

Electoral Institutions and Distributive Policies in Parliamentary Systems: An Application to State Aid Measures in EU Countries

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Abstract

Electoral institutions should systematically affect the propensity of a country to rely and spend on distributive measures. Supporting evidence is however still rare because of the difficulty in finding comparable cross-country data, the employment of dummy variables to account for the electoral systems, and the failure to recognize the interacting effects of different electoral rules on policy outcomes. Employing national data on state aid expenditure and number of measures across European Union countries, we provide evidence that legislators elected in higher magnitude districts spend less. More interestingly, we show the interlocking policy effects of electoral institutions. Where high district magnitude is combined with ballot control, party-based voting and pooling, these rules conjunctly dampen politicians' incentives to cultivate a personal vote and lead to lower spending on *and* use of distributive measures. Where high district magnitude is not combined with these rules, results are inconclusive. With one exception though, if leaders do not have control over the ballot rank, higher magnitude increases reliance on distributive measures. Results are robust to several alternative political-economy explanations of fiscal policy outcomes.

Electoral institutions matter for policy outcomes and considerable scholarly attention have been devoted recently to the policy consequences of different electoral designs (Cao et al. 2007; Chang 2008; Edwards and Thames 2007; Hallerberg and Marier 2004; Milesi-Ferretti et al. 2002; Persson and Tabellini 1999; 2000; 2003; 2004; Rickard forthcoming; 2009; Zahariadis 2010). According to one of the most prominent theories (Persson and Tabellini 1999; 2000), electoral institutions should systematically affect the propensity of a country to rely and spend on distributive measures. Notwithstanding some recent exceptions (Chang 2008; Rickard forthcoming; 2009), supporting evidence is still rare though. A first cause of this paucity of research resides on the difficulty in finding comparable cross-country data on measures such as public infrastructural investments (Rickard 2009: 674). A second cause may be related to the employment of dummy variables to account for the electoral systems. These measures solely distinguish, with some coding differences, between single member district systems and the others (Cao et al. 2007; Chang 2008; Persson and Tabellini 2003; 2004; Rickard forthcoming; 2009). As we will argue below, Persson and Tabellini (1999; 2000) suggest that normalized district magnitude should drive spending on distributive measures. The information on the heterogeneity of this attribute within mixed and proportional representation (PR) systems is lost if we employ a dummy variable. There are clearly significant differences between the Spanish and the Dutch PR systems, the former with fifty-two districts and the latter with a single one. Unfortunately however, works that include measures of district magnitude do not yield corroborating evidence (Milesi-Ferretti et al. 2002). This could be related to a third cause. The theory could be incomplete. Electoral systems are more than just about district magnitude. They comprise of other rules that may significantly influence the propensity of politicians to spend and rely on distributive measures, even across systems with similar magnitude. Why, for instance, should we expect the incentives of politicians to be the same in the Spanish closed-list and the Polish open-list systems?

In this article, we address these three shortcomings. First, we argue that the policy on state aid control of the European Union (EU) offer comparable information that both fits the definition of

distributive policy and allows us to exploit enough cross- and within-country variation for testing institutionalist hypotheses properly. We employ data on state aid expenditure by EU member states between 1992 and 2009 and on applications to grant aid logged by such states between 1999 and 2009 and we show how electoral institutions affect both spending and reliance. With the exception of a related work of Rickard (2009), the latter research question has been mostly ignored so far. Second, we provide direct evidence, for the first time to our knowledge, that legislators elected in higher magnitude districts spend proportionally less on distributive measures. Third, more interestingly, we illustrate the contextual impact of other electoral rules, specifically those affecting the incentives of politicians to cultivate a personal vote (Carey and Shugart 1995). Electoral institutions are particularly effective when they influence incentives concertedly in the same direction. Where high district magnitude is combined with control of the ballot rank by party leaders, party-based voting and pooling, these electoral rules conjunctly dampen the incentives of politicians to cultivate a personal vote and lead to lower spending on *and* use of distributive measures. Where high district magnitude is not combined with these rules, results are inconclusive. With one exception though, if leaders do not have ballot control, higher magnitude increases policy fragmentation, that is, it leads to the adoption of *more* measures. These results are robust to several alternative explanations.

In the next section, we employ the political-economy literature on the determinants of fiscal policy, especially of distributive measures, to develop three hypotheses on spending and frequency. Next, we present the data we have collected, the operationalization of the variables and discuss alternative explanations. The last section introduces the results, evaluates their robustness and draws the conclusions.

Electoral Institutions and Distributive Policies in Parliamentary Systems

It has been argued for quite some time that the incentives of politicians for adopting distributive measures increase with the number of electoral districts (Weingast et al. 1981). Because the benefits of such measures are concentrated while their costs are shared across all the districts through the

mechanism of generalized taxation, the higher the number of districts is, the higher the district-specific net benefits. Politicians seeking re-election in any given district will therefore demand more distributive measures, giving rise to a common-pool resource problem. Weingast et al. (1981) assume that legislators decide unanimously, but Persson and Tabellini (1999; 2000) show that such problem arises also if the assembly acts by majority voting. They model two office-seeking parties that offer a policy platform composed of a district-specific transfer payment (i.e., a distributive measure) and a public good. For concreteness, the choice could be between a regional development grant and a general increase in unemployment benefits. Persson and Tabellini (1999; 2000) show that, in equilibrium, the size of the distributive transfer is higher, and the provision of the public good is lower, in a polity with three districts than in one with a single district. In the former system, it is optimal for the parties to spend more on distributive measures, targeted to the marginal districts, and less on the public good. The costs of these distributive transfers are smaller because ‘the parties now do not internalize the votes lost in the non-marginal districts’ as a consequence of the lower provision of the public good (Persson and Tabellini 1999: 712). These lost votes do not affect the chances of reelection, so this cost is not internalized.¹ We should therefore expect more spending on distributive measures.

In sum, some features of the electoral system, such as its district magnitude, may discourage the internalization of costs and, therefore, influence the choice between different types of fiscal measures. Moreover, we should also expect greater policy fragmentation, exactly because distributive measures are geographically targeted. The key expectation is therefore that *the less the electoral system encourages the internalization of costs (the lower the district magnitude), the higher the spending on and frequency of use of distributive measures* (H1).

¹ For a similar, but less formal, argument see Lancaster (1986) and McGillivray (2004). Lizzeri and Persico (2001) and Milesi-Ferretti et al. (2002) produce models with similar conclusions.

Persson and Tabellini (2003: 169-79; 2004) show that countries with majoritarian electoral systems spend less on broad (public good-like) entitlement programs, such as pensions and unemployment insurance, than countries that employ PR systems. However, no direct evidence is provided to support the claim that a country with a low cost-internalizing electoral system spends more on distributive measures. Since Persson and Tabellini (2003: 169-79; 2004: 169-79) also show that the size of government of countries with majoritarian electoral systems is smaller, it may well be the case that they spend less. Milesi-Ferretti et al. (2002), Cao et al. (2007) and Zahariadis (2010), for instance, fail to find corroborating evidence.² More reassuringly, Chang (2008) has recently produced evidence of slightly more distributive spending, close to elections, in countries with a single veto player and single member electoral districts. Rickard (forthcoming) has found that countries with majoritarian or mixed-member majoritarian electoral systems spend more on subsidies. There is also some related evidence on the frequency of use of distributive measures. Rickard (2009) shows that countries with majoritarian electoral systems face a higher number of formal WTO complaints related to narrow distributive transfers, indicating a greater propensity to employ this policy instrument. All these works employ dummy variables to account for the different electoral systems.³

This still weak empirical record may be due to other features of the electoral system that influence the preferences of politicians. In addition to how seats are allocated among parties, an electoral formula specifies how they are allocated among candidates within parties. Carey and Shugart (1995:

² Milesi-Ferretti et al. (2002) do not find evidence that countries with low district magnitude electoral systems spend more on (geographically targeted) purchases of goods and services, while Cao et al. (2007) do not find that PR shifts spending from industrial subsidies to social welfare programmes. Zahariadis (2005) finds higher spending on (general and sectoral) aid as electoral competition increases but only if there is a trade deficit, while higher competition leads to less spending when trade is balanced. His measure of competition ignores electoral institutions however.

³ Zahariadis (2010), an exception, employs a vote-seat index which is more related to the outcome rather than to the features of an electoral system.

417-8) argues that these rules ‘affects the extent to which individual politicians can benefit by developing personal reputations distinct from those of their party’. This is important because building a personal reputation is frequently associated with securing policies that deliver district-specific benefits (e.g. Carey and Shugart 1995; Lancaster 1986). Carey and Shugart (1995) identify four key characteristics: the degree of control party leaders exercise over access to their party’s label (ballot); whether votes cast for one candidate of a given party also contribute to the number of seats won in the district by the party as a whole (pool); whether voters are allowed to cast only a single vote for a party, multiple votes, or a single vote for a candidate (votes); and the district magnitude. The incentives to cultivate a personal vote, and to pursue distributive measures, increase with weaker ballot control, less pooling and multiple or candidate-level voting, more so as district magnitude increases. We should expect these features leading to higher policy fragmentation as well.

Carey and Shugart (1995: 431) put particular emphasis on how control over the ballot switches the direction of the impact of district magnitude on these incentives. District magnitude operates in the same way as suggested by Persson and Tabellini (1999; 2000) only if leaders have strong control over the ballot (e.g. in closed-list systems). The abovementioned expectation can therefore be reformulated as follows: *if party leaders have control over the ballot, the less the electoral system encourages the internalization of costs (the lower the district magnitude), the higher the spending on and frequency of use of distributive measures (H2a). If such control is missing, Carey and Shugart (1995) contend that the relation should reverse because higher magnitude increases intraparty competition and the incentives to cultivate a personal vote. The higher the magnitude is, the higher the spending and frequency of use (H2b).*

The other two electoral institutions (pool and votes) do not possess this switching property. Carey and Shugart (1995) argue that there should be a straightforward positive interaction with district magnitude. In other words, *as the electoral system strengthens the (pool and votes) incentives to*

cultivate a personal vote, the higher the district magnitude, the higher the spending on and frequency of use of distributive measures (H3).

Empirical support is however still tenuous and indirect. In Latin American countries for instance, if electoral institutions that encourage pursuing a personal vote are associated with a weak executive, the budget balance tends to worsen (Hallerberg and Marier 2004). More recently, Edwards and Thames (2007) provide evidence that total and education expenditures increase with district magnitude in systems with low incentives to cultivate a personal vote, while they decrease in systems with high incentives.⁴

This latter result disconfirms Persson and Tabellini's (public good-related) expectation, but it does not necessarily confirm Carey and Shugart's, for two reasons. First, the substantive focus of both these works is not distributive spending. Second, Edwards and Thames interact the district magnitude with an average score over the three dimensions (ballot, pool and votes), but disregard the switching impact of ballot control alone - hypothesis 2 above -, which is one of Carey and Shugart's (1995: 431) core expectations. Hallerberg and Marier add up the scores on ballot, pool and votes, which are then either added to or divided by the (log of) district magnitude, depending on the type of electoral system, to produce a personal vote index. This operationalization is in the spirit of Carey and Shugart (1995), but it is not designed to test specifically their expectation. The substantive interest of Hallerberg and Marier is on how the interaction between this syncretic index and executive strength affects the budget balance.

Data, measurement and alternative explanations

State aid spending and measures

⁴ Golden and Picci (2008) show that, in the open-list PR system operating in Italy until 1994, individually powerful politicians affiliated with governing parties were able to secure infrastructure investment at the expense of the core areas of ruling party strength.

A distributive policy is 'a political decision that concentrates benefits in a specific geographic constituency and finances expenditures through generalized taxation ... what distinguishes a distributive policy is that benefits are geographically targeted' (Weingast et al. 1981: 644). State aid in the EU fits nicely this definition.⁵ According to administrative practice and case law, four criteria need to be met for a measure to be considered a state aid. The measure needs to provide an, otherwise unattainable, economic advantage to the beneficiary. It must be selective. That is, it must be granted to a specific company, a group of companies, an economic sector or a geographical area.⁶ It must be state funded and, lastly, it must potentially distort competition and trade across the EU (Buts et al. 2010: 401). An aid is therefore a state funded measure that provides a selective financial benefit to social groups that are geographically concentrated and easy to identify. Although diffuse groups, such as consumers, may also benefit from such measure, taxpayers always foot the bill. Importantly, non-selective measures are not considered state aid. The criterion on the distortive effects implies that we limit our attention to measures above a given spending threshold. These state aid measures take different forms. Some may be designed to facilitate restructuring or rescuing of firms in difficulty, such as the debt-write off granted by the Slovakian government to Compel Rain (Banská Bystrica), and the soft loan given by the French government to Renault (Vendée). Common aid measures for groups of companies or economic sectors include tax exemptions and reductions for biofuel production, direct grants to shipyards and aid schemes to the audio-visual, arts and entertainment industries, such as the cultural investment funds in the Netherlands and Hungary. Equally specific are the direct grants and interest subsidies to solar energy production in Extremadura (Spain) and Piedmont (Italy). Measures for developing business and regional infrastructure are also common. Region-specific measures include interest subsidies to

⁵ Chang (2008) and Rickard (forthcoming) make a similar argument with regard to subsidies.

⁶ Article 107 of the Treaty on the Functioning of the European Union (TFEU) states that aid that may be compatible with EU law includes measures that promote the economic development of poorer areas or regions, that facilitate the development of certain economic areas and that promote the execution of important public works.

aid investment and employment in Navarre (Spain) and grants to the industrialization fund of Lorraine (France).

Employing these data on state aid in the EU offers several advantages. First, they are comprehensive in terms of coverage of transactions and economic sectors. National account statistics, for instance, are limited to cash subsidies, while they disregard loans, guarantees and other transactions. Data available from the WTO and the OECD cover only aid to industry; they are less consistent and/or limited to few years (Buigues and Sekkat 2010). Second, a government can grant a state aid only upon notification to and approval by the European Commission. Cross-national data are therefore highly comparable because a supranational bureaucracy is in charge of enforcing EU law and ensuring that these measures fall under exactly the same legal parameters.⁷ Third, with the adoption of Council Regulation 659/1999, which further codified the procedure of state aid control, information on each measure is now easily available for all EU countries.

We have collected data on the national expenditure on aid of twenty-six EU member states⁸ for the period between 1992 (or their year of accession) and 2009, available from the State aid Scoreboard - a benchmarking database developed by the Commission in July 2001. *State aid expenditure* is the expenditure on state aid to industry and services, in a given year and country, divided by the GDP.⁹ Additionally, from March 1999 onwards, an online database stores the documentation on each member state's application to grant an aid. It is therefore possible to collect information on the frequency of use of distributive measures across member states. *State aid applications* is the natural

⁷ Moreover, the policy on state aid control has consolidated and compliance has significantly improved since the eighties (Cini and McGowan 2008).

⁸ Cyprus is excluded because it has a presidential system. Semi-presidential (or mixed) systems are included because the executive depends on the support of a legislative majority to stay in office. Malta is excluded from model 3 in Table 2 because we do not have data on fiscal institutions.

⁹ The Scoreboard can be accessed at ec.europa.eu/competition/state_aid/studies_reports/expenditure.html.

logarithm of the number applications (plus one) for new state aid to industry and services, by a given country in a given year.¹⁰

Countries make extensive use of policies that employ public funds for the benefit of specific geographical constituencies. Since 1992, EU governments have spent on average 0.53 percent of their GDP on state aid – more than 2 billion euros a year. Between 1999 and 2009, they have logged 3,077 applications to grant new aid (thirteen new measures per country a year). These aggregate figures hide significant heterogeneity. Annual spending can be as low as 7 million and as high as 26 billion euros, from less than 0.05 percentage points of a country's GDP to more than 3.3 percent. Government spending in Belgium for instance, as proportion of GDP, is on average twice as much as it is in the Netherlands and about one third less than in France. Spending in Hungary is almost twice as it is in Poland. The number of applications varies from none to 82 in any given year and country. Despite the smaller size of the economy, Spanish governments on average adopt more measures than Italian ones, and Belgian governments more than Swedish ones.

Explanatory variables

For our first hypothesis, we have calculated the district magnitude for each electoral system that a country has employed in the time period of interest. In case of majoritarian and PR systems, the magnitude of the average district results from dividing the number of assembly seats by the number of electoral districts, at the electoral tier at which votes are translated into seats. For (proportional)

¹⁰ We adopted this operationalization because the estimation model can be more easily interpreted. The online database is accessible at ec.europa.eu/competition/elojade/isef/index.cfm. Data have been collected between September 2009 and January 2010. The Commission approves, sometimes conditionally, more than 90 percent of these applications. We included also those few proposed measures that were not eventually implemented because we are interested in the national propensity to grant aid. In the few cases where more than one dossier was opened with regard to same measure, we counted the first application only. Finally, both spending and applications related the 2008-9 financial crisis are excluded because those measures were designed to remedy a serious economy-wide disturbance and therefore were not geographically targeted.

multi-tier systems and mixed systems, we first computed the magnitude of each tier by dividing the number of seats assigned or won at a given tier in a given election by the number of electoral districts at that tier. We have then summed the resulting tier-level values, weighted by the proportion of tier-level seats over the assembly size.¹¹ This (weighted) average district magnitude has a lower boundary of one and an upper boundary that is a positive function of the assembly size. Since we have no reasons to expect that countries with larger assemblies are better at internalizing costs, we have normalized this measure of district magnitude by the assembly size (i.e. magnitude \times 100/assembly size). The resulting normalized values of *District magnitude* range from close to zero to 100.

Higher values of *District magnitude* should be negatively associated with state aid expenditure and applications. However, according to Carey and Shugart (1995), this should apply only if party leaders have control over the ballot. Otherwise, *District magnitude* may have a positive impact. The conditioning variable in this second hypothesis, *Ballot control*, is a dummy that takes the value of

¹¹ For instance, in the 2003 Estonian elections of the 101-seat Riigikogu, 74 seats were allocated at the 12 lower tier districts and the remaining 27 ‘compensation mandates’ were assigned at the single nation-wide district. The average district magnitude was therefore $[(74/12) \times (74/101)] + (27 \times 27/101) = 11.74$. In Sweden, there are 39 ‘adjustment’ seats allocated nation-wide and 310 assigned to 29 districts, the magnitude is $[(310/29) \times (310/349)] + (39 \times 39/349) = 13.85$. The weighting tends to increase the magnitude capturing more internalization of costs. The tier at which votes are converted into seats is crucial because this is where costs are internalized. This can have important consequences. For instance, the allocation of the PR seats in Bulgaria, Italy and Germany is based on nation-wide results. Finally, in systems where bonus seats are assigned to the largest party (for instance in Greece since 2007), we deduct the number of bonus seats from the number of seats at the lowest tier and from the assembly size. This results in a lower magnitude value, therefore capturing the majoritarian effect of bonus seats. We use data available from Golder (2005), Birch (2001), the election reports of *Electoral Studies*, and the political data yearbooks of the *European Journal of Political Research*. In bicameral systems, we use data only from the lower chamber.

one if a large majority of the members of the assembly are elected under a closed-list electoral system.¹²

For the third hypothesis, we employ the data on electoral systems and the personal vote collected by Johnson, Wallack and their colleagues (Johnson and Wallack 2007; Wallack et al. 2003). These scholars have produced average values across electoral tiers, weighted by the share of members that originate from each tier, for the three electoral rules - district magnitude aside - identified by Carey and Shugart's (1995) as shaping the incentives to cultivate a personal vote. Similarly to Edwards and Thames (2007), *Personal vote* is the average of the two scores on pool and votes. It should be positively associated with expenditure and applications, as district magnitude increases.¹³

Alternative explanations and control variables

We introduce other explanations to those suggested above. Empirical support for these alternatives relies on both broad fiscal aggregates and distributive spending, but we limit our comments to studies that focus on the latter.

First, according to the political business cycle model, a government manipulates the level of unemployment, through measures such as public investments, in order to maximize the probability of winning the next election (Nordhaus 1975: 174). We may therefore expect spending on and

¹² We follow the convention of considering single member district electoral systems as open-list (Wallack et al. 2003: 137). In these systems, candidate selection is sometimes made through primaries. Moreover, the barriers to entry for independent candidates are comparatively low. Coding them as closed-list overemphasizes the control that parties have over the ballot, compared to that of voters. It would mean coding the British system to exert the same pressure to cultivate personal votes as the post-2005 Italian closed-list system. Some multi-member district systems are closed-list but have a few single member districts (2 in Slovenia, 2 in Spain, 31 in Bulgaria in 2009 and 1 Italy from 2006). They are nevertheless coded as one because the large majority of the members (e.g. more than 87 percent in Bulgaria) are elected under closed-list.

¹³ *Personal vote* excludes the score on ballot because such information is already present, dichotomously, in *Ballot control*. It would be inappropriate to include the same attribute in two separate explanatory variables. It is worth noting however that results hold if *Personal vote* were to include the ballot score as well.

frequency of use of distributive measures to increase as a government approaches an election. With the exception of the above mentioned Chang (2008), evidence is either unresponsive (Zahariadis 1997; 2010) or points to the opposite direction (Aydin 2007). We include a variable *Election* that takes the value of one in the year preceding an election in a given country and zero in the other years. In the election year, the value is the weighted pre-election period of the year.¹⁴

Second, we relax the assumption of office-seeking parties. The partisan theory of macroeconomic policy for instance puts emphasis on the policy-seeking motivations of politicians. Because of the consequences of economic outcomes on key constituencies, left-wing governments are more inclined to pursue expansionary fiscal policies than right-wing governments (Hibbs 1977). We may therefore expect spending on distributive measures to increase as government composition moves to the left (there is no reason for fragmentation to be affected though). Empirical results remain inconclusive however. What appears to be supporting evidence in the 1980s seems reversed in the 1990s (Blais 1986; Zahariadis 1997). Zahariadis (2010) finds that right-wing governments spend more on aid. Relatedly, Cao et al. (2007) show a left-wing governments' preference for industrial subsidies over social welfare at low to moderate increases in imports. In other works, the impact of partisanship on distributive spending seems to be conditioned by the trade openness of a country. Left-labor power leads to *lower* government spending on subsidies to industry when trade openness is low and to *higher* spending when openness is high (Clark 2002: 60-66; Garrett 1998: 80-84). These results suffer from two shortcomings. First, Garrett and Clark employ national account statistics on subsidies to industry. These data include only cash transfers and ignore other important instruments, such as soft loans and guarantees (Buigues and Sekkat 2010). Second, their composite

¹⁴ As in Franzese (2002: 78), it equals to $(\text{number of completed pre-election months}/12) + (\text{number of pre-election days in the incomplete month}/\text{total number of days in the incomplete month})/12$. This formula is also used when any other explanatory variable changes during a given year. The value in this year is the time-weighted sum of pre-change and post-change values.

left-labor power index aggregates six distinct attributes¹⁵ and it is unclear which one actually matters.

This policy is underpinned by the traditional left-right economic cleavage, pitting market liberals against interventionists - those favoring a small state and low taxation against the supporters of public spending and intervention in the economy. To measure the positions of governments, we employ the “taxes v. spending” dimension used by Laver and Hunt (1992) and Benoit and Laver (2006) in their expert surveys on party positions. The dimension ranges from 1 for a party that promotes raising taxes to increase public services to 20 for one that promotes cutting public services to cut taxes. *Government preference* is the sum of the positions of each government party along this dimension, weighted by its share of ministerial portfolios. Accordingly, it may be negatively associated with spending (but it is unrelated to policy fragmentation).

Third, we look *inside* executive policymaking of parliamentary democracies. According to Hallerberg et al. (2009), we should expect greater fiscal laxity - more spending on distributive measures - if specific types of parliamentary governments operate under specific types of fiscal institutions; more specifically, if homogeneous coalition or single party governments operate under more ‘contract’ than ‘delegation’ fiscal institutions and if heterogeneous coalition governments operate under more ‘delegation’ than ‘contract’ fiscal institutions (fragmentation should be unaffected). Delegation fiscal institutions consist of a set of rules underpinning a centralized budgeting process where the minister of finance has agenda setting and monitoring power over the spending ministers. Under contract fiscal institutions, the budget is negotiated among government parties (or spending ministers) and the minister of finance plays a secondary role. We measure conflict within the executive - *Government range* - as the absolute difference among the extreme positions of government parties along the “taxes v. spending” dimension in Laver and Hunt (1992)

¹⁵ Such as, for instance, a seat-weighted left-right indicator of the legislature and the share of unionized workers who are member of the largest labor confederation.

and Benoit and Laver (2006).¹⁶ *Fiscal institutions* is instead the net aggregate score for delegation and contract institutions for each country and time period, provided by Hallerberg et al. (2009) and, for eastern European member states, by Hallerberg and Yläoutinen (2010).¹⁷ The theory suggests that these variables should interact positively.

Finally, as most studies on government spending, we control for the lagged values of the per capita GDP, the real GDP growth rate and the trade openness (the sum of imports and exports over GDP). Moreover, the consequences of a loose fiscal policy differ depending on the exchange rate regime a government adopts. According to the Mundell-Fleming model, under full capital mobility, government spending increases output if a country operates under a fixed exchange rate regime, but it has no effect under a flexible regime.¹⁸ We include therefore a dummy variable that takes the value of one for the time periods a country has joined the Economic and Monetary Union, and zero otherwise. Finally, we add a control variable for the time periods following the adoption of Regulation 659/1999, *de facto* a time fixed effect.

¹⁶ For minority governments, we computed the government range on the basis of the parliamentary support coalition. In addition to government parties, they include either those which supported the formal government investiture, those which offered external support or, as a last resort, those closest to the government parties on the economic left-right scale. Information on government composition, portfolios, parliamentary seats, external support and vote of investiture is taken from the political data yearbooks of the *European Journal of Political Research*.

¹⁷ The index for delegation fiscal institutions covers four legislative-executive features of the budgetary process. The index for contract fiscal institutions measures the extent to which a country uses multiannual budget plans. This latter measure is called ‘targets’ by Hallerberg et al. (2009) and ‘contracts 1’ by Hallerberg and Yläoutinen (2010). A more encompassing measure for contracts, employed by Hallerberg et al. (2009), shares some of the attributes with the delegation index and would not discriminate enough between the two. Results hold if we use two separate indices for delegation and contact institutions, both interacted with *Government range*.

¹⁸ No doubt, the growth and stability pact has been designed to prevent exactly this outcome, but it remains to be seen how effective it has been.

Given its novelty, we have less guidance in the choice of control variables for the analysis on applications. We do not find any compelling reason to expect that the above mentioned control variables should affect policy fragmentation as well. We suspect however that governments of larger and, given our focus on industry and services, more industrialized economies may adopt more measures. We therefore include the lagged values of the (log of) GDP and the agriculture value added as share of the GDP.¹⁹ Table 1 lists the descriptive statistics of the variables we employ in the analysis.

< TABLE 1 HERE >

Results

Electoral institutions and expenditure on distributive measures

The nature of our datasets is time-series cross-sectional, with the latter property becoming predominant over time. The number of countries begins with twelve in 1992 and increases to twenty-five (twenty-six in the applications dataset) as new states joined the EU. In preliminary analyses of the expenditure dataset, the Breusch-Pagan test indicates heteroskedasticity. We therefore employ panel corrected standard errors which perform well when the number of years and units are similar. Since we find evidence of serial correlation, we control also for first-order autoregressive correlation with a common coefficient across countries. Finally, we include country fixed effects to deal with unobserved unit heterogeneity (Beck and Katz 1995; 2004; Wilson and Butler 2007).²⁰

< TABLE 2 HERE >

¹⁹ In a preliminary analysis, this variable was also included in the spending models but it was dropped as it had no significant impact. A possible additional control variable, the unitary-federal structure of a country, is largely time invariant and cannot be included in our model specification with country fixed effects.

²⁰ Hausman tests comparing fixed- with random-effects models indicate that only the fixed-effect estimator is consistent.

Table 2 presents the results of three Prais-Winsten regressions. Model 1 is a test of Persson and Tabellini's (1999; 2000) expectation. We find for the first time direct evidence that countries where legislators are elected in higher magnitude districts spend less on distributive measures, most likely because politicians internalize the costs of such measures. A standard deviation increase in normalized *District magnitude*, equivalent to the difference between the French and the German electoral systems, accounts for a 0.09 decrease in the percentage of state aid expenditure over GDP. This is comparable to a reduction in spending of more than 214 million euros per year for the average European economy, about a tenth of the mean expenditure on these measures.

More interestingly, the dampening effect on spending of a higher district magnitude is reinforced if legislators operate in an environment where other institutional features conjointly discourage cultivating a personal vote. Models 2 and 3 in Table 2 provide a test of Carey and Shugart's (1995) expectations. The bottom part of the table lists the average marginal effects of *District magnitude*, conditional on whether party leaders have control over the ballot.²¹ Where such control is exercised, a standard deviation increase in *District magnitude* accounts for a 0.20 decrease in the percentage of expenditure over GDP, more than 480 million euros a year. Italy switched from a *de facto* open to a closed-list system in 2005. Had it adopted the current system in the early nineties, even without modifying the district magnitude, mean annual spending on distributive measures would have been almost 740 million euros lower.

District magnitude is instead irrelevant when ballot control is lacking. This result is not exactly as Carey and Shugart expect; the coefficients in the last row of Table 2 have not turned positive. In open-list systems, higher magnitude does not seem to intensify intraparty competition and increase the provision of particularistic benefits. In sum, ballot control is a necessary condition for district magnitude to have a constraining effect on distributive spending.

²¹ In the spirit of Brambor et al. (2006), we employ STATA postestimation command *margins* to compute these values as well as to draw Figure 1.

Consider now the other two electoral institutions. Recall that our third hypothesis states that spending should increase with district magnitude in the presence of electoral rules (pool and votes) that strengthen the incentives to cultivate a personal vote. The top and bottom panels of Figure 1 display the marginal effects of district magnitude on spending, as *Personal vote* varies across the potential range of values and in case of ballot control. The other variables are set at their means, while the gray area covers the 95% confidence interval. The middle panel displays the kernel density of *Personal vote*. Note that this index does not exceed 0.52 in case of ballot control. Equivalently, it never goes below 0.49 in case of no ballot control (chart not shown). Ballot, pool and votes have therefore a tendency to go hand in hand. Unsurprisingly, stronger control tends to be associated with more pooling and single party voting (i.e., lower values of *Personal vote*), while weaker control with less pooling and multiple or candidate-level voting (i.e., higher values).

< FIGURE 1 HERE >

Figure 1 shows that in electoral systems where these three rules conjointly dampen the incentives to cultivate a personal vote (i.e., ballot control and low *Personal vote* values), an increase in district magnitude lowers spending. For instance, both the post-2005 Italian and the Portuguese electoral systems exhibit low values for *Personal vote*, but the Italian system has a much higher magnitude. As a share of GDP, distributive spending in Italy is less than a third of Portuguese spending.

The marginal effects become insignificantly different from zero as *Personal vote* increases, but this is substantively meaningless because we do not observe these high values. We do not find however evidence for the symmetric expectation - our third hypotheses, as originally formulated by Carey and Shugart (1995). At high values of *Personal vote* (observationally associated with no ballot control), spending does not increase with district magnitude.

These results are robust controlling for the political business cycle and government ideology as well as for the interaction between conflict within the government and fiscal institutions (see model 3 of Table 2). Closeness to elections and left- (or right-) wing governments are not associated with more spending. We also do not find evidence of more spending when homogeneous governments operate

under more contract than delegation fiscal institutions and when heterogeneous ones operate under the opposite scenario. Only two other factors appear to have an impact. As countries get richer, they spend slightly more on distributive measures (0.08 percentage points of spending over GDP). A country like France would spend about 200 million euros more across the entire period. Perhaps surprisingly, joining the Economic and Monetary Union leads a country to cut spending on distributive measures, by approximately 0.43 percentage points (one billion euros). The fiscal framework underpinning the single European currency appears to undercut the Mundell-Fleming incentives to run looser fiscal policies.

In sum, electoral institutions have greater impact on spending on distributive measures when they reinforce each other in dampening the incentives to cultivate a personal vote. Where the district magnitude is high, party leaders control access to the ballot rank and voting is party-based and pooled, legislators face low pressure to deliver particularistic benefits and spend less on distributive measures. Results are instead inconclusive when electoral institutions do not reinforce each other or jointly strengthen such incentives. Higher magnitude does not lead to less spending *if* party leaders do not control ballot access. It does not lead to more spending when voting is multiple or candidate-based, and it is not pooled.²²

²² Although a state aid measure is selective, it follows either industry-specific ('sectoral') or general ('horizontal') rules (Cini and McGowan 2008: 189). The latter type, the most common and preferred by the Commission, covers objectives such as regional development, environment protection, rescue and restructuring of firms in difficulty, research and development, and training. Results are the same if we employ this type of state aid spending only. Results remain robust, but somewhat weaker, under the alternative dynamics of a lagged dependent variable and if we exclude countries of the 2004 and 2007 enlargements. They strengthen with alternative specifications, such as those interacting left ideology with trade openness (Clark 2002; Garrett 1998), increases in imports (Cao et al. 2007) or elections. To consider Chang's (2008) results, we would need to add *Election* as a third component to the two interactions plus an interaction between *Election* and *Government range*. Fortunately, there is no need for this because Britain is the only country in our dataset with a single veto player and single member districts. Chang's (2008) results are controlled for with the country fixed effects.

Electoral institutions and fragmentation of distributive measures

Table 3 presents the results of two OLS regressions on the number of applications. Model 1 includes only district magnitude. It does not lend support to our first hypothesis derived from Persson and Tabellini (1999; 2000). Higher magnitude *per se* does not reduce policy fragmentation.

< TABLE 3 HERE >

Model 2 includes all the electoral variables (we do not expect ideology or fiscal institutions to affect fragmentation). The marginal effects at the bottom of Table 3 offer strong corroborating evidence in favor of the second expectation of Carey and Shugart (1995). District magnitude has a significant impact on policy fragmentation, conditional on whether party leaders have control over the ballot. In close-list systems, politicians that operate in higher magnitude contexts tend to adopt fewer measures - about one less measure every year if magnitude increases by a standard deviation. Both the Spanish and the post-2005 Italian electoral systems are closed-list, but the latter displays significantly higher district magnitude. Despite the larger economy, Italian governments have applied for less than twenty-seven measures a year in this period, Spanish authorities for more than thirty-four a year.

On the other hand, if party leaders cannot control the ballot rank, a standard deviation increase in magnitude leads to higher policy fragmentation, twelve more measures every year. This is no small amount, considering that the typical country adopts on average about fourteen measures a year. Both Slovakia and the Czech Republic operate a PR system with preferential vote, but Slovakia has a single national district while the Czech Republic fourteen districts. Slovakian governments have implemented on average as many measures as the Czech authorities, despite an economy which is half the size of their neighbor. Slovak politicians most probably face stronger intraparty competition.

As far as the third hypothesis is concerned, when electoral institutions dampen the incentives to deliver particularistic benefits (i.e. in case ballot control and low *Personal vote* values), fewer measures are adopted as district magnitude increases. This result captures the differences between

the Spanish, pre-2003 Belgian and Slovenian systems, all *de facto* closed-list and with below average values of *Personal vote*. The number of measures adopted by these countries is not solely explained by the size of their economies but probably also by the significantly increasing district magnitude from the former to the latter electoral system. This effect loses significance however when the *Personal vote* index approaches zero (i.e. below 0.14). Finally, as in the spending case, policy fragmentation does not increase with district magnitude at high values of *Personal vote* (observationally associated with no ballot control). It is also unaffected by the proximity of elections, while it decreases in larger economies.

In sum, electoral institutions affect also the number of distributive measures countries adopt, especially when they reinforce each other in dampening the incentives to pursue particularistic policies. As Carey and Shugart (1995) expects, the rules governing control over the ballot rank operate as a switching mechanism. If party leaders have control, an increase in district magnitude reduces the number of measures. This seems to be somewhat reinforced when the other rules, voting and pooling, also discourage pursuing particularistic policies. On the other hand, if leaders do not enjoy such control, higher district magnitude increases intraparty competition and leads of the adoption of *more* distributive measures.

Conclusion

Prominent political-economy theories suggest that electoral institutions should matter for distributive policy outcomes, but the empirical record is far from overwhelming. We have argued that the difficulty in finding appropriate data as well as a rough operationalization of a key explanatory factor - district magnitude - may be the causes of this paucity. Using comparable information on expenditure and reliance on distributive measures across EU countries, we show that district magnitude has indeed a dampening effect on distributive spending. More interestingly, we have argued that this institution should work alongside other rules that affect as well the incentives to cultivate a personal vote. Indeed, we show that, if high magnitude is combined with ballot control, party-based voting and pooling, politicians tend to spend and rely less on distributive

measures. Where high magnitude is combined with a lack of control, we find instead greater reliance on these measures. Future theoretical developments need therefore to take more seriously into account how different combinations of rules structure incentives.

These findings have important implications for the design of institutions and its policy consequences. In Italy, signatures have been collected to repeal the bonus-adjusted PR system and reinstall the earlier system, predominantly based on single-member district plurality. Aside from the specific reasons behind this campaign, it is worth noting that distributive spending has dropped from 0.70 to 0.31 percent of GDP since 2005 - a reduction of 1.5 billion euros a year, while the mean number of applications to grant aid has decreased from thirty-three to less than twenty-seven a year. This outcome can be plausibly linked to the higher magnitude and closed-list rule of the current system.

Moreover, several institutions matter. In 2003, the Belgian parliament repealed the upper tier and halved the number of districts of its PR system. It also reduced significantly the leeway for party leaders to shape the ballot rank. The mean number of measures fell from eleven to seven a year and spending decreased from 0.46 to 0.31 percent of GDP. In absolute terms, spending however *increased* by 90 million euros a year. These weaker effects can be explained by a reform that associated higher magnitude with diminished ballot control. The former dampened the incentives to cultivate a personal vote, whereas the latter strengthened them. This underscores how central it is, for understanding the policy consequences of institutional design, appraising all the effects, reinforcing or compensating as they may be, of each of the variegated set of rules that make up an electoral system.

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TABLE 1. Descriptive statistics

Variable	Description	Mean	St.dev.	Min	Max
<i>State aid expenditure</i>	Expenditure on aid to industry and services over GDP	0.53	0.45	0.05	3.36
<i>State aid applications</i>	ln(number of applications + 1)	2.19	1.02	0.00	4.42
Electoral institutions					
<i>District magnitude</i>	Tier-weighted average district magnitude, normalized by assembly size	16.57	27.99	0.15	100.00
<i>Ballot control</i>	1 if a majority of assembly members are elected under a closed-list electoral system, 0 otherwise	0.19	0.39	0	1
<i>Personal vote</i>	Incentives to cultivate personal votes. Average of scores on pool and votes	0.67	0.39	0.00	1.50
<i>Election</i>	1 in preelection year, time-weighted share of 1 in election year, 0 otherwise	0.39	0.44	0.00	1.00
Political control variables					
<i>Government preference</i>	Portfolio-weighted position of government parties on taxes-spending dimension	11.22	2.77	5.80	17.21
<i>Government range</i>	Absolute difference between extreme positions of government parties on taxes-spending dimension	4.72	3.71	0.00	12.59
<i>Fiscal institutions</i>	Aggregate score for delegation fiscal institutions - aggregate score for contract fiscal institutions	-0.13	0.25	-0.56	0.71
Other control variables					
<i>GDP per capita</i>	GPD per capita (thousands), at t-1	26.15	15.14	4.10	119.14
<i>GDP growth rate</i>	Real GDP growth rate, at t-1	3.28	2.61	-5.13	12.23
<i>Trade openness</i>	Sum of import and export over GDP, at t-1	99.28	54.22	35.39	326.76
<i>Eurozone</i>	1 if a country is member of EMU, 0 otherwise	0.42	0.49	0	1
<i>Regulation</i>	1 for time periods following adoption of Regulation 659/1999, 0 otherwise	0.69	0.45	0	1
<i>GDP</i>	ln(GDP), at t-1	26.13	1.53	22.33	28.93
<i>Agriculture added value</i>	Agriculture value added as share of GDP, at t-1	2.76	1.58	0.36	10.51

TABLE 2. Electoral institutions and expenditure on distributive measures, EU countries 1992-2009

	(1)	(2)	(3)
<i>District magnitude</i>	-0.319** (0.161)	-2.216 (3.018)	-1.318 (3.150)
<i>Ballot control</i>		-20.83 (26.26)	-15.52 (27.33)
<i>District magnitude</i> × <i>Ballot control</i>		1.597 (3.065)	0.701 (3.211)
<i>Personal vote</i>		-33.18*** (12.88)	-33.69** (13.66)
<i>District magnitude</i> × <i>Personal vote</i>		-0.351 (0.379)	-0.355 (0.433)
<i>Election</i>		1.293 (3.000)	2.294 (2.843)
<i>Government range</i>			0.0818 (1.120)
<i>Fiscal institutions</i>			-26.96 (23.58)
<i>Government range</i> × <i>Fiscal institutions</i>			3.136 (3.104)
<i>Government preference</i>	1.173 (0.762)	1.204 (0.769)	1.115 (0.733)
<i>GDP per capita</i> _{t-1}	0.364 (0.228)	0.365 (0.231)	0.431* (0.231)
<i>GDP growth rate</i> _{t-1}	0.234 (0.591)	0.223 (0.583)	0.655 (0.554)
<i>Trade openness</i> _{t-1}	-0.202 (0.156)	-0.201 (0.154)	-0.252 (0.189)
<i>Eurozone</i>	-42.52*** (13.05)	-42.68*** (13.02)	-39.49*** (11.76)
<i>Regulation</i>	19.93* (11.37)	20.38* (11.30)	14.96 (9.583)
Constant	44.65*** (13.37)	68.07*** (19.12)	65.70*** (19.29)
Observations	321	321	315
R-squared	0.572	0.579	0.461
Number of groups	26	26	25
Wald chi2	62684	21892	3645
Rho	0.390	0.385	0.348
<i>Average marginal effects of district magnitude[†]</i>			
<i>District magnitude</i> / <i>Ballot control</i>		-0.722*** (0.257)	-0.722*** (0.266)
<i>District magnitude</i> / <i>No ballot control</i>		-2.483 (2.966)	-1.589 (3.090)

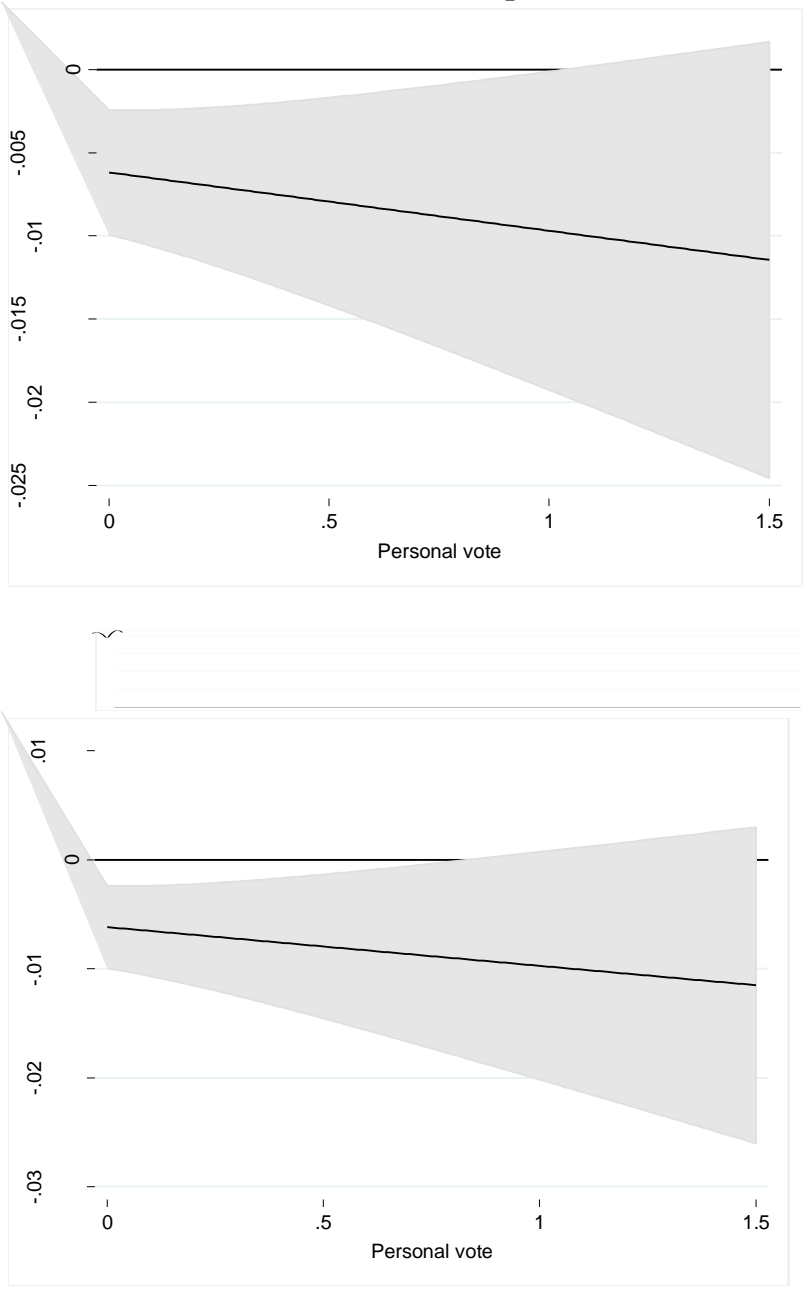
Note: Prais-Winsten regressions with panel corrected standard errors and country fixed effects. Pairwise selection and common AR1 error correction. Dependent variable: *State aid expenditure*. Coefficients and standard errors, in parentheses, multiplied by 100. *** p<0.01, ** p<0.05, * p<0.1. [†] The other variables are set at the means of the subsets with and without ballot control.

**TABLE 3: Electoral institutions and frequency of distributive measures,
EU countries 1999-2009**

	(1)	(2)
<i>District magnitude</i>	-0.0502 (0.140)	13.18*** (4.998)
<i>Ballot control</i>		129.1** (57.52)
<i>District magnitude</i> × <i>Ballot control</i>		-13.39** (5.317)
<i>Personal vote</i>		65.55 (45.88)
<i>District magnitude</i> × <i>Personal vote</i>		-5.590*** (1.571)
<i>Election</i>		4.742 (9.016)
<i>GDP</i> _{<i>t-1</i>}	-61.59** (25.37)	-60.21** (26.52)
<i>Agriculture added value</i> _{<i>t-1</i>}	-3.103 (12.90)	-2.487 (14.22)
Constant	1,876*** (685.3)	1,761** (734.0)
Observations	225	225
R-squared	0.786	0.790
Number of groups	26	26
Wald chi2	278.7	227.6
<i>Average marginal effects of district magnitude[†]</i>		
<i>District magnitude</i> / <i>Ballot control</i>		-1.858** (0.758)
<i>District magnitude</i> / <i>No ballot control</i>		8.916** (4.434)

Note: OLS regressions with panel corrected standard errors and country fixed effects. Unbalanced panel. Dependent variable: *State aid applications*. Coefficients and standard errors, in parentheses, multiplied by 100. *** p<0.01, ** p<0.05, * p<0.1.
[†]The other variables are set at the means of the subsets with and without ballot control.

FIGURE 1. Expenditure and electoral institutions



Note: Model 2 (upper panel) and 3 (lower panel) with ballot control. Kernel density of personal vote in case of ballot control (middle panel).

TABLE A: Further tests, spending

	'Horizontal rules' aid	Lagged DV	Ideology-trade interaction	Ideology-election interaction	Pre-2004 countries
<i>District magnitude</i>	-2.199 (2.513)	-1.737 (2.147)	-1.285 (3.178)	-1.628 (3.210)	9.717 (7.109)
<i>Ballot control</i>	-20.36 (21.41)	-18.16 (19.04)	-15.04 (27.50)	-17.97 (27.86)	63.11 (44.18)
<i>District magnitude</i> × <i>Ballot control</i>	1.709 (2.585)	1.331 (2.192)	0.670 (3.238)	0.997 (3.276)	-10.84 (7.081)
<i>Personal vote</i>	-23.63** (10.84)	-21.89 (13.74)	-32.99** (13.61)	-34.19** (13.75)	15.82 (78.09)
<i>District magnitude</i> × <i>Personal vote</i>	-0.0969 (0.312)	-0.222 (0.372)	-0.388 (0.439)	-0.350 (0.475)	-14.56 (12.20)
<i>Election</i>	0.495 (1.335)	2.272 (3.002)	2.340 (2.862)	11.89 (12.07)	3.291 (2.775)
<i>Government range</i>	-0.216 (0.391)	-0.00385 (0.906)	0.0313 (1.111)	0.0466 (1.120)	0.0619 (1.134)
<i>Fiscal institutions</i>	-7.042 (5.534)	-12.54 (19.90)	-24.81 (26.25)	-26.25 (23.55)	-21.51 (24.65)
<i>Government range</i> × <i>Fiscal institutions</i>	0.247 (0.784)	1.571 (2.589)	2.866 (3.294)	3.024 (3.104)	2.392 (3.326)
<i>Government preference</i>	0.430 (0.360)	1.302** (0.542)	0.642 (1.346)	1.481* (0.874)	1.225* (0.709)
<i>Government preference</i> × <i>Trade openness</i>			0.00664 (0.0171)		
<i>Government preference</i> × <i>Election</i>				-0.860 (1.053)	
<i>GDP per capita</i> _{t-1}	0.465*** (0.151)	0.322** (0.147)	0.451* (0.232)	0.444* (0.232)	0.383 (0.234)
<i>GDP growth rate</i> _{t-1}	0.555* (0.325)	0.769 (0.492)	0.685 (0.560)	0.649 (0.549)	0.815 (0.780)
<i>Trade openness</i> _{t-1}	-0.0977 (0.120)	-0.0641 (0.164)	-0.350 (0.296)	-0.258 (0.189)	-0.187 (0.192)
<i>Eurozone</i>	-16.92*** (5.558)	-36.39*** (9.319)	-39.11*** (11.77)	-39.46*** (11.76)	-24.32*** (7.793)
<i>Regulation</i>	8.081	15.34**	14.65	15.08	3.468

Constant	(5.612) 45.89***	(7.647) 38.81**	(9.583) 72.19***	(9.524) 63.52***	(5.199) 16.31
Observations	(12.93) 315	(15.18) 300	(23.43) 315	(19.60) 315	(41.02) 261
R-squared	25	25	25	25	15
Number of groups	0.450	0.614	0.460	0.464	0.425
Wald chi2	2263	5384	35432	8194	1052
Rho	0.478	0.478	0.351	0.344	0.392
<i>Average marginal effects of district magnitude[†]</i>					
<i>District magnitude /</i> <i>Ballot control</i>	-0.518** (0.213)	-0.472* (0.252)	-0.729*** (0.264)	-0.733*** (0.271)	-5.294 [‡] (3.394)
<i>District magnitude /</i> <i>No ballot control</i>	-2.273 (2.454)	-1.906 (2.072)	-1.581 (3.108)	-1.894 (3.147)	-1.511 (4.778)

Note: Prais-Winsten regressions (except for the lagged DV model) with panel corrected standard errors and country fixed effects. Pairwise selection and common AR1 error correction (except for the lagged DV model). Dependent variable: *State aid expenditure*. Coefficients and standard errors, in parentheses, multiplied by 100. *** p<0.01, ** p<0.05, * p<0.1, [‡] p=0.11. [†] The other variables are set at the means of the subsets with and without ballot control.

TABLE B: Further tests, applications

	Ideology-election interaction
<i>District magnitude</i>	14.49*** (4.668)
<i>Ballot control</i>	139.9** (55.34)
<i>District magnitude</i> × <i>Ballot control</i>	-14.69*** (4.960)
<i>Personal vote</i>	66.99 (43.76)
<i>District magnitude</i> × <i>Personal vote</i>	-5.543*** (1.391)
<i>Election</i>	-35.58 (30.95)
<i>Government preference</i>	-1.895 (1.849)
<i>Government preference</i> × <i>Election</i>	3.532 (2.661)
<i>GDP</i> _{<i>t-1</i>}	-61.61** (26.46)
<i>Agriculture added value</i> _{<i>t-1</i>}	-2.611 (14.20)
Constant	1,813** (727.0)
Observations	225
R-squared	26
Number of groups	0.791
Wald chi2	845.1
<i>Average marginal effects of district magnitude</i> [†]	
<i>District magnitude</i> <i>Ballot control</i>	-1.823*** (0.710)
<i>District magnitude</i> <i>No ballot control</i>	10.265** (4.053)

Note: OLS regressions with panel corrected standard errors and country fixed effects. Unbalanced panel. Dependent variable: *State aid applications*. Coefficients and standard errors, in parentheses, multiplied by 100. *** p<0.01, ** p<0.05, * p<0.1. [†] The other variables are set at the means of the subsets with and without ballot control.