

Distributive Policies in Parliamentary Systems: The Interplay between Electoral and Fiscal Institutions¹

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Abstract: Why do some countries spend more on and make more use of distributive measures than others? Which institutions systematically shape spending on and frequency of use of these measures? Cross-country longitudinal tests of theories that emphasize the institutional determinants of distributive policies are rare. We test a battery of competing hypotheses on spending and frequency using data on state aid expenditure by EU member states between 1992 and 2009 and on applications logged by such states between 1999 and 2009. We find that higher district magnitude lowers spending on and use of distributive measures if party leaders control access to the ballot rank. More measures are adopted if an increase in magnitude is associated with lack of ballot control. In case of single party majority or cohesive coalition governments, greater reliance on delegation rather than contract fiscal institutions lowers spending.

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Why do some countries spend more on and make more use of distributive measures than others? Which institutions systematically shape spending on and frequency of use of these measures? According to several scholars, electoral and fiscal institutions should systematically affect the propensity of a country to rely and spend on this type of policies (e.g. Carey and Shugart 1995; Hallerberg, Strauch, and von Hagen 2009; Persson and Tabellini 1999; 2000), but cross-country longitudinal studies are rare. Additionally, to our knowledge, no work exists that tries to explain their frequency of use. This is unfortunate because, besides social welfare or economic efficiency, partisan-political factors do matter, as several country studies show (see the review in Golden and Picci 2008: 268). These works however can hardly subject to test theories that emphasize institutional determinants, simply because a country's institutions tend to remain invariant or change very slowly over time.

A primary cause of this paucity of research resides on the difficulty in finding comparable cross-country data on measures such as public infrastructural investments or intergovernmental grants. According to Weingast, Shepsle, and Johnsen (1981: 644), 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'. In this article, we argue that the policy on state aid control of the European Union (EU) offer comparable country-level data, across several countries and several years, that fit this definition. We test a battery of competing hypotheses on spending on and frequency of use of distributive measures employing 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. We show that the interaction between two electoral institutions, district magnitude and ballot control, affects both spending and frequency of use. We also show that, under given circumstances, greater reliance on delegation rather than contract fiscal institutions lowers spending.

In the next section, we employ the political-economy literature on the determinants of fiscal policy, especially of distributive fiscal measures, to develop a series of hypotheses on spending and frequency. Next, we present the data we have collected and the operationalization of the variables. The last section discusses the results and draws some conclusions.

The Politics of Distribution in Parliamentary Systems

We employ four sets of theories that put emphasis on, respectively, electoral rewards, types of government, fiscal institutions, and government ideology. Empirical tests of these theories predominately focus on broad fiscal aggregates rather than on the narrower spending on distributive measures.

Elections and electoral institutions

First and foremost, politicians want to be reelected. This was the fundamental intuition behind Nordhaus' (1975) political business cycle model. A government manipulates the level of unemployment, through economic policies such as public investments, in order to maximize the probability of winning the next election (Nordhaus 1975: 174). In the context of a short-run trade-off between inflation and employment and with backward-looking and short-sighted voters, a purely office-seeking government will pursue an expansionary fiscal policy as it approaches a new election, followed by a contraction immediately after.² Straightforwardly, we should therefore expect that governments will more likely adopt distributive measures as a new election is approaching. In other words,

Hypothesis 1: *As a government approaches an election, spending on and frequency of use of distributive measures increase.*

² This is likely to occur even if voters are not short-sighted, but as long as they are imperfectly informed about government performance (see e.g. Cukierman and Meltzer 1986).

The earlier empirical record of the political business cycle is not particularly strong (see, e.g., Alesina, Mirrlees, and Neumann 1989; Alesina, Roubini, and Cohen 1997), but more recent research confirms this expectation, in some cases conditional on a fixed exchange rate regime (Clark 2002; Clark and Hallerberg 2000; Franzese 2002).³ Rare as it is to come by, cross-country evidence on the electoral effect on the adoption of distributive measures however points to the opposite direction. Aydin (2007) employs data collected by an ad-hoc OECD project on public support to industry between 1989 and 1995, and shows that proximity to elections lowers the level of subsidies.

One reason for this unsatisfactory empirical support can be found in how electoral rules structure the choice among different *types* of spending. Faced with an industrial crisis, a government may decide to grant a state aid to a company in distress, a typical distributive measure designed to benefit a specific geographic constituency and financed through generalized taxation. But the government may also opt for a measure of general economic interest, such as increasing unemployment benefits. Aside from the impact on broad fiscal aggregates, this choice has a direct bearing on our object of study, the intensity and frequency of distributive measures.

According to Weingast, Shepsle and Johnsen (1981), the incentives of politicians for choosing a distributive measure increase with the number of electoral districts. Because the benefits of such measure are concentrated while its costs are shared across all the districts through the mechanism of generalized taxation, the higher the number of districts, the higher the district-specific net benefits of such measure. Politicians seeking re-election in any given district will therefore demand more distributive measures, giving rise to a common-pool problem. Weingast, Shepsle and Johnsen (1981) assume that legislators adopt measures unanimously, but Persson and Tabellini (1999; 2000) show that

³ Franzese (2002) is interested in government's transfers (e.g. social security, pensions, and welfare) over GDP, while Clark (2002) and Clark and Hallerberg (2000) in the broader ratio of gross debt to GDP. For single country evidence of electoral effects on geographically targeted government spending see the literature cited by Golden and Picci (2008: 268).

the common pool problem arises also if the assembly decides by majority voting.⁴ They model two pure office-seeking parties that offer a policy platform composed of a group-specific transfer payment (i.e., a distributive measure) and a public good.⁵ For concreteness and with reference to the example above, they model the choice between a direct grant to a company in distress and an increase in unemployment benefits. Persson and Tabellini (1999; 2000) show that the size of the distributive transfer is higher, and the provision of the public good is lower, in a polity with three districts (where groups coincide with districts) than in one with a single district. Because the votes lost in non-marginal districts, following a lower provision of the public good, are not internalized by a party, while each district still contribute equally to the budget, the costs of a distributive program is lower in a three-district than in a single-district polity.⁶ In sum, some institutional features of the electoral system, such as its district magnitude, may discourage the internalization of costs and, therefore, shape the choice between different types of fiscal measures. Additionally, because distributive measures are geographically (district) targeted, we should also expect greater fragmentation. We formulate the following expectation

Hypothesis 2: The more the electoral system encourages the internalization of costs (the higher the district magnitude), the lower the spending on and frequency of use of distributive measures.

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 proportional electoral representation. However, no direct

⁴ Majority voting in the legislature is implied by Persson and Tabellini (1999). In a three-district polity, a party needs to win in two districts to be in government.

⁵ The platform includes also rents for politicians and a common tax rate.

⁶ For an informal, but similar, argument see Lancaster (1986). Lizzeri and Persico (2001) and Milesi-Ferretti, Perotti and Rostagno (2002) produce models with similar conclusions, at least to the extent that we should expect more spending on policies with geographically concentrated benefits as the number of electoral districts increases.

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, Perotti and Rostagno (2002), for instance, fail to find corroborating evidence.⁷

This still weak empirical record may be due to other institutional features of the electoral system that condition the preferences of politicians for distributive measures. 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: 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 than deliver district-specific benefits (e.g. Carey and Shugart 1995; Lancaster 1986). Carey and Shugart (1995) identify four institutional features that shape the incentive to cultivate a personal vote: 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, increases with weaker ballot control, less pooling and

⁷ More specifically, Milesi-Ferretti, Perotti and Rostagno (2002) do not find evidence that countries with low district magnitude electoral systems spend more on (geographically targeted) purchases of goods and services. They do instead find that these countries spend less on transfers, which are not geographically targeted. Total government spending is also lower as expected, but only in the subset of OECD countries. Note that these scholars include subsidies to firms in the transfers. In our view, subsidies have a strong geographical component. In single country studies, there is evidence instead of targeting spending to swing voters in marginal districts (e.g. Dahlberg and Johansson 2002; John and Ward 2001).

multiple or candidate-level voting, more so as district magnitude increases. Additionally, we should expect these features to increase the fragmentation of distributive measures.

Importantly, according to Carey and Shugart (1995), the degree of leaders' control over the ballot conditions the impact of the district magnitude on these incentives. If leaders have strong control over the ballot (e.g. in closed-list systems), the district magnitude operates in the same way as suggested by Persson and Tabellini (1999; 2000). But if they have weak control, the higher the district magnitude is, the higher the incentives to cultivate a personal vote, exactly the opposite of Persson and Tabellini's expectation. We can equally expect fragmentation to increase. We formulate these expectations as follows.

Hypothesis 3: Hypothesis 2 applies only if party leaders have control over the ballot. Otherwise, as district magnitude increases, the higher the spending on and frequency of use of distributive measures.

Hypothesis 4: As the electoral system strengthens the incentives to cultivate a personal vote, the higher the district magnitude, the higher the spending on and frequency of use of distributive measures.

Edwards and Thames (2007) find evidence that total and education expenditures increase with district magnitude in systems with low incentives to cultivate the personal vote, while they decrease in systems with high incentives.⁸ While the latter result disconfirms Persson and Tabellini's (public good-related) expectation, it does not necessarily confirm Carey and Shugart's hypotheses, for two reasons. First, Edwards and Thames do not test the impact of these electoral institutions on 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 conditioning impact of ballot control alone - hypothesis 3 above -, which is one of Carey and Shugart's (1995: 431) core expectations.

⁸ Golden and Picci (2008) show that, in the open-list proportional representation (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.

Types of government

A government in a parliamentary system needs the support from a legislative majority to stay in office. Parties offer the disciplining and coordinating mechanisms to form, sustain and dissolve the parliamentary support coalition for the government, that is, the set of parliamentarians expected to support government initiatives. Electoral incentives may not find their way into policy outcomes, at least not as expected as in the theories described above, because of the constraints imposed by the need to form and sustain an executive.

Persson, Roland and Tabellini (2007) have recently modeled the choice of government parties to spend on group-specific goods, under majoritarian and proportional electoral systems. They show that spending under a two-party coalition government is higher than spending under a single party government, *regardless of* the electoral rules employed. This result rests on the assumption of a ministerial government and on a common pool problem among government parties. First, Persson, Roland and Tabellini assume that cabinet ministers solely determine the outcomes of the policies under their jurisdiction, as in Laver and Shepsle (1996). In a coalition, each government party sets *unilaterally* the level of spending on the good benefiting its represented group. Second, because voters *can* discriminate among government parties in the voting booth, there is higher intra-government competition in coalition than in single-party governments. And because coalition parties do not internalize the full cost of providing their group-specific goods borne by their coalition partners, spending is higher in coalition governments.⁹

The choice variable in this model is a transfer - a local public good - to groups. Under a majoritarian electoral system, group members are distributed uniformly across the electoral districts. This is clearly different from the group-specific transfer payment in Persson and Tabellini (1999) where groups coincide with districts and, therefore, the benefits of a measure are geographically concentrated. It is

⁹ See Bawn and Rosenbluth (2006) for a less formalized version of this argument.

not clear if Persson, Roland and Tabellini's expectation holds in case of a skewed distribution of party voters across districts, which would approximate the scenario described in Persson and Tabellini (1999). Nevertheless, since the type of government does *not* affect spending on general public goods (Persson, Roland, and Tabellini 2007: 158), it should plausibly affect spending on distributive measures. We have however no reason to expect that the type of government would also have an impact on the fragmentation of government intervention. We expect therefore

Hypothesis 5: Single party majority governments display lower spending on distributive measures.

Bawn and Rosenbluth (2006) and Persson, Roland and Tabellini (2007) find evidence that, respectively, the (lagged) number of parties in government and coalition governments increase overall spending. Persson, Roland and Tabellini also add that electoral rules affect spending *only* indirectly through their impact on the size of the party system and, therefore, on the types of government.¹⁰

Fiscal institutions

We look now *inside* executive policymaking of parliamentary democracies. A ministerial government - a key assumption of Persson, Roland and Tabellini - is Pareto-inferior to a negotiated compromise between cabinet members across policies (Thies 2001). Government parties therefore employ several mechanisms, such as coalition policy documents, shadowing junior ministers and interministerial committees (e.g. Gallagher, Laver, and Mair 2006: 40-43; Müller and Strøm 2000; Thies 2001), to limit the drift toward this type of government.

¹⁰ More specifically, Persson, Roland and Tabellini show that majoritarian electoral systems and district magnitude are both valid instruments for the types of government and they do not affect spending directly. In a next step, these scholars endogenize the size of the party system with a model of strategic entry by a party. Broadly speaking, they formalize Duverger's (1964) theory. This is why they instrument the type of government with measures of the proportionality of the electoral system. Their propositions rest however on two important assumptions. First, party voters are distributed homogeneously across districts. Second, they assume a four-group (cleavage) polity. As Duverger reminds us, geographical concentration of party voters and few cleavages qualify significantly his theory.

In case of budgeting, a ministerial government is equivalent to the situation in which each minister *unilaterally* sets the level of spending in the policy area under her jurisdiction, subject to a government-wide budget constraint. Hallerberg, Strauch and von Hagen (2009: 25-26) have recently modeled this decentralized budgeting process in which a minister internalizes only the share of the costs of her spending programmes that fall on her constituency.

According to these scholars, two alternative mechanisms may be employed to limit the drift toward ministerial government in budgeting. On the one hand, a government may adopt a centralized budgeting process where the minister of finance has agenda setting and monitoring power over the spending ministers. In this context, the finance minister takes into account the entire cost of the spending programmes, therefore internalizing the common pool externality of decentralized budgeting (Hallerberg, Strauch, and von Hagen 2009: 28-29). This arrangement does not work well (it leads to more spending) when there is conflict between the finance minister and her colleagues in the executive, such as in heterogeneous government coalitions. In this setting, the finance and spending ministers may not belong to the same party and the former is likely to have biased spending preferences. Hallerberg, Strauch and von Hagen show that, for the minister to be delegated agenda setting powers by the other ministers and to pursue her spending priorities, she has to agree to a level of spending that is ultimately equivalent to a decentralized budgeting process. The rules governing a centralized budgeting process are called delegation fiscal institutions by Hallerberg, Strauch and von Hagen. Under this set of institutions, we expect that it would be harder to spend on state aid when the government is cohesive, while there would be no implications for fragmentation.

The alternative to a centralized budget procedure is a budget negotiated among government parties (or spending ministers) that internalizes the full cost and is Pareto-superior to the budget produced by the decentralized procedure. The set of rules underpinning this budgetary process are called contract fiscal institutions. What preclude however a spending minister from renegeing on this deal during implementa-

tion? In other words, how do we avoid a drift toward ministerial government? Crucial here is the credibility of the mechanism to enforce the agreed-upon budget. According to Hallerberg, Strauch and von Hagen (2009: 36-37), noncompliance can be more credibly sanctioned in a coalition than in single party government. Note first that, in this setting, the finance minister has limited powers. If a spending minister reneges on the budget agreement, a coalition party can threaten to leave the government. Instead,

‘for a single-party government... the enforcement mechanism of the contract approach is rather weak. To see this, consider a single-party government with a weak prime minister and finance minister. [If a spending minister reneges on the budget agreement] the other cabinet members cannot credibly threaten the defector with dissolution of the government, since they would punish themselves by calling for elections’ (Hallerberg, Strauch, and von Hagen 2009: 37).

Absent a credible threat, ministers will not comply and spending will rise at the level of the decentralized budget procedure. In sum, under this alternative set of institutions, we expect that it would be harder to spend when the government is less cohesive, with no consequences for fragmentation. The two expectations are summarized as follows:

Hypothesis 6: *As conflict within the government decreases, spending on distributive measures lowers under delegation fiscal institutions.*

Hypothesis 7: *As conflict within the government increases, spending on distributive measures lowers under contract fiscal institutions.*

Since single party majority governments tend to display the highest level of internal cohesion, hypothesis 6 essentially conditions hypothesis 5 on delegation fiscal institutions. Hallerberg, Strauch and von Hagen (2009: 77-93) show that countries display a significant decrease in the gross debt ratio over the gross domestic product (GDP) and an improvement of the budget balance if homogeneous governments operate under delegation fiscal institutions and heterogeneous ones operate under contract

fiscal institutions.¹¹ To our knowledge, no work exists that tests these implications on government spending.

Ideology

The theories we have reviewed rest on the assumption that parties are office-seeking actors that set the policies only to maximize the probability of reelection. The partisan theory of macroeconomic policy puts emphasis on the policy-seeking motivations of politicians (Hibbs 1977). Because of the redistributive consequences of economic outcomes on key constituencies, left-wing governments are more inclined to pursue expansionary fiscal policies than right-wing governments. In other words, they trade off lower unemployment for higher inflation in a (short-run) Philips curve-type of fashion. Within-country partisan differences in policy outputs may persist even controlling for the preferences of the median voter which, in a Downsian model of party competition, would have annulled them (Aldrich 1983). We formulate the expectation on the spending bias of left-wing governments as follows (we expect no impact for fragmentation).¹²

Hypothesis 8: *As government composition moves to the left, spending on distributive measures increase.*

Empirical evidence does not however point in the same direction. Garrett (1998: 76-85) and Clark (2002: 59-67) offer evidence of the positive impact of a composite left-labor power index on

¹¹ Hallerberg, Strauch and von Hagen also instrument the changes in fiscal institutions with the values the institutional variables and debt level of 1991 to deal with the possible endogeneity due to the necessity to comply with the obligations of the Treaty of Maastricht. These do not seem to be valid instruments.

¹² If voters can rationally anticipate government policies, Alesina (1987) argues that an expansionary fiscal policy is pursued by left-wing governments only at the beginning of their term of office (for supportive evidence see Alesina 1989; Alesina, Roubini, and Cohen 1997). We do not find support in favor of this expectation.

government spending, when capital is mobile and trade openness is high. For Franzese (2002: 98-99), however, partisanship affects government transfers only weakly.¹³

Closer to our interests, the impact of partisanship on spending in distributive measures seems to be conditioned by degree of 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, the left-labor power index aggregates six distinct attributes, such as a left-right indicator for the legislature (seat-weighted) and for the executive (portfolio-weighted), union density and the share of unionized workers who are member of the largest labor confederation. It is unclear which one of these attributes actually matters.¹⁴

Data

State aid in the EU fits nicely Weingast, Shepsle, and Johnsen's (1981: 644) definition of distributive policy. 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.¹⁵ It must be state funded and, lastly, it

¹³ Partisanship instead does not seem to interact in a significant way with the exchange rate regime in affecting spending (Clark 2002: 75) and, under capital mobility, it appears also to have a limited effect on the debt-GDP ratio (Boix 2000: 64).

¹⁴ Boix (2000: 61) does not find robust results in a test on the debt GDP ratio after separating political from labor market attributes. We do not find evidence of a significant interactive effect of ideology and trade openness on spending.

¹⁵ 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.

must potentially distort competition and trade across the EU (Buts, Jegers, and Joris 2010: 3). An aid is therefore a state funded measure that provides a selective financial benefit to social groups that are relatively geographically concentrated and easy to identify. Although diffuse groups, such as consumers, may also benefit from such measure, taxpayers always foot the bill. Importantly, measures that are not selective are not considered state aid. The last criterion implies that we focus on measures above a given spending threshold.¹⁶

Employing data on state aid in the EU offer 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.¹⁷ 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. Third, with the adoption of Council Regulation 659/1999, which further codified the procedure of state aid control, we can now collect systematic information on the frequency of adoption of distributive measures across EU countries.

We have collected data on the national expenditure on aid of twenty-six EU member states¹⁸ for the period between 1992 (or their year of accession) and 2009, available from the State aid Scoreboard - a

¹⁶ Currently, such threshold is set at €200000, over any period of three fiscal years, for aid (€100000 for road transport aid) and €1.5 million for loan guarantees (Commission Regulation 1998/20006).

¹⁷ In exceptional circumstances, the EU Council of Ministers may unanimously approve a proposed measure. The regulation on state aid is based on Articles 107 to 109 of the TFEU.

¹⁸ 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 models 5 to 10 on expenditure (Table 2) because we do not have data on fiscal institutions.

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 gross domestic product. Additionally, from March 1999 onwards, each application to grant state aid is documented in an online database and it is therefore possible to collect information on the frequency of use of distributive measures across member states. *State aid applications* is the natural logarithm of the number applications (plus one) for new state aid to industry and services, by a given country in a given year.¹⁹

Countries make extensive use of policies that employ public funds for the benefit of specific geographical constituencies. Since 1992, EU governments have spent every year 0.53 percent of their GDP on average on distributive measures – more than 320 billion US dollars. Between 1999 and 2009, they have logged 3,077 applications to grant new aid to industry and services (thirteen new measures per country every year). These aggregate figures hide significant variations. For instance in Belgium, government spending, as proportion of GDP, is on average twice as much as it is in the Netherlands and about one third less than in France. The proportion of government spending in Hungary is almost twice as it is in Poland. Despite the smaller size of the economy, Spanish governments apply, on average, for more measures than Italian ones, and Belgian governments for more measures than Swedish ones.

¹⁹ The Scoreboard can be accessed at http://ec.europa.eu/competition/state_aid/studies_reports/expenditure.html. The online database is accessible at <http://ec.europa.eu/competition/elojade/isef/index.cfm>. Data have been collected between the September 2009 and January 2010. The Commission approves, sometimes subject to conditions, 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 a few cases, more than one file was opened with regard to same measure, maybe because resubmission was necessary for administrative reasons. We counted the first application only. 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.

We have then produced several explanatory variables. *Preelection* 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.²⁰ According to hypothesis 1, *Preelection* should be positively associated with state aid expenditure and applications.

For hypothesis 2, we have first calculated the district magnitude for each electoral system that a country has employed in the time period of interest. In case of majoritarian and proportional representation 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) 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.²¹ This (weighted) average district

²⁰ 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. For instance, if a change of government has occurred in a given year, the variable measuring government preferences is the sum of the two (or more) governmental positions, weighted by the yearly share of time in office of each government.

²¹ 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

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. $\text{magnitude} \times 100/\text{assembly size}$). 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 applies only if party leaders have control over the ballot. Otherwise, *District magnitude* should display a positive association with expenditure and applications. The conditioning variable in this latter hypothesis 3, *Ballot control*, is an indicator that takes the value of one if a large majority of the members of the assembly are elected under a closed-list electoral system.²²

For hypothesis 4, we employ the data on electoral systems and the personal vote developed by Johnson, Wallack and their colleagues (Johnson and Wallack 2007; Wallack, Gaviria, Panizza, and Stein 2003). These scholars have produced average values across electoral tiers, weighted by the share of members that originate from each tier, for the three institutional features - district magnitude aside - identified by Carey and Shugart's (1995) as shaping the incentives to cultivate a personal vote. Following Edwards and Thames (2007), *Personal vote* is the average of the three values taken by the scores on ballot, pool and votes. It should be positively associated with expenditure and applications, as district magnitude increases.

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.

²² Unlike Carey and Shugart (1995), a single member district electoral system is not considered closed-list for this purpose. Wallack et al (2003: 137) argue that coding these systems as closed-list overemphasizes the control that parties have over the ballot, compared to that of voters. 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.

We need a measurement of government preferences to test hypotheses 5 to 8. State aid policy is underpinned by the traditional left-right economic cleavage which pits 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. According to hypothesis 8, it should be negatively associated with aid spending. *Government range* is instead the absolute difference among the extreme positions of government parties along such dimension.²³ This variable takes the values of zero in case of single party majority governments and it is therefore highly negatively correlated with an indicator variable for these types of governments ($p < 0.01$). Following hypotheses 5 and, conditionally, 6, *Government range* should therefore be positively associated with aid expenditure.

What remains is the operationalization of fiscal institutions operating in a country. We use the aggregate scores for delegation and contract institutions provided by Hallerberg, Strauch and von Hagen (2009) and, for eastern European member states, by Hallerberg and Yläoutinen (2010). *Delegation* is an index that takes into account the extent to which a) budget negotiations in the cabinet are centralized by the ministry of finance, b) the parliament cannot affect the budget structure and c) it is difficult to change the budget once adopted. According to hypothesis 6, *Government range* is

²³ 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*.

positively associated with aid expenditure only for high values of *Delegation*. *Contract* is instead an index that measures the extent to which a country uses multiannual budget plans.²⁴ Hypothesis 8 suggests that *Government range* is negatively associated with aid spending for high values of *Contract*. 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 degree of trade openness (the sum of import and export 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.²⁵ We include therefore an indicator 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. For the analysis on applications, we expect that governments of larger and, given our focus on industry and services, more industrialized economies to adopt more measures. We therefore include the lagged values of the (log of) GDP and the agriculture value added as share of the GDP. Descriptive statistics of the variables we employ in the analysis are illustrated in Table 1.

< TABLE 1 HERE >

²⁴ The variable is called ‘targets’ by Hallerberg, Strauch and von Hagen (2009) and ‘contracts 1’ by Hallerberg and Yläoutinen (2010). A more encompassing index for contracts, employed by Hallerberg, Strauch and von Hagen (2009), shares some of the attributes with the delegation index and the two should not be used in the same regression model.

²⁵ For instance, Boix (2000: 64) shows that countries operating under a fixed exchange rate regime tend to run higher debt-GDP ratios. 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.

Results

Electoral and fiscal 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 regressions with panel corrected standard errors which perform well when the number of years and units are similar. Since we also find evidence of autocorrelation, we estimate these models with first-order autocorrelation AR(1) with a common coefficient across countries (Beck and Katz 1995). We report the results of models both with and without country fixed effects. Although fixed effects are designed to deal with omitted variable bias, they come at a significant cost for our study. In these models, the estimated effect of the independent variables is solely based on information about how they affect *changes* in spending and applications *within* a single country, ignoring both level effects and cross-country variation. This is particularly problematic for our institutional variables, not only because their cross-country impact is substantively interesting but also because these variables change slowly over time and their within-country impact may be negligible. Moreover, although the theories discussed above do not differentiate clearly, it is plausible to suspect that some independent variables may also display level effects (Beck and Katz 2004; Plumper, Troeger, and Manow 2005).²⁶

< TABLE 2 HERE >

Table 2 presents the results of five Prais-Winsten regressions, with and without country fixed effects. Models 1 and 2 include only the variables that have a direct effect on spending. They are therefore a test of hypotheses 1, 2, 5 and 8. Models 3 and 4 add the interactive effects of electoral institutions as

²⁶ We also must note that one of independent variables that we employ, *eurozone*, operates like a group fixed effect variable. Hence the results of models with country fixed effects may have to be interpreted with some caution.

formulated by hypotheses 3 and 4. Models 5 and 6 add the interactive effects of fiscal institutions as formulated by hypotheses 5 and 6. Finally, following Hallerberg, Strauch and von Hagen (2009), the remaining models 7 to 10 rerun models 5 and 6 only for expected delegation and contract country-years.²⁷

We do not find evidence in favor of hypotheses 1 and 8. Closeness to elections and the ideological orientation of governments do not have an impact on spending on distributive measures. Hypothesis 5 is also disconfirmed. More cohesive executives, such as single party majority governments, do not spend less. In model 3 of Table 2, we actually find some (weak) evidence to the contrary, but this impact is not robust in the other specifications.²⁸

In line with hypotheses 2, we find for the first time direct evidence that district magnitude is an electoral institution that facilitates the internalization of costs. On average, a standard deviation increase in normalized *District magnitude*, equivalent to the difference between the German and the French electoral systems, accounts for a 0.08 – 0.09 decrease in the percentage of state aid expenditure over GDP. This is equivalent to a reduction in spending of approximately between 27 and 29 billion U.S. dollars per year for the average European economy.

Partially as hypothesis 3 contends, district magnitude affects spending in combination with other electoral institutions. Its impact is much larger if candidates are elected under a closed-list electoral system. In these circumstances, a standard deviation increase in magnitude accounts for a 0.15 – 0.22 decrease in the percentage of expenditure, equivalent to between 48 and 71 billion U.S. dollars.²⁹ Note however

²⁷ Observations with below average values of *Government range* fall under the expected delegation subset, those above under the expected contract one. This subdivision is very similar to the one employed by Hallerberg, Strauch and von Hagen (2009: 50).

²⁸ The result holds if we substitute *Government range* with an indicator variable for single party majority governments.

²⁹ Only model 5 does not confirm this result, but, as it can be seen below, this model includes fiscal institutions that do not behave as expected in the full dataset.

that there is no evidence in support of the second part of hypothesis 3. An increase in district magnitude does not lead to more spending when party leaders have no control over the ballot.

< FIGURE 1 HERE >

Figure 1 presents the marginal effect of district magnitude on spending, as incentives to cultivate personal votes vary across the observable range of values. The other variables are set at their means, while the gray area covers the 95% confidence interval. Such incentives and district magnitude do not interact positively as hypothesis 4 suggests. We have a different dynamics. In electoral systems where there are weak incentives, an increase in district magnitude lowers spending. Compare for instance the post-2006 Italian and the Portuguese electoral systems. These systems display very low incentives to cultivate personal votes, but the Italian one has a much higher magnitude. This could partially account for the fact that spending on distributive measures, as share of GDP, is three times higher in Portugal. The curves in Figure 1 however are either flat or downward sloping. In other words, there is no evidence that an increase in magnitude leads to more spending at high incentives to cultivate the personal vote.

In sum, two electoral institutions, district magnitude and ballot control, have the greatest impact on spending. We find conditional support of Persson and Tabellini's (1999; 2000) argument on the negative impact that district magnitude should have on spending. Higher district magnitude leads to less spending on distributive measures *if* party leaders control access to the ballot rank. Otherwise, it has no effect. This latter result also means that Carey and Shugart (1995) are only partially correct. As they predict, when an increase in magnitude is associated with a lack of incentives to cultivate personal votes, spending on distributive measures is reduced. But when such incentives are present, we do not find evidence that they positively interact with increases in magnitude.

As far as the control variables are concerned, their impact on spending is limited and operates solely within countries. As a country gets richer, it spends slightly more on distributive measures. A country like France would increase spending by about 29 billion US dollar across the entire period (an increase

of 0.09 percentage points). We also see an increase in spending after the adoption of Regulation 659/1999 (0.19 percentage points, 61 billion US dollar). Perhaps surprisingly, joining the Economic and Monetary Union leads a country to cut spending on distributive measures, by approximately 0.41 percentage points (about 133 billion US dollar). The fiscal framework surrounding the single European currency appears to undercut the incentives to run looser fiscal policies that the Mundell-Fleming model purports. These effects disappear across countries however; hence, they need to be interpreted with caution. Richer countries do not spend on distributive measures more than poorer ones, and those that have adopted the euro do not spend less than those which have not. The time impact following the adoption of the regulation also disappears.

< FIGURE 2 HERE >

We move on now to models 5 to 10 of Table 2. Fiscal institutions do not operate as expected in models 5 and 6 but, as Hallerberg, Strauch and von Hagen (2009: 81-92) find, they do so in some of the models that include a subset of cases. Figure 2 presents the marginal effect of delegation and contract institutions on spending, as government range varies across the observable set of values. The other variables are set at their means, while the gray area covers the 95% confidence interval. Take models 7 and 8 in Table 2, where only delegation states are included. In single party majority governments, where government range takes the value of zero, an increase of the index of delegation fiscal institutions from its minimum to its maximum lowers spending by 0.51 – 0.66 percentage points, between 164 and 214 billion U.S. dollars. In these circumstances, an increase of the index of contract fiscal institutions *increases* spending by 0.82 – 1.07 percentage points (266 - 347 billion U.S. dollars). However, in case of a coalition government, such as the socialist-liberal coalition that ruled Hungary from 2004 onwards, an increase of delegation fiscal institutions leads to *more* spending (0.006-0.009 percentage point, 2-3 billion U.S. dollars) and an increase of contract fiscal institutions leads to *less* spending (0.005 percentage point, 2 billion U.S. dollars in model 8).

The two panels in the upper part of Figure 2 show these results. The curve of the marginal effect of delegation fiscal institutions cuts across the zero line and displays an upward slope as government range increases. As hypothesis 6 suggests, the adoption of delegation fiscal institutions reduces spending in single party majority or cohesive coalition governments. It instead increases spending in more heterogeneous coalitions. The curve of the marginal effect of contract fiscal institutions also cuts across the zero line, but it displays a downward slope as the range increases. Contract fiscal institutions therefore increase spending in single party majority or cohesive coalition governments. In line with hypothesis 7, these institutions reduce spending in heterogeneous coalitions.

Results however are not confirmed in models 9 and 10 of Table 2, where only contract states are included. No support is found in favor of hypothesis 7. Contract institutions do not appear to reduce spending in more heterogeneous governments. Additionally, if highly heterogeneous governments, such as some of the Belgian, Finnish or Italian coalitions, adopt more delegation fiscal institutions, spending *decreases* by 0.006 percentage points (2 billion U.S. dollars). This is illustrated in the bottom left panel of Figure 2. At high levels of intra-government conflict, the adoption of delegation fiscal institutions leads to a reduction in spending.

In sum, fiscal institutions affect spending, but only under specific circumstances. In single party majority or cohesive coalition governments, if the budget is adopted and implemented prevalently under delegation rather than contract fiscal institutions, spending on distributive measures is reduced. If the opposite were true, we would find more spending. In other types of government, fiscal institutions do not appear to operate in a unique way.

Electoral institutions and fragmentation of distributive measures

Table 3 presents the results of three OLS regressions, with and without country fixed effects, on the number of applications. Models 1 and 2 are a test of hypotheses 1 and 2. Models 3 and 4 add hypothesis 3, while models 5 and 6 add hypothesis 4.

< TABLE 3 HERE >

We do not find evidence in favor of the first two hypotheses. As an election approaches, countries do not adopt more measures and district magnitude *per se* does not reduce fragmentation. Electoral institutions operate in the more complex way described in hypothesis 3. If party leaders have control over the ballot, a standard deviation increase in district magnitude reduces the number of distributive measures, by not a large amount however – less than a measure a year. But when these leaders cannot control the ballot rank in electoral list systems, a standard deviation increase in magnitude leads more fragmentation – less than one more measure in model 3, ten more measures in model 6. This is no small amount, considering that, on average, the typical country adopts thirteen measures a year. However, this large effect is only relevant to the within-country specification (model 6), hence to the limited cases where a country, such as Italy, Belgium and Romania, experienced a change in district magnitude and ballot control provisions.

< FIGURE 3 HERE >

Figure 3 presents the marginal effect of district magnitude on the number of measures, at different levels of incentives to cultivate personal votes. As in Figure 1, the other variables are set at their means and the gray area covers the 95% confidence interval. Also in this circumstance we have a different dynamics from the one predicted by hypothesis 4. When leaders can exercise strong ballot control, an increase in district magnitude lowers fragmentation in electoral systems with weak incentives (top panel in Figure 3). Compare again the post-2006 Italian and the Portuguese electoral systems - with similarly low incentives to cultivate personal votes, but a much higher magnitude in Italy. Considering that the Italian economy is fourteen times the size of the Portuguese one and, proportionally, only half as much is based on agriculture, we find on average only 27 Italian measures compared to seven Portuguese ones.

We do not find evidence however of the positive interaction between magnitude and incentives. Actually, in the within-country specification (model 6), the curve is downward sloping, meaning that an increase in magnitude reduces fragmentation, more so at high incentives to cultivate personal votes (middle panel). Eleven countries, with relatively high incentives, have experienced changes in district magnitude in this time period. In five cases, the electoral system was modified while, in the remaining ones, new districts were established or the number of compensation seats changed. Overall, the effects of these modifications on the district magnitude were generally small in size (with the only exception of Belgium in 2003) and the related impact on the number of measures was equally small, never exceeding one more measure a year. Finally, the bottom panel of Figure 2 shows, as hypothesis 3 suggests, that an increase in magnitude leads to more measures when party leaders cannot control the ballot rank. But such effect diminishes as the incentives to cultivate the personal vote increase, again disconfirming hypothesis 4.

In sum, district magnitude and ballot control are again the two most important electoral institutions determining the number of distributive measures countries adopt. The results are less robust than in the analysis on expenditure because we cover a shorter time period. Also, because these institutions either remain invariant or change slowly over time, their effect is more noticeable in model 3 which excludes fixed effects. If party leaders have control over the ballot, an increase in district magnitude reduces the number of distributive measures. If they do not enjoy such control, an increase in magnitude leads to more fragmentation. The other institutions that shape the incentives to cultivate the personal vote, included in models 5 and 6, do not behave as expected.

Conclusions

These findings have important implications for the study of the institutional determinants of policy outcomes. First, we provide for the first time evidence, across several countries and several years, of how electoral and fiscal institutions affect spending on and frequency of use of distributive measures.

This was possible because the EU policy on state aid control offers comparable country-level data on this type of measures. Second, in line with the expectation of Persson and Tabellini (1999; 2000), we have shown that, in countries where the number of legislators elected from the districts is relatively high on average, spending on distributive measures is proportionally lower because legislators' demand is more likely to factor in their cost for the taxpayer. However, as Carey and Shugart (1995) argue, to have such an effect, district magnitude must be associated with another electoral institution: ballot control. If party leaders cannot control access to the ballot rank, such as in open-list electoral systems, the politicians' incentives to pursue personal votes heighten the demand for distributive measures, thus offsetting the impact of larger magnitude. Where party leaders control access instead, an increase in district magnitude reduces spending as expected. In these circumstances, fewer measures are also adopted. There is also, less robust, evidence that more measures are adopted if an increase in magnitude is associated with lack of ballot control. Finally, we have shown that fiscal institutions operate along the lines suggested by Hallerberg, Strauch and von Hagen (2009) only in case of single party majority or cohesive coalition governments. Greater reliance on delegation rather than contract fiscal institutions leads to lower spending on distributive measures.

In conclusion, besides social welfare or economic efficiency considerations, electoral and fiscal institutions systematically structure the incentives of politicians to adopt geographically targeted measures.

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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
<i>Preelection</i>	1 in preelection year, time-weighted share of 1 in election year, 0 otherwise	0.39	0.44	0.00	1.00
<i>Government preference</i>	Portfolio-weighted position of government parties on taxes-spending dimension	11.22	2.77	5.80	17.21
<i>District magnitude</i>	Tier-weighted average district magnitude, normalized by assembly size	16.57	27.99	0.15	100.00
<i>Personal vote</i>	Incentives to cultivate personal votes. Average of scores on ballot, pool and votes	0.79	0.43	0.00	1.67
<i>Ballot control</i>	1 if majority of assembly members are elected under a closed-list electoral system, 0 otherwise	0.19	0.39	0	1
<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>Contract</i>	Aggregate score for contract fiscal institutions	0.68	0.26	0.00	1.00
<i>Delegation</i>	Aggregate score for delegation fiscal institutions	0.56	0.17	0.23	0.90
<i>GDP per capita</i>	GPD per capita, at t-1	26169.16	15155.86	4101.16	119137.40
<i>GDP growth rate</i>	Real GDP growth rate, at t-1	3.28	2.62	-5.13	12.23
<i>Trade openness</i>	Sum of import and export over GDP, at t-1	99.06	54.16	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	26.21	1.47	22.33	28.93
<i>Agriculture added value</i>	Agriculture value added as share of GDP, at t-1	3.08	1.90	0.36	10.51

TABLE 2. Determinants of Expenditure on Distributive Measures, EU countries 1992-2009

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Preelection</i>	-0.0156 (3.177)	1.507 (3.083)	-0.176 (3.164)	1.220 (3.021)	1.371 (2.757)	2.210 (2.825)
<i>Government preference</i>	-0.240 (0.952)	1.199 (0.744)	-0.0749 (0.945)	1.175 (0.757)	0.587 (0.930)	1.223 (0.759)
<i>District magnitude</i>	-0.295*** (0.0799)	-0.325** (0.163)	-0.329 (0.566)	-1.710 (2.930)	-0.658 (0.522)	-1.054 (2.986)
<i>Personal vote</i>			-22.11** (10.85)	-50.44*** (15.77)	-19.18* (11.44)	-50.75*** (15.56)
<i>District magnitude</i> × <i>Personal vote</i>			0.0314 (0.883)	-0.342 (0.423)	0.697 (0.775)	-0.245 (0.461)
<i>Ballot control</i>			1.713 (12.71)	-34.31 (27.40)	10.24 (12.80)	-32.47 (27.56)
<i>District magnitude</i> × <i>Ballot control</i>			-0.212 (0.612)	0.997 (2.990)	0.184 (0.559)	0.399 (3.038)
<i>Government range</i>	-1.375 (0.946)	-0.178 (0.814)	-1.609* (0.933)	-0.296 (0.835)	-0.0132 (2.351)	0.0260 (2.272)
<i>Contract</i>					32.94 (24.98)	22.67 (23.36)
<i>Government range</i> × <i>Contract</i>					-4.052 (3.377)	-3.080 (3.219)
<i>Delegation</i>					-44.08 (39.45)	-43.60 (35.77)
<i>Government range</i> × <i>Delegation</i>					4.215 (4.879)	3.246 (4.139)
<i>GDP per capita</i> _{t-1}	-0.000310 (0.0003)	0.000394* (0.000224)	-0.000188 (0.000295)	0.000380* (0.000228)	0.000334 (0.000224)	0.000436* (0.000223)
<i>GDP growth rate</i> _{t-1}	-0.901 (0.976)	0.291 (0.598)	-0.825 (0.951)	0.255 (0.582)	0.306 (0.623)	0.675 (0.540)
<i>Trade openness</i> _{t-1}	0.0580 (0.115)	-0.215 (0.151)	0.0468 (0.122)	-0.201 (0.152)	-0.178** (0.0762)	-0.235 (0.182)
<i>Eurozone</i>	-14.15 (12.01)	-41.19*** (12.89)	-17.14 (12.14)	-41.03*** (12.86)	-14.86 (9.681)	-39.93*** (11.75)
<i>Regulation</i>	2.155 (10.23)	18.70* (11.26)	4.055 (10.53)	18.92* (11.32)	-1.588 (8.846)	18.91* (10.27)
Constant	78.85*** (15.66)	44.20*** (13.40)	93.58*** (16.46)	84.12*** (20.76)	80.34*** (19.31)	87.85*** (23.88)
<i>District magnitude</i> <i>Ballot control</i> [†]			-0.535** (0.257)	-0.780*** (0.240)	-0.336 (0.229)	-0.704*** (0.235)
<i>District magnitude</i> <i>No ballot control</i> [†]			-0.300 (0.266)	-2.026 (2.839)	-0.015 (0.207)	-1.281 (2.910)
Country fixed effects	no	yes	no	yes	no	yes
Observations	320	320	320	320	315	315
R-squared	0.154	0.579	0.171	0.582	0.184	0.463
Number of groups	26	26	26	26	25	25
Wald chi2	32.89	62378	74.80	9199	93.99	28746
Rho	0.623	0.374	0.621	0.378	0.625	0.350

Prais-Winsten regressions with panel corrected standard errors. Pairwise selection and common ARI 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 their means.

TABLE 2. Determinants of Expenditure on Distributive Measures, EU countries 1992-2009 - continued

	(7)	(8)	(9)	(10)
<i>Preelection</i>	2.615 (4.395)	6.381 (4.646)	-2.747 (3.754)	-5.303 (3.787)
<i>Government preference</i>	0.939 (1.397)	1.298 (1.015)	-0.0570 (1.210)	0.0469 (0.986)
<i>District magnitude</i>	-0.252 (0.693)	-56.99 (94.96)	0.446 (0.972)	37.49*** (10.50)
<i>Personal vote</i>	-3.735 (13.80)	948.9 (3,228)	3.464 (18.50)	293.7*** (88.74)
<i>District magnitude</i> × <i>Personal vote</i>	0.338 (0.947)	40.31 (135.2)	-0.938 (1.399)	-59.35*** (14.59)
<i>Ballot control</i>	36.67** (15.82)	121.8 (1,285)	32.87 (26.04)	174.8*** (64.54)
<i>District magnitude</i> × <i>Ballot control</i>	-0.397 (0.721)	97.69 (169.9)	-0.814 (1.005)	-38.00*** (10.47)
<i>Government range</i>	-5.559 (7.375)	-5.598 (5.497)	10.72 (6.873)	9.831** (4.505)
<i>Contract</i>	82.16*** (30.28)	107.1*** (30.63)	77.38 (69.61)	10.02 (37.14)
<i>Government range</i> × <i>Contract</i>	-24.01** (10.08)	-33.75*** (10.29)	-8.465 (7.078)	-3.543 (4.212)
<i>Delegation</i>	-98.68** (40.24)	-75.70** (32.81)	51.30 (81.26)	20.27 (69.41)
<i>Government range</i> × <i>Delegation</i>	38.76*** (13.36)	44.67*** (11.85)	-8.492 (8.069)	-9.079 (7.645)
<i>GDP per capita</i>	7.09e-05 (0.000363)	0.000406 (0.000403)	0.000489 (0.000346)	0.000345 (0.000210)
<i>GDP growth rate</i>	-0.995 (0.830)	0.505 (0.800)	0.513 (0.696)	0.513 (0.598)
<i>Trade openness</i>	-0.0440 (0.120)	-0.407 (0.260)	-0.256*** (0.0781)	0.156 (0.218)
<i>Eurozone</i>	-25.13* (13.45)	-57.67*** (12.71)	-22.81 (16.99)	-48.36* (25.23)
<i>Regulation</i>	-4.589 (10.51)	8.751 (10.38)	11.12 (15.49)	41.92* (23.88)
Constant	63.03** (26.77)	-476.5 (2,155)	-20.53 (73.09)	-210.1*** (79.63)
<i>District magnitude</i> <i>Ballot control</i> †	-0.593** (0.254)	47.43 (99.53)	-0.60 (0.43)	-15.03*** (3.52)
<i>District magnitude</i> <i>No ballot control</i> †	0.090 (0.433)	-16.19 (53.04)	-0.33 (0.21)	-11.90*** (3.89)
Country fixed effects	No	yes	no	yes
Subsets	Delegation states	Delegation states	Contract states	Contract states
Observations	164	164	151	151
R-squared	0.257	0.614	0.329	0.649
Number of groups	20	20	20	20
Wald chi2	54.79	951.5	1940	1042
Rho	0.568	0.243	0.609	0.129

Prais-Winsten regressions with panel corrected standard errors. Pairwise selection and common ARI 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 their means.

TABLE 3: Determinants of State Aid Applications in the EU, 1999-2009

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Preelection</i>	1.596 (10.41)	1.940 (8.834)	1.468 (10.35)	1.982 (8.804)	1.839 (10.51)	2.311 (8.938)
<i>District magnitude</i>	0.0864 (0.159)	-0.0414 (0.141)	0.334** (0.141)	7.749 (6.143)	0.335 (1.086)	14.13*** (5.427)
<i>Ballot control</i>			26.73** (12.20)	82.45 (64.17)	29.90* (15.70)	143.8** (67.50)
<i>District magnitude</i> × <i>Ballot control</i>			-1.016*** (0.314)	-8.146 (6.424)	-1.009 (1.065)	-14.35** (5.724)
<i>Personal vote</i>					4.446 (10.96)	71.27 (65.63)
<i>District magnitude</i> × <i>Personal vote</i>					0.0165 (1.502)	-6.113*** (1.723)
<i>GDP_{t-1}</i>	53.93*** (3.305)	-64.30** (25.28)	54.63*** (3.168)	-67.01*** (25.38)	54.37*** (4.523)	-63.46** (26.13)
<i>Agriculture added value_{t-1}</i>	4.914* (2.670)	-3.640 (13.18)	5.460* (3.080)	-5.315 (13.35)	5.524** (2.794)	-3.558 (14.31)
Constant	-1.207*** (85.71)	1.947*** (683.6)	-1.232*** (82.83)	1.990*** (689.9)	-1.230*** (112.9)	1.836** (734.3)
<i>District magnitude</i> <i>Ballot control</i> †			-0.683** (0.283)	-0.397 (0.366)	-0.671* (0.351)	-1.357* (0.700)
<i>District magnitude</i> <i>No ballot control</i> †			0.334** (0.141)	7.749 (6.143)	0.350 (0.309)	8.599* (4.687)
Country fixed effects	no	yes	no	yes	no	yes
Observations	219	219	219	219	219	219
R-squared	0.559	0.781	0.576	0.782	0.576	0.784
Number of groups	25	25	25	25	25	25
Wald chi2	272.3	114.9	302.3	289.0	395.6	389.5

OLS regressions with panel corrected standard errors. 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 their means.

FIGURE 1. Expenditure and electoral institutions

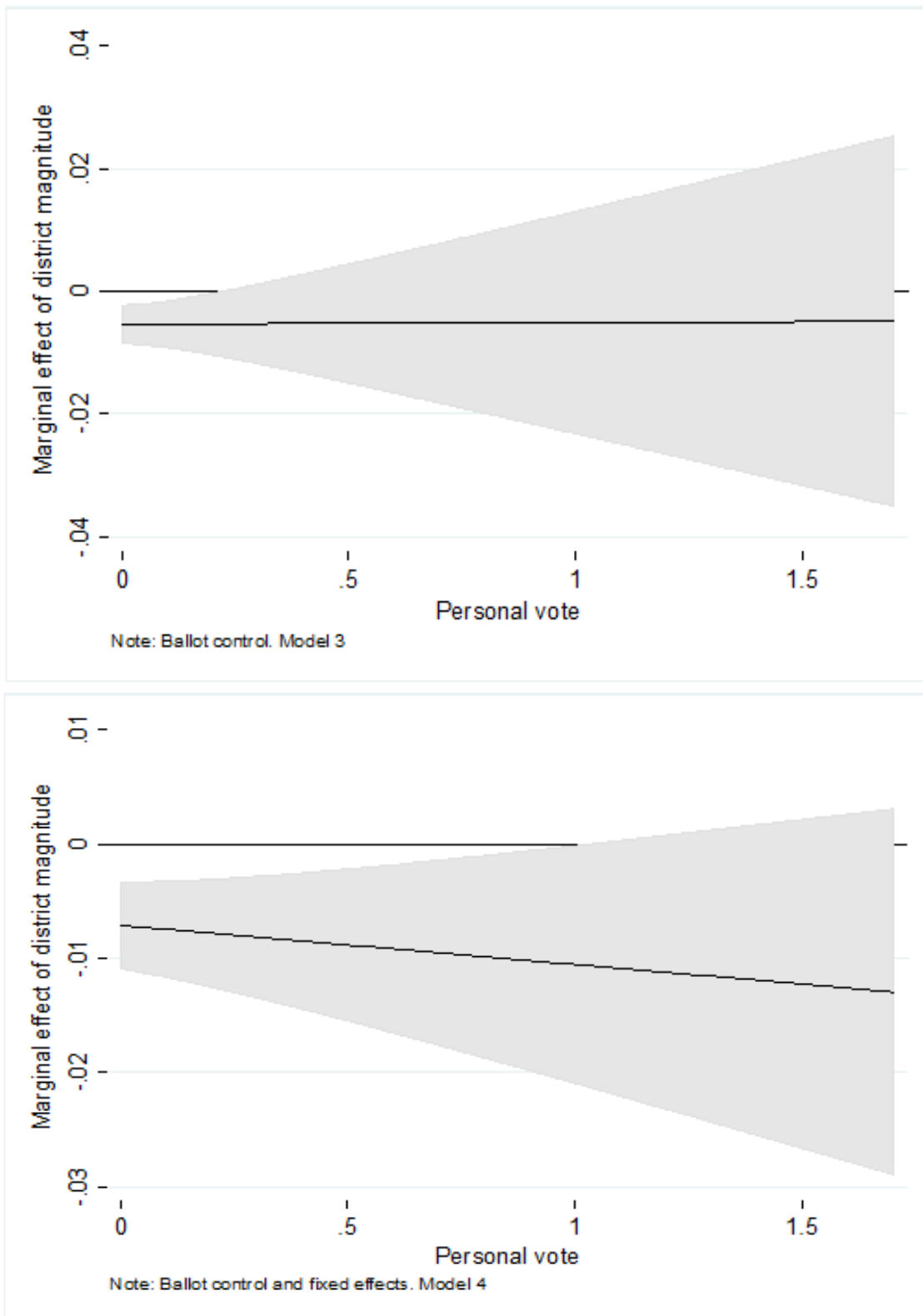


FIGURE 2. Expenditure and fiscal institutions

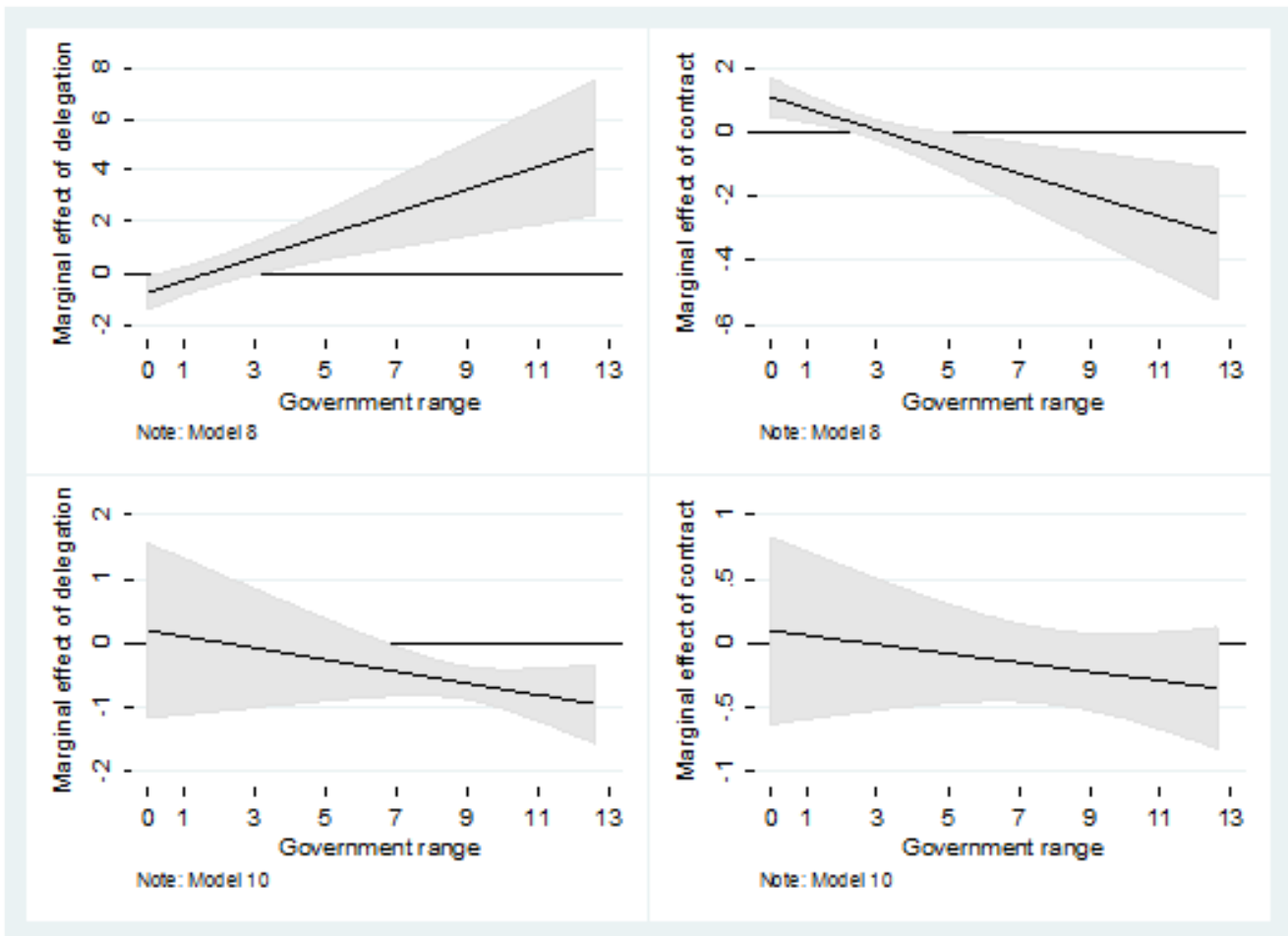


FIGURE 3. Fragmentation and electoral institutions

