Do political institutions shape economic policy?

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Abstract

Do political institutions shape economic policy? I argue that this question should naturally appeal to economists. Moreover, the answer is in the affirmative, both in theory and in practice. In particular, recent theoretical work predicts systematic effects of electoral rules and political regimes on the size and composition of government spending. And results from ongoing empirical work indicate that such effects are indeed present in the data. Some empirical results are consistent with theoretical predictions: presidential regimes have smaller governments and countries with majoritarian elections have smaller welfare-state programs and less corruption. Other results present puzzles for future research: the adjustment to economic events appears highly institution-dependent, as does the timing and nature of the electoral cycle.

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1. Initial remarks

In the last five to ten years, political economy – or political economics, as I prefer to call it – has been a rapidly growing field. As the label suggests, this field deals with issues related to politics using the tools of modern economics. The most recent work is attractive in that it draws on several traditions: the older public-choice school, the rational-choice school in political science, and the equilibrium theory of macroeconomic policy. Collected works, monographs and textbooks now start to appear, drawing on the contributions in the last decade. One such piece is Persson and Tabellini (2000a), others include Mueller (1997), Austen-Smith and Banks (1999), Drazen (2000), and Grossman and Helpman (2001).

An obvious motivation for this literature comes from observing economic policy outcomes. Looking across time and place, one observes large differences in policy, but also some common patterns. An example is given in *Figure 1,* which shows a measure of the size of government in about sixty countries over the last four decades. In the figure we see that government expenditure in a typical year ranges from below 10 percent of GDP to well 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. (I will discuss these data further in Section 3.) Such differences and similarities cry out for an explanation. An important goal in the literature has thus been to construct a positive theory of economic policy.

This brings me to the subject-matter of my lecture, which I will devote to current research on largely unresolved issues. As the title suggests, I will focus on attempts to identify what systematic effect political institutions might have on economic policy outcomes. This is, of course, a very broad question.[1] To narrow it down, I will confine the discussion to the institutions governing electoral rules and political regimes. And I will consider their effect on fiscal policy, broadly defined to include rents sought by corrupt politicians. This question is not only of academic interest. For instance, reform of electoral institutions has recently taken place in Japan, Italy and New Zealand and is a hotly debated issue in other countries. Theory and evidence on the policy consequences of alternative electoral rules would enlighten this debate.

[1] Other work by economists on the broad question includes the literatures on the links between budgetary institutions and budget deficits (see, for instance, the contributions in Poterba and von Hagen, 1999) and between fiscal federalism and the size of government (surveyed by Inman and Rubinfeld, 1997); see Persson and Tabellini (2000a) for further references.

I would like to make two main points. (1) It is natural for an economist to pose the question whether political institutions shape policy. (2) The answer is yes; empirically, electoral rules and political regimes do seem to systematically influence the choice of fiscal instruments, as well as the incidence of corruption.

Next, I will outline the main ideas in a recent wave of theoretical work on the topic of the lecture (Section 2). Then, I will describe some data that have been assembled with the aforementioned theory as the main guide in sampling and measurement (Section 3). A good part of my lecture will then report on results from ongoing empirical projects, dealing with the link from political institutions to fiscal policy and corruption (Section 4). Finally, I will sum up and discuss where research might go next (Section 5).

2. Theoretical ideas

2.1. An organizing framework

Space is too limited to allow any formal analysis. It is still helpful to survey the theoretical ideas against the backdrop of some specific notation. Specifically, let me appeal to a very simple public-finance framework, which highlights the size of the government budget and its allocation to different purposes. [2]

The population in a country is divided into a large number of groups, labeled by J. Membership of each group is defined by the prospective benefits of public spending. Everybody has the same preferences over policy:

$$\begin{aligned} w^J &= U(c^J) + H(g) \\ c^J &= y - \tau + f^J \ . \end{aligned}$$

Group J has size N^J. Its members thus enjoy private consumption, c^J, given by after-tax income plus a group-specific transfer f^J. All groups pay the same tax, τ, and enjoy the same benefits of public spending on g. What is important about f^J is that it allows for (geographically) targeting specific groups, not the specific form of targeting. Centrally provided local public goods would have served the same purpose. Similarly, g is a non-targeted form of spending benefiting all citizens; a broad social transfer program would have served the same purpose.

[2] The frameworks shares features and notation with the positive models to explain fiscal policy in Persson and Tabellini (2000a).

Policy choices are summarized by $\mathbf{q}$, a vector constrained to include non-negative elements only:

$$\begin{aligned} \mathbf{q} &= [\{f^J\}, g, \tau, r] \geq 0 \\ r &= N\tau - g - \sum_J N^J f^J \ . \end{aligned}$$

The budget constraint is standard except for the variable r, which does not appear directly in the citizens' payoffs. Literally, r represents direct extraction of rents by politicians for private use. Less literally, it may represent – on reduced form – corrupt activities, or inefficiently designed activities that constitute a drain for the citizens but benefit politicians or their close friends.

This framework is obviously very stylized. A richer economic environment can certainly be added. Citizens would then also interact in markets, making purposeful economic choices influenced by policy. Similarly, we could enrich the simplistic form of rent extraction.

Yet, even this stylized framework entails enough building blocks to permit a rich analysis of the politics of policymaking. To see this, note that the choice of $\mathbf{q}$ generates conflicts of interest in three different dimensions: (i) We have the traditional conflict among different groups of voters over the allocation of targeted spending $\{f^J\}$. (ii) The second is an agency problem: the voters at large would like higher g or lower τ, but rent-seeking politicians would instead like to spend these resources on r. (iii) A final source of conflict is that different politicians, or political parties, will compete for any available rents.

2.2. General ideas

The basic idea in the recent literature is this: the resolution of the three conflicts, and thus the fiscal policy we observe, hinges on the political institutions in place. This idea should appear very natural to an economist. Consider an analogy from micro theory. Markets generate conflicts of interest between consumers and producers over price and product quality, and among different producers over profit. How these are resolved depends on market institutions. Equilibrium prices, qualities and profits hinge on regulation, 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 policy is the same.

Political institutions certainly have many dimensions. Arguably, however, the most fundamental aspects of constitutions decide 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; these are closely associated with the form of government, sometimes called the regime type.

While economists have not paid much attention to the consequences of these institutions, political scientists certainly have. A large, mostly empirical literature has focused precisely on electoral rules and regime types. But the analysis has generally been confined to purely political phenomena, such as the number of parties, the propensity for crises, etc. It has ignored economic policy, our topic here.[3]

This general discussion suggests a way of modeling the outcome of policy-making: $\mathbf{q}$ in our simple framework. In that approach, policy is the equilibrium outcome of a delegation game, where the interaction between rational voters and politicians is modeled on extensive form. Multiple principals, the voters, elect political representatives who, in turn, set policy to further their own opportunistic 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 the constitution becomes like an incomplete contract, leaving the politicians with some power in the form of residual control rights. Alternative constitutions can now be represented by alternative rules for how this extensive-form game is being played. An exercise in "comparative politics" amounts to comparing the policy outcomes across the resulting equilibria.

2.3. Specific predictions

Let me now describe the main ideas in a handful of recent studies that apply this comparative politics approach. I just outline the results, however, focusing on the specific predictions. Those interested can find most of the analytical details in Persson and Tabellini (2000a, Part III).

[3] An exception is a recent book by Lijphart (1999), which includes cursory evidence on economic policy outcomes. Modern classics within the political science literature on comparative politics include Bingham Powell (1982), Lijphart (1984), 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.

Electoral rules. I begin with the rules for electing a country's legislature. Legislative elections around the world differ in several dimensions. The political science literature emphasizes two: *district size* and the *electoral formula*. 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 winners of the highest vote shares get seats in a given district, whereas proportional representation instead awards seats in proportion to the vote share.

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, like g in the framework above, are more effective in seeking broad support and targeted programs, like f^J, more effective in seeking narrow support. Electoral rules with larger districts should thus bias spending towards broad, non-targeted programs. This point is made formally by Persson and Tabellini (1999) in a probabilistic-voting model, where policy is determined by electoral platforms before the election. Milesi-Ferretti, Perotti and Rostagno (2000) obtain a similar result in a model of strategic delegation in voting, where policy is set after the election in bargaining among the elected politicians.

Larger districts also facilitate entry in the political process by additional candidates or parties. Myerson (1993) uses a model of electoral competition to show how a larger number of candidates may produce lower equilibrium rents. Essentially, with more available candidates, voters can throw out corrupt parties at a lower ideological cost.

How about the electoral formula? The winner-takes-all property of plurality rule reduces the minimal coalition of voters needed to win the election, as votes for a party not obtaining plurality are lost. 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, leading to the prediction of larger broad spending programs under proportional representation. Lizzeri and Persico (2001) make this point in a model with binding electoral promises, while Persson and Tabellini (2000a, Ch. 9) instead consider policy choices by an incumbent subject to re-election.

Under first-past the post elections, with many small districts and plurality rule, electoral competition often becomes concentrated to a subset of identifiable "marginal districts". As these have close races with many swing voters, the perceived electoral punishments for inefficient programs become large. These expected vote losses should induce candidates involved in electoral competition to chooses policies entailing smaller rents (r in the model) than when districts are fewer and larger, a result derived in Persson and Tabellini, 1999.

While voters 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 have career-concerns in the style of Holmström (1982). They find that proportional representation (list voting) should be associated with a larger extraction of rents, as the career-concern, re-election motive becomes a weaker counterweight to the rent-extraction motive for collectively accountable politicians. A second prediction is that electoral cycles, showing up in spending or taxes, should be weaker under proportional representation. This is because the incumbents' career concerns are stronger with the individual accountability under plurality rule and because these concerns are at their strongest just before elections.

We note also that the theory has few robust predictions regarding electoral rules and the size of government.

Anticipating already here the empirical part, we find a strong correlation between 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 whoever collects the most votes 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 classification.

Regime types. Two especially interesting aspects of the legislative regime concern the powers over legislation: to make, amend, or veto policy proposals. The first concerns the *separation* of these powers across different politicians and offices. The second concerns the *maintenance* of these 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 cruder classification of real-world regimes. 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, who decide on policy in different forms of legislative bargaining, are held accountable by retrospective voters. They show that a larger concentration of powers in parliamentary regimes makes it easier for politicians to collude with each other at the voters' expense; the weaker electoral accountability results in higher rents and taxes (r and τ in the framework above).

Another idea has to do with the confidence requirement. The parties supporting the executive hold valuable proposal powers which they risk to lose 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. First, 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 instead tends to pit the interests of different minorities against each other for different issues on the legislative agenda. Equilibrium spending in parliamentary regimes thus becomes more directed towards broad programs (g rather than f^J). Second, in parliamentary regimes, the stable majority of incumbent legislators, as well as the majority of the voters backing them, become prospective residual claimants on additional revenue. Both majorities favor high taxes and high spending. In presidential regimes, on the other hand, no such residual claimants on revenue exist and majorities therefore resist high spending. These forces produce larger governments (higher τ) in parliamentary regimes.

2.4. Discussion

Let me summarize the main predictions with the help of *Table 1.* According to the theory of political regimes, the predictions regarding different constitutional features reinforce each other. Presidential regimes with stronger separations of powers and lack of confidence requirements should thus have smaller governments than parliamentary regimes, less spending on broad programs, and less rents for politicians. Given the correlation between district size and the electoral formula mentioned above, some predictions in the literature on electoral rules reinforce each other. Thus, both smaller districts and plurality rule imply that we should see less spending on broad programs under majoritarian elections than under proportional elections. In the case of rents, however, these features pull in opposite directions: while smaller districts imply larger rents, plurality rule *cum* voting on individuals implies smaller rents. These are all "cross-sectional" predictions; they have been derived by comparing equilibria in static models. The prediction of more pronounced electoral cycles under majoritarian (plurality-rule) elections, however, relies on a dynamic model and is thus a "time-series" prediction.

Is this kind of analysis convincing? One might certainly question whether the simple assumed game forms capture the essence of real-world political institutions. Such skepticism would parallel the criticism against theoretical IO that "you could prove anything by picking the right extensive form and the right informational assumptions", or the criticism against incomplete contract theory that "there are many alternative assignments of control rights and you have no strong basis for choosing this particular one". These arguments have some force, but may be less damaging as long as we deal with *positive* theory rather than *normative* "constitutional engineering". A wealth of historical, political and legal studies document how the world's democracies carry out elections and allocate political and legislative control. Thus, the rules defining a particular game need not rely on the researcher's imagination. They can and should be given a solid empirical foundation.

Defending the underlying assumptions is not the only way of convincing skeptics, however. Another criterion of success is the empirical contents of the theory. Does it help us uncover new empirical regularities? To shed some light on this question, I now turn to the empirical part of the lecture.

3. Data and specification

Data. Let me start by briefly describing some data on political institutions and policy outcomes assembled in ongoing empirical research, where the theory I just sketched has served as the main guide in sampling and measurement.

Most of the results I will discuss in the next section rely on an international panel with data from at most 61 countries. Observations are yearly and runs from at most 1960 to 1998, a total of 39 years. But many observations are missing – for different reasons – which makes the panel unbalanced. What follows is only a brief description of these data; more details can be found in Persson and Tabellini (2000b).

Which countries are included in the panel? The theory suggest we should study countries with democratic institutions. A country's democratic status in a given year, is assessed by the well-known Freedom House indexes of political rights: the so-called GASTIL-indexes.[4] Here, I will only present results based on the a mid-way sampling rule (a GASTIL-score less than or equal 3.5, applied year by year). As the more comprehensive analysis in Persson and Tabellini (2000b) demonstrates, most results are similar in the two alternative sets of democracies (either all "free" and "semi-free" countries, or only the "free" countries).

Which political institutions enter the panel? Following the theory discussion in Section 2, I will report on results based on two crude classifications of electoral rules and regime types. First, we code countries that relied fully on plurality (or majority) rule in their most recent elections to the legislature (lower chamber) as majoritarian, and the other countries as proportional. The binary variable MAJ takes a value of 1 in the former case, 0 in the latter.[5] Second, countries where the survival of the executive does not require the confidence of the legislature are coded as presidential, the other countries as parliamentary. The resulting binary variable is called $PRES$.

Figure 2 illustrates the institutional variation across countries in 1995. The colored portions of the map represent the 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

[4] There are actually two indexes, one on political rights one on civil liberties. Each index runs from 1 to 7, where a country with scores of 1 or 2 are "free", 3 to 5 "semi-free", and 6 to 7 "not free". We take the simple average of these indexes.

[5] In Persson and Tabellini (1999) electoral rules were instead classified on the basis of district magnitude. The present classification based on the electoral formula yields a similar, but not identical grouping of countries.

shade proportional elections ($MAJ = 0$). The least common system is the US-style (dark-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. For example, Switzerland is classified as presidential, whereas France is not. The map also illustrates that the selection of countries into political institutions is not random, but a mirror of systematic cultural and historical forces. Indeed our classifications change very little over time, reflecting an inertia 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. A key maintained assumption is that institutions and outcomes are not simultaneously determined (I return to these issues in the next section).

Fiscal policy outcomes are measured as suggested by the theory. For the size of government (corresponding to τ in the framework of Section 2) we use different measures: central government expenditure, central government revenue, and general government expenditure, all as percentages of GDP. For the composition of government spending (g vs. $\{f^J\}$) we use two measures: social security and welfare spending (by central government), either as a percentage of GDP, or as a ratio to spending on goods and services. The assumption is thus that it is harder to target broad transfer programs, like pensions and unemployment insurance, to specific voting districts than it is to target spending on goods and services. These various fiscal policy measures do vary greatly across time and countries. Indeed, *Figure 1* in the introduction was a plot of our panel data for central government expenditure as a percentage of GDP.

At the end of Section 4, I will also describe some results from another ongoing project, which relies on a cross-sectional data set for more than 80 countries in the latter half of the 1990s. Here, rent extraction by politicians (r in the model) is approximated by available measures of corruption. Due to the counteracting predictions of the effects on rents of district magnitude and list voting (recall *Table* 1), electoral rules are characterized by two continuous measures, rather than a single binary measure. More details can be found in Persson, Tabellini and Trebbi (2000).

Specification. The empirical work is partly concerned with testing specific hypotheses. An additional purpose is to establish empirical regularities, however. We therefore adopt a relatively eclectic empirical specification describing policy outcomes:

$$y_{it} = \alpha + \nu_i + \boldsymbol{\beta}_i \mathbf{u}_t + \boldsymbol{\gamma}_i \mathbf{s}_{it} + \boldsymbol{\delta} \mathbf{x}_{it} + \boldsymbol{\eta} \mathbf{z}_{it} + \varepsilon_{it} \ . \tag{1}$$

Here, y_{it} denotes a policy outcome in country i and year t. The specification allows for a country-specific component, ν_i. Policy can be affected directly by the institutions $\mathbf{z}_{it}$, concretely by the value of the two dummy variables *MAJ* and *PRES* in i at t. It also depends on (vectors of) common variables $\mathbf{u}_t$ and idiosyncratic variables $(\mathbf{s}_{it}, \mathbf{x}_{it})$. Some slope coefficients are allowed to differ across countries.

Given (1), we pose the question of a systematic effect from institutions to policy in two different ways. One is to test the null hypothesis

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

i.e., the absence of a *direct* effect. Most of the theory discussed in Section 2 was really about such direct effects. The other way is to test for the absence of an *indirect* (non-linear) effect

$$H_0^I : \ \boldsymbol{\beta}_i = \boldsymbol{\beta}_j \ \text{ and/or } \boldsymbol{\gamma}_i = \boldsymbol{\gamma}_j, \ \text{ even if } \mathbf{z}_{it} \neq \mathbf{z}_{jt} \ ,$$

i.e., whether different institutions make policy respond to common or idiosyncratic variables in a different way. (The rationale for this test will be given shortly.) The parameters are estimated in several different fashions, which are probably best explained in the context of a specific example.

4. Empirical regularities?

4.1. Size of government

Cross-sectional results. Consider first the size of government. To arrive at a straightforward test for a direct effect on policy, take the time average of (1) to obtain

$$\overline{y}_i = \alpha + \boldsymbol{\delta} \overline{\mathbf{x}}_i + \boldsymbol{\eta} \overline{\mathbf{z}}_i + (\nu_i + \boldsymbol{\beta}_i \overline{\mathbf{u}} + \boldsymbol{\gamma}_i \overline{\mathbf{s}}_i) + \overline{\varepsilon}_i \ , \tag{2}$$

where an overbar denotes a time average. We can retrieve the parameter(s) of immediate interest, namely $\boldsymbol{\eta}$, by regressing the average size of government, on a constant and the country (time) averages of controls and institutional indicators. Obviously, country-specific determinants of the size of government (the bracketed term in (2)) cannot be identified from such a cross-sectional regression and go into the error term. To take account of the unbalanced panel underlying the cross-section, it is convenient to use the so-called between estimator, which is equivalent to OLS with each country weighted by the number observations in its panel.

Results are displayed in *Table 2.* The dependent variable is either central government spending (as a percentage of GDP), or central government revenue. The control variables in $\mathbf{x}_1$ include a number of socio-economic factors identified by earlier studies as empirical determinants of the size of government (see the note to *Table* 2). Given the clustering of observations in *Figure 2,* we use dummies for continents and colonial origin as additional controls. The table displays the estimated $\boldsymbol{\eta}$ parameters for the *PRES* and *MAJ* dummies. Bracketed expressions are p-values for false rejection of $\boldsymbol{\eta} = 0$. Boldface font denotes a coefficient significantly different from zero, at the 10% level.

The two institutional dummies always enter with a negative sign. But *MAJ* is rarely significantly different from zero.[6] On the other hand *PRES* typically is, even though one can find specifications where it is not. This finding is clearly in line with the theoretical prediction in Section 2. According to the point estimates, governments in presidential regimes are smaller by more than 5 percent of GDP. This is a quantitatively large effect. As a comparison, consider upward shifts by one standard deviation in three of the controls: income per capita, openness, and the share of the population above 65. According to the point estimates underlying column 2 of *Table* 2, these shifts are associated with larger governments by about 0.5, 2, and 3.5 percent of GDP, respectively.

As column 3 shows the negative effect of *PRES* is even stronger, above 10 percent of GDP, when the cross sections are based on data from the 1990s rather than the whole sample. Moreover, it is more precisely estimated (cf. also the empirical results in Persson and Tabellini, 1999). These results suggest that the negative estimates largely reflect 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 1*, except that the data is partitioned into presidential regimes, marked with black diamonds and a thicker curve for the average, and parliamentary regimes, marked with circles and a thinner curve.

[6] Milesi-Ferretti, Perotti and Rostagno (2000) find *general* government expenditure to be smaller in countries with majoritarian elections in their study of the OECD countries over the same period. Using general government expenditures as the dependent variable cuts down the sample considerably (to about 40 countries due to data availablility); then, the results correspond to those obtained by Milesi-Feretti et al.

While these cross-sectional estimates are suggestive, they are potentially subject to bias. The most serious concern has to do with non-random selection into different regimes and electoral rules. Systematic selection could bias the estimates in different ways. As discussed in more detail in Persson and Tabellini (2001), one source of bias is that non-random selection on *observables* (e.g., systematic correlation between $\overline{\mathbf{x}}_i$ and $\overline{\mathbf{z}}_i$) makes $\overline{\mathbf{z}}_i$ correlated with omitted non-linearities in the relation between observable variables and the size of government. To address this Persson and Tabellini (2001) use alternative non-parametric matching estimators that allow for general functional forms and directly address non-random selection. The matching estimates of $\boldsymbol{\eta}$ are similar to conventional regression estimates, suggesting that this kind of bias is not too serious.

Another obvious source of bias is omitted variables: i.e., systematic selection on *unobservables* makes $\overline{\mathbf{z}}_i$ correlated with the error term in (2) – $\overline{\mathbf{z}}_i$ could be correlated either with the terms in brackets or with $\overline{\varepsilon}_i$. Eliminating that problem requires isolating some truly exogenous variation in political institutions. A standard method would be to find a suitable instrument. But, so far, we have been unable to find variables correlated with institutions that could credibly be claimed independent of policy. Another standard method would be to use time variation in institutional measures, relying on fixed-effect estimation. Unfortunately, that way is closed by the above-mentioned lack of institutional reforms in the data set. For practical purposes, $\mathbf{z}_{it}$ is given by a constant, $\mathbf{z}_i$, equal to the time average $\overline{\mathbf{z}}_i$. Thus, we cannot separately identify the effects on policy of a country's institutions $\mathbf{z}_i$ and other time-invariant, country-specific features ν_i.

Could we somehow exploit the considerable time variation in policy outcomes to gauge whether and how institutions matter? That is the idea behind the test of H_0^I, namely whether different political institutions shape different policy responses to economic and political events. Clearly, looking for such indirect effects is a different question than looking for direct effects. But the alternative question may involve weaker identifying assumptions. Even if some "historical omitted variables" were to drive both selection into different political institutions and average policy outcomes, it is less likely that these historical forces are systematically correlated with the response to economic and political events during our recent sample period.

Below, I thus follow Persson and Tabellini (2000b) and report results on institution-dependent adjustments to economic and political events. However, this means leaving the tests of specific predictions, instead entering a search for empirical regularities. For the theoretical predictions discussed in Section 2 were all based on static models, with the exception of the career-concern model of electoral cycles.

Unobservable common events It is plausible that a set of common political and economic events have affected fiscal policy in all countries in the last four decades. Think e.g., of the worldwide turn towards 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 which they use to estimate 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 affect policy in the OECD countries.

Assume that the response to observable idiosyncratic variables is the same in all countries, $\boldsymbol{\gamma}_i = \boldsymbol{\gamma}_j$ in (1). Then we can lump all country-specific variables together in $\mathbf{x}_{it}$ and rewrite (1) by a specific parametrization of $\boldsymbol{\beta}_i$:

$$y_{it} = \alpha + [1 + \boldsymbol{\lambda}(\mathbf{z}_i - \overline{\mathbf{z}})]\boldsymbol{\beta}\mathbf{u}_t + \boldsymbol{\delta}\mathbf{x}_{it} + (\nu_i + \boldsymbol{\eta}\mathbf{z}_i) + \varepsilon_{it} \ . \tag{3}$$

We can use a set of time dummies to estimate, $\boldsymbol{\beta}\mathbf{u}_t$, the common impact on policy of the common events in (3). The institution-specific effect of the common events $\mathbf{u}_t$ is proportional to the term $\boldsymbol{\lambda}(\mathbf{z}_i - \overline{\mathbf{z}})$, where $\overline{\mathbf{z}}$ denotes the cross-country average of $\mathbf{z}_i$. The form of (3) suggests that we should estimate the crucial parameter $\boldsymbol{\lambda}$ by NLS, while including fixed effects to control for the country-specific average $(\nu_i + \boldsymbol{\eta}\mathbf{z}_i)$.

Table 3 shows some results based on annual data. (Persson and Tabellini, 2000b also report results based on five-year averages, which may better handle measurement error and allow for discretionary adjustments of policy.) The country-specific controls are the same variables as in the cross-sectional regressions. The interaction parameters $\boldsymbol{\lambda}$ for both *PRES* and *MAJ* are negative and highly significant. To interpret the results, consider a common event in period t that raises government spending by 1 percent of GDP in an average country; i.e., an event such that $\boldsymbol{\beta}(\mathbf{u}_t - \mathbf{u}_{t-1}) = 1$. Coefficients around -1 and -0.5 mean that the effect of this event is 1 percent smaller in presidential (compared to parliamentary) regimes and 0.5 percent smaller under majoritarian (compared to proportional) elections.

Another way of gauging the results is to ask how the *cumulative* effect of the common events over the course of the sample period, given by $\boldsymbol{\beta}(\mathbf{u}_T - \mathbf{u}_1)$, differs across institutions. The point estimates suggest that the cumulative difference between presidential and parliamentary regimes is above 10 percent of GDP. This number fits well with the estimated cross-sectional difference from the 1990s reported in *Table 2*. Thus, we can attribute much of the current size difference between these regimes to a different adjustment to a set of common events in the preceding decades.

Observable economic events Do institutions shape the adjustment to observable economic events? Specific parametrizations of $\boldsymbol{\beta}_i$ and $\boldsymbol{\gamma}_i$ and another rewrite of (1) give:

$$y_{it} = \alpha + (\boldsymbol{\beta} + \boldsymbol{\phi}\mathbf{z}_i)\mathbf{u}_t + (\boldsymbol{\gamma} + \boldsymbol{\mu}\mathbf{z}_i)\mathbf{s}_{it} + \boldsymbol{\delta}\mathbf{x}_{it} + (\nu_i + \boldsymbol{\eta}\mathbf{z}_i) + \varepsilon_{it} \ . \qquad (4)$$

In (4) the parameters $\boldsymbol{\phi}$ and $\boldsymbol{\mu}$ thus allow for institution-dependent adjustments to common and idiosyncratic variables. As an observable common variable in $\mathbf{u}_t$ we have used the world oil price, and as idiosyncratic variables in $\mathbf{s}_{it}$ we have included lagged policy y_{it-1}, the share of the population above 65, and the deviation of income from its (Hodrick-Prescott) trend. One way of estimating the $\boldsymbol{\phi}$ and $\boldsymbol{\mu}$ parameters in (4) is to take account of the country-specific term $(\nu_i + \boldsymbol{\eta}\mathbf{z}_i)$ by including fixed effects. To get more efficient estimates of spending and revenue equations, we also estimate these fixed effects regressions jointly with SUR. An alternative way is to wipe out the country-specific effect by taking first differences. In this case, we use two different estimators. One is an IV-estimator: we include Δy_{it-1} in the regression and instrument it by y_{it-2} and $(\Delta\mathbf{u}_{t-1}, \Delta\mathbf{s}_{it-1}, \Delta\mathbf{x}_{it-1})$ (plus the corresponding interaction terms). The other is a GLS-estimator: we do not include Δy_{it-1} in the regression, but allow for panel-specific autocorrelation and heteroskedasticity in ε_{it}.[7]

The results in *Table 4* indicate systematic indirect effects of institutions when it comes to income shocks and lagged policy. (Results for oil prices and population shares are less robust.) The fixed-effects estimates in columns 1-3 suggest that negative income shocks raise spending as a share of GDP. But this effect is absent, or even overturned, in presidential regimes and under majoritarian elections. These systems are also associated with less inertia in spending, although here the

[7]It is well-known that the presence of a lagged dependent variable can bias the fixed effects estimator (see e.g, Baltagi, 1995). The problem may be less serious in this international panel than in the typical labor context, as the bias diminishes in T and T is about 40. The IV-estimator was suggested by Anderson and Hsiao (1981) and Arrellano and Bond (1991) to correct for the bias in dynamic panels.

effect of majoritarian elections is weaker. The instrumental variable estimates in column 4 and the GLS estimates in column 5 show that the results for income shocks are very robust; the point estimates on inertia are similar but have higher standard errors.

Persson and Tabellini (2000b) distinguish between positive and negative income shocks. Their results point towards an asymmetry: in parliamentary and proportional systems negative income shocks significantly raise the spending share, whereas positive income shocks do not lower the spending share. In presidential and majoritarian systems, on the other hand, positive shocks raise spending, whereas negative shocks do nothing. This suggests ratchet effects in the growth of government, but of a very different nature across political systems.

Understanding better the reasons behind the different adjustments to income shocks is an intriguing topic for future theoretical and empirical research.

Electoral cycles Is there an electoral cycle in total government spending or tax revenue and is this cycle institution-dependent? To ask that question, expand $\mathbf{s}_{it}$ – the country i variables with institution-specific effects on policy – to also include dummies for election years as well as post-election years. Otherwise, the estimation methods and specification, including all the economic shocks and controls, is the same as in *Table 4* (except that a set of common time dummies replace oil prices to allow more precise estimates of the electoral cycle).

When the institutional dummies are not included, we find a significant and sizeable post-election cycle in spending, with spending cuts being postponed until after the election. For revenues, we find significant cuts in the election year and (less robust) hikes in the post-election year.[8]

As *Table 5* reveals, however, these electoral cycles are highly institution-dependent. The post-election cycle – a cut in spending by about 1 percent of GDP and a gain in revenue by 0.5 to 1 percent of GDP – is clearly present only in presidential regimes. The pre-election tax cuts, on the other hand, are visible only in parliamentary regimes. As in the case of the adjustment to income shocks, we do not have a solid explanation for these differences.

We have also tried to test the prediction of the career-concern model discussed in Section 2 of a stronger pre-electoral cycle under majoritarian elections. While the signs of the point estimates are consistent with this prediction, their significance is not robust across samples and estimation methods.

[8] See Shi and Svensson (2000) for a recent and comprehensive study using international panel data and Alesina, Roubini and Cohen (1997) or Drazen (2000) for surveys of the earlier literature.

4.2. Composition of government

Let me turn to the composition of government spending. Recall that our measures here are central government spending on social security and welfare as a percentage of GDP and the ratio of the same variable to spending on goods and services. Persson and Tabellini (2000b) carry out the same battery of tests as those for government size above. Here, I will just give a brief overview of the results.

The *cross-sectional results* show that broad, non-targeted programs are indeed smaller under majoritarian elections, as predicted by the theory. *Ceteris paribus*, social security and welfare spending appears to be about 2 percentage points smaller as a share of GDP, and 20-30% lower as a ratio to spending on goods and services. Statistically, these results are more fragile than the results for overall spending. Qualitatively, they are in line with findings of Milesi-Ferretti et al (2000) for the OECD countries. In this case, however, we find no systematic effect of the regime type.

Unobservable common events are estimated to have a much smaller effect on the spending ratio under majoritarian elections. The cumulative effect on this ratio (from the early 1970s to the 90s) is on the order of 10 %. Common events have a smaller effect on social security and welfare in presidential regimes, with a cumulative effect of 4-5 percent of GDP. But the latter result may largely capture the higher overall growth of government in parliamentary regimes.

Observable economic events again trigger institution-specific adjustments. As for aggregate spending, we find negative effects of income shocks on social security and welfare spending. But these effects are significantly smaller, or even nullified, under majoritarian elections and presidential regimes.

For *electoral cycles* in this component of spending, the findings are quite intriguing. Without conditioning on political institutions, no electoral cycle is observed. However, conditioning reveals systematic evidence of both pre-election and post-election effects. In connection with a typical election, spending on social security and welfare *rises* by about 0.2 percent of GDP both before and after the election in countries with proportional elections in parliamentary regimes. Under majoritarian elections in parliamentary countries *no* effects are visible, however, while in presidential regimes social spending tends to *fall* by 0.1-0.2 percent of GDP after elections (in consistency with the results for aggregate spending). It is perhaps plausible that we should see spending hikes in parliamentary and proportional systems if politicians in these systems indeed have stronger incentives to rely just on broad programs to get elected or re-elected, as suggested by the theory surveyed in Section 2. A theory of the composition of the electoral cycle under different political institutions has not yet been worked out, however, and constitutes a further challenge for future research.

4.3. Corruption

It is not easy to find empirical counterparts to rent extraction (r in the simple model) which are comparable across countries. The best proxies are probably those international surveys that try to measure the extent of corruption. I will end by reporting on another ongoing project (Persson, Tabellini and Trebbi, 2000) that relies on such corruption data. More precisely, it uses a careful survey of surveys assembled by Transparency International. This data source is attractive in that it includes measures of "grand corruption" at the highest levels of government, which conforms well with the theoretical models discussed in Section 2. The TPI score runs from 0 (perfectly clean) to 10 (highly corrupt).

The project also includes finer measures of the electoral rule than the single, dichotomous MAJ dummy. Based on the theoretical predictions mentioned in Section 2, we use two continuous variables. $DISMAG$ measures district size (1 minus the inverse of average district magnitude, in legislative elections). $PLIST$ instead measures the electoral formula, namely the share of legislators elected via party lists (rather than individually). Both measures run between 0 and 1: a score of 0 on both of them corresponds to first past the post in one-member districts, whereas a score of 1 on both corresponds to full proportionality in very large districts.

Unfortunately, both the corruption scores and the variables describing electoral rules are only available for the late nineties. Therefore, the study is limited to cross-country data. The intersection of corruption, electoral and socio-economic data limits the study to at most 82 countries. Some results are shown in *Table 6.* The control vector $\mathbf{x}_b$ consists of a dozen economic, social and cultural variables found to correlate closely with corruption in earlier studies (see Persson, Tabellini and Trebbi, 2000). As the first (empty) column shows, these variables explain close to 90% of the cross-country variance in corruption. Nevertheless, the earlier dichotomous dummies, $PRES$ and MAJ, improve the fit (in terms of adjusted R^2). Both have the negative sign expected from theory, but only MAJ is statistically significant.[9]

[9] As in the case of size of government discussed above, Persson Tabellini and Trebbi (2000) use alternative, non-parametric matching estimators to allow for more flexible functional forms and correct for non-random selection (of the electoral rule) on observables. The matching estimates

But this crude measure turns out to mask two effects running in opposite directions. Larger districts – higher $DISMAG$ – lowers corruption, whereas greater use of list voting – higher $PLIST$ – raises it. Both results are consistent with the theory in Section 2: lower barriers to entry (larger districts) decrease corruption, while blunter career concerns (more party list voting) increase it. As columns 3 to 5 demonstrate, these effects are robust to including other institutional variables, namely the legal and colonial origin of countries, which earlier empirical studies have found to correlate with corruption (see e.g., Treisman, 2000).

These effects are not only statistically significant, but also quantitatively important. Consider Chile, a country considerably less corrupt than its South American neighbors; its residual from the regression in the first column in *Table 6* is about - 2.5, whereas the average South-American country has a residual close to 0. Our results suggest that between a quarter and a half of this difference might be due to Chile's electoral system, the only one in the region where voters cast their ballots for individual candidates under plurality rule (in two-seat districts).

5. Final remarks

Do political institutions shape economic policy? I have argued that this question is theoretically appealing and that it presents an opportunity for empirically founded applications of game theory. I have also reported on ongoing empirical work, which suggests that the answer to the question is a resounding yes.

The results are summarized in *Table 7.* Empirically, presidential regimes are associated with smaller governments than parliamentary regimes, a smaller and less persistent response of spending to income shocks, a stronger post-election cycle in aggregate spending and revenue, but a weaker cycle in social transfers. Majoritarian elections are associated with smaller broad spending programs than proportional elections and with less corruption; they also have smaller (and perhaps less persistent) spending responses to income shocks, and a weaker election cycle in social transfers. Several of these empirical regularities, those marked with black and bold in *Table 7*, are in line with the first wave of theory. But others, marked in gray and bold, are still awaiting a theoretical explanation. This is especially so for the results indicating institution-dependent adjustments of policy to economic events and the institution-dependent electoral cycles.

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largely accord with the regression estimates, suggesting that majoritarian elections indeed have a robust negative effect on corruption.

These are promising first steps in a research program. Much work certainly remains, however. So, where might research go next? One direction is clearly to refine the *theory* of policy. As just noted, our empirical results on policy adjustments and electoral cycles are in search of a theory. To understand them, we need dynamic rather than static models of the relationship between institutions and policy. In its applications, the research so far has concentrated on government spending. It would be interesting – and certainly feasible – to use similar methods in studying other policy instruments, such as the structure of taxation including trade policy. 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.[10]

This suggests another direction, 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 is to trace detailed changes in electoral rules over time; concretely, to compile panel data for variables like *DISMAG* and *PLIST*. In other cases, better measures will require the collection of new primary data. A concrete example is to construct empirical measures of the separations of powers in different political regimes. As this may be a labor-intensive and open-ended task, it is important to use theory as a guide.[11] Compiling and collecting such data would consistently account for institutional reforms, which would help achieve more convincing identification of the effects of institutions on policy.

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 in trading off the prospective biases due to measurement and specification errors. Sharper hypotheses, derived from dynamic models, would be especially helpful in avoiding the pitfalls of estimation in dynamic panels. As discussed above, non-random selection into different regimes and electoral rules may also be an important source of bias. Theoretical work endogenizing institutional choice would certainly help in empirically addressing these selection issues.[12]

[10] Diermeier and Merlo (1999) study a structural model of different confidence and parliamentary procedures and test its empirical implications; so far, however, they only deal with consequences for political phenomena, like the longevity of governments and incidence of minority governments.

[11] Existing attempts to create such measures can be found in Shugart and Carey (1992) and in Beck et al (2000).

[12] Recent examples of such studies are Acemoglu and Robinson's (2000) work on extensions of the franchise and Boix (1999) work on the reform of electoral institutions.

All in all, a close interplay of theory, measurement and statistical work appears essential for making progress on the broad question I have dealt with in this lecture. I hope some readers will provide some help, both in posing the question more precisely, and in probing the data for an answer.

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Table 1
Summary of Theory

	PRES (vs. *PARL*)	*MAJ* (vs. *PR*)
Size	–	?
Composition (broad vs. narrow)	–	–
Rents	–	+ / –
Electoral Cycle	NA	+

Table 2
Size of Government
Cross Sections

Dep. Variable		Central Spending		Central Revenue
Sample	1960-98	1960-98	1990-95	1960-98
Estimation		Weighted OLS		
PRES	**- 9.06**	**- 7.54**	**- 12.14**	**- 6.46**
	(.001)	**(.034)**	**(.003)**	**(.046)**
MAJ	-2.17	- 3.33	- 2.88	- 2.45
	(.334)	(.178)	(.262)	(.262)
Controls	$\mathbf{x}_1$	$\mathbf{x}_1$ Cont.&Col.	$\mathbf{x}_1$ Cont.&Col.	$\mathbf{x}_1$ Cont.&Col.
# Countries	59	58	54	57
R^2	0.52	0.58	0.69	0.58

p-values in brackets. Boldface fonts denote significance at the 10% level.
Between estimator based on country means, where each country is weighted by its number of annual observations.
$\mathbf{x}_1$ includes controls for income, openness, the population between 15 and 64, and over 65 (see Persson and Tabellini, 2000b).
Cont. and Col. refer to sets of dummies for continents and colonial origin, respectively (see Persson and Tabellini, 2000b).

Table 3
Size of Government
Unobservable Common Events 1960-98

Dep. Variable	Central spending			Central revenue
Estimation	NLS, FE			
PRES	**- 0.91**		**- 0.99**	**- 1.42**
	(.000)		**(.000)**	**(.000)**
MAJ		**- 0.29**	**- 0.43**	**- 0.47**
		(.000)	**(.000)**	**(.000)**
$\beta*(u_T - u_1)*PRES$	**- 12.73**		**- 13.46**	**- 7.17**
$\beta*(u_T - u_1)*MAJ$		**- 2.99**	**- 5.84**	**- 2.37**
Controls	$\mathbf{x}_1$	$\mathbf{x}_1$	$\mathbf{x}_1$	$\mathbf{x}_1$
# Obs.	1519	1519	1519	1492
R^2	0.87	0.86	0.87	0.88

p-values in brackets. Boldface fonts denote significance at the 10% level.
$\mathbf{x}_1$ includes the same variables as in Table 2; all regressions include a set of country dummies.

Table 4
Size of Government
Observable Economic Events 1960-98

Dep. variable	Central Spending		Revenue	Central Spending	
Estimation	FE Levels	SUR, FE Levels.	SUR, FE Levels	IV Diffs.	GLS Diffs.
LAG_SIZE	**0.84**	**0.83**	**0.83**	**0.66**	
	(.000)	**(.000)**	**(.000)**	**(.002)**	
P∗LAG_SIZE	**- 0.29**	**- 0.28**	**- 0.25**	- 0.35	
	(.000)	**(.000)**	**(.000)**	(.318)	
M∗LAG_SIZE	**- 0.05**	- 0.04	**- 0.04**	- 0.12	
	(.073)	(.115)	**(.040)**	(.804)	
YSHOCK	**- 0.19**	**- 0.19**	**- 0.07**	**- 0.24**	**- 0.23**
	(.000)	**(.000)**	**(.092)**	**(.002)**	**(.000)**
P∗YSHOCK	**0.27**	**0.29**	**0.10**	**0.32**	**0.30**
	(.000)	**(.000)**	**(.058)**	**(.000)**	**(.000)**
M∗YSHOCK	**0.23**	**0.23**	**0.11**	**0.22**	**0.12**
	(.000)	**(.000)**	**(.020)**	**(.001)**	**(.001)**
Controls	$\mathbf{x}_2$	$\mathbf{x}_2$	$\mathbf{x}_2$	$\mathbf{x}_2$	$\mathbf{x}_2$
# Obs.	1475	1432	1432	1421	1472
R^2	0.81	0.95	0.96		

p-values in brackets. Boldface fonts denote significance at the 10% level.
P and *M* denote interaction with the *PRES* and *MAJ* dummies, respectively
$\mathbf{x}_2$ is equal to $\mathbf{x}_1$ plus the trend corresponding to *YSHOCK* and the oil price (see Persson and Tabellini, 2000b).
R^2 in the fixed-effects regression (column 1) refers to the within estimator.

Table 5
Size of Government
Electoral Cycles 1960-95

Dep. Variable	Central	Spending	Central	Revenue
Estimation	FE, SUR Levels	IV Diffs.	FE, SUR Levels	IV Diffs.
$PRES{*}EL_t$	0.10	- 0.15	- 0.31	- 0.47
	(.784)	(.710)	(.328)	(.297)
$PRES{*}EL_{t-1}$	**- 0.80**	**- 1.02**	**0.52**	**0.87**
	(.031)	**(.017)**	**(.095)**	**(.013)**
$PARL{*}EL_t$	- 0.03	- 0.27	**- 0.31**	**- 0.45**
	(.899)	(.261)	**(.066)**	**(.026)**
$PARL{*}EL_{t-1}$	- 0.11	- 0.22	0.15	0.31
	(.565)	(.373)	(.366)	(.129)
Controls	$\mathbf{x}_3$	$\mathbf{x}_3$	$\mathbf{x}_4$	$\mathbf{x}_4$
# Obs.	1350	1339	1350	1316
R^2	0.95		0.96	

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.
$\mathbf{x}_3$ is equal to $\mathbf{x}_2$ plus all the variables (including the interaction terms) in column 1 of *Table 4* minus oil prices plus a set of year dummies; $\mathbf{x}_4$ is constructed exactly as $\mathbf{x}_3$ but with lagged central revenue taking the place of lagged central spending.

Table 6
Corruption
Cross sections

Dep. variable			TPI-scores 1996-98		
Estimation			WLS		
PRES		- 0.30 (.369)			
MAJ		**- 0.61** **(.015)**			
PLIST			**1.48** **(.010)**	**1.51** **(.009)**	**1.40** **(.021)**
DISMAG			- 1.09 (.101)	**- 1.47** **(.034)**	**- 1.40** **(.041)**
Controls	$\mathbf{x}_b$	$\mathbf{x}_b$	$\mathbf{x}_b$	$\mathbf{x}_b$ Leg.	$\mathbf{x}_b$ Leg. & Col.
# Obs.	82	81	80	80	80
R^2	0.88	0.90	0.91	0.92	0.93

p-values in brackets. Boldface fonts denote significance at the 10% level.
Each observation is weighted by the inverse of the standard deviation of its TPI-score (see Persson, Tabellini and Trebbi, 2000)
$\mathbf{x}_b$ includes a set of 12 socio-economic variables; Leg. and Col. denote sets of dummies for legal and colonial origin, respectively (see Persson, Tabellini and Trebbi, 2000)

Table 7
Summary of Results

	PRES (vs. *PARL*)		*MAJ* (vs. *PR*)	
	Evidence	Theory	Evidence	Theory
Size	–	–	0	?
Composition (broad vs. narrow)	0	–	–	–
Rents	0	–	+ / –	+ / –
Electoral Cycle	+ / –	NA	0	+
Adjustment to events	–	NA	–	NA

Figure 1
Size of Government 1960-98

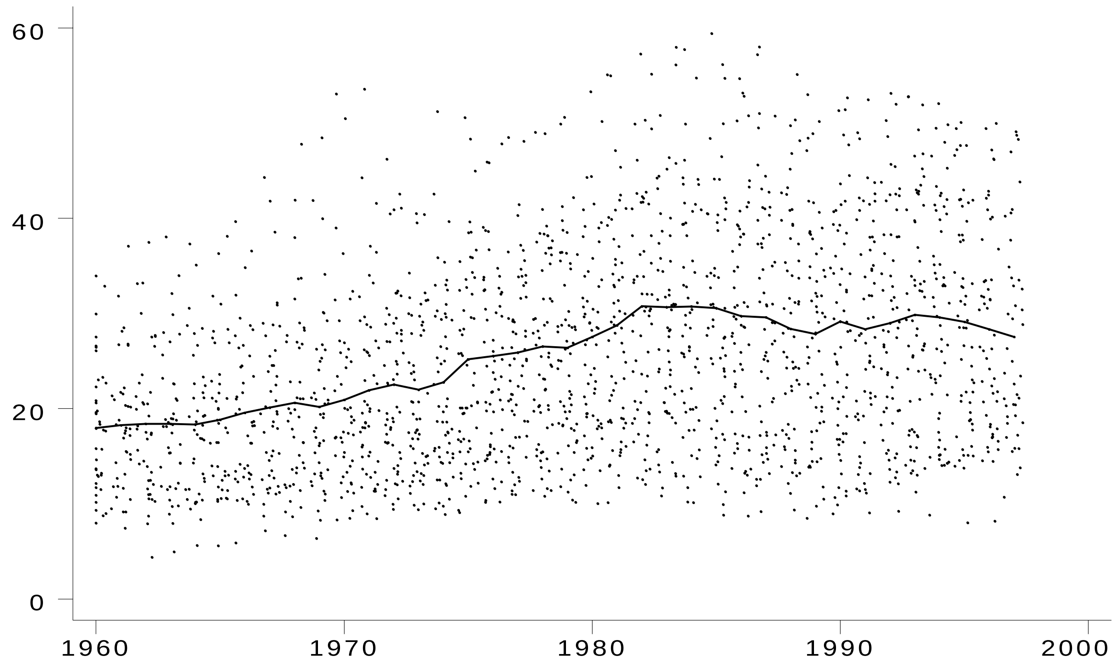

Figure 2

Political Institutions 1995

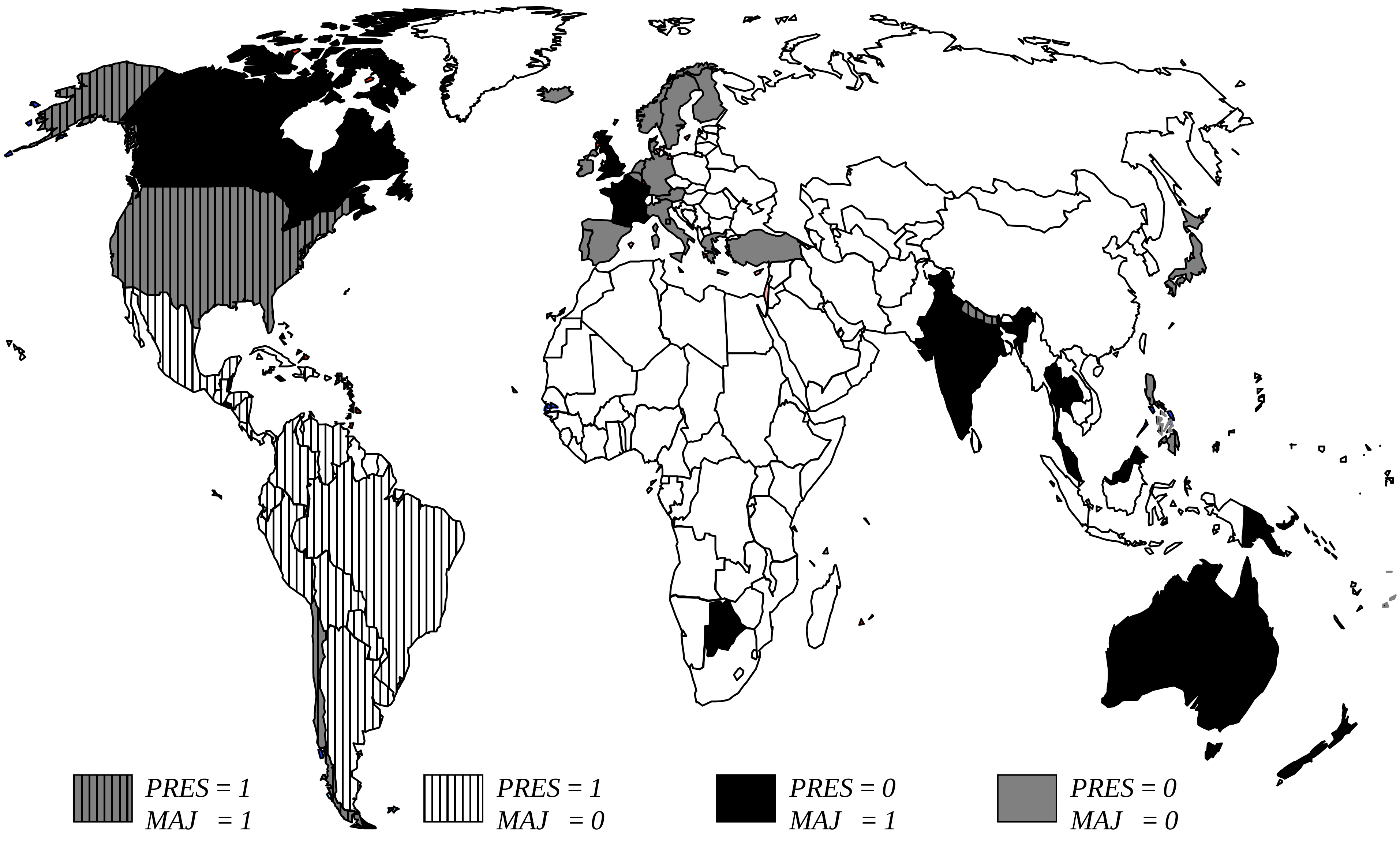

Figure 3
Size of Government 1960-98