Do political institutions shape economic policy ?

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First draft: August 21, 2000
This version: October 18, 2000

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 studies issues related to politics with the tools commonly used in 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 (2000).

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 a number of countries over the last four

*This paper is based on my Walras-Bowley lecture given at the Eighth Word Congress of the Econometric Society, in Seattle August 2000. I am heavily indebted to Guido Tabellini, who is a co-author of all the work underlying this leture and provided detailed comments on a previous draft. I would also like to thank Givanni Favara for research assistance and Christina Lönnblad for editorial assistance. The research is supported by grants from the European Commission and the Swedish Council for Research in the Humanities and Social Sciences.

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decades. 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. (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 of my lecture, which I would like to devote to current research on largely unresolved issues. As the title suggests, I will focus on ongoing work attempting to identify what systematic effect political institutions might have on economic policy outcomes. To narrow this broad question down, I will confine the discussion to the institutions governing electoral rules and political regimes and their effect on fiscal policy, broadly defined.[1] This question is not only of academic interest. Reform of electoral institutions, for instance, have 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 the discussion of reform.

I will make two main points. First, the question whether political institutions shape policy should naturally appeal to an economist. Second, 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 we have just assembled, with the aforementioned theory as the main guide in sampling and measurement (Section 3). A good part of my lecture will report on two 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).

[1] Other work by economists on the broader 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.

2. Theoretical ideas

2.1. An organizing framework

Let me describe the theoretical ideas against the backdrop of a very simple model. In particular, let us consider a bare-bones model of fiscal policy which highlights the size of the government budget and its allocation to different purposes. [2]

The population 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 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. Government spending can thus be targeted to specific groups, as in the case of targeted transfer programs, or local public goods. But it can also take a non-targeted form benefiting all citizens, as in the case of general public goods, or broad social programs.

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 one item. The variable r does not appear directly in the citizens' payoffs. Literally, it 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 model is obviously very stylized. Richer economic models can certainly be studied along the same lines. Citizens would then also interact in markets, making purposeful economic choices influenced by policy. Similarly, we could replace the simplistic form of rent extraction with a structural model.

[2] Models like this one forms the core of the positive models of fiscal policy in Persson and Tabellini (2000a).

Yet, already the bare-bones model permits a rich analysis of the politics of policymaking. To see this, note that the choice of q generates conflicts of interest in three different dimensions:

(i) First, we have the traditional conflict among different groups of voters over the allocation of targeted spending $\{f^{\mathsf{J}}\}$. (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 way the three conflicts are resolved, and thus what 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. In a market, we have 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, which determines the barriers to entry and the scope for competition between producers. They also hinge on legislation, which determines how easily consumers can hold producers accountable for bad product quality or collusive pricing behavior. The basic idea here 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; that is, what political scientists call 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]

[3]Recent classics within the political science literature on comparative politics include Bingham Powell (1982), Lijphart (1984), Taagepera and Shugart (1989), Shugart and Carey (1992),

This general discussion suggests a way of modeling the outcome of policymaking: q in our simple model. 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 that 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).

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.

Anticipating already here the empirical part, 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 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

and Cox (1997); see Myerson (1999) for a discussion of this literature.

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.

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 model above, are more effective in seeking broad support and targeted programs, like $f^{\mathtt{J}}$, more effective in seeking narrow support. Proportional elections with larger districts should thus be more biased towards broad, non-targeted programs. This point has formally been made by Persson and Tabellini (1999) and Milesi-Ferretti, Perotti and Rostagno (2000).

Larger districts also facilitate entry in the political process by additional candidates or parties. Myerson (1993) has shown how this may produce lower equilibrium rents under proportional elections. 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, which reinforces the previous prediction associating broader spending programs with proportional elections (Lizzeri and Persico, 2000, Persson and Tabellini, 2000a, Ch. 9).

Under majoritarian elections, 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 larger. Smaller expected vote losses under proportional elections make candidates choose policies entailing larger rents (Persson and Tabellini, 1999).

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 (2000a, Ch. 9) examine the policy consequences of this feature in Holmström (1982)-style, career-concern models. They find that proportional representation 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. 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 under plurality and because these concerns are at their strongest just before elections.

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 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 cruder classification of real-world regimes. Presidential regimes (abbreviated *PRES*) 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 (*PARL*), 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 taxes (r and τ in the model).

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. Spending in parliamentary

regimes thus optimally 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, presidential regimes should have smaller governments than parliamentary regimes, less spending on broad programs, and less rents for politicians. Under majoritarian elections, we should see less spending on broad programs than under proportional elections, and less rents. These are "cross-sectional" predictions; they have been derived by comparing equilibria in static models. The prediction of more pronounced electoral cycles under majoritarian elections, however, relies on a dynamic model and is thus a "time-series" prediction.

Is this kind of analysis convincing? Some of you may be skeptical. One critique might question whether the simple assumed game forms capture the essence of real-world political institutions. This would parallel the critique against theoretical IO that "you could prove anything by picking the right extensive form and the right informational assumptions". A related complaint would parallel the critique against incomplete-contract theory that "there are many alternative assignments of control rights and you have no strong basis for choosing this particular one".

Such criticism has some force, but may be less damaging in this case, 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. From this perspective, comparative politics might offer a more convincing application of game theory than other examples in economics.

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 the data on political institutions and policy outcomes we have assembled for ongoing empirical research. More details can be found in Persson and Tabellini (2000b). In fact, the theory I just sketched has served as our guide in sampling and measurement. We have data for 61 countries. The data is yearly and runs from 1960 to 1998, a total of 39 years. This panel includes a large number of economic, social and political variables. But many observations are missing – for different reasons – which makes the panel unbalanced.

Which countries are included in the panel? The theory suggest we should study countries with democratic institutions. To assess a country's democratic status in a given year, we rely on the well-known Freedom House index of political rights: the so-called GASTIL-index. So far, we have used two selection rules. One is to include a country in the sample from the first time in the sample period it first reaches a GASTIL-score of less than or equal to 5, signifying that the country is "free", or "semi-free".[4]. Another more demanding rule is to require a score strictly less than 4, year by year for inclusion in the sample. These criteria imply about 80 candidate countries towards the end of the sample period (considerably fewer in the beginning). Unfortunately, non-availability of data on political institutions or fiscal policy cuts down the sample size further. In this paper I will only present results based on the broader sample. As the more comprehensive analysis in Persson and Tabellini (2000b) demonstrates, most results are similar in the more narrow set of democracies.

Which political institutions do we study? Following the theory, I will report on results that (mostly) rely 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 dummy variable MAJ takes a value of 1 in the former case, 0 in the latter.[5] Second, countries where the survival

[4]The index runs (in half points) from 1 to 7, where a countries with scores of 1 or 2 are "free", 3 to 5 "semi-free", and 6 to 7 "not free".

[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.

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*.

There are very few changes over time in these classifications. 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 our key maintained assumption, namely to treat institutions as exogenous and given by history, may be correct.

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 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 a presidential regime, whereas France is not.

We include fiscal policy outcomes suggested by the theory. For the size of government (corresponding to τ in the model) 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. $\{f^{\mathrm{J}}\}$ in the model) 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 presumption is that broad transfer programs, like pensions and unemployment insurance, are much harder to target narrowly than spending on goods and services.

These policy measures do vary greatly across time and across countries. Indeed, *Figure 1* in the introduction was a plot of our panel data for central government expenditure as a percentage of GDP.

Towards the end of the next section, I will also describe some results from a second ongoing project (Persson, Tabellini, and Trebbi, 2000). There, we proxy rent extraction by politicians (r in the model) by available measures of corruption. We also characterize the electoral rule with two, continuous measures, rather than by a single binary measure.

Specification. Our empirical work is certainly motivated by theory. We aim as much at establishing empirical regularities as at testing specific hypotheses, however. We therefore adopt a relatively eclectic empirical specification describing policy outcomes:

$$y_{it} = \alpha_i + \boldsymbol{\beta}_i u_t + \boldsymbol{\gamma}_i s_{it} + \boldsymbol{\delta} x_{it} + \boldsymbol{\eta} z_{it} + \varepsilon_{it} \ . \tag{1}$$

Here, y_{it} denotes a policy outcome in country i and year t. We allow for a country-specific component, α_i. Policy can be affected directly by the institutions 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 u_t and idiosyncratic variables (s_{it}, 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 nul hypothesis

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

i.e., the absence of a *direct* effect. Strictly speaking, this is what most of the theory discussed in Section 2 was really about. The other way is to test for the absence of an *indirect* effect

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

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.) We estimate these parameters 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

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

As (2) shows, the η parameter can be readily estimated on cross-sectional data. To take account of the unbalanced panel, we use Weighted Least Squares, weighing each country by the number observations in its panel.

Results from such regressions are displayed in *Table 2.* The dependent variable is either central government spending (as a percentage of GDP), or central government revenue are also included. 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. Given the clustering of observations in *Figure 2,* we use dummies for continents and colonial origin as additional controls. The table displays the estimated η parameters for the *PRES* and *MAJ* dummies. Bracketed expressions are p-values for false rejection of $\boldsymbol{\eta} = \mathbf{0}$. 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 statistically significant.[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, presidential regimes are associated with smaller governments by about 5 percent of GDP.

As columns 3 and 5 show, however, the negative effect of *PRES* is stronger, closer to 10 percent of GDP, for cross sections based on data from the 1990s, rather than the whole sample. Moreover, it is statistically more robust (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.

While these cross-sectional estimates are suggestive, they are potentially subject to simultaneity (omitted-variable, or selection) bias. We would therefore like to exploit the time variation in the data. But the lack of institutional variation over time makes it infeasible to circumvent the problem by conventional fixed-effects estimation. For practical purposes, $\mathbf{z}_{it}$ is given by a constant, $\mathbf{z}_i$, equal to the time average $\mathbf{z}_i$. Thus, we cannot separately estimate the effects on policy of a country's institutions $\mathbf{z}_i$ and other time-invariant, country-specific features α_i.

This is why we turn to the slightly different question embodied in the test of

[6] Milesi-Ferretti, Perotti and Rostagno (2000) find government expenditure to be smaller in countries with majoritarian elections in their study of the OECD countries over the same period. When we use general government expenditures as our dependent variable – as did Milesi-Feretti et al. – we reach a similar conclusion. In this case data availability cuts down our sample size considerably (to about 40 countries).

H_0^I, namely whether different political institutions shape different policy responses to economic and political events. Recall, however, that the models in Section 2 are all static, with the exception of the career-concern model of electoral cycles. Most of our tests for indirect effects of institutions should thus be seen as a search for empirical regularities rather than tests of specific predictions.

Unobservable common events It is plausible that a set of common economic and political events have affected fiscal policy in all countries. Think e.g. of 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 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, $\gamma_i = \gamma_j$ in (1). Then we can lump all country-specific variables together in x_{it} and rewrite (1) as:

$$y_{it} = (\alpha_i + \eta z_i) + [1 + \lambda(z_i - \bar{z})]\beta u_t + \delta x_{it} + \varepsilon_{it} \ . \tag{3}$$

We can use a set of time dummies to estimate, βu_t , the common effect of the common events in (3). The institution-specific effect of the common events u_t is proportional to the term $\lambda(z_i - \bar{z})$, where $\bar{z}$ denotes the cross-country average of z_i. The form of (3) suggests that we should estimate the crucial parameter λ by NLS, and include fixed effects to control for the country-specific intercept. We use both annual data and five-year averages. The latter may better handle measurement error and allow for discretionary adjustments of policy.

Table 3 shows some results. The country-specific controls are the same variables as in the cross-sectional regressions. 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 $\beta(u_t - u_{t-1}) = 1$. Coefficients of -0.7 and -0.35 mean that the effect of this event is 0.7 percent smaller in presidential (compared to parliamentary) regimes and 0.35 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, $\beta(u_T - u_1)$, differs across

institutions. The point estimates suggest that the cumulative difference between presidential and parliamentary regimes is close to 10 percent of GDP. This number fits well with the estimated cross-sectional difference from the 1990s reported in *Table 2*.

Observable economic events Alternatively, we can test for an effect of institutions on the adjustment to observable events. We start by economic events, then turn to electoral events. Yet another rewrite of (1) is given by the following expression:

$$y_{it} = (\alpha_i + \eta z_i) + (\beta + \phi z_i)u_t + (\gamma + \mu z_i)s_{it} + \delta x_{it} + \varepsilon_{it} \ . \tag{4}$$

In (4) the parameters ϕ and μ allow for institution-dependent adjustments to common and idiosyncratic variables. The observable common variable in u_t is the oil price, and the idiosyncratic variables in s_{it} include lagged policy, the share of the population above 65, and the deviation of income from its (Hodrick-Prescott) trend. One way of estimating the parameters in (4) is to control for the first, country-specific term by fixed country effects. Another is to wipe it out by taking first differences. In the latter case, we use either the OLS-estimator (in which case we include y_{it-1} in the regression) or the GLS-estimator (in which case we do not include y_{it-1}, but instead allow for heteroskedasticity and panel-specific autocorrelation in ε_{it}).

The results in *Table 4* once again indicate systematic indirect effects of institutions. Our estimates in columns 1-3 based on yearly data 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. The same institutions are also associated with less inertia in spending. Moreover, spending appears to adjust less to population shocks under majoritarian elections.

The estimates in column 4, based on 5-year averages, give a slightly different picture. Negative oil shocks (higher prices) raise spending, but less so in presidential regimes and under majoritarian elections. The positive impact of populations shocks on spending is now stronger, but is dampened under both these institutions. The differences relative to the yearly results probably reflect the greater possibility of discretionary adjustment as well as the averaging of the data.[7]

[7] Income shocks are measured at the business cycle frequency and thus largely wash out when we take five-year averages (results based on income growth rates accord better with the yearly results). The population shocks originally derive from census data at the five-year frequency, such that the yearly data in between largely reflect interpolation.

Understanding better the reasons behind these differences in the adjustment to different events is an interesting topic for future theoretical and empirical research.

Electoral cycles Finally, we look for an electoral cycle in total government spending or tax revenue and whether this cycle depends on political institutions. For this purpose, we expand 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 specification, including all economic shocks and controls, is the same as in *Table 4* (except that the we do not include the oil shocks but instead include a set of common time dummies to allow more precise estimates of the electoral cycle). We also use the same estimation methods.

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. These results are remarkable in their own right, as earlier studies have typically not found evidence of an electoral cycle in international data, with the exception of the recent study by Shi and Svensson (2000).[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 percent of GDP – is present only in presidential regimes. The pre-election tax cuts, on the other hand, are visible only in parliamentary regimes. We do not have a good explanation for why. But according to column 5 these tax cuts only appear under majoritarian elections, which is indeed the result predicted by the career-concern model of Section 2.

4.2. Composition of government

Let me turn to the composition of government. 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. In Persson and Tabellini (2000b), we 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*,

[8] See Alesina, Roubini and Cohen (1997) and Drazen (2000) for surveys of the earlier literature.

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 %. The 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 expected, we find positive effects of population shocks and negative effects of income shocks on social security and welfare spending. But these effects are significantly smaller, or even nullified, under majoritarian elections.

For *electoral cycles,* finally, the findings are quite intriguing. When we do not condition on political institutions, no electoral cycle is observed. But when we do condition we find 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, but in presidential regimes social spending tends to *fall* by 0.1-0.2 percent of GDP. Understanding the reasons behind these differences in the composition of the electoral cycle under different political institutions is again a 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.

Transparency International conducts a careful survey including 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). Unfortunately, these scores are only available annually from the late nineties. Therefore, we must limit our study to cross-country data from 82 countries.

On the other hand, this study includes finer measures of the electoral rule than the single, dichotomous *MAJ* dummy. Based on the theory, 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.

Some results from this study 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.

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.

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 as much as 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 posing it offers an attractive opportunity for a convincing application of game theory. I have also reported on ongoing empirical work, which suggests that the answer is a resounding yes.

Our results are summarized in *Table 7.* Empirically, presidential regimes are associated with smaller governments than parliamentary regimes, a smaller response of spending to different events, and a stronger post-election cycle but a weaker pre-election cycle. Majoritarian elections are associated with smaller broad spending programs than proportional elections and with less corruption; they also have smaller spending responses to events and a stronger pre-election cycle in taxes. 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 different events.

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, 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 the relationship between institutions and policy. Dynamic models are also necessary to understand government deficits. The results in Persson and Tabellini (2000b) indeed point to systematic differences in deficit behavior across political institutions.

On the policy side, 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.

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.[9]

Some *econometric issues* certainly need to be explored in more detail. Even

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

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 also help in avoiding 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 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	1990-95
Estimation			WLS		
PRES	**- 7.95**	- 5.44	**- 8.65**	**- 4.99**	**- 7.46**
	(.005)	(.106)	**(.019)**	**(.080)**	**(.038)**
MAJ	-2.98	**- 3.90**	- 3.51	- 1.81	- 1.99
	(.178)	**(.095)**	(.155)	(.338)	(.394)
Controls	$\mathbf{x}_1$	$\mathbf{x}_1$ Cont.&Col.	$\mathbf{x}_1$ Cont.&Col.	$\mathbf{x}_1$ Cont.&Col.	$\mathbf{x}_1$ Cont.&Col.
# Obs.	1871	1789	335	1836	329
# Countries	61	61	60	61	59
R^2	0.54	0.64	0.69	0.72	0.68

p-values in brackets. Boldface fonts denote significance at the 10% level.

$\mathbf{x}_1$ includes the variables *TRADE*, *LYH*, *PROP1564*, *PROP 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
Sampling	Yearly	Yearly	Yearly	5-y avg.	Yearly
Estimation	NLS, FE				NLS, FE
PRES	**- 0.56**		**- 0.71**	**- 0.69**	**- 0.79**
	(.000)		**(.000)**	**(.000)**	**(.000)**
MAJ		**- 0.29**	**- 0.40**	**- 0.36**	**- 0.37**
		(.000)	**(.000)**	**(.001)**	**(.000)**
$\beta*(u_T - u_1)*$ *PRES*	**- 9.21**		**- 11.09**	**- 9.88**	**- 6.60**
$\beta*(u_T - u_1)*$ *MAJ*		**- 2.73**	**- 6.24**	**- 5.30**	**- 3.09**
Controls	$\mathbf{x}_1$	$\mathbf{x}_1$	$\mathbf{x}_1$	$\mathbf{x}_1$	$\mathbf{x}_1$
# Obs.	1871	1871	1871	402	1836
R^2	0.84	0.83	0.85	0.88	0.87

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			
Sampling	Yearly	Yearly	Yearly	5-y avg.
Estimation	FE, levels	OLS, diff.	GLS, diff.	GLS, diff.
LAG_SIZE	**0.85**	**- 0.18**		
	(.000)	**(.000)**		
P∗LAG_SIZE	**- 0.11**			
	(.000)			
M∗LAG_SIZE	**- 0.08**			
	(.031)			
PROP65	**0.20**	**1.23**	**1.17**	**1.13**
	(.075)	**(.003)**	**(.000)**	**(.001)**
P∗PROP65	0.10	- 0.24	- 0.66	**- 0.99**
	(.690)	(.774)	(.157)	**(.040)**
M∗PROP65	0.17	**- 1.44**	**- 1.00**	**- 0.96**
	(.213)	**(.024)**	**(.008)**	**(.007)**
OIL	**0.07**	**1.38**	**0.68**	**3.12**
	(.000)	**(.000)**	**(.003)**	**(.000)**
P∗OIL	0.07	**- 0.96**	**- 0.65**	**- 1.43**
	(.713)	**(.062)**	**(.021)**	**(.000)**
M∗OIL	0.02	**- 1.20**	**- 0.70**	**- 1.04**
	(.905)	**(.026)**	**(.010)**	**(.001)**
YSHOCK	**- 0.13**	**- 0.21**	**- 0.19**	- 0.07
	(.015)	**(.000)**	**(.000)**	(.688)
P∗YSHOCK	**0.16**	**0.27**	**0.24**	- 0.19
	(.006)	**(.000)**	**(.000)**	(.313)
M∗YSHOCK	**0.12**	**0.14**	**0.07**	- 0.03
	(.044)	**(.006)**	**(.048)**	(.862)
Controls	$\mathbf{x}_2$	$\mathbf{x}_2$	$\mathbf{x}_2$	$\mathbf{x}_2$
# Obs.	1815	1752	1809	1474
R^2	0.79	0.07		

p-values in brackets. Boldface fonts denote significance at the 10% level.
P and *M* denote interaction with the *PRES* and *MAJ* dummies, respectively
$\mathbf{x}_2$ includes the same variables as $\mathbf{x}_1$ except *PROP65*, plus the income trend corresponding to *YSHOCK* (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, levels	Time FE, diff.	FE, levels	Time FE, diff.	Time FE, diff.
$PRES*EL_t$	0.38	0.18	- 0.06	- 0.19	- 0.19
	(.262)	(.589)	(.807)	(.447)	(.439)
$PRES*EL_{t-1}$	**- 0.98**	**- 1.18**	**0.521**	**0.59**	**0.59**
	(.004)	**(.000)**	**(.044)**	**(.022)**	**(.022)**
$PARL*EL_t$	- 0.03	0.13	**- 0.38**	**- 0.28**	- 0.07
	(.895)	(.552)	**(.019)**	**(.074)**	(.714)
$PARL*EL_{t-1}$	- 0.22	- 0.10	0.05	0.17	0.17
	(.320)	(.640)	(.738)	(.272)	(.383)
$MAJ*EL_t$					**- 0.54**
					(.064)
$MAJ*EL_{t-1}$					0.00
					(.999)
Controls	$\mathbf{x}_3$	$\mathbf{x}_3$	$\mathbf{x}_4$	$\mathbf{x}_4$	$\mathbf{x}_4$
# Obs.	1718	1656	1686	1656	1623
R^2	0.80	0.13	0.83	0.08	0.08

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.
$\mathbf{x}_3$ includes the same variables as $\mathbf{x}_2$ plus all the variables (including the interaction terms) in column 1 of *Table 4* except *OIL*, 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.
R^2 in the fixed-effects regressions (columns 1 and 3) refers to the within estimator.

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.
$\mathbf{x}_b$ 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

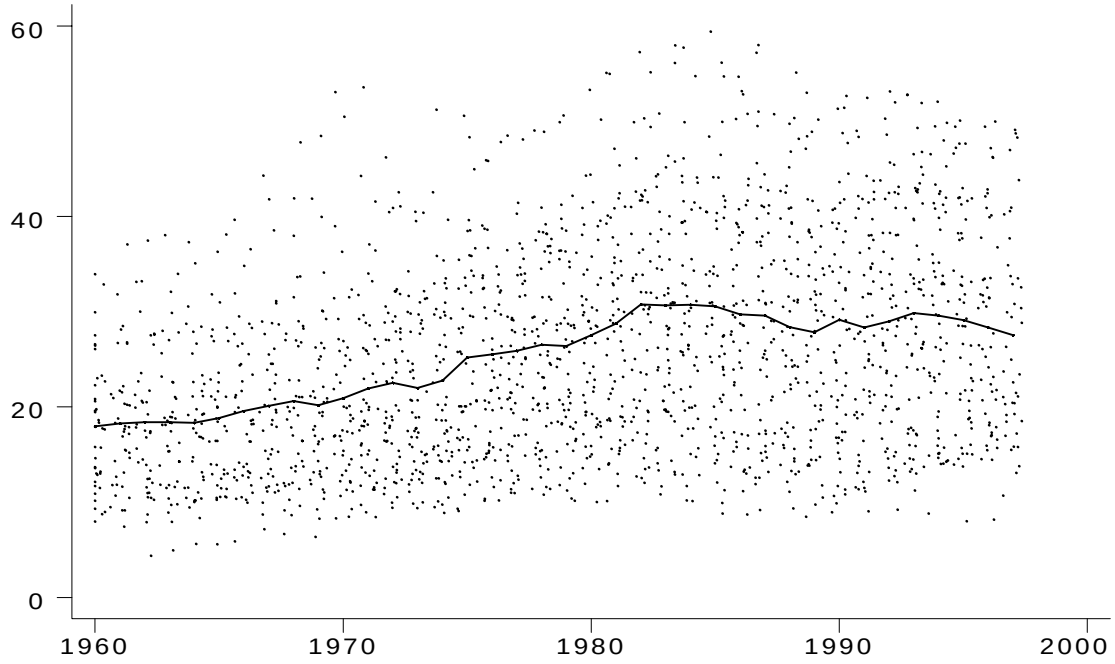

Figure 2
Political Institutions 1995

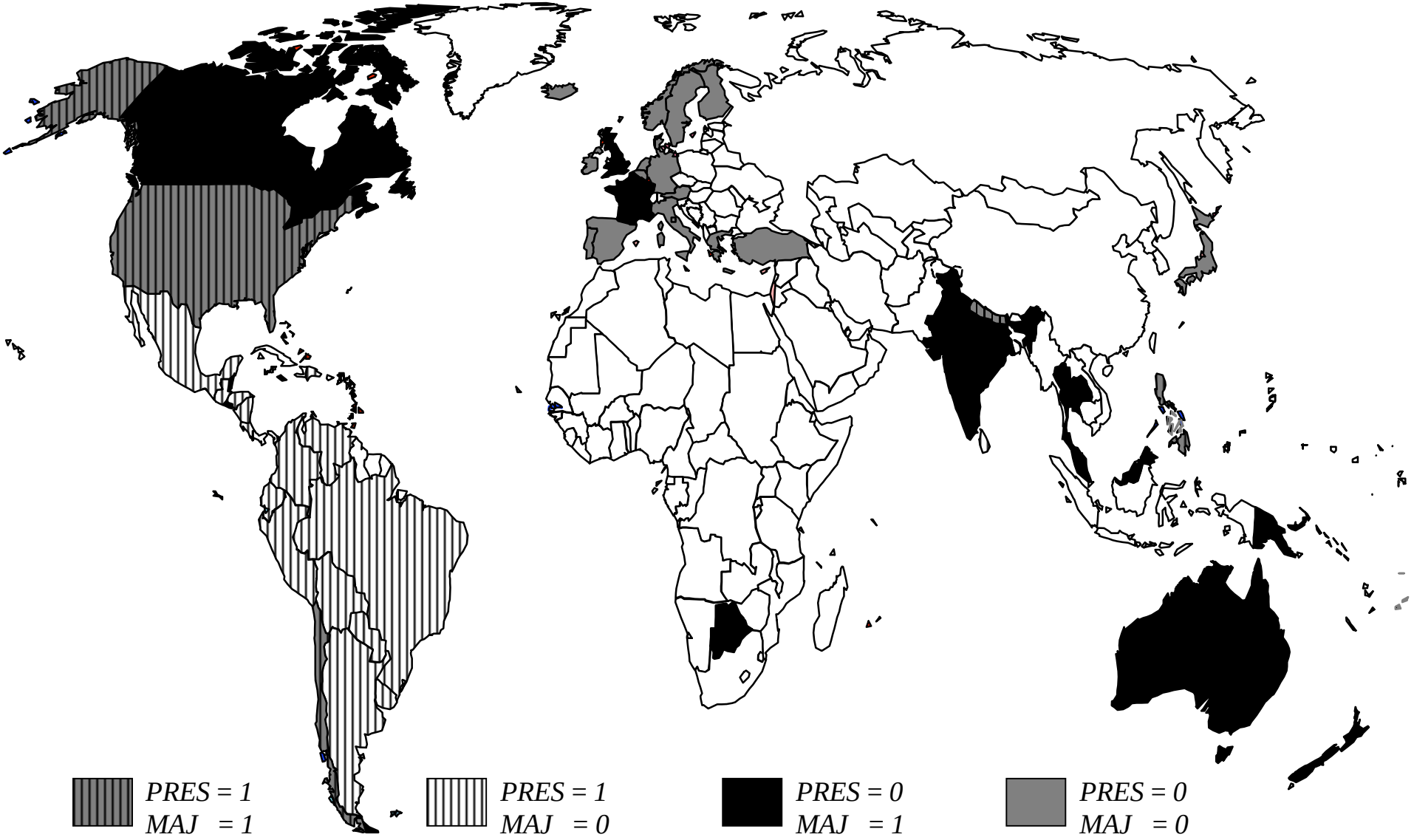

Figure 3
Size of Government 1960-98