Consequences of Constitutions*

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Abstract

The paper presents empirical findings regarding the economic policy consequences of constitutional arrangements, in three different dimensions. First, the data are consistent with several theoretical predictions about the consequences of electoral rules and forms of government for fiscal policy and rent extraction. Second, we should consider not only the direct consequences of constitutions via policy-making incentives, but also the indirect consequences via the structure of political representation. Empirical tests of the predictions from a new comprehensive model of parliamentary democracy show that proportional elections raise government spending through their indirect consequences for party structures and types of government. Third, constitutional arrangements may have important consequences for structural polices promoting long-run economic performance. New empirical results suggest that such consequences of constitutions may furnish us with a missing link in the chain from history to current economic performance. All these empirical findings appear statistically robust, and the estimated effects are large enough to be of genuine economic interest. (JEL: D72, E60, H00)

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Five years ago, in Berlin, I gave the Marshall lecture with Guido Tabellini. That lecture described our first steps, mostly theoretical steps, into a new research field: how constitutional rules influence economic policy (Persson and Tabellini, 1999). In this lecture, I would like to report on how that research is developing, and how it might develop in the future. My discussion will come in three parts, three variations on a common theme: the consequences of constitutions. Let me begin by putting the three parts in context.

Recent theoretical studies by economists have asked how incentives for policymaking might differ under alternative electoral rules and alternative forms of government. *Figure 1* illustrates the purpose of the theory. We represent different constitutional arrangements by different rules in an extensive-form game, and try to make ceteris paribus predictions about the consequences for economic policy outcomes. These predictions have motivated data collection and subsequent empirical studies. Embarking on such empirical studies, however, one quickly runs into problems of statistical inference.

The most serious one is illustrated in the figure. The data strongly suggest that constitutional rules are not randomly selected. And the same features of history, geography and culture may well influence both constitutional rules and the policy outcomes these rules are predicted to shape. Resolving this endogeneity problem is a major goal in the literature. Part 1 of the lecture will present some results from this work, drawing on a new book (Persson and Tabellini, 2003) and new articles (Persson and Tabellini, 2004, and Persson, Tabellini, and Trebbi, 2003). The constitutional effects turn out to be statistically robust and quantitatively important.

The research in the first part sits close to the border between economics and political science. In Part 2, I will discuss how one might build a more solid bridge between the disciplines. Traditional political science does not really deal with the direct consequences of constitutional rules, operating through incentives for policymaking.

As illustrated in *Figure 2*, however, it deals very much with the consequences for the structure of political representation – the number of parties, the type of governments, and so on. Such political outcomes have typically been taken as given in the research by economists. But they are likely to spill over into policy decisions, creating indirect consequences for policy. How do we sort out the direct and indirect consequences?

Obviously, we need a more encompassing approach. In Part 2, I will give an example of such an approach when describing the main results in a new paper with Gerard Roland and Tabellini (Persson, Roland, and Tabellini,

2003). The paper proposes a model of a parliamentary democracy, which is rich enough to allow for direct as well as indirect consequences of rules for elections. In addition, we take the model's predictions of the consequences of alternatives rules to the data. These predictions appear to hold up, both when it comes to political outcomes and economic outcomes.

The final part is about bridging another gap. Recent empirical research on the macroeconomics of development has singled out certain "structural polices" as essential for long-run economic performance (Hall and Jones, 1999, and Acemoglu, Johnson, and Robinson, 2001). In doing so, it has tackled a fundamental endogeneity problem that stems from performance feeding back to policy. As illustrated in *Figure 3*, the approach has been to suggest specific instruments: historical, geographical or cultural features that influence structural policies but have no independent influence on performance. This work supports the common idea that "institutions" are essential for development. But the concept of institutions remains a bit vague.

In Part 3, I will propose that constitutions may have systematic consequences also for growth-promoting policies. If they do, this would help fill the institutional void between history and current performance, in the way illustrated in the figure. To shed more light on this suggestion, I have just assembled some new data. I use these, first, to replicate some of the central results in the earlier literature, then, to investigate whether there is any empirical support for the idea of systematic consequences of constitutions in this dimension. My first round of empirical results are quite encouraging.

After laying out this crude road map, I'd like to turn to the first part.

1 Consequences for economic policy

The empirical research I want to describe here deals with the consequences of alternative electoral rules and forms of government. I will briefly describe the general approach and specific results for fiscal policy and political rents. Much greater detail can be found in the original work (Persson and Tabellini, 2003, 2004, Persson, Tabellini, and Trebbi, 2004).

The most interesting variation in electoral rules and forms of government is found at the national, not the sub-national, level.[1] We have put together

[1] Besley and Case (2003) survey the empirical work on the policy consequences of the rich variation in other aspects of political institutions across the US states.

two data sets, each with as many democracies as a generous definition permits. Specifically, we have a pure cross section, with average observations for 85 countries over the 1990s, and a panel with annual observations for 60 countries going back to 1960.

Two features of the constitutional data are crucial to recognize. First, deep reforms of electoral rules and forms of government are very rare among democracies, even though marginal reforms occur more frequently. In some cases, estimation of constitutional effects on policy must thus rely on the cross-sectional variation in the data.

Second – and, as I already mentioned – constitutions have not been selected at random. For instance, majoritarian elections are over-represented in previous British colonies and under-represented in Europe. Presidential regimes are over-represented in Latin America, in poor economies and in young democracies. Systematic selection is likely also on less observable features. Estimation of constitutional effects must thus beware against different forms of selection bias.

To avoid prospective statistical pitfalls, we apply several estimation methods. The first is regression with a "rich right-hand side". But this standard method relies on conditional independence and linearity, which may both go astray in the wake of non-random selection. Thus we try to relax these assumptions in various ways.

In order to reduce selection bias from unobservables – that influence both constitutions and policy choices – we can try to find a good instrument. This way is attractive, but often difficult. Our best idea has been to exploit systematic co-variation between the relative frequency of alternative constitutional rules and their date of introduction. We thus use pure timing as an instrument for constitutional choice, while controlling for the effect of age of democracy on policy outcomes. Another way to relax conditional independence is to correct the estimates, Heckman-style, for selection bias. In applications where piecemeal reforms are relevant, we can also use our panel data to eliminate bias, at least from time-invariant unobservables.

If constitutional effects are heterogeneous, we may have problems even with selection on observables. If a reform from presidential to parliamentary governance has different effects in Latin America than elsewhere, over-representation of presidentialism in Latin America may bias our estimates. Obviously, we can test explicitly for such interaction effects and do so for a few possible culprits. A more general and parsimonious way to relax linearity is to rely on non-parametric methods, such as matching.

I would like to report on three sets of empirical results. For each of them, I will make a few remarks about the underlying theory and about our measurement. I will then summarize the constitutional effects that are robust to alternative methods of estimation.

Hypothesis E1 A first hypothesis has to do with electoral rules. Suppose legislative seats are awarded by plurality, rather than proportional representation (PR). This should tilt policy towards spending programs with benefits targeted to particular groups not the electorate at large, and (perhaps) towards higher overall spending. (A similar effect can be expected by smaller district magnitude, i.e., by the legislature being elected in a larger number of districts.)

The main theoretical idea behind this hypothesis is quite straightforward. If candidates with the highest vote shares win every seat at stake - rather than seats in proportion to their vote share - it becomes more attractive to target small and geographically concentrated groups of voters. (The same will hold true if each district has a small share of the electorate.)

We thus classify actual electoral systems according to their electoral formula (classifying by district magnitude gives similar results). A binary indicator (called *maj*) is coded to 1 where the plurality method is used, and 0 where it is not. For spending, we proxy geographically non-targeted spending by welfare-state programs, such as pensions and unemployment insurance. Overall spending is measured by total expenditures by central government.

The results indicate that a reform from an all-PR to an all-plurality-rule system would cut welfare spending by about 2% of GDP in the long run. This estimate is robust for a set of older democracies, a little more shaky for all democracies. When it comes to overall spending, the finding is robust: an electoral reform would cut spending by about 5% of GDP - a substantial number indeed.

Hypothesis G1 A second hypothesis concerns the form of government. Presidential, as opposed to parliamentary, regimes are expected to cut total spending and non-targeted spending programs.

A key point in the theory is whether the executive is subject to a confidence requirement in the legislature. As presidential regimes have no such requirement, legislative majorities become less stable than in parliamentary regimes. If majorities re-form, issue-by-issue, broad spending programs suf-

fer. No well-defined stable majority becomes a residual claimant on tax revenue, and this reduces the incentives to boost overall taxation and spending.

In the data, we thus classify forms of government by a binary indicator (called *pres*), which is coded to 0 or 1 depending on whether the executive is or isn't accountable to the legislature. Broad programs and overall spending are measured as before.

When it comes to welfare state programs, we find the hypothesized result only among good democracies, where presidential regimes spend less, by about 2% of GDP. For overall spending the results are very robust, however, and in line with the hypothesis. Presidential regimes have smaller governments by at least 5 % of GDP - again, a large number.

Hypothesis E2 A third hypothesis has to do with detailed features of electoral rules. Systems where a larger number of lawmakers are elected in each district, and systems where they are elected on personal rather than party-list ballots, are expected to reduce rent extraction by politicians.

The key idea here is that extraction is better deterred if chances of reelection for individual lawmakers (or parties) may respond more forcefully to their performance. Larger electoral districts achieve this by allowing easier entry and a larger number of candidates than small districts. Personal ballots impose individual accountability and stronger incentives than party-list ballots, which impose only collective accountability.

Empirically, we thus measure each system's district magnitude along a continuous scale, as political scientists do. We also measure the share of lawmakers elected on party lists. vs. personal ballots. As proxies for political rents, we use different perception indexes of corruption, or of inefficiency in the delivery of government services.

We find sizable effects in the hypothesized direction. Here, we can exploit a number of recent reforms in electoral rules that change district magnitude, ballot structure, or both. Luckily, the estimates from cross-sectional and panel data agree pretty well.

In addition to these tests of theoretical predictions, we have done more exploratory work, to establish some stylized facts about constitutions and policy. For instance, we find smaller budget deficits in majoritarian than proportional democracies. Adjustment of fiscal policy to income shocks differs systematically across different forms of government. And electoral bud-

get cycles take quite different forms, depending on both these constitutional features.

More details about these results, and about our data and empirical methods, can be found in work cited above.

2 Consequences for political representation *and* economic policy

To introduce the second part of the lecture, let me return to the finding that overall spending is higher under proportional than majoritarian elections. The theory behind **Hypothesis E1** suggests this to be a direct effect, due to different aggregation of policy preferences by alternative electoral rules. But that theory treats the structure of political representation as exogenous.

Research by political scientists suggests that this omission may be important. A long tradition in comparative politics, involving Duverger (1954), Lijphart (1990) and many others, shows how electoral rules shape party structure. Majoritarian elections produce fewer parties and a more skewed distribution of seats than proportional elections. Moreover, ample documentation, by Taagepera and Shugart (1989), Strom (1990) and others, suggests that party structure shapes the type of government in parliamentary systems. Few parties mean more frequent single-party majority governments, and less frequent coalition governments, than many parties. Finally, research by economists and political scientists finds that coalition governments spend more than single-party governments (see e.g., Kontopoulos and Perotti, 1999), perhaps because they are more plagued by so-called common-pool problems. At least for twenty or so developed parliamentary democracies, there is some evidence for each of these links separately.

These findings suggest a possible indirect explanation of higher spending under proportional elections: a larger number of parties boost spending via a higher incidence of high-spending coalition governments. So, to what extent do the empirical findings reflect direct and indirect consequences of electoral rules? Because the empirical results described in Part 1 are on reduced form, they are not helpful in discriminating between the two.

To sort out the question, we need theoretical guidance how to approach the data. Allowing for both direct and indirect effects means that we must model not only policy formation, but also party formation and government

formation, as endogenous. Models of this kind do not yet exist. One reason is that the modeling challenge is hard. Another is probably that the challenge falls in the cracks between two academic disciplines.

I will give an example of how we might approach the task, by summarizing the main arguments in Persson, Roland and Tabellini (2003). There, we build a comprehensive model of parliamentary democracy. We also test empirically the joint predictions regarding the political and economic consequences of different electoral rules. I begin by briefly describing the model, and some of its main predictions. Next, I describe the data, and selected empirical results.

Model structure We consider a single period of economic activity. The population is divided in four groups of equal size, indexed by J. These have quasi-linear preferences, given by

$$V^J = y - \tau + H(g^J) \ .$$

A universal tax rate, τ, cuts the resources available for consumption. Each group derives concave benefits from group-specific spending, g^J, provided by the government. The sitting government decides how to use the revenue from taxing all the groups

$$4\tau = \sum_J g^J + \sum_P r^P \ ,$$

either for group-specific spending programs or for party-specific rents, r ; the index P thus refers to parties.

While the economic side of the model is very simple, the political side entails a four-stage game. (1) Groups of opportunistic politicians make forward-looking, strategic decisions which parties to form. (2) A government is formed by a subset of the parties. (3) Parties in government make strategic policy decisions. (4) Backward-looking voters cast their ballots non-strategically, but conditional on policy.

The modeling revolves around two ideas. In party formation, politicians weigh expected electoral outcomes against expected rents if holding government office. And policy formation is driven by electoral conflicts: all governments face a conflict with the opposition, but strong intra-government conflict is present only in coalition governments.

At the party formation stage, there are four pre-existing groups of legislators (we don't consider the preceding election), one per economic group J. Each legislative group has to decide whether to merge with another group into a large party or form a small party on its own. Given the choices of others, each group maximizes expected equilibrium payoffs: $E\{E({}_N W_G^J)\}$. At this party formation stage, there is double uncertainty, concerning government status, G (see below), and electoral outcomes. To simplify things, we impose some constraints on the party coalitions allowed to form. From this stage, we obtain an equilibrium party configuration, where the number of parties, N, can be 2, 3 or 4.

Government formation is modeled very simply, as a mechanical lottery. Nature decides on the parties in government, under the constraint that they command 50% of the legislature. If $N = 2$ only single-party governments are possible. If $N = 4$ only coalition governments are possible, and if $N = 3$ both types can occur. This stage produces a government-status outcome for each party: it can be part of a single-party government, S, a coalition government, C, or the opposition (out of government), O.

At the policy formation stage, the parties in government maximize an objective defined over current rents and expected seats:

$$E({}_N W_G^P) = r_G^P + E({}_N s_G^P)R$$

A single-party government makes all decisions on spending, $\{g^J\}$, and rents, r^P, unilaterally (the tax rate, τ , follows from the government budget constraint). In fact, if we allowed the two groups forming a large party to bargain over policy, they would agree on all policy instruments. In coalition government, the two parties agree on spending targeted to groups not represented in government. We assume that each party unilaterally sets spending for its own economic group $g^J, J = P$, and its own party rents,.r^P. (Think about an allocation of ministers to parties.) As parties now face separate electoral rewards (see below), these choices create intra-government conflict.

Finally, backward-looking voters cast their ballots; some only on the basis of ideology, others also on the basis of policy. The latter, semi-attached, voters drive most results. Consider the semi-attached voters of group J. Suppose their own party, $P = J$, is in government. They then reward the party, by casting their ballot in its favor, if their utility exceeds a reservation level: the right-hand side of

$$V^J \geq V^{*J} + \omega^i + \delta \ . \qquad ((*))$$

The popularity shocks, ω^i and δ, serve to create electoral uncertainty at the previous policy formation stage. If the utility of the semi-attached voters in J is lower than the reservation level, they instead support the opposition – randomly, if the opposition consists of two parties. If their own party is not in government, the semi-attached randomly support the government parties if $(*)$ is satisfied; if not, they vote for their own party. This probabilistic voting behavior creates distinct incentives in different governments. Voters cannot separately reward two groups of legislators who have merged into a single large party. But they can, if the two groups have formed small parties and joined forces in a coalition government.

An equilibrium in the model is a party structure, a set of possible governments, and policy choices for each government, produced by subgame perfect choices at each stage.

Theoretical results and empirical predictions We compare equilibrium outcomes under two stylized electoral systems, which decide how votes are converted to seats. Under proportional elections, all legislators are elected by PR in a large, nation-wide electoral district. Under majoritarian elections, all legislators are instead elected by plurality rule in small, single-seat districts.

Moreover, we consider alternative compositions of electoral districts (under majoritarian elections). Our benchmark case is symmetric: all districts have exactly the same composition. But we also study alternatives, where economic groups are asymmetrically distributed across districts.

We show that government spending depends only on the type of government, coalitions spending more than single-party governments. (With asymmetries this has to be slightly modified, substituting mainly for only.) Intuitively, electoral conflict within government coalitions raises spending on the groups represented in government and thereby overall spending. The electoral system thus only (mainly) exerts indirect effects on spending, by affecting the type of government.

When it comes to overall rents, the electoral system does have direct effects. Single-party governments may extract more or less under proportional than majoritarian elections, depending on parameters. Coalition governments extract more rents than single-party governments, but only under proportional elections. Intuitively, intra-government competition may cut per-party rents, but this mechanism operates only in a winner-takes-all sys-

tem.

Equilibrium party structure depends crucially on the electoral rule. 4 parties is the unique outcome under proportional elections, producing only coalition governments. In the symmetric benchmark case, 2 parties is the unique outcome under majoritarian elections, producing only single-party governments. With asymmetric distributions of voters, 3-party (and 4-party) systems become possible equilibria, however, opening up for coalition governments. Intuitively, under majoritarian elections with plurality rule a small party has no advantage in terms of rents from office, but a clear electoral disadvantage. Under proportional elections, the opposite holds: a small party expects to capture larger rents at no electoral cost.

Two sets of predictions stand out. They can be summarized in the following two hypotheses regarding the political and economic effects of electoral rules.

> **Hypothesis PE**. Proportional elections raise the incidence of coalition governments, but only indirectly via party fragmentation (larger number of parties).
>
> **Hypothesis EE**. Proportional elections raise spending, but only indirectly via more frequent coalition governments.

In terms of the discussion at the beginning of this part, our model predicts indirect consequences, but no direct consequences of electoral rules on total spending.

How should we confront these hypotheses with data? Obviously, we can test whether the effects go in the expected direction. But the predictions are sharper, each hypothesis implying some over-identifying restrictions. While **PE** says that measures of electoral rules should be valid instruments for party structure, when explaining the type of government, **EE** says that electoral rules should be valid instruments for the type of government, when explaining government spending.

Data To carry out the tests, we combine data from two sources. One is the data used in the empirical work described in Part 1. The other source is a new panel data set for constitutional features and political outcomes, assembled in collaboration with political scientists (see Lundell and Karvonen, 2003).

For the 1990s, we can put together a cross section with data from about 50 parliamentary democracies. But we also have a panel, starting in 1960, with data from about 330 legislative periods in more than 35 democracies. As mentioned earlier, most existing political-science research on electoral rules, parties and governments relies on data from at most 20 democracies.

Table 2.1 shows means and standard deviations of four central variables, measured across countries and legislatures and cross-tabulated by electoral formula. The *party fragmentation* variable is computed as a Herfindahl index, going from 0 to 1, with higher values indicating more fragmentation. The type of government is measured by the incidence of *coalition* or *single-party* governments, respectively (The remaining type is minority government, but as our model does not allow them, we consider both complementary types). Total *government spending* is measured as in Part 1. Evidently, all means in the table conform to the model predictions: majoritarian electoral rules (numbers in the left column) are associated with less fragmented party systems, fewer coalition governments but more single-party governments, and smaller government spending.

Empirical results What happens when we approach the data in more sophisticated ways? First consider the political effects. They are estimated in three samples. Two of these only exploit the variation across parliamentary democracies, during the 1990s and 1960-98, respectively. But, given the prospective pitfalls in cross-sectional inference, we also exploit the within-country variation associated with electoral reforms. We use a panel with the last six legislatures in each democracy, because actual reforms are concentrated in the last two decades.

Column 1 in *Table 2.2* reports on a regression of *party fragmentation* in the 1990s on a set of controls and on measures of electoral rules. The first two of the latter are (binary) indicators of *majoritarian* and *mixed* electoral formulas, the third a (continuous) measure of *district magnitude*. All three have the expected sign and are statistically significant at the 10% level of better.

In column 2, we use these measures, plus a measure of electoral thresholds, as instruments for party fragmentation when explaining the incidence of coalition governments. As expected, more fragmentation has a strong positive effect on coalition governments. Moreover, we cannot reject the over-identifying restriction that the three electoral-rule variables of interest

only influence coalition governments indirectly. The chi-2 statistic from the Sargan test is comfortably within the acceptance region. Results are similar if we replace party fragmentation with the number of parties, or the incidence of coalition governments with the incidence of single-party governments.

Columns 3 and 4 show that, essentially, the same results come out when we average over four decades in a smaller number of countries. Columns 5 and 6 show that the main results stand when identified from the time variation around electoral reforms. All in all, **Hypothesis PE** holds up well.

What about the predicted effects on economic policy in the same three samples? *Table 2.3* speaks to this question. Now, the dependent variable is total government spending. Column 1 shows a reduced-form regression of spending on a set of controls and the exogenous political variables, including the electoral-rule variables. District magnitude and mixed electoral rules have a significant influence on spending. Column 2 shows a structural form, where we use the electoral rule variables of interest to instrument for coalition governments. Evidently, coalitions have a strong positive effect on spending. We saw before that the mean of *coalition* differs by about 0.3 across electoral rules. This translates into a spending difference of about 5% of GDP, on par with the reduced-form estimate reported in Part 1. As for the over-identifying restrictions, we cannot reject that electoral rules only have an indirect effect on spending.

The remaining columns show the corresponding estimates for the longer cross section and the panel. In both cases, the results are consistent with the predictions in **Hypothesis EE**.

More work certainly remains on this project. On the empirical front, we have not taken all of the model's predictions to the data, including its non-linear predictions for rents (coalition governments should have higher rents, but only in proportional democracies). On the theoretical front, we have not worked out the precise effects of different forms of heterogeneity. But, so far, the results are pretty promising.

3 Consequences for structural policy and economic performance

The third variation on my constitutional theme is about building a bridge to the macroeconomics of development. When faced with the question which

policies are particularly important for long-run economic performance, many economists would probably point to regulatory policies, providing wide protection of property rights and promoting accumulation of capital, human capital or productive knowledge. Another common answer might be non-protectionist trade policies, permitting appropriate price signals and promoting efficient resource allocation.

But attempts to empirically confirm such conjectures, at the macroeconomic level, faces major endogeneity problems. One is reverse causation: economic performance is likely to feed back to policy formation. Another problem is measurement error: gauging regulatory or trade policies across countries, or time, is quite a hazardous exercise.

Some recent contributions in the macro development literature made progress on these problems, by using history, geography or culture to isolate exogenous variation in structural policies. Hall and Jones (1999) launched the general idea that societies are more likely to pursue good structural policies the more strongly they have been exposed to Western European influence – for historical or geographical reasons. They thus used four instruments for a country's structural policy: its latitude, its predicted trade from geographical and population characteristics, and its current population shares with a European language, and with English, as their mother tongue.

Acemoglu, Johnson and Robinson (2001) took the argument further. Their ingenious idea was to use data on settler mortality in 19th century colonies to measure Western European influence. They argued that investment in "good institutions" was less intensive in places with a more dangerous disease environment, that good institutions are long-lasting, and that good institutions are conducive to good policy.

Replicating earlier results To be more concrete, let me illustrate these results by a little replication exercise. For each country, economic performance is measured by (the logarithm of) *output per worker;* that is, a measure of labor productivity. The results are similar for GDP per capita or TFP. *Structural policy* is the average of two indexes, one for the protection of private property rights, another for the years with open borders since 1950. The two indexes, and thus the overall index, are normalized to lie between 0 and 1. These variables are defined in the same way as the "output per worker" and "social infrastructure" variables in Hall and Jones (1999). But the data are new, in two dimensions. More recent statistics are used.

And each measure is computed not only as an average for a cross section of countries, but for every year since the early 1980s.[2]

Consider first the results for average outcomes in the 1990s, in the full set of about 100 countries where all data is available. *Table 3.1* begins by reporting a two-stage estimate of the effect of *structural policy* on *output per worker* (estimating by GMM means allowing for heteroskedasticity on unknown form). Column 1 shows the first stage. Controlling for continental location and identity of prior colonial powers, two of the four Hall and Jones instruments pass the relevance *cum* exogeneity test. As shown in the table, higher shares of European speakers and more favorable conditions for trade lead to better structural policies. Column 2 shows the second stage. Structural polices have a large, positive, and precisely estimated effect on performance. By these estimates, improving structural policy by one standard deviation raises long-run productivity by about 150%.

What happens when we run the same specification on the smaller (about 60 obs.) cross section, where the Acemoglu et al settler mortality data is available? Columns 3 and 4 show that the results from the larger sample hold up. If anything, the effect of structural policies is now larger.

Columns 5 and 6 use Acemoglu, Johnson and Robinson's (2001) measure of (the logarithm of) *settler mortality* as the sole instrument for structural policy, to more convincingly isolate exogenous variation in policy due to Western influence. The effect of structural policy on performance is still precisely estimated and even larger than before.

What do we make of these results? They certainly strengthen the presumption that structural policies shape long-run economic performance. A common interpretation in the literature is that good policy requires good institutions. This sounds very reasonable, but the institutional lessons remain quite vague. In particular, they do not give much guidance for reform.

[2]The data for computing output per worker are obtained from the Penn World Tables 6.1. The structural policy variable is computed as the simple average of two indexes: (GADP +YRSOPEN)/2. The GADP index measures the protection of private property rights in five dimensions; it is obtained from the IRIS-3 data set. The YRSOPEN index measures the number of years since 1950 a country is open according to five different criteria; it is an update by the original Sachs-Warner index, obtained from Easterly, Levine and Roodman (2003) and kindly made available by David Roodman.

There are two small differences vis a vis the measurement in Hall and Jones (1999). First, the labor productivity measure is not adjusted for output in the mining sector, as done by these authors. Secondly, when data are missing, we do not impute fitted values to replace missing data.

Do constitutions shape growth-promoting policy? Getting back to my main theme, I would like to suggest that certain constitutional rules might be a tangible source of good policy. If that were the case this would furnish us with one of the missing institutional links in the chain from history to current performance.

Why is this a plausible idea? From a theoretical perspective, we can think about wide protection of property rights and non-protectionist trade policy as provision of public goods, or as non-targeted policies benefiting broad groups in the population. Certain constitutional arrangements may better promote such policies, by similar arguments as in the existing models of fiscal policy. In part 1, I referred to the argument that parliamentary systems tend to better serve the interests of stable majorities of voters than presidential systems. I also referred to the argument that proportional elections tend to better serve the interests of broad majorities than majoritarian elections. Perhaps the mere existence of a stable democratic regime – whatever its type – is also favorable for policies with broad benefits, by allowing more widespread political participation than a non-democratic regime.

What do the data say? Persson and Tabellini (2003, Ch.7) start on this question, but provide only cross-sectional evidence for a set of democracies. Here, I would like to extend the sample to non-democracies, as well as to panel data.

Specifically, I will ask if some specific constitutional indicators help explain good structural policy and economic performance. One is an indicator for democratic rule. Following Persson and Tabellini (2003), I use two measures. I start by coding a country as democratic if its Gastil index from Freedom House takes a value below 5; alternatively I do so if its POLITY index in the Polity IV data set takes a positive value. *Age of democracy* is the number of years with uninterrupted democratic rule (according to the same criteria), going backwards from year 2000. *Parliamentary* and *proportional democracy* indicators are obtained by interacting the democracy indicators with the binary indicators for parliamentary regimes and proportional elections discussed in Part 1 (the indicators labelled *pres* and *maj*). Naturally, the prospective simultaneity problems remain, so I will also have to plausibly argue that the constitutional variables are exogenous to economic performance.

Cross section evidence I begin with cross-sectional estimates. Here, I first add my constitutional indicators alongside the two Hall and Jones instruments in the earlier two-stage specification. Columns 1 and 2 of *Table 3.2* show the first and second stage of the GMM estimates for the full 1990s cross section. The democracy, and proportional democracy indicators turn out not to be significant. But as shown in the table, the estimates suggest that parliamentary democracies, and older democracies, have better structural policies. The estimated effect of structural policy on productivity is close to the one in *Table 3.1.* We can test for exogeneity of all the instruments but one, and for exogeneity of the two constitutional variables by themselves. Neither test rejects (see the Sargan-Hansen J and C-statistics at the bottom of the table). According to the estimates, the constitutional effects are substantial in magnitude. Introducing parliamentary democracy (or, equivalently as the democracy indicator is insignificant, going from presidential to parliamentary democracy) improves structural policy so as to raise long-run productivity by about 40%.

The next two columns show that the results hold up in the smaller settler mortality sample. Auxiliary evidence can be obtained by scrutinizing the first stage of these estimates. I thus re-estimate the structural-policy equation, and use *settler mortality* as an instrument for parliamentary democracy. As an additional instrument, I use one of the constitutional timing variables mentioned in Part 1: *con5180* is set to 1 if the country is democratic and the present electoral rule or form of government was introduced in the period from 1951 to 1980, and to 0 otherwise. The last two columns of the table show that higher mortality (less Western European influence) reduces the likelihood of parliamentary democracy and this rule continues to have a strong favorable effect on policy. Moreover, we cannot reject that *settler mortality* only affects structural policy indirectly, through *parliamentary democracy.* Repeating the same exercise, but treating *age of democracy,* rather than *parliamentary democracy,* as endogenous, yields similar results.

The results clearly support the idea that constitutional arrangements may be an important link between history and growth-promoting policy.

Time series evidence More support comes from the panel data. In Part 1, I argued that deep constitutional reforms are rare among democracies. But as the data now include non-democracies, we can exploit reforms with entry into, or exit from, democratic status and different forms of democracy.

Depending on the specific democracy measure used (from Freedom House or Polity IV), we have about 25 or 40 such entries and exits in the data. The econometric specification always includes fixed country effects, so as to use only the within-country variation around the reforms. It also includes a set of fixed time effects, so as to obtain "difference-in-difference" estimates.

Column 1 in *Table 3.3* displays such estimates of the effect of reforms on structural policy based on a yearly panel. As in the cross-sectional data, *parliamentary democracy* has a positive effect. Introducing presidential democracy in a non-democracy now has a small negative effect on structural policy. Column 2 shows fixed-effect, instrumental-variable estimates of *output per worker*, where the instruments for *structural policy* are the constitutional variables appearing in column 1. As indicated by the test-statistic for the overidentifying restrictions, we cannot reject that (one set of) these reforms are exogenous to output per worker.

Columns 3 and 4 repeat the same analysis. But here democratic status is obtained from the Polity IV data set, rather than from Freedom House, with differences in country coverage as well as the coding of democratic status. Now, the introduction of democracy with proportional elections also improves *structural policy*. The instrumental-variable estimate of the effect on *output per worker* is more or less identical to that in column 2. Finally, columns 5 and 6 replace the yearly data with 5-year averages, estimating the same specification with GMM, while explicitly allowing for a dynamic panel structure (see the notes to *Table 3.3* for the precise specification). A similar result comes out.

These results are really very preliminary and based on a first round of empirical work. With that qualification, however, the results do suggest that constitutional arrangements are indeed instrumental for structural policies that foster good economic performance. The subject is certainly important enough to warrant further investigation.

4 Final remarks

Five years ago (Persson and Tabellini, 1999), we embarked on some new research with the vague idea that constitutional rules may have systematic consequences for economic policy.

In this lecture, I have referred to a new book (Persson and Tabellini, 2003). Based on its results, I have argued that the data indeed seem consis-

tent with several theoretical predictions about the consequences of electoral rules and forms of government for fiscal policy and rent extraction. The empirical results seem robust to non-random selection of these constitutional features. Moreover, the estimated effects are large enough to be of genuine economic interest.

I have also referred to a new paper (Persson, Roland, and Tabellini, 2003). Based on its results, I have argued that it may be useful to model not only the direct consequences of constitutions via policymaking incentives, but also the indirect consequences via the structure of political representation. Building a bridge to research in political science, we will be better equipped to inspect the mechanism whereby constitutions shape economic policy.

Finally, I have referred to new research for this lecture. Based on preliminary empirical results, I have argued that constitutions may have important consequences for structural polices promoting long-run economic performance. Building a bridge to research in development economics may teach us important lessons about institutional reform.

Tangling out this web of constitutional consequences is certainly not an easy task. To make further progress we must dig deeply into various corners of our tool-box. We need solid game-theoretic modeling, painstaking assembly of new data sets, and careful econometrics.

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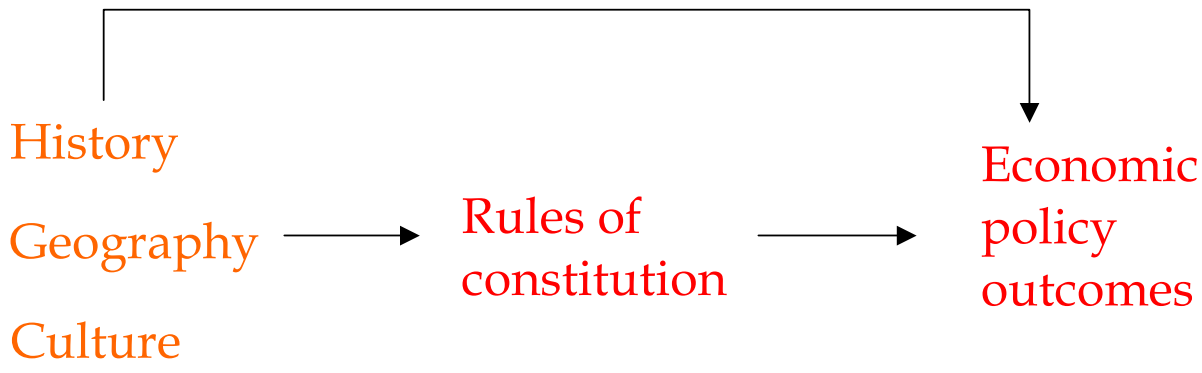

Figure 1
Consequences for policy when constitution selection non-random?

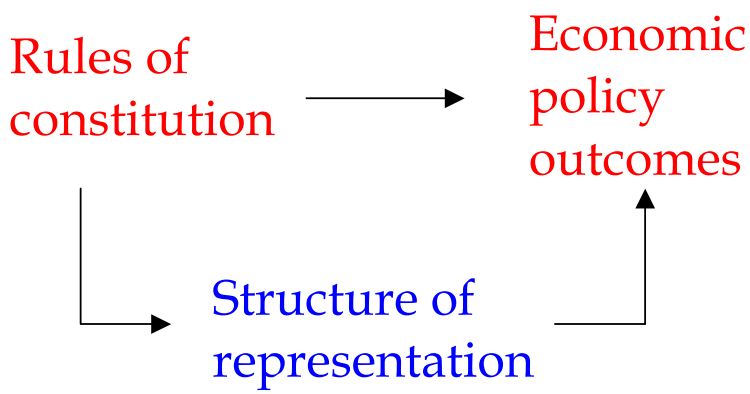

Figure 2
Direct or indirect consequences for policy ?

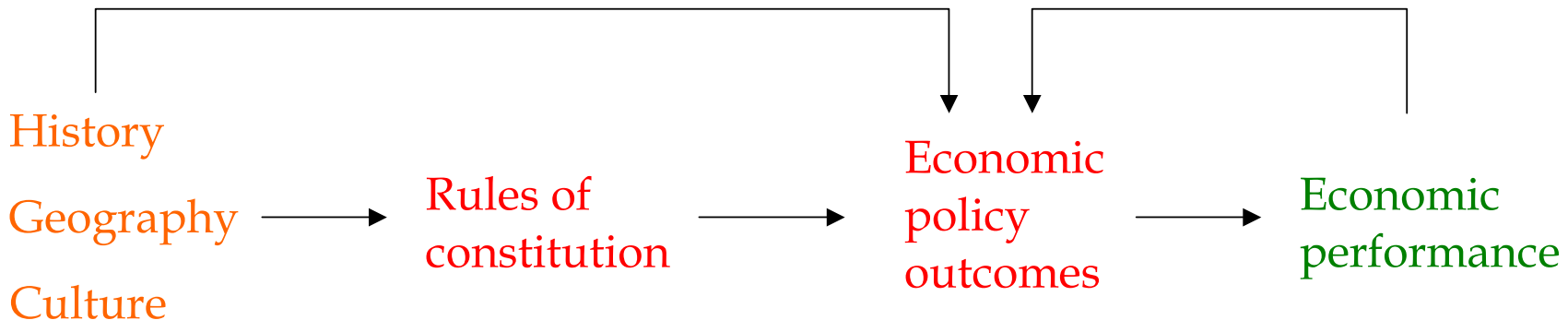

Figure 3
Constitutional consequences for growth-promoting policies ?

Table 2.1
Political and economic outcomes by electoral rules

	Majoritarian (N=138)	Proportional (N=187)
party fragmentation index	0.54 (0.17)	0.70 (0.09)
incidence of coalition governments	0.24 (0.41)	0.55 (0.47)
incidence of single-party governments	0.63 (0.47)	0.17 (0.37)
government spending as % of GDP	25.94 (9.05)	35.12 (9.30)

Table 2.2
Effects of electoral rules on political outcomes

Sample	Cross 90s		Cross 60-98		Panel	
Dep. var	*party frag*	*coalition*	*party frag*	*coalition*	*party frag*	*coalition*
party frag		2.05**		2.62***		1.35*
majoritarian	-0.13***		-0.09*		-0.06	
mixed	-0.14*		-0.01			
district magnitude	0.11*		0.14**		2.31***	
Over-id chi-2(df)		5.54(3)		3.41(3)	4.16(3)	3.83(6)
Estimation	OLS	2SLS	OLS	2SLS	GMM	GMM

Table 2.3
Effects of electoral rules on government spending

Sample	Cross 90s		Cross 60-98		Panel	
coalition		16.90***		30.07***		6.51**
majoritarian	-4.67		-2.73		1.08	
mixed	-8.14*					
district magnitude	14.34***		17.29***		37.50***	
Over-id chi-2(df)		2.09(3)		0.89(3)	5.35(3)	10.24(9)
Estimation	OLS	2SLS	OLS	2SLS	GMM	GMM

Table 3.1 Cross sectional estimates
History and structural policy shape economic performance

Cross section	Full		Mortality			
Dep. variable	*structural policy*	*output/ worker*	*structural policy*	*output/ worker*	*structural policy*	*output/ worker*
structural policy		3.80***		5.17***		9.00***
European speakers	0.21***		0.33***			
pred. trade share	0.09***		0.05***			
settler mortality					–0.06**	
Over-id chi-2(df)		2.39(1)		0.61(1)		
Estimation		GMM		GMM		GMM

Table 3.2 Cross sectional estimates
Constitutions may be the missing link

Cross section	Full		Mortality			
Dep. variable	*structural policy*	*output/ worker*	*structural policy*	*output/ worker*	*parl dem*	*structural policy*
structural policy		3.59***		4.78***		
parliam. democracy	0.10**		0.17**			0.31**
age of democracy	0.32***		0.34**			0.38***
const reform 51-80					0.25**	
settler mortality					–0.10**	
Over-id (all – 1)		3.03(3)		1.93(3)		0.31(1)
Over-id (const vars)		0.68(2)		1.39(2)		

Table 3.3 Panel estimates
Constitutional reforms may improve growth-promoting policies

Panel	Yearly				5-year	
Dep. variable	*structural policy*	*output/ worker*	*structural policy*	*output/ worker*	*structural policy*	*output/ worker*
structural policy		1.56**		1.50**		1.31**
parl.dem (gastil)	0.06***					
democracy (gastil)	–0.01***					
parl.dem (polity)			0.04***		0.01	
prop.dem (polity)			0.02***		0.03**	
Over-id		0.24(1)		0.01(1)		5.85(5)
Estimation	FE	FEIV	FE	FEIV	GMM	GMM