Political Institutions and Policy Outcomes: What are the Stylized Facts?*

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Abstract

We investigate the effect of electoral rules and political regimes on fiscal policy outcomes – the size and composition of government spending, and government deficits – in a panel of about 60 democracies from 1960 and onwards. Presidential regimes are associated with smaller governments than parliamentary regimes, a smaller response of spending to different economic events, and a stronger post-election cycle but a weaker pre-election cycle. Majoritarian elections are associated with smaller broad spending programs and smaller deficits than proportional elections; they also have smaller spending responses to events and a stronger pre-election cycle in taxes. Several of these empirical regularities are in line with recent theoretical work; others are still awaiting a theoretical explanation.

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1. Introduction

A recent literature on comparative politics has asked how political institutions might shape economic policy. In particular, a number of theoretical contributions by economists have posed the question whether electoral rules and political regimes systematically influence fiscal policy outcomes: see Persson and Tabellini (2000) for a survey. But empirical work is still scant. Whereas a large and interesting literature discusses how constitutional features of state and local governments correlate with policy outcomes (see for instance Bohn and Inman, 1996, Pommerhene, 1990, Feld and Matsusaka, 2000), only a few empirical studies have compared fiscal policy in large samples of countries governed by different electoral rules or regime types. Some recent exceptions are Poterba and Von Hagen (1999), Milesi-Ferretti, Perotti and Rostagno (2000) and Persson and Tabellini (1999). Naturally, political scientists have done extensive empirical work on comparative politics. But their focus has been on political phenomena, such as the number of parties, the frequency of elections, or the attributes of governments under different constitutions, and does not touch on fiscal policy. As a result, very little is known about whether and how fiscal policy varies across political institutions, particularly when the analysis is extended to non-OECD countries.[1]

We try to fill this gap. Specifically, we try to establish some stylized facts regarding the mapping from electoral rules and political regimes to policy outcomes. We look exclusively at the effects on fiscal policy: the size and composition of government spending and government deficits. A companion paper (Persson, Tabellini and Trebbi, 2000) studies the incidence of corruption across different political institutions.

The political constitution seems to matters a lot for policy. We find striking similarities between presidential regimes and majoritarian electoral rules. Both institutions are associated with smaller governments, compared to parliamentary and proportional systems. This is particularly true of presidential regimes and when we consider the growth of government over time. The reaction of government spending to economic and political events is also systematically correlated with institutions. Presidential and majoritarian systems have a more dampened and less persistent reaction to income shocks, compared to proportional parliamentary systems. This could be related to a different composition of spending (social transfer programs tend to be smaller in presidential and majoritarian democ-

[1] Tanzi and Schuknecht (2000) provide an extensive and detailed description of fiscal policy in a very large sample of countries, but they do not ask how policy varies across constitutions.

racies), or it could be a different reaction of the collective decision process to changing economic circumstances. The different dynamic and stochastic behavior of government spending is also reflected in budget deficits, which tend to be smaller and less reactive to shocks in presidential and majoritarian democracies. Finally, the electoral cycle of fiscal policy is also institution dependent. In presidential regimes we observe a post-election cycle, with painful fiscal adjustments postponed until after the election. In parliamentary regimes, we instead observe a pre-election cycle, with tax cuts taking place in the election year.

Section 2 provides a background, by sketching some of the main ideas in recent theoretical work. Section 3 describes our data set, in which the measurements of fiscal policy outcomes as well as political institutions are clearly motivated by the theory. Our statistical methodology is described in Section 4. While some of our estimates aim at direct tests of specific hypotheses, we also go beyond such tests in our search for systematic relationships in the data. Section 5 describes our empirical results. Section 6 summarizes our results and makes suggestions for future research.

2. Motivation

Why would political institutions shape economic policy? The basic idea is that policy choices entail conflicts between different groups of voters, between voters and politicians (agency problems), and between different politicians. The way these conflicts are resolved, and thus what fiscal policy we observe, hinges on the political institutions in place

Political institutions certainly have many dimensions. Arguably, however, the most fundamental aspects of constitutions determine how the "control rights" over policy are acquired and how they can be exercised. Thus, which politicians get the power to make policy decisions is determined by voters, but is crucially influenced by **rules for elections**. Policy choices are made by elected politicians, but are crucially influenced by rules for rule-making and legislation; that is, what political scientists call the **regime type**.

As mentioned in the introduction, a recent theoretical literature has tried to model the consequences of these institutions for fiscal policy choices. It has focused on the level of taxation and on the composition of spending, distinguishing between three types of programs: (i) broad, non-targeted programs benefiting large groups of the electorate; (ii) narrow, targeted programs benefiting small groups; (iii) programs benefiting mainly incumbent politicians. Political institutions are modeled as the rules for a specific legislative bargaining game with delegation, where voters elect political representatives who in turn bargain over fiscal policy. Alternative constitutions amount to alternative rules for how to play this game and an exercise in "comparative politics" amounts to comparing equilibrium outcomes. Below we describe the main ideas in a handful of recent studies which have applied this comparative politics approach. We just outline the results, emphasizing the specific predictions regarding the size and composition of public spending. Interested readers can find the details in Persson and Tabellini (2000, Part III).

Electoral rules Legislative elections around the world differ in several dimensions. The political science literature emphasizes two: **district size** and the **electoral formula.**[2] District size simply determines how many legislators acquire a seat in a voting district. The electoral formula determines how votes are translated into seats. Under plurality rule, only the individual(s) winning the highest vote share(s) get the seat(s) in a given district, whereas proportional representation instead awards seats to parties in proportion to their vote shares.

We find a strong correlation in these features across real-world electoral systems. Some systems can be described as **majoritarian**, combining small voting districts with plurality rule. Archetypes here are elections to the UK parliament or the US Congress, where the candidate collecting the largest vote share in a district gets the **single** seat. Some electoral systems are instead decidedly **proportional**, combining large electoral districts with proportional representation. Archetypes are the Dutch and Israeli elections, where parties obtain seats in proportion to their vote shares in a single national voting district. While we find some intermediate systems, most countries fall quite unambiguously into this crude, binary classification.

Why would district size matter for government spending? One idea is that larger voting districts diffuse electoral competition, inducing parties to seek support from broad coalitions in the population. Smaller districts steer electoral competition towards narrower, geographical constituencies. Clearly, broad programs are more effective in seeking broad support and targeted programs more effective in seeking narrow support. Proportional elections with larger districts should thus be more biased towards broad, non-targeted programs, a point which has been formally made by Persson and Tabellini (1999) and Milesi-Feretti, Per-

[2] Other aspects of the electoral system that differ across countries include thresholds for representation and the rules governing party lists. See e.g Cox (1997) and Blais and Massicotte (1996) for recent descriptions of variations in electoral rules across countries.

otti and Rostagno (2000).

How about the electoral formula? One effect of the winner-takes-all property of plurality rule is to reduce the minimal coalition of voters needed to win the election. Votes for a party that does not obtain plurality are wasted. With single-member districts and plurality, a party thus needs only 25 % of the national vote to win: 50 % in 50 % of the districts. Under full proportional representation it needs 50% of the national vote. Politicians are thus induced to internalize the policy benefits for a larger segment of the population, which reinforces the prediction that proportional elections should be associated with broader spending programs (Lizzeri and Persico, 2000, Persson and Tabellini, 2000, Ch. 9).

While voters choose among individual candidates under plurality rule, they choose among party lists under proportional representation. Such lists may dilute the incentives for individual incumbents to perform well. Persson and Tabellini (2000, Ch. 9) examine the policy consequences in Holmström (1982)-style, career-concern models. They derive the predictions that electoral cycles, showing up in spending or taxes, should be weaker under proportional elections. The reason is that incumbents' career concerns are stronger under plurality rule and at their strongest just before elections.

A pitfall of the recent theoretical literature is that it has neglected the implications of the electoral rule on the party structure. Many empirical contributions by political scientists deal with precisely this aspect of alternative electoral rules (see for instance Lijphart,1994). Majoritarian elections are typically associated with a smaller number of parties represented in the legislature. This too shapes policy decisions, even though the party structure is a by-product of the electoral rule and not a property of the electoral rule itself. On the one hand, proportional elections have lower barriers to entry for new parties. This allows for the formation of parties catering to specific and small groups of voters. On the other hand, governments in majoritarian parliamentary regimes are more likely to be supported by a single party with absolute majority, whereas coalition governments are more likely under proportional elections. This could have various consequences for economic policy, stressed in several papers from the 1980s and 1990s. First, the common-pool problem in fiscal policy might be more pervasive under coalition governments. Kontopoulos and Perotti (1999) have argued that this could lead to larger government spending. Second, as coalition governments have more veto players, the status-quo bias in the face of adverse shocks could be more pronounced (Roubini and Sachs, 1989, Alesina and Drazen, 1991). Third, government crises are more likely and indeed empirically more frequent under proportional elections, which could lead to greater policy myopia and larger budget deficits (Alesina and Tabellini, 1990, Grilli, Masciandaro and Tabellini, 1991). Not all these ideas have been fleshed out with the same analytical rigor as in the more recent theoretical literature discussed above. But they can certainly suggest interpretations for the empirical findings we report below.

Regime types Two crucial aspects of the legislative regime concern the powers over legislation: to make, amend, or veto policy proposals. The first concerns the separation of those powers across different politicians and offices. The second concerns the maintenance of powers; in particular, whether the executive needs sustained confidence by a majority in the legislative assembly.

As in the case of electoral rules, we can make a crude empirical classification of real-world regimes with regard to these aspects. Presidential regimes typically have separation of powers, between the president and Congress, but also between congressional committees that hold important proposal (agenda-setting) powers in different spheres of policy (think about the US). But they do not have a confidence requirement: the executive can hold on to his powers without the support of a majority in Congress. In parliamentary regimes the proposal powers over legislation are instead concentrated in the hands of the government. Moreover, the government needs the continuous confidence of a majority in parliament to maintain those powers throughout an entire election period.

Why should separation of powers matter for policy? A classical argument is that checks and balances constrain politicians from abusing their powers. Persson, Roland, and Tabellini (1997, 2000) formally demonstrate this old point in models where incumbents are held accountable by retrospective voters. The upshot is that we should expect weaker political accountability in parliamentary regimes, resulting in higher rents and higher taxes.

Another idea is associated with the confidence requirement. Parties supporting the executive hold valuable proposal powers which they risk losing in a government crisis. Therefore, they have strong incentives to maintain a stable majority when voting on policy proposals in the legislature. Building on this idea of "legislative cohesion", due to Diermeier and Feddersen (1998), Persson, Roland and Tabellini (2000) derive two additional predictions.

In parliamentary regimes, a stable majority of legislators tends to pursue the joint interest of its voters. In presidential regimes, the (relative) lack of such a majority tends to pit the interests of different minorities against each other for different issues on the legislative agenda. Spending in parliamentary regimes thus optimally becomes more directed towards broad programs.

In parliamentary regimes, the stable majority of incumbent legislators, and its voters, become prospective residual claimants on additional revenue. Both favor high taxes and high spending. In presidential regimes, on the other hand, majorities are not residual claimants on revenue and therefore resist high spending. These forces produce larger governments (higher taxes) in parliamentary regimes.

Summary Let us summarize the main predictions with the help of Table 1. According to the theory, presidential regimes have smaller governments than parliamentary regimes and less spending on broad programs. Under majoritarian elections, we should observe less spending on broad programs than under proportional elections. The common pool argument suggests that the electoral rule could also matter for the size of government, with proportional elections associated with bigger governments. These are all cross-sectional predictions, in that they have been derived by comparing equilibria in static models. The result of more pronounced electoral cycles under majoritarian elections instead relies on a dynamic model and is thus a time-series prediction. Models stressing the greater status quo bias and myopia of coalition governments have other time series predictions. Proportional parliamentary regimes are expected to have larger steady states debts, and in the transition also larger budget deficits. To derive specific implications about the reaction to shocks under these different systems, one would need a more precise model, including detailed assumptions about the status quo policy.

3. Data

In putting our data set together, we have relied on the theory described in Section 2 for the measurement of political institutions and fiscal policy outcomes. Naturally, data availability also affects the sample, which comprises yearly data for 61 countries over almost four decades (1960-98). This panel includes a large number of economic, social and political variables. Because of missing data and our rules for sampling (described next), however, it is an unbalanced panel. (The sources for all the data used in the paper are listed in the Appendix.)

Which countries? The theory suggests that we should confine our study to countries with democratic political institutions. To assess a country's democratic status, we have relied on a well-known classification by Freedom House. The so-called Gastil index of political rights varies (by steps of 1/2) on a scale from 1 to 7, low values being associated with better democratic institutions.[3] The Gastil index is available annually, from 1972 and onwards. For the earlier period, we follow Barro (1998) in relying on a measure compiled by Bollen (1990), available every five years (which we re-scaled onto a scale from 1 to 7).

We use three different rules for including countries in the sample, and we report results for all three samples. The most permissive one is to include a country from the point in time when it first obtains a Gastil-score of 5 or lower, but not exclude it from the sample in the wake of a temporarily higher score reflecting restricted democratic rights. This rule permits a maximum of 61 countries in the sample. We refer to this sample of countries as the Broad sample. A more restrictive rule is to exclude a country from the sample in any given year when it has a Gastil score of 4 or lower. This is our Default sample. This rule cuts the number of annual observations in the panel by about 350. As an example, the more restrictive rule temporarily excludes countries like Turkey (intermittently) and Argentina (in the 80s) after their first entry into the panel. A yet more restrictive rule identifies countries and years where the Gastil score is less than or equal to 2. Here we lose many more observations, particularly in the early part of the sample, since we are really restricting attention to well functioning democracies. We call this our Narrow sample of countries. As in the default sample, a few countries enter and exit from the sample at different points of time. Throughout, we treat these censored observations as randomly missing and do not attempt to model sample selection. The three samples are listed in Table 2.

Which political institutions? Following the theoretical discussion in Section 2, we classify electoral rules and regime types by means of two indicator (dummy) variables: MAJ and PRES. Majoritarian countries (MAJ $= 1$) are those that relied exclusively on plurality rule in its previous most recent election to the legislature (lower house), the others are proportional (MAJ $= 0$). Relying on district size rather than the electoral formula would produce a similar but not identical classification.[4] In some sensitivity analysis, not reported below, we have also allowed for a finer partition that discriminates between three types: majority,

[3] According to the index, countries scoring 1 or 2 are "free", countries scoring from 3 to 5 "semi-free", while countries scoring 6 or 7 are "non-free".

[4] Persson and Tabellini (1999) rely on district size, classifying all countries with an average district size below two (seats per district) as majoritarian, others as proportional.

proportional and mixed systems. But when it comes to the effect on fiscal policy outcomes, the effects of mixed and proportional systems appear to be similar. In our companion paper (Persson, Tabellini and Trebbi, 2000), we use continuous measures both of district size and of the use of plurality rule.

With regard to regime type, we classify as presidential ($PRES = 1$) countries where the executive is not accountable to the legislature through a vote of confidence, and those where it is as parliamentary ($PRES = 0$). Thus, we try to capture the institutions producing stable legislative majorities, as discussed in Section 2. (We have not tried to classify countries on the basis of the checks and balances entailed in the separation of powers granted by their constitutions.)

There are very few changes over time in these classifications ($PRES$ does not vary at all, whereas MAJ displays time variation in a few countries such as France, which had a brief period of proportional representation in 1985-86). This stability reflects an inertia of political institutions sometimes called an "iron law" by political scientists. The lack of time variation is unfortunate in that it provides us with almost no "experiments" in the form of regime changes. But it is also an indication that it may be correct to treat institutions as exogenous and given by history. A key assumption, maintained throughout the paper, is thus that institutions are strictly exogenous and do not respond to policy.

Figure 1 illustrates the institutional variation across countries in 1995. The colored portions of the map represent the 61 countries in the sample. Striped areas indicate presidential regimes ($PRES = 1$), solid areas parliamentary regimes ($PRES = 0$). Darker shade indicates majoritarian elections ($MAJ = 1$), lighter shade proportional elections ($MAJ = 0$). The least common system is the US-style (gray striped) combination of a presidential regime with majoritarian elections, with only five countries. But each of the other three combinations is well represented in the sample.

As the map illustrates, using theory in the classification sometimes produces results contrary to popular perception. According to our classification, parliamentary regimes include France, Portugal and Finland, with a directly elected president, but where the government is accountable to the elected assembly. Conversely, the presidential regimes include Switzerland, where there is no popularly elected president but the permanent coalition executive cannot be brought down by the legislative assembly.

The electoral rule does not exhibit a particular pattern in terms of geography or degree of development, but most Anglo-Saxon countries and countries of British colonial origin have $MAJ = 1$. Presidential regimes, on the other hand, are largely confined to non-OECD countries (among the OECD-countries, only the US and Switzerland have $PRES$ = 1). Moreover, many presidential regimes happen to be in Central and South America, though the sample also includes several non-presidential Caribbean countries. Other presidential regimes are Nepal, the Philippines and Senegal.

Which fiscal policy outcomes? We include fiscal policy outcomes as suggested by the theory. Thus, we measure the size of government mainly by the ratio of central government spending to GDP expressed as a percentage (CGEXP). But we have also looked at central government revenues and at general government spending, both as a percentage of GDP. For the composition of government spending we use two measures: social security and welfare spending (by central government) as a percentage of GDP (SSW/GDP), or as a ratio to spending on goods and services (SSW/GDS). The presumption is that broad transfer programs, like pensions and unemployment insurance, are much harder to target narrowly than spending on goods and services. Finally, we look at the size of the budget surplus of the central government, as a percent of GDP (SURPLUS).

The measures of size and deficits are available for most OECD countries for the entire period 1960-98. For many developing countries availability is limited to the period from the 1970s and onward. Similarly, the measures of the composition of spending do not become available until the early 1970s. The statistical source for these variables is the IMF. For the size of government and budget deficits we rely on IFS data, which includes social security in the measures of central government spending and is available for a longer time series. The composition of spending is taken instead from the GFS database.

These policy measures vary a great deal, both across time and countries. As an illustration consider Figure 2, which shows the size of government as measured by central expenditures in our sample. In the figure, we see that government expenditure in a typical year ranges from below 10 percent of GDP to above 50 percent. 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 percent of GDP from the 1960s to the mid 1990s. Most of this growth takes place in the 1970s and 80s.

Our measures of the composition of spending also show a wide distribution where spending on social security and welfare drifts upwards at least until the mid 1980s. The deficits are also widely distributed across countries, with average deficits having their peak in the period from the mid 70s to the mid 80s.

Which socio-economic controls? The theory we have surveyed in Section 2 should clearly be understood as providing *ceteris paribus* predictions about fiscal policy. Therefore, we control for other variables likely to shape government outlays and revenues. Specifically, we always include in our regressions the level of development, measured by the log of real per capita income (LYH), a measure of openness (TRADE), defined as exports plus imports over GDP, and two variables measuring the demographic composition, defined as the percentages of the population between 15 and 64 years of age (PROP1564), and above 65 years of age (PROP65), respectively. These variables have been show to correlate with measures of fiscal policy in previous studies, such as Cameron (1978), Rodrik (1998), and Persson and Tabellini (1999). We will refer to this basic set of controls by X_1.

Depending on the specification, the dependent variable and the frequency of sampling, we have also included several other variables, such as the price of oil in US dollars, income shocks, measured either as the growth rate of real GDP or as the log difference between real GDP and its trend computed with the Hodrick-Prescott filter, and levels of government debt, as a percentage of GDP. We also use several sets of indicator variables, measuring geographic locations, colonial origins, and election dates. All these variables are defined more precisely in the data appendix.

Summary statistics Tables 3a and 3b display the correlation matrix between our main variables of interest. Table 3a shows cross-country correlations, with data averaged over the full period for which we have observations for each variable-country pair. Table 3b instead pools together all yearly observations for all countries. Both tables display a similar pattern of correlations. While the electoral rule appears uncorrelated with the socio-economic controls, the regime type is much more correlated with the level of development and the demographic structure, in line with our previous observation that most presidential regimes are outside the OECD countries. We also see that presidential regimes are associated with smaller governments and smaller social security and welfare spending, whereas majoritarian electoral rules are correlated with larger surpluses and smaller social security and welfare spending. These correlations are not inconsistent with the predictions summarized in Table 1.

4. Methodology

Our empirical analysis is certainly motivated by theory. We aim as much at establishing empirical regularities, however, as at testing hypotheses derived from specific models. That is, we would like to succinctly describe systematic relations in the data, establishing some stylized facts about the effect of institutions on policy outcomes. For this reason, we follow an eclectic approach.

A general formulation The regressions we estimate in the paper are all derived from the following general formulation:

$$y_{it} = \alpha_i + \boldsymbol{\gamma}_i \mathbf{s}_{it} + \boldsymbol{\beta}_i \mathbf{q}_t + \boldsymbol{\delta} \mathbf{x}_{it} + \boldsymbol{\eta} \mathbf{z}_i + u_{it} \ . \tag{1}$$

In (1), y_{it} denotes a specific policy outcome in country i in year t and Greek boldface letters denote vectors of unknown parameters to be estimated, possibly varying across countries or groups of countries. We allow for a country-specific average, α_i. Policy can be affected directly by the institutions $\mathbf{z}_{it}$, concretely the two dummy variables MAJ and $PRES$. It can also be affected by vectors of socio-economic control variables: $\mathbf{s}_{it}$ and $\mathbf{q}_t$ denote country-specific and common variables whose slope coefficients are allowed to vary, whereas the variables in $\mathbf{x}_{it}$ are instead constrained to have the same impact on all countries. Finally, u_{it} is an unobserved error term.

We want to test two sets of hypotheses. The first is whether institutions have a **direct** impact on policy outcomes, which is really what most of the theory discussed in Section 2 was about. The nul hypothesis corresponding to this question can be formulated as:

$$H_0^D : \boldsymbol{\eta} = 0 \ .$$

Cross-section regressions To see how we may test the first hypothesis, H_0^D, we take time averages of (4.1) within each country, and rewrite it as (a bar over a variable denotes a time average):

$$\bar{y}_i = (\alpha_i + \boldsymbol{\gamma}_i \bar{\mathbf{s}}_i + \boldsymbol{\beta}_i \bar{\mathbf{q}}) + \boldsymbol{\eta} \bar{\mathbf{z}}_i + \boldsymbol{\delta} \bar{\mathbf{x}}_i + \bar{u}_i \ . \tag{2}$$

Equation (4.1) can be estimated on cross sectional data with standard methods, with the estimated intercept capturing the effect of all variables within brackets. The t-statistic on $PRES$ and MAJ is then a test of the nul hypothesis H_0^D.

Time variation in the data Such cross-sectional estimates have the advantage of being closely related to some existing theories. But they do not exploit the time variation in the data. Moreover, they might be subject to simultaneity problems in the form of omitted-variable bias: some forces selecting political institutions in historical times may also drive economic policy outcomes. The institutional variation over time is too small to circumvent this problem by conventional fixed-effects estimation in panel data. For practical purposes, z_{it} is given by a constant, z_i, equal to the time average $\bar{z}_i$. Thus, we cannot separately estimate the effects on policy of a country's institutions, z_i, and other time-invariant, country-specific features, α_i.

For this reason we also ask a slightly different question, namely whether political institutions have an **indirect** (non-linear) effect on policy. In particular, we ask whether different electoral rules and political regimes induce different policy responses to economic and political events. Even if the cross-section results might possibly be plagued by simultaneity, it is much less plausible that the forces selecting the observed political institutions in historical times would be systematically correlated with the response to economic and political events during our recent sample period.

The nul hypothesis corresponding to this second question is whether countries with different values of z_i nevertheless have the same coefficients $\boldsymbol{\gamma}$ and $\boldsymbol{\beta}$ in (4.1):

$$H_0^I : \boldsymbol{\gamma}_i = \boldsymbol{\gamma}_j \quad \text{and/or} \quad \boldsymbol{\beta}_i = \boldsymbol{\beta}_j \quad \text{even if} \quad z_i \neq z_j \ .$$

Recall, however, that the specific theoretical contributions discussed in Section 2, either are all static, or have rather loose predictions concerning the link between institutions and policies. Most of our tests for indirect effects should thus be seen as a search for empirical regularities rather than tests of specific predictions.

Non-observable common events There are various ways of testing H_0^I, that is, the absence of an indirect effect of institutions. It is plausible that a set of common economic and political events have affected fiscal policy in all countries. We need only think about the worldwide turn to the left in the late 1960s and 70s, or the productivity slowdown and oil shocks in the 1970s and 80s. But suppose we do not want to commit to, or cannot observe, all such events. Blanchard and Wolfers (2000) suggest a simple statistical method for estimating how labor-market institutions might influence the adjustment of unemployment to unobservable shocks. Milesi-Ferretti, Perotti and Rostagno (2000) indeed apply this method to study how the proportionality of electoral systems affects policy in the OECD countries.

Assume that the response to observable country-specific variables is the same in all countries, $\gamma_i = \gamma_j$ in (1). Then we can lump all the variables in s_{it} together with those in x_{it} and rewrite (1) as:

$$y_{it} = (\alpha_i + \eta z_i) + (1 + \lambda(z_i - z))\beta q_t + \delta x_{it} + u_{it} \ . \qquad (3)$$

We can use a set of time dummies (one per time period) to estimate, βq_t, the common effect of the common events in (3). The institution-specific effect of common events q_t is proportional to the term $\lambda(z_i - z)$, where z is the cross-country average of z_i. The form of (3) tells us to estimate the crucial parameter λ by NLS, and include fixed effects, to pick up the country-specific intercept. We use both annual data and five-year averages. The latter may be more robust to measurement error and allow better for discretionary adjustments of policy than yearly data.

Observable economic events Yet another way of testing whether institutions induce different policy responses to shocks and other variables is to focus on specific observable events. These may be economic events, such as changes in the price of oil, or country income. To assess whether the impact of such common or country-specific events on policy outcomes depends on institutions, we can re-write (4.1) as:

$$y_{it} = (\alpha_i + \eta z_i) + \beta q_t + (\gamma + \mu z_i) s_{it} + \delta x_{it} + u_{it} \ . \qquad (4)$$

A finding that the coefficients μ differ from zero implies an indirect effect of institutions through these observable events. We use two basic estimation methods: (i) fixed effects estimation, to control for the first country-specific term on the right-hand side of (4); sometimes we jointly estimate spending, revenues and deficit equation by seemingly unrelated regressions; (ii) we take first differences to wipe this term out and then estimate by instrumental variables. We always include the lagged dependent variable y_{it-1} either in x_{it} or in s_{it}. [5] We also report some GLS

[5] As is well known, the presence of a lagged dependent variable can bias the fixed-effects estimator even if the error term is not correlated over time. But in panels where the time series dimension is as long as ours, the bias is rather small. Transforming the data to first differences removes the fixed effect error term, but may aggravate the correlation between the error term and the lagged dependent variable (see, for instance Baltagi, 1995, Ch 8). This is why when differencing we rely on instrumental variable estimation, where the instruments are the lagged explanatory variables (in differences) and the lagged dependent variable in level lagged twice, as suggested by Anderson and Hsiao, 1981 or Arrellano and Bond, 1991).

estimates of the difference specification (with no lagged dependent variable), to allow for heteroskedasticity and panel-specific autocorrelation in u_{it}.

Electoral cycles Finally, we test for an institution-dependent response to observable political events, in the form of elections. As we saw in Section 2, theory indicates that we should expect at least the electoral rule to affect the strength of the electoral cycle. For this purpose, we construct an indicator variable, EL_t, taking a value of 1 if there was an election in country i in year t, and 0 otherwise. For presidential regimes, the election date is that of the president, for parliamentary regimes it is that of the legislative assembly's lower house. We then expand $\mathbf{s}_{it}$, the vector of country-specific events, to include indicator variables for election years, EL_t, and post-election years, EL_{t-1}. Otherwise, the specification is identical to that in our tests for institution-dependent responses to economic events. The estimation methods are as described above, except that now the specification also includes a set of common time dummies, to allow a more precise estimation of the electoral cycle.

5. Results

In this section, we report the results obtained by applying the methodology discussed in the previous section to our three policy outcomes: the size of government, the composition of government spending, and the government deficit. We describe each policy outcome in turn.

5.1. Size of government

Cross-section regressions We begin with the cross-sectional regressions testing H_0^D for the presence of a direct effect of institutions on the size of government. The results are displayed in Table 4. The major dependent variable is expenditures by central government (Columns 1-3 and 7), but we also include results for central government revenue (Columns 4-5) and general government expenditure (Column 6). Every specification includes our basic set of controls X_1 and all but one also include dummies for continents and colonial origin. Every regression except the last one relies on data from the full length of the panel. Most regressions refer to our default sample of countries (a Gastil index strictly below 4, applied year by year), but two (Columns 3 and 5) refer to the Broad sample. All variables are measured in levels. The estimation method is Weighted Least Squares, where each country's weight is proportional to the length of its panel (the results for unweighted OLS regressions are similar).

The table displays the estimated η parameters for the PRES and MAJ dummies. Bracketed expressions are p-values for false rejection of $\eta = 0$. Boldface font denotes a coefficient significantly different from zero at the 10% level.

Our two institutional measures always enter with a negative sign. But the effect for MAJ is statistically insignificant in half the cases. The finding that majoritarian countries have smaller governments in terms of revenue, but not in terms of spending, turns out to reflect systematically smaller deficits (see Section 5.3 below). Evidence of a large and statistically robust negative effect of majoritarian elections is limited to general government expenditures. Note, however, that – due to data availability – the panel in this case is both shorter and restricted to a much smaller number of countries. The finding that majoritarian countries have smaller general governments is consistent with the findings by Milesi-Ferretti et al. (2000) for the OECD countries.

The presidential dummy variable is instead consistently significant, except in the case of general government where the sample includes considerably fewer presidential regimes. The finding that presidential regimes have smaller governments is clearly in line with the theoretical prediction in Section 2. According to the point estimates, the effect is substantial: about 5 percent of GDP. It appears to be slightly smaller in the larger sample, which corresponds to the broader definition of democracy.

As the last column shows, the negative effect of PRES is much stronger – above 10 percent of GDP – for cross sections based on data from the 1990s, rather than the whole sample. It is also statistically much more robust. These findings are consistent with the empirical results in Persson and Tabellini (1999), who considered data from around 1990. Together the findings suggest that the negative sign of the PRES dummy might largely reflect a faster growth of government in parliamentary regimes in the last four decades. As Figure 3 illustrates, this pattern is clearly visible already in the raw data. The graph is identical to Figure 2, except that the data is partitioned into presidential regimes, marked with black diamonds and a thick curve for the average, and parliamentary regimes, marked with circles and a thin curve.[6]

[6] The result that the estimated coefficient on PRES is larger in absolute value in the more recent cross sectional estimates is not due to a different sample of countries beeing included in later years compared to the early period, since it holds even if we hold the sample of countries fixed.

Unobserved common events Next, we turn to the time variation in the data, testing H_0^I for (the absence of) an institution-dependent reaction of the size of government to economic and political events. We begin with the effect of unobservable common events variables, using the specification in equation (3).

Table 5 displays selected results for expenditures and revenue as the dependent variable, for yearly data and five-year averages, and for the broad and default sample of countries. All variables are measured in levels and each specification includes country fixed effects on top of the basic controls in X_1. The first two rows in the table report the coefficients on the institutional variables: our estimates of $\boldsymbol{\lambda}$ in (3). The results remain similar if we extend the vector of observable controls to include the lagged dependent variable or income shocks, as in Table 6 below.

Both PRES and MAJ are negative and highly significant across all specifications. One way of interpreting the results is to consider a common event in some period t that raises government spending by 1 percent of GDP in an *average* country: i.e. an event corresponding to $\boldsymbol{\beta}(\mathsf{q}_t - \mathsf{q}_{t-1}) = 1$. Then, a coefficient of about -1 on PRES means that the effect is about 1.4 percent of GDP in parliamentary regimes, but only 0.4 percent in presidential regimes (recall that z_i in (3) is adjusted by the sample mean, which is about 0.4 for PRES). Similarly, the effect is $\frac{1}{3}$ of a percent smaller under majoritarian rather than proportional elections.

The estimated effects of the common events, the sequence of $\boldsymbol{\beta}\mathsf{q}_t$ in (3), are not reported. But they reflect the time pattern apparent from Figures 2 and 3: the estimated coefficients on the time dummies grow until the mid 1980s, then they remain constant or drop slightly. Their sign depend on the specification, but the coefficients are generally negative early on, and positive from the early 1970s onwards (since we include fixed effects, data are measured in deviations from country means). The negative parameter estimates reported in Table 4 thus say that whatever unobservable events caused the growth in government in the sample as a whole, their effect was significantly smaller in countries with presidential regimes and majoritarian elections.

This discussion suggests another way of gauging the results, namely by considering the *cumulative* effect of the common events over the course of the sample, as measured by $\boldsymbol{\beta}(\mathsf{q}_T - \mathsf{q}_1)$. This cumulative effect is positive on average (i.e., for the sample as a whole). The last two rows in the table show how much this cumulative effect differs across institutions, according to our point estimates. For government spending, the difference between presidential and parliamentary regimes is just above 10 percent of GDP, which well matches our estimate in the last column of Table 4 of a cross-sectional difference in the 1990s of just above 10 percent. The influence of the electoral rule is also statistically significant but quantitatively less important, between 3 and 6 percentage points of GDP, again about the same order of magnitude as in the cross sectional regressions.

Altogether, the results in Tables 4 and 5 convey a similar message. The size of government is strongly influenced by the political constitution. Proportional, parliamentary systems spend the most, while presidential regimes and systems with majoritarian electoral rules for electing the legislature spend the least. The effect of the regime type is larger and more robust than that of the electoral rule.

Observable economic events We now ask whether the impact of observable determinants on the size of government depends on institutions. Thus, we interact our institutional measures with observable economic variables, s_{it}, to estimate the interaction terms $\boldsymbol{\mu}$ in equation (4). The variables in s_{it} consist of the lagged dependent variable and the deviation of income from its (Hodrick-Prescott) trend. We also tried to interact institutions with other socio-economic common and country-specific control variables, such as the oil price or the proportion of population above 65 years of age. Some of these interaction terms were occasionally statistically different from zero, but the results were not robust to specification or estimation method, though they reinforce the general message described below. The reported results are instead very robust to estimation methods, samples and measurements (in particular, we also measured income shocks as the yearly growth rate in income, and obtained similar findings)

Table 6 displays the results, for different measures of the size of government and for the three estimation methods discussed in Section 4. Most reported results are very stable across estimation methods. The vector of other controls X_2 not reported in the table includes the same basic variable as in the previous tables, plus the oil price and the trend of aggregate real income from which the shock is computed. Time-dummy variables, colonial origin and continental dummy variables are not included in the regression. A P* in front of a variable denotes that the variable is interacted with the PRES dummy variable, while a M* denotes interaction with the MAJ dummy.

Again, we find that institutions matter a lot. Consider the first three columns. In proportional and parliamentary countries, income shocks affect central government spending as a proportion of GDP. The estimated coefficient of YSHOCK is negative and around -0.2, meaning that a 10% drop in real income induces a rise in the spending ratio of 2 percentage points. This reaction of spending to income shocks could reflect entitlement programs whose outlays are fixed in cash terms, or perhaps even inversely related to income. More generally, it suggests that government outlays do not move in proportion to aggregate income. When the size of government is measured by revenues, rather than by spending, the estimated coefficient drops in absolute value, but remains negative and statically significant. [7] Since spending and revenue are highly serially correlated, this effect persists over time. By contrast, presidential and majoritarian countries are not affected by the income shock; in presidential countries it appears that spending could even be pro-cyclical. And serial correlation of the size of government is significantly smaller, particularly among presidential regimes. Both findings could be interpreted as suggesting that entitlement programs are less important in these countries, or more generally that aggregate spending and taxation move proportionally with income. When turning to other estimation methods (columns 4 and 5), the results on the income shocks stand, but the estimated autocorrelation coefficient in spending drops and differences across institutions disappear. This last finding is important, as this coefficient could be biased in the level-specification due to the panel structure of the data.

The last column disaggregates the effect of income shocks into positive (YSHOCK_POS) and negative (YSHOCK_NEG) shocks. An asymmetry is apparent. Only negative income shocks have a statistically significant effect on the spending ratio, and their estimated coefficient is much larger in absolute value. This asymmetric effect suggests that a ratchet effect might be in place. A negative income shock induces a lasting expansion in the size of government, which is not undone when income grows above potential. But this effect is not present in presidential or majoritarian countries, in which a ratchet effect instead appears to be associated with positive income shocks. This difference across constitutional forms could contribute to the faster growth of government in parliamentary and proportional democracies described in the previous subsections. But this is just a conjecture, and institution-dependent ratchet effects certainly deserve more attention in future research.

Electoral cycles Finally, we ask whether there is an electoral cycle in spending or revenue, whether it occurs before or after the elections, and whether its magnitude depends on institutions. As explained in Section 4, we estimate the same specifications as those underlying Table 6, except that we expand s_{it} with

[7] Here estimation is by seemingly unrlated regressions (SUR), with spending and revenue jointly estimated

indicator variables for current and lagged elections. We now drop the price of oil from the specification, and include instead time dummy variables, so as to identify the effect of elections more precisely. PRES and MAJ are still interacted with the lagged dependent variable and with YSHOCK, as in Table 6. When estimating in levels, we jointly estimate spending and revenues equations by seemingly unrelated regressions (SUR).

Table 7 reports the results. There is a strong electoral cycle in spending and taxation, but it takes a very different form in presidential and parliamentary democracies.[8] There is strong evidence that presidential regimes postpone fiscal adjustments until after the election. Nothing of statistical significance happens during the election year. But once the election is over, spending is cut by almost 1 percent of GDP and revenues are hiked by about 0.5 percent of GDP. In parliamentary regimes, on the other hand, the electoral cycle is observed during the election year. Revenues are cut by about 0.3% at the time of elections, while government spending does not seem to be affected by the election date. Contrary to some predictions of the theory, the electoral rule is not associated with significantly different patterns of electoral cycles (cf. the last column).

To understand why presidential regimes display a post-election spending and revenue cycle, while parliamentary countries have a pre-election revenue cycle is an interesting issue for further work.

5.2. Composition of spending

We now turn to the composition of government. Recall that our two measures of composition include central government spending on social security and welfare, either as a percent of GDP (SSW/GDP), or as a ratio to central government spending on goods and services (SSW/GDS). As the methodological considerations closely follow those in the previous subsection, we keep the discussion of our results more brief.

Cross-section regressions We start with cross-sectional tests for a direct effect of institutions. Estimation results are shown in Table 8 for both our measures

[8] Earlier studies on international data conducted with different methodologies had typically not found evidence of an electoral cycle (see Alesina, Roubini and Cohen, 1997 for a summary). An exception is the recent study by Shi and Svensson (2000), who use panel data for over 100 countries and find significant electoral cycles in spending, revenues and government deficits. But they only serach for pre-election cycles and do not explore institutional differences across countries.

of composition. Note that the full sample here is restricted to the period from 1972. The results indicate that broad, non-targeted programs are indeed systematically smaller under majoritarian elections, as predicted by the theory discussed in Section 2. *Ceteris paribus*, social security and welfare spending is smaller by 1-2 percentage points, when measured as a percentage of GDP, and about 0.20-0.40 points lower, when measured as a ratio to spending on goods and services (in this latter case, the dependent variable takes values close to 1 on average). Statistically, these results are more fragile to the sample and the inclusion of socio-economic controls than were the results for overall spending. Qualitatively, they are in line with the findings of Milesi-Ferretti et al (2000) for the OECD countries.

Unlike for the size of government, however, we find no discernible effect of the regime type on our measures of composition.

Unobservable common events What about the indirect effects of institutions? Results from our estimates of the adjustment to common unobservable events are collected in Table 9. As in the case of overall spending, we find a strong and significant influence of political institutions. Now both the electoral rule and the regime type matter. Unobservable common events have a smaller effect on the spending ratio (SSW/GDS) under majoritarian elections and under presidential regimes. When social security and welfare is measured as a share of GDP, the estimated effect of presidential regimes is particularly relevant, with a cumulative difference of about 5 percent of GDP. The last result can be interpreted as evidence of more rapid growth of welfare-state spending in parliamentary than in presidential regimes. This in turn may explain the above finding of income shocks having a larger impact on overall spending in parliamentary regimes. Finally, note that the influence of political institutions appears weaker in the broader sample of democracies. A likely reason is that this broad sample includes a number of developing countries, where the welfare state is too small to be meaningfully compared to the larger welfare states of more advanced societies.

Observable economic events Table 10 summarizes results regarding the adjustment to observable economic events. Here we only report results on social security and welfare as a share of GDP, as the results for SSW/GDS are less robust. The pattern of estimated coefficients resembles the one obtained in Table 6 for the overall size of government. Presidential and majoritarian regimes have a dampened reaction to income shocks, and less persistence, compared to parliamentary proportional regimes. The result on persistence is less robust across estimation methods, however, as already found in Table 6. Moreover, comparing these estimates with those in Table 6, income shocks have a smaller impact on this component of the budget than on the overall budget size, suggesting that there are other spending items whose cash outlays are fixed and do not react to income shocks.

Electoral cycles Do we find a systematic effect of elections on the composition of spending? The answer is positive, but with some important differences relative to our findings on the overall size of government. [9] As Table 11 shows, the post-election cycle in presidential regimes can be detected in only some specification or estimation methods. On the other hand, parliamentary regimes now display a statistically significant pre-election cycle in this component of spending (about 0.2 percent of GDP), which continues once the election is over. But this hike in social security spending is present only in proportional, parliamentary systems. Although the estimates are not entirely stable across samples and estimation methods, our results suggest quite a subtle pattern. In presidential regimes, spending on social security falls after the elections, as painful adjustments seem to be delayed. In parliamentary regimes, on the other hand, program expansions seem to take place during election years. In proportional parliamentary regimes favors granted during the electoral campaign are sustained after the elections.

We find these results intriguing: without taking explicit account of electoral rules and political regimes, we would not have discovered these systematic patterns in the data. The greater reliance on social-security spending around election time is perhaps plausible if – as in the theory discussed in Section 2 – politicians indeed have greater overall incentives to seek electoral support with broad programs in proportional systems. But it remains to work out the details – and auxiliary predictions – of such a theory.

5.3. Deficits

In earlier subsections we have investigated the short-run responses to economic and political events of central government spending and revenue. As the central government surplus (deficit) is equal to the difference between these two

[9] When estimating by SUR, the SSW/GDP equation is jointly estimated with the corresponding equation on the size of government. .

aggregates, it is natural analyze how the same events manifest themselves in the government surplus. Thus, we have carried out the same kind of test battery for the government surplus as for the other policy measures. To save space, we try to summarize the results as succinctly as possible.

Cross-section regressions The cross-sectional estimates in Table 12 indicate that average deficits are smaller in countries with either presidential regimes or majoritarian elections. The effect of the electoral system is considerably more robust to inclusion of regional and colonial dummies, however. Consistent with our findings on spending and revenue in Table 4, the estimates imply a smaller average deficit by 1.5 to 2 percent of GDP under plurality rule. As suggested in section 2, possible explanation could be a greater reliance on majority single-party governments – and therefore less inertia in adjustment – under this electoral formula.

Unobservable common events The results from NLS estimation of the adjustment to unobservable common events are displayed in Table 13. The estimates indeed indicate that these events have smaller effects, not only under majoritarian elections but also in presidential regimes. Thus, an unobservable common event that raises the average country's deficit by 1 percent of GDP, has an effect about 0.5 percent smaller in presidential (vs. parliamentary) regimes and under majoritarian (vs. proportional) elections.

Observable economic events We estimate the effect of observable events on the surplus using two different specifications. In one we enter the right-hand-side variables in level forms and include the lagged levels of spending and revenue plus the corresponding interactions terms. Here, we estimate the surplus equation either on its own or by SUR together with spending and revenue. In the second specification we enter the independent variables in first-difference form and include the lagged deficit. Then, we estimate by IV, using spending, revenue and the deficit all lagged twice as instruments.[10]

Table 14 shows the results. As expected, we find that the surpluses are procyclical – they go up with positive income shocks – in the average country. But presidential regimes are different, with acyclical or even countercyclical surpluses.

[10] Both these specifications can be derived from a model formulation where the surplus is defined as the difference between revenue and spending, if we assume that spending (revenue) adjusts downwards (upwards) in periods when lagged debt is above its steady state-value.

Majoritarian elections seem to have a similar effect, albeit not as strong and robust. Finally, deficits in presidential regimes appear to have much less inertia (more mean reversion) than in parliamentary regimes. Majoritarian elections modify the dynamics in a similar way, but, again, not as strongly. These results are consistent with the results for spending and revenue shown in Table 6. But the puzzle remains as to what drives these differences in policy behavior across political institutions.

Electoral cycles Finally, we look for evidence of institution-dependent electoral cycles. As Table 15 shows , we find a post-election cycle: improvements in the surplus on the order of 0.5-1% points of GDP are postponed until the year after the election. Again, this electoral cycle is present only in presidential regimes, as already found with government spending. There is no evidence of a pre-election deficit cycle in parliamentary regimes. Neither is there any systematic influence of the electoral rule in these regimes (results not shown in the Table).

6. Conclusion

Do political institutions shape economic policy? Our empirical results summarized in Table 16, strongly suggest that the answer is yes. Empirically, presidential regimes are associated with smaller governments than parliamentary regimes, a smaller response of spending to income shocks, and a stronger post-election cycle but a weaker pre-election cycle, and smaller budget deficits. Majoritarian elections are associated with smaller welfare programs than proportional elections; they also have smaller spending responses to events, a stronger pre-election cycle in taxes and smaller budget deficits. Several of these empirical regularities, those marked in black and bold, are in line with the first wave of theory discussed in Section 2. But others, marked in gray and bold, are still awaiting a satisfactory theoretical explanation. This applies particularly to the results indicating institution-dependent adjustments of policy to different events and the results regarding the deficit.

These are promising first steps in a research program. Much work certainly remains, however. One direction is clearly to refine the theory of policy. As noted, the empirical results on the adjustment of spending are in search of a theory. To understand them, we need dynamic rather than static models of institutions and policy. Dynamic models are obviously also necessary to understand the behavior of government deficits.

On the policy side, we have concentrated on government spending. It would be interesting, and certainly feasible, to study other policy instruments — such as the structure of taxation, including trade policy — with similar methods. On the institutional side, one should study the effect on policy of more detailed constitutional features; for instance, different types of checks and balances, or different types of confidence requirements.

This suggests another direction of research, namely refined **measurement** of political institutions. In some cases, this will involve a mere, but time-consuming, compilation of data from existing sources. One example would be to collect panel data for continuous measures of the two aspects of the electoral rule discussed in Section 2: district size and the electoral formula. In other cases, better measures will require the collection of new primary data. An example would be to try and find continuous or multidimensional measures of checks and balances in different political regimes.[11] As this may be a labor-intensive and open-ended task, it is important to use theory as a guide.

Some **econometric issues** certainly need to be explored in more detail. Even with refined measurement, considerable measurement error will remain in our data. Sharper theory would help trade off the prospective biases due to measurement and specification errors. Sharper hypotheses, derived from dynamic models, would also help avoid the pitfalls of estimation in dynamic panels.

All in all, a close interplay of theory, measurement and statistical work appears essential for making progress on the broad questions dealt with in this paper.

7. Appendix

Data description to be added

[11] Attempts to construct such measures have been made by Beck et al (1999) and Shugart and Carey (1992).

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Table 1
Summary of Theory

	PRES (vs. *PARL*)	*MAJ* (vs. *PR*)
Size	–	– /?
Composition (broad vs. narrow)	–	–
Electoral Cycle	NA	+/ NA
Reaction to shocks	NA	NA
Budget deficits	NA	–

Table 2
Sample of countries

	Narrow	Default	Broad
USA	1960-98	1960-98	1960-98
UK	1960-98	1960-98	1960-98
AUSTRIA	1960-98	1960-98	1960-98
BELGIUM	1960-98	1960-98	1960-98
DENMARK	1960-98	1960-98	1960-98
FRANCE	1960-98	1960-98	1960-98
GERMANY	1960-98	1960-98	1960-98
ITALY	1960-98	1960-98	1960-98
LUXEMBOURG	1960-98	1960-98	1960-98
NETHERLANDS	1960-98	1960-98	1960-98
NORWAY	1960-98	1960-98	1960-98
SWEDEN	1960-98	1960-98	1960-98
SWITZERLAND	1960-98	1960-98	1960-98
CANADA	1960-98	1960-98	1960-98
JAPAN	1960-98	1960-98	1960-98
FINLAND	1960-98	1960-98	1960-98
GREECE	1975-98	1975-98	1960-98
ICELAND	1960-98	1960-98	1960-98
IRELAND	1960-98	1960-98	1960-98
MALTA	1988-98	1960-98	1960-98
PORTUGAL	1977-98	1977-98	1960-98
SPAIN	1978-98	1978-98	1960-98
TURKEY	-	-	1960-98
AUSTRALIA	1960-98	1960-98	1960-98
NEW ZEALAND	1960-98	1960-98	1960-98
ARGENTINA	-	1983-98	1960-98
BOLIVIA	-	1982-98	1960-98
BRAZIL	-	1980-98	1960-98
CHILE	1991-98	1960-73 1989-98	1960-98
COLOMBIA	-	1960-98	1960-98
COSTA RICA	1960-98	1960-98	1960-98
DOMINICAN RE	-	1960-98	1960-98
ECUADOR	-	1979-98	1960-98
EL SALVADOR	-	1960-77 1986-98	1960-98

GUATEMALA	-	1960-79	1960-98
HONDURAS	-	1980-98	1960-98
MEXICO	-	1996-98	1960-98
NICARAGUA	-	-	1960-98
PARAGUAY	-	1990-98	1960-98
PERU	-	1981-98	1960-98
URUGUAY	1986-98	1985-98	1960-98
VENEZUELA	1971-91	1960-98	1960-98
BAHAMAS	1960-98	1960-98	1960-98
BARBADOS	1960-98	1960-98	1960-98
BELIZE	1960-98	1960-98	1960-98
ST.VINCENT&G	1960-98	1960-98	1960-98
TRINIDAD&TOB	1960-98	1960-98	1960-98
CYPRUS (G)	1983-98	1960-74 1980-98	1960-98
ISRAEL	1960-98	1960-98	1960-98
SRI LANKA	-	1960-89	1960-98
INDIA	-	1960-98	1960-98
MALAYSIA	-	1960-76	1960-98
NEPAL	-	1981-98	1960-98
PHILIPPINES	-	1985-98	1960-98
SINGAPORE	-	1981-98	1960-98
THAILAND	-	1960-98	1960-98
BOTSWANA	1990-98	1960-98	1960-98
GAMBIA	-	1960-98	1960-98
MAURITIUS	1983-98	1960-98	1960-98
FIJI	1960-86	1960-87 1992-98	1960-98
PAPUA N.GUIN	1960-86	1960-98	1960-98

Narrow refers to countries with a Gastil index of political right less than or equal to 2, year by year
Default refers to countries with a Gastil index of political right less or equal to 3.5, year by year.
Broad refers to countries with a Gastil index of political right less than or equal to 5, art an initial year.

Table 3a

Partial Correlations

Cross sections

	CGEXP	*SURPLUS*	*SSW/GDS*	*LYH*	*GASTIL*	*TRADE*	*PROP1564*	*PROP65*	*PRES*
SURPLUS	- 0.29								
SSW/GDS	0.47	- 0.04							
LYH	0.46	0.02	0.71						
GASTIL	- 0.60	0.04	- 0.56	- 0.73					
TRADE	0.32	0.27	- 0.13	0.07	- 0.07				
PROP1564	0.44	- 0.02	0.72	0.76	- 0.61	0.17			
PROP65	0.56	- 0.11	0.82	0.80	- 0.71	- 0.04	0.82		
PRES	- 0.60	0.09	- 0.28	- 0.48	0.58	- 0.36	- 0.56	- 0.50	
MAJ	- 0.03	0.23	- 0.27	- 0.12	- 0.02	0.23	- 0.06	- 0.22	- 0.24

Table 3b

Partial Correlations

Pooled yearly data

	CGEXP	*SURPLUS*	*SSW_GDS*	*GROWTH*	*LYH*	*GASTIL*	*TRADE*	*PROP1564*	*PROP65*	***PRES***
SURPLUS	- 0.41									
SSW/GDS	0.47	- 0.08								
GROWTH	- 0.15	0.15	- 0.18							
LYH	0.49	0.01	0.65	- 0.11						
GASTIL	- 0.46	0.08	- 0.47	0.14	- 0.59					
TRADE	0.32	0.13	- 0.13	0.10	0.13	- 0.03				
PROP1564	0.44	- 0.01	0.60	- 0.12	0.76	- 0.48	0.19			
PROP65	0.56	- 0.08	0.79	- 0.16	0.79	- 0.59	0.02	0.78		
PRES	- 0.49	0.07	- 0.21	- 0.05	- 0.45	0.46	- 0.35	- 0.47	- 0.47	
MAJ	- 0.05	0.12	- 0.28	0.05	- 0.04	0	0.16	- 0.02	- 0.17	- 0.26

Table 4
Size of Government
Cross Sections

Dep. variable		Central Spending		Central Revenue		General Spending	Central Spending
Sample	1960-98	1960-98	1960-98 Broad	1960-98	1960-98 Broad	1972-98	1990-95
Estimation				WLS			
PRES	**- 7.95** **(.005)**	**- 6.28** **(.073)**	- 5.44 (.106)	**- 6.14** **(.038)**	**- 4.98** **(.080)**	- 6.62 (.161)	**- 10.92** **(.011)**
MAJ	-2.98 (.178)	**- 4.62** **(.052)**	**- 3.89** **(.095)**	- 2.80 (.151)	- 1.80 (.338)	**- 9.36** **(.029)**	- 2.94 (.246)
Controls	X_1	X_1 Cont.&Col.	X_1 Cont.&Col.	X_1 Cont.&Col.	X_1 Cont.&Col.	X_1 Cont.&Col.	X_1 Cont.&Col.
# Obs.	1519	1445	1789	1420	1756	457	251
# Countries	59	58	61	57	60	36	53
R^2	0.54	0.64	0.64	0.71	0.72	0.84	0.73

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* (see the text and Appendix).
Cont. and Col. refer to two sets of dummies for continents and colonial origin, respectively (see the Appendix).

Table 5
Size of Government
Unobservable Common Events 1960-98

Dep. variable			Central Spending			Central Revenue	
Sampling	Yearly	Yearly	Yearly	Yearly Broad	5-y avg.	Yearly	Yearly Broad
Estimation			NLS, FE				
PRES	**- 0.91**		**- 0.99**	**- 0.71**	**- 1.09**	**- 1.42**	**- 0.79**
	(.000)		**(.000)**	**(.000)**	**(.000)**	**(.000)**	**(.000)**
MAJ		**- 0.29**	**- 0.43**	**- 0.40**	**- 0.35**	**- 0.47**	**- 0.37**
		(.000)	**(.000)**	**(.000)**	**(.007)**	**(.000)**	**(.000)**
$\beta*(q_T - q_1)*$ *PRES*	**- 12.73**		**- 13.46**	**- 11.09**	**- 9.05**	**- 7.17**	**- 6.60**
$\beta*(q_T - q_1)*$ *MAJ*		**- 2.99**	**- 5.84**	**- 6.24**	**- 2.90**	**- 2.37**	**- 3.09**
Controls	X_1	X_1	X_1	X_1	X_1	X_1	X_1
# Obs.	1519	1519	1519	1871	328	1492	1836
R^2	0.87	0.86	0.87	0.85	0.91	0.88	0.87

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* (see the text and Appendix). All the equations include a set of country dummies.

Table 6

Size of Government

Observable Economic Events 1960-98

Dep. Variable	Central Spending		Revenue	Central Spending		
Sampling			Yearly			
Estimation	FE Levels	FE, SUR levels		IV Diffs.	GLS diffs.	FE Levels
LAG_SIZE	**0.84** **(.000)**	**0.83** **(.000)**	**0.83** **(.000)**	**0.67** **(.002)**		**0.84** **(.000)**
P∗LAG_SIZE	**- 0.29** **(.000)**	**- 0.28** **(.000)**	**- 0.25** **(.000)**	- 0.35 (.318)		**- 0.29** **(.000)**
M∗LAG_SIZE	**- 0.05** **(.073)**	- 0.04 (.115)	**- 0.04** **(.040)**	-0.12 (.804)		**- 0.05** **(.055)**
YSHOCK	**- 0.19** **(.000)**	**- 0.19** **(.000)**	**- 0.07** **(.092)**	**- 0.24** **(.002)**	**- 0.24** **(.000)**	
P∗YSHOCK	**0.27** **(.000)**	**0.29** **(.000)**	**0.09** **(.058)**	**0.32** **(.000)**	**0.30** **(.000)**	
M∗YSHOCK	**0.23** **(.000)**	**0.23** **(.000)**	**0.11** **(.020)**	**0.21** **(.001)**	**0.12** **(.001)**	
YSHOCK_POS						- 0.11 (.263)
P∗YSHOCK_POS						**0.28** **(.012)**
M∗YSHOCK_POS						**0.27** **(.013)**
YSHOCK_NEG						**- 0.26** **(.007)**
P∗YSHOCK_NEG						**0.26** **(.019)**
M∗YSHOCK_NEG						**0.20** **(.070)**
Controls	X_2	X_2	X_2	X_2	X_2	X_2
# Obs.	1475	1432	1432	1421	1472	1475
R^2	0.81	0.95	0.96			0.81

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.

P and *M* denote interaction with the *PRES* and *MAJ* dummies, respectively

X_2 includes the same variables as X_1, plus the income trend corresponding to *YSHOCK* (see the text and Appendix).

R^2 in the fixed-effects regressions refers to the within estimator.

Table 7
Size of Government
Electoral Cycles 1960-95

Dep. variable	Central Spending			Central revenue		
Sampling	Broad			Broad		
Estimation	FE, SUR levels	FE, SUR levels	IV diffs.	FE, SUR levels	FE, SUR levels	IV diffs.
$PRES*EL_t$	0.10	0.46	- 0.23	- 0.30	- 0.11	- 0.70
	(.784)	(.180)	(.563)	(.328)	(.662)	(.164)
$PRES*EL_{t-1}$	**- 0.80**	**- 0.98**	**- 1.00**	**0.52**	**0.47**	**0.87**
	(.031)	**(.004)**	**(.015)**	**(.095)**	**(.058)**	**(.021)**
$PARL*EL_t$	- 0.03	- 0.02	- 0.17	**- 0.31**	**- 0.37**	- 0.26
	(.899)	(.932	(.475)	**(.066)**	**(.019)**	(.332)
$PARL*EL_{t-1}$	- 0.11	- 0.21	- 0.24	0.15	0.06	0.12
	(.565)	(.345)	(.307)	(.366)	(.692)	(.633)
$MAJ*EL_t$						- 0.50
						(.258)
$MAJ*EL_{t-1}$						0.56
						(.159)
Controls	X_3	X_3	X_3	X_4	X_4	X_4
# Obs.	1350	1670	1339	1350	1670	1316
R^2	0.95	0.94		0.96	0.96	

Broad refers to the less restrictive definition of a democracy (see Table 2). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
EL_t and EL_{t-1} are dummy variables for the election and post-election years, respectively.
X_3 includes the same variables as X_2 plus all the variables (including the interaction terms) in column 1 of *Table 6* except *OIL*, plus a set of year dummies; X_4 is constructed as X_3 but with lagged central revenue taking the place of lagged central spending (see the text and Appendix).

Table 8
Composition of Government
Cross Sections 1972-98

Dep. Variable	SSW/GDP			SSW/GDS		
Sample			Broad			Broad
Estimation			WLS			
PRES	- 0.03	- 2.13	- 0.75	0.15	0.13	0.22
	(.982)	(.229)	(.642)	(.442)	(.591)	(.323)
MAJ	- 1.54	**- 2.41**	- 1.86	- 0.25	**- 0.47**	**- 0.35**
	(.137)	**(.062)**	(.122)	(.117)	**(.022)**	**(.050)**
Controls	X_1	X_1 Cont.&Col.	X_1 Cont.&Col.	X_1	X_1 Cont.&Col.	X_1 Cont.&Col.
# Obs.	901	865	1063	881	845	1040
# Countries	55	54	59	53	52	57
R^2	0.77	0.80	0.79	0.69	0.74	0.74

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* (see the text and Appendix).
Cont. and Col. refer to two sets of dummies for continents and colonial origin, respectively (see the Appendix).

Table 9
Composition of Government
Unobservable Common Events 1972-98

Dep. variable	SSW/GDP			SSW/GDS		
Sampling	Yearly	Yearly	Yearly Broad	Yearly	Yearly	Yearly Broad
Estimation	NLS, FE					
PRES	**- 0.63**	**- 0.66**	**- 0.69**		**- 0.38**	**- 0.18**
	(.000)	**(.000)**	**(.000)**		**(.002)**	**(.089)**
MAJ		**- 0.14**	**- 0.12**	**- 0.28**	**- 0.24**	**- 0.20**
		(.028)	**(.080)**	**(.017)**	**(.023)**	**(.056)**
$\beta*(q_T - q_1)*$ *PRES*	**- 4.70**	**- 4.92**	**- 4.04**		**- 0.13**	**- 0.07**
$\beta*(q_T - q_1)*$ *MAJ*		**- 1.04**	**- 0.70**	**- 0.14**	**- 0.20**	**- 0.08**
Controls	X_1	X_1	X_1	X_1	X_1	X_1
# Obs.	901	901	1104	881	881	1081
R^2	0.96	0.96	0.96	0.95	0.95	0.95

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* (see the text and Appendix). All the equations include a set of country dummies.

Table 10
Composition of Government
Observable Economic Events 1972-98

Dep. variable	SSW/GDP						
Sampling	Yearly	Yearly	Yearly Narrow	Yearly Broad	Yearly	Yearly Narrow	Yearly Broad
Estimation	FE, levels	FE, SUR levels	FE, SUR, levels	FE, SUR levels	IV, diff.	IV, diff.	IV, diff.
LAG_COM	**0.81**	**0.81**	**0.81**	**0.80**	**0.39**	**0.39**	**0.33**
	(.000)	**(.000)**	**(.000)**	**(.000)**	**(.001)**	**(.001)**	**(.009)**
P∗LAG_COM	-0.03	- 0.05	- 0.03	- 0.04	- 0.58	0.06	**- 0.63**
	(.524)	(.180)	(.556)	(.212)	(.182)	(.862)	**(.030)**
M∗LAG_COM	**-0.06**	**- 0.07**	- 0.04	- 0.04	**- 0.49**	- 0.32	- 0.22
	(.027)	**(.004)**	(.128)	(.122)	**(.076)**	(.201)	(.430)
YSHOCK	**- 0.11**	**- 0.11**	**- 0.13**	**- 0.08**	**- 0.10**	**- 0.12**	**- 0.64**
	(.000)	**(.000)**	**(.000)**	**(.000)**	**(.000)**	**(.001)**	**(.005)**
P∗YSHOCK	**0.05**	**0.05**	0.04	**0.06**	**0.08**	**0.09**	**0.03**
	(.026)	**(.022)**	(.303)	**(.001)**	**(.000)**	**(.016)**	**(.055)**
M∗YSHOCK	**0.07**	**0.07**	**0.07**	0.03	**0.09**	**0.10**	**0.04**
	(.002)	**(.001)**	**(.029)**	(.114)	**(.000)**	**(.001)**	**(.037)**
Controls	X_2	X_2	X_2	X_2	X_2	X_2	X_2
# Obs.	847	847	616	1031	789	578	953
R^2	0.77	0.99	0.98	0.98	0.05	0.05	0.03

Broad refers to the less restrictive definition of a democracy (see Table 2). *p*-values in brackets. SUR is jointly estimated with CGEXP.
Boldface fonts denote significance at the 10% level.
P and *M* denote interaction with the *PRES* and *MAJ* dummies, respectively
X_2 includes the same variables as X_1, plus the income trend corresponding to *YSHOCK*
R^2 in the fixed-effects regressions (columns 1, 2, 4 and 5) refers to the within estimator.

Table 11

Composition of Government

Electoral Cycles 1972-95

Dep. variable	SSW/GDP						
Sample			Narrow	OECD		Narrow	
Estimation	FE levels	FE, SUR levels	FE, SUR Levels	FE, SUR levels	IV diffs.	IV diffs.	GLS diffs.
$PRES*EL_t$	0.04	0.04	0.06	0.14	- 0.14	- 0.26	**- 0.19**
	(.780)	(.753)	(.774)	(.589)	(.355)	(.321)	**(.003)**
$PRES*EL_{t-1}$	- 0.19	- 0.19	**- 0.39**	- 0.16	- 0.19	**- 0.52**	**- 0.19**
	(.225)	(.193)	**(.067)**	(.543)	(.207)	**(.019)**	**(.005)**
$PARL*EL_t$	**0.20**	**0.20**	**0.25**	**0.23**	**0.23**	**0.26**	**0.12**
	(.043)	**(.033)**	**(.011)**	**(.012)**	**(.012)**	**(.014)**	**(.052)**
$PARL*EL_{t-1}$	**0.21**	**0.21**	**0.26**	**0.23**	0.15	0.15	**0.14**
	(.034)	**(.025)**	**(.007)**	**(.011)**	(.110)	(.150)	**(.024)**
$MAJ*EL_t$	**- 0.29**	**- 0.29**	**- 0.40**	- 0.29	- 0.21	**- 0.28**	- 0.11
	(.060)	**(.048)**	**(.020)**	(.119)	(.129)	**(.094)**	(.133)
$MAJ*EL_{t-1}$	- 0.23	- 0.23	**- 0.29**	- 0.11	- 0.04	- 0.06	**- 0.15**
	(.137)	(.115)	**(.089)**	(.573)	(.761)	(.742)	**(.046)**
Controls	X_5	X_5	X_5	X_5	X_6	X_6	X_7
# Obs.	806	806	587	463	751	550	805
R^2	0.80	0.99	0.99	0.99	0.18	0.21	

Broad refers to the less restrictive definition of a democracy (see Table 2). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
SUR regression is estimated jointly with *CGEXP*. EL_t and EL_{t-1} are dummy variables for the election and post-election years, respectively.
X_5 includes the same variables as X_2 plus all the variables (including the interaction terms) in column 1 of *Table 9*, except *OIL*, plus a set of yearly dummies; X_6 includes the same variables as X_5 except the lagged dependent variable; X_8 includes the same variables as X_2 plus all the variables (including the interaction terms) in column 4 of *Table 9*, except *OIL*, plus a set of yearly dummies (see the text and the Appendix).
R^2 in the fixed-effects regression (column 1) refers to the within estimator.

Table 12
Government Surplus
Cross Sections 1960-98

Sample				Broad		Broad
Estimation			WLS			
PRES	**2.38**	**1.90**	0.67	0.78	0.37	1.52
	(.018)	**(.092)**	(.593)	(.515)	(.795)	(.213)
MAJ	1.14	1.32	**1.70**	**1.84**	**1.82**	**1.68**
	(.177)	(.153)	**(.059)**	**(.034)**	**(.069)**	**(.073)**
Controls	X_1	X_8	X_1 Cont.&Col.	X_1 Cont.&Col.	X_8 Cont.&Col.	X_8 Cont.&Col.
# Obs.	901	1015	1472	1800	1015	1238
# Countries		56	58	61	56	59
R^2	0.96	0.39	0.41	0.37	0.51	0.44

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* ; X_8 is identical to X_1 plus the level of net debt as a share of GDP (see the text and Appendix).
Cont. and Col. refer to two sets of dummies for continents and colonial origin, respectively (see the Appendix).

Table 13

Government Surplus

Unobservable Common Events 1960-98

Sampling	Yearly	Yearly	Yearly	Yearly Broad	5-y avg.	5-y avg. Broad
Estimation			NLS, FE			
PRES	**- 0.53** **(.000)**		**- 0.60** **(.000)**	**- 0.52** **(.000)**	**- 0.58** **(.070)**	**- 0.50** **(.022)**
MAJ		- 0.14 (.236)	**- 0.31** **(.005)**	**- 0.34** **(.001)**	- 0.44 (.102)	**- 0.49** **(.027)**
Controls	X_1	X_1	X_1	X_1	X_1	X_1
# Obs.	1499	1499	1542	1879	334	405
R^2	0.51	0.50	0.51	0.48	0.55	0.55

Broad refers to the less restrictive definition of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
X_1 includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 65* (see the text and Appendix). All equations include a set of country dummies.

Table 14
Government Surplus
Observable Economic Events 1960-98

Sampling			Yearly			
			Broad	Narrow		Narrow
Estimation	FE, levels	FE, SUR Levels	FE, SUR Levels	FE, SUR levels	IV diffs.	IV diffs.
LAG_SUR					**0.86**	**0.87**
					(.000)	**(.000)**
P∗LAG_SUR					- 0.05	**- 0.28**
					(.245)	**(.019)**
M∗LAG_SUR					- 0.03	**- 0.10**
					(.321)	**(.056)**
LAG_EXP	**- 0.53**	**- 0.61**	**- 0.60**	**- 0.68**		
	(.000)	**(.000)**	**(.000)**	**(.000)**		
P∗LAG_EXP	**0.26**	**0.26**	**0.10**	**0.29**		
	(.000)	**(.000)**	**(.001)**	**(.002)**		
M∗LAG_EXP	**- 0.08**	- 0.02	- 0.02	**0.10**		
	(.068)	(.573)	(.498)	**(.021)**		
LAG_REV	**0.50**	**0.61**	**0.60**	**0.70**		
	(.000)	**(.000)**	**(.000)**	**(.000)**		
P∗LAG_REV	**- 0.34**	**- 0.28**	**- 0.12**	**- 0.50**		
	(.000)	**(.000)**	**(.002)**	**(.000)**		
M∗LAG_REV	**0.12**	0.05	**0.06**	**- 0.14**		
	(.012)	(.182)	**(.061**	**(.003)**		
YSHOCK	**0.12**	**0.12**	**0.09**	**0.13**	- 0.02	0.02
	(.027)	**(.030)**	**(.056)**	**(.028)**	(.713)	(.719)
P∗YSHOCK	**- 0.17**	**- 0.17**	**- 0.13**	**- 0.28**	**- 0.11**	**- 0.17**
	(.009)	**(.008)**	**(.016)**	**(.009)**	**(.040)**	**(.038)**
M∗YSHOCK	**- 0.11**	**- 0.11**	- 0.04	- 0.09	- 0.02	- 0.07
	(.073)	**(.078)**	(.460)	(.241)	(.758)	(.284)
Controls	X_2	X_2	X_2	X_2	X_2	X_2
# Obs.	1417	1414	1733	949	1356	911
R^2	0.39	0.65	0.62	0.68	0.65	0.74

Broad and Narrow refer to less and more restrictive definitions of a democracy (see text). *p*-values in brackets.
Boldface fonts denote significance at the 10% level. SUR estimated jointly with *CGEXP* and *CGREV*
P and *M* denote interaction with the *PRES* and *MAJ* dummies, respectively
X_2 includes the same variables as X_1 , except *PROP 65* , plus the income trend corrsponding to *YSHOCK*
R^2 in the fixed-effect regression (column 1) refers to the within estimator

Table 15
Government Surplus
Electoral Cycles 1960-95

Sample				Broad	Narrow	
Estimation	FE Levels	FE, SUR levels	IV diffs.	IV Diffs.	IV diffs.	GLS diffs.
*PRES*EL*$_t$	- 0.19	- 0.04	- 0.29	- 0.38	- 0.29	0.12
	(.622)	(.910)	(.425)	(.201)	(.620)	(.429)
*PRES*EL*$_{t-1}$	0.61	**0.85**	**0.86**	**1.14**	**1.02**	**0.69**
	(.128)	**(.026)**	**(.015)**	**(.000)**	**(.089)**	**(.000)**
*PARL*EL*$_t$	- 0.02	- 0.01	- 0.01	- 0.10	- 0.12	**- 0.16**
	(.909)	(.942)	(.978)	(.581)	(.513)	**(.046)**
*PARL*EL*$_{t-1}$	0.26	0.29	0.05	0.04	**0.49**	**- 0.14**
	(.212)	(.149)	(.805)	(.823)	**(.033)**	**(.087)**
Controls	X_9	X_9	X_9	X_9		X_{10}
# Obs.	1339	1338	1281	1569	872	1425
R^2	0.43	0.69	0.71	0.68	0.77	

Broad and Narrow refer to the less and more restrictive definitions of a democracy (see text). *p*-values in brackets. Boldface fonts denote significance at the 10% level.
EL$_t$ and *EL*$_{t-1}$ are dummy variables for the election and post-election years, respectively. SUR estimated together with *CGEXP* and *CGREV*.
X_9 includes the same variables as X_2 plus all the variables (including the interaction terms) in column 1 of *Table 13* , except *OIL*,
plus a set of yearly dummies; X_{10} is identical to X_9 except that the lagged surplus is not included (see the text and Appendix).
R^2 in the fixed-effects regression (column 1) refers to the within estimator.

Table 16
Summary of Results

	PRES (vs. *PARL*)		*MAJ* (vs. *PR*)	
	Evidence	Theory	Evidence	Theory
Size	–	–	–/0	– /?
Composition (broad vs. narrow)	–/0	–	–	–
Electoral cycle	+/–	NA	+/0	+ /NA
Reaction to shocks	–	NA	–	NA
Budget deficits	–	NA	–	–

Figure 1
Political Institutions 1995

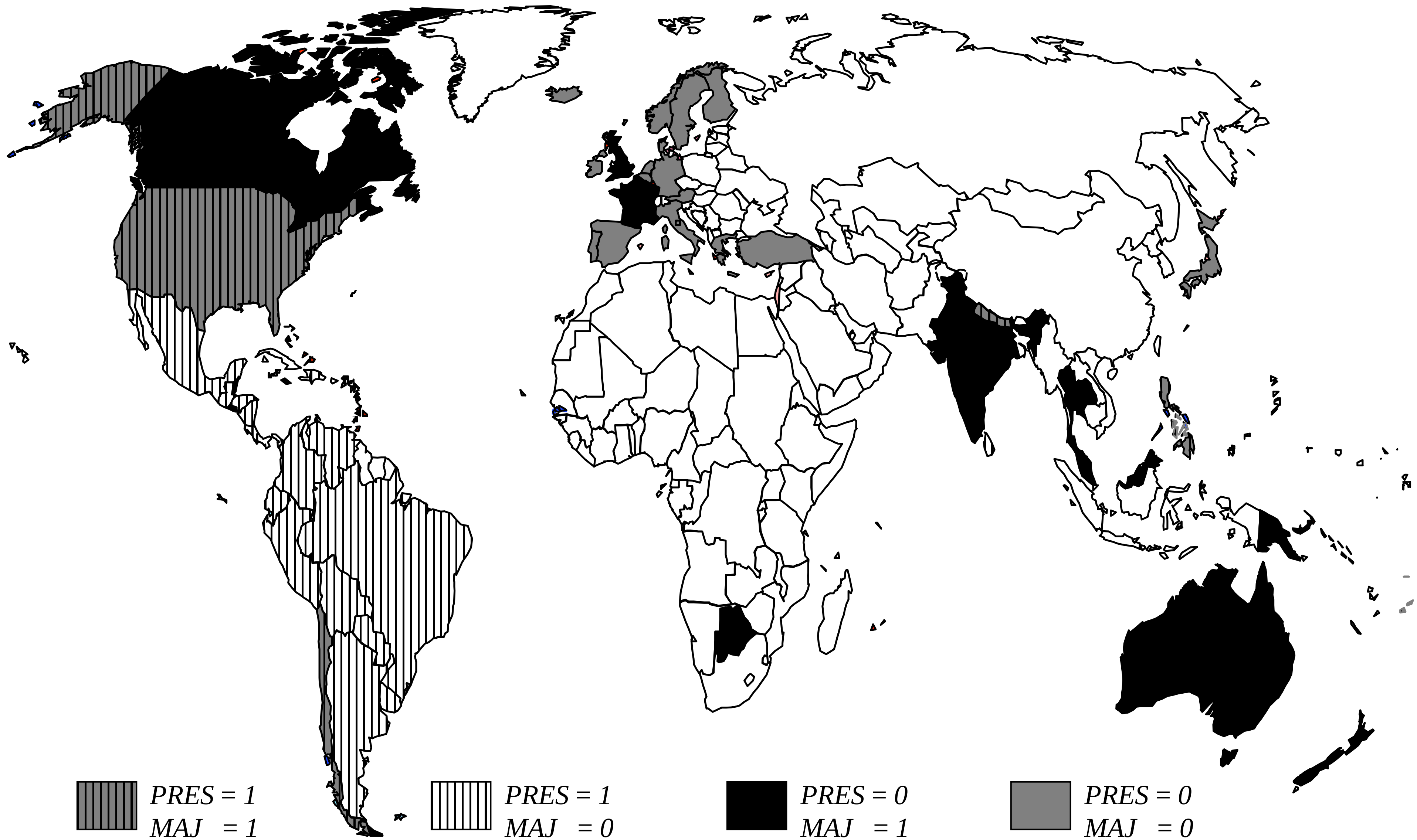

Figure 2
Size of Government 1960-98

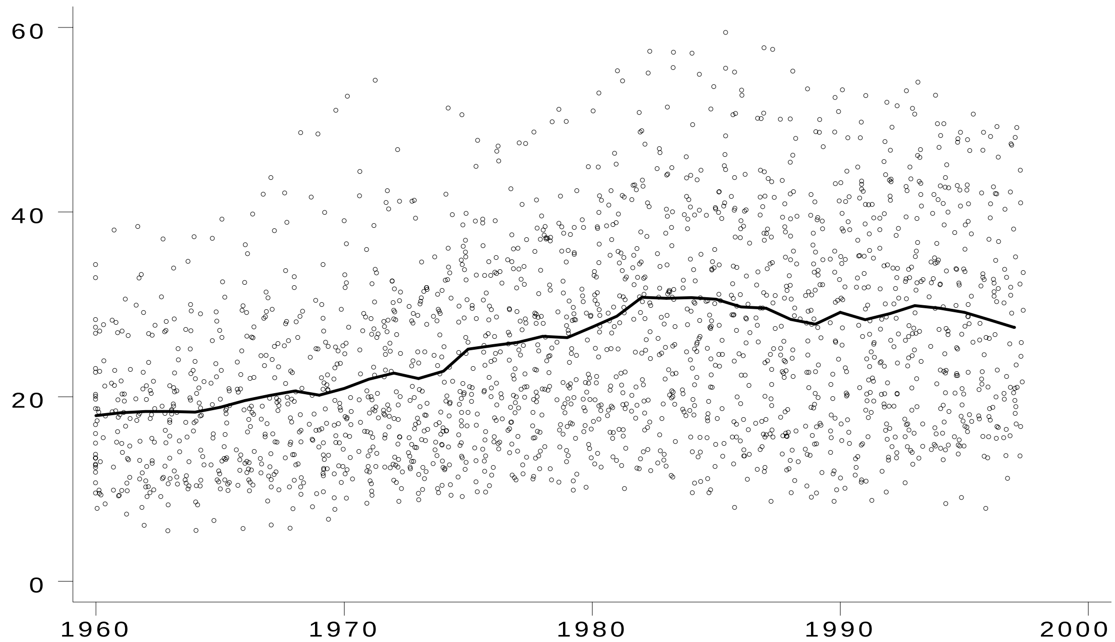

Figure 3
Size of Government 1960-98