Legislative institutions and fiscal policy

Working paper

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Abstract: A number of studies, using different samples and datasets, claim that various legislative institutions affect fiscal policy. This paper uses data from a 2003 survey of budgeting practices to comprehensively evaluate the effect of a range of legislative institutions on public spending in 25 OECD countries. It finds no evidence for most institutional hypotheses. Only the power of the legislature to amend the budget proposal of the executive has a significant impact on public expenditures.

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Political scientists and economists increasingly invest in the development of comparative tools for the cross-national study of political institutions and their performance (Congleton and Swedenborg 2006). Rigorously constructed quantitative measures are useful for testing theories with larger samples of countries than the case study method allows, and to broaden our perspective beyond a handful of frequently studied cases. However, the development of such comparative tools also entails pitfalls. New measures may quickly gain widespread acceptance despite possible refinements or alternatives. Moreover, aggregate indices can sometimes obfuscate the impact of individual component variables. In short, while the development of quantitative measures is crucial for advancing the comparative study of institutional effects on policy, it is equally important to carry out careful testing and continuous reassessment to ensure the quality of measures in use.

This paper reconsiders comparative measures of legislative control of public finance. Distrust in the ability of legislators to maintain fiscal discipline in the budgetary process is not a new phenomenon. Since the 1990s, a number of studies, using different variables and datasets, have claimed that certain institutional features are conducive to maintaining fiscal discipline during the budget approval process in the legislature. This paper provides an overview and assessment of two prominent quantitative measures of the power of parliaments in budgetary matters (Von Hagen 1992, Alesina *et al.* 1996). I also consider other legislative variables that different authors claim to be determinants of fiscal outcomes. The assessment of alternative

measures is complicated by the fact that empirical studies typically use different datasets that may not allow the reconstruction of other existing measures. This paper is the first to systematically assess two main alternative measures of legislative budget power and other relevant variables on the basis of a single dataset. Moreover, cross-sectional analysis is complemented with results using a new method for estimating coefficients of time-invariant variables in panel data, which addresses a frequent methodological problem in the empirical literature.

The paper is organised in three parts. The first provides a brief introduction to the theoretical arguments and institutionalist hypotheses in the literature. The second part discusses data and methodological issues. In the third part I present empirical results on the impact of legislative institutions on fiscal policy outcomes, using cross-sectional and panel data. The conclusion considers the implications of these findings for further research.

Institutionalist hypotheses

The literature on the fiscal effect of budget institutions builds on the basic insight that spending will be higher when decision-makers do not internalise the full costs of their actions. Weingast, Shepsle and Johnson (1981) express this as the 'law of 1/n'. In their model, expenditure *x* can be targeted at a particular geographical district where it produces benefits *b*, while costs *c* are shared equally across all districts. This implies that the optimal level of spending for district *i* is achieved when its marginal benefit equals its marginal cost: $b_i'(x) = (1/n)\, c'(x)$. The larger *n* the smaller the share of the

tax burden considered. Hence, the authors conclude that 'the degree of inefficiency in project scale… is an increasing function of the number of districts' (Weingast *et al.* 1981: 654). In other words, the possibility to disperse costs and target benefits leads to higher spending the greater the number of decision-makers. This suggests that the spending bias in a legislative setting is potentially large.

Von Hagen and Harden (1995: 772-775) build on a similar idea and explore the aggregate implications of different decision-making procedures. They show that the aggregate budget outcome resulting from a bottom-up process, in which spending ministers independently develop their spending plans, is larger than the optimal total of the government as a whole. When a minister without portfolio, who has an incentive to consider the overall impact of excess taxation, is given strategic power vis-à-vis spending ministers, the resulting amount of total spending is closer to the joint optimum than under the bottom-up process. The model can be adapted to different contexts, such as legislative decision-making, or where the process involves disciplined political parties in a coalition government (Hallerberg 1999 and 2004: 22-27). The basic result is always that a spending bias will result when decision-makers do not internalise the full cost of their actions, i.e. when they suffer from 'fiscal illusion' (Von Hagen and Harden 1995: 772).

The fiscal institutionalist response to what is also referred to as the 'common pool resource' or 'fiscal commons' problem is to impose hierarchical budget institutions. These are institutional arrangements that centralise budgetary decision-making in the hands of the finance minister, who is more likely to consider overall costs than spending ministers, and hence contain free-riding and support fiscal discipline (Von

Hagen 1992, Poterba and Von Hagen 1999, Strauch and Von Hagen 1999). This has spawned a substantial body of mainly empirical work on the fiscal effects of budget institutions notably in Western Europe (Von Hagen 1992, Hallerberg 2004), but also Latin America (Stein *et al.* 1998, Alesina *et al.* 1999b, Hallerberg and Marier 2004), and more recently Central and Eastern Europe (Gleich 2003, Yläoutinen 2004). In the following, I review the hypotheses about the fiscal impact of legislative institutions put forward in this literature.

In a groundbreaking and widely cited paper prepared for the European Commission, Von Hagen (1992) argues that institutions that weaken the role of special interests in the budget process affect fiscal stability. He develops three different versions of a 'structural index' that consist of up to four different items (pp. 43-44). Based on fiscal data for European Community countries in the 1980s, his empirical analysis finds support for the 'structural hypothesis' that a budget process with a dominating role of the finance minister vis-à-vis spending ministers, restricted powers of amendment for parliament, and limiting adjustments to the budget during implementation is strongly conducive to fiscal discipline (p. 53).

Item two of the structural index combines several components to assess the 'structure of the parliamentary process' (p. 70). In the discussion below, the respective scores assigned by Von Hagen are indicated in square brackets. Components one and two relate to the amendment powers of the legislature. They indicate whether amendment powers are limited [4] or unlimited [0] and whether changes are required to be

offsetting [4] or not [0].[1] The third considers whether amendments can cause the fall of the government [4] or not [0]. Component four indicates whether all expenditures are passed in one vote [0] or chapter-by-chapter [4], with an intermediate score [2] for what Von Hagen classifies as 'mixed' cases. The fifth component looks at whether the process commences with a global vote on the size of the total budget [4] or whether totals are voted only at the conclusion of the process [0]. The individual scores are summed to derive the total score for item two. Accordingly, the scores on this item can range between zero and a maximum of 20, with the latter indicating a more centralised parliamentary budget process that, according to Von Hagen, should be more conducive to fiscal discipline.

The effect of some of the components of item two is contested. Notably, Von Hagen (1992: 36) argues that a global vote on the budget prior to allocative decisions contains total spending. However, Ferejohn and Krehbiel (1987) demonstrate that such a two-step process may result in relatively large budgets. Empirically, Alesina and colleagues (1999b: 270) find evidence that such a process imposes an effective constraint, but Helland (1999: 130-132) does not. Von Hagen later revised his initial view (Hallerberg and Von Hagen 1997, Ehrhart *et al.* 2001). Moreover, Von Hagen (1992: 36) merely offers a 'conjecture' that voting the budget chapter-by-chapter is more constraining than authorisation in a single vote. The findings presented in the

[1] It is not entirely clear how Von Hagen scored this item in his 1992 paper when legislatures can only accept or reject the budget. Notably, his Table A6 reports that the Irish Parliament has no powers to amend expenditure proposals. Scoring Ireland on his item two, he gives four points because amendments are limited, but zero points for the offsetting component. In the 2001 update (Hallerberg *et al.* 2001: Table 2b) the authors count the offsetting item as not relevant for Ireland and accordingly assign a score of zero, which is more consistent. In reconstructing Von Hagen's item two, I assigned a score of four for the offsetting component when a legislature can either (i) only accept or reject the budget, or (ii) when amendment powers impose an aggregate constraint.

following section add to the empirical debate about the effect of these institutional features.

The paper by Alesina and colleagues (1996) extends the geographical application of this approach. It establishes a parsimonious measure of the budgetary power of the legislature vis-à-vis the government as part of a ten-item index of budget institutions that the authors use to classify budgetary systems as 'hierarchical' or 'collegial'. Using a sample of 20 Latin American and Caribbean countries, they present evidence that more hierarchical budget institutions were associated with greater fiscal discipline in the 1980s and early 1990s. They sum components five and six to construct their subindex three, which they argue measures the relative position of the government vis-à-vis the legislature in the approval stage, and find that it is a significant determinant of fiscal performance (p. 23). In later versions of their paper they use a different disaggregation of their main index (Alesina *et al.* 1999a and 1999b). However, only the 1996 subindex three focuses exclusively on the legislature and it is this original measure that I refer to in the following. Hallerberg and Marier (2004: 578-579) use a rescaled version of this subindex for their analysis of the interaction of budget institutions and electoral incentives. Cheibub (2006: 364) also draws heavily on these variables and finds evidence that the effect of presidentialism on budget balances is conditional upon the powers of the president in the budget process.

Subindex three combines variables on amendment powers and the reversionary budget (Alesina *et al.* 1999a: 34-35). With regard to amendment powers, it distinguishes countries where amendments cannot increase the size of the budget or its size and the deficit [10], from those where the legislature can do so only with

government approval [7.5], where it can only propose changes that may not increase the deficit [5], and where there are no constraints [0]. With regard to the reversionary budget, the extreme case is that the government proposal is implemented even if the legislature explicitly rejects or fails to approve it [10]. In some instances a distinction is made according to which the lack of timely approval results in the enactment of the government proposal, while rejection triggers reversion to last year's budget [8]. Alesina and colleagues argue that reversion to the previous budget is more favourable for the government than a requirement for tabling a new budget as long as it can redistribute spending between items [6], but not when this is disallowed [2]. Where a new budget has to be presented, they give higher scores where the government has discretion to reallocate until the adoption of the new budget [4] than to those where there is no reallocation [2] or where Congress reallocates expenditures [0].

A few scores are not covered in this account, but can be deduced from Table A6 in Alesina *et al.* (1999a) or the 1996 version of their paper. First, they assign the middle possible score [5] to cases where the government resigns in case of non-approval, arguing that 'this drastic possibility could go either way' since on the one hand the legislature may want to avoid a situation that is costly to the country while the government may be induced to present a 'more palatable' budget in order to avoid loss of office (Alesina *et al.* 1996: 13). This intermediate score is only assigned once in their dataset, to the Bahamas. Second, when no funds may be expended in case of non-approval, Alesina and colleagues (1999a: Table A6) give eight points, which according to their dataset is the case only in Mexico. They add the scores for these two variables, so that a maximum of 20 on subindex three indicates a high degree of executive control of the parliamentary agenda, which they predict to have a positive

effect on fiscal discipline. This is confirmed in their empirical analysis, which finds a negative association of subindex three with primary deficits in Latin American countries in the 1980s and early 1990s (Alesina *et al.* 1996: Table 6).

There are legislative features other than those covered in the above indices that might impact on fiscal aggregates. Heller (1997: 486) argues that the existence of second chambers with budgetary powers increases the number of actors who can veto or modify legislation and this 'forces the government to include more spending in the budget than it would need to if the budget had to pass in only one legislative chamber'. Using a sample of 17 industrialised countries, he finds that deficits are higher in parliamentary systems with bicameral than those with unicameral legislatures. However, with budget deficits rather than expenditures as the dependent variable, it is impossible to distinguish his proposition that budgetary bicameralism leads to higher spending from the rival hypothesis that bicameralism can increase gridlock (e.g. Alt and Lowry 1994). There are also problems with the empirical analysis. The results are to a substantial extent driven by the Italian case (Heller 1997: 502-503), and the classification of countries can be challenged.[2] Moreover, the use of pooled time-series cross-section regression is problematic, since the time-invariant nature of the variable of interest calls for cross-sectional analysis (Kittel 1999). Bicameralism is also discussed by Gleich (2003: 18), who argues on the basis of the common pool perspective that it adds to the fragmentation of the legislature, and hence contributes to a spending and deficit bias. On the other hand, Bradbury and Crain (2001: 322) conclude that 'splitting the legislative branch into two chambers

[2] For instance, Canada is classified as budgetary bicameral.

mitigates the fiscal commons problem'. In short, the impact of bicameralism remains unclear.

Other authors have explored how the fragmentation of spending authority across different legislative committees affects fiscal policy. Crain and Muris (1995) consider the impact of committee structures on fiscal policy at the subnational level in the US. Cogan (1994) provides an interesting historical account of the evolution of committee spending authority in the US Congress, while Dharmapala (2003 and 2004) develops a formalised treatment of this topic. One proposition is that the consolidation of financial decision-making in a single committee is an institutional remedy for the common pool problem and helps to contain spending pressures. In a balkanised committee setting partial spending decisions are distributed across different committees and no single committee is responsible for the overall level of expenditure, which encourages free-riding. Using state-level data from the US Crain and Muris (1995) find that the centralisation of spending decisions in a single committee indeed restrains expenditures compared with balkanised systems. The empirical work on the fiscal effects of committee structures focuses almost exclusively on US legislatures; this paper adds cross-national results.

Data and methods

One of the major drawbacks of the institutionalist literature on fiscal policy is that its empirical work uses different datasets and variable definitions. To enable a more systematic review I use data from a 2003 survey of budget practices by the

Organisation for Economic Co-operation and Development (OECD) and the World Bank to reconstruct the measures discussed above. The survey asked more than 370 questions and was administered to senior budget officials in each participating country.[3] I use data from this survey to reconstruct Von Hagen's (1992) item two and subindex three by Alesina and colleagues (1996), as documented in Table 1. It is convenient to standardise the various indices by rescaling them so that a maximum score of one can be interpreted as most constrained from a legislative perspective and a score of zero as least constrained. This rescaling is also helpful for the multiple regression analysis in the third section. In the following, I work with the rescaled indices. Moreover, any components from these indices are also standardised for the regression analysis, so that all institutional variables of interest are either dummy variables or range between zero and one, with the latter always indicating an institutional feature that is predicted to reduce the level of public spending.

[TABLE 1 ABOUT HERE]

In addition, I reconsider whether bicameralism affects public spending, as Heller's (1997) original hypothesis suggests. I use a simple dummy to indicate budgetary unicameralism, where 1 = the second chamber has lesser budgetary powers than the lower chamber or parliament is unicameral and 0 = otherwise. To explore the fiscal impact of committee structure I use another dummy variable, where 1 = a budget or finance committee plays a central role in the approval process and 0 = otherwise. Unfortunately, cross-national data for OECD countries are not very useful for testing

[3] The survey was completed by 41 countries, including most OECD countries. I focus on the latter group, since these data are more reliable. Moreover, several other countries included in the survey, such as Cambodia and Jordan, are not democracies.

these two hypotheses, due to lack of variation (see Table 1). Most OECD countries are either unicameral or have second chambers with lesser budgetary powers, and most involve a finance or budget committee in the decision-making process on public expenditures (OECD 2002b, OECD and World Bank 2003). Hence, the results for these variables should be treated with caution.

An assessment of the impact of legislative institutions on the size of government requires appropriate left hand side fiscal variables and data. One important choice relates to coverage, i.e. whether to use data for central or general government. Moreover, databases differ in their inclusion of extra-budgetary entities, for instance social security funds. Of the studies reviewed above, Von Hagen (1992) uses general government data, whilst Alesina and colleagues (1999b) use central government data. Elsewhere, Woo (2003: 390-391) points out that central government data can be misleading when other parts of the public sector contribute substantially to fiscal outcomes. To the contrary, Volkerink and De Haan (2001: 222) prefer central government data, arguing that most theories relate to central government. Persson and Tabellini (2003: 38) add data availability as a practical reason in favour of central government data, and they further claim that these data are more reliable. Evidently, many justifications are plausible, but there is no consensus on this issue.

A range of sources for fiscal data are available. The OECD (2005a and 2005b) publishes comprehensive central and general government figures for (most) member countries. The *Government Finance Statistics* (GFS) of the International Monetary Fund (IMF) include central and general government data for a large number of countries (IMF 2005a). However, while countries increasingly report GFS data on an

accrual basis, cash based reporting is still common. These two types of data are not strictly comparable, which restricts sample size and introduces analytical breaks into time series. The IMF also publishes the *International Financial Statistics* (IFS) that include some central government fiscal data (IMF 2005b). For European Union (EU) members and accession candidates, Eurostat (2006) publishes fiscal data based on the 1995 European System of National and Regional Accounts. Hence, the choice of data source has implications for sample characteristics and the exact nature of fiscal variables.

To fully appreciate the implications, it is useful to consider the variation in central government data in particular. For instance, the 1999 to 2003 average for central government spending in Belgium is 29.7 per cent of GDP according to the OECD *National Accounts*, while GFS indicate a share of 43 per cent and IFS a mere 17.7 per cent. These are massive differences in public finance terms that would suggest fundamentally different roles of central government in the economy, and which will impact on results from cross-sectional analysis. Hence, central government data can be highly problematic, since different classifications and reporting bases are in use to define the central government sector and to underpin fiscal reporting. Without an explicit theoretical basis as to why a certain definition of central government should be preferred this raises the prospect that an arbitrary or poorly informed choice of data source affects empirical results. Moreover, the quality of data can vary greatly between different sources. For instance, there are some erratic movements in the IFS data due to breaks in analytic comparability. Again using Belgium, the expenditure to GDP figure calculated from IFS data is 45.7 per cent for 1998, which drops to 18.2 per cent in the following year. The notes for the IFS government finance items

acknowledge that these data are not consistently reported.[4] While this data source is very popular with some researchers (e.g. Persson and Tabellini 2003, Alt and Lassen 2006) because it contains data for a relatively large number of countries, the extent of inconsistency is highly problematic.

In contrast to data on central government, general government data from different sources are highly correlated, with coefficients of around .9 for this sample. The impact of different reporting standards is by far not as substantial compared with central government data, which makes it less likely that the choice of data source will affect empirical results. Moreover, there is a theoretical reason for preferring general to central government data. Because revenue raising powers tend to be more centralised than expenditure responsibilities, decentralised systems to varying degrees suffer from a vertical fiscal imbalance or 'fiscal gap' that has to be filled with intergovernmental transfers and grants, usually from the central government (Ter-Minassian 1997, Shah 1994). Therefore, even when spending is *accounted* for at the subnational level, it is likely that at least a share of it flows via the central government budget and is *voted* by the national parliament. As a result, central and subnational budgets cannot be neatly separated, and they are intimately connected in producing fiscal outcomes (Quigley and Rubinfeld 1996). It is questionable to what extent a simple federalism dummy can account for the complexities of intergovernmental fiscal relations when using central government data (Pierson 1995: 473). Overall, this

[4] For some countries, the IFS data cover the budgetary central government and for others the consolidated central government, but the latter 'may not necessarily include all existing extrabudgetary units'. Moreover, while some countries report specifically for IFS, data for others are as reported for GFS or from 'unpublished worksheets and are therefore not attributed to a specific source' (IMF 2005b: XX).

discussion provides strong reasons for using general government data, even if this means a loss in degrees of freedom due to lower data availability.

A related issue is the choice of appropriate indicators of 'fiscal discipline' or 'fiscal performance'. As with the choice of data coverage, the literature offers a variety of possibilities. Von Hagen (1992) considers gross debt, net lending (i.e. the negative of the conventional deficit) and net lending excluding interest payments (i.e. the negative of the primary deficit). Alesina and colleagues (1999b: 263) use only the primary deficit as the dependent variable, arguing that it is less sensitive to inflation-induced increases in interest payments than the conventional deficit, and that it is a better indicator of the fiscal stance of the current government, whose interest payments are largely determined by previously accumulated debt. Stein *et al.* (1998) use the same institutional data as Alesina and colleagues (1999b), but test the effect on a variety of dependent variables. Interestingly, they find no association between budget institutions and government size, and the most convincing evidence when using the primary balance. Of the other papers reviewed in the first section, Heller (1997) uses conventional deficits, while Crain and Muris (1995) use the logarithm of state revenues and expenditures per capita. Apparently, there is no agreement on what constitutes the most appropriate indicator of fiscal discipline.

The disagreement about appropriate fiscal variables for empirical testing cannot be explained with reference to differences in the underlying theoretical approaches. Formal models in the common pool tradition generate in the first instance predictions about relative levels of public spending (e.g. Von Hagen and Harden 1995, Hallerberg 2004: 22-28), whilst much of the empirical testing uses different fiscal indicators. In

this literature, the use of the deficit as the dependent variable can be justified by assuming at least partly non-Ricardian tax payers who shift some of the cost of today's consumption to future generations (Von Hagen 1992: 32). Still, the most direct test would be to consider the impact of institutional arrangements on levels of public spending. Similarly inconsistent, Heller's (1997) model makes predictions about spending levels, yet he uses deficits as the dependent variable for his empirical test. To align the empirical analysis with the underlying theories, this paper investigates the effect of institutional arrangements on general government expenditures as a percentage of GDP (multiplied by 100).

In terms of control variables, I draw on Persson and Tabellini (2003: 39), who review a range of country characteristics that on the basis of theoretical or empirical work can be expected to influence the size of the public sector. Following Wagner's Law, which suggests that the demand for government services is income elastic, I control for levels of economic development with the natural log of per capita income (in constant 2000 US$). The demographic structure of the population has implications for public spending and is accounted for with two variables: the share of the population between age 15 and 64, and the share of the population age 65 or above (multiplied by 100). Finally, Rodrik (1998) argues that demand for social protection increases with trade openness, which is measured as the share of GDP of imports plus exports of goods and services (multiplied by 100).[5] These data are from the *World Development Indicators* (World Bank 2005).

[5] I omit several controls that Persson and Tabellini (2003) include in their basic model. First, they use central government data and control for fiscal decentralisation with a federalism dummy. Here, the dependent variable relates to general government. Second, they include a dummy to indicate OECD membership prior to 1993, excluding Turkey. I drop this dummy, since all except four countries in this

The question of how legislative institutions affect public spending levels calls for cross-sectional analysis. However, the small sample size restricts degrees of freedom. Moreover, it is possible that a relationship between variables is not stable across time. In the empirical analysis that follows, I rely mainly on cross-sectional data, but complement this with time-series cross-section analysis. Since institutional data are often time-invariant or rarely changing, such variables raise methodological issues in the context of fixed effects panel models. Unit fixed effects are collinear with time-invariant variables and 'soak up most of the explanatory power' of rarely changing variables (Beck 2001: 285). Random effect models on the other hand assume that unobserved effects are a random sample drawn from a large population (Baltagi 2005: 35). This is not tenable in macro-comparative research. Faced with this issue, one option is to discard unit fixed effects when investigating the impact of time-invariant institutional variables (e.g. Hallerberg and Marier 2004, Cheibub 2006). However, this introduces omitted variable bias and forfeits the advantage of accounting for unit heterogeneity.

As a possible alternative, Plümper and Troeger (2006) suggest a three-step process that they call 'fixed effects vector decomposition' (FEVD) to estimate time-invariant and rarely changing variables in models with unit fixed effects. The first stage is to estimate the unit fixed effects with a model excluding the completely time-invariant explanatory variables. The second stage decomposes the unit fixed effect by

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sample (the Czech Republic, Hungary, Slovakia and South Korea) are traditional OECD members. Its inclusion does not substantively affect the results. Finally, there is no need to control for the quality of democracy, as fiscal data for Turkey and Mexico are not available and the Freedom House scores for the remaining countries in this sample are very similar.

regressing them onto the time-invariant variables excluded from stage one plus any rarely changing variables included in stage one, using OLS. The third stage estimates a pooled model with all explanatory variables as well as the unexplained part of the unit fixed effects, and calculates standard errors with adjusted degrees of freedom that account for the number of estimated unit effects in the first stage. The following section turns to the empirical analysis and reports results using these approaches.

Analysis

In this part, I systematically test subindex three by Alesina *et al.* (1996) and Von Hagen's (1992) item two. My approach is index decomposition, as used for instance by Edin and Ohlsson (1991) to qualify Roubini and Sachs' (1989) study of the effect of partisan variables on fiscal adjustment. This entails taking apart the indices to test the impact of each component separately. I start with a basic model for the 25 OECD countries for which there are data on both general government spending as well as the institutional variables of interest. This includes the controls for demographic structure, level of economic development, and trade openness. I use averages of the data over the 1999 to 2003 period.[6] Together, the socio-economic variables account for about half of the variation in general government expenditures in this sample (see column 1 in Table 2).

Table 2 reports the results with Von Hagen's legislative variables. The coefficient for the standardised version of item two is large and significant at the 5 per cent level

[6] I also used alternative 1994 to 2003 averages, which did not affect the substantive results.

(column 2). I proceed to test the effect of each component. The coefficients for the two variables associated with limitations on amendment powers are significant at the 5 per cent level or higher (columns 3 and 4), but not those of any other component variable (columns 5 to 7). Moreover, the coefficients for the last two components have the wrong sign. Component four (column 6) considers whether all expenditures are passed chapter-by-chapter, and the fifth component (column 7) looks at whether the process commences with a global vote on the size of the total budget. When each separate component is included simultaneously, none of them is significant (column 8). However, the amendment limits and offsetting dummies are jointly significant ($F = 7.70$, $p = .005$). This provides evidence that the results for item two are driven by one particular institutional feature, namely differences in the legislative powers to amend the budget proposed by the executive.

[TABLE 2 ABOUT HERE]

Table 3 repeats this exercise with the standardised version of Alesina *et al.*'s (1996) subindex three. The coefficient for subindex three is large and significant at the 5 per cent level, and it has the predicted sign (column 1). Tested separately, the coefficient for the amendment powers variable has the predicted sign and is significant at the 5 per cent level (column 2). In contrast, the coefficient for the reversionary budget variable is not significant, although it has the predicted sign (column 3).[7] When both components are included simultaneously, only the coefficient for amendment powers achieves statistical significance at the 10 per cent level (column 4). Here again, there is evidence that one particular component drives the results. Moreover, as in the

[7] I also used a different operationalisation of the reversionary budget variable, proposed by Wehner (2006), which yielded a similar result.

reanalysis of Von Hagen's item two, it is the variable associated with the amendment powers of the legislature that is significant, and the coefficient also has a similar size. This provides reassurance that the result that this variable affects levels of public spending is not due simply to a particular operationalisation. This finding is of interest in the light of recent contributions that attribute significant importance to both variables, but fail to distinguish their impact in empirical analyses (Hallerberg and Marier 2004, Cheibub 2006).

[TABLE 3 ABOUT HERE]

Table 3 also reports results for the committee hypothesis by Crain and Muris (1995) and Heller's (1997) claim about the fiscal effect of bicameralism. The coefficient for the budget committee dummy has the wrong sign and is not significant (column 5). However, only four cases in this sample (Australia, Canada, Netherlands, and the UK) do not use a specialised budget committee during the approval stage of the budget, and all of these except the Netherlands severely restrict parliamentary powers of amendment. Hence, these data provide a poor test for the committee hypothesis, and the result should not be over-interpreted. With regard to the budgetary unicameralism dummy, the coefficient has the wrong sign and is far from significant (column 6). I also used a more permissive version of this variable, where systems with second chambers that have powers over taxation but not expenditure (Germany) are also counted as bicameral, but this did not substantively affect the result. There is no evidence in these data to support Heller's (1997) theory about the pro-spending bias of bicameralism. However, the limited occurrence of budgetary bicameralism in the

sample (Australia, Italy, the Netherlands, and the US) again cautions against reading too much into this finding.

The results so far consistently indicate that variously defined indicators of legislative powers of amendment are the only legislative variable with a significant impact on public spending. I conduct some robustness checks with the simplest indicator, Von Hagen's (1992) dummy for limits on amendment powers. Table 4 confirms that the results are very robust. In the first column I add two additional institutional variables identified by Persson and Tabellini (2003) as significant determinants of the size of government, i.e. presidentialism and a plurality rule electoral system.[8] I then make the cases more homogenous, first by excluding the two presidential systems, i.e. the US and South Korea (column 2), and second by restricting the sample to OECD members that joined the organisation prior to 1993, which means dropping the Czech Republic, Hungary, Slovakia and South Korea (column 3). The results in column 4 are for an alternative indicator of the size of government, i.e. total revenues as a percentage of GDP (multiplied by 100). The amendment powers dummy remains significant throughout. Finally, I use a dummy indicating former UK colonies (with independence in the past 150 years) as an instrument, which assumes that this variable does not influence fiscal policy except through its effect on institutional arrangements. With two stage least squares (2SLS), the significance of the coefficient for amendment powers is exactly at the 10 per cent level (column 5). Overall, these results are very robust and suggest that, in the advanced industrialised democracies,

[8] Japan and New Zealand carried out reforms in 1994 and 1996 respectively that entailed a move from a majoritarian to a mixed electoral system (Persson and Tabellini 2003: 83) and are coded as majoritarian to account for the long term effect of the previous electoral system. Changing the coding for these two countries to reflect the new system does not substantively affect the results.

restrictions on parliamentary powers to amend budgets constrain the size of the overall public sector relative to GDP by about five percentage points compared with systems that do not limit these powers.[9]

[TABLE 4 ABOUT HERE]

Unfortunately, there is no comprehensive dataset that documents how all of the legislative institutions of interest have evolved over a longer period of time. On the other hand, parliamentary powers of amendment over the budget are highly time-invariant. Some countries did reform provisions governing amendment powers, such as France in 2001 (Chabert 2001) and New Zealand in 1996 (Lienert and Jung 2005: 330), but a fundamental switch from restricted to unrestricted powers of amendment or vice versa is rare.[10] Since constitutional provisions on amendment powers are costly to change, they can reasonably be treated as an exogenous variable in at least the short to medium run (Alesina and Perotti 1996: 4).

To exploit the variation of the non-institutional variables in the time dimension, I construct a panel dataset covering the period 1970 to 2003, for which the OECD publishes fiscal data (OECD 2005b). As in the cross-sectional analysis, there are no data for Mexico and Turkey, and a further three countries are omitted because they did not respond to the 2003 survey of budget procedures (Luxembourg, Poland and

[9] I also ran these regressions with IMF and Eurostat data for general government, which did not substantively affect the findings. These results are not reported, but can be obtained from the author.

[10] Such changes occurred recently in some countries outside this sample. For instance, there were no restrictions in Poland until 1998, but article 220(1) of the constitution now prohibits amendments that result in a higher deficit than in the executive draft budget. Romania introduced similar restrictions with article 17(3) of the 2002 Law on Public Finance (Yläoutinen 2004).

Switzerland). I thus maintain the sample of 25 cases used for the cross-sectional analysis. The resulting panel is unbalanced. Fiscal data for New Zealand are only available as from 1985. I exclude years prior to democratisation for countries that made a transition from authoritarian rule during the sample period, i.e. South Korea (1988), Portugal (1977), Greece (1975), and Spain (1978). The data series for the transition countries in the sample start later, i.e. for Hungary in 1991 and for the successor states of Czechoslovakia, the Czech Republic and Slovakia, in 1993 and 1994 respectively. The socio-economic data are readily available for the entire period (World Bank 2005). The variable of interest is the time-invariant dummy indicating limits on amendment powers. Several other institutional variables may affect spending levels and are either time-invariant or rarely changing. The sample contains no case that switched from pure presidentialism to other forms of government or vice versa. Only three countries implemented relevant reforms of their electoral systems during the sample period. Japan (1994) and New Zealand (1996) moved away from plurality rule and introduced mixed systems, while France briefly abandoned plurality rule during 1985 and 1986 (Persson and Tabellini 2003: 83-88).

I first estimate the effect of limiting parliamentary amendment powers with a model that excludes unit fixed effects. To mitigate omitted variable bias, I include various time-invariant or hardly changing variables, in particular the dummies for OECD membership prior to 1993, former UK colonies, presidentialism and plurality rule. I also control for a possible Maastricht effect in the run-up and during monetary union by including a dummy variable (coded 1 = 1992 or later, 0 = otherwise) for the twelve

members of the Euro area, the so-called EU12.[11] Since the prospect of EU membership in Eastern Europe may have induced fiscal tightening to meet convergence criteria, I also include a dummy to indicate former communist countries or their successor entities. Here, I adopt the Beck and Katz standard, viz. a lagged dependent variable on the right hand side to address autocorrelation, and panel corrected standard errors (Beck and Katz 1995). In substantive terms, the lagged dependent variable accounts for the stickiness of spending by capturing the influence of past expenditures on annual levels (Davis *et al.* 1966).

[TABLE 5 ABOUT HERE]

Table 5 presents the results. I start with estimates for the entire sample period (1971 to 2003) using OLS with year dummies and a lagged dependent variable (column 1). The coefficient for the amendment dummy has the predicted sign and is significant at the 5 per cent level. To explore whether this relationship is stable over time, I split the sample into two periods and estimate the same model for each separately. For the first half of the sample period (1971 to 1987) the amendment dummy is no longer significant (column 2). However, for the second half of the period (1988 to 2003) the coefficient is large, has the predicted sign, and is significant at the 1 per cent level (column 3). The FEVD specification suggest that limitations on amendment powers

[11] The EU12 are Austria, Belgium, Finland, France, Germany, Greece (since 2001), Ireland, Italy, Luxembourg, the Netherlands, Portugal, and Spain. I experimented with different versions of this variable. First, I used a dummy for the EU15, i.e. the EU12 plus Denmark, Sweden and the UK. Second, following Volkerink and De Haan (2001: 236), I used different years from which onwards the EU12 dummy is set equal to one. Only with a start date of 1997 or 1998 is the coefficient both negative and significant. However, since this did not substantively affect the coefficients for the variable of interest, I do not report these results here.

account for a difference in general government expenditure of about 3 per cent of GDP over the entire sample period (column 4). The next two columns again present separate results for each half of the sample period. The amendment dummy is significant for both periods. In the later period, the effect on general government expenditure is estimated to represent almost 6 per cent of GDP, which is similar to the results obtained in the cross-sectional analysis. The evidence is mutually reinforcing.

Conclusions

There is a growing interest in the fiscal effects of institutional variables. Several findings in this paper add to this research. In terms of variables and data, the paper serves as a reminder that this research agenda would benefit from paying more careful attention to the dependent variable. The choice of fiscal indicator should be closely linked to theoretical work. Similarly, the choice of data source should consider the implications of different classifications and reporting standards. In terms of methods, fixed effects vector decomposition appears to be a useful complement to standard cross-section and panel analysis in a context where the variables of interest are rarely changing or time-invariant, which is common in institutionalist research.

In substantive terms, this analysis suggests that we should not rush to accept the superiority of complex composite indices over more simple and transparent variables when investigating institutional determinants of fiscal policy. This paper is the first to directly compare different indices of legislative budget power on the basis of a common dataset. The conclusion is that the empirical performance of composite

measures of legislative power is driven by one particular variable, i.e. the power of the legislature to amend the budget. Other budget institutions that are often combined into indices do not appear to significantly affect the size of the public sector. A larger sample of countries would allow for more fine-grained assessment of the fiscal impact of various institutional features. However, the finding that amendment powers have the most explanatory power amongst a range of legislative institutions discussed in the literature is very robust and unlikely to be affected.

Finally, these results are relevant for constitutional economics. The design of the power of the purse is a basic constitutional choice that fundamentally affects the role of the legislature in public finance. Data for this variable can be collected relatively easily for a large number of countries from existing surveys and constitutional documents, thus making it a strong candidate for inclusion in further work on the economic effects of constitutions. Recent work by Cheibub (2006) goes into this direction, and qualifies regime effects of public spending with a more fine-grained understanding of executive control over fiscal policy. Future research should explore these interactions more comprehensively.

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Table 1: Institutional variables

	(1)	(2)	(3)	(4)	(5)	Σ	(6)	(7)	Σ	(8)	(9)
Legislature	Amendments limited	Amendments offsetting	Amendments cause fall	One vote on expenditure	Global vote	Von Hagen item two[a]	Amendment restrictions	Reversionary budget	Alesina subindex three[b]	Budget committee[c]	Budgetary unicameralism[d]
Australia	4	0	0	2	0	6	10	8	18	0	0
Austria	0	0	0	0	0	0	0	2	2	1	1
Belgium	0	0	4	0	0	4	0	8	8	1	1
Canada	4	0	4	2	0	10	10	8	18	0	1
Czech Republic	0	0	0	0	4	4	0	6	6	1	1
Denmark	0	0	4	0	0	4	0	6	6	1	1
Finland	0	0	0	0	0	0	0	10	10	1	1
France	4	0	0	2	4	10	10	10	20	1	1
Germany	0	0	0	0	4	4	0	6	6	1	1
Greece	4	4	4	0	0	12	10	6	16	1	1
Hungary	0	0	0	0	0	0	0	8	8	1	1
Iceland	0	0	0	0	0	0	0	5	5	1	1
Ireland	4	4	0	0	0	8	10	10	20	1	1
Italy	0	0	0	0	0	0	0	10	10	1	0
Japan	4	4	0	0	0	8	10	8	18	1	1
Mexico	4	4	0	0	0	8	5	8	13	1	1
Netherlands	0	0	0	4	0	4	0	6	6	0	0
New Zealand	4	0	4	2	0	10	10	8	18	1	1
Norway	0	0	0	0	4	4	0	8	8	1	1
Portugal	0	0	0	0	4	4	0	6	6	1	1
Slovakia	0	0	0	0	0	0	0	10	10	1	1
South Korea	4	0	0	0	0	4	10	6	16	1	1
Spain	4	4	0	0	4	12	5	6	11	1	1
Sweden	0	0	0	4	4	8	0	8	8	1	1
Turkey	4	4	0	0	0	8	5	8	13	1	1
United Kingdom	4	0	4	0	0	8	10	8	18	0	1
United States	0	0	0	4	4	8	0	8	8	1	0

Notes: Where the OECD data were inconsistent with those reported by Wehner (2006) the latter are preferred. In addition, Slovakia is scored as having unfettered amendment powers during the sample period (see Gleich 2003: 25, Yläoutinen 2004: 71), since there are no constitutional limitations, although the EU convergence programme contains deficit targets (personal correspondence received from the Chancellery of the National Council of the Slovak Republic). [a] Based on responses to questions 2.7.d (amendments limited), 2.7.e (amendments offsetting), 2.7.h (amendments cause fall), 2.8.a (one vote on expenditure) and 2.7.j (global vote) in OECD and World Bank (2003). [b] Based on responses to questions 2.7.e (amendment restrictions) as well as 2.7.c and 3.2.a.4 (reversionary budget) in OECD and World Bank (2003). [c] Based on responses to question 2.10.a in OECD and World Bank (2003). [d] Based on Heller (1997) and current constitutions.

Table 2: OLS estimates of Von Hagen's variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Von Hagen item two		-12.20						
		(5.13)**						
Amendments limited			-5.79					-3.91
			(2.33)**					(4.74)
Amendments offsetting				-8.17				-4.82
				(2.17)***				(4.73)
Amendments cause fall					-0.48			0.90
					(3.01)			(3.75)
One vote on expenditure						2.38		1.30
						(2.90)		(2.87)
Global vote							0.27	-1.06
							(2.11)	(1.79)
Log of GDP per capita	-2.63	-2.75	-2.62	-1.76	-2.65	-2.74	-2.59	-2.29
	(2.21)	(1.91)	(1.68)	(1.68)	(2.30)	(2.27)	(2.20)	(1.68)
Working age population share	-1.56	-1.81	-1.44	-0.99	-1.59	-1.39	-1.55	-1.04
	(0.75)**	(0.72)**	(0.63)**	(0.58)	(0.77)*	(0.74)*	(0.76)*	(0.62)
Old age population share	1.72	1.72	1.48	2.11	1.73	1.82	1.72	1.87
	(0.36)***	(0.29)***	(0.33)***	(0.30)***	(0.36)***	(0.40)***	(0.37)***	(0.53)***
Trade as share of GDP	0.05	0.03	0.02	0.05	0.05	0.05	0.05	0.03
	(0.04)	(0.03)	(0.03)	(0.02)**	(0.04)	(0.03)	(0.04)	(0.03)
Constant	145.99	168.79	145.84	94.88	148.57	133.48	144.65	109.18
	(67.32)**	(63.84)**	(56.24)**	(53.86)*	(70.05)**	(65.00)*	(67.82)**	(53.79)*
Observations	25	25	25	25	25	25	25	25
Adjusted R-squared	0.51	0.59	0.64	0.67	0.48	0.49	0.48	0.63

Notes: * p < .1, ** p < .05, *** p < .01. Robust standard errors in parentheses. The dependent variable for all models is general government total outlays as a percentage of GDP multiplied by 100 (OECD 2005b). The dependent variable and all economic control variables are averaged over the 1999 to 2003 period. All institutional variables are standardised to range between zero and one. See text for further details.

Table 3: OLS estimates of other variables

	(1)	(2)	(3)	(4)	(5)	(6)
Alesina subindex three	-9.29					
	(4.03)**					
Amendment restrictions		-5.38		-5.03		
		(2.44)**		(2.42)*		
Reversionary budget			-6.41	-3.15		
			(5.40)	(4.45)		
Budget committee					1.43	
					(2.80)	
Budgetary unicameralism						1.11
						(1.85)
Log of GDP per capita	-2.87	-2.56	-3.04	-2.77	-2.41	-2.50
	(1.67)	(1.73)	(2.21)	(1.75)	(2.52)	(2.38)
Working age population share	-1.72	-1.51	-1.84	-1.65	-1.53	-1.53
	(0.64)**	(0.65)**	(0.73)**	(0.63)**	(0.78)*	(0.78)*
Old age population share	1.45	1.44	1.67	1.44	1.69	1.71
	(0.31)***	(0.35)***	(0.32)***	(0.34)***	(0.38)***	(0.36)***
Trade as share of GDP	0.03	0.03	0.05	0.03	0.05	0.05
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Constant	169.86	150.12	174.04	163.63	141.14	142.32
	(56.08)***	(56.82)**	(67.81)**	(55.60)***	(73.02)*	(71.94)*
Observations	25	25	25	25	25	25
Adjusted R-squared	0.62	0.61	0.51	0.60	0.49	0.49

Notes: * p < .1, ** p < .05, *** p < .01. Robust standard errors in parentheses. The dependent variable for all models is general government total outlays as a percentage of GDP multiplied by 100 (OECD 2005b). The dependent variable and all economic control variables are averaged over the 1999 to 2003 period. All institutional variables are standardised to range between zero and one. See text for further details.

Table 4: Robustness checks

	(1)	(2)	(3)	(4)	(5)
Amendments limited	-6.96	-7.24	-5.63	-6.13	-5.53
	(2.20)***	(2.20)***	(2.53)**	(3.02)*	(3.20)
President	-6.76				
	(4.51)				
Plurality rule	1.51				
	(2.84)				
Log of GDP per capita	-2.15	-2.38	-2.08	0.56	-2.62
	(1.79)	(1.82)	(2.94)	(2.27)	(1.69)
Working age population share	-1.02	-1.56	-1.65	-2.08	-1.44
	(0.73)	(0.87)*	(0.98)	(0.93)**	(0.60)**
Old age population share	1.12	1.18	1.61	0.82	1.49
	(0.37)***	(0.37)***	(0.47)***	(0.55)	(0.33)***
Trade as share of GDP	0.01	0.00	0.03	0.03	0.03
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)
Constant	120.64	158.84	152.15	166.21	145.84
	(59.67)*	(70.76)**	(75.02)*	(82.93)*	(56.20)**
Method	OLS	OLS	OLS	OLS	2SLS
Dependent variable	Exp.	Exp.	Exp.	Rev.	Exp.
Observations	25	23[a]	21[b]	25	25
Adjusted R-squared	0.64	0.53	0.47	0.49	0.64

Notes: * p < .1, ** p < .05, *** p < .01. Robust standard errors in parentheses. The dependent variable is general government expenditure (Exp.) as a percentage of GDP multiplied by 100 (OECD 2005b), except in column 4, where it is revenues (Rev.). The dependent variable and all economic control variables are averaged over the 1999 to 2003 period. All institutional variables are dummies. See text for further details. The instrumented variable in column 5 is Von Hagen's amendment limits dummy; in addition to the second stage controls the first stage includes the UK colony dummy. [a] Sample restricted to countries with parliamentary systems of government. [b] Sample restricted to OECD members before 1993.

Table 5: Time-series cross-section analysis, 1971 to 2003

	(1)	(2)	(3)	(4)	(5)	(6)
Amendments limited	-0.39	0.30	-0.84	-2.96	-7.86	-5.62
	(0.19)**	(0.27)	(0.30)***	(0.19)***	(0.95)***	(0.24)***
EU12	-0.10		0.08	0.19		-0.02
	(0.15)		(0.16)	(0.79)		(0.30)
President	-0.83	0.12	-1.59	-3.65	-7.50	-6.15
	(0.24)***	(0.27)	(0.37)***	(0.75)***	(0.85)***	(0.29)***
Plurality rule	-0.24	-0.73	-0.03	-0.07	5.53	0.37
	(0.18)	(0.27)***	(0.21)	(0.71)	(0.81)***	(0.30)
Former UK colony	-0.01	0.48	-0.25	1.16	-1.51	3.09
	(0.25)	(0.18)***	(0.42)	(0.69)*	(0.89)*	(0.30)***
Former communist country	-1.61		-1.82	-3.64		-5.35
	(1.01)		(1.13)	(0.67)***		(0.29)***
OECD member before 1993	-1.13		-1.78	-0.59		-1.82
	(0.85)		(1.21)	(0.57)		(0.31)***
Log of GDP per capita	-0.02	-0.02	0.07	-2.53	-9.78	-3.32
	(0.21)	(0.32)	(0.31)	(1.06)**	(1.95)***	(1.37)**
Working age population share	-0.01	0.10	-0.07	0.05	-0.39	0.39
	(0.03)	(0.06)*	(0.06)	(0.06)	(0.08)***	(0.11)***
Old age population share	0.15	0.21	0.14	0.14	-0.45	0.60
	(0.05)***	(0.05)***	(0.08)*	(0.07)**	(0.08)***	(0.13)***
Trade as share of GDP	-0.00	0.01	-0.01	-0.05	-0.03	-0.07
	(0.00)*	(0.00)**	(0.00)***	(0.01)***	(0.02)	(0.01)***
Lagged dependent variable	0.92	0.91	0.90	0.82	0.76	0.63
	(0.01)***	(0.03)***	(0.02)***	(0.02)***	(0.03)***	(0.04)***
Constant	4.38	-6.04	9.57	33.85	142.26	23.67
	(2.59)*	(5.56)	(4.25)**	(0.74)***	(0.13)***	(0.86)***
Method	OLS	OLS	OLS	FEVD	FEVD	FEVD
Country dummies	No	No	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	21	25	25	21	25
Years	1971-2003	1971-1987	1988-2003	1971-2003	1971-1987	1988-2003
Observations	703	321	382	703	321	382

Notes: * p < .1, ** p < .05, *** p < .01. Panel corrected standard errors in parentheses. The dependent variable is general government expenditure as a percentage of GDP multiplied by 100 (OECD 2005b). The second stage of FEVD includes the dummies for amendment limits, presidentialism, plurality rule, former UK colony, former communist country and OECD membership prior to 1993. The regressions for 1971 to 1987 exclude dummies without any variation over this period.