

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, even when non-random constitution selection is taken into account. Second, 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, rather than through their direct effects on policymaking incentives. Third, new empirical results suggest that constitutional arrangements may have important consequences for structural policies that promote long-run economic performance, hinting at a missing link in the causal 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, O11)

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Five years ago, in the Berlin congress, I gave the Marshall lecture together 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. The first part 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 on fiscal policy and on corruption 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 the second part, 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 the second part,

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. It also takes to the data the model's predictions of the consequences of alternative rules for political outcomes, as well as fiscal policy outcomes. These predictions appear to hold up.

The final part is about bridging another gap. Recent empirical research on the macroeconomics of development has singled out certain "structural policies" 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 vague.

In the third part, I will suggest that the logic of the research program on constitutions should lead us to expect systematic consequences also for growth-promoting policies. If this is the case, it would help fill the institutional void between history and current performance, as illustrated in the figure. To shed more light on this suggestion, I have 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. The 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, 2003).

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 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 broad time period of introduction. We thus use pure timing as an instrument for currently observed constitutions, while controlling for the effect of age of democracy on policy outcomes. Another way to relax conditional independence is to adjust the estimates, Heckman-style, for selection bias. In applications where piecemeal reforms are relevant, we can also use our panel data to eliminate bias from (time-invariant) unobservables.

If constitutional effects are heterogeneous, we may have problems even with selection on observables. For example, if a reform from presidential to parliamentary governance has different effects on fiscal policy choices in

¹Besley 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.

Latin America than elsewhere, over-representation of presidentialism in Latin America may bias our estimates of the constitutional effect on fiscal policy. 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 appear robust to alternative methods of estimation.

Hypothesis E1 A first hypothesis is about the effects of electoral rules (hence the label **E1**). Suppose legislative seats are awarded by plurality rules rather than proportional representation (PR). This should tilt policy towards spending programs with benefits targeted to particular geographical 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 a given size legislature being elected in a larger number of districts.)

The main theoretical idea behind this hypothesis is quite straightforward (see Persson and Tabellini, 1999, Lizzeri and Persico, 2001, Milesi-Feretti, Perotti, and Rostagno, 2002). If candidates with the highest vote shares win every seat at stake – rather than seats in proportion to their vote shares – it becomes more attractive to target small and geographically concentrated groups of voters. (The same will hold true if each district represents 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 whenever 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 across samples as well as estimation methods: an electoral reform would cut spending by about 5% of GDP – a substantial number indeed.

Hypothesis E2 A second hypothesis concerning electoral rules has to do with their detailed features. 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 both expected to reduce rent extraction by politicians.

The key idea here is that extraction is better deterred if chances of re-election for individual lawmakers (or parties) may respond more forcefully to their performance (see Myerson, 1993 and Persson and Tabellini, 2000). Large district magnitude achieves 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.

Hypothesis G A third 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 (see Person, Roland and Tabellini, 2000). 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 suffer. 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 (labeled *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 (and old) 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.

Other findings 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 budget 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 the work cited at the beginning of the section.

2 Consequences for political representation and fiscal 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. Some of 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 omits from the analysis any effects on the structure of political representation, which is treated 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 Section 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). In that paper, 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 sketching 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) 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 earned from 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, taking into account the uncertainty about government status, G (see below), as well as electoral outcomes (see below). Given the choices of others, each group thus maximizes expected equilibrium payoffs: $E\{E(NW_G^J)\}$, where $E(NW_G^J)$ is defined from the party payoffs below – if the group forms a small party its payoff is identical to the party payoff; if it merges with another group it obtains half the party payoff. To simplify matters, 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, s :

$$E(NW_G^P) = r_G^P + E(Ns_G^P)R$$

A single-party government makes all decisions on spending, $\{g^J\}$, and rents, r^P , (the tax rate, τ , follows from the government budget constraint) as a unified actor, even though the large party forming it consists of two primitive groups. This assumption is without loss of generality; if we allowed the two groups to bargain over policy, they would agree on all policy instruments. In a coalition government formed by two small parties, these agree on

spending which is targeted to groups *not* represented in government. As for the remaining policy decisions, 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, the voters cast their ballots. Voters are non-strategic and follow simple voting rules, some of them care only about ideology (attached voters), others also care about policy (semi-attached voters). The latter drive most results. Consider these voters of group J , in the case their own party, $P = J$, is in government. They then reward the party with their vote, if their utility exceeds a reservation level: the right-hand side of

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

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 – if the opposition consists of two parties, these are supported with equal probability. If their own party is not in government, the semi-attached support one of the government parties (with equal probability) if (*) is satisfied; if not, they support 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 the party-formation and policy-formation stages, given the rules followed at the government-formation and election stages.

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). In a symmetric benchmark case all districts have

exactly the same composition. But we also study alternatives, where economic groups (and ideological voters) 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. Each type of government may extract more or less under proportional than majoritarian elections, depending on parameters. Coalition governments always extract more rents than single-party governments. Intuitively, each of the coalition parties does not internalize the full electoral costs of unilaterally extracted rents, as the punishment is shared with the coalition partner and the two parties have separate electoral objectives.

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, there are typically both 2-party and 4-party equilibria under majoritarian elections, producing alternatively coalition or single-party governments. With asymmetric distributions of voters, 3-party systems also become possible equilibria, however, opening up the possibility for alternating single-party and coalition governments. Intuitively, under majoritarian elections with plurality rule a group contemplating staying on as a small party at the party-formation stage expects a clear electoral disadvantage. Under proportional elections, the group can expect to capture larger rents at no electoral cost, by staying a small party instead of merging.

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 section, 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 political and economic 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 Section 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 use both complementary types as dependent variables, effectively classifying minority governments in two alternative ways). Total *government spending* is measured as described in Section 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 cross-sectional variation among parliamentary democracies, either during the 1990s or during 1960-98. Given the prospective pitfalls in cross-sectional inference, however, we also exploit the within-country time variation associated with electoral reforms. We use a panel with the last six legislatures in each democracy, because observed electoral reforms are concentrated in the last two decades.

Column 1 in *Table 2.2* reports on a pure cross-sectional regression of *party fragmentation* in the 1990s on a set of controls and on four measures of electoral rules (see the Notes to the table and Persson, Roland and Tabellini, 2003 for further information on all specifications). The first two of the electoral rule variables are (binary) indicators of *majoritarian* and *mixed* electoral formulas, the third a (continuous) measure of *district magnitude*; the fourth is a (continuous) measure of electoral thresholds for representation, results not displayed). All three variables have the expected sign and two are statistically significant at the 5% level.

In column 2, we use these four measures as instruments for party fragmentation when explaining the incidence of coalition governments, also adding to the second stage other constitutional variables expected to affect the likelihood of coalition governments, such as bicameralism and constructive votes of confidence. As expected, more fragmentation has a strong positive effect on coalition governments. The chi-2 statistic from the Sargan test is at the boundary of the acceptance region, when we test the over-identifying restriction that the three electoral-rule variables of interest only influence coalition governments indirectly.. 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; in these cases, the statistic for the test of the overidentifying restrictions is safely within the acceptance region.

Columns 3 and 4 show that, essentially, the same results come out when we average over four decades, instead of one, in a smaller number of countries. Columns 5 and 6 show that the main results stand when identified exclusively from the within-country 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 cross-sectional regression of spending on a set of controls and the exogenous constitu-

tional 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 as instruments 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 Section. As for the over-identifying restrictions, we cannot reject that electoral rules only have an indirect effect on spending.

Columns 3 through 6 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**. So are the results when we use single-party government as the intervening variable in place of coalition government, and or a panel with observations corresponding to years rather than legislatures.

More work certainly remains to do on this topic. On the empirical front, we have not taken all of the model's predictions to the data, including its predictions for rents (coalition governments should have higher rents). 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 growth-promoting 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, face 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 have 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 growth-promoting 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.

As mentioned in the introduction, I will argue that it makes sense to think about democratic constitutions as a source of variation in growth-promoting policies, and that the data indeed seem to support this argument. Before doing so, however, let me use a little replication exercise to provide a concrete illustration of the results in the existing literature.

Replicating earlier results For each country, economic performance is measured by (the log 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 of these indexes refers to government anti-diversion policy, related to the protection of private property rights, another refers to the number of 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 “output per worker” and “social infrastructure” in Hall and Jones (1999). But they are based on more recent data. And each measure is computed not only as an average for a cross section of countries, but for a panel including every country and year since the early 1980s.²

²The 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 government diversion policies in five dimensions (law and order, bureaucratic quality, corruption, risk of expropriation, government repudiation of contracts); it is obtained from the IRIS-3 data set. The *YR-*

Consider first the results for average outcomes in the 1990s, in the full cross section of about 100 countries where all data is available. *Table 3.1* begins by reporting a two-stage instrumental-variables estimate of the effect of *structural policy* on *output per worker* (estimating by GMM means allowing for heteroskedasticity of 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 do not pass the relevance *cum* exogeneity test (latitude and the fraction of English speakers). As shown in the table, however, higher shares of European speakers and more favorable conditions for trade do (by the F-statistic in col. 1 we can reject irrelevance, and by the chi-2 statistic in col.2 we cannot reject exogeneity). These variables are clearly associated with better structural policy. Column 2 shows the second stage. Structural policy has a large, positive, and precisely estimated effect on performance. According to these estimates, if one could engineer an improvement in structural policy by one standard deviation this would raise 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 log 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 policies on regulation and trade, in a broad sense, do shape long-run economic performance. A common interpretation in the literature is that good policy requires good institutions. This sounds very reasonable,

SOPEN index measures the number of years since 1950 (as a share) a country has been open according to five different criteria (tariffs, non-tariff barriers, capital controls, the black-market premium, and socialist rule); it is an update by the original Sachs-Warner index, from Easterly, Levine and Roodman (2003), which was kindly made available by David Roodman.

There are two 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, data is not imputed for the missing values, as done by these authors.

but the institutional lessons remain quite vague. In particular, they do not give much guidance for reform.

Do constitutions shape growth-promoting policy? Getting back to the main theme of this lecture, 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 enduring and broad protection of property rights as provision of public goods or, at least, as (geographically) non-targeted policies benefiting broad groups in the population. Even though protectionist trade policies tends to be primarily targeted towards sectors of production, rather than regions of residence, sectors and regions may correlate well, particularly in developing countries. Non-protectionist trade policy thus also has broad, rather than (geographically) targeted, benefits. Certain constitutional arrangements may better promote such policies, by similar arguments as in the existing models of fiscal policy. In Section 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 and stable benefits, by allowing more widespread political participation than a non-democratic regime.

What do the data say? Persson and Tabellini (2003, Ch.7) make a start on answering this question, but provide only cross-sectional evidence for a set of democracies, so it is interesting to extend the analysis 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 (obtained from Freedom House) takes a value below 5; alternatively I do so if its POLITY index (obtained from 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. *Parliamen-*

tary and *proportional democracy* indicators are obtained by interacting the democracy indicators with the binary indicators for parliamentary regimes and proportional elections discussed in Section 1 (specifically, 1 minus the indicators *pres* and *maj*, respectively).

Cross sectional 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. The democracy, and proportional democracy indicators turn out not to be significant determinants of structural policy. But the form of democratic government, and the age democracy do. Columns 1 and 2 of *Table 3.2* show the first and second stage of the GMM estimates for the full 1990s cross section, when these two variables are added to the first stage (cf. the standard errors and the F statistic in Column 1). As shown in the table, parliamentary democracies, and older democracies, have better structural policies. Of course, the earlier concerns about simultaneity remain. We thus 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). The estimated effect of structural policy on productivity is close to the one in *Table 3.1*. 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 are equally strong in the smaller settler mortality sample. In this sample, I try to obtain additional insight by scrutinizing the first stage of the estimates. I thus re-estimate the structural-policy equation, using *settler mortality* as an instrument for parliamentary democracy. As an additional instrument, I use one of the constitutional timing variables mentioned in Section 1: the indicator *constitutional reform51-80* is set to 1 if a country is democratic and its 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. The resulting variation in the form of government is now estimated to have an even stronger effect on structural policy than in columns 1 and 3. 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.

Panel evidence More support comes from the panel data. In Section 1. This may seem puzzling at first given the observation in Section 1 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, to investigate the outcomes associated with different constitutional rules in democracies. Depending on the specific democracy measure used (from Freedom House or Polity IV), we have 25-40 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 difference in difference estimates of the effect on structural policy, based on a yearly panel. As in the cross-sectional data, *parliamentary democracy* improves structural policy. Introducing presidential democracy in a non-democracy now has a small negative effect. Column 2 shows difference in difference estimates of *output per worker*, where the constitutional variables appearing in column 1 are used as instruments for *structural policy*. 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, when democratic status is obtained from the Polity IV data set, rather than from Freedom House. (This implies different country coverage as well as a different 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, in the last two columns we replace yearly data with 5-year averages. Column 5 shows the estimates from the same specification as in column 3 (except that the time effects refer to five-year-period dummies instead of year dummies). Column 6 reports on instrumental-variable (GMM) estimates, while explicitly allowing for a dynamic panel structure (see the notes

to *Table 3.3* for the precise specification). The effect of structural policy on productivity is similar to the earlier estimates.

Based on a first round of empirical work, these results are really very preliminary. 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 consistent 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 policies promoting long-run economic performance. Building a bridge to research in development economics, we may learn important lessons about growth-enhancing 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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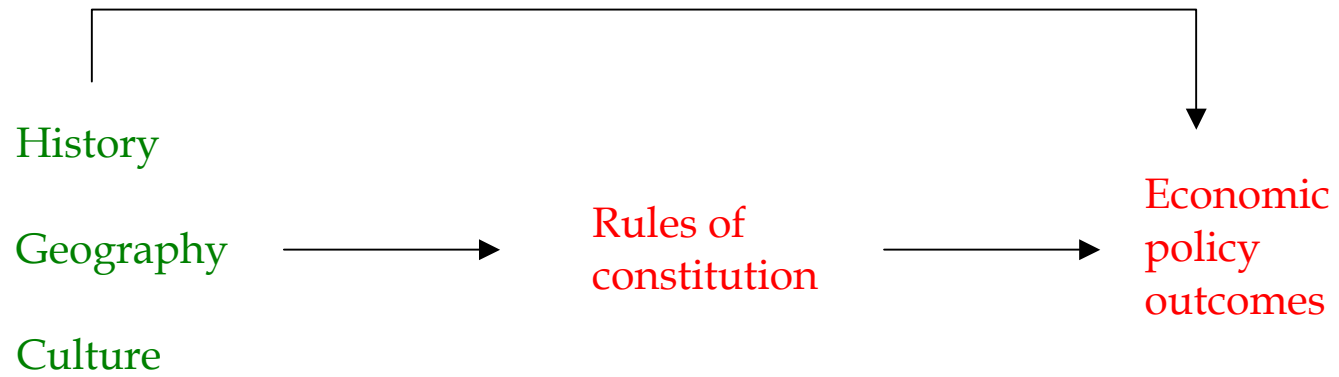


Figure 1
Consequences for policy when constitution selection non-random?

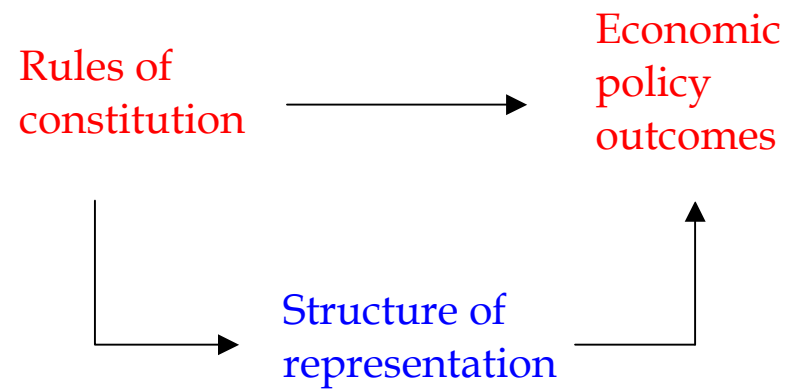


Figure 2
Direct or indirect consequences for policy?

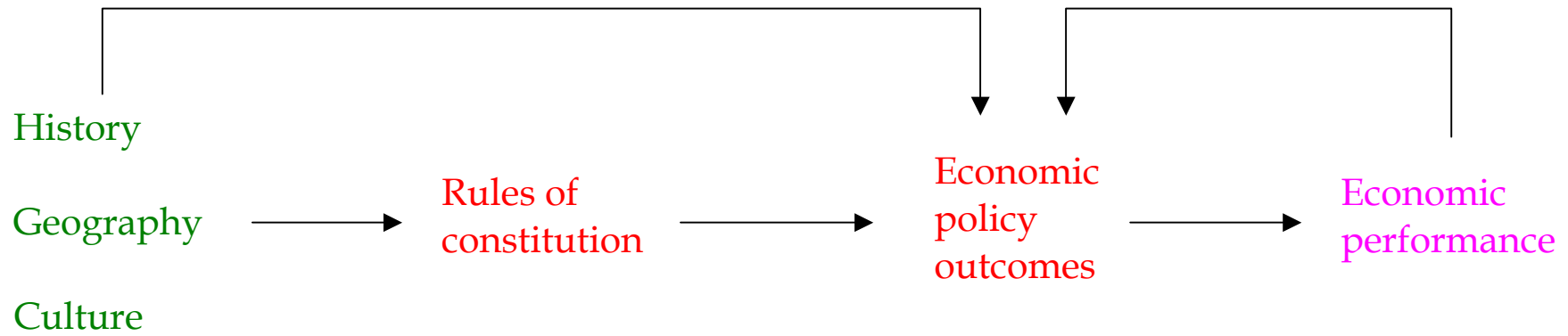


Figure 3
Constitutional consequences for growth-promoting policies?

Table 2.1
Political and economic outcomes cross-tabulated by electoral rules

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

Basic observations refer to legislative periods in 52 parliamentary democracies going back as far as 1960, if data permit.. Means displayed in the table are computed by pooling observations across all legislatures elected by plurality rule, and by pure PR , respectively (mixed systems excluded).. Standard errors in brackets. Data sources described in the text..

Table 2.2
Effects of electoral rules on political outcomes

Sample	Cross 90s		Cross 60-98		Panel	
Dep. var	<i>party fragmentation</i>	<i>coalition</i>	<i>party fragmentation</i>	<i>coalition</i>	<i>party fragmentation</i>	<i>coalition</i>
<i>party fragmentation</i>		2.01** (0.71)		2.62*** (0.74)		1.35* (0.81)
<i>majoritarian</i>	-0.12** (0.05)		-0.09* (0.05)		-0.06 (0.04)	
<i>mixed</i>	-0.22** (0.09)		-0.01 (0.06)			
<i>district magnitude</i>	0.11 (0.07)		0.14** (0.07)		2.31*** (0.24)	
Over-id, chi-2(df)		7.91(3)**		4.25(3)	4.16(3)	3.83(6)
Estimation	OLS	2SLS	OLS	2SLS	GMM	GMM
N. obs	52	47	38	36	141	97

Robust standard errors in brackets: *** significant at 1%, ** significant at 5%, * significant at 10% .

OLS regressions also control for *electoral thresholds, population, old British colony, plus fractionalization* (col. 1 only)..

Second stage in 2SLS includes *population, old British colony, investiture vote, constructive vote of confidence, plus bicameralism and fractionalization* (col. 2 only)

First stage in 2SLS includes all second-stage variables, and *party fragmentation, majoritarian, mixed, and electoral thresholds*.

GMM specification (in 1st differences) always includes *population, quality of democracy, plus one* (col. 5) or two (col. 6) lags of dependent variable. Instruments for lagged dependent variable are further lags of that variable. In col. 6 *party fragmentation* is treated as endogenous, with *majoritarian, district magnitude, electoral threshold* and one lag of *party fragmentation* as additional instruments.

Over-id chi-2(df) refers to Sargan-Hansen test statistic for over-identifying restrictions, critical values (at 5%) are 7.81 (cols. 2, 4, 5), and 12.59 (col. 6) .

Table 2.3
Effects of electoral rules on government spending

Sample	Cross 90s		Cross 60-98		Panel	
<i>coalition</i>		17.00*** (3.72)		30.07*** (10.79)		6.51** (3.12)
<i>majoritarian</i>	-4.76 (3.39)		-2.73 (2.55)		1.08 (1.67)	
<i>mixed</i>	-8.31* (4.53)					
<i>district magnitude</i>	14.52*** (2.05)		17.29*** (2.29)		0.25*** (0.08)	
Over-id chi-2(df)		2.16(3)		1.29(2)	5.35(3)	10.24(9)
Estimation	OLS	2SLS	OLS	2SLS	GMM	GMM
N. obs	46	46	34	34	111	105

Robust standard errors in brackets: *** significant at 1%, ** significant at 5%, * significant at 10% .

OLS regression and second stage of 2SLS, cols. 1-2, include *income, population above 65, federalism, old British colony, trade, and fractionalization*

OLS regression and second stage of 2SLS, cols. 3-4, include *income, population above 65, federalism, and old British colony.*

First stage in 2SLS, cols 2 and 4.: all second-stage variables, electoral variables displayed in cols. 1 and 3, plus *investiture vote* (col. 2), or *electoral threshold* (col. 4).

GMM specifications (in 1st differences) always include *population, population above 65, output gap, length of legislature*, plus one lag of *government spending*. Instruments for lagged *government spending* is one further lags of that variable. In col. 6 *coalition* is treated as endogenous, with *majoritarian, district magnitude, electoral threshold* and one lag of *coalition* as additional instruments.

Over-id chi-2(df) refers to Sargan-Hansen test statistic for over-identifying restrictions, critical values (at 5%) are 5.99 (col. 4), 7.81 (cols. 2 and 5), and 16.92 (col. 6) .

**Table 3.1 Cross sectional (GMM) estimates
History and structural policy shape economic performance**

Cross section	Full data set		Settler mortality data set			
Dep. variable	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>
<i>structural policy</i>		3.81*** (0.66)		5.17*** (1.06)		9.00*** (2.57)
<i>European speakers</i>	0.21*** (0.06)		0.33*** (0.09)			
<i>predicted trade share</i>	0.09*** (0.03)		0.05 (0.04)			
<i>settler mortality</i>					-0.06** (0.02)	
Excl. instruments, F	11.01***		6.82***		5.98**	
Over-id, chi-2(df)		2.39(1)		0.61(1)		
N. obs	98	98	58	58	59	59

Robust standard errors in brackets; *** significant at 1%, ** significant at 5%, * significant at 10%.

Second stage includes *structural policy* (endogenous) and indicators for earlier colonial powers (Britain, France, Spain-Portugal, Other) and continents (Latin Am., Africa, Asia)

First stage includes the same seven indicators, and instruments as indicated in cols 1, 3 and 5.

Excl. instruments F refers to test statistic of hypothesis that the instruments do not belong to the first-stage regression.

Over-id chi-2(df) is the Hansen J statistic when testing the over-identifying restriction for the instruments, critical value (cols 2 and 4) at 5% is 3.84.

**Table 3.2 Cross sectional (GMM) estimates
Constitutions may be the missing link**

Cross section	Full data set		Settler mortality data set			
Dep. variable	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>	<i>parliamentary democracy</i>	<i>structural policy</i>
<i>structural policy</i>		3.59*** (0.64)		4.78*** (0.86)		
<i>parliamentary democracy</i>	0.10** (0.05)		0.17** (0.08)			0.31** (0.16)
<i>age of democracy</i>	0.32*** (0.09)		0.34** (0.15)			0.38*** (0.14)
<i>constitutional reform 51-80</i>					0.25** (0.11)	
<i>settler mortality</i>					-0.10** (0.04)	
Excl. instruments, F	10.50***		5.88***		4.75**	
Over-id (df, all - 1)		3.03(3)		1.94(3)		0.31(1)
Over-id (df, const vars)		0.68(2)		1.39(2)		
N. obs	97	97	57	57	57	57

Robust standard errors in brackets; *** significant at 1%, ** significant at 5%, * significant at 10%.

Second stage in cols 2, 4 includes *structural policy* (endogenous) and indicators for earlier colonial powers (Britain, France, Spain-Portugal, Other) and continents (Latin Am., Africa, Asia). Second stage in col. 6 includes *parliamentary democracy* (endogenous), *age of democracy* (exogenous) and same seven indicators.

First stage includes exogenous second-stage variables and instruments as indicated in cols 1, 3 and 5.

Excl. instruments F refers to test statistic of hypothesis that the instruments do not belong to the first-stage regression.

Over-id chi-2(df, all -1) is the Hansen J statistic when testing the over-identifying restriction for all (but one) of the instruments.

Over-id chi-2(df, const.vars) is the C statistic when testing the over-identifying restriction for the two constitutional instruments.

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

Panel	Yearly		5-year			
Dep. variable	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>
<i>structural policy</i>		1.56** (0.73)		1.50** (0.68)		1.31** (0.59)
<i>parliamentary democracy</i> (Freedom House data)	0.06*** (0.01)					
<i>democracy</i> (Freedom House data)	-0.01*** (0.00)					
<i>parliamentary democracy</i> (Polity IV data)			0.04*** (0.01)		0.01 (0.02)	
<i>proportional democracy</i> (Polity IV data)			0.02*** (0.01)		0.03** (0.01)	
Over-id		0.24(1)		0.01(1)		5.85(5)
Estimation	FE	FEIV	FE	FEIV	FE	GMM
N. obs (countries)	1428 (102)	1428 (102)	1432 (102)	1432 (102)	406(107)	272(94)

Robust standard errors in brackets: *** significant at 1%, ** significant at 5%, * significant at 10% .

FE specifications in cols. 1, 3 , and 5 include fixed country and period (year or 5-year) effects in addition to the constitutional variables displayed in these columns. FEIV specifications in cols. 2 and 4 include fixed country and year effects in addition to *structural policy* (endogenous); with the constitutional variables in cols. 1 and 3 used as instruments. GMM specification (in 1st differences) in col. 6 includes fixed period effects and one lag of *output per worker* in addition to *structural policy* (endogenous) . Instrument for lagged *output per worker* is one further lag of that variable, and instruments for *structural policy* are: one lag of *structural policy* plus the constitutional variables in col. 5.

Over-id chi-2(df) refers to Sargan-Hansen test statistic for over-identifying restrictions, critical values (at 5%) are 3.84 (cols. 2 and 4), and 11.07 (col. 6) .