Do parties matter?
Estimating the effect of political representation in multi-party systems

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Abstract: This paper estimates the causal effect of political representation in local governments on tax policy in municipalities under a proportional election system. The main challenge in estimating the causal effect of parties on policy is to isolate the effect of representation from underlying voter preferences and the selection effect of parties. We use an instrumental variable approach where close elections provide the exogenous variation in our measures of representation: seat shares and voting power. Using data from German municipalities our estimation results suggest that representation does matter. The effects are mostly small, but statistically significant. Somewhat surprisingly, the center-left party is found to lower the local taxes, whereas The Greens increase the property tax considerably. These effects remain robust to weighting voting power by the likelihood of coalitions and different definitions of close elections and the instrument.

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JEL classification:	H10, H11, H77

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1 Introduction

Does political representation matter for policy? In a majoritarian system, this question is usually equivalent to: does it matter who wins the election? The reason is that after Duverger's law a two-party system is likely to emerge. In a proportional election system, however, the question is often more complicated. We frequently observe a multitude of parties and winning an election is not as clear a concept as in a majoritarian system.

There are two reasonable measures of political representation for a multi-party system, both of which are equivalent in a two-party system. The first is seat shares. In a two-party system, the party with the higher seat share is the winner. In a multi-party system the seat shares of parties do not necessarily indicate the winner of an election: the party with the highest share may well find itself in the opposition to a coalition of smaller parties. Nevertheless, the seat share gives us a measure of representation in the legislature of the party in question.

The second measure is voting power.[1] Voting power can be investigated from two different angles, a policy-oriented and an office-motivated perspective. Since we are interested in the policy decisions of a legislature, we will use the purely probabilisitc measure of voting power after Penrose (1946) and Banzhaf (1965).[2] In a two party system, voting power will either be one or zero, depending on who has the higher seat share. Thus, there is a simple mapping of seat shares on voting power. For multi-party systems on the other hand, voting power may differ substantially from seat shares. As an introductory example, consider three parties, where two big parties have 48% each, and the third party has only 4%. Their relative voting power will be one third each because each party is equally useful in forming a winning coalition.

In the current paper, we estimate the causal effect of seat shares and relative voting power of parties on policy outcomes in a municipal council. Three key results emerge. First, there certainly are correlations between seat shares or voting power on the one hand and policy outcomes like taxes and spending on the other. However, these OLS results are misleading because the causal estimation shows different results. Second, political representation measured in seat shares and voting power does affect policy. And third, the political influence of small parties depends on measuring the voting power of these parties rather than approximating power with seat shares.

[1] See Felsenthal and Machover (1998) for details on voting power. The exposition here is based on their book.

[2] The office-motivated measure is the game-theoretic index after Shapley and Shubik (1954) which measures the power to aquire a share of a price, e.g. an office.

Estimating the causal effect of political representation on policy crucially depends on overcoming a fundamental endogeneity problem: the correlation of political representation with (unobserved) voter preferences. The observation that, say, a pro-business party obtains 3 seats or a certain amount of voting power in a town council and the local business trade tax is observed to be below average does not imply causality of political representation of the liberal party on tax rates. Ideally, we would like to run an experiment, where we assign political representation randomly to municipalities. In the perfect experiment, voter preferences are similar on average in treatment and control group and we could measure the causal effect of a higher seat share or more voting power for a specific party. This is (and should be) impossible.

In the absence of experimental data, a new literature has emerged in political economics that applies techniques from the program evaluation literature to draw valid inference from observational data. Pettersson-Lidbom (2008) and Lee, Moretti, and Butler (2004) were the first to use a regression discontinuity design (RDD) to estimate the effect of parties on policy. Lee, Moretti, and Butler (2004) estimate the effect of party affiliation in the US House of Representatives on policy. Under the assumption that vote outcomes are subject to some randomness, they argue that the party affiliation (Democrat or Republican) of a representative of a district is a quasi-random event if the margin of victory was very small. They show significant effects of party affiliation on the voting record of the representatives. Pettersson-Lidbom (2008) studies the policy effects of having a left-block majority (of one or more parties) in Swedish municipalities. The identification is also a RDD at the 50% vote margin. He finds significant effects of block majorities on tax rates and different spending categories. Ferreira and Gyorko (2009) investigate the effect of having a Democratic or a Republican mayor in office in US cities on fiscal outcomes in a similar RDD framework. They find effects of the major on policy only if Tiebout competition between municipalities is weak.

In all these studies, the threshold is the absolute majority of the votes that is needed to gain control of a certain political office or institution. The political setting in all studies is a two-party majoritarian system or an assumed two-block party structure in a proportional system. This has two consequences for the research design. First, there is a simple mapping of seat shares on voting power, as outlined above. Second, it is possible to use a simple RDD framework at a fixed threshold. While our research design also relies on close elections, we are specifically interested in different measures of political representation that are not only binary variables of winning or losing. Moreover, in proportional election systems blocks of parties are very difficult to define, especially in local politics. Finally, if we take the proportional system seriously, there are no fixed thresholds at which a party gains or loses a seat or voting power.

Folke (2010) was the first to estimate the effect of seat shares in Swedish municipalities on policy outcomes under a proportional election system. He finds interesting results for the green party, which has an effect on local environmental policy, and an anti-immigration party, which he shows to have an effect on local immigration policy. He finds no effect of seat shares on taxes. A study that uses a related identification design is Liang (2009). He estimates the effects of party representation on *political* outcomes in subsequent local elections. The two incumbency effects that he investigates are the effect of holding a seat and the effect for each member of the council to be part of the government coalition. He finds a positive incumbency effect of holding a seat, but no incumbency effect of being part of the government.

Identification in Folke (2010) stems from observations in which a certain seat allocation was very close: he determines counterfactual observations as those in which, for example, party A was either very close to winning or very close to losing a seat. By assuming that such a close seat allocation is the result of an as-good-as-random event, he solves the endogeneity problem of political representation and is able to isolate a causal effect of seat shares on policy.

In our analysis, we generally build on the estimation strategy developed in Folke (2010). However, we investigate both seat shares and voting power and show that seat shares have no effect while voting power does. Second, we outline a conceptual framework of randomness in elections. This conceptual framework leads naturally to a different method to identify close seat allocations. We shortly describe this method in the following for the purpose of introduction.

In general, a certian seat allocation depends on both, the council size and the entire vector of votes of all parties. Both are arguments in the seat allocation function that translates votes into seats. Folke (2010) uses an alogrithm based on this seat allocation function to calculate the minimal vote change that is required for a seat to change. This is done for every party in the council. This minimal vote change is his measure of closeness of a seat allocation for a specific party.[3]

In our method, we repeatedly perturb the vote vector for each observation by adding a random variable to the votes of each party. Then we simulate the seat allocation and voting power and observe whether they change. Observations whose seat allocations

[3]We will discuss this in more detail in section 3.2.

or voting power change often are considered close and are used for identification for the respective parties. Those observations for party A with repeated seat *gains* during the perturbations will receive a *negative* treatment because they were close to gaining a seat but did not receive it in the actual seat allocation. Positive treatment is assigned accordingly. This treatment variable is then used as an instrument for seat shares. For voting power, we calculate the average gain or loss of voting power when voting power does change. The instrument for voting power is then given by combining this average with the number of perturbated simulations in which it did change.

We have compiled a new data set that combines information on both election outcomes and fiscal data at the municipal level in Germany. As of now, we have complete data for the two recent municipal elections in four out of 13 German states: Thuringia, Bavaria, Hesse and North Rhine-Westphalia.[4] In the current analysis, we use the results of about 7200 independent elections and subsequent fiscal outcomes. The election data contains information on total votes and seats for all parties. For the fiscal variables, we have yearly information on the three local tax rates (trade tax as well as property tax A and B) and the aggregate spending data for each municipality.

The paper is organized as follows. In section 2, we outline the electoral rules and give some background on the responsibilities of German municipalities. Section 3 presents our empirical strategy, including the methodological background on how to define close elections and how to calculate voting power. The data is presented in section 4, section 5 contains the results before the analysis is concluded in section 6.

2 Institutional Background

In this section, we describe the seat allocation functions that are used in German municipalities and give some institutional background on local politics in Germany.

2.1 Elections and electoral rules

In Germany, there are four tiers of government: federal, state, county and municipality. Our focus is on municipal elections and policies. In municipalities, the legislative body is the council (*Gemeinderat* or *Stadtrat*, depending on the size). It is elected every 5 years[5] in a proportional election system, sometimes with elements of a mixed

[4]The three states Berlin, Bremen, and Hamburg are city-states where municipality, county and state coincide. In total there are 16 German states. We have to restrict the analysis to those four states due to limitation in data availability.

[5]Except in the two states Bremen and Bavaria where it is 4 and 6 years respectively.

member proportional system.[6] The parties that participate are the 5 major parties in Germany: a center-right party (CDU - Christlich Demokratische Union), a center-left party (SPD - Sozialdemokratische Partei Deutschlands), a pro-market party (FDP - Freie Demokratische Partei), the Greens (DIE GRUENEN) and a socialist party (DIE LINKE), as well as some minor small parties and local parties.[7] The Free Voters (Freie Wähler), while purely local and independent, are often member of a collective of Free Voter parties, either regional or on a state level. The mayor of the municipality is elected by the public as well. Often, the timing of the elections coincide. The mayor is head of the administration and also member of the council in most states. Even though the mayor proposes the budget and generally sets the agenda, the council is free to change it and has the power over the legislation.

In every proportional election system, a seat allocation function is used to distribute (discrete) seats to parties based on their votes. This seat allocation function is by design a step-wise function since there are more votes than seats. The locations of these steps for party A, however, are not predetermined. They are jointly determined by all arguments of the seat allocation function: the votes of *all* parties, the sum of those votes and the council size. In order to clarify this issue, we first describe the seat allocation functions used in German municipalities before returning to the question of where the seat thresholds for a party A lie.

The states choose the allocation method for their municipalities such that all municipalities in a certain state have the same seat allocation function. There are two different seat allocation functions used in German municipalities.[8]

The first is the largest remainder method (or Hare-Niemeyer method). The first step in this method is to calculate the Hare quota: total votes divided by total seats. This gives the "price" in terms of votes that a party has to "pay" for one seat. Then the

[6]A mixed member proportional system combines elements of a majoritarian election in districts and proportional elections of the overall council. The resulting composition of the council, however, will be determined by the proportional election part of the election – unless a party won more direct seats than it should receive given the proportional election result. As we cannot identify such cases, we will have some measurement error in the specification for the state of North-Rhine Westphalia where such a system is used.

[7]The CDU is the party of the current federal chancellor Angela Merkel, of Helmut Kohl and Konrad Adenauer, the SPD is the party of Gerhard Schröder, Helmut Schmidt and Willy Brandt, the FDP is the party of Guido Westerwelle and Hans-Dietrich Genscher, The Greens is the party of Joschka Fischer and The Left is the former PDS which was founded as the successor party of the Socialist Unity Party (SED) in former East Germany.

[8]The state of North Rhine-Westphalia recently reformed its voting legislation and is now using a third allocation function. However, under the ruling law in our time period we only observe the two functions explained in this section.

votes of all parties are divided by this price. The resulting quotient is the exact number of seats that each party should receive in case of perfect proportionality. However, it is rarely, if ever, an integer. Therefore, the largest remainder method allocates the seats according to the integer of this quotient. This results almost always in at least one remaining seat. The remaining seat(s) are then distributed according to the rank order of the remainders of each party.

The second method is the highest averages method (or d'Hondt method).[9] This method proceeds just as the largest remainder method by calculating the price of a seat (the Hare quota), dividing each party's votes by this price and then distributing seats according to the integer of the resulting quotient.[10] As under Hare-Niemeyer, there will be at least one remaining seat. Under the d'Hondt method, however, the price of the seats is lowered in small increments in order to distribute the remainig seat(s). The procedure is repeated with a lowered price and seats are allocated according to the integer until the procedure results in a complete allocation of all seats. [11]

Now we return to the question of where a seat threshold lies for party A. Consider a council with 10 members, and 100 voters. The seat allocation function is the Hare-Niemeyer method. The Hare quota or price is 10 votes per seat. If party A has 56 votes, and parties B and C 22 each, party A receives 6 seats (5 seats from the integer, and the remaining seat for having the largest remainder) and parties B and C two each. If parties B and C have 27 and 17 votes respectively, party A receives only 5 seats for its 56 votes. However, it was very close to gaining the sixth seat. Therefore, the distance to a seat change depends on the whole composition of the vote vector, not only on the votes for a specific party. A similar argument would hold under the d'Hondt method.

2.2 Responsibilities of German municipalities

The local government is head of the administration of the municipalities, and it manages a yearly budget of about 1400 Euro per capita on average. This amounts to

[9] There are several highest averages methods, for instance the method of Sainte-Lague. Since only the method of d'Hondt is used in Germany, we will describe only this method.

[10] There are several different ways of reaching the final seat allocation in the highest averages method. The other common form is the use of a divisor series. Both yield the exact same result.

[11] The two approaches may lead to different seat allocations since the d'Hondt method slightly favors larger parties. The intuition is as follows: the "price" of a seat in the d'Hondt method is lowered until the distribution of seats according to the integer leads to a full distribution of seats. If we regard the votes of a party as its budget, lowering the price is best for the party that already has the most seats – compared to the Hare-Niemeyer method where just the remainders are compared.

roughly one third of total per capita government spending in Germany. Moreover, two thirds of all investment spending is allocated by municipalities, and they employ around 40% of all state employees.[12]

The municipalities set three tax rates whose revenues completely accrue to them: a trade tax for businesses and two types of property taxes. The local budget also contains a share of the income tax revenue raised in the municipality and a part of the VAT revenue. Setting the rates for those two taxes is however not a municipal responsibility. Another part of their budget is federal or state allocated funds that the municipalities administer, e.g. for public schools or social services.

The municipalities spend their revenues in the following areas:

- general administration
- public order
- public schools, cultural centers and services, social services (elderly care, immigration housing, child care, youth services)
- sport and recreation
- infrastructure investments (housing projects, roads), public transport, business development, management of public firms

However, the division of tasks between the tiers is often complex: which tier of government pays for the service or investment, or for part of it, which tier enacts the law or by-law, which tier administers the service and so forth. More often than not, each of these tasks is itself divided between the tiers of government. Thus, the degree of discretion for the municipality varies by field. While municipalities are, for example, completely free to decide about cultural or recreational institutions, most social services have to be administered within clearly defined laws and by-laws. Those rules are to a large extent enacted by the federal or state government and the municipalities use mandated funds from higher tiers. However, even there the municipalities can choose to apply rules strictly or not. What is more, they can for example supplement social services, expand or restrict child care or start an initiative for public transport.

[12]See Bundesbank (2007).

3 Empirical Strategy

In this section, we will outline the empirical strategy for both, seat shares and voting power. We will start with seat shares and the problems of identifying a causal effect. Then we will turn to using close election results only and describe in subsection 3.2 how we define close elections. In the last subsection (3.3), we will discuss voting power and show how we can construct instruments for voting power in our setting using close elections.

3.1 Estimation setup for seat shares

In general, the effect of parties on policy is hard to distinguish from the underlying preferences of the voters. Assume that we have parties A, B and C, a vote vector $\mathbf{v}_i$, seat shares s_i^j and outcome Y_i in municipality i. We would like to estimate the effects of the parties on policy outcome Y_i. Experimental data is impossible to obtain and OLS of the outcome on seat shares or voting power is biased by underlying voter preferences if we estimate

$$Y_i = \alpha + \beta_B s_i^B + \beta_C s_i^C + e_i. \tag{1}$$

The error term in this equation contains not only a random component w_i but also unknown voter preferences ϕ_i:

$$e_i = w_i + \phi_i$$

These voter preferences affect both the outcome and the seat shares, as voter preferences are a main determinant of the election results. Therefore, $E(s_i^j e_i) \neq 0$ and the coefficients will be biased.

However, we could use the fact that the seat allocation function is a discontinuous function of the vote vector: a party can only gain a full seat, not a fraction of a seat. That in turn means, vote shares and seat shares do not necessarily coincide. If we interpret vote shares as reflecting voter preferences and take seat shares as our measure of political representation, we could estimate

$$Y_i = \alpha + \beta_B s_i^B + \beta_C s_i^c + f(\mathbf{v}_i) + u_i. \tag{2}$$

The problem with this approach is that the function $f(\cdot)$ needs to capture the voter preferences ϕ_i correctly over the whole range of possible values for $\mathbf{v}_i$ – and for the interaction of parties – in order for the seat shares to be uncorrelated with the error term. Formally, the condition is

$$E(s_i^j e_i \mid f(\mathbf{v}_i)) = 0$$

It is hard to argue that we will be able to accomplish that.

However, there is a different way to use the step-wise nature of the seat allocation function: only use seat allocations that were close to being changed for identification. This relaxes the need to specify the whole function $f(\cdot)$ correctly. The two assumptions that we need to make is that $f(\cdot)$ is continuous at the steps, and that it is correctly specified close to these steps. As explained in section 2.1, these steps are not predetermined, but depend on the votes of *all* parties, the total votes and the council size. This in turn means that the steps could be anywhere in the vote vector space. Does that imply that we need to specify $f(\cdot)$ correctly over the whole range, just as in equation (2), if we use close seats only?

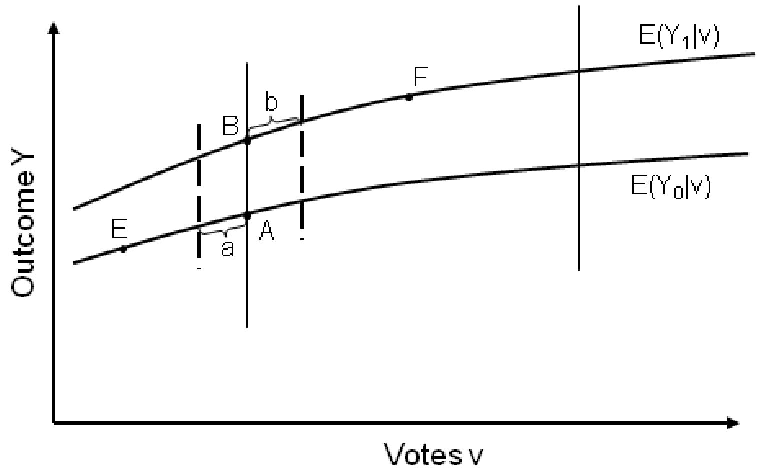
In order to answer this question, consider an extreme case where we have almost unlimited amounts of data. This implies that we can get arbitrarily close to seat thresholds. If we are just one vote away from a seat change for each close observation that we identify, it is safe to argue that the averages that we calculate on both sides (for close seat gains and close seat losses) will be unbiased and any difference is the result of the seat change. In figure 1, we draw the potential outcome functions and a hypothetical seat threshold (the solid vertical line), using the votes for this party on the horizontal axis.[13] Our interest lies in comparing A and B, because the difference between the two is the causal effect of representation on policy outcome Y. If we can get arbitrarily close to the threshold, we get unbiased estimates of A and B.
Of course, we need to relax this assumption since we do not have unlimited amounts of data. However, we stay very close to the seat thresholds. In figure 1, we stay between the dotted vertical lines. In these ranges we calculate averages on both sides, in the segments a and b. Without any control for the underlying functional relationship between the vote vector (the forcing variable) and the outcome variable, those averages will be biased: the average over all observations in b will be too high, and too low over all in a.[14]

Therefore, we have to use the function of the vote vector to account for this distance

[13] As outlined above, this is not entirely correct because thresholds depend on the whole vote vector, not only the votes of one party. The figure is therefore just for illustrative purposes

[14] See Hahn, Todd, and Van der Klaauw (2001) for a formal treatment of this bias.

Figure 1: Using the discontinuity



The figure is based on Lee and Lemieux (2009).

to a threshold when calculating averages. This distance on the other hand is small, and so is our reliance on the functional form of $f(\mathbf{v}_i)$: we only need to make sure that the averages on both sides of the thresholds correspond to the points A and B that we are ultimately interested in. In contrast to the specification in equation (2), we do not use $f(\mathbf{v}_i)$ to compare observations in E and F.[15]

For the close seats in a council of size Z_i we will define a treatment variable t_i^j. This treatment variable takes on the value $+\frac{1}{2}/Z_i$ if the close seat was just above a threshold, that is, the party was close to *losing* this seat, and $-\frac{1}{2}/Z_i$ in the opposite case. We call the two values positive and negative treatment respectively:

$$t_i^j = \begin{cases} +\frac{1}{2}/Z_i & \text{for positive treatment,} \\ -\frac{1}{2}/Z_i & \text{for negative treatment,} \\ 0 & \text{otherwise.} \end{cases}$$

The values ensure that the difference between gaining and losing a seat is one divided by the council size, that is, the difference between positive and negative treatment is measured in terms of seat shares.[16] This facilitates the interpretation of the coefficients

[15]In other words, we rely on $f(\mathbf{v}_i)$ only for very short distances, for which even an linear approximation might suffice.

[16]Below, we will describe in detail how we assign positive and negative treatment for close elections.

and makes them comparable to OLS results, as we will discuss later.

Moreover, we define the variable c_i^j:

$$c_i^j = \begin{cases} 1 & \text{for positive and negative treatment,} \\ 0 & \text{otherwise.} \end{cases} .$$

These closeness dummies c_i^j ensure that our treatment variables t_i^j only act as an instruments for those observations for which party j was close to gaining or losing a seat, that is, where the instrument is unequal to zero. The observations for which it was not close – where the instrument is zero – will only be used in order to add precision to the estimation of the control function $f(\mathbf{v}_i)$ and increase overall efficiency of the estimates.

We could use these variables in a regression of the outomce y_i without instrumenting. If we had just one forcing variable and could get arbitrarily close to the thresholds, the difference between negative and positive treatment observations would be equal to one seat and we could just use it in a regression.[17] However, the forcing variable is in fact a vector and we cannot get arbitrarily close to the thresholds. Therefore, our treatment variable is only approximately correct and we therefore use an intrumental variable strategy, where we instrument for seat shares with our treatment variable. This ensures that our regression coefficients represent the effect we want to estimate: the effect of 1% additional seat share.

The regression that we are going to estimate therefore takes the following form:

$$y_i = \alpha + \sum_j \beta_j s_i^j + f(\mathbf{v}_i) + \mathbf{X}_i\gamma + \mu_i + e_i, \tag{3}$$

where $f(\cdot)$ is the flexible function of the vote share, $\mathbf{X}_i$ is a set of control variables and μ_i is a municipality fixed effect. We instrument for the seat shares s_i^j with our treatment variables t_i^j. The set of control variables include population of the municipality, dummies for each state-wide municipal election and the closeness dummies c_i^j.

Note that we leave out one party, the CDU, in all specifications. The reason is that seat shares add up to one by definition. This is also true for relative voting power, as we discuss below. The CDU is the biggest party and takes part in almost all of the elections in the western states, and most of the elections in Thuringia. The

[17]We would correct for different council sizes, of course.

interpretation of the β's is therefore: the effect of an increase in party j's seat share by 1% at the expense of the CDU on the policy outcome.[18]

There are two important assumptions for the validity of our research design: individuals (here: parties or voters) cannot manipulate the vote vector such that a party ends up just above or just below such a seat threshold. And second, parties cannot manipulate policy in anticipation of a close election.

Election manipulation is (hopefully) impossible in Germany, and voters have no precise information about which side of a seat threshold parties are when making their voting decision (they neither know the voting behavior of others, nor do they understand the seat allocation functions), so we can safely argue that the first condition is satisfied.[19] The other issue is more difficult to dismiss a priori. However, we show evidence that there are no party effects for policies enacted *before* the election. Moreover, we include municipality fixed effects and a dummy for close elections in order to control for some of this variation.

The next step in the empirical setup is to define which seats can be considered close and to assign positive and negative treatment accordingly. In contrast to standard RDD settings, the cutoff points in our setting are not immediately obvious. Our forcing variable is a whole vector, and the votes for one party alone tell us nothing about those cutoffs. Therefore, we need to find a way to determine which seat allocations are close.

3.2 Defining close elections

In this section, we discuss how we define closeness of a seat allocation. We intend to use close elections as a source of exogenous variation because we assume that there is some random component in elections (e.g. the weather, errors in polls or popularity shocks). Thus, randomness and closeness are linked. In fact, the way we think about the randomness in elections determines the way we define which elections or seats are close. In the following, we outline a conceptual framework of randomness in elections. From this conceptual framework, we deduce our definition of close elections and the method that we use.

[18] We measure seat shares from 0 to 100 in order to allow for this interpretation.

[19] **?** shows for mayor elections, that are easier to understand and manipulate, that there are no signs of manipulation by the voters.

To begin the discussion of a conceptual framework, we may consider sincere voting and assume a continuum of voters' bliss points along one policy dimension. Let us further assume that parties are exogenously given and their allocation along this dimension is known and fixed. However, the distribution of voting decisions along this dimension changes, according to popularity shocks to the parties. In other words, voters take policy *and* party popularity into account when making a voting decision.

Under the assumption that everyone votes, parties may gain or lose votes only at the expense of other parties (and not at the expense of non-voters). In our simple one dimensional setting, the parties next to each other are gaining or losing from one another. For higher policy dimensions, that is, a policy vector, the voter migration pattern will be more complex. Whether a seat allocation was close for party A then depends on how much voter migration between parties is necessary for party A to gain or lose a seat. Alternatively, a close seat is one that changes often in repeated elections with the same preferences and party locations, but newly drawn popularity shocks.

As a second step, we can introduce the voters' decision to participate in the election. The participation shocks may be independent across parties (mobilization shock), or related (weather shock).[20] Thus, in this setting parties not only gain votes at the expense of other parties (their neighbours) but also from non-voters. Whether a seat of a party is close depends as before on the voter migration between parties, but it is now combined with the participation shocks. Alternatively, a close seat is one that changes often in repeated elections with the same preferences and party locations, but newly drawn popularity and participation shocks. The difference between popularity and participation shocks is not of much practical relevance, and is for the most part a purely theoretical concept. One could think of just a popularity shock for parties that not only leads to voter migration between parties, but equally to changes at the participation margin.

Another story assumes strategic voting. Here, each voter has a preferred seat allocation in parliament that best matches his own policy preferences. Polls and other information provide a background against which the voters can assess and formulate their voting strategy. However, voters need to interpret the information. In interpreting the information, voters make mistakes and thus their voting is a combination of a strategic choice and a random error term. The error might consist of two parts: a

[20] It is hard to imagine a participation shock that is the same for all parties because any reason for such a shock may affect the voters of different parties differently.

common error that may be caused by erroneous news reports or some other correlated errors in interpreting the information, and an individual interpretation error. A close seat is therefore one that changes often in repeated elections with the same preferences and information, that is, the same strategically optimal decision, but newly drawn errors. The migration patterns are less clear here: strategic voters may prefer to vote for a rather "distant" party, to prevent an unwanted coalition or party from winning.

Based on these notions of election randomness, we propose the following definition of closeness of elections. For each observation i with vote vector $\mathbf{v}_i$ and the resulting (known) seat allocation we add a vector of random variables to the vector of votes. We then calculate the resulting seat allocation from this perturbed vote vector and track whether the seat allocation has changed. This procedure is repeated multiple times. In practice we add a vector $\mathbf{r}_i$ of independently normally distributed random variables to the vote vector of observation i with expectation zero and variance $\left(kv_i^j\right)^2$. The standard deviation of these random variables is thus k percent of the votes of party j. This ensures that for a small party the perturbation is small. Seat allocations for party j in municipality i are considered close, if in repeated perturbations of the vote vector, the seat allocation for this party j changes more than $q\%$ of the time.

Next we discuss our choice of q and its intepretation. For normally distributed random variables, roughly $\frac{1}{3}$ of the probability mass lies outside the interval of the mean plus/minus one standard deviation. Moreover, we observe from our perturbations that almost all seat changes go in one direction only, not both.[21] It follows that if we observe one additional seat for party j in municipality i in $\frac{1}{6}$th of our perturbations, we know that roughly one standard deviation in vote change for this party was required for this change in seat allocation.[22] When we vary the degree of closeness in later specifications, we will only change k in the standard deviation of the random variables. The share q will always be $\frac{1}{6}$ in order to allow for this interpretation.[23]

In accordance to the specification above, we define close seats as those that change more than $\frac{1}{6}$th of the time, and assign treatment as follows. We assign negative

[21]It is possible, that a seat is close in both directions: for instance, if three parties have very similar remainders in the Hare-Niemeyer method. However, such situations occur only rarely for very small perturbations like the ones in the present paper.

[22]That does not mean that for every seat change one standard deviation vote gain or loss was necessary for the party in question. A seat change for party A can be the result of vote changes for other parties, too. The interpretation given here only offers an idea of the magnitudes involved.

[23]If we use $k = 2\%$, a party that received 100 votes we will perturbed such that the vote count is between [98,102] in about 66% of the cases and between [96,104] in 95% of the cases.

treatment to those that were close to gaining an extra seat and therefore, had one extra seat in more than $\frac{1}{6}$th of the perturbations. And we give positive treatment to those that were just above a threshold and therefore close to losing one seat.

In future work, we would like to constrain the vector of random variables to reflect a certain covariance structure in the shocks of parties (instead if assuming independence). This covariance matrix should reflect the actual migration patterns between parties and at the participation margin. We intend to estimate this migration from the municipal election data. As a result, the closeness would be reflected more realistically compared to using independently distributed random variables.

In contrast to our approach, Folke (2010) identifies close elections by analytically computing the vote change from the realized vote vector to the closest point in the vote space where the seat allocation changes. This *minimal* vote change requires that there is no voter migration except the migration that leads to exactly this vote change. This approach runs into problems if this migration pattern is utterly unrealistic (a green party gaining votes at the expense of a right-wing party for instance) and may therefore incorrectly identify close seats.

In our approach, especially when we add the estimated migration between parties, randomness in the elections translates naturally into determining which seats were close. Besides this conceptional advantage of the perturbed simulations there are two additional benefits to our approach. First, the implementation is substantially easier. Folke (2010) develops a complex algorithm to compute the minimal vote change. This algorithm is specific to the electoral system in use in Sweden and is not easily adjustable to variations in the electoral system. Specifically, it cannot be used without substantial adjustments in the electoral system in some of the German states. Our approach does not depend on the specifics of the electoral system, but can be applied to any system. This is especially valuable for us, as each German state does in fact have its distinct electoral rules. Secondly, it is simple to implement different structures of randomness, for example a voter migration matrix or conditioning the randomness on the party's size.

3.3 Estimation setup for voting power

In this section, we will first define voting power and then discuss our instrumental variables strategy. As outlined in the introduction, we use the policy-oriented measure of voting power after Penrose (1946) and Banzhaf (1965). This a purely probabilistic measure which is defined as follows.[24] In the set N there are n different parties with weights equal to their respective seat shares. The quota is one half which means that every coalition of parties whose seat shares sum up to more than one half is a winning coalition. The power set 2^N with its 2^n elements consists of all possible coalitions plus the empty set. For a party A, there are $2^{(n-1)}$ possible coalitions that it could be a part of. Party A is said to be critical in a coalition, if this coalition (with A) is a winning coalition, but without A it is not.

Then the (absolute) voting power of party A is defined as

$$\beta_a = \frac{\eta_a}{2^{(n-1)}}, \tag{4}$$

where η_a is the number of times party A is critical. If we assume that all coalitions are equally likely to form, then β_a measures the a priori probability of party A to be in a position to change the fate of the decision. It is this interpretation that makes this measure of voting power a measure of influence on policy.

Voting power measured in this way does not necessarily add up to one, as indicated in the introduction. To construct an index of voting power that adds up to one, we divide the η_a not by $2^{(n-1)}$, but by the sum of the η_j's:

$$\beta'_a = \frac{\eta_a}{\sum_{j \in N} \eta_j}. \tag{5}$$

Which one is the appropriate measure of voting power in our context? Note first that in case of an absolute majority for a party A, the absolute and the relative measure of voting power will be equal to one for this party, and zero for all other parties. If there is no absolute majority there will be negotiations about coalitions. And in these negotiations, the parties with more *relative* voting power will be more likely to influence policy. The absolute measure of voting power is less relevant because even if party C has some absolute voting power, the resulting policy will be more in line with those of party A and B if their absolute voting power is even higher. Therefore, we will use relative voting power as our measure of representation.

As indicated above, the (a priori) measure of voting power assumes that all coalitions are equally likely to form. This is unlikely to hold in practice.[25] Therefore, we will add weights to the coalitions when we calculate voting power. In practice, we will locate

[24] The definition and discussion of voting power is based on Felsenthal and Machover (1998).

[25] The a priori voting power measure is used for example to analyse different voting rules, where it is very useful. In our context, where we want to analyse the power of known parties with a certain policy position in a simple majority rule council, the a priori measure is less useful.

parties in a two-dimensional policy space. These dimensions capture unknown policy dimensions (often called left-right or liberal-conservative). The distance between two parties is then a measure of the likelihood of a coalition between the parties. This likelihood will then be the weight of this coalition:

$$w_{kj} = 1 - (d_{kj})^s, \tag{6}$$

where d_{kj} measures the distance between party k and party j, and $s > 1$ affects the curvature of our distance measure. It only measures the curvature because we normalize the distance between the most extreme parties to be unity. For those parties (like a strongly right-wing party and a communist party) the weight on their coalition will be zero which implies that they do not receive any voting power from this coalition, even if it were a winning coalition and each of them were critical. If we have a coalition of three or more parties, the distance within this coalition will be equal to the distance between the two parties within that coalition that are farthest away from each other.

Relative voting power with weights will then be calculated as:

$$\beta_a'^w = \frac{\eta_a^w}{\sum_{j \in N} \eta_j^w}, \tag{7}$$

where

$$\eta_j^w = \sum_{j:critical} w_{kj}. \tag{8}$$

In words, η_j^w is the sum of all the weights of those coalitions in which party j is critical.[26] So far, we have guessed the party locations since we are still in the process of estimating them. However, already these guesses illustrate how voting power and subsequent results can change, once voting power is weighted by coalition likelihoods. We return to the comparison between a priori and actual voting power in the results section.

Now we turn to the specification of the regression and how we define our instrument. The regression that we would like to estimate is

$$y_i = \alpha + \sum_j \beta_j p_i^j + f(\mathbf{v}_i) + \mathbf{X}_i \gamma + \mu_i + e_i, \tag{9}$$

[26] Bilal, Albuquerque, and Hosli (2001) propose a similar approach to weighted relative voting power.

where p_i^j is relative voting power of the parties, $f(\cdot)$ is the flexible function of the vote share, $\mathbf{X}_i$ is a set of control variables and μ_i is a municipality fixed effect. Note that we have to leave out one party as relative voting power adds up to one. However, voting power is endognous so we instrument for voting power using close election outcomes in the following way.

We again perturb the vote vector of each observation, simulate the new seat allocation, but this time also calculate the voting power of the parties under this new seat allocation. If one seat switches there are three possible consequences for voting power. First, nothing changes because the seat change was not crucial for whether a coalition is a winning coalition or whether a party is critical. As an example, consider an absolute (super)majority for party A where this party A loses one seat but maintains its absolute majority. Second, the voting power of those parties changes that had a seat change. For instance, if party A has 6 and party B has 7 seats, and after the perturbation party A receives 7 and party B 6 seats. For the other parties, nothing changes. With weighted voting power, this scenario is almost impossible. And third, the voting power of more than two or even all parties changes. This is far from uncommon: when a seat change leads to different winning coalitions, the voting power of all parties is likely to change, especially with weighted voting power.

To construct our instrument, we first count the number of changes in voting power for each party in a municipality i during the perturbations. However, the size of the jump may also contain useful information that we would like to keep. Therefore, we also calculate the average change in voting power for the times that it did in fact change. Our instrument is then

$$z_i^j = \begin{cases} \frac{1}{2}(p_i^j - \bar{p}_{i,perturb}^j) & \text{if it changes more than } q\% \text{ of the time,} \\ 0 & \text{otherwise.} \end{cases}$$

where $\bar{p}_{i,perturb}^j$ is the average voting power of party j in municipality i during the perturbations when it in fact changed. The reason for dividing the instrument by two is the same as for the seat shares: we compare observations that had a positive difference to those with a negative difference in the instrument specification. If we take the full difference for each observations, we in fact double the difference.

One might wonder how we can use z_i^j as an instrument for p_i^j when the former contains the latter. However, the difference between p_i^j and $\bar{p}_{i,perturb}^j$ is in fact unrelated to p_i^j when the election outcome was close – under the assumption that some form of randomness in elections determines whether you are close and above or close and below a threshold. We again let q be $\frac{1}{6}$ because this allows for the interpretation that roughly one standard deviation (of our random variable) was necessary to induce this shift.

4 Data

We have compiled a new data set that combines information on both election outcomes and fiscal data on the municipality level in Germany. We use data from four German states: Bavaria, North Rhine-Westphalia and Hesse from the western part and Thuringia from the eastern part.

Table (1) shows the descriptive statistics for the political variables. For each state we have election data on two municipal elections. There are between 400 and 2050 municipalities in each state. The center-right party (CDU - Christlich Demokratische Union) and the center-left party (SPD - Sozialdemokratische Partei Deutschlands) participate in almost all elections in western states and in many communities in the eastern state. The green party (DIE GRUENEN) and the pro-market party (FDP - Freie Demokratische Partei) participate in roughly half the elections in the western states (except in Bavaria), but in considerably less elections in the east. For the socialist party (DIE LINKE or PDS), which had a strong focus on eatern Germany until recently, the pattern is the reverse.

Table (2) shows the descriptive statistics for the fiscal outcomes. For all these outcomes, we took the average over the period between two elections. We left out the data from the election years because we are unable to assign them to a government term.[27]

The tax multipliers require some explanation. The trade tax is a tax on business income, where "business" includes all companies and firms as well as self-employed that do not belong to the "free professions" (*Freiberufler*). These include for example artists, lawyers, scientists, teachers, accountants, doctors, all medical therapists, architects, journalists, fotographers and engineers. The tax payment is calculated based on federal tax law and then multiplied by the trade tax multiplier that the municipality sets. This trade tax is separate from the federal business income tax.

[27]In cases where we do not have data for the whole period – for instance for the election in 2004, the term just ended recently – we took the average over all years that we had data for. Our data on fiscal outcomes starts in 1998.

The multipliers range from 0 to 800,[28] and the actual tax rates for this trade tax is in the range of about 7% to 28%. The property taxes in Germany are *ad valorem* taxes where tax A is applied to agricultural property and tax B to all other property. Again, the tax payment is calculated based on federal law and then multiplied by the municipal tax multiplier.
All spending data is measured in Euros per capita. The data display considerable variation in all spending categories and, to a lesser extent, in tax multipliers. We use fixed effects in order to reduce the this large variation across municipalities. However, the large variation – especially in spending – is a problem for our estimation since we are estimating the effect of small changes in seat shares and voting power.

5 Results

The results section is organized in three parts: first, we shortly present the results for seat shares, second we show the main results for voting power and finally we dicuss instrument validity and robustness.

5.1 Seat shares

In the left part of table (3), we present the OLS results corresponding to equation (1). These results give us a first indication of whether political parties are at all statistically associated with policy outcomes. The coefficients for each party give the effect of a 1% change in seats from the CDU to the respective party.[29] Column (1) shows that the seat share of parties is indeed a predictor of the trade tax rate with the expected sign: the "left" parties (SPD, Left/PDS, Greens) show a positive sign relative to the CDU, and the pro-market party (FDP) shows a negative sign. For the other taxes, the relationship is less clear. Interpreting the size of the coefficient, the Left/PDS is associated with 5.7 points increase in the trade tax multiplier if it gains 10% of the seat share from the CDU. 200 points roughly equal 7% in the final tax rate, so 5.7 points correspond to 0.2% increase in the actual tax rate.

Since the OLS results contain only statistical associations rather than causal effects, we turn to the instrumental variable (IV) approach. The results of estimating equation (3) with our treatment variables as instruments are presented in columns (4) - (6).

[28]During a reform in 2003 a statutory federal minimum of 200 for the trade tax multipliers was implemented. However, there was only a handful of communities that were directly affected by this reform.

[29]The coefficients in equation (1) measure the effect of a switch of 100% seat share of the CDU to 100% seat share of the respective party. In the results, we divided this coefficient by 100.

Only a seat change of the social democrats (SPD) relative to the benchmark center-right party (CDU) has a significantly effect on taxes, with the FDP and the Others being marginally significant for some taxes.

The first stage results of the IV approach for seat shares are presented in the first two columns of table (4). Each coefficient is the result of a separate regression: equation (3) with the actual seat share of one party as outcome variable. We expectedly observe a highly significant effect of about 100 for all parties: our coefficient measures the effect of a 100% change in the seat share of the council towards the party in question on the outcome, in this case the seat share.[30]

The aim of analysing seat shares is to identify *average* effects of an additional seat. However, not every seat is of equal importance. The effect of a seat that increases a party representation from, say, 65% to 70% might not matter at all, likewise a seat that does not change the coalition options in a council. The effect of a seat that has an impact on the identity of the governing coalition on the other hand, probably has the largest impact. Therefore, we will explore the effects of voting power on policy outcomes in the following section.

5.2 Voting power

The OLS results for voting power are shown in table (5) in columns (1) - (3). Again, the coefficient estimates for the trade tax go in the expected direction but are much smaller than for seat shares. For the property taxes, both SPD and Left/PDS have positive and significant coefficients. As before, those results are no proof of voting power to causally affect policy outcomes. Rather they illustrate the link between voter preferences and local policy as well as a selection effect of parties not to run in specific municipalities.

To identify causal effects of voting power, we again turn to the instrumental variable estimation of equation (9). Only for property tax B, the non-agricultural tax, do we find effects: for the SPD a 2.5 points decline in the tax multiplier for an increase in (relative) voting power by 10 percentage points relative to the CDU, for the Greens the effect is an increase of 9.3 points. Since the multiplier ranges from 200 to 800, this may seem like a small effect. However, the average change in the multiplier of the property tax B over an election period is around 7 points.

[30]The very high values for the F-Statistics (in squared brackets) indicate that the instrumental variable strategy identifies higher seat shares for parties almost by definition.

So far we have taken the average value during a government term as the outcome, excluding the election years since we are unable to pin down changes during a year. Alternatively, we could look at the outcomes in the last year before the election. The top panel of table (6) contains the result for this IV regression. The effects of the SPD and The Greens on the property tax B are the same, whereas the effect of the SPD on the agricultural property tax increases slightly in magnitude and is now significant. The effect of The Greens on property tax A increases as well and is now marginally significant. These results for the end period suggest that the effects found for the average are not driven by a political business cycle.

For the voting power approach, we also look at spending and other sources of revenue. The first three columns of table (7) suggest that parties' voting power has no impact on spending: neither total nor personnel or investment spending display any significant effect. However, spending is highly variable and only in part the result of a local policy decision. Since we only measure the effect of small increases in voting power, these results are not surprising. The results for other sources of revenue – debt, fees and investment grants – on the other hand display a similar pattern: no effects of voting power. One explanation for the lack of an effect on fees could be that we look at revenues here, not fee rates. The direct policy instrument, the rates, could be affected but the market reaction (for instance fees for house construction, for sewage or waste collection) may counteract those changes.

The first stage results of the IV approach for voting power are presented in columns (3) and (4) of table (4). As before, each coefficient is the result of a separate regression: equation (9) with the actual voting power as the outcome variable. The effects are high and statistically significant. We also expect effects around 100 as the coeffcient should measure the effect of a one percent increase in relative voting power on the outcome variable. The coefficients show larger variation around 100 than the seat shares, indicating that our instrument is not as precise. This is also reflected in the much lower F-statistics. Nevertheless, the first stage works well overall.

5.3 Validity and robustness

Apart from examining the first stage of our instrumental variable estimation, we conduct tests on the validity of the instrument. The second part of this section analyses several robustness tests.

Our assumption is that close election outcomes provide us with exogenous variation in seats and voting power that we can use to analyze local public policy making. If this assumption is correct, we should *not* observe any of the following:

- an effect of instrumented voting power on variables *before* the government term in question;
- different distributions of vote shares for positive and negative instrument values;
- skewed distributions of the instrument for each party: the difference between the actual voting power and the voting power during the perturbations.

We will discuss these aspects in turn.

The lower panel of table (6) shows the IV regression of equation (9) using the outcome variable in the last year *before* the election. The results are clearly insignificant. Moreover, the effects for the Greens and the SPD that we found for the end of the government term (see the upper panel) are economically sizeable whereas the same effects on the policy measure before the government term are economically negligible. For spending and revenues (not included in the table), there are also no effects on outcomes before the period in question.

As another validity check, we look at the distributions of vote shares and of the instrument for each party. Figure (2) shows the frequency of a positive or negative value of the instrument by the vote share of that party. All graphs are reasonably well balanced, even though the CDU seems to be getting more positive than negative treatments, contrary to the SPD. In figure (3) we plot the distribution of the instrument, which is also reasonably well balanced for each party.

We conduct four robustness tests to check whether our results hold up to changes in the specification and data: changes in the IV speicification, changes in the degree of closeness, estimates by state, and finally weighted voting power.

The specification of the instrument can be changed in two opposite ways: extract more or less information out of our perturbations. First, we extract more information by adding the squared instrument to the specification. The reason is that large changes in voting power during the perturbations have a different impact on voting power than small changes: large parties have disproportionately high voting power and therefore relatively high differences, too. The squared instrument allows for such a non-linear relationship. The top panel of table (8) shows the results for this specification. The results do not change.

The other way is to extract less information from our perturbations, for instance by using only the direction of the difference, not the the size of the difference. The lower panel of table (8) contains the estimates from this IV regression. The results change slightly, but remain robust.

When we change our parameter k in the perturbation from 1% to 2% or .5%, we expand or restrict the sample we use for our estimation. The results of doing so are presented in table (9). In the top panel, we have expanded the sample to also include less close elections. The effects of the SPD and The Greens on property tax B are in the same range, albeit less significant. If we restrict the sample to include only very close elections (shown in the lower panel), we find that the effects remain in the same range but lose significance. We interpret these tests as confirmation that our results are robust to a change in the definition of closeness.

The estimates are so far based on the data in four German states. If we estimate each state separately, we find that results indeed change (table (10)). For Bavaria alone, the results of the SPD and The Greens are consistent with the overall results, even though the SPD seems to affect all tax rates. In Hesse, only The Greens affect a tax, namely the property tax B. In North Rhine-Westphalia (NRW) we do not find any effects.[31] For Thueringia, there are no effects, except that the SPD and The Left seem to affect the trade tax, even though this effect is only marginally significant.

As a further check, we excluded all observations with a council size below twelve, thus restricting the sample to larger communities. We find that the signs and significance of coefficients do not change although point estimates vary slightly (not included in the tables).

Finally, we use weighted voting power as explained in the estimation strategy. Weighted voting power takes into account that not all coalitions are equally likely to form. Since we are still in the process of estimating the coalition likelihoods from our data, we used a rough approximation for party locations in Germany as a starting point. Figure (4) shows as an example for two-dimensional party positions. The results are shown in table (11). The results are almost unchanged.

[31]Note that NRW was the state in which the mixed member proproportional system poses some problems for our estimation, adding noise to the instrument.

6 Conclusion

The importance of political representation in a proportional election system has long been a topic of the theoretical literature in political economy. However, the empirical literature has been mostly unsuccessful in reliably identifying the effects of representation. This paper attempts to estimate the causal effects of political representation on fiscal policy in local governments.

Our analysis builds on the estimation strategy developed in Folke (2010) to estimate *causal* effects of representation. We use seat shares *and* relative voting power as our measures of party representation in the municipal council. The empirical analysis is based on an instrumental variable approach in which we identify the effects of individual parties by focussing on narrow seat allocations. Given that election outcomes are subject to some randomness, the exact seat allocation on the margin can be regarded as an as-good-as random event. To determine whether a party was close to lose or win a seat, we develop an algorithm based on random perturbations that is both simple to implement and easy to adjust to any electoral system. Moreover, we show that this algorithm can be directly linked to a conceptional framework for randomness in elections.

We use data on about 7200 elections and subsequent fiscal outcomes in German municipalities from 4 different states. While OLS results hint at a clear correlation pattern between political representation and fiscal policy, the results of the causal analysis are less definite. However, they indicate that representation does matter. For both seat shares and voting power, the effects are mostly small, but statistically significant. The center-left party is, somewhat surprisingly, found to *lower* the local tax rates. The Greens on the other hand increase the property tax considerably. This effect is only present in the estimation setup with voting power and remains robust to modifications in the specification. This suggests that small parties can influence major policy decisions if their voting power is sufficiently taken into account.

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A Appendix

Table 1: Data set - descriptive statistics for the election data

Election	Observations	Participation rate in the elections for				
State/Year		CDU	SPD	FDP	Greens	Left
Thur 1999	766	0.69	0.38	0.18	0.01	0.32
Thur 2004	716	0.66	0.33	0.14	0.01	0.29
Hes 1997	417	0.98	1.00	0.24	0.55	0.00
Hes 2001	414	0.98	0.99	0.50	0.49	0.00
NRW 1999	396	1.00	1.00	0.82	0.84	0.05
NRW 2004	396	1.00	1.00	0.88	0.82	0.06
Bay 1996	2056	0.84	0.66	0.08	0.18	0.00
Bay 2002	2056	0.83	0.65	0.09	0.16	0.00
Total	7217					

Notes: The table shows the number of observations for each election in the first column. The remaining columns show how often the different parties participated (in shares) in the respective elections. The abbreviations for the states are: Thur - Thuringia, Hes - Hesse, NRW - North-Rhine Westphalia, Bay - Bavaria. The abbreviations for the party are: CDU - conservative center-right (*Christlich Demokratische Union*), SPD - socialdemocrats center-left (*Sozialdemokratische Partei Deutschlands*, FDP - pro-market (*Freie Demokratische Partei*), The Greens - green party (*Buendnis 90/Die Gruenen*), The Left - communists (*Die Linke* former *Partei des Demokratischen Sozialismus*). *Source*: Own calculations.

Table 2: Data set - descriptive statistics for the fiscal data

Fiscal Category	Observations	Mean	Std. dev	Min	Max
Trade tax multiplier	7217	329.4	38.3	170	490
Property tax A multiplier	7217	287.5	68.2	0	800
Property tax B multiplier	7217	318.0	52.8	140	800
Total Spending	7217	1479.6	591.0	504.0	17746.7
Spending on Personel	7217	262.1	111.9	17.9	1268.2
Investment Spending	7217	303.2	222.7	2.8	3088.5
Total Debt (stock)	4035	710.7	588.4	0.7	13340.2
Revenue from Fees	7217	142.4	124.0	0	2653.4
Revenue from Invest Grants	7217	100.6	108.4	−6.9	2381.1

Notes: The table shows the descriptive statistics for the fiscal data used as outcome variables in the analysis. The information on the local taxes refer to the respective multipliers in the tax formula. These multipliers are bounded between 0 - 800 (in the period of observation). The data on the aggregate spending categories as well as stock in debt and revenues from fees and investment grants are per capita. Note that we have data on debt only for Bavaria. *Source*: Own calculations.

Table 3: OLS and IV results for seat shares - average tax rate multipliers

	OLS			IV		
	Average Taxes			Average Taxes		
	Trade Tax	Property A	Property B	Trade Tax	Property A	Property B
	1	2	3	4	5	6
SPD	0.21***	0.17**	0.03	−0.70**	−0.69	−1.01**
	(0.03)	(0.07)	(0.06)	(0.27)	(0.44)	(0.43)
The Greens	0.53***	0.25	0.33	0.06	0.37	0.57
	(0.15)	(0.31)	(0.26)	(0.59)	(0.95)	(0.94)
FDP	−0.23***	0.22*	0.01	−0.40	−1.66*	−1.03
	(0.06)	(0.13)	(0.10)	(0.59)	(0.95)	(0.94)
The Left	0.57***	0.26	0.27	−0.11	−0.19	−0.36
	(0.11)	(0.23)	(0.19)	(0.62)	(1.01)	(1.00)
Others	0.03	0.00	−0.02	−0.25	−0.43	−0.49*
	(0.02)	(0.05)	(0.04)	(0.19)	(0.30)	(0.30)
N	7217	7217	7217	7217	7217	7217

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Columns 1-3 refer to the OLS regressions of seat shares for the respective parties on the three direct policy instruments (tax rate multipliers). Each regression contains a population control as well as dummies for each party if it did not receive any votes and state-election dummies. Columns 4-6 refer to the estimates of the IV regressions, in which seat shares are instrumented by the seat share shifts around a threshold in close elections. The instrument is based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. *Source*: Own calculations.

Table 4: First stage results and # of close elections

	Seat shares		Voting power	
	# of close elections	FS coef	# of close elections	FS coef
	1	2	3	4
CDU	744 (12.3%)	113.5*** (46.9) [1289.3]	569 (9.6%)	107.7*** (10.7) [129.5]
SPD	566 (11.7%)	110.3*** (65.4) [1170.0]	504 (10.6%)	107.8*** (16.6) [82.1]
The Greens	148 (8.2%)	105.7*** (38.9) [627.5]	221 (12.7%)	106.4*** (17.0) [51.7]
FDP	126 (8.1%)	105.7*** (88.0) [1322.7]	169 (11.3%)	54.3*** (8.63) [100.2]
The Lefts	47 (9.2%)	98.6*** (60.3) [1543.4]	53 (10.7%)	150.3*** (19.0) [129.6]
Others	600 (8.9%)	97.6*** (61.8) [1570.1]	585 (8.2%)	113.3*** (10.5) [140.8]

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 3 present the number of close elections in which seats or voting power was at the margin. The percentage points in round parentheses refer to the share of these close elections in the total number of lelections in which the party participated. Columns 2 and 4 show the results of the first stage regressions for each party's representation measure. T-values (based on robust standard errors) in round parentheses are the respective values on each coefficent. F-Stat values are in squared brackets and refer to the F- Statistic for the join test of all variables in the first stage. Each coefficient represents a separate regression. Included controls: 2nd order vote share function including squared vote shares of all parties, population, dummies if the parties received no votes,the c_i^j dummies for each party if it was close, as well as state-election dummies. *Source*: Own calculations.

Table 5: OLS and IV results for voting power - average tax rate multipliers

	OLS			IV		
	Average Taxes			Average Taxes		
	Trade Tax	Property A	Property B	Trade Tax	Property A	Property B
	1	2	3	4	5	6
SPD	0.10***	0.16***	0.10***	−0.08	−0.19*	−0.25**
	(0.02)	(0.03)	(0.03)	(0.07)	(0.11)	(0.11)
The Greens	0.20***	−0.12	0.10	−0.03	0.46	0.93***
	(0.07)	(0.16)	(0.13)	(0.19)	(0.32)	(0.32)
FDP	−0.15***	0.01	−0.03	−0.05	−0.21	−0.09
	(0.04)	(0.08)	(0.07)	(0.30)	(0.49)	(0.49)
The Left	0.17**	0.32**	0.26**	0.15	−0.06	−0.23
	(0.07)	(0.15)	(0.13)	(0.21)	(0.35)	(0.35)
Others	0.00	−0.04	−0.03	−0.11*	−0.14	−0.11
	(0.01)	(0.03)	(0.02)	(0.06)	(0.09)	(0.09)
N	7103	7103	7103	7103	7103	7103

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parenthese. Columns 1-3 refer to the OLS regressions of voting power for the respective parties on the three direct policy instruments (tax rate multipliers). Each regression contains a population control as well as dummies for each party if it did not receive any votes and state-election dummies. Columns 4-6 refer to the estimates of the IV regressions, in which the voting power measures are instrumented by the shifts in voting power around a threshold in close elections. The instrument is based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. *Source*: Own calculations.

Table 6: IV results voting power - end versus start of the election period

	Average Taxes		
	Trade Tax	Property Tax A	Property Tax B
	1	2	3
	Panel 1: Tax rates at the END of the election period		
SPD	-0.14	-0.24^{**}	-0.27^{**}
	(0.09)	(0.13)	(0.14)
The Greens	-0.24	0.63^{*}	0.97^{**}
	(0.25)	(0.38)	(0.38)
FDP	0.23	-0.11	-0.01
	(0.38)	(0.58)	(0.58)
The Left	0.01	-0.22	-0.24
	(0.27)	(0.42)	(0.42)
Others	-0.07	-0.04	-0.03
	(0.07)	(0.11)	(0.11)
	Panel 2: Tax rates BEFORE the election period		
SPD	0.03	-0.10	-0.15
	(0.10)	(0.14)	(0.14)
The Greens	0.04	0.04	0.06
	(0.26)	(0.37)	(0.38)
FDP	-0.46	-0.84	0.18
	(0.38)	(0.55)	(0.57)
The Left	0.11	0.04	-0.26
	(0.25)	(0.36)	(0.38)
Others	-0.10	-0.10	-0.04
	(0.07)	(0.10)	(0.10)

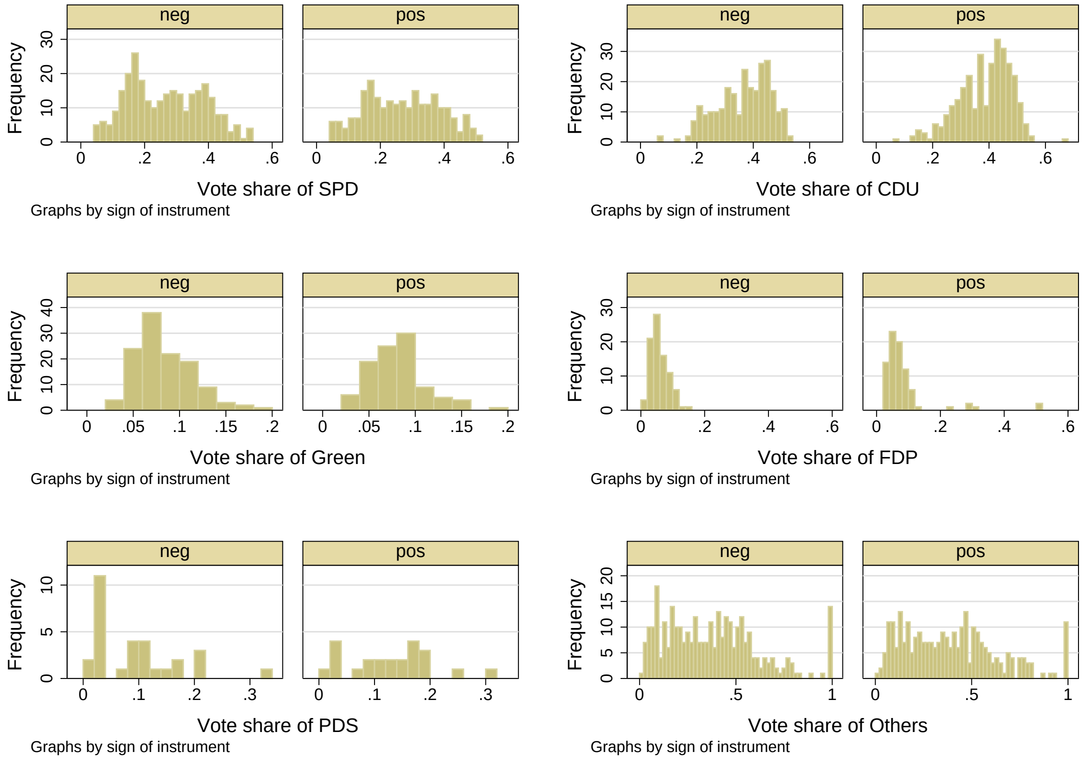
Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The table shows the estimates of IV regressions for voting power, in which the the tax rate outcomes are measured at the end of the election period (panel 1) and just before the start of the election period (panel 2). The instrument is based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. The number of observations in each regression is 7103.*Source*: Own calculations.

Table 7: IV results voting power - aggregate spending and revenues sources

	Aver spending (per cap)			Aver revenue (per cap)		
	Total	Personnel	Investment	Stock in Debt	Fees	Invest Grants
	1	2	3	4	5	6
SPD	1.50	0.13	1.84	0.00	0.23	−0.09
	(2.12)	(0.27)	(1.31)	(0.00)	(0.27)	(0.69)
The Greens	4.26	−0.44	−0.01	0.00	−0.23	−1.15
	(5.94)	(0.76)	(3.66)	(0.01)	(0.75)	(1.94)
FDP	0.47	−0.30	−2.41	0.00	−0.12	−3.10
	(9.12)	(1.16)	(5.62)	(0.03)	(1.16)	(2.98)
The Left	−5.27	0.16	−5.06		0.12	−3.46
	(6.56)	(0.84)	(4.05)		(0.83)	(2.15)
Others	−2.45	−0.12	−0.87	0.00	0.16	0.13
	(1.75)	(0.22)	(1.08)	(0.00)	(0.22)	(0.57)

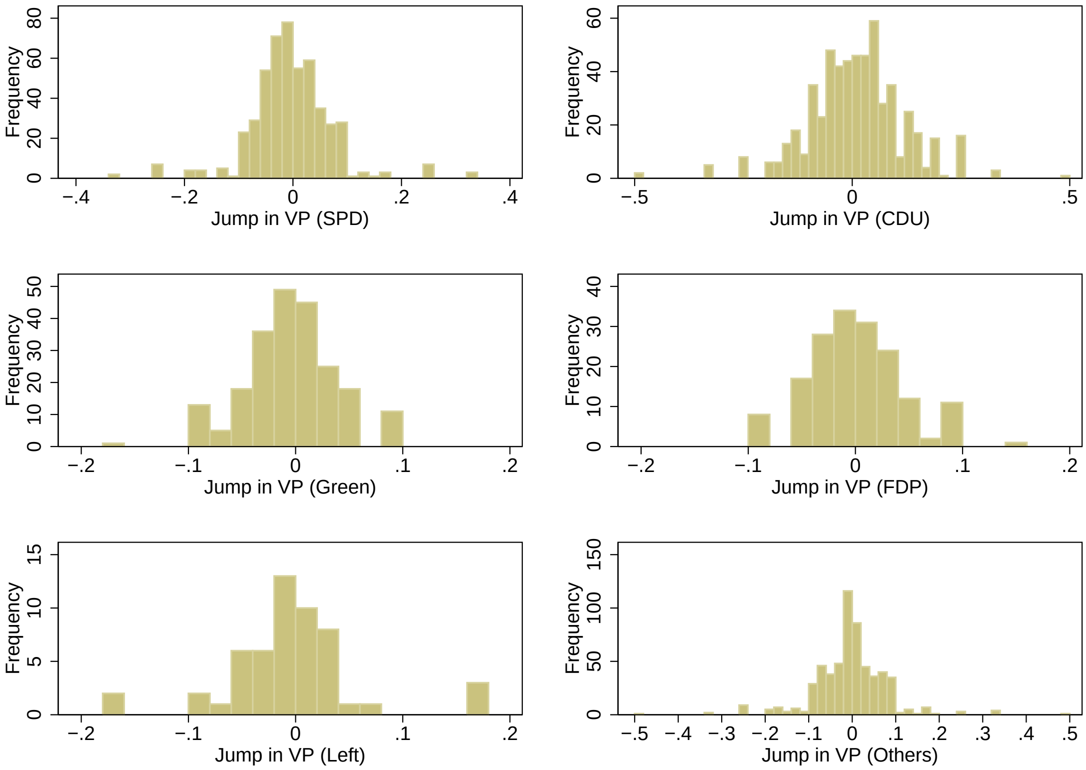
Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The table presents IV voting power estimates for the following additional fiscal outcome variables (per capita): total spending, spending on personnel, investment spending, stock of debt, Revenue from fees for communal services and revenue from higher level grants for investment. The instrument is again based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. The number of observations in each regression is 7103 (except for column 4 for which we only have data for Bavaria, N=3951). *Source*: Own calculations.

Figure 2: Distribution of vote share by sign of instrument



Notes: This figure graphically illustrates the distribution of the vote share of the respective parties separated into the distinct subsample when the instrument for voting power was positive or negative. A positive instruments indicates that an observation for this party has narrow obtained the extra voting power (through one narrow seat), but was close to lose that seat. A negative instruments relates to the reverse accordingly. The graph excludes all observations that were not coded as close elections. The six different panels show the respective graphs for each party. Note that, the graph for the "Others" (lower right) is special as it shows the aggregate vote share of all other parties. *Source:* Own calculations.

Figure 3: Design 1



Notes: This figure graphically illustrates the distribution of the voting power jump that is used as instrument in the voting power analysis. The graph excludes all observations that were not coded as close elections. The six graphs show the distribution of each partyrespectively. Note that, the graph for the "Others" (lower right) is special as it shows the aggregate mean of the instrument over the Others parties. *Source:* Own calculations.

Table 8: IV results voting power - varying the instrument

	Average Taxes		
	Trade Tax	Property Tax A	Property Tax B
	1	2	3
	Panel 1: Adding the squared jumps in vp		
SPD	-0.06	-0.18	-0.25^{**}
	(0.07)	(0.11)	(0.11)
The Greens	-0.06	0.42	0.90^{***}
	(0.19)	(0.31)	(0.31)
FDP	-0.10	-0.27	-0.11
	(0.25)	(0.40)	(0.40)
The Left	0.12	-0.04	-0.21
	(0.20)	(0.34)	(0.33)
Others	-0.12^{**}	-0.14	-0.11
	(0.06)	(0.09)	(0.09)
	Panel 2: Using only 1/ -1 information		
SPD	-0.22^{**}	-0.38^{**}	-0.41^{**}
	(0.10)	(0.17)	(0.17)
The Greens	-0.05	0.39	0.79^{**}
	(0.22)	(0.36)	(0.36)
FDP	-0.26	-0.27	-0.26
	(0.30)	(0.50)	(0.50)
The Left	0.04	-0.39	-0.54
	(0.30)	(0.50)	(0.50)
Others	-0.06	-0.16	-0.10
	(0.10)	(0.16)	(0.16)

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The table shows a robustness test of the voting power analysis on tax rate multipliers in which we have altered the instrument that we use. In panel 1, we additionally use the squares of the jump in the first stage regression. In panel 2, we limit the information of the instrument to the sign of the jump and use indicator variable for a negative or a positive jump. The instrument is again based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. The number of observations in each regression is 7103. *Source*: Own calculations.

Table 9: IV results voting power - alternative close samples

	Average Taxes		
	Trade Tax	Property Tax A	Property Tax B
	1	2	3
	Panel 1: 2% closeness definition		
SPD	−0.07	−0.13	−0.16*
	(0.05)	(0.09)	(0.09)
The Greens	−0.11	0.15	0.53**
	(0.15)	(0.24)	(0.24)
FDP	0.19	−0.15	0.03
	(0.20)	(0.33)	(0.32)
The Left	0.14	0.10	−0.08
	(0.19)	(0.32)	(0.31)
Others	−0.05	−0.03	−0.09
	(0.05)	(0.08)	(0.08)
	Panel 2: 0.5% closeness definition		
SPD	0.01	−0.19	−0.32*
	(0.11)	(0.18)	(0.18)
The Greens	−0.29	0.14	0.91
	(0.38)	(0.62)	(0.62)
FDP	0.16	−0.47	−0.53
	(0.35)	(0.57)	(0.57)
The Left	0.28	−0.19	−0.49
	(0.37)	(0.60)	(0.60)
Others	−0.12	−0.18	−0.18
	(0.08)	(0.13)	(0.13)

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The table shows a robustness test of the voting power analysis on tax rate multipliers in which we use a different definition of closeness in the perturbation used to calculate close elections. In the upper panel 1, we perturb the vote vector using a variance of 2% of the vote count. In panel 2, we use choose a narrow band for the variance of 0.5% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. The number of observations in each regression is 7103. The number of close elections in those specifications is as follows: CDU - 1080 (2% sample) and 284 (0.5% sample), SPD - 955 and 261, The Greens - 419 and 113, FDP - 319 and 84, PDS - 89 and 26, Others - 1108 and 294. *Source*: Own calculations.

Table 10: IV results voting power - by states

	Average Taxes		
	Trade Tax	Property Tax A	Property Tax B
	1	2	3
	Panel 1: Bavaria		
SPD	−0.36**	−0.66**	−0.68**
	(0.15)	(0.26)	(0.27)
The Greens	−0.32	1.00*	1.06*
	(0.34)	(0.58)	(0.60)
Others	−0.06	−0.04	−0.04
	(0.10)	(0.17)	(0.17)
N	4028	4028	4028
	Panel 2: Hesse		
SPD	−0.00	0.02	0.17
	(0.18)	(0.44)	(0.39)
The Greens	0.59	0.66	1.90**
	(0.42)	(1.03)	(0.92)
FDP	1.09	0.45	2.37
	(0.69)	(1.69)	(1.52)
Others	−0.04	0.14	0.29
	(0.23)	(0.57)	(0.51)
N	823	823	823
	Panel 3: NRW		
SPD	0.12	0.13	0.16
	(0.15)	(0.25)	(0.24)
The Greens	−0.11	−0.23	0.31
	(0.27)	(0.46)	(0.43)
FDP	−0.34	−0.34	−0.36
	(0.25)	(0.42)	(0.40)
Others	−0.30	−0.33	0.06
	(0.23)	(0.39)	(0.36)
N	736	736	736
	Panel 4: Thuringia		
SPD	0.26*	0.20	−0.02
	(0.15)	(0.14)	(0.15)
The Left	0.54*	0.34	0.17
	(0.30)	(0.29)	(0.30)
Others	−0.07	−0.05	−0.07
	(0.11)	(0.10)	(0.10)
N	1455	1455	1455

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. In this table we repeat the analysis of the effect of voting power on tax decisions by each state. Panel 1 through 4 show the estimates for Bavaria, Hessen NRW and Thuringia respectively. We excluded specific parties if there were to few cases within the state that close elections occured. The instrument is again based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. *Source*: Own calculations.

Table 11: IV results weighted voting power

	Average Taxes		
	Trade Tax	Property Tax A	Property Tax B
	1	2	3
	Panel 1: Weighted by distance in *one* dimensional policy space		
SPD	−0.08	−0.18	−0.26**
	(0.07)	(0.11)	(0.11)
The Greens	−0.02	0.28	0.74***
	(0.17)	(0.28)	(0.27)
FDP	−0.17	−0.14	−0.01
	(0.19)	(0.31)	(0.31)
The Left	0.19	0.20	−0.22
	(0.29)	(0.47)	(0.47)
Others	−0.11*	−0.18*	−0.13
	(0.06)	(0.09)	(0.09)
	Panel 2: Weighted by distance in *two* dimensional policy space		
SPD	−0.08	−0.19**	−0.26**
	(0.07)	(0.11)	(0.11)
The Greens	−0.01	0.42	0.90***
	(0.19)	(0.31)	(0.30)
FDP	−0.14	−0.18	−0.05
	(0.26)	(0.42)	(0.42)
The Left	0.24	0.16	−0.29
	(0.33)	(0.54)	(0.54)
Others	−0.11*	−0.17*	−0.13
	(0.06)	(0.10)	(0.09)

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. In this table we use weight voting power in coalitions with an approximated party distance. Panel 1 show the results on tax making when we use a traditional one-dimensional distance measure. In panel 2, we instead use a two dimensional policy space. The instrument is again based on 1000 perturbations of the vote vector using a variance of 1% of the vote count. Each regression contains population controls and council size, dummies for each party if it did not receive any votes, state-election dummies, and a polynomial control function that is quadratic in each party's vote share. The specification is estimated using municipality fixed effects. *Source*: Own calculations.

Figure 4: Party locations in policy space

Notes: This figure presents the assumed party positions in a two dimensional policy space. The dimensions can be thought of as pro-market versus state ruled (vertical axis) and conservative versus progressive (horizontal axis). *Source:* Own graphic.