for private gain (reduce rents and corruption). On the other hand, marginal groups of voters may be targeted in electoral platforms or overrepresented in the assembly, so that narrow programs benefiting these voters may crowd out broad programs benefiting larger groups of citizens, such as welfare-state spending and general public goods. The general idea of a trade-off between accountability and representation is both intuitive and theoretically robust. Now, we can add that the trade-off shows up in observed policies, given our empirical findings on political rents, the character of electoral cycles, and the size of broad welfare-state programs.

But perhaps the terms of this trade-off can be made more favorable by specific reforms. A lesson suggested by our results is that any real-world electoral reform should pay attention to the finer details of the electoral system. The accountability effects of majoritarian systems seem to be directly related to ballot structures and plurality rule, as well as to the size of electoral districts. Voting over individuals in two or three-member districts, as in Chile and Mauritius, might be a way of reaping the benefits of plurality rule and individual accountability, without erecting too high barriers for entry in the electoral process. Such hybrid systems might present an interesting alternative to the mixed-member systems introduced by a number of countries in the 1990s (cf. Chapter 4).

Another theoretical idea, indirectly supported by our evidence, is that majoritarian elections may help resolve the "common pool" problem in fiscal policy. A robust finding in the comparative-politics literature cited in Chapter 2 is that PR promotes coalition governments. The theoretical literature on political economics has suggested that such governments have a hard time controlling government spending and budget deficits, because of inefficient bargaining inside the coalition. The idea may not be as fully fleshed out in formal terms as the accountability/representation trade-off, but it does suggest that proportional electoral rule induces both larger government spending and larger budget deficits. This is precisely what we uncover in the data, with the large and robust constitutional effects estimated in Chapter 6.

Another idea about coalition governments is that they are more prone to a status-quo bias, because of their greater number of veto players. Hence, their reaction to adverse economic shocks is more likely to be inefficient. The findings in Chapter 8 on the cyclical response of fiscal policy lend some indirect support to this idea. According to the data, governments elected under majoritarian rule seem to react to cyclical downturns by cutting taxes

(in consistency with Keynesian stabilization policies). Governments elected under proportional rule, on the other hand, are more likely to let spending rise (as a percent of GDP) during downturns, but are unable to scale it down during upturns – this is the ratchet effect uncovered in Chapter 8.

9.1.2 Forms of government

The theory of policy choices under different forms of government is less developed than that on electoral rules. But a central theoretical prediction is that presidential countries are less plagued by political rent extraction (corruption) than parliamentary countries, at the expenses of less public-good provision and smaller transfers to broad population groups. Another strong prediction is a smaller overall size of government in presidential regimes. No clear-cut predictions are available for the other outcomes listed in *Table 9.1* (the budget deficit, the dynamic and cyclical response of policy, electoral cycles, structural policy and economic performance).

With regard to the size of government, the data strongly support the predictions. According to the cross-sectional estimates in Chapter 6, presidentialism reduces the overall size of government at least as much as majoritarian elections, by about 5% of GDP. The interaction effects uncovered by our panel-data analysis in Chapter 8 suggest even larger differences. Indeed, much of the difference in the size of government across regimes can be traced back to a less rapid growth of government in presidential regimes during the 1970s and 1980s. Compared to parliamentary regimes, government spending in presidential democracies is also much less persistent, with a more dampened response to common unobserved events. Moreover, the ratchet effect on government spending in response to cyclical fluctuations, which we observe in proportional-parliamentary democracies, is certainly not a feature of presidential democracies.

Unconditionally, presidential democracies do have lower welfare spending than parliamentary democracies, in line with our prior, as well as smaller deficits. But here, the constitutional effects estimated in Chapter 6 are less robust and it is difficult to separately identify the constitutional effect from that of other policy determinants: smaller welfare spending can be attributed to younger populations, and smaller budget deficits could result from tighter borrowing constraints in more unstable and crisis-prone societies, rather than from institutionally induced policy preferences.

An electoral fiscal-policy cycle in presidential countries is evident from

our results in Chapter 8. But it takes a peculiar, post-electoral form, quite different from the cycle observed in parliamentary democracies: spending cuts and fiscal contractions by as much as 1% of GDP are postponed until an incumbent president has survived, or a newly elected president has been installed.

According to the empirical results of Chapter 7, and contrary to the predictions of the theory, perceptions of corruption and government ineffectiveness are not generally higher under parliamentary forms of government. Moreover, presidential regimes are associated with significantly worse economic performance, due to worse structural policies, where the legal infrastructure is less respectful of property rights and less likely to enforce government contracts. This effect is quantitatively significant: our estimates in Chapter 7 suggest that an adoption of a presidential regime in Spain would eliminate the country's lead over Greece in structural policy and productivity. Both effects seems to interact with the quality of democratic institutions, however: a negative effect of presidentialism on corruption seems to be present among better democracies, while the negative effect of presidentialism on productivity and growth-promoting policies appears to be much stronger among worse democracies.

The theory discussed in Chapter 2 suggests an analogy between the constitutional choices associated with electoral rules and forms of government. Although the reasons are somewhat different, both imply a trade-off between accountability and representation. For the form of government, this trade-off is not apparent in the data, however. We obtain robust support for one prediction (presidential regimes have smaller governments than parliamentary regimes), but our results for the central predictions regarding rent extraction and welfare programs are much more fragile.

A possible reason for this inconsistency, is that theory relies on two features, which are not well captured empirically by a single binary classification. In the theory surveyed in Chapter 2, a presidential democracy has two features: the executive is not accountable to the legislature through a confidence requirement, and institutional checks and balances induce effective separation of powers between the executive and the legislature, or between different congressional committees. Our empirical classification is based on the first dimension (lack of a confidence requirement on the executive), neglecting the separation of powers aspect.

As noted several times in the book, presidential regimes are over-represented in Latin America and among more dubious, or at least younger, democracies.

Thus, they are less likely to have effective checks and balances, not only due to imperfect political institutions, but also because the media are less likely to be independent and the respect for democratic traditions is less deeply entrenched. Presidential states typically have stronger executives than parliamentary states. If bad democracies have fewer checks and balances, the resulting concentration of political power could lead to harmful policies. At the other side of the coin, a good democracy may be needed for a presidential regime to restrain the abuse of political power. Our preliminary results in Chapter 7, that presidentialism possibly restrains corruption among the best democracies while it particularly harms economic performance among the worst democracies, give some, indirect support to this interpretation. They also suggest that presidentialism could lead to overall better policies in consolidated and solid democracies, but not in more precarious democratic situations. A more direct way to address this interpretation is to collect more data, along the lines suggested in Chapter 4, trying to document the dispersion in the separation of powers across countries. This observation takes us right into the agenda for future research discussed in the next section.

9.2   What next?

The comparison between theoretical priors and empirical findings in *Table 9.1* is certainly encouraging. Several of the empirical regularities discovered in the book are in line with the first wave of theory. The constitutional effects on fiscal policy and political rents found in the data match up strikingly well with theory, particularly for the electoral rule.

But in many ways, the state of our knowledge is still very preliminary. The theoretical models motivating our empirical investigation are only a first step. And the constitutional effects uncovered in this book concern reduced forms in the data – from constitutional rules to policy outcomes. A first-order priority in the next wave of research, theoretical and empirical, should be to gain a better understanding of the detailed mechanisms through which the constitution influences policy. Making progress on this task would also help to build a stronger bridge between the existing research in economics and political science.

Consider the electoral rule, for example. Existing theories formulated by economists have mainly focused on how the electoral rule shapes electoral competition or electoral accountability, mainly in a two-party system,

and how this, in turn, affects policy outcomes. This way of formulating the comparative-politics problem neglects the links from the electoral system to party structure, from party structure to government formation and legislative bargaining, and from these political outcomes onto policy formation. As mentioned already in Chapters 1 and 2, political scientists have studied each of these links as a separate phenomenon. Understanding the relative importance of the direct effects, via policymaking incentives for given political outcomes, and the indirect effects, via altered political outcomes, requires a more encompassing approach.

Bridging the gap between the economics and political science research on electoral rules, constitutes an important, interesting and very open agenda. In theoretical research, the agenda entails addressing the difficult issues of legislative bargaining under different electoral rules, perhaps with an endogenous number of political parties. In empirical research, it entails studying how observed policy outcomes correlate with observed political outcomes – party structures, types of government, legislative majorities – and how those, in turn, are associated with alternative electoral rules.

While data on such political outcomes are readily available for a small group of developed democracies, this is not true for most of the other countries in our two data sets. Further empirical work thus requires a non-trivial investment in data collection, particularly to obtain political-outcome data going back in time. New data collection is also necessary to exploit the time variation associated with electoral reform for more secure causal inference. While the broad features of electoral systems are very stable, there is much more tinkering with the finer details – district magnitudes, ballot structures, thresholds for representation, the openness of party lists, etc. Once more, it is necessary to invest considerable time and effort to document all such piece-meal reforms for, say, 60 countries over 40 years.

The future research agenda on alternative forms of government is even more open. Little is known in theory about how alternative rules for government formation or dissolution, or alternative rules for the functioning of legislatures, shape economic policy outcomes. Even less is known about the empirical association of these detailed institutional features and observed economic policies. At the end of the former section, we mentioned that our empirical measures are incomplete in the separation of powers dimension, and that this makes them less suitable for the testing of certain predictions.

Existing theory of policymaking and comparative politics is restrictive also in a different dimension: it is generally confined to *static* models of

economics and politics. The lack of dynamics becomes a glaring omission when we try to interpret the empirical puzzles associated with time patterns in observed policy. To understand how fiscal policy responds to economic fluctuations, why fiscal adjustments are delayed, why some political systems are more likely to pile up government debt, or face tight borrowing constraints, we obviously need dynamic models. In particular, we need models which assign an important role to state variables, such as government debt, and models which include links between current policy decisions and future status-quo points.

On the policy side, we have concentrated on fiscal policy and rent extraction. We have also scratched the surface of the policies most likely to promote economic growth. But much more could be done to gain a better understanding of how constitutional features shape economic performance through public policy. It would then be interesting and feasible to study other policy instruments – such as the structure of taxation, micro-regulatory policies, more detailed measures of trade policy, and perhaps environmental policy – with the methods illustrated in this book.

On the constitutional side, we have concentrated on electoral rules and forms of government. In the process, however, we have also discovered interesting effects of other fundamental constitutional features, such as federal structure and the quality of democratic institutions. The quality and age of democracies seem to interact with the electoral rule and the form of government in shaping various aspects of economic policies. These findings further strengthen our belief that several aspects of the constitution help shape economic policy. Our data indicate important and subtle complementarities between political rights, democratic traditions, details of the electoral rule, and the form of government.

All in all, exploring further the constitutional effects on economic policy and performance is a worthwhile but challenging task, which requires an iteration between rigorous theory, careful collection of data, and solid statistical work. Progress on this task will advance the research frontier in economics as well as political science.

Table 9.1
Constitutions and economic policy
Questions and findings

Policy outcome	Electoral rules		Form of goverment	
	Majoritarian vs. proportional		Presidential vs. parliamentary	
	Theory	Data	Theory	Data
Overall size of government	– /?	–	–	–
Composition: broad vs. narrow programs	–	–	–	0 / –
Rent extraction	+ / –	+ / –	–	0
Government deficits	– /?	–	?	0
Structural policy/economic performance	?	+ / –	?	–
Adjustment to shocks	?	0 / –	?	–
Electoral Cycles	+/?	+ / –	?	+ / –

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Data Appendix

The two data sets used in the book and including the variables defined below are available at our home pages: http://www.iies.su.se/~perssont/ and http://www.uni-bocconi.it/index.php?frcnav=@11%2C527%2C2315

$AFRICA$: regional dummy variable, equal to 1 if a country is in Africa, 0 otherwise.

AGE: age of democracy, defined as: $AGE = (2000 - DEM\_AGE)/200$ and varying between 0 and 1, with US being the oldest democracy (value of 1). Source: see DEM_AGE.

$ASIAE$: regional dummy variable, equal to 1 if a country is in East Asia, 0 otherwise.

$AUTOC$: indicator of institutionalized autocracy, derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Source: Polity IV Project (http://www.cidcm.umd.edu/inscr/polity/index.htm).

$AVELF$: index of ethnolinguistic fractionalization, approximating the level of lack of ethnic and linguistic cohesion within a country, ranging from 0 (homogeneous) to 1 (strongly fractionalized) and averaging 5 different indexes. Source: La Porta et al. (1998). For Central and Eastern Europe countries computations follow Mauro (1995) with data from Quain (1999).

$CATHO80$: percentage of the population belonging to the Roman Catholic religion in 1980. Source: La Porta et al. (1998).

CCG_NET_0: consolidated central government net domestic debt as a percentage of ross national disposable income, in the first year for which a value of SPL is available. Consolidated Central Government (CCG) is defined as budgetary central government plus extra-budgetary central government plus social security agencies. Definition of central government equivalent to that of general government minus local and regional governments. Source: World Savings Database

$CGEXP$: central government expenditures as a percentage of GDP, constructed using the item Government Finance - Expenditures in the IFS, divided by GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

$CGREV$: central government revenues as a percentage of GDP, constructed using the item Government Finance - Revenues in the IFS, divided

by GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

COL_ESP: dummy variable, equal to 1 if the country is a former colony of Spain or Portugal, 0 otherwise. Source: Wacziarg (1996).

COL_ESPA: Spanish colonial origin, discounted by the years since independence (T_INDEP), and defined as $COL\_ESPA = COL\_ES * (250 - T\_INDEP)/250$. Source: Wacziarg (1996).

COL_OTH: dummy variable, equal to 1 if the country is a former colony of a country other than Spain, or Portugal, or the UK, 0 otherwise. Source: Wacziarg (1996).

COL_OTHA: defined as $COL\_OTH * (250 - T\_INDEP)/250$. See also COL_ESPA. Source: Wacziarg (1996).

COL_UK: dummy variable, equal to 1 if the country is a former UK colony, 0 otherwise. Source: Wacziarg (1996).

COL_UKA: defined as $COL\_UKA = COL\_UK*(250 - T\_INDEP)/250$. See also COL_ESPA. Source: Wacziarg (1996).

$CON2150$: dummy variable for the period in which the current constitutional features originated, equal to 1 if either $YEARELE$ or $YEAREG$ falls in the period between 1921 and 1950, 0 otherwise. Source: see $YEARREG$ and $YEARELE$.

$CON5180$: dummy variable for the period in which the current constitutional features originated, equal to 1 if either $YEARELE$ or $YEAREG$ falls in the period between 1951 and 1980, 0 otherwise. Source: see $YEARREG$ and $YEARELE$.

$CON81$: dummy variable for the period in which the current constitutional features originated, equal to 1 if either $YEARELE$ or $YEAREG$ falls in the period after 1981, 0 otherwise. Source: see $YEARREG$ and $YEARELE$.

$CONFU$: dummy variable for religious tradition, equal to 1 if the majority of population is Confucian/Buddhist/Zen, 0 otherwise. Source: Wacziarg (1996), CIA-The World Factbook 2000.

$CPI9500$: corruption perception index, measuring perceptions of abuse of power from public officials. Average of the CPI Index over the period 1995-2000, which ranges from 0 to 10, with higher values denoting more corruption. Source: Transparency International (www.transparency.de) and Internet Center for Corruption Research (www.gwdg.de/~uwvw).

DEM_AGE: first year of democratic rule, corresponding to the first year of an uninterrupted string of positive yearly values of the variable $POLITY$

(see below) until the end of the sample, given that the country was also an independent nation. Does not count foreign occupation during WWII as an interruption of democracy. Source: See $POLITY$.

$DEMOC$: institutionalized democracy index, derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Source: Polity IV Project (http://www.cidcm.umd.edu/inscr/polity/index.htm).

$DCGEXP$: first difference of $CGEXP$. Source: see $CGEXP$.

$DISTRICTS$: the number of electoral districts in a country (including the number of primary as well as secondary and tertiary if applicable). Sources: Quain (1999), Kurian (1998), and national sources.

$DSSW$: first difference of SSW. Source: see SSW

$EDUGER$: total enrolment in primary and secondary education, as a percentage of the relevant age group in the population. Computed dividing the number of pupils (or students) enrolled in a given level of education regardless of age by the population of the age-group which officially corresponds to the given level of education, and multiplying the result by 100. Source: UNESCO - Education Indicator - Category Participation. Available on www.unesco.org

$ELEX$: dummy variable for executive elections, equal to 1 in a year when the executive is elected, and 0 otherwise. Takes into consideration both presidential elections and legislative elections. Source: http://www.ifes.org/eguide/elecguide.htm plus other national sources.

$ELLEG$: dummy variable for legislative elections, equal to 1 in the year the legislature is elected, independent of the form of government. Source: http://www.ifes.org/eguide/elecguide.htm plus other national sources.

$EL\_MAJ = MAJ * ELEX$. Source: see $ELEX$ and MAJ.

$EL\_MAJPAR = (1 - PRES) * MAJ * ELEX$. Source: see $ELEX$, $PRES$ and MAJ.

$EL\_MAJPRE = PRES * MAJ * ELEX$. Source: see $ELEX$, $PRES$ and MAJ.

$EL\_PAR = (1 - PRES) * ELEX$. Source: see $ELEX$ and $PRES$.

$EL\_PRE = PRES * ELEX$. Source: see $ELEX$ and $PRES$.

$EL\_PRO = (1 - MAJ) * ELEX$. Source: see $ELEX$ and MAJ.

$EL\_PROPAR = (1 - PRES) * (1 - MAJ) * ELEX$. Source: see $ELEX$, $PRES$ and MAJ.

$EL\_PROPRE = PRES * MAJ * ELEX$. Source: see $ELEX$, $PRES$ and MAJ.

$ENGFRAC$: the fraction of the population speaking English as a native language. Source: Hall and Jones (1999).

$EURFRAC$: the fraction of the population speaking one of the major languages of Western Europe: English, French, German, Portuguese, or Spanish. Source: Hall and Jones (1999).

$FEDERAL$: dummy variable, equal to 1 if the country has a federal political structure, 0 otherwise. Source: Adserà, Boix and Paine (2001).

$FRANKROM$: natural log of the Frankel-Romer forecasted trade share, derived from a gravity model of international trade that only takes into account country population and geographical features. Source: Hall and Jones (1999).

$GADP$: index of government's anti-diversion policies, measured around 1985. It is an equal-weighted average of these five categories: i) law and order, ii) bureaucratic quality, iii) corruption, iv) risk of expropriation and v) government repudiation of contracts (each of these items has higher values for governments with more effective policies towards supporting production) and ranges from zero to one. Source: Hall and Jones (1999).

$GASTIL$: average of indexes for civil liberties and political rights, where each index is measured on a one-to-seven scale with one representing the highest degree of freedom and seven the lowest. Countries whose combined averages for political rights and civil liberties fall between 1.0 and 2.5 are designated "free", between 3.0 and 5.5 "partly free" and between 5.5 and 7.0 "not free". Source: Freedom House, Annual Survey of Freedom Country Ratings.

GDP: gross domestic product at current price. Source: IFS CD-Rom and IFS Yearbook.

$GINI\_8090$: Gini index on income distribution, computed as the average of two data points: the observation closest to 1980 and the observation closest to 1990. When only one of the two years year is available, only that year is included. Source: Deininger and Squire (1996).

$GOVEF$: point estimate of "Government Effectiveness", the third cluster of the Kaufmann et al.(1999a) governance indicators. Combines perceptions of the quality of public service provision, the quality of the bureaucracy, the competence of civil servants, the independence of the civil service from political pressures, and the credibility of the government's commitment to policies into a single grouping. Ranges from around 0 to around 10 (lower

values correspond to better outcomes). Sources: Kaufmann et al. (1999a), available at http://www.worldbank.org/wbi/gac.

$GRAFT$: point estimate of "Graft", the sixth cluster of Kaufmann et al.'s governance indicators, focusing on perceptions of corruption. Ranges from around 0 to around 10 (lower values correspond to better outcome). Sources: Kaufmann et al. (1999a), available at www.worldbank.org/wbi/gac.

$LAAM$: regional dummy variable, equal to 1 if a country is in Latin America, Central America or the Caribbeans, 0 otherwise.

$LAT01$: rescaled variable for latitude, defined as the absolute value of $LATITUDE$ divided by 90 and taking values between 0 and 1. Source: Hall and Jones (1999).

$LATITUDE$: distance from the equator (in degrees), ranging between -90° to 90°. Source: Hall and Jones (1999).

$LCGEXP$: one-year lag of $CGEXP$. Source: see $CGEXP$.

$LCGREV$: one-year lag of $CGREV$. Source: see $CGREV$.

$LEGOR(UK, FR, GE, SO, SC)$: dummy variables for the origin of the legal system, classifying a country's legal system into Anglo-Saxon Common Law (UK), French Civil Law (FR), German Civil Law (GE), Socialist Law (SO), or Scandinavian Law (SC). Source: La Porta et al. (1998).

$LELEX$: one year lag of $ELEX$. Source: see $ELEX$.

LEL_MAJ: one year lag of EL_MAJ. Source: see EL_MAJ.

LEL_MAJPAR: one year lag of EL_MAJPAR. Source: see EL_MAJPAR.

LEL_MAJPRE: one year lag of EL_MAJPRE. Source: see EL_MAJPRE.

LEL_PRO: one year lag of EL_PRO. Source: see EL_PRO.

LEL_PROPAR: one year lag of EL_PROPAR. Source: see EL_PROPAR.

LEL_PROPRE: one year lag of EL_PROPRE. Source: see EL_PROPRE.

$LIST$: number of lower-house legislators elected through party list systems (see the text in Chapter 4 for further discussion and clarification). Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999) and Kurian (1998) and national sources.

$LOGA$: natural log of total factor productivity, measured in 1988. Source: Hall and Jones (1999).

$LOGYL$: natural log of output per worker, measured in 1988. Source: Hall and Jones (1999).

$LPOP$: natural log of the total population (in millions). Source: World Bank

$LSPL$: one-year lag of SPL. Source: see SPL

$LSSW$: one-year lag of SSW. Source: See SSW.

LYP: natural log of per capita real GDP ($RGDPH$). $RGDPH$ is defined as real GDP per capita in constant dollars (chain index) expressed in international prices, base year 1985. Data through 1992 are taken from the Penn World Table 5.6 (variable named RGDPC), while data on the period 1993-98 are computed from data from the World Development Indicators. These later observations are computed on the basis of the latest observation available from the Penn Word Tables and the growth rates of GDP per capita in the subsequent years computed from the series of GDP at market prices (in constant 1995 U.S. dollars) and population, from the World Development Indicators. Sources: Penn World Tables - mark 5.6 (PWT), available at http://datacentre2.chass.utoronto.ca/pwt/docs/topic.html. The World Bank's World Development Indicators; www.worldbank.org.

$MAGN$: inverse of district magnitude, defined as $DISTRICTS$ over $SEATS$. Sources: see $DISTRICTS$ and $SEATS$.

MAJ: dummy variable for electoral systems equal to 1 if all the lower house is elected under plurality rule, 0 otherwise. Only legislative elections (lower house) are considered (see the text in Chapter 4 for further clarification). Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), Kurian (1998), and national sources.

$MAJ\_BAD = MAJ * GASTIL$. Source: see MAJ and $GASTIL$.

$MAJ\_GIN = MAJ * GINI\_8090$. Source: see MAJ and $GINI\_8090$.

$MAJ\_OLD = MAJ * AGE$. Source: see MAJ and AGE.

$MAJPAR = MAJ * (1 - PRES)$. Source: see MAJ and $PRES$.

$MAJPRES = MAJ * PRES$. Source: see MAJ and $PRES$.

$MINING\_GDP$: share of mining sector over GDP. Source: UN National accounts.

$MIXED$: dummy variable for electoral systems, equal to 1 if the electoral formula for electing the lower house is neither strict plurality rule nor strict proportionality, 0 otherwise. Semi-proportional (or mixed) electoral rule identifies those electoral systems characterized by both proportional and first-past-the-post representation for allocating seats (for example Bolivia, Germany, Italy after the reform of 1993). The share of the total number of seats allocated under the Proportional rule can be greater or smaller than the complementary plurality-allocated share. Only legislative elections considered. Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998) and national sources.

$NEGYG$: negative values of $YGAP$, 0 if $YGAP$ is positive. Source: see

$YGAP$.

$OECD$: dummy variable, equal to 1 for all countries that were members of OECD before 1993, 0 otherwise, except for Turkey coded as 0 even though an OECD-member before the 1990s.

OIL: price of oil in US dollars. Source: Datastream.

OIL_EX: OIL times a dummy variable equal to 1 if net the exports of oil are positive, 0 otherwise. Source: See OIL

OIL_IM: OIL times a dummy variable equal to 1 if the net exports of oil are negative, 0 otherwise. Source: See OIL

$PIND = 1 - \frac{LIST}{SEATS}$. Source: see $LIST$ and $SEATS$.

$POLITY$: score for democracy, computed by subtracting the $AUTOC$ score from the $DEMOC$ score, and ranging from +10 (strongly democratic) to -10 (strongly autocratic). Source: Polity IV Project (http://www.cidcm.umd.edu/inscr/polity/index.htm).

$POLITY\_GT$: interpolated version of $POLITY$, rescaled with the same units as $GASTIL$ (i.e., higher values denote worse democracies). Computed as the forecasted value obtained by regressing the rescaled values of $POLITY$ on $GASTIL$. Source: see $POLITY$ and $GASTIL$.

$POSYG$: positive values of $YGAP$, 0 if $YGAP$ is negative. Source: see $YGAP$.

$PRES$: dummy variable for forms of government, equal to 1 in presidential regimes, 0 otherwise. Only regimes where the confidence of the assembly is not necessary for the executive (even if an elected president is not chief executive, or if there is no elected president) are included among presidential regimes. Most semi-presidential and premier-presidential systems are classified as parliamentary (see the text in Chapter 4 for further discussion and clarification). Source: Shugart and Carey (1992) and national sources).

$PRES\_BAD = PRES * GASTIL$. Source: see $PRES$ and $GASTIL$

$PRES\_GIN = PRES*GINI\_8090$. Sources: see $PRES$ and $GINI\_8090$

$PRES\_OLD = PRES * AGE$. Source: see $PRES$ and AGE

$PROP1564$: percentage of the population between 15 and 64 years old in the total population. Source: World Development Indicators CD-Rom 1999.

$PROP65$: percentage of the population over the age of 65 in the total population. Source: World Development Indicators CD-Rom 1999.

$PROPAR = (1 - MAJ) * (1 - PRES)$. Source: see MAJ and $PRES$.

$PROPRES = (1 - MAJ) * PRES$. Source: see MAJ and $PRES$.

$PROT80$: percentage of the population in each country professing the Protestant religion in 1980. Source: La Porta et al. (1998).

SDM: district magnitude (i.e., as seats over districts), computed as a weighted average, where the weight on each district magnitude in a country is the share of legislators running in districts of that size. Relative to the original variable in Seddon et al. (2001), this variable is divided by 100 so that it takes values comparable to those of $MAGN$. Source: Seddon et. al (2001).

$SEATS$: number of seats in lower or single chamber for the latest legislature of each country. It is also related to the number of districts in which primary elections are held. Source: International Institute for Democracy and Electoral Assistance (1997), Quain (1999), Kurian (1998) and national sources.

SPL: central government budget surplus (if positive) or deficit (if negative), as a percentage of GDP, constructed using the item Government Finance - Deficit and Surplus in the IFS, divided by the GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

$SPROPN$: share of legislators elected in national (secondary or tertiary) districts rather than sub-national (primary) electoral districts. Source: Seddon et al. (2001)

SSW: consolidated central government expenditures on social services and welfare as a percentage of GDP, as reported in the GFS Yearbook, divided by GDP and multiplied by 100. Source: IMF - GFS Yearbook 2000 and IMF - IFS CD-Rom.

T_INDEP : years of independence, ranging from 0 to 250 (the latter value is used for all non-colonized countries). Source: Wacziarg (1996).

$TRADE$: sum of exports and imports of goods and services measured as a share of GDP. Source: The World Bank's World Development Indicators, CD-Rom 2000.

$YEARELE$: the year when the current electoral rule, as coded by MAJ, was first introduced, or the first year of democratic rule, whatever came last.

$YEARREG$: the year when the current form of government, as coded by $PRES$, was first introduced, or the first year of democratic rule, whatever came last.

$YRSOPEN$: index for openness to international trade, compiled by Sachs and Werner (1995), measuring the fraction of years during the period 1950-1994 that the economy has been open and ranging between 0 and 1. Source: Hall and Jones (1999).

$YGAP$: deviation of aggregate output from its trend value in percent, computed as the difference between the natural log of real GDP in the country

and its country-specific trend (obtained, using the Hodrick-Prescott filter). Source for real GDP: World Bank.