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Proportional influence? Electoral rules and special-interest spending*

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Abstract

Conventional wisdom has it that proportional representation leads to more coalition governments and so to greater government spending, especially in redistributive categories favoured by special-interest groups. In contrast, we show in a theoretical model that first-past-the-post systems of government may give special interests greater influence in the winning electoral coalition than they would have in the corresponding legislative coalition under proportional representation. Evidence from a quasi-experimental reform in German local government supports this view. Introduction of a mayor directly elected under first-past-the-post rules caused a significant increase in local government expenditure, particularly in redistributive spending categories.

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1 Introduction

Why do some countries spend more, and redistribute more, than others? A recent literature in economics and political science emphasizes the importance of differences in electoral rules among countries that affect the incentives of political parties, and that in turn affect the propensity of governments to spend on the “special interests” of subsets of the electorate. The political incentives for such activities may vary systematically between countries that use proportional representation (PR) and those that use some form of first-past-the-post (FPTP) elections. It is often held that PR, by reducing barriers to entry to the legislature, results in more political parties gaining representation, greater instability in the governing legislative coalition, and so greater use of redistributive government spending to maintain coalition stability. In effect, special-interest groups gain a legislative voice under PR, are more likely to hold the balance of power, and so gain disproportionate influence on legislative outcomes.

In this paper, we call into question this conventional view. We present a theoretical model of the effects of the two electoral rules, and we contrast the policies that serve to maintain a winning *electoral* coalition under first-past-the-post to those required to maintain a winning *legislative* coalition under PR. In our model, voters differ in their attitude to an ideological issue and to “special interest” spending that benefits only a minority of the electorate. Two incumbent political parties differ in their stance on the ideological issue, and they promise levels of special-interest spending to voters in order to compete for office. The level of special-interest spending is then determined in equilibrium by the interplay between parties’ desire to win power and implement their preferred ideology, and the need to be competitive among the minority of voters for whom special-interest spending is most salient.

Competition between dominant parties in first-past-the-post elections can enhance spending on special interests, since electoral uncertainty increases the ex ante probability that special interest voters will be pivotal in the election, even when special interests are not pivotal ex post in the legislative bargaining game. When ideological differences among voters are sufficiently strong, electoral competition between dominant parties is intense, which results in higher equilibrium spending on special interests than would occur under proportional representation. In contrast, when ideological differences are small, costly competition for special interest voters is limited, and equilibrium spending under first-past-the-post is reduced. Thus, in our theory, the effect of first-past-the-post on redistributive spending is ambigu-

ous in general, but it is largest when ideological and partisan attachments of voters are strongest.

In our empirical application, we examine the impacts on government spending of an electoral reform for municipalities in the German state of Lower Saxony. In the reform, key decision-making power was transferred from the municipal council, elected from party lists under proportional representation, to a mayor newly elected under first-past-the-post rules. Important for our empirical approach, the reform was phased in among municipalities over a ten-year period, which admits a difference-in-difference estimator of its effect. We proxy the ideological preferences of voters, pointed to in our theoretical model, by an indicator variable that captures whether the mayor is affiliated with a national political party or ran as an independent candidate. Consistent with the theory, we find that the introduction of first-past-the-post voting led to a significant increase in government spending, particularly in redistributive categories, when the mayor is affiliated with a dominant political parties, but not for independent mayors. Overall, the impact of the reform on spending is fairly small, amounting to a three to five per cent increase in average total expenditures following the election of partisan mayors.

1.1 Previous literature

Our theoretical model bears some resemblance to that of Persson et al. (2007), who also compare PR and FPTP in a model which features both electoral competition and post-electoral legislative bargaining. In their model, voters vote retrospectively, based on past spending decisions in the legislature, but they make systematic mistakes in attributing spending decisions to parties that are members of a governing coalition. The result is a “common pool problem” among minority parties in the governing coalition (a feature of the equilibrium under PR) that leads to higher redistributive spending than pursued by the single majority governing party under FPTP. In our model, in contrast, the focus is on frictions in electoral competition rather than legislative bargaining.

The model in Persson et al. (2007) is related to the work of Tsebelis (1995), which analyzes how proportional representation may increase the number of “veto players” in the bargaining game that determines which government policies are enacted. In rough terms, political parties that are actual or potential members of the governing coalition are veto players that may block specific policy proposals, and the rise in the effective number of parties under PR results in a potential for “legislative gridlock” and status

quo bias in policy. As emphasized by Alesina and Drazen (1991), in a fiscal environment of rising costs and declining revenues, status quo bias may result in higher levels of government spending.

More recently – and more closely linked to our empirical work on local electoral institutions – Coate and Knight (2009) have studied differences in spending between cities that use “council-manager” and “mayor-council” systems of government, a difference that corresponds roughly to the German electoral reforms that we study.¹ The theoretical framework of Coate and Knight (2009) is a veto-player model, in which the status quo spending level on each project considered by government is zero. Since mayor and council on average have different preferences for public spending, and each has a veto over spending proposals, introduction of a mayor leads to lower spending than under the council-manager system. Exploiting cross-sectional comparisons and some electoral reforms in US municipalities, they find that government spending is indeed lower under mayor-council governments. In contrast, our theory emphasizes electoral factors that induce mayoral governments to spend more than the council-dominated form, and our empirical work exploits exogenous variation in the form of government induced by a staggered state-wide reform of institutions.

The existing theoretical literature is largely motivated by the observed cross-sectional differences in government spending between PR and FPTP countries. Electoral systems with greater degrees of proportionality are on average associated with higher government spending as a share of GDP (Persson and Tabellini, 2003), and a higher share of government spending devoted to redistributive transfer payments (Milesi-Ferretti et al., 2002). One possible mechanism behind these associations is indeed the potential instability of legislative politics under PR: Proportional systems are associated with an increase in the effective number of parties in the national legislature and greater incidence of coalition and minority governments (Lijphart, 1994). In turn, coalition and minority governments spend more and run greater government deficits (Halleberg and Von Hagen, 1999).

While this evidence is suggestive, there are well-known problems in giving it a causal interpretation. National electoral reforms are extremely rare, and cross-sectional differences in government spending may reflect omitted factors related to national culture and institutions, or in the measurement of the scope of government, that are correlated with electoral rules. Likewise,

¹In fact, the post-reform governments we study are of the “strong-mayor” form, in which council holds relatively little power independent of the mayor. Thus the comparison of government forms in our paper is somewhat different.

electoral rules may be endogenous to preferences for spending, if for example voters in countries that prefer greater equity in fiscal policies also prefer the greater inclusiveness for minority political interests that results from proportional representation. Lastly, electoral differences typically come as a package, and it is difficult to disentangle the effects of the electoral rule from related factors such as the size of electoral districts (which may affect the ease of geographic targeting of government spending), the differences between presidential and parliamentary systems, and so on.

Our empirical application deals with these problems by exploiting cross-section–time-series variation in the timing of the electoral reform, which allows us to control for unobservable differences in fiscal policies of municipalities that might be correlated with the electoral rule at a single point in time, and for state-wide changes in the economic and fiscal environment that occurred coincident with the reform. Moreover, confining the analysis to municipalities in a single state ensures that preferences, institutions, and fiscal environments are rather similar in treatment and control groups, both before and after the reform.

A separate branch of the literature examines the differing electoral incentives that arise under the two rules, and it reaches different conclusions from the papers cited above. Thus Myerson (1993) compares transfers to minority groups under alternative rank-order scoring rules for aggregating voter preferences and shows that winner-take-all elections are associated with more inequality in transfers than rules that are more proportional in the sense that the payoffs to runners-up are positive. Building on the Myerson model, Lizzeri and Persico (2001) show that redistribution can be larger (and universal public goods provision smaller) under first-past-the-post, if parties maximize vote share under PR but maximize the probability of victory under FPTP. The intuition is that redistributive spending may be more readily targeted to swing voters than public goods, and the rewards to doing so are larger under FPTP than PR.

These papers, like ours, emphasize the idea that special-interest voters may have more influence under FPTP than PR. But the formal model of electoral competition is somewhat abstract, and the emphasis on voter polarization, central to our theory, is absent.

2 The model

We consider a model of two political parties, labelled L and R , which compete for office by offering spending promises x_L and x_R . There is a contin-

uum population of voters with heterogeneous preferences over parties and spending. Each voter may be of the “special interest” type S or of the “partisan” types L and R . Type S voters value government spending $x \in [0, b]$ on a targeted public good, which is financed through taxes imposed on all citizens; holding spending fixed, special interest voters are indifferent between the two parties. Type L partisan voters, in contrast, dislike spending x but have a fixed “ideological” attachment to voting for party L ; the preferences of type R for party R are symmetric. The assumption that type L and R are “captive” voter blocs is extreme, but it captures in a stark and simple way the differing salience of issues for the different voter groups. For type S voters, only spending and not ideological issues are salient in determining the voting decision, whereas for partisan voters only ideology is salient, no matter how high the preferred party’s spending promise. As we will, this assumption gives rise to the potential for a very high level of special-interest spending in equilibrium, even when S constitutes a small minority of the population.

The proportions of voters of types L , R and S are respectively denoted λ , ρ and σ , where $\lambda + \rho + \sigma = 1$. These proportions are uncertain ex ante and distributed according to some distribution function $H(\lambda, \rho, \sigma)$ that is known to parties. We assume that $H(\lambda, \rho, \sigma)$ is symmetric in (λ, ρ) , and that

$$\text{Prob}(\lambda > \rho + \sigma) = \text{Prob}(\rho > \lambda + \sigma) = \pi < \frac{1}{2} \quad (1)$$

and

$$\text{Prob}(\sigma > \rho + \lambda) = 0 \quad (2)$$

The first equation states that either voter group L or R may constitute a majority of the population with symmetric, positive probability. The second equation states that group S is almost surely a minority of the population. Thus our model is one of two symmetric political parties that compete for office by designing campaign promises that appeal to an unattached minority group.

The two parties compete for office in a first-past-the-post election by simultaneously making spending commitments (x_L, x_R) to be implemented if elected. Voters observe promises and vote sincerely. As noted, this implies that L and R voters supported their preferred parties, and voters of type S vote for L if $x_L > x_R$ and R if $x_R > x_L$. If $x_L = x_R$, each type S voter votes for each party with equal probability. Likewise, if the vote shares of the two parties are equal, then the outcome of the election is decided by a coin toss.

Parties are both office-motivated and policy-motivated. If a party promises spending x , its payoff is $v - x$ if elected and zero otherwise, where $v > 0$ is

a parameter indexing the perceived value of winning election relative to the alternative in which the competing party is elected. Thus v may be interpreted as representing the rents to elective office, or alternatively the degree of polarization on ideological issues between the two parties. We assume that $v - b \geq 0$, so that the payoff to election is non-negative even if the maximal spending $x = b$ is promised to special interest voters.

Since each party's vote share is increasing in its spending promise, and since the distribution of voter types is symmetric, this game admits exactly one candidate pure strategy Nash equilibrium, in which the two parties promise maximal special-interest spending

$$x^* = x_L^* = x_R^* = b \quad (3)$$

and each party wins election with probability one-half. The payoff to parties in the candidate equilibrium is therefore

$$u^* = \frac{1}{2}(v - b) \quad (4)$$

The pure strategy equilibrium may however not exist. Consider a deviation by party L from x^* to $x_L = 0$; since group L is in the majority and wins without the support of S voters with probability π , the payoff to the deviation is

$$u(0) = \pi v$$

A pure strategy equilibrium therefore does not exist when $v - b < 2\pi v$ or

$$v < \frac{b}{1 - 2\pi} \quad (5)$$

Given the symmetry of the parties, a natural alternative outcome of the game is an equilibrium in symmetric mixed strategies. To construct such an equilibrium, suppose that party R plays x_R according to some cumulative density function $F(x_R)$ with support on the interval $[0, k]$. It is evident that F must have positive, continuous density through the interval; otherwise, for the reason just argued, L would have a profitable deviation to a spending promise below the interval on which R is randomizing. Likewise, F cannot have a mass point for any $x < b$; otherwise L could deviate to a spending promise just above the probability mass in R 's strategy and increase the probability of victory.

Given the strategy played by R , suppose that L chooses a spending promise x . Party L wins the election if group L is in the majority, which occurs with probability π , or if neither partisan group is in the majority and $x > x_R$,

which occurs with probability $(1 - 2\pi)F(x)$. The expected payoff of x for L is therefore x is

$$u(x) = [\pi + (1 - 2\pi)F(x)](v - x)$$

The foregoing argument indicates that $x = 0$ is in the support of the equilibrium strategy and has payoff $u(0) = \pi v$, so that each party will randomize over strategies $x \in [0, k]$ only if $u(x) = u(0)$. Solving this identity for the distribution function that supports equilibrium randomization gives

$$F(x) = \frac{\pi}{1 - 2\pi} \frac{x}{v - x} \quad (6)$$

on $[0, k]$, where

$$k = \min \left\{ \frac{1 - 2\pi}{1 - \pi} v, b \right\} \quad (7)$$

Observe that when $v > b(1 - \pi)/(1 - 2\pi)$ the equilibrium strategy has a mass point at $x = b$.

The mixed strategy equilibrium places positive probability on spending promises $x < b$, and so it is immediate that $Ex < b = Ex^*$ in equilibrium. Furthermore, since F is decreasing in the parties' "ideological" payoff v , it follows that Ex is increasing in v in the mixed strategy equilibrium.

We summarize this as:

Proposition 1 *In equilibrium with first-past-the-post elections, special-interest spending is a non-decreasing function of party attachment v . For party attachment above a threshold, spending is maximal: $x^* = b$.*

2.1 Proportional representation

Our purpose is to briefly sketch a model of elections and legislative bargaining under proportional representation to which the equilibrium outcomes under first-past-the-post elections may be compared. Proportional representation is an electoral rule that specifies that parties are allocated seats in a legislature in proportion to their shares of the popular vote. Given legislative seat shares, a government is formed and special interest spending x is determined through a process of legislative bargaining and a majority vote of legislators.

Proportional representation creates greater incentives for parties to enter electoral competition than first-past-the-post. We posit that a third party S enters the election under PR and is supported by voters of type S . Partisan voters of types L and R continue to vote for their respective dominant

parties.² In the legislative phase, the seat shares of the three parties are equal to the ex post realization of the vote shares (λ, ρ, σ) . If neither dominant party is in the majority, then one must govern with the support of either the special-interest party or its dominant opponent, and the legislative compromise supporting the governing coalition may involve positive equilibrium special-interest spending $x \in (0, b]$. If however a dominant party obtains a majority, which occurs with probability 2π , then it forms a government without the support of another party, and implements its preferred special-interest spending $x = 0$ in equilibrium. It follows that, regardless of the details of the legislative bargaining game, expected equilibrium spending under PR cannot exceed $(1 - 2\pi)b$, compared to equilibrium spending of $x^* = b$ under FPTP when party attachment v is high. The reason is that the greater pre-electoral uncertainty under FPTP increases the expected influence of special-interests as pivotal voters.

This establishes quite robustly the main qualitative prediction that we take to the data: average spending is higher under FPTP (mayoral) elections than PR (council) elections when party attachment is high.

It is worthwhile demonstrating that the upper bound on spending under PR is tight. To do so we consider a simple version of one commonly-studied extensive-form model of legislative bargaining, the alternating-offer model due to Baron and Ferejohn (1989). In the legislative phase, if no party has a majority, then bargaining proceeds in a sequence of rounds. In each round, one of the three parties is recognized with equal probability to make a legislative proposal. A proposal consists of an “ideology” of the type favoured by voters of either type L or R , and a level of spending x . If the proposal is supported by at least one other party (and so commands majority support) then it passes, the policy proposal is implemented, and the game ends. If not, then the game proceeds to the next round, a party is again selected randomly to make a proposal, and so on.

A strategy for a party in this game consists of a legislative proposal to make if selected as proposer following any history, and a voting rule in response to any proposal at any history. As in the electoral game analyzed above, dominant parties L and R receive a payoff $v \geq b$ if their preferred ideology is adopted, less the cost of special interest spending x . Party S is indifferent to ideology, and its payoff is simply the level of spending that is implemented. There is no discounting of payoffs between bargaining

²Thus, for simplicity, the model assumes sincere voting by voters of all types. But it is easy to verify that in equilibrium the payoff to each voter type is non-decreasing in the electoral vote share of its respective party. The equilibrium is therefore robust to sophisticated voting behaviour.

rounds.

We seek to characterize a subgame perfect equilibrium of the legislative game with the following properties:

- Parties play stationary strategies, independent of history;
- Every proposal that is made in equilibrium receives majority support (on the first and subsequent rounds);
- L and R play symmetric strategies, proposing their respective preferred ideologies and a common level of special-interest spending \hat{x} ;
- S plays a symmetric mixed strategy, proposing the ideology supported by L (resp. R) with probability one-half, and proposing spending \hat{x}_S .

To construct such an equilibrium, calculate the continuation payoffs to each party given equilibrium strategies to be, for the special-interest party,

$$\hat{u}_S = \frac{2}{3}\hat{x} + \frac{1}{3}\hat{x}_S$$

since subsequent proposals will come from a dominant party with probability two-thirds and from S otherwise, and they will be accepted. For the dominant parties L and R , the continuation payoff is

$$\hat{u} = \frac{1}{3}(v - \hat{x}) + \frac{1}{3}(v/2 - \hat{x}_S)$$

since proposals from S randomize between the two ideologies.

Given continuation payoffs, each party's best-response voting rule is to support proposals that offer at least the continuation payoff and to vote against proposals otherwise. A best-response spending proposal is therefore, for S , the highest feasible x that is supported by one dominant party, implying $v - \hat{x}_S = \hat{u}$, or

$$\hat{x}_S = \min \left\{ \frac{3v + 2\hat{x}}{4}, b \right\} \quad (8)$$

A best-response spending proposal for a dominant party is likewise the lowest feasible x supported by S (and opposed by the other dominant party in equilibrium), implying $\hat{x} = \hat{u}_S$, or

$$\hat{x} = \hat{x}_S \quad (9)$$

It is easily verified that the unique equilibrium spending level, given by the fixed point of (8)–(9), is

$$\hat{x} = \hat{x}_S = b$$

Thus expected spending under PR is $E\hat{x} = (1 - 2\pi)b$ in this equilibrium. We may compare this outcome not only to the pure strategy equilibrium under FPTP, but also to the mixed equilibrium that uniquely exists when party attachment is low. In general, this comparison is ambiguous, but for low levels of ν and π we may establish that expected spending is in fact *lower* under FPTP than PR. To see this, note that when ν is below the threshold implied by (7), there is no mass point at $x = b$, and expected spending may be computed by integrating (6) to obtain

$$Ex^* = \left[1 - \frac{\pi}{1 - 2\pi} \log\left(\frac{1 - \pi}{\pi}\right) \right] \nu \quad (10)$$

under FPTP. For party attachment at its lower bound $\nu = b$, we may then calculate that $Ex^* < E\hat{x}$ iff

$$2 - 4\pi < \log \frac{1 - \pi}{\pi}$$

which holds for π sufficiently near zero.

We summarize this section with:

Proposition 2

- *If party attachment ν is high, expected spending is higher under FPTP than PR.*
- *If party attachment ν is low, the comparison is ambiguous in general. For ν and π sufficiently small, expected spending is higher under PR than FPTP.*

3 Electoral reform in Germany

In our empirical work, we examine a reform that introduced first-past-the-post election of mayors to local governments in Lower Saxony (Niedersachsen), a large industrial state in northwestern Germany.

In the period beginning with Allied occupation of Germany following World War II, Lower Saxony operated a council-manager system of local government, similar to that of Britain and some US cities. Elected local councils were the sole local decision-making body, and operated along parliamentary lines. Councils have a large number of elected members (varying in our data between 8 and 65 members). Elections occur every five years, and council members are elected from party-nominated lists of candidates

through a system of community-wide (single-district) proportional representation. National political parties play a strong role in local politics, and the institutions and practices of local councils largely echo the national parliamentary system of government (Kuhlmann, 2010). Under this system of government, the mayor served a relatively weak role in local government – the office was in fact an honorific title bestowed upon the chair of the council. Administrative matters were delegated to a chief executive officer, appointed for a term of twelve years and responsible to council.

In 1997, the state of Lower Saxony amended its municipal charter legislation to introduce direct election of mayors, chosen in community-wide first-past-the-post elections.³ As well as introducing first-past-the-post elections, the reform involved a substantial shift of power from council to the office of the mayor.⁴ Post-reform mayors in Lower Saxony remain voting members of the council, and also chair a steering committee of council known as the administrative committee. Mayors have the sole right of preparing resolutions of the council and the administrative committee, which gives them the power to set the agenda for debates on policy and personnel issues (Gissendanner and Kersting, 2005). Moreover, mayors directly control the administration of the municipal government and have the sole right to nominate municipal employees. Political scientists (e.g. Bogumil, 2001) regard the reform as having induced a fundamental shift in executive power to mayors and a decline in council's influence within the political system.

In short, the 1997 reform resulted in a significant devolution of power from the municipal council, elected under single-district proportional representation, to the mayor, elected under single-district first-past-the-post or majoritarian rules. Moreover, and key for our empirical approach, the new electoral system was phased in gradually among municipalities in Lower Saxony over a period of ten years, following expiration of the long-term contract with the municipality's chief executive officer. It is this feature of the reform that permits us to employ a quasi-experimental approach to estimating the effects of introducing first-past-the-post, in effect using observations on municipalities that had not yet reformed as controls for other,

³In our sample period, mayors were chosen through a two-round system of runoff elections, which was replaced in 2010 with a single-round system. For a discussion of single-round and runoff FPTP and local government policies, see Bordignon et al. (2010) and Chamon et al. (2009).

⁴The reform in Lower Saxony was part of a nationwide move to direct election of mayors during the 1990s. All states other than the three city-states now have "strong" mayors directly elected, up from two in the immediate post-war period (Gissendanner and Kersting, 2005).

time-varying unobservable factors that influence government spending over the reform period. It is important to note, therefore, that the order in which municipalities experienced the reform indeed appears to be random. In particular, there is no evidence that “early retirement” of chief executives influenced the timing of reforms at the municipal level.

Municipalities in Lower Saxony are large, modern administrations with broader responsibilities in delivering local public goods and social services than in many Western countries. A high proportion of local spending is devoted to social security (welfare) policy. Social security expenditures include cash and in-kind transfers (such as housing subsidies) targeted to youth, the needy, and elderly. Other local responsibilities include local public services and planning, law and order, and primary and secondary schools, responsibilities broadly similar to those of local governments in most other Western countries. Municipal spending is financed with a variety of taxes on local residents and businesses, and through transfers and revenue sharing arrangements with state and federal governments.

The 1990s were a period of fiscal retrenchment for all levels of government in Germany, including the municipalities, as governments coped with rising taxes and deficits and an economic slowdown following reunification. As part of the reforms, some traditional responsibilities of state governments were downloaded to municipalities, accompanied by substantial changes in taxes, transfers, and revenue sharing arrangements. These changes placed substantial new financial pressures on local governments, and expenditure control became a predominant focus of local governments and local politics (Kuhlmann, 2010).

The direct election of mayors was viewed by some actors as a means of dealing with legislative “gridlock” in fragmented municipal councils and so of speeding legislative response to fiscal problems (Wollmann, 2004). Thus, consistent with the “common pool” view discussed above, a primary objective of introducing directly elected mayors appears to have been to reduce or control the level of local government spending. As we will see, actual experience with the reform has been different.

4 Data and estimation strategy

In our empirical work, we use information on government spending, municipal elections, the characteristics of mayors, and economic and demographic characteristics of municipalities. Information on government spending for the 1992–2006 period is from official public accounts, obtained from the

State Statistical Office of the state of Lower Saxony. In cases where small municipalities have partially amalgamated their operations with neighbouring municipalities (*Samtgemeinden*), the public accounts reports spending for the aggregated jurisdiction. Our working sample consists of all 399 municipalities with population over 5000.

Summary statistics for the principal variables of interest are reported in Table 1. Total expenditures averaged 1533 euros per capita. Two functional categories of expenditure comprise a large part of the budget and which anecdotal evidence suggests are relatively discretionary and redistributive, and so which fit the predictions of our model particularly well. The first is Social Security expenditures, which includes services and transfers to youth and low-income families. Social security spending averaged 237 euros per capita, or about 15 per cent of the total on average. (This total includes spending on certain programs mandated by higher level governments but devolved to the municipalities and financed in part by grants-in-aid.) The second is spending on Administration and Miscellaneous categories, which together average 686 euros per capita or 45 per cent of the budget. On the revenue side of the budget, own-source tax revenues average 494 euros per capita or 33 per cent of the operating and capital budget for the average municipality, and consist of revenues from local property taxes and business income taxes (see Egger et al., 2010). Funding for the capital budget comes mainly from borrowing and withdrawals from reserve funds, over which municipalities have discretion, and through grants from upper-level governments; capital funding averages 360 euros per capita annually.

Information on party seat shares in municipal council elections was obtained from the state statistical office for the 1991, 1996, and 2001 electoral cycles. Partisan politics in German local government, as nationally, is relatively fragmented, with between one and four national parties electing members to council in our sample. The four national parties are the right-wing Christian Democratic Union (CDU), the centre-right Free Democratic Party (FDP), the left Social Democratic Party (SPD), and the Green Party (Greens). The average seat shares of the four are reported in Table 1. In addition to the four national parties, a small number of council seats have been held by members representing local electoral alliances (*Wählergruppen*) and independents, which are not reported in the table. On average, councils are dominated by the two main national parties, the SPD and CDU. But there is considerable variation in political leanings over time and among cities, with the FDP and Greens obtaining a plurality of council seats in a number of cases. As evidence of the fragmentation of party representation in councils, observe that a single party obtained a council majority in just

46.5 per cent of cases in our sample.

Our data on the date of the first mayoral election, and the political party and past work experience of mayors was obtained from a survey of local governments conducted by Scott Gissendanner and described further in Gissendanner (2005). Since the 1997 reform, mayoral elections have been dominated by the CDU and the SPD, the two main national parties, which have respectively held office in about 37 per cent and 32 per cent of the municipality-years since reform. But about 28 per cent of mayors in our sample report themselves to be independents (*Einzelbewerber*), running without affiliation to a national party. (The averages reported in Table 1 are smaller than these figures, since the full sample includes pre-reform years for which no mayor was elected.) Independent mayors are important to our empirical strategy since, as discussed below, we will use their presence or absence as a proxy for the degree of local voters' ideological attachment, which is the key to our theoretical results.

Our primary controls for factors influencing spending other than the electoral reform are a full set of municipal and year fixed effects in all regressions. Thus we estimate the effect of the reform as the average difference between spending changes in municipalities with first-past-the-post and proportional electoral systems at a point in time. In addition to fixed effects, we include controls for the population, per capita income, and the share of children and the elderly in the municipal population. These data are also supplied by the state statistical office, and estimates of local population and income are updated annually by the agency using a variety of administrative and survey data sources.

Our general empirical strategy is to estimate equations of the form

$$\text{SPEND}_{it} = \alpha_i + \delta_t + \beta \text{REFORM}_{it} + x'_{it}\theta + \epsilon_{it} \quad (11)$$

where SPEND_{it} is some measure of the logarithm of government spending per capita in municipality i and year t , α and δ are vectors of municipality and year fixed effects, and x_{it} is a vector of controls.⁵ The principal coefficient of interest is β , the coefficient associated with the dummy variable REFORM , equal to one in years following the first election of a mayor in the municipality, and zero otherwise.

The inclusion of year effects and municipality effects means that the parameters are identified by variation in spending within each municipality

⁵The control variables in all specifications are a cubic polynomial in municipal population, the logarithm of per capita income, and the share of youth and elderly. In subsequent specifications, we include additional controls for other time-varying economic and political characteristics of municipalities; see below.

over time. Thus, in our baseline specification, without controls, the estimated effect of the reform is simply the average change in log per capita spending in years following the reform, relative to the average change in other municipalities. Any time-invariant characteristics of municipalities will be captured by the municipality fixed effect, and will therefore not influence our estimates. Likewise, changes in spending over time common to all municipalities will be captured by the year fixed effects, and not by the REFORM variable.

The key to our empirical strategy is therefore the fact that introduction of directly elected was staggered across municipalities in the years following the 1997 reform in state laws. If municipalities were free to choose the date of their own first mayoral election, and if factors influencing the decision to implement the reform were correlated with the determinants of spending and spending growth, then estimates of the treatment effect from our specification (11) would be biased. In Lower Saxony, however, elections generally occurred on a fixed schedule, following expiry of the contract of the municipality with the administrative chief executive under the previous council-manager system. Since these contracts were of long duration, the date of expiry is unlikely to be correlated with the unobservable determinants of spending growth following the mayoral reform.

On this basis, there is reason to believe that the timing of reforms is “as good as random”, and estimates of the treatment effect of reform from (11) unbiased. To validate this idea, Table 2 presents information on the number of municipalities reforming in each year, and on the total spending average over the full sample period for the reforming governments, relative to a control group of all other municipalities. While relatively many municipalities did reform in each year between 1997 and 2004, the large number of reforms occurring in 2002 is unlikely to be random. One possible explanation is that councils may have terminated or briefly extended managers’ contracts in order that the first mayoral election would coincide with the 2001 council elections (in which case we code the first year under the new mayor to be 2002). The spending data show that average spending by early and late reformers was broadly similar, suggesting that self-selection is not a significant concern. However, municipalities reforming in 2000-1 do have significantly higher, and those reforming in 2003-4 significantly lower, average expenditures over the full sample period than the others.

Loosely speaking, self-selection of this kind poses a problem for estimation only if its determinants are correlated with spending growth around the date of reform. To provide further evidence on the issue, Figure 1 graphs the average spending in each broad cohort of reformers for all years in the

sample. If treatment were random, and if the number of municipalities were large, then the graphs of average spending in each cohort would roughly coincide up to the reform date, and would only diverge thereafter due to the treatment effect of the reform. The data in Figure 1 fit this pattern for all cohorts other than the 2000-1 and 2003-4 cohorts. But even in these cases, while the level of average spending differs substantially, the pattern of year to year variation up to the reform date remains similar to the other cohorts. It therefore seems plausible to argue that any self-selection in implementing the reform around the 2001 election year was a level effect rather than a growth effect. For example, average spending in the sample is strongly correlated with municipal population. One possible explanation is that small municipalities were more likely to extend managers' contracts expiring just prior to the 2001 election, or terminate early contracts expiring just after it, in order to economize on the expense of a separate mayoral election process. Since we control for municipal population and municipal fixed effects, such sources of self-selection would have no impact on our estimates of the effect of the reform.

5 Results

Table 3 reports results for the regression model (11) using aggregate total expenditures as the dependent variable. In this and subsequent tables, standard errors are based on the Huber–White sandwich estimator of the covariance matrix, implemented using Stata's robust command. The first column of the table reports the simple difference-in-difference estimates of the reform effect, in which control variables other than the fixed effects are excluded. The coefficient estimate of 0.017 in column (1) implies that expenditures rose about 1.7 per cent on average in municipalities with a directly elected mayor, compared to those still governed by a council elected under proportional representation. The estimate is significant at the 99 per cent level, but the effect is fairly small, representing about 26 euros per capita per year for the average municipality.

Our theoretical model suggests that the comparison between spending under proportional and first-past-the-post systems is ambiguous in general, so that the positive effect of mayoral reform in Table 2 cannot be construed as supporting or rejecting the theory—although the result is at odds with results from cross-country comparisons reported by Persson et al. (2007) and others. Our theory however implies that mayoral reform increases spending when voters' ideological attachment to incumbent parties is strong, and

it decreases spending otherwise. Naturally, it is difficult to obtain direct measures of the partisan attachment of voters in each of the 399 municipalities in our sample. As a proxy for partisan attachment, we exploit the presence of independent mayors—unaffiliated with any of the national political parties—in the data. We hypothesize that ideological attachment is smallest among voters in municipalities that elect independent mayors. We therefore interact the REFORM variable in equation (11) with a dummy variable for independent mayors, in effect estimating the impact of the reform separately for the two classes of municipalities. Our hypothesis is that the reform effect should be positive for municipalities with partisan mayors, and of ambiguous sign for those with independent mayors.

In column 2 of the table, the difference-in-difference effect of mayors is estimated separately for partisan and independent mayors. Consistent with our hypothesis, the reform had a larger significant effect on spending when a Party mayor was elected, with an estimated effect of 0.029. When the mayor elected was an independent, the effect on spending was significantly lower. In fact, summing the two coefficients indicates that spending declined on average with independent mayors, although the combined effect is not significant at the 95 per cent significance level. Adding economic and demographic controls for spending has little impact on the estimated treatment effects, as reported in column 3 of the table. The signs of the estimates for the control variables are largely as expected. Spending per capita is decreasing in populations up to about the median of the distribution, and increasing thereafter. This likely reflects the competing effects of economies of scale in local services and of greater devolution of responsibilities from the state government to larger communities. Spending is increasing in the young population, which reflects spending on schools and programs targeted to youth.

Changes in spending may lag changes in government, and so it is interesting to estimate the dynamics of the reform effect. One approach is to estimate a separate average treatment effect for each year since the date of the reform. In Figure 2, these estimated reform effects for partisan mayors are depicted (the solid line), together with the 95 per cent confidence interval around the point estimates (the dashed lines). The corresponding estimates for independent mayors, which are centred around zero and generally insignificant, are omitted for clarity. The effect of mayors generally rises with the number of years since the reform, with peaks in the fifth and tenth (last) years, and the estimate is significantly different from zero in most cases, albeit with widening confidence intervals for later years, for which there are relatively few observations. The mildly non-monotonic pat-

tern in Figure 2 is consistent with an electoral cycle in spending, since the fifth and tenth years after the reform are election years for mayors.

The evidence in support of our model adduced from the comparison and partisan and independent mayors is admittedly indirect. The election of an independent to the mayoralty may reflect factors other than ideological preferences of voters, and it might in principle even be endogenous to contemporaneous changes in the fiscal environment of the municipality. Such considerations, if predominant, would bias our estimates and invalidate our approach. We note however that in some German states with longer histories of directly elected mayors, candidates are habitually affiliated with national parties, while in other states they are not. In the short history of mayoral elections in Lower Saxony, such a convention has yet to be established. In that environment, it is plausible that partisan candidates stand for election and win in municipalities where voters' attachment to national political parties is strongest.

5.1 Controlling for political factors

In Table 4, we report estimates from a number of specifications that control for political factors that, while absent from our theory, may in principle be driving spending decisions in a way that biases our results.

One alternative explanation for our results may be that voters' tastes for government may simply have shifted over time and among municipalities in a way that was correlated with the implementation of the reform. To control for this, we include in column 1 as additional control the council seat share of the four national political parties. (These shares do not sum to one, since other smaller parties and city-specific voter groups do win council seats, although only rarely the mayoralty.) The estimates of the reform effects are essentially unchanged.

Another possibility considered in Table 4 is that our results may reflect the political tastes of one party, rather than a general difference between the behaviour of partisan and independent mayors. To investigate this, we interact the reform variable with indicators for mayors from the centre-left SPD and an aggregate of smaller national and local parties⁶ In this specification, as reported in column 2, the estimated coefficient on the Reform variable of 0.021 represents the average effect of mayors of the centre-right CDU. The estimates show that there is no significant difference in the aver-

⁶Other national-party mayors are from the FDP, a centre-right party, and the Greens. The individual reform effects for the two parties are similar.

age effect of mayors from the main parties of the left and right, the SPD and the CDU, although mayors from the smaller parties do spend significantly more. Most importantly, independent mayors do behave in a way that is significantly different from partisan mayors.

Another possibility is that the difference between partisan and independent mayors reflects the way they work with councils, rather than differences in the electoral incentives. In some cases, partisan mayors serve alongside a council with a plurality of representatives from their own party, while in others there is divided government in the sense that the mayor's and council plurality party are different. In contrast, independent mayors always operate in divided government in this sense, as they do not nominate a slate of candidates in the council election. In column 3, we add to our baseline specification an indicator for cases in which a partisan mayor faces a divided government; if the hypothesized effect is at work, then the effect of divided partisan government should be similar to the effect of independent mayors. While the estimated coefficient on divided government is indeed negative, it is small in magnitude and insignificant, allowing us to reject the hypothesis that the partisan mayor effect merely captures the effect of non-divided government.

In some cases, previously appointed city managers were elected as mayors, and managers are more likely to win office as independents than as partisans in our sample. If previous managers act differently as mayors than others, then the estimated difference in spending under partisans and independents may simply reflect the effect of managerial experience. Our data include an indicator for sitting mayors' past experience as a city manager, which is included as a separate control in the final column. While previous managers spend less than others, the difference is not significant, and the estimated reform effects are essentially unchanged.

As noted, a key to our empirical strategy is the assumption that the election of an independent rather than party-affiliated mayor is a valid proxy for low partisan attachment among voters in the municipality. One way to validate that assumption is to examine municipal voting patterns in concurrent statewide elections. Under the German system, each voter casts two ballots in elections for state (and national) legislatures. The first ballot is for an individual candidate in an electoral district, for which the seat is allocated on a first-past-the-post basis. The second ballot is cast for a political party, and additional seats in the legislature are allocated in such a way that each party's seat total is proportional to its total votes.

If municipalities electing independent mayors do indeed display weaker party attachment than those with partisan mayors, then voting in statewide

elections there should also show more evidence of a “personal vote” for individual candidates rather than for parties and party labels of candidates. To investigate this possibility, we obtained municipal-level data on vote shares of the main national political parties in the five statewide elections from 1990 to 2008. Summary statistics for the data are presented in Table 5, with the relevant sample means reported separately for the municipalities that ever elected independent mayors and those that always elected party mayors. (Municipalities not experiencing a mayoral election in our sample period are excluded.)

The first two rows of the table report the mean vote share of the two dominant national parties (the CDU and SPD) on the two ballots. On the second, party ballot, the mean vote shares are nearly the same in municipalities with independent and partisan mayors (82.2 per cent versus 82.5 per cent, $p = .137$), indicating that the two groups of voters did not have significantly different preferences for large and small parties than those electing party mayors. However, those electing party mayors did allocate significantly more of their first ballot votes to candidates of the two dominant parties (85.9 per cent and 86.8 per cent, $p = .001$). In this sense, party preferences appear to influence the evaluation of particular candidates more in places that also elected partisan mayors in place of independents. The same patterns appear somewhat more starkly when examining the probability that voters allocated a majority of votes to a single party on either ballot, as reported in the next two rows of the table. A single-party majority on the party ballot (always CDU or SPD) occurred in 36.0 per cent of cases where independents were elected mayor and insignificantly more often where partisan mayors were elected. A majority was common on the individual ballot, perhaps reflecting the greater name recognition of dominant-party candidates. But the higher probability is especially pronounced where voters chose partisan mayors ($p = 0.059$), suggesting that party labels matter more to the evaluation of individual candidates there. A final, somewhat more direct, approach is to compare the sample correlation between individual and party votes for the two dominant parties across municipalities and over time. Again, the hypothesis is that where party attachment of voters is weaker, the correlation will be smaller, as individual attributes of candidates uncorrelated with party are more likely to influence individual votes. Consistent with this idea, we find that, while the correlation is high in both subsamples, it is significantly greater in municipalities electing a partisan mayor.

5.2 The pattern of spending and revenue

Our theory suggests that first-past-the-post elections increase competition for swing voters when attachment to the dominant parties is high. A further test of the theory is therefore to examine whether the estimate impacts on spending are especially pronounced in subcategories of expenditure that may plausibly be more easily targetted to specific groups of voters, compared to general public goods and public services that affect most voters in similar ways. In Table 6 we report estimates of the corresponding treatment effects for specific subaggregates of expenditures and revenues. While all regressions include the political and demographic controls of Table 4, for brevity we report only the coefficients on the Reform variable for partisan and independent mayors, as well as the effect of previous managerial experience.

Social Security expenditures are the most clearly redistributive category, with the bulk of spending devoted to social service agencies and transfers to poor families and youth. The estimated effects of reform are larger for Social Security expenditures, with election of a partisan mayor approximately associated with a 4.1 per cent increase in average expenditure, and (summing the coefficients) of an independent mayor with a 6.8 per cent decrease. Past city managers increase Social Security spending by 3.6 per cent, although the estimate is marginally insignificant.

Next we examine spending on General Administration and Miscellaneous categories. Administrative expenditures is a frequent target for discretionary reductions in government spending, and it may capture the debate over rents to public-sector workers that often figures prominently in local electoral competition. The results in column 2 show that partisan mayors increased expenditures on this category by about 2.5 per cent; independent mayors spent significantly less, and their effect on Administration was essentially zero.

The focus on Social Security and Administrative expenditures is somewhat arbitrary, but these categories appear to fit the notion of special-interest spending reasonably well. A related approach, pursued by Milesi-Ferretti et al. (2002) using cross-country national accounts data, is to focus on government spending on transfer payments as opposed to goods and services as the measure of government's redistributive activity. The closest analogue to that approach for our data is the Social Security category, which comprises a mix of cash and in-kind payments to needy and elderly local residents.

To provide contrast, we next examine effects on aggregate spending on

public infrastructure and public services,⁷ categories of spending that are arguably least targeted to specific groups of voters. While the sign of the partisan and independent reform effects are the same as before, the estimated effects are not significantly different from zero. Thus we find that the spending effects of the mayoral reform are concentrated in redistributive spending categories rather than general public services.

The final two columns address the revenue side of the budget. German municipalities have considerable discretion over own-source tax revenues, and they may borrow to finance capital expenditures, but the operating budget must balance on an annual basis. To finance increased spending, partisan mayors relied mainly on funding for the capital budget, which consists of borrowing in credit markets, withdrawals from reserve funds, and grants from upper level governments. Independent mayors raised significantly less in capital funding, and they also reduced own-source revenues, relative to other municipalities.

5.3 Alternative hypotheses

Our theoretical model emphasizes the role of special-interest spending in supporting a winning electoral coalition under first-past-the-post elections, whereas some prominent competing theories focus on the corresponding difficulties in maintaining legislative coalitions under proportional representation. The next two tables of results report some simple tests of implications of the alternative theories using our data.

One influential view in the literature is that proportional representation increases the number of “veto players” in legislative bargaining games. In the work of Tsebelis (1995), veto players are political parties that are actual or potential members of the governing coalition, which may block specific policy proposals from passage through the legislature. Consequently, the rise in the effective number of parties under proportional representation results in a potential for “legislative gridlock” and status quo bias in policy. In a fiscal environment of rising costs and declining revenues, status quo bias may result in higher levels of government spending (Alesina and Drazen, 1991). Related, Coate and Knight (2009) study a veto player model in which the status quo is effectively zero spending, and they show that the introduction of a mayor with veto powers over council decisions results in a decrease in spending.

⁷This is the sum of spending in the public accounts on Streets and Housing, Sewage Treatment, Waste Collection, and Water Supply.

If the main effect of the electoral rule is on the number of veto players in government and hence on status quo bias in policy, then the effects of the German reform on spending should be largest (whether positive or negative) in municipalities whose councils exhibit the highest degree of party fragmentation. Since municipalities faced increasing fiscal pressures over the reform period and control of spending growth was a major concern for many governments, the veto player hypothesis suggests that introduction of a mayor should reduce spending most in highly fragmented councils. Indeed, this reasoning was a primary motivation behind the reform (see Section 3).

We measure fragmentation by the Herfindahl index of the seat shares of the four national parties, and we classify municipalities as being of high, medium, or low fragmentation based on values of the index for the council in place in the current year. The effect of pre-reform fragmentation on the reform is then estimated by interacting our Reform dummy variable with dummy variables for in the top and bottom quartiles of the distribution. (The excluded category is therefore fragmentation in the interquartile range.) Column 1 of Table 6 reports results for this specification, where the dependent variable is again taken to be log total expenditures per capita, and the regression includes the economic and political control variables from column 1 of Table 4. (These coefficient estimates are not reported for the sake of brevity.) In the specification, we also control for the direct effect of fragmentation on spending by including a linear term in the fragmentation index for the current electoral cycle.

Results are inconsistent with the veto player hypothesis. The results show that the effect of the mayoral reform was in fact to increase spending significantly where fragmentation of councils was in the top quartile. The reform effect is also positive in the bottom quartile of fragmentation, while in the middle quartiles it is effectively zero. In short, while the reform did operate differently in highly fragmented councils, the direction of the effect is the opposite of that predicted by existing veto player theories, and the effect is not monotone in fragmentation.

Other, related theories emphasize that coalition governments, prevalent under proportional representation, may behave differently than those formed by a single electoral party in a first-past-the-post system. In the model of Persson et al. (2007), proportional representation leads to more parties represented in the legislature, and legislative bargaining among minority parties in the governing coalition is subject to a “common pool problem” that leads to higher redistributive spending than pursued by the single party that governs under majoritarian electoral rule. Thus, as the authors

emphasize, in this view, “the electoral rule affects government spending, but only indirectly: proportional elections induce a more fragmented party system and a larger incidence of coalition governments than do majoritarian elections.”

If the main (indeed, only) effect of the electoral rule is on the incidence of coalition government, then the reform introducing first-past-the-post elections should decrease spending in municipalities where there is no majority in council, but it should have no impact where a majority party exists in the legislature and can govern without need for coalition bargaining. To test this possibility, we interact our reform variable with a dummy variable equal to one in municipalities and years where no party holds a majority of council seats (“No Council majority”) and equal to zero otherwise. In this formulation, under the Persson-Roland-Tabellini hypothesis, the direct effect of Reform on spending is predicted to be zero, and the interaction with No Council majority is predicted to be negative.

The second column of Table 6 reports the results for this specification. Inconsistent with the hypothesis, the estimated coefficient for Reform*(No Council majority) is in fact significantly positive. Thus while the effect of the reform is concentrated in municipalities without a legislative majority, the effect is to increase rather than decrease spending. Thus there is effectively no evidence in the data that the effect of the reform was to reduce spending undertaken by minority councils.

6 Conclusion

There is a general perception that proportional representation leads to greater government spending, particularly of a redistributive kind. One broad class of mechanisms that generate such results, explored by a number of authors, is that PR leads to a greater number of effective parties in the legislature (Duverger’s Law) and so to more coalition governments, more veto players, and more government resources devoted to maintaining coalition stability. Our framework offers a more nuanced view of these comparisons. In the model, competition between dominant parties in first-past-the-post elections can enhance spending on special interests, since electoral uncertainty increases the ex ante probability that special interest voters will be pivotal in the election, even when special interests are not pivotal ex post in the legislative bargaining game. When ideological differences among voters are sufficiently strong, electoral competition between dominant parties is intense, and higher equilibrium spending on special interests results. In con-

trast, when ideological differences are small, costly competition for special interest voters is limited, and equilibrium spending under first-past-the-post may be smaller than under proportional representation.

These ideas have some support in our empirical application, which investigates changes in government spending after a (phased-in) reform in local electoral rules in Germany. Since mayors are elected in city-wide first-past-the-post elections, and they supplanted municipal councils elected from party lists based on a system of single-district proportional representation, the reform in Lower Saxony is an apt laboratory in which to study the differential effects of the two electoral systems. Important for this interpretation, and for our empirical strategy, is the quasi-experimental nature of the reform, which was phased in gradually among municipalities in the state over a ten-year period. In our data, we find in our data that spending rises under first-past-the-post when it is followed by the election of a mayoral candidate affiliated with a national political party, but not for non-partisan mayors. The effect is relatively small in aggregate but significant, and it is especially pronounced for Social Security and some other categories of redistributive spending.

In the context of established theories, our empirical results are rather surprising. Previous evidence from cross-country comparisons (largely cross-sectional) of electoral systems, has led researchers to conclude that proportional representation is typically associated with higher levels of redistributive spending, perhaps because of the greater incidence of coalition and minority governments in PR systems. Our evidence is manifestly inconsistent with that view, and specific tests based on our data appear to reject the “veto players” and coalition-bargaining hypotheses.

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Table 1: Summary statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Total expenditure	5,970	1,533	466.8	730.5	5,737
Administration and miscellaneous	5,970	686.0	275.6	209.0	4,647
Social security	5,970	236.8	151.3	29.92	1,325
Public services	5,970	293.8	136.0	36.03	1,016
Tax revenue	5,970	494.0	168.6	-86.02	2,473
Capital funding	5,970	359.6	190.9	22.18	2,563
Controls:					
Income per capita	5,174	9,474	3,265	0	24,752
Population (00,000)	5,970	0.194	0.332	0.0502	5.258
Share age 0-18	5,970	0.173	0.0229	0.0921	0.292
Share age 65+	5,970	0.167	0.0325	0.00342	0.330
Council seat shares:					
SPD	5,988	0.393	0.126	0.0588	0.714
CDU	5,988	0.464	0.141	0	0.941
FDP	5,988	0.0350	0.0474	0	0.316
Greens	5,988	0.0417	0.0388	0	0.207
Council Majority	5,988	0.465	0.499	0	1
SPD Mayor	5,988	0.125	0.331	0	1
CDU Mayor	5,988	0.118	0.322	0	1
Other Party Mayor	5,988	0.0119	0.108	0	1
Independent Mayor	5,988	0.0992	0.299	0	1

Table 2: Timing of electoral reforms

Year	Average spending of:		
	Number of Reformers	New Reformers	Others
1997	55	1494.3	1473.9
1998	20	1565.6	1512.4
1999	39	1462.3	1549.6
2000	30	1651.9	1525.1
2001	12	1718.4	1510.9
2002	137	1547.6	1489.2
2003	11	1310.8	1502.2
2004	16	1262.5	1497.4
2005	7	1531.6	1500.2
2006	2	2904.5	1546.1

Figure 1: Total spending by year and reform year

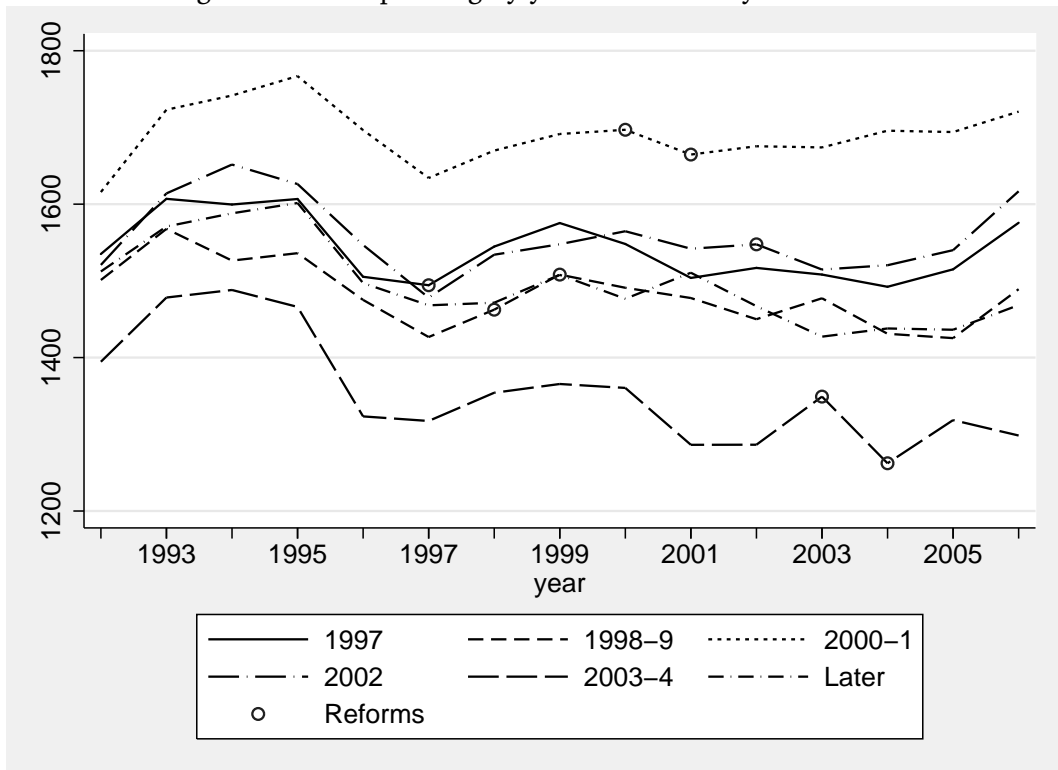


Table 3: Estimated effect of electoral reform on total spending

VARIABLES	(1)	(2)	(3)
Reform	0.017*** (0.006)	0.029*** (0.007)	0.027*** (0.007)
Reform*Independent		-0.045*** (0.009)	-0.042*** (0.009)
Population			-4.746*** (0.448)
Population squared			1.194*** (0.277)
Population cubed			-0.104*** (0.034)
Log of income per capita			-0.068 (0.043)
Share age 65+			0.220 (0.305)
Share age 0-18			0.621** (0.293)
Observations	5,970	5,970	5,970
R-squared	0.750	0.752	0.760

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All regressions include country and year fixed effects.

Robust standard errors in parentheses.

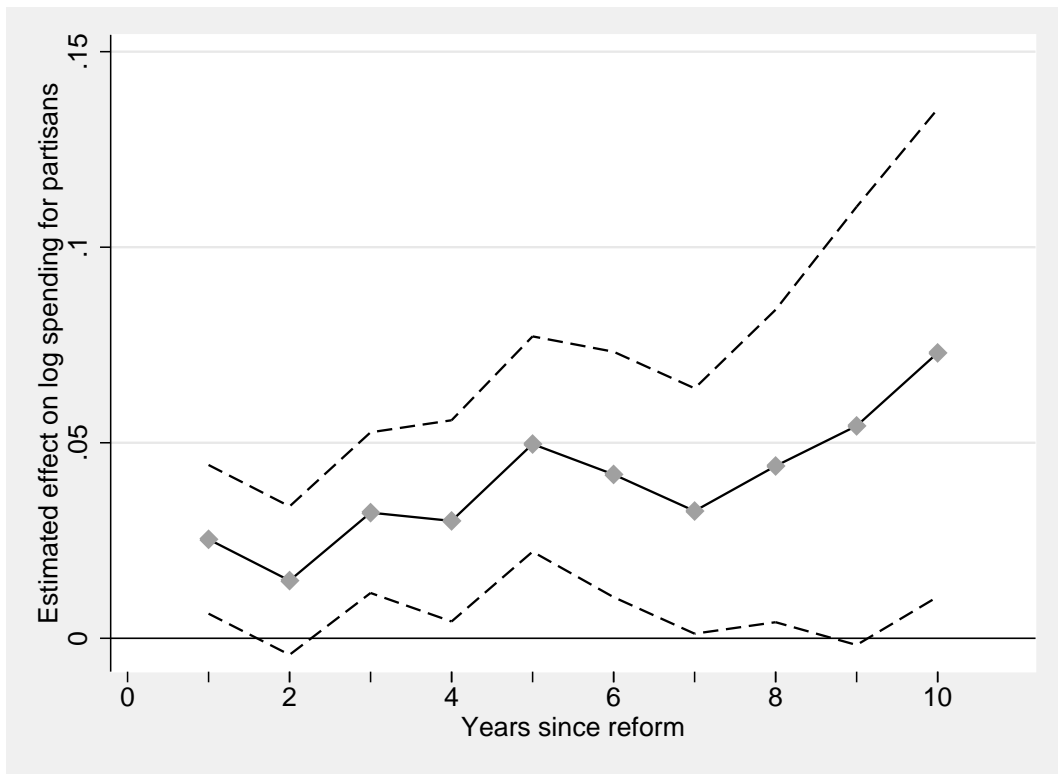


Figure 2: Estimated treatment effect of partisan mayors over time

Table 4: Controlling for political factors

VARIABLES	(1)	(2)	(3)	(4)
Reform	0.027*** (0.007)	0.021** (0.009)	0.029*** (0.008)	0.030*** (0.007)
Reform*Independent	-0.042*** (0.009)	-0.037*** (0.011)	-0.044*** (0.010)	-0.042*** (0.009)
SPD Mayor		0.003 (0.010)		
Other Party Mayor		0.080*** (0.027)		
Reform*Divided			-0.006 (0.010)	
Manager				-0.012 (0.010)
Council seat shares:				
SPD	-0.021 (0.066)	-0.008 (0.066)	-0.023 (0.066)	-0.018 (0.066)
CDU	-0.051 (0.069)	-0.020 (0.070)	-0.052 (0.069)	-0.046 (0.069)
FDP	0.034 (0.103)	0.032 (0.102)	0.034 (0.103)	0.032 (0.103)
Greens	0.079 (0.131)	0.080 (0.132)	0.087 (0.131)	0.083 (0.131)
Observations	5,970	5,970	5,970	5,970
R-squared	0.760	0.760	0.760	0.760

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All regressions include country and year fixed effects.

Robust standard errors in parentheses.

Table 5: Independent mayors and state-level voting patterns

	Party Mayor	Independent Mayor	Difference (<i>p</i> -value)	
Dominant party vote shares:				
- Party vote	0.825	0.822	(0.137)	
- Individual vote	0.868	0.859	(0.001)	***
Single party majority:				
- Party vote	0.388	0.360	(0.150)	
- Individual vote	0.521	0.479	(0.059)	*
Correlation coefficient:				
- Party and individual vote	0.975	0.968	(0.001)	***

Table 6: Patterns of spending and revenue

VARIABLES	(1) Social Security	(2) Administration	(3) Public Services	(4) Tax Revenue	(5) Capital Funding
Reform	0.041*** (0.013)	0.025** (0.011)	0.017 (0.016)	-0.006 (0.007)	0.048** (0.022)
Reform*Independent	-0.109*** (0.019)	-0.027** (0.013)	-0.023 (0.020)	-0.021** (0.009)	-0.102*** (0.026)
Manager	0.036* (0.019)	-0.012 (0.015)	-0.019 (0.021)	0.030*** (0.009)	0.012 (0.028)
Observations	5,970	5,970	5,970	5,969	5,970
R-squared	0.794	0.595	0.667	0.818	0.487

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

All regressions include country and year fixed effects, as well as the political and demographic control variables of the preceding table. Robust standard errors in parentheses.

Table 7: Alternative hypotheses

VARIABLES	(1) Total	(2) Total
Reform	0.002 (0.007)	-0.001 (0.008)
Reform*Low fragmentation	0.023** (0.010)	
Reform*High fragmentation	0.040*** (0.010)	
Fragmentation	-0.198 (0.149)	
Reform*No Council majority		0.030*** (0.009)
No Council majority		0.002 (0.008)
Observations	5,970	5,970
R-squared	0.760	0.759

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All regressions include country and year fixed effects, as well as the political and demographic control variables of the preceding table. Robust standard errors in parentheses.