DIRECT DEMOCRACY AND GOVERNMENT SIZE: EVIDENCE FROM SPAIN

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(*) A previous version of this paper covered a more limited sample period (2002-2011). I am thankful to Manuel Bagüés for sharing the budget data for the entire period in which the 100-inhabitant threshold separated the two systems (1987-2011).

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Abstract

Direct democracy is spreading across the world, but little is known about its effects on policy. I provide evidence from a unique scenario. In Spain, national law determines that municipalities follow either direct or representative democracy, depending on their population. Regression discontinuity estimates indicate that direct democracy leads to smaller government, reducing public spending by around 8%. Public revenue decreases by a similar amount and, therefore, there is no effect on budget deficits. These findings can be explained by a model in which direct democracy allows voters to enforce lower special-interest spending.

Keywords: public finance, political economy, direct democracy, government spending, deficits, budget, regression discontinuity.

JEL classification: D7, H.

Resumen

La democracia directa se está expandiendo por el mundo, pero aún se sabe poco sobre sus efectos en las políticas públicas. En este artículo se da evidencia a partir de un escenario único. En España, la Ley determina que los municipios se rijan por un sistema de democracia directa o representativa, dependiendo de su población. Mediante una regresión de discontinuidad, se estima que la democracia directa lleva a un menor tamaño del gobierno, reduciendo el gasto público en alrededor de un 8 %. Los ingresos públicos disminuyen en una cantidad similar y, por tanto, no hay un efecto en el déficit. Estos resultados pueden explicarse mediante un modelo en el que la democracia directa permite a los votantes imponer un menor gasto en intereses particulares.

Palabras clave: economía pública, economía política, democracia directa, gasto público, déficit, presupuestos, regresión de discontinuidad.

Códigos JEL: D7, H.

1 Introduction

A pressing concern about representative democracy is that it leads to a government that is too large.[1] As noted by Besley and Coate (2008), that citizens have only one vote to cast for candidates who have responsibility for choosing a bundle of issues may lead to policies that are incongruent with the position favored by the majority. In particular, if politicians are interested in increasing spending to favor special-interest groups, then representative democracy will result in overspending. One way to align policy with the preferences of the median voter is to expand the use of direct democracy, in which people decide policy initiatives directly.[2] By issue unbundling, direct democracy allows the median voter's preferences to prevail along different dimensions and, therefore, constrains politicians' ability to increase expenditures (Matsusaka (2005)).

The theoretical development of this argument coincides with a dramatic expansion in the use of direct democracy across the world. In emerging countries, decisions at the local level are increasingly adopted in participatory meetings. These include the *Gram Panchayat* in India, the participatory budget (*Conselho do Orçamento Participativo*) in Brazil, and many others in countries such as Bolivia, Indonesia, Mexico, Peru, Uganda, and Venezuela. In the United States, town meetings are widely used at the local level in New England, and voters have decided more than 1,600 state-wide ballot propositions in the 21st century.[3] In Europe, direct voter participation is a hallmark of Switzerland and has become increasingly popular at the local level in Germany. Its use also has been debated in Italy, the Netherlands, Spain, and in European Union's institutions.

In this paper, I empirically test whether direct democracy in the form of participatory meetings reduces the size of government. Despite the rapid expansion of direct democracy in recent years, settings with an exogenous source of variation in the use of direct democracy are rare. As a result, it is difficult to obtain credible causal estimates on the effects of direct democracy.[4]

[1]Besley and Case (2003) argue that "there is a widespread belief that agency problems lead to a government that is too large" under representative democracy.

[2]Direct democracy is a form of democracy in which people decide policy initiatives directly, as opposed to representative democracy, in which people vote for representatives who then decide policy initiatives. The main forms of direct democracy are participatory or town meetings, referendums, and initiatives.

[3]From 2001 to 2015, 1,645 state-wide ballot measures have been voted in the United States. Source: www.ncsl.org. Also in the US, direct democracy has been one of the battlegrounds of the Occupy Wall Street movement.

[4]A noteworthy exception is Hinnerich and Pettersson-Lidbom (2014), but they study a historical context—Sweden in the period following the introduction of universal suffrage in the 1920s—and focus on welfare spending. Previous literature is summarized by Matsusaka (2005).

I provide evidence from a unique scenario to study the effects of direct democracy on policy. In Spain, national law mandates that municipalities operate under a direct or representative democracy, depending on the size of their population. Municipalities with 100 or more inhabitants follow a representative-democracy system, while those with fewer than 100 inhabitants use direct democracy. Under representative democracy, citizens elect a city council every fourth year, and the council decides on policy. Under direct democracy, the role of the city council is played out in open town meetings held approximately four times a year.

This institutional framework presents several attractive features. First, the use of direct or representative democracy is determined by population size, as mandated by a *national* law. This allows the use of a regression discontinuity design to estimate effects. Second, no other rule changes at the threshold, unlike what is often the case for municipal population thresholds. I therefore can attribute the differences between municipalities at each side of the threshold to the government system and not to some other regulation. Third, the number of observations is large, leading to precise estimates. There are more than 8,000 municipalities in Spain, and, furthermore, many municipalities have a population size close to the threshold (around 2,000 municipalities with a population of 250 of fewer inhabitants—see Appendix Table A1). Fourth, Spanish municipalities have substantial autonomy in both expenditures and revenues. As a result, there is considerable variation in fiscal policy across them. For example, the municipality in the 90$^{\text{th}}$ percentile of expenditures spends four times more than the one in the 10$^{\text{th}}$ percentile.

I employ a rich panel dataset that contains yearly data from the budgets of municipalities in the period 1988–2011. An issue that requires careful attention is that some sorting is observed around the threshold. I show that a regression discontinuity design extended with municipality and year fixed effects, that exploits switches in government system of municipalities over time, deals well with this issue and provides credible estimates. I show that there is very good covariate balance around the threshold, and conduct a battery of robustness checks to assess the validity of the empirical approach, including testing for pretrends in the outcomes of interests, separate analyses for switches into and out of direct democracy, placebo tests at other population thresholds, and donut regressions following Barreca, Guldi, Lindo, and Waddell (2011), in which observations for which the sorting is concentrated are dropped. The results from these tests provide assurance of the validity of the empirical strategy.

The main finding is that direct democracy reduces the size of government. In the preferred specification, direct democracy reduces spending by almost 8%. Revenues are decreased by a similar amount, and budget deficits are therefore equal under both systems. I explain the results with a model, based on Besley and Coate (2008), that I develop to combine common arguments from two different strands of the literature: that direct democracy curbs special interest spending, and that it aligns policy with the preferences of the median voter by issue unbundling. In the model, a majority of the population (citizens) prefer low special-interest spending, but they care more for some other dimension of policy. There is, however, a minority (special interests) that want high spending and whose vote is determined by special-interest spending. Under representative democracy, the two issues get bundled, as individuals have only one vote (to elect a representative who will implement policy). The consequence is that, even though the majority prefer low spending, parties propose high spending in equilibrium to attract the votes of the special interests. As a result, special-interest spending is high in representative democracy (independent of which candidate wins the election). In contrast, in direct democracy, the two dimensions are unbundled, as individuals vote separately on issues. In this case, the preferences of the median voter prevail along the two dimensions, and this results in low special-interest spending.

Although the results can be explained by the the mechanism highlighted in this model, I consider four other potential mechanisms: elite capture in direct democracy, differential costs of participation in meetings and elections, direct democracy directly affecting what policy individuals prefer, and municipalities in direct democracy obtaining more transfers from upper-level governments. To shed light on their plausibility, I present five additional sets of results. First, there are no partisan differences in spending in representative democracy. This is consistent with the model, which predicts that the two parties should converge to high spending. Second, expenditures and revenues are equally volatile in direct and representative democracy. This suggests that the costs of participation in meetings do not drive the results—if that were the case, policy should be more volatile in direct democracy (Osborne, Rosenthal, and Turner (2000)). Third, against a hypothesis of gradual learning, the effects do not seem to change over time: they appear in the first term a municipality works under direct democracy, and remain similar as municipalities spend more time under direct democracy. Fourth, direct democracy does not affect subsequent elections' behavior (e.g., being exposed to participatory meetings does not lead to more voter turnout in subsequent elections). Fifth, direct-democracy municipalities do not obtain more transfers from upper-level governments. In light of this evidence, and of the political and social characteristics of Spain, I conclude that, although it is not possible to rule out completely that some of these mechanisms are at work, the weight of the evidence indicates that they do not drive the results.

This paper contributes to the growing literature on the impact of political institutions on public policy (see, for example, Besley and Case (2003) and Persson and Tabellini (2005)). More specifically, it expands our knowledge about the policy implications of participatory institutions. As mentioned, these institutions are increasingly used in villages across the world, and have been subject of recent work, both theoretical and empirical. At the theory level, several papers have modeled decision-making in meetings (Aragonès and Sánchez-Pagés (2009), Osborne, Rosenthal, and Turner (2000), Turner and Weninger (2005)). At the empirical level, the literature has worked on estimating the impact of participatory institutions on several outcomes: for example, Besley, Pande, and Rao (2005) find that village meetings in South India improved the targeting of welfare programs for the poor; Hinnerich and Pettersson-Lidbom (2014) find that town meetings reduced welfare spending in the context of Sweden in the 1920s and 30s; and Olken (2010), in a randomized controlled trial, finds that the use of plebiscites improved voter satisfaction with public policy in Indonesian villages. The present paper adds to that literature by providing the first quasi-experimental evidence on how town meetings affect public spending and taxation decisions in a contemporary setting.

This paper also contributes to the literature, pioneered by De Haan and Sturm (1997) and Roubini and Sachs (1989), that analyzes how public budgets are shaped by political institutions, and, in particular, by direct-democracy institutions. Evidence from the US and Switzerland suggests that initiative and referendums reduce public spending (see Matsusaka (1995) for the US, and Feld and Matsusaka (2003) and Funk and Gathmann (2011) for Switzerland). Berry (2014), however, has recently questioned the causal interpretation of the findings for the US, and Asatryan, Baskaran, Grigoriadis, and Heinemann (2015) and Asatryan (2016) have found that direct democracy at the local level increased expenditures in Germany. This paper contributes to that literature by providing evidence from an unexplored setting, in which, unlike what happens in the aforementioned countries, the use of direct democracy is determined by population size, therefore reducing endogeneity concerns.

Finally, this paper expands the work that uses regression discontinuity designs to estimate the effects of institutional rules on policy (for example, Bordignon, Nannicini, and Tabellini (2016), Ferraz and Finan (2009), Fujiwara (2015), Gagliarducci, Nannicini, and Naticchioni (2011), Gagliarducci and Nannicini (2013), Hinnerich and Pettersson-Lidbom (2014), and Pettersson-Lidbom (2012)).

The rest of the paper is organized as follows. Section 2 presents the theoretical framework. Section 3 provides background on the Spanish government systems. The empirical strategy and the data are outlined in Sections 4 and 5. Section 6 presents the main results. The robustness of the results is the focus of Section 7. Section 8 explores other potential mechanisms. Section 9 concludes the paper.

2 Theoretical Framework

Social scientists have long been interested in how switching from representative to direct democracy might change policy. One strand of the literature argues that, in representative democracy, an agency problem exists between voters and their elected representatives due to free-rider problems in monitoring and disciplining officeholders (Matsusaka (2005)). This gives elected officials leeway to pursue costly policies that are not in the interests of their constituents, resulting in overspending. Frey (1994) argues that a model that pictures politicians as forming a coalition against taxpayers and voters seems to be an apt illustration of representative democracy. In this view, the elected representatives are a well-defined group that jointly reaps rents or cartelizes against the interests of citizens. Besley and Case (2003), in summarizing this literature, conclude that "there is a widespread belief that agency problems lead to a government that is too large" under representative democracy. The use of direct-democracy institutions is a way to address this issue. By allowing people to vote directly on policy, direct democracy gives them a way to circumvent representative institutions that may have been captured by elites or other special interests and aligns policy with the preferences of the median voter (Matsusaka (2005)).

Another strand of the literature emphasizes that, under representative democracy, citizens have only one vote to cast for candidates who have responsibility for choosing a bundle of issues (Besley and Coate (2008)). This may lead to policies that are incongruent with the position favored by the median voter in some of the dimensions. By unbundling issues, direct democracy allows the median voter's preferences to prevail along different dimensions.

I formalize these ideas in a simple model, adapted from Besley and Coate (2008), that I summarize here and present in full in the Appendix. In the model, policy is two-dimensional. There is a main ideological issue (henceforth, ideology) and a secondary issue (special-interest spending). Although the majority of individuals prefer low (possibly zero) special-interest spending, there exists a minority that prefer high spending, perhaps because they will benefit directly from it. The majority care more about ideology but the minority care more about spending. Under representative democracy, policy is implemented by a representative elected in an election between two candidates, proposed by two political parties. Assuming the minority is sufficiently large, in the unique equilibrium, both parties propose high special-interest spending, against the wishes of the majority. In contrast, under direct democracy, individuals vote separately on the ideological issue and on special-interest spending, and the position favored by the median voter in each dimension is chosen. Direct democracy, therefore, results in low special-interest spending, unlike what happens under representative democracy.

The model, therefore, blends the points made by two different strands of the literature: that direct democracy curbs special interest spending, and that it aligns policy with the preferences of the median voter through the unbundling of issues. In addition, the model predicts that spending in representative democracy should not depend on which party is in office. In Section 6, I provide empirical evidence that supports this additional prediction, and discuss other alternative mechanisms.

3 Institutional Background

In this section, I provide some institutional background and describe the two government systems that are used by Spanish municipalities. Spain is a very decentralized country. It is politically divided into 17 regions, 50 provinces, and more than 8,000 municipalities, which are administered by local governments. Local governments spend 13% of the overall spending of the country (Gil-Ruiz Gil-Esparza and Iglesias Quintana (2007)) and have substantial autonomy. National law (Local Government Regulatory Law, *Ley de Bases del Régimen Local*) requires them to provide a variety of services, including public lighting, waste collection, street cleaning, road paving, household water supply, sewerage, access to villages, and food and beverage control. In addition, they usually provide other services useful to the municipality, such as organizing local festivities or providing tourist information. Local governments can decide how much to spend on the required goods and services and whether to provide additional goods and services. There is considerable variation in expenditures across municipalities: The 90–10 ratio is 4.6.[5]

Local governments obtain their own revenues from taxes and fees: they can set the tax rates—within certain limits imposed by national law—and the prices and user fees of the goods and services that they provide. According to national law, municipalities can impose five different taxes: property tax *(impuesto de bienes inmuebles)*, a tax on economic activities *(impuesto de actividades económicas)*, a motor vehicle tax *(impuesto sobre los vehículos de tracción mecánica)*, a tax on improvements to real property *(impuesto de instalaciones, construcciones y obras)*, and a tax on the increased value of urban land *(impuesto sobre el incremento de valor de los terrenos de naturaleza urbana)*.[6] As happens with expenditures, there is substantial variation in revenues across municipalities: The 90–10 ratio is 4.5.

In addition to these own expenditures and revenues, some expenditures are financed through transfers from upper-level governments.[7] Throughout the paper, I focus on the municipalities' own expenditures and revenues (that is, overall revenues and expenditures net of transfers), as they are the ones that can be controlled by local governments. In Section 8, I study if the amount of received transfers can drive the results of the paper.

The Law on Budget Stability *(Ley de Estabilidad Presupuestaria)* states that municipalities must have a balanced budget.[8] However, no penalty is stipulated for municipalities that fail to comply with the law. Indeed, there was a deficit in 39% of the budgets in the sample, and in many cases a sizable one: for example, in 14% of the budgets I observe a deficit of more than 3% of revenues (see the histogram of

[5]I calculate this number by taking the average value of expenditures by municipality over time for those municipalities. This way, I do not use the cross-time variation, which would overstate the variation in expenditures.

[6] The first three are required taxes that national law forces municipalities to levy while the last two are optional taxes. For both required and optional taxes, municipalities are free to set the tax rates and, in some cases, introduce exemptions and deductions, within certain limits imposed by national law. For example, for the most important tax, the property tax, they are allowed to set the tax rate on urban land from 0.4 to 1.1% of the property value.

[7]Transfers account for approximately 46% of overall expenditures and revenues.

[8]In 2011, in the midst of the European debt crisis, the Constitution was amended to include this principle.

deficits in Appendix Figure A2).[9] For this reason, it makes sense to test whether direct democracy has an effect on deficits.

Municipalities must follow a government system that is determined by their population size one year before the local elections, which are held simultaneously in all municipalities in the country every four years. Therefore, municipalities change the government system at most once every four years. During the sample period (1988–2011), municipalities with 100 or more inhabitants had to followed a representative-democracy system, while those with fewer than 100 inhabitants followed a direct-democracy system (called "open council", or *concejo abierto*). Appendix Table A1 shows the number of direct-democracy municipalities covered in the sample, by term. Between 603 and 827 municipalities operated under direct democracy, with the number increasing over time as a consequence of the declining population of small municipalities.

In the representative-democracy system, individuals elect a city council every four years on local election day. The council elects a mayor among its members and is entitled to approve the budget, decide on expenditures in various fields, control the governing bodies, and to decide on the roll-call vote of confidence on the mayor. The mayor chairs the meetings of the council, casts the decisive vote in the event of a tie, heads the local police, and appoints mayoral deputies and cabinet members, among other responsibilities. The size of the city council and the electoral rules differ according to the population of the municipality. In particular, there is an important change in the electoral system at 250 inhabitants.[10] Municipalities below that population size elect a council of five members in an open-list, plurality-at-large election.[11] To avoid dealing with multiple thresholds, and given that the estimation will exploit municipalities with a population size close to the 100-inhabitant threshold, throughout the paper I restrict the sample to municipalities with 250 or fewer inhabitants.[12]

[9]Deficits can be financed by issuing debt or by reducing cash balances. The Law on Local Government Financing *(Ley de Haciendas Locales)* authorizes municipalities to issue debt, but with some limits. For example, if the debt outstanding exceeds 110% of current revenues in the previous year, municipalities must ask for the authorization of the regional or national governments and present a consolidation plan (see articles 48-55 of the *(Ley de Haciendas Locales)* for a complete description of the requirements). However, that there was no penalty associated with those regulations led many municipalities to not observe the law (Vila (2012)).

[10]Sanz (2015) studies the effects of that discontinuity on voter turnout.

[11]More specifically, political parties can present candidate lists of up to five candidates, and voters can vote for up to four candidates from the same or different party lists. The five most-voted candidates are elected members of the council.

[12]Also, I drop from the analysis the nine newly created municipalities as well as the municipalities from which they seceded.

In the direct-democracy system, the role of the city council is played through open town meetings, so most relevant decisions, including the budget, must be approved directly by individuals in a pure direct democracy framework.[13] (In Appendix B, I describe the origins and the evolution in the application of this system.) The specific working rules of the meetings may differ across municipalities, as the law states that municipalities can work according to the local traditions. However, national law *(Real Decreto 2568/1986)* provides some guidelines. Any individual eligible to vote (age 18+) may attend the meetings. The meetings are chaired by the mayor, who is elected by first-past-the-post on local election day. Town meetings are to be called on a Sunday or a holiday at least once every three months. A quorum of one-third of the population is required. Citizens can delegate their vote to any other citizen of the municipality, but no citizen can represent more than a third of all individuals.[14] Decisions are adopted by simple plurality rule. Data for attendance are not publicly available, but I have collected data by hand from the minutes of the meetings in one Spanish province.[15] Average attendance in the sample was 11.6%. Taking vote delegation into account, the average share of represented voters was 43.7%.

4 Data

Data for the public budgets and population size of municipalities are from the Spanish Ministry of Finance *(Ministerio de Hacienda)* and are publicly available for 73.6% of annual budgets.[16] During the sample period, local elections were held every four years from 1987 to 2011, so the data set covers six terms. Because local elections are in May or June, the (possible) change in government system happens

[13]Figure 1 shows a meeting in the municipality of Madarcos, in the region of Madrid.

[14]The delegation of the vote must be written and can be for a specific meeting or permanent.

[15]The minutes of the meetings are stored in the archives of the regional administration. I collected data from the province of Valladolid in the region of *Castilla y León* from the years 2006 and 2008. Data on attendance were available for 31% (11/36) of municipalities, as attendance at the meetings is recorded only if the secretary wishes to do so.

[16]Municipalities were required to report their budgets to the Ministry of Finance, which makes them public. However, data from some budgets are missing because either they did not comply with their obligations or because their data have not been digitized. A placebo test shows that direct democracy does not have an effect on a dummy variable that indicates whether the observation is available (see Appendix Table A8).

halfway through the year. As spending and revenues are determined by the budget approved at the end of the previous year, I assign election-year observations to the previous term.[17] Appedix Figure A1 provides a graphical representation of this information.[18]

Table 1 shows the summary statistics. The first three variables are the main outcomes: real expenditures per capita *(Expenditures)*, real revenues per capita *(Revenues)*, and budget deficit *(Deficit)*. To obtain the variables in real terms, I divide the nominal variables by the GDP deflator.[19] The deficit is the difference between expenditures and revenues. The average municipality collects €647 per capita per year in taxes and fees, and spends €671. Therefore, the average deficit is €24 per capita per year. Appendix Figure A2 shows the histograms for these variables. The next four variables, obtained from the Spanish Ministry of Interior *(Ministerio del Interior)*, are used to study the relevance of political parties in driving the results. *RW Mayor* and *LW Mayor* are dummy variables that indicate whether the mayor is from the main right-wing party (Popular Party, PP) or the main left-wing party (Socialist Party, PSOE). With the sample restricted to those elections in which these two were the two most-voted parties, the next variables are the percent-point difference between the PP and the PSOE *RW Difference*, and a dummy that indicates whether the PP was the most voted (so that a positive (negative) value of *RW Difference* implies $RW\ Winner = 1$ ($RW\ Winner = 0$)). The next six variables, provided by the National Statistics Institute *(Instituto Nacional de Estadística)*, are from national Congress electoral results, and are used to test

[17]In Spain, the fiscal year coincides with the calendar year, and budgets for year y are approved in November or December of year $y-1$.

[18]If expenditures and revenues are sticky, expenditures and revenues for municipality m at year y are partially determined by the government system that municipality m followed in years before y. When a municipality m switches systems, this implies that some of the expenditures and revenues for year y may be determined by a government system that does not correspond to the government system municipality m is following at that year. In general, this will make the estimated coefficients closer to zero than in a scenario with no changes in government system. These considerations, however, are not likely to play a significant role in practice. First, most expenditures and revenues in small municipalities are decided on a year-to-year basis. For example, it is rare that they incur in multi-year capital expenditures, unlike what happens in larger municipalities. It is true, nonetheless, that other expenditures, such as personnel, could be more sticky. Second, if sticky policies were playing a large role in the estimation, we should expect to see that the effects of direct democracy on policy increase (in absolute value) over time, as municipalities spend more time in a given system. However, I find that the effects of direct democracy on policy are already produced in the first year that a municipality switches into direct democracy and do not vary significantly over time (see Section 8).

[19]Data for the deflator are from the Spanish National Institute for Statistics.

for covariate balance around the threshold. They are the shares of votes for the three main parties in Spain—the right-wing PP (*Votes Right*), the left-wing PSOE (*Votes Left*), and the far-left-wing United Left (*Votes Far Left*)—, the difference in the share of votes for the two most-voted parties (*Votes Difference*), the percentage of votes for the most-voted party (*Votes Winner*), and voter turnout (*Turnout*).[20] The final six variables are demographic variables, which I also employ to study covariate smoothness around the threshold, are the average age in the municipality (*Mean Age*); the share of young (*Young*), middle-aged (*Middle-Aged*), and old (*Old*) individuals; the share of immigrants (*Immigrants*); and the share of EU immigrants over total immigrants (*EU Immigrants*).[21]

5 Empirical Strategy

A discontinuity in the density of population sizes is observed at the threshold (see Figure 2).[22] The shape of the discontinuity raises the concern that some municipalities self-select into representative democracy, and that, as a result, direct- and representative-democracy municipalities differ in other characteristics that may themselves affect policy. I take this issue seriously. First, I will discuss why and how the sorting around the threshold arises, and then I will explain how I deal with this issue in the empirical approach.[23]

In Spain, the official population size of a municipality is given by the number of citizens who are registered in the municipal registry *(padrón municipal)*. Municipalities keep track of all the variations in the population in the public registry and report periodically the data to the National Statistics Institute (INE). The INE validates the information it receives, checking that there is no fraud—for example,

[20]Some election variable files are missing from the official files, and, for this reason, the number of observations is lower than for the budget and population variables. In addition, the sample size is smaller for *Votes Far Left* because the United Left did not run in some regions in some elections. See Appendix C for a precise definition of the election variables.

[21]The precise definition of these variables is in Appendix C. They are available only from 1996. Also note that, in municipalities with no immigrants, the share of EU immigrants is missing by construction. For this reason, the number of observations is lower for *EU Immigrants*.

[22]The discontinuity is significant at the 1% level, according to McCrary (2008)'s test.

[23]Sorting in regression discontinuity designs that use population thresholds is studied in depth by Eggers, Freier, Grembi, and Nannicini (2015), who provide evidence that sorting also exists in France, Germany, and Italy. They also argue that, even when extensive strategic sorting is taking place, a regression discontinuity design "may still be an attractive approach to studying policy effects; after all, the alternative is to study settings where all political units (not just those that are able to manipulate their population figures) choose which policies to adopt".

it ensures that, for every registration, there is a corresponding unregistration in another municipality—and, yearly, makes the final population figures public. While this system makes it difficult to imagine that there is direct fraud or manipulation of population figures, sorting around the threshold can appear as mayors (or other local politicians) persuade some individuals to register in the municipality, with the goal of reaching the population threshold and falling into representative democracy. This is facilitated by the fact that individuals who have dwellings in more than one municipality can, in practice, decide in which of them to register: Although individuals are required to register in the municipality in which they spend the most time, this requirement is almost impossible to monitor and is not enforced in practice.

Naturally, the question is why politicians would prefer to be under representative democracy. There are at least three possible reasons. One is that representative democracy is easier to operate, as it does not require calling town meetings to adopt decisions.[24] A second possible reason is that there are five political positions in representative democracy (the five city councilors), but only one in direct democracy (the mayor). Although, in most cases, these positions are not remunerated, people may still derive non-monetary benefits for holding them. Finally, following the logic of the model presented in Section 2, if the mayor is a special interest, he or she will prefer to be under representative democracy as a means to implement his or her preferred policy.[25]

While the existence of sorting is certainly an issue that requires close attention, it does not imply, by itself, that the estimates will be biased. In his canonical work on sorting, McCrary (2008) explains that there may be a discontinuity in the running variable even when there is no failure of identification, so that a continuous density is not necessary (or sufficient) for identification, except under auxiliary assumptions.

[24]Conversations with mayors and other local government officials make me think that this is probably the most relevant reason.

[25]Of course, if the mayor is a citizen instead of a special interest, he or she will prefer direct democracy. This raises the theoretical possibility of two-way sorting. Although it is not possible to directly test for the existence of two-way sorting, I believe that sorting into direct democracy is negligible. First, as mentioned, mayors and local government officials indicate that the main reason for sorting is the first, i.e., that representative democracy is easier to operate. Second, it is hard to conceive local government officials trying to persuade people to go to register in another municipality. Also, note that the empirical approach and the robustness checks to assess the validity of the strategy, in particular the donut regressions, do not depend on the sorting being one-way.

I start by considering a standard regression-discontinuity design framework:

$$Outcome_{myt} \quad = \tilde{\beta} DirDem_{mt} + f(Population_{mt} - 100) + \tilde{u}_{myt}, \tag{1}$$

where $Outcome_{myt}$ is the outcome of interest in municipality m at year y in term t, $DirDem_{mt}$ is the treatment dummy variable that takes the value of 1 if municipality m follows direct democracy in term t and 0 otherwise, $Population_{mt}$ is the assignment variable (population one year before the local elections), f is a smooth function of the assignment variable, and u_{myt} is an error term. The parameter of interest is $\tilde{\beta}$.

The results from estimating Equation (1) show a reduction of expenditures and revenues in direct democracy (see Appendix Table A2). However, there are some concerns about the validity of this approach. In particular, there is some covariate imbalance (Table A3) and evidence of a pretrend in the outcomes of interest (Table A4).

Next, to deal with these issues, I consider a regression discontinuity design extended with fixed effects. I find that this strategy yields credible estimates of the treatment effects, and use it as the main specification throughout the paper.[26] Specifically, I consider the following estimating equation:

$$Outcome_{myt} \quad = \alpha_m + \gamma_y + \beta DirDem_{mt} + f(Population_{mt} - 100) + u_{myt}, \tag{2}$$

where α_m is a municipality fixed effect and γ_y is a year fixed effect.

Identification is therefore based on switches in government system, of which there are 513 in the sample (see Appendix Table A1).[27] The identification assumption is that the characteristics that make a municipality more likely to sort across the threshold are not correlated with other characteristics that affect policy, *conditional on the fixed effects*. This implies that it is the timing of the sorting that matters.

[26] An empirical strategy that combines regression discontinuity and fixed effects is also followed by Pettersson-Lidbom (2012) and Corbi, Papaioannou, and Surico (2014). Other papers that use related strategies that combine the cross-sectional discontinuity and the longitudinal structure of the data are **?**) Campa (2011), Casas-Arce and Saiz (2015), Cellini, Ferreira, and Rothstein (2010) Deshpande (2014), Grembi, Nannicini, and Troiano (2012), Lemieux and Milligan (2008).

[27] Most of these switches (379) are from representative to direct democracy, as there is a general trend of a falling population in small Spanish municipalities. As a result, the empirical strategy is based mainly on these switches instead of on switches out of direct democracy, which are more suspicious, given the shape of the sorting. In Section 7, I show the results of the estimation based on considering the two types of switches separately.

For example, suppose that the whole sorting process is driven by local government officials who are very good at persuading individuals to register. A violation of the identification assumption would not be that local government officials' characteristics are correlated with other factors that affect policy but, rather, that the precise period in which the sorting is achieved coincides with a change in those other factors.

Although the identification assumption is, of course, not directly testable, I perform five sets of tests to assess the validity of this empirical approach, following previous research that has dealt with similar situations (Pettersson-Lidbom (2012), Barreca, Guldi, Lindo, and Waddell (2011)). First, I check if (conditional on the fixed effects) municipalities at each side of the threshold are similar in other time-varying variables that may have an effect on the outcomes of interest. Second, I test if there is an effect on the outcomes at the *previous* period, and if the results are robust to including municipality-specific trends. Third, I test whether the effect is similar in municipalities that switch into direct democracy and in those that switch out of direct democracy. Finding that the effect is similar would reinforce the credibility of the estimates, as it would be difficult to explain that correlation from some omitted factor. Fourth, I conduct placebo tests at other (artificial) population thresholds. Fifth, I consider donut regressions, following Barreca, Guldi, Lindo, and Waddell (2011). In this test, observations very close to the threshold, where self-selection is concentrated, are dropped from the analysis. The results from all these tests provide assurance of the validity of the empirical strategy.

I use nonparametric local linear regressions to estimate Equation (2), as suggested by Hahn and der Klaauw (2001), Porter (2003) and Gelman and Imbens (2014). A key ingredient to this approach is the bandwidth. A larger bandwidth increases the efficiency of the estimation at the cost of more bias. I choose a baseline bandwidth according to the procedure suggested by Imbens and Kalyanaraman (2012) and provide the results at different fractions of that bandwidth.[28] I use a rectangular kernel, as recommended by Imbens and Lemieux (2008) and Lee and Lemieux (2010). This is equivalent to estimating standard linear regressions over the interval of the selected bandwidth on both sides of the cutoff point. I cluster standard errors at the municipality level (Bertrand, Duflo, and Mullainathan (2004)). Because the running variable is discrete, I also cluster standard errors at the running variable, as recommended by Lee and Card (2008). Therefore, I follow a multi-clustering approach (Cameron, Gelbach, and Miller (2011)).[29]

[28]The results are very similar if I use the optimal bandwidth suggested by Calonico, Cattaneo, and Titiunik (2014). This method selects slightly smaller bandwidths.

[29]The results are very similar if standard errors are clustered just by municipality.

6 Main Results: Effect of Direct Democracy on the Size of Government

This section presents the main results of the paper. Additional results will be discussed in Section 8 to shed light on the mechanisms. Table 2 presents the estimates of the impact of direct democracy on the public finances of local governments. The table shows the results of estimating Equation (2), with the log of *Expenditures*, the log of *Revenues*, and *Deficit* as the outcomes. A graphical representation of the results is shown in Figure 3.[30]

I find evidence of an effect of direct democracy on public expenditures (Panel A of Table 2). Under the preferred specification (Column 1), which uses the bandwidth from Imbens and Kalyanaraman (2012), direct democracy reduces public spending by almost 8%, and the effect is significant at the 1% level. On average, municipalities just to the right of the threshold (100 inhabitants) spend €709 per capita; thus, the effect is equivalent to a decrease of €57 per capita.[31] Next, I study robustness to bandwidth choice and alternative specifications. Columns 2 and 3 show the results for 50% and 150% of the optimal bandwidth (Appendix Figure A3 displays the results graphically for a wide range of bandwidths). Columns 4 to 6 show that the results are robust to fitting high-order polynomials to the whole sample. Finally, as shown in Column 7, the results are also robust to an alternative specification that includes municipality-specific time trends. This specification will be discussed in more detail in the next section.

Panels B and C of Table 2 concern whether the differences in public spending go together with a change in revenues or whether they are created by different deficits. According to the preferred specification (Column 2), direct democracy reduces rev-

[30]To present a graphical representation that incorporates the fixed effects and, therefore, matches the estimated equation, I estimate $Outcome_{myt} = \alpha_m + \gamma_y + \sum_{j=100-OBW}^{100+OBW} \delta_j Population_{j,mt}) + u_{myt}$ using the observations within the optimal bandwidth for $Outcome_{myt}$, where $Population_{j,mt}$ is a dummy that indicates whether municipality m has population size j at term t. Then, in the y-axis, I plot the estimated coefficients $\hat{\delta}_j$. I normalize the coefficients so that the average bin immediately to the right of the threshold takes the value of zero.

[31]Because the outcome variable is the log of expenditures, the exact percent effect on expenditures is $100 * (\exp(-0.0377) - 1) = -7.688$ (Halvorsen and Palmquist (1980)).

enues by 5%, and the effect is statistically significant at the 1% level. The rest of the columns and Figure A3 show that the results are robust to different bandwidths and specifications. Finally, the results for deficits (Panel C) are close to zero and not significant under any specification. Therefore, the evidence indicates that direct democracy reduces expenditures and revenues by a similar amount, without affecting budget deficits.

7 Robustness

In this section, I present five robustness checks to assess the validity of the empirical strategy: placebo tests to study covariate balance at the threshold, placebo tests for pretrends in the outcomes of interest, placebo tests at other (artificial) population thresholds, separate analyses for municipalities that switch into and out of direct democracy, and donut regressions. In the Appendix, I show additional robustness checks to study whether the results change when top-coding outliers (Table A7) and whether the number of missing observations changes at the threshold (Table A8).

7.1 Covariate Smoothness

I study covariate smoothness around the threshold by performing placebo tests: I estimate Equation (2) with a variety of political and demographic variables as outcomes. If the empirical strategy is valid, there should be no effect of direct democracy on these variables.

A possible concern is that municipalities that switch into direct democracy do a more conservative policy only because they are becoming more ideologically conservative. To test for this, I exploit data from national Congress elections. These data are especially suited for this purpose because, although results are reported at the municipality level, the electoral district is at the province level and, therefore, the 100-inhabitant threshold does not play any role in the elections. Further, Congress elections are the most important elections in Spain, and turnout is high (78% in the average municipality during the sample period); thus, they are likely to capture political differences across municipalities.[32] I estimate Equation (2) with previous elections' variables as the outcomes. For year y in term t, I consider Congress elections held during the previous term $t-1$. In particular, I use the share of votes for

[32]Spain is a parliamentary democracy, so there are no direct elections for the executive branch. Citizens elect the Congress, which, in turn, elects the Prime Minister.

the three main political parties—the right-wing PP (*Votes Right*$_{m,t-1}$), the left-wing PSOE (*Votes Left*$_{m,t-1}$), and the far-left-wing United Left (*Votes Far Left*$_{m,t-1}$)—, the difference in the share of votes for the two most-voted parties and the share of votes for the most-voted party (*Votes Difference*$_{m,t-1}$, and *Votes Winner*$_{m,t-1}$), which give a measure of how politically polarized the municipality is, and voter turnout (*Turnout*$_{m,t-1}$), which has been shown to correlate with variables that also may affect people's preferences toward economic policy, such as age, education, or income (Blais and Achen (2010), Matsusaka and Palda (1999)).[33] Next, I study covariate smoothness in demographic variables: average age in the municipality (*Mean Age*); the share of young (*Young*), middle-aged (*Middle-Aged*), and old (*Old*) individuals; the share of immigrants (*Immigrants*); and the share of EU immigrants over total immigrants (*EU Immigrants*). These variables will capture how economic conditions are evolving, as immigrants are more likely to locate in booming municipalities.

The results from these tests are shown Table 3 and represented graphically in Figure 4. All of the variables are smooth at the threshold, with no coefficient being significant at even the 10% level. This provides assurance about the validity of the empirical strategy, as it indicates that, conditional on the fixed effects, municipalities at both sides of the threshold are similar in a variety of political and demographic variables.

7.2 Pretrends in the Outcomes of Interest

In this section, I exploit the longitudinal dimension of the data to test for pretrends in the outcomes of interest. First, I lag the outcome variables four years (so that they correspond to the outcomes at the same year of the previous term) and perform placebo tests by estimating Equation (2) with these lagged variables as outcomes. Finding a discontinuity in the contemporaneous outcome but not in the lagged one would be a strong piece of evidence supporting the validity of the regression discontinuity design (Lee and Lemieux (2010)). The results of these tests are shown in Table 4 and Appendix Figure A4. The coefficients in Table 4 are starkly different to those that show the contemporaneous effect shown in Table 2, and show that there is no effect of direct democracy on the previous term's expenditures, revenues and deficit.

[33]The rest of the parties with representation in Congress during the entire period are regional parties that do not run in the whole country.

An alternative check on the identification strategy is to add municipality-specific time trends to the main specification. In particular, I consider:

$$Outcome_{myt} = \alpha_{0m} + \alpha_{1mt} + \gamma_y + \tilde{\tilde{\beta}} DirDem_{mt} + f(Population_{mt} - 100) + \tilde{\tilde{u}}_{myt}, \quad (3)$$

where α_{0m} is a municipality-specific intercept, as before, and α_{1mt} is a municipality coefficient multiplying the time-trend variable, y. The last column of Table 2 shows that the results are robust to this alternative specification, with direct democracy reducing expenditures and revenues by approximately 5% (results significant at the 10% and 5% levels, respectively).

The results in this section appease the concern that municipalities that switch systems in a given direction were conducting different policies than the rest *before* the switch. In particular, one concern would be that mayors would try to cross the threshold by using taxes or public spending to attract people to the municipality. If this happened, we should see an effect of direct democracy this term on taxes or spending in the previous term. The results rule out this possibility.

7.3 Switches into and out of Direct Democracy

The preferred specification is based on switches in the government system. Given the sorting process and the shape of the density of population sizes, which suggests a preference for representative democracy, one concern is that the results are driven by time-varying omitted variables associated with a switch from direct to representative democracy. I study whether this is the case by performing two separate regressions. I first base the estimation on switches into direct democracy and exclude from the sample the municipality-years that correspond to terms in which a municipality switched out of direct into representative democracy. I then estimate Equation (2) with the restricted sample. Next, I do the reverse and drop the municipality-years that correspond to switches out of direct into representative democracy. If the results from these two different samples point to a reduction in expenditures and revenues, that would reinforce the credibility of the estimates, as it would be difficult to explain that correlation by some omitted factor.

Appendix Table A5 shows the results. For expenditures, the preferred specification for switches into direct democracy yields a point estimate of -6.4% (significant at the 5% level), and the one for switches out of direct democracy, -13.1% (significant at the 1% level). The results for revenues also tend to support the main findings. In switches into direct democracy, direct democracy reduces revenues by 4.2% in the preferred specification—the effect is not significant, but it is significant at the 5% level at both 50% and 150% of that bandwidth. In switches out of direct democracy, the effect is -8.4% (significant at 10%). Finally, the results for deficits are mostly insignificant in the two different subsamples.

7.4 Placebo Tests at Other Population Thresholds

In this section I conduct placebo tests by estimating the effect of crossing population thresholds that are irrelevant (e.g., the effect of having more than 115 inhabitants). Specifically, I create placebo treatments at all other population sizes from 30 to 220 inhabitants, by defining dummies that indicate if the population of a municipality-year is above or below a given population size.[34] I then run equation (2) with every placebo treatment (so I run 190 regressions per outcome variable). If the effect of direct democracy has an actual causal effect, then the estimate of direct democracy on policy, based on the 100-inhabitant threshold, should be an outlier in the distribution of placebo coefficients.

I show the results from these tests in Figure 5. I show the empirical cumulative distribution function of point estimates and t-statistics for the 190 regressions considered for each variable. I also show the implied p-values, which are the share of placebo regressions in which I obtain a point estimate (or t-statistic) that is larger in absolute value than the one for the true threshold. For expenditures, the implied p-value is 0.036 for point estimates, and 0.01 for t-statistics. For revenues, the p-values are 0.126 and 0.068, respectively and, for deficits, 0.405 and 0.242.

These tests specifically address the concern that the bandwidths used in the main specifications are inadequate and lead to artificially significant effects.[35] However, if that were the case, then we should see similarly large effects at other thresholds. These tests show that this is not the case.

[34] Below 30 (above 220) inhabitants the effects become very imprecisely estimated as there are few observations below (above) that population size—as explained in Section 4, at 250 inhabitants there is another threshold, so I focus on population sizes below that threshold to avoid confounding effects.

[35] For example, it could be argued that the bandwidths used are too large. Even though I calculate the optimal bandwidth following the standard procedure by Imbens and Kalyanaraman (2012) and show the results at different fractions of that bandwidth, precision considerations might lead the procedure to select bandwidths that are too large.

7.5 Donut Regressions

This section considers donut regressions in the spirit of Barreca, Guldi, Lindo, and Waddell (2011). These regressions exclude from the estimation observations that are very close to the threshold, where most of the sorting is concentrated. Given the nature of the sorting process, it is likely that most of the self-selection of municipalities into representative democracy is concentrated in a small window around the threshold. It does not seem plausible that the population size of municipalities that are self-selecting is much larger than what is strictly necessary to be above the threshold or that municipalities with a population size far below the threshold attempt to cross it.[36] Thus, finding a similar effect when municipalities very close to the threshold are excluded would reinforce the credibility of the estimates.

For each outcome variable, I consider six regressions. The first one is the benchmark regression. The second excludes observations within a 1% interval around the threshold (that is, municipalities with 99 and 100 inhabitants), the third excludes those within an interval of 2%, and so on, until the sixth regression, which excludes 5%. Appendix Table A6 shows these results. It is reassuring that, in line with the baseline specification, all of the point estimates imply a reduction in expenditures and revenues, ranging from 4 to 7% in expenditures, and from 1 to 4% in revenues, while the estimates for deficits remain close to zero. The results, however, are for the most part not statistically significant, due to a considerable increase in the standard errors. For example, excluding 5 inhabitants yields a point estimate almost identical to the baseline (-0.072 versus -0.079, respectively), while almost doubling the standard error (0.049 versus 0.026). Therefore, it is safe to conclude that the baseline results are not exclusively driven by the observations very close of the threshold, in which the sorting could be more worrisome.

8 Mechanisms

In this section, I dig into the mechanisms that may drive the results documented so far. I start by testing an additional prediction of the model outlined in Section

[36] For example, a municipality that would have “naturally” had 97 inhabitants may well end up having 103 inhabitants if enough individuals are willing to register so that their municipality is above the threshold. However, it is unlikely that this municipality would end up having 115 inhabitants. Likewise, it is unlikely that a municipality with a population far below the threshold would alter its population to reach the threshold: If individuals know that the population is far from getting the desired system, they will not even attempt to reach it.

2. Then, I examine four other potential mechanisms, and discuss their plausibility in light of several additional results and the political and social circumstances in Spain.

Effects of Political Parties on Policy

The model presented in Section 2 predicts that, under representative democracy, the two parties converge to high spending. Therefore, we should not see differences in spending across parties. Here I test whether that prediction is supported by the data. Although this, by no means, pretends to be a definite test of the validity of the model, it would be reassuring to find that policy does not differ across parties. Conversely, finding that it does would raise concerns about the applicability of the model to the Spanish case.

To test this, I replicate the analysis in Arenas and Bagüés (2015) and consider (representative-democracy) elections in which the right-wing PP and the left-wing PSOE were the two most-voted parties. I perform a fuzzy regression discontinuity design, in which the running variable is the difference in the percentage of votes for the PP and the PSOE (*RW Difference*) and the treatment variable is having a PP mayor (*RW Mayor*).[37] Thus, when the running variable is positive (negative), the most-voted party (*RW Winner*) is the PP (PSOE). More formally, I estimate:

$$
\begin{aligned}
Outcome_{myt} &= \delta + \kappa RWMayor_{mt} + g(RWDifference_{mt}) + \epsilon_{myt}, \\
RWMayor_{myt} &= \tilde{\delta} + \tilde{\kappa} RWWinner_{mt} + \tilde{g}(RWDifference_{mt}) + \tilde{\epsilon}_{myt},
\end{aligned} \tag{4}
$$

where the coefficient of interest is κ. I estimate this equation by local linear regression, and I cluster standard errors at the municipality level. Tests for covariate balance and manipulation of the running variable *RW Difference*, shown in Table A10 and Figure A6 in the Appendix, provide assurance that this is a valid empirical approach.

The first stage is strong. As shown in Panel A of Appendix Table A9, being the most-voted party increases the probability of obtaining the mayor by 55 percentage points.[38] Panels B to D in Appendix Table A9 show the estimated coefficients of

[37] One caveat to this approach is that, to obtain precise estimates, I need to use all municipalities under representative democracy, not only those close to the threshold as in the main results.

[38] There are two forces behind this strong first stage: one is that the most voted party is more likely to have one more seat in the council, thus normally increasing its bargaining power to appoint the mayor; the second is that, even in cases in which the two most voted parties tie in seats, the most voted party is—surpisingly— much more likely to appoint the mayor. This phenomenon is studied in depth by Fujiwara and Sanz (2016).

interest $\hat{\kappa}$, and Appendix Figure A5 provides a graphical representation of the first-stage and reduced-form results. There is no evidence of differences in expenditures and revenues between PP and PSOE mayors, as the effects are close to zero and mostly insignificant. For deficits, the results seem to suggest that right-wing mayors may reduce it, but the effects become insignificant with alternative specifications.[39]

These results suggest that the increased spending in representative democracy comes from both parties, as predicted by the model. A similar analysis can be done for direct-democracy municipalities, in which there is also a mayor. In the model, political parties play no role in direct democracy, so spending and revenues should not depend on the affiliation of the mayor, just as we showed it is the case under representative democracy. The results, shown in Appendix Table A11, reveal no significant differences in policy across parties.[40] However, the results are very imprecise due to the lack of observations with sufficiently close elections in direct democracy.

Alternative Mechanisms

(a) Direct democracy may be more prone to elite capture, as was recently proposed by Hinnerich and Pettersson-Lidbom (2014). An elite who prefers low spending may exert more influence under direct democracy than in a representative democracy setting. Hinnerich and Pettersson-Lidbom (2014) provide three arguments that may show that this was a likely situation in the case studied (Sweden at the beginning of the 20$^{\text{th}}$ century). First, the lack of political parties in direct democracy made it more difficult for the citizens to solve their collective action problems (e.g., Acemoglu and Robinson (2008)). Second, the chairman of the town meeting, often a member of the elite, had substantial power to set the agenda. Third, many decisions at meetings were taken by an open vote, and there was the potential for intimidation by the elite.

Although Hinnerich and Pettersson-Lidbom (2014) present compelling evidence in favor of this mechanism in the case that they study, there are several reasons to

[39] These results replicate the findings by Arenas and Bagüés (2015) for Spain and are in line with the findings of Ferreira and Gyourko (2009) for United States cities.

[40] Given that these are first-past-the-post elections, where the most voted candidate automatically becomes mayor, this is pure (not fuzzy) regression discontinuity.

doubt that it is relevant in the setting explored in this paper. First, their argument is based on a context with a conflict between the landed local elite (e.g., farmers) and citizens (e.g., the agricultural workers) at the time when Sweden was still a poor, mostly agrarian society. Thus, the landed elite would like to keep the old labor-repressive economic system, while the citizens would like to have an economic system based on wage labor. This type of situation is not at all present in contemporary Spain. Second, it is not obvious how the elite could capture the meetings in the Spanish setting. The first two reasons that Hinnerich and Pettersson-Lidbom (2014) provided for Sweden do not apply: There are political parties under direct democracy, and the meetings are mostly deliberative, and, therefore, the agenda-setting power of the mayor is limited. Finally, although it is the case that many decisions are taken by an open vote and the possibility of intimidation cannot be completely ruled out, conversations with mayors and other local officials make me think that intimidation is not an issue in Spanish town meetings. Third, it is not obvious why the elite would want a smaller local government. Gobernado Rebaque (2003) shows that fiscal policy is only very slightly progressive at the local level; even though some expenditures are progressive, this is almost totally offset by the regresiveness of local taxes.[41] Alternatively, if we think of the elite that would want to capture the meetings as the special interests of the model, then the elite would prefer a *larger* government, which is not consistent with the results.

(b) The differences in policy may be driven by costs of participation. An alternative model for direct democracy is developed by Osborne, Rosenthal, and Turner (2000). In this model, individuals decide whether to participate, at a cost, in a meeting whose outcome is a compromise among the participants' favorite positions. In deciding whether to participate, each person compares the cost of participation with the impact of his or her presence on the compromise. One prediction of the model is that the outcome will be extremely volatile: in the presence of even a small amount of randomness, the equilibrium outcome may vary dramatically.[42]

[41]A caveat is that Gobernado Rebaque (2003) studies only large municipalities. However, it is likely that the overall system is even more regressive in smaller towns; while the regresiveness of the taxes (mainly the property tax) applies to both, the progressive expenditures singled out by Gobernado Rebaque (2003) (for example, public transportation) have a very limited role in small municipalities.

[42]One source of such randomness is the players' behavior in a mixed-strategy equilibrium. Another source of randomness is exogenous, and arises in a model in which each player is prevented from attending the meeting with some positive probability.

Therefore, it is reasonable to predict that, if direct democracy behaves according to Osborne, Rosenthal, and Turner (2000), policy will be more variable than in a representative-democracy setting.[43]

To shed light on the plausibility of this mechanism, I estimate the effect of direct democracy on the variability of policy. I proceed in two steps.[44] First, I estimate Equation (2) and obtain the residuals. Second, I estimate Equation (2) letting the outcome variable be the absolute value of the residuals obtained in the previous step. The coefficient for *DirDem* from this second stage captures the effect of direct democracy on the variability of the outcome.

The results, shown in Appendix Table A12, suggest that there is no effect of direct democracy on the variability of expenditures or revenues (or, if anything, that direct democracy leads to *less* variability). In the main specification, direct democracy leads to expenditures that deviate from the predicted values around four percentage points less than in representative democracy. However, the results become insignificant and closer to zero with other bandwidths or polynomials. For revenues and deficits, the results are even more clear in pointing to a null effect. The results are similar if, instead of the absolute value of the residuals, I consider the squared residuals, the log of the absolute value of the residuals, or the log of the squared residuals. Therefore, the evidence indicates that, contrary to Osborne, Rosenthal, and Turner (2000), policy is not more volatile in direct democracy.

(c) Direct democracy may affect the policy that individuals prefer. For example, the deliberative nature of the meetings may allow citizens to learn by aggregating information, or it may make the drawbacks of some public expenditures more salient.[45]

Although it is not possible to provide a definitive test for this mechanism, I conduct two tests that can be helpful in assessing its plausibility. First, if the effects of direct democracy on policy were driven by citizens' gradual learning from participating in town meetings, we should observe that the effects grow over time as municipalities spend more time under direct democracy. Here I test whether that

[43] Of course, this depends on the mechanics of the representative-democracy system, but none of the standard models would predict such big variability.

[44] Eggers, Freier, Grembi, and Nannicini (2015) follows a similar procedure.

[45] Thus, we may see different policies even if the median voter theorem holds in both representative and direct democracy and even if there are no *previous* differences in policy preferences between the citizens who participate in direct and representative democracy. Under this scenario, differences in policy arise because direct democracy brings about a shift in the median voter's optimal point.

is the case, by considering this equation:

$$Outcome_{myt} = \alpha_m + \gamma_y + f(Population_{mt} - 100) + \beta DirDem_{mt} + \sum_{j=2}^{4} \Big[N_{j,mt} f_j(Population_{mt} - 100) + \beta_j N_{j,mt} DirDem_{mt} \Big] + u_{myt}, \quad (5)$$

where $N_{j,mt}$ is a dummy variable that indicates how many terms municipality m at year y has been under direct democracy. For a municipality in its first term, $N_{1,mt} = 1$ and $N_{j,mt} = 0$ for $j \neq 1$ and, analogously, for a municipality in its second or third terms. For a municipality in its fourth or longer term, $N_{4,mt} = 1$ and $N_{j,mt} = 0$ for all $j \neq 4$.[46] Therefore, β captures the effect of direct democracy in the first term under direct democracy, and the β_j's indicate how the effect varies in subsequent terms.[47]

The results are shown in Appendix Table A13. For expenditures, the coefficient on $DirDem_{mt}$ under the optimal bandwidth is -6.3% and significant at the 10% level, indicating that direct democracy reduces expenditures in the first term. The results are also significant for other bandwidths (Columns 2 and 3). For $N_{2,mt}DirDem_{mt}$, $N_{3,mt}DirDem_{mt}$ and $N_{4,mt}DirDem_{mt}$, the coefficients are not significant, suggesting that the effect does not vary over time. It is true, however, that, due to the short length of the panel, the estimates are not very precise, so learning cannot be completely ruled out. A similar pattern appears in the estimation for revenues. For deficits, neither the coefficient on $DirDem_{mt}$ nor the interaction terms are significant.

Second, I ask the following questions: How does being in direct democracy affect voter behavior in subsequent elections? Does being in this participatory system lead to more voter turnout? Does it benefit right or left-wing parties? For example, if the frequent deliberation in town meetings makes individuals more engaged in

[46] I consider consecutive terms, so a municipality m that switches out of direct democracy and switches back into direct democracy at term t has $N_{1,mt} = 1$ at t. A caveat is that I cannot observe whether a municipality followed direct democracy before the sample period starts, as only municipalities of fewer than 25 (as opposed to 100) inhabitants were required to follow direct democracy before 1987. Larger municipalities could choose between the two systems. My understanding, based on conversations with local government officials, is that very few municipalities opted for direct democracy. Thus, I assume that, for municipalities under direct democracy in the first term $t = 1$, $N_{1,m1} = 1$.

[47] I cannot rule out the possibility that town meetings affect people's preferences in shorter periods of time. For example, attending merely one meeting might make individuals change their minds about policy, thus driving the results.

the democratic process, we should expect that voter turnout increases in elections after a municipality switches into direct democracy. In addition to being important questions in themselves, they can indirectly shed light on whether being under direct democracy affects individuals' preferences.

To answer these questions, I test whether being under direct democracy on term t has an effect on national elections after the beginning of term t—during period t itself ($VotesRight_{m,t}$, $VotesLeft_{m,t}$, $VotesFarLeft_{m,t}$, $Turnout_{m,t}$), and in the subsequent period $t+1$ ($VotesRight_{m,t+1}$, $VotesLeft_{m,t+1}$, $VotesFarLeft_{m,t+1}$, $Turnout_{m,t+1}$)—by estimating Equation (2) with those variables as outcomes. The results show that there is no effect on any of the outcomes (see Appendix Table A14). All the coefficients are very close to zero and not significant at conventional levels.

(d) The differences may be driven by the amount of transfers received from upper-level governments. In particular, if municipalities in direct democracy have more projects financed through these transfers, that could explain why they decide to set lower taxes and fees and spend less. Although some transfers are determined as a function of population and cannot jump at the threshold, others can, as they are given at will by provincial and regional governments.[48] If direct-democracy municipalities are better at "lobbying" for these transfers, or if upper-level governments prefer to fund direct-democracy municipalities, that could lead to more transfers under direct democracy. It is not clear why we should expect this to happen, but I can anyway directly test it in the data, by estimating whether there is an effect of direct democracy on transfers received. In Appendix Table A15, I show the results of estimating Equation (2) with the log of *Transfers* as the dependent variable. All the estimates are insignificant and very close to zero, indicating that there is no effect of the treatment on transfers. Therefore, it is unlikely that this mechanism can be driving the results.

[48] For example, Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2015) show that regional governments give more transfers to municipalities that are politically aligned.

9 Conclusion

This paper has provided empirical evidence on the effects of direct democracy on policy. Using a regression discontinuity design in the context of Spanish local governments, I have shown that direct democracy results in a smaller government. Compared with a standard representative-democracy system, direct democracy reduces public spending by almost 8%. Revenues are reduced by a similar amount, thus leaving budget deficits unchanged.

This paper expands our knowledge of the impact of participatory institutions, by providing the first quasi-experimental evidence on how town meetings affect spending and taxation decisions in a contemporary setting. Of course, research from other settings is needed to get a more complete understanding of this issue. Finally, this paper opens up an interesting avenue for future work, which is to use this previously unexplored setting to study how other outcomes—for example, corruption—are affected by the use of participatory meetings.

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Tables

Table 1: Summary Statistics

	mean	sd	p1	p50	p99	count
Expenditures (€ per cap.)	671.0	1321.9	74.5	460.1	3524.6	42162
Revenues (€ per cap.)	647.3	1290.3	87.9	449.3	3375.5	42162
Deficit (€ per cap.)	23.8	270.8	-272.3	0.0	875.4	42162
RW Mayor	0.5	0.5	0.0	1.0	1.0	42073
LW Mayor	0.2	0.4	0.0	0.0	1.0	42073
RW Winner	0.6	0.5	0.0	1.0	1.0	19807
RW Difference (%)	12.4	41.6	-90.0	13.7	96.2	19807
Votes Right (%)	51.6	21.1	2.1	54.1	92.2	42056
Votes Left (%)	30.6	15.5	0.0	29.7	69.4	42056
Votes Far Left (%)	3.2	3.7	0.0	2.2	17.1	35095
Votes Difference (%)	31.0	21.8	0.0	27.5	87.5	42056
Votes Winner (%)	59.3	13.2	32.7	57.9	92.3	42056
Turnout (%)	78.3	8.8	52.9	79.3	95.5	42056
Mean Age (years)	53.0	6.1	39.3	53.0	67.7	27582
Young (%)	9.2	5.5	0.0	8.9	23.4	27582
Middle-Aged (%)	53.4	9.2	29.2	53.6	74.6	27582
Old (%)	37.3	11.6	12.9	37.0	67.9	27582
Immigrants (%)	2.6	4.8	0.0	0.5	22.4	27569
EU Immigrants (%)	45.4	42.1	0.0	41.7	100.0	14338
Population (inhabitants)	130.9	62.4	19.0	128.0	247.0	42162

The unit of observation is a municipality-year. The sample size is smaller for election variables because some data are missing from the official files. The sample is even smaller for *Votes Far Left* because the United Left party did not run in every election. For *RW Winner* and *RW Difference*, the sample is restricted to elections in which the PP and the PSOE were the two most-voted parties. For the demographic variables, data are available only from 1996. Also note that, in municipalities with no immigrants, *EU Immigrants* is missing by construction. See Appendix C for a precise definition of the variables.

Table 2: Effect of Direct Democracy on Public Finances

Panel A: Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.	(7) Log Exp.
DirDem	-0.0799***	-0.0605**	-0.0897**	-0.0729**	-0.0658*	-0.111***	-0.0491*
	(0.0267)	(0.0249)	(0.0408)	(0.0309)	(0.0354)	(0.0420)	(0.0261)
Observations	11932	17646	5964	42026	42026	42026	11932
Municipalities	1102	1405	756	2637	2637	2637	1102
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Opt.
Mun. trends							Yes

Panel B: Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.	(7) Log Rev.
DirDem	-0.0521**	-0.0637***	-0.108***	-0.0725**	-0.0694**	-0.118***	-0.0542**
	(0.0261)	(0.0231)	(0.0412)	(0.0284)	(0.0326)	(0.0392)	(0.0235)
Observations	10625	16074	5542	42111	42111	42111	10625
Municipalities	1047	1340	729	2638	2638	2638	1047
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Opt.
Mun. trends							Yes

Panel C: Deficit (euros)

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit	(7) Deficit
DirDem	-7.877	-5.951	0.242	6.323	6.647	3.186	10.32
	(8.050)	(8.456)	(9.387)	(15.36)	(18.96)	(21.10)	(17.60)
Observations	34570	41761	20647	42111	42111	42111	11958
Municipalities	2239	2620	1556	2638	2638	2638	1102
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Opt.
Mun. trends							Yes

Results from estimating Equation (2) (columns 1 to 6) and Equation (3) (column 7). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects, and the last column also includes municipality-specific trends. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 27, [b] Optimal BW = 25. [c] Optimal BW = 98. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Placebo Tests: Covariate Smoothness around the Threshold

Variable	(1) Mean	(2) Optimal BW Value	(3) Opt. BW Results	(4) .5 x Opt. BW Results
Votes Right (%)	51.6	29	-0.0866	0.0524
		[N = 12642]	(0.669)	(0.966)
Votes Left (%)	30.6	31	-0.258	-0.382
		[N = 13506]	(0.550)	(0.771)
Votes Far Left (%)	3.2	28	0.102	0.0510
		[N = 10023]	(0.271)	(0.413)
Votes Difference (%)	31.0	33	0.603	1.284
		[N = 13904]	(1.014)	(1.382)
Votes Winner (%)	59.3	34	0.330	0.374
		[N = 14308]	(0.603)	(0.825)
Turnout (%)	78.3	30	-0.328	-0.144
		[N = 13099]	(0.440)	(0.630)
Mean Age (years)	53.0	34	-0.143	-0.0412
		[N = 9847]	(0.191)	(0.246)
Young (%)	9.2	41	-0.0993	-0.367
		[N = 11428]	(0.243)	(0.290)
Middle-Aged (%)	53.4	37	0.226	0.523
		[N = 10396]	(0.426)	(0.494)
Old (%)	37.3	40	-0.177	-0.225
		[N = 11191]	(0.433)	(0.501)
Immigrants (%)	2.6	39	-0.240	-0.523
		[N = 11185]	(0.324)	(0.397)
EU Immigrants (%)	45.4	29	-0.189	2.452
		[N = 4056]	(3.836)	(4.978)

Column 1 shows the mean of the variables. Column 2 shows the optimal bandwidth value and number of observations for a placebo test that estimates the effect of direct democracy on the corresponding variable, according to Equation (2). Columns 3 and 4 show the results for the placebo tests: each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. The optimal bandwidth (BW) is based on Imbens and Kalyanaraman (2012). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Placebo Tests: Lagged Outcomes

Panel A: Log Expenditures (t-1)

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.	(7) Log Exp.
DirDem	-0.0192	0.00203	-0.0124	-0.0148	-0.000786	-0.0176	-0.00907
	(0.0291)	(0.0287)	(0.0420)	(0.0354)	(0.0417)	(0.0493)	(0.0305)
Observations	9433	13679	4738	33486	33486	33486	9433
Municipalities	1003	1294	678	2506	2506	2506	1003
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal[a]
Mun. trends							Yes

Panel B: Log Revenues (t-1)

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.	(7) Log Rev.
DirDem	-0.00816	0.00801	-0.00210	-0.00493	0.0175	-0.0126	-0.00908
	(0.0261)	(0.0257)	(0.0376)	(0.0322)	(0.0377)	(0.0446)	(0.0270)
Observations	9455	14027	4753	33553	33553	33553	9455
Municipalities	1003	1311	679	2507	2507	2507	1003
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal[b]
Mun. trends							Yes

Panel C: Deficit (t-1) (euros)

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit	(7) Deficit
DirDem	-5.832	-2.534	-3.448	-7.204	-17.86	-11.41	-0.517
	(9.638)	(10.88)	(13.62)	(11.91)	(13.97)	(15.42)	(9.515)
Observations	10738	16034	5360	33553	33553	33553	9455
Municipalities	1100	1436	727	2507	2507	2507	1003
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal[c]
Mun. trends							Yes

Results from estimating Equation (2) (columns 1 to 6) and Equation (3) (column 7). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects, and the last column also includes municipality-specific trends. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 27, [b] Optimal BW = 28. [c] Optimal BW = 31. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

Figure 1: A town meeting in the municipality of Madarcos, in the Madrid region. Source: documentary "Concejo Abierto", by Carmen Comadrán: http://www.tierravoz.com/concejoabierto/el-corto/.

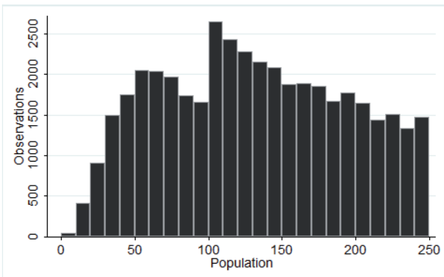
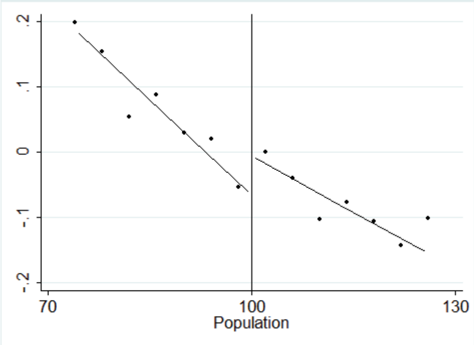
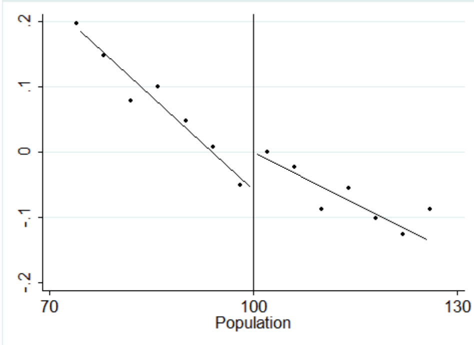


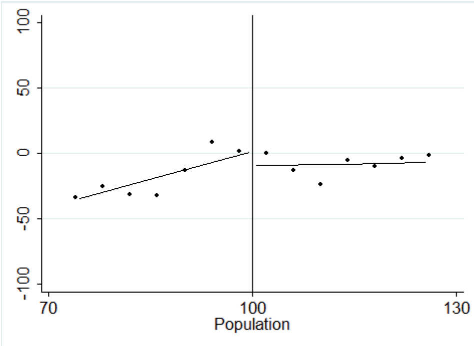
Figure 2: Histogram of population sizes. An observation is a municipality-year. Bins are 10-inhabitant wide.



(a) Log Expenditures

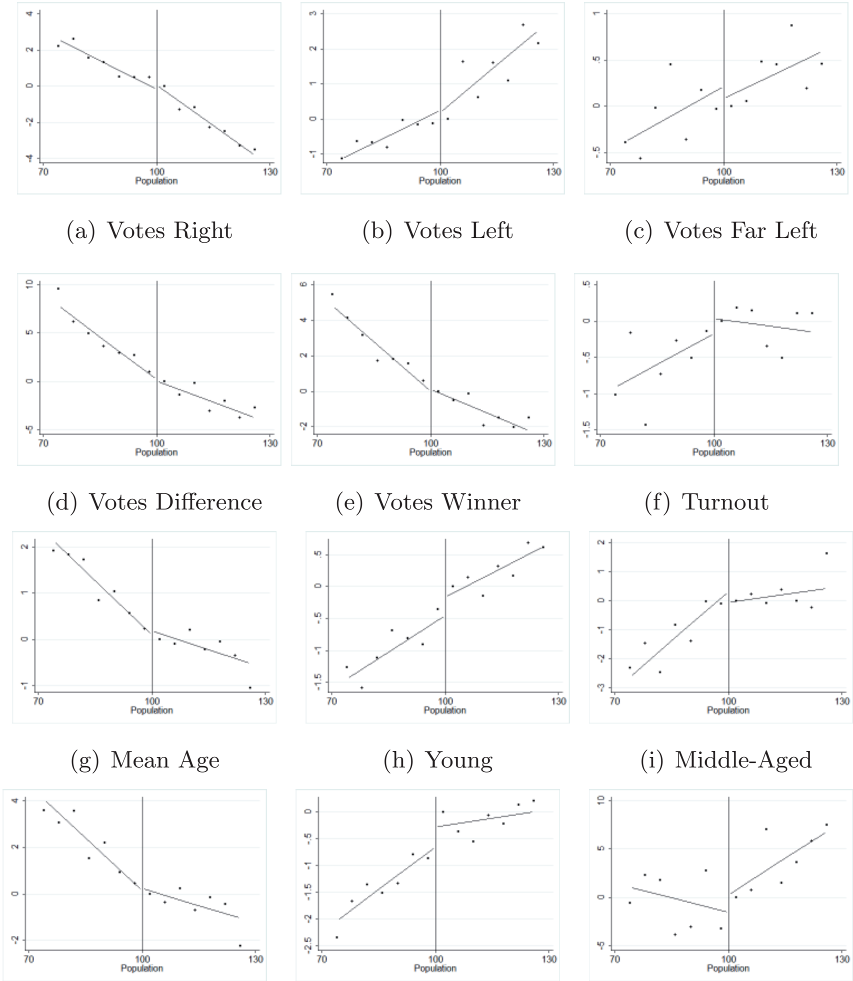


(b) Log Revenues



(c) Deficit

Figure 3: Effect of direct democracy on expenditures, revenues, and deficit. I estimate $Outcome_{myt} = \alpha_m + \gamma_y + \sum_{j=100-OBW}^{100+OBW} \delta_j Population_{j,mt} + u_{myt}$, where $Population_{j,mt}$ is a dummy that indicates whether municipality m has population size j at term t. In the y-axis, I plot the estimated coefficients $\hat{\delta}_j$, averaged to 4-inhabitant-wide bins. I normalize the coefficients so that the average bin immediately to the right of the threshold takes the value of zero. The lines are linear fits on $\hat{\delta}_j$, fitted separately for observations above and below the threshold. I use the observations within the optimal bandwidth for $LogExpenditures_{myt}$, so that all graphs show the same range in the x-axis.



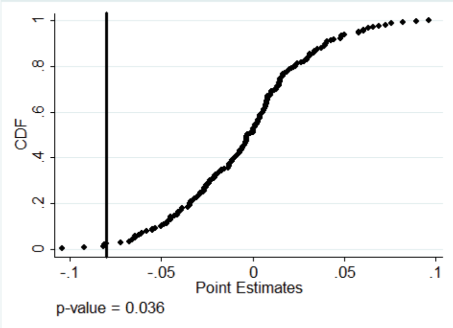
(a) Votes Right (b) Votes Left (c) Votes Far Left

(d) Votes Difference (e) Votes Winner (f) Turnout

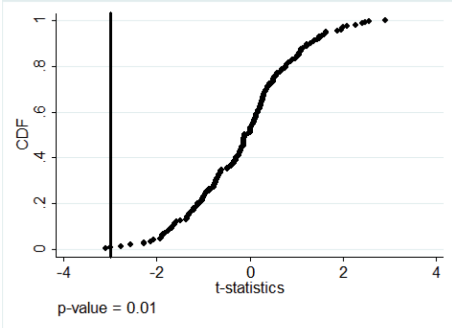
(g) Mean Age (h) Young (i) Middle-Aged

(j) Old (k) Immigrants (l) EU Immigrants

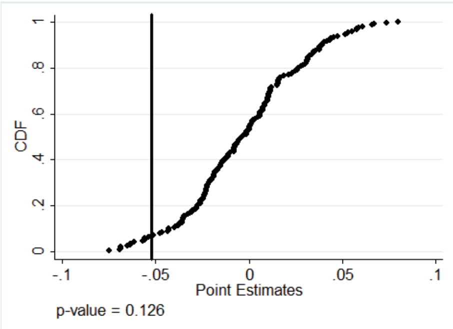
Figure 4: Placebo Tests: Effect of direct democracy on political and demographic variables. I estimate $Outcome_{myt} = \alpha_m + \gamma_y + \sum_{j=100-OBW}^{100+OBW} \delta_j Population_{j,mt} + u_{myt}$, where $Population_{j,mt}$ is a dummy that indicates whether municipality m has population size j at term t. In the y-axis, I plot the estimated coefficients $\hat{\delta}_j$, averaged to 4-inhabitant-wide bins. I normalize the coefficients so that the average bin immediately to the right of the threshold takes the value of zero. The lines are linear fits on $\hat{\delta}_j$, fitted separately for observations above and below the threshold. I use the observations within the optimal bandwidth for $LogExpenditures_{myt}$, so that all graphs show the same range in the x-axis.



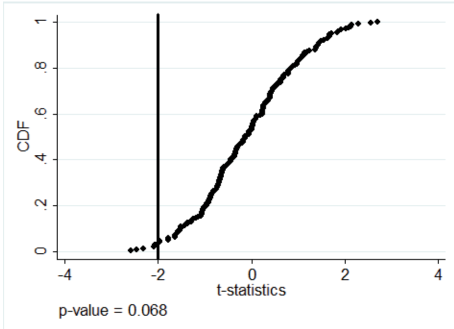
(a) Log Expenditures



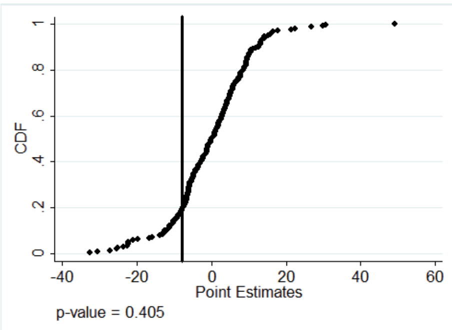
(b) Log Expenditures



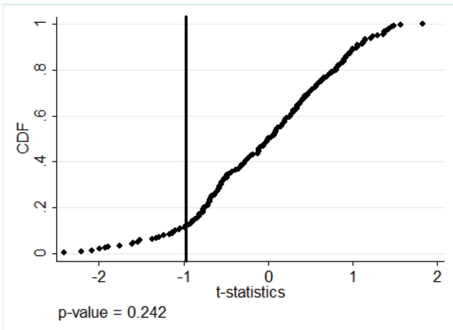
(c) Log Revenues



(d) Log Revenues



(e) Deficit



(f) Deficit

Figure 5: Placebo tests at other population thresholds. I run Equation (2) at all (fake) population thresholds between 30 and 220 inhabitants, by replacing *DD* with a dummy that indicates whether population is larger than the given population threshold. The graphs show the empirical cumulative distribution functions for the resulting point estimates and t-statistics. The vertical lines show the point estimates (or t-statistics) for the effects of direct democracy, obtained at the (true) 100-inhabitant threshold. The p-value below each graph shows the share of point estimates (or t-statistics) that are larger in absolute value than the one for the 100-inhabitant threshold.

Supplementary Appendices

Appendix A: Model

There is a continuum of individuals who have to decide on two dimensions of policy. The first one is a primary policy dimension, which I will call ideological stance, or, simply, ideology, i. Ideology can be left wing, denoted by $\underline{i}$, or right wing, $\overline{i}$, so $i \in \{\underline{i}, \overline{i}\}$. We can think of ideology as a salient policy issue, such as the progressiveness of policy.[49] The second issue is targeted or special-interest spending, denoted by s. Spending also can take two values, $s \in \{\underline{s}, \overline{s}\}$, where $\underline{s}$ denotes low (maybe zero) spending and $\overline{s}$ denotes high spending.

Individuals' preferences differ over the two issues. With respect to ideology, individuals are divided into leftists and rightists, $v \in \{l, r\}$. A fraction γ_l of individuals are leftists and prefer $\underline{i}$. The rest, $\gamma_r = 1 - \gamma_l$, are rightists and prefer $\overline{i}$. Let $i^*(v)$ denote the optimal ideological policy from the perspective of an individual of type v (hence, $i^*(l) = \underline{i}$ and $i^*(r) = \overline{i}$). Without loss of generality, I assume that leftists are the majority, thus $\gamma_l > .5$. With respect to special-interest spending, individuals are divided (independently of their preferences in regard to ideology) into citizens and special interests, $w \in \{c, x\}$. A fraction γ_c of individuals are citizens, c, and prefer low spending, $\underline{s}$. The remaining individuals, $\gamma_x = 1 - \gamma_c$, are special interests and prefer high spending, $\overline{s}$. The special interests are those who benefit from special-interest spending, so they do not necessarily constitute an economic elite—they could be, for example, public employees who are favored by the government. Special interests are a minority, thus $\gamma_x < \gamma_r$. Let $s^*(w)$ denote the optimal ideological policy from the perspective of an individual of type w (hence, $s^*(c) = \underline{s}$ and $s^*(x) = \overline{s}$).

[49] For example, municipalities can use tax deductions to benefit poor individuals. Even if it may seem surprising that a main "ideological" issue determines the vote at the local level, the correlation between votes to the main right (left)-wing party in local and national elections is .63 (.57), suggesting that the determinants of voting are closely related at the two levels.

Individuals' utilities are given by:

$$\begin{aligned}
u_{l,c}(i,s) &= b_{lc}\mathbb{1}[i=\underline{i}] + \theta_{lc}\mathbb{1}[s=\underline{s}], \\
u_{r,c}(i,s) &= b_{rc}\mathbb{1}[i=\bar{i}] + \theta_{rc}\mathbb{1}[s=\underline{s}], \\
u_{l,x}(i,s) &= b_{lx}\mathbb{1}[i=\underline{i}] + \theta_{lx}\mathbb{1}[s=\bar{s}], \\
u_{r,x}(i,s) &= b_{rx}\mathbb{1}[i=\bar{i}] + \theta_{rx}\mathbb{1}[s=\bar{s}], \qquad (6)
\end{aligned}$$

where $u_{v,w}$ denotes the utility of type (v,w). Citizens are more concerned about ideology, so that $b_{lc} > \theta_{lc}$ and $b_{rc} > \theta_{rc}$. The special interests, in contrast, care more about special-interest spending, as they benefit directly from it: $b_{lx} < \theta_{lx}$ and $b_{rx} < \theta_{rx}$.

Policy under Representative Democracy

Under representative democracy, policy is delegated to an elected representative, who is an individual and will always implement his or her preferred policy $i^*(v), s^*(w)$ —there is no possibility of commitment. Candidates in the election are put forward by two political parties P, denoted A and B, $P \in \{A, B\}$. Each party is comprised of member individuals bound together by their views on ideology. All members in Party A are leftists, and all members in B are rightists. Within each party, however, there can be any combination of citizens and special interests, so that, even if in the entire population citizens are a majority, that may or may not be the case within political parties. Each party selects a candidate that a majority of its members prefer. Because every individual within a party shares the same preferences toward ideology, the preferences of the majority in the spending dimension will determine which candidate the party proposes. Let s_P^* denote the preferences of the majority of Party P on spending. Parties are not restricted to proposing a member of their own party; thus, in principle, they could propose somebody with an opposing ideology, but that will not happen in equilibrium.

To introduce uncertainty into the election, Besley and Coate (2008) assume that there are some noise voters, a fraction of whom will vote for A's candidate according to the realization of some random variable. To keep the notation simple, I assume instead that the probability that Party A's candidate wins the election is given by the share of individuals who prefer Party A's candidate over Party B's candidate.[50]

[50] The insights and conclusions are the same with both approaches.

Naturally, an individual prefers Party A's candidate if the policy that Party A's candidate will implement gives him or her more utility than will the policy that Party B's candidate will implement, according to (6). More formally, an individual of type $\{v, w\}$ faced with candidates (v_A, w_A) and (v_B, w_B) will favor Party A's candidate if $u_{v,w}(i^*(v_A), s^*(w_A)) > u_{v,w}(i^*(v_B), s^*(w_B))$, and will favor Party B's candidate if $u_{v,w}(i^*(v_A), s^*(w_A)) < u_{v,w}(i^*(v_B), s^*(w_B))$. If both candidates give him or her the same utility, then he or she will favor each candidate with probability 1/2.

Party members know the election probabilities associated with different candidate pairs and take them into account when selecting candidates. An equilibrium is a pair of candidates, one for each party, such that each party's majority members do not want to deviate from their choice, given the other party's choice. More formally, a pair of candidates (v_A, w_A) and (v_B, w_B) is an equilibrium if type (l, s_A^*) individuals prefer a type (v_A, w_A) candidate to any other type of candidate, given that Party B is running a type (v_B, w_B) candidate and, conversely, type (r, s_B^*) individuals prefer a type (v_B, w_B) candidate to any other type of candidate, given that Party A is running a type (v_A, w_A) candidate. Any equilibrium results in a probability distribution over outcomes. The policy outcome will be that associated with Party P's candidate with a probability equal to the chance that Party P's candidate wins.

Equilibrium under Representative Democracy

Case 1: For each $\mathbf{P}, \mathbf{s}_\mathbf{P}^* = \overline{\mathbf{s}}$.

In this case, special interests are a majority in both political parties. This represents a situation in which political parties are captured by special interests. In this case, an equilibrium exists in which both parties run with a special interest of their preferred ideological position, and that equilibrium is unique. This is summarized in Proposition 1.

Proposition 1. *If, for each $P, s_P^* = \overline{s}$, then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is the unique equilibrium.*

Proof. I need to show that if, for each P, $s_P^* = \overline{s}$, then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is the unique equilibrium.

I first show that $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is an equilibrium.

In the proposed equilibrium, Party A wins the election with probability γ_l, and its median voter, who is a leftist special interest, gets utility $b_{lx} + \theta_{lx}$ if that happens.

If Party B wins, the median voter in Party A gets utility θ_{lx}. Therefore, the expected utility of the median voter in Party A, which I will denote by $u(A)$, is $u^*(A) = \gamma_l(b_{lx} + \theta_{lx}) + \gamma_r\theta_{lx} = \theta_{lx} + \gamma_l b_{lx}$ in the proposed equilibrium.

Party A has three possible deviations: (a) If it deviates to proposing $(v_A = l, w_A = c)$, Party A reduces both the probability of winning (to $\gamma_c\gamma_l$) and the utility in case of a win (to b_{lx}), so $u(A) = \gamma_c\gamma_l b_{lx} + (1-\gamma_c\gamma_l)\theta_{lx} < u^*(A)$. (b) If it deviates to $(v_A = r, w_A = c)$, Party A increases the probability of winning from γ_l to γ_c but at the cost of getting zero utility if that happens (it is proposing its least preferred policy): $u(A) = \gamma_x\theta_{lx} < u^*(A)$. (c) If it deviates to $(v_A = r, w_A = x)$, then both parties propose the same policy and win with 1/2 probability, and $u(A) = \theta_{lx} < u^*(A)$.

Now consider Party B. In the proposed equilibrium, its median voter, who is a rightist special interest, has expected utility $u^*(B) = \gamma_r(b_{rx} + \theta_{rx}) + \gamma_l\theta_{rx} = \theta_{rx} + \gamma_r b_{rx}$. Consider the three possible deviations: (a) If it deviates to proposing $(v_B = r, w_B = c)$, $u(B) = \gamma_c\gamma_r b_{rx} + (1 - \gamma_c\gamma_r)\theta_{rx} < u^*(B)$. (b) If it deviates to $(v_B = l, w_B = c)$, $u(B) = \gamma_x\theta_{rx} < u^*(B)$. (c) If it deviates to $(v_B = l, w_B = x)$, $u(B) = \theta_{rx} < u^*(B)$. This completes the proof that $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is an equilibrium.

I now show that the equilibrium $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is unique. To do so, I will show that $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$ is not an equilibrium (it is trivial to show that other possible proposals cannot be an equilibrium). Under $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$, $u^*(A) = \gamma_l b_{lx}$. If A deviates to $(v_A = l, w_A = x)$, $u(A) = (\gamma_c\gamma_l + \gamma_x)(b_{lx} + \theta_{lx}) > u^*(A)$. Thus, A prefers to deviate, and $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$ cannot be an equilibrium.

□

The intuition is as follows. Party A will not want to switch to a leftist citizen, as it would lower the probability of winning (from γ_l to $\gamma_l\gamma_c$, as it loses all of the votes of the special interests), and it would lower the utility in case of a win for the median individual in the party, which is a special interest. The same happens if Party A switches to a rightist special interest—in this case, the probability of winning goes down to 1/2, as both parties propose the exact same types of candidates. Finally, a "radical" switch to a rightist citizen increases the probability of winning (to γ_l) but at the cost of sacrificing the preferred policy in both dimensions, so the utility in the case of winning is zero. The reasoning for Party B is analogous. Finally, note that both parties running with a citizen of their preferred ideological type cannot be an equilibrium, as both parties would want to deviate to a special interest of their preferred ideological type.

Case 2: For each $\mathbf{P}, \mathbf{s}^*_\mathbf{P} = \underline{\mathbf{s}}$.
In this case, citizens are a majority in political parties, as they are in the entire population. In this case as well, an equilibrium exists in which both parties run with a special-interest candidate of their preferred ideological position, under certain conditions that are indicated in the following proposition.

Proposition 2. *(i) If, for each P, $s^*_P = \underline{s}$, and, for each v,*

$$\frac{b_{vc}}{\theta_{vc}} > \frac{\gamma_c}{\gamma_x},$$

then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is an equilibrium.

(ii) If, additionally,

$$\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_x + \gamma_c\gamma_l}{\gamma_x + \gamma_c\gamma_l - \gamma_l}$$

or

$$\frac{b_{rc}}{\theta_{rc}} > \frac{\gamma_x + \gamma_c\gamma_r}{\gamma_x + \gamma_c\gamma_r - \gamma_r},$$

then the equilibrium is unique.

Proof. (i) I need to show that, if for each P, $s^*_P = \underline{s}$, and for each v,

$$\frac{b_{vc}}{\theta_{vc}} > \frac{\gamma_c}{\gamma_x},$$

then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is an equilibrium.

Under the proposed equilibrium, the median voter in Party A, who is a leftist citizen, has an expected utility of $u^*(A) = \gamma_l b_{lc}$. Consider his or her three possible deviations: (a) If it deviates to $(v_A = l, w_A = c)$, Party A increases the utility in case of a win (to $b_{lx} + \gamma_{lc}$) but at the cost of reducing the probability of winning (to $\gamma_c\gamma_l$). Because $\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_c}{\gamma_x}$, by assumption, $u(A) = \gamma_c\gamma_l(b_{lc} + \gamma_{lc}) < u^*(A)$. (b) If it deviates to $(v_A = r, w_A = c)$, $u(A) = \gamma_c\theta_{lc}$. Because $\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_c}{\gamma_x}$, by assumption, $u(A) < u^*(A)$. (c) If it deviates to $(v_A = r, w_A = x)$, $u(A) = 0 < u^*(A)$. For Party B, the argument is symmetric.

(ii) I need to show that if, additionally,

$$\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_x + \gamma_c\gamma_l}{\gamma_x + \gamma_c\gamma_l - \gamma_l}$$

or

$$\frac{b_{rc}}{\theta_{rc}} > \frac{\gamma_x + \gamma_c\gamma_r}{\gamma_x + \gamma_c\gamma_r - \gamma_r},$$

then the equilibrium is unique.

I will first show that $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$ is not an equilibrium. Under $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$, $u^*(A) = \gamma_l b_{lc} + \theta_{lc}$. Consider the best deviation possible deviation for A, which is $(v_A = l, w_A = x)$ (it is trivial to show that the two other possible deviations are worse). Then, $u(A) = (\gamma_c\gamma_l + \gamma_x)b_{lc} + (1 - \gamma_c\gamma_l - \gamma_x)\theta_{lc}$. If

$$\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_x + \gamma_c\gamma_l}{\gamma_x + \gamma_c\gamma_l - \gamma_l},$$

then $u(A) < u^*(A)$, so Party A will prefer to deviate. For Party B, the best possible deviation is $(v_B = r, w_B = x)$. By symmetry, if

$$\frac{b_{rc}}{\theta_{rc}} > \frac{\gamma_x + \gamma_c\gamma_r}{\gamma_x + \gamma_c\gamma_r - \gamma_r},$$

party B will prefer to deviate. Because it is sufficient that one of the two parties deviates, if

$$\frac{b_{lc}}{\theta_{lc}} > \frac{\gamma_x + \gamma_c\gamma_l}{\gamma_x + \gamma_c\gamma_l - \gamma_l}$$

or

$$\frac{b_{rc}}{\theta_{rc}} > \frac{\gamma_x + \gamma_c\gamma_r}{\gamma_x + \gamma_c\gamma_r - \gamma_r},$$

then $(v_A = l, w_A = c)$ and $(v_B = r, w_B = c)$ is not an equilibrium. □

Assumption (i) rules out that parties want to deviate by running with a citizen instead of a special interest. By proposing a citizen, the median voter in the parties sacrifices some probability of winning (for Party A, it goes down to $\gamma_c\gamma_l$ from γ_l) but obtains some additional utility if the party wins the election, $(\gamma_c l)$. The assumption guarantees that the first effect dominates the second by requiring that ideology, relative to spending, is sufficiently important to citizens, given the distribution of types in the population (or, stated differently, that there are enough special interests, given the preferences of citizens). Assumption (ii) rules out that both parties' choosing a citizen is an equilibrium.

Case 3: $\mathbf{s}^*_{\mathbf{P}} = \underline{\mathbf{s}}$ for some P, $\mathbf{s}^*_{\mathbf{P'}} = \overline{\mathbf{s}}$ for $\mathbf{P} \neq \mathbf{P'}$.
This case is a combination of the other two: Citizens are the majority in one party but not in the other.

Proposition 3. *If $s^*_l = \underline{s}$, $s^*_r = \overline{s}$, and $b_{lc}/\theta_{lc} > \gamma_c/\gamma_x$, then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is the unique equilibrium. If $s^*_l = \overline{s}$, $s^*_r = \underline{s}$, and $b_{rc}/\theta_{rc} > \gamma_c/\gamma_x$, then $(v_A = l, w_A = x)$ and $(v_B = r, w_B = x)$ is the unique equilibrium.*

Proof. The proof of Proposition 3 follows directly from the proof of Proposition 1 and the proof of Proposition 2. □

Following the logic of the first two cases, now the requirement that special interests are sufficiently large is required only for the party in which citizens are a majority.

Policy and Equilibrium under Direct Democracy

Under direct democracy, ideology and spending are voted on separately. The equilibrium outcomes are therefore $i = \underline{i}$ and $s = \underline{s}$. Intuitively, as issues are unbundled, the position preferred by the median voter (a leftist in ideology and a citizen in spending) prevails in both dimensions.

In sum, the model predicts that direct democracy will reduce spending if special interests have captured political parties, or if they are sufficiently large. In addition, the model predicts that spending in representative democracy should not depend on which party is in office: Both parties, A and B, converge to the overspending position. Finally, it is important to note that the model does not yield a clear-cut prediction in terms of welfare. Even if direct democracy leads to policy more in line with the median voter's preferences, a switch from representative to direct democracy creates winners and losers. Citizens, who are the majority, benefit from direct democracy as a result of reduced special-interest spending, while special interests lose. But special interests may feel more intensely about the issue. Hence, a measure of welfare—for example, utilitarian—could rise or fall.

Appendix B: Historical Overview

The use of *concejo abierto* dates from centuries ago (for a detailed historical overview, see Salanova Alcalde (2009)). In a primitive form, they appeared in the Christian territories in he Early Middle Ages, when neighbors organized themselves in assemblies to decide on the government of villages. Traditionally, municipalities themselves decided whether to work under direct or representative democracy. A first attempt to introduced a population threshold took place in 1924, when a national law *(Estatuto Municipal)* imposed the use of direct democracy to all municipalities with fewer than 500 inhabitants. However, this provision was never enforced. During the Second Spanish Republic, another attempt was made to extend the use of direct democracy to all municipalities with fewer than 500 inhabitants (1935 Law), but this attempt never materialized either, as the regime lasted only one more year before the onset of the Spanish Civil War in 1936. The situation did not change during Franco's regime. A 1955 law *(Ley de Régimen Local)* required the following of the direct-democracy system only for those municipalities in which that was the traditional form of government. It was not until after the restoration of democracy after Franco's death in 1975 that the rules changed substantially. In 1978, national law *(Ley de Elecciones Locales)* required all municipalities whose population in the election-year was smaller than 25 inhabitants to follow the direct democracy-system. In 1985, a reform extended the requirement to all municipalities with fewer than 100 inhabitants.[51] This regulation was in force until 2011, and is the focus of this paper. In 2011, the law was changed to eliminate the requirement to follow direct democracy for municipalities with fewer than 100 inhabitants. The rationale for this change was that most local politicians preferred to avoid direct democracy (which is consistent with the sorting around the threshold) for the reasons discussed in Section 5. Municipalities can still adopt the direct-democracy system, by following the procedure described in the national law (similar to the one described in footnote 51), but no municipality is now required to follow direct democracy. No official data regarding the use of direct democracy after 2011 exist yet, but partial data indicate that it is low (8.3%), consistent with the purpose of the change in the law.

[51] Municipalities with 100 or more inhabitants could follow a demanding procedure to adopt the direct-democracy system. Specifically, a majority of the citizens of the municipality had to sign a petition, and two-thirds of the members of the council and the regional government had to approve. To the best of my knowledge, no municipality ever used this procedure. This implies that the regression discontinuity component of the estimation is sharp, as the probability of treatment jumps from 0 to 1 at the threshold.

Appendix C: Definition of Variables

In this Appendix, I provide details on the definition of the variables used in the paper.

Budget Variables

Expenditures is defined as the sum of all (non-financial) chapters of the expenditures budget, net of transfers:

$$Expenditures_{myt} = \sum_{k=1}^{7} E_{k,myt} - R_{4,myt} - R_{7,myt},$$

where m denotes municipality, y denotes the year, t denotes the term, E_k is expenditures per capita on Chapter k of the expenditures budget, and R_k denotes revenues per capita from Chapter k of the revenues budget.

Analogously, *Revenues*, *Transfers*, and *Deficit* are defined as follows:

$$\begin{aligned} Revenues_{myt} &= \sum_{k=1}^{7} R_{k,myt} - R_{4,myt} - R_{7,myt}, \\ Transfers_{myt} &= R_{4,myt} + R_{7,myt} - E_{4,myt} - E_{7,myt}, \\ Deficit_{myt} &= Expenditures_{myt} - Revenues_{myt}. \end{aligned}$$

Elections Variables

I use the following variables from national elections:

$$\begin{aligned} Turnout_{mt} &= 100\frac{VotesCast_{mt}}{ElectoralCensus_{mt}}, \\ VotesRight_{mt} &= 100\frac{PPVotes_{mt}}{ValidVotes_{mt}}, \\ VotesLeft_{mt} &= 100\frac{PSOEVotes_{mt}}{ValidVotes_{mt}}, \\ VotesFarLeft_{mt} &= 100\frac{IUVotes_{mt}}{ValidVotes_{mt}}, \end{aligned}$$

where

$$\begin{aligned} VotesCast_{mt} &= VotesForCandidates_{mt} + BlankVotes_{mt} + SpoiltVotes_{mt}, \\ ValidVotes_{mt} &= VotesForCandidates_{mt} + BlankVotes_{mt}. \end{aligned}$$

That is, *Turnout* measures the proportion of citizens who cast a vote over the set of potential voters (the electoral census). There is no voter registration in Spain: Potential voters are all citizens of Spain and other EU countries as well as countries under Reciprocity Treaties; older than 18, and not disenfranchised by court order.[52] Valid votes include votes for candidates and blank votes but not spoiled votes. I use this denominator because it is relevant for the allocation of seats, as it is used to determine whether parties reach the election threshold to get seats (3% in national elections). Accordingly, it is the one that is normally reported by the media.

Observations for year y are from the Congress elections that take place during the term t to which year y belongs. Because Congress elections have always alternated perfectly with local elections, there is only one Congress election per term. For example, for years 1988-1991, I consider the Congress elections of 1989. As explained in the text, I lag the variables to use them as placebos.

Demographic Variables

The age distribution data are provided by the National Institute for Statistics in intervals of five years, up to "85 or more" until 2010 and up to "100 or more" from 2011. To calculate the average age *Mean Age*, I use the mid-points of those intervals. For ages 85–100 (for which there is no five-year information until 2011), I calculate the average age in 2011 (89.01) and use it for the rest of the years. For ages "100 or more," I assume the average is 102.5 years.

I define the share of young people *Young* as the share of individuals in the first four intervals (19 years old or younger), *Middle-Aged* as the share of individuals in the next nine intervals (ages 20–64), and *Old* as the share of individuals in the final five intervals (ages 65 or older).

[52] Disenfranchisement is mostly for disability reasons. In 2011, the number of disenfranchised individuals was 79,398 (including individuals younger than 18) or approximately 0.18% of the population.

Appendix Tables

Table A1: Number of Municipalities and Switches by Government System

Panel A: Number of Municipalities by Government System

Term	DirDem	RepDem	Total
1987–1991	603	1385	1988
1991–1995	614	1319	1933
1995–1999	695	1387	2082
1999–2003	781	1475	2256
2003–2007	797	1486	2283
2007–2011	827	1510	2337

Panel B: Number of Switches in Government System

Term	RepDem $\rightarrow$ DirDem	DirDem $\rightarrow$ RepDem	Total
1st to 2nd	50	13	63
2nd to 3rd	93	21	114
3rd to 4th	94	19	113
4th to 5th	69	38	107
5th to 6th	73	43	116
Total	379	134	513

The data refer to the municipalities used in the estimation—that is, those with a population of 250 or fewer inhabitants, and with non-missing data for some year(s) of the term.

Table A2: Effect of Direct Democracy on Public Finances (No Fixed Effects)

Panel A: Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.
DirDem	-0.187***	-0.142***	-0.156***	-0.168***	-0.169***	-0.138***
	(0.0357)	(0.0365)	(0.0288)	(0.0403)	(0.0397)	(0.0445)
Observations	11975	17691	6001	42078	42078	42078
Municipalities	1145	1450	793	2689	2689	2689
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Panel B: Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.
DirDem	-0.152***	-0.141***	-0.152***	-0.171***	-0.177***	-0.147***
	(0.0320)	(0.0329)	(0.0248)	(0.0379)	(0.0371)	(0.0414)
Observations	10662	16117	5578	42162	42162	42162
Municipalities	1084	1383	765	2689	2689	2689
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Panel C: Deficit (euros)

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit
DirDem	-1.851	0.266	-1.722	-3.571	12.82	-6.372
	(7.883)	(7.585)	(9.366)	(15.24)	(19.27)	(19.50)
Observations	34617	41811	20694	42162	42162	42162
Municipalities	2286	2670	1603	2689	2689	2689
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Results from estimating Equation (1). Each column is a separate regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 54, [b] Optimal BW = 53. [c] Optimal BW = 122. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Placebo Tests: Covariate Smoothness around the Threshold (No Fixed Effects)

Variable	(1) Mean	(2) Optimal BW Value	(3) Opt. BW Results	(4) .5 x Opt. BW Results
Votes Right (%)	51.6	94	0.619	0.359
		[N = 12687]	(1.684)	(2.139)
Votes Left (%)	30.6	76	-2.768***	-1.984
		[N = 13551]	(1.003)	(1.257)
Votes Far Left (%)	3.2	77	0.0979	0.0346
		[N = 10071]	(0.280)	(0.330)
Votes Difference (%)	31.0	58	2.203	2.308
		[N = 13952]	(1.517)	(2.031)
Votes Winner (%)	59.3	58	0.508	0.761
		[N = 14355]	(0.915)	(1.279)
Turnout (%)	78.3	55	-0.543	-0.944*
		[N = 13142]	(0.482)	(0.561)
Mean Age (years)	53.0	104	0.275	0.431
		[N = 9910]	(0.381)	(0.398)
Young (%)	9.2	79	0.140	-0.0464
		[N = 11483]	(0.333)	(0.318)
Middle-Aged (%)	53.4	83	-0.897	-1.440*
		[N = 10450]	(0.691)	(0.848)
Old (%)	37.3	148	0.764	1.306
		[N = 11247]	(0.803)	(0.931)
Immigrants (%)	2.6	150	-0.206	-0.0970
		[N = 11242]	(0.275)	(0.272)
EU Immigrants (%)	45.4	62	-3.182	3.855
		[N = 4161]	(4.255)	(5.538)

Column 1 shows the mean of the variables. Column 2 shows the optimal bandwidth value and number of observations for a placebo test that estimates the effect of direct democracy on the corresponding variable, according to Equation (1). Columns 3 and 4 show the results for the placebo tests: each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. The optimal bandwidth (BW) is based on Imbens and Kalyanaraman (2012). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Placebo Tests: Lagged Outcomes (No Fixed Effects)

Panel A: Log Expenditures (t-1)

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.
DirDem	-0.143***	-0.131***	-0.0817**	-0.155***	-0.127***	-0.0803
	(0.0395)	(0.0408)	(0.0379)	(0.0471)	(0.0480)	(0.0529)
Observations	9501	13734	4790	33561	33561	33561
Municipalities	1071	1349	730	2581	2581	2581
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Panel B: Log Revenues (t-1)

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Rev.	Log Rev.	Log Rev.	Log Rev.	Log Rev.	Log Rev.
DirDem	-0.131***	-0.108***	-0.0675*	-0.145***	-0.108**	-0.0697
	(0.0392)	(0.0408)	(0.0396)	(0.0466)	(0.0472)	(0.0534)
Observations	9524	14086	4805	33628	33628	33628
Municipalities	1072	1370	731	2582	2582	2582
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Panel C: Deficit (t-1)

	(1)	(2)	(3)	(4)	(5)	(6)
	Deficit	Deficit	Deficit	Deficit	Deficit	Deficit
DirDem	-15.76*	-14.06*	-8.795	-17.46	-7.398	-26.68*
	(8.187)	(7.905)	(11.39)	(11.82)	(14.34)	(15.91)
Observations	10808	16103	5415	33628	33628	33628
Municipalities	1170	1505	782	2582	2582	2582
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Full	Full	Full

Results from estimating Equation (1). Each column is a separate regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 54, [b] Optimal BW = 53. [c] Optimal BW = 122. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect by Switches into and out of Direct Democracy

Panel A: Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.	(7) Log Exp.
DirDem	-0.0799***	-0.0665**	-0.0518*	-0.0786*	-0.141***	-0.0835**	-0.116
	(0.0267)	(0.0289)	(0.0268)	(0.0439)	(0.0452)	(0.0362)	(0.0792)
Observations	11932	11347	16987	5520	10379	16025	4664
Municipalities	1102	1093	1402	736	1090	1398	696
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[a]	Opt.	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.
Switches	All	Into	Into	Into	Out of	Out of	Out of

Panel B: Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.	(7) Log Rev.
DirDem	-0.0521**	-0.0422	-0.0551**	-0.0991**	-0.0877*	-0.0968***	-0.0729
	(0.0261)	(0.0283)	(0.0249)	(0.0451)	(0.0467)	(0.0348)	(0.0841)
Observations	10625	10054	15436	5107	9076	14464	4281
Municipalities	1047	1034	1337	709	1029	1333	666
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[b]	Opt.	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.
Switches	All	Into	Into	Into	Out of	Out of	Out of

Panel C: Deficit (euros)

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit	(7) Deficit
DirDem	-7.877	-6.187	-3.236	1.468	-17.49*	-13.77	-5.560
	(8.050)	(8.465)	(8.572)	(9.726)	(9.283)	(9.398)	(10.20)
Observations	34570	33680	40796	19965	32726	39842	19006
Municipalities	2239	2233	2614	1555	2232	2613	1554
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[c]	Opt.	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.
Switches	All	Into	Into	Into	Out of	Out of	Out of

Results from estimating Equation (2). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects. Columns with switches "Into" ("Out of") exclude from the sample the municipality-years corresponding to terms in which a municipality switched from direct (representative) to representative (direct) democracy. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 27, [b] Optimal BW = 25. [c] Optimal BW = 98. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Donut Regressions

Panel A: Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.
DirDem	-0.0799***	-0.0671**	-0.0388	-0.0439	-0.0635	-0.0724
	(0.0267)	(0.0290)	(0.0325)	(0.0376)	(0.0417)	(0.0496)
Observations	11932	11484	11017	10553	10152	9679
Municipalities	1102	1099	1095	1092	1090	1083
Specification	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[a]	Opt.	Opt.	Opt.	Opt.	Opt.
Excluded	0	1%	2%	3%	4%	5%

Panel B: Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.
DirDem	-0.0521**	-0.0428	-0.0111	-0.0169	-0.0141	-0.0358
	(0.0261)	(0.0288)	(0.0324)	(0.0375)	(0.0437)	(0.0534)
Observations	10625	10176	9708	9243	8836	8363
Municipalities	1047	1044	1037	1034	1030	1023
Specification	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[b]	Opt.	Opt.	Opt.	Opt.	Opt.
Excluded	0	1%	2%	3%	4%	5%

Panel C: Deficit

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit
DirDem	-7.877	-5.328	-4.899	-3.978	-9.502	-13.50
	(8.050)	(8.586)	(9.043)	(9.562)	(8.459)	(8.866)
Observations	34570	34121	33653	33189	32783	32309
Municipalities	2239	2238	2238	2237	2236	2234
Specification	Linear	Linear	Linear	Linear	Linear	Linear
Bandwidth	Optimal[c]	Opt.	Opt.	Opt.	Opt.	Opt.
Excluded	0	1%	2%	3%	4%	5%

Results from estimating Equation (2). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 27, [b] Optimal BW = 25, [c] Optimal BW = 98. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Robustness to Top-Coding Outliers

Panel A: Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.
DirDem	-0.0799***	-0.0815***	-0.0625***	-0.0797***	-0.0394	-0.0793**
	(0.0267)	(0.0255)	(0.0232)	(0.0244)	(0.0278)	(0.0392)
Observations	11932	11932	17646	14763	8914	5964
Municipalities	1102	1102	1405	1263	946	756
Bandwidth	Optimal[a]	Opt.	1.5 x Opt.	1.25 x Opt.	.75 x Opt.	.5 x Opt.
Top-Coding	NO	YES	YES	YES	YES	YES

Panel B: Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.
DirDem	-0.0521**	-0.0534**	-0.0700***	-0.0742***	-0.0492*	-0.0987**
	(0.0261)	(0.0252)	(0.0217)	(0.0234)	(0.0263)	(0.0394)
Observations	10625	10625	16074	13555	8064	5542
Municipalities	1047	1047	1340	1198	892	729
Bandwidth	Optimal[b]	Opt.	1.5 x Opt.	1.25 x Opt.	.75 x Opt.	.5 x Opt.
Top-Coding	NO	YES	YES	YES	YES	YES

Panel C: Deficit

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit
DirDem	-7.877	-4.460	-3.072	-3.619	-4.848	-2.212
	(8.050)	(4.990)	(4.848)	(4.902)	(5.211)	(5.935)
Observations	34570	34570	41761	38310	29227	20647
Municipalities	2239	2239	2620	2438	1960	1556
Bandwidth	Optimal[c]	Opt.	1.5 x Opt.	1.25 x Opt.	.75 x Opt.	.5 x Opt.
Top-Coding	NO	YES	YES	YES	YES	YES

Results from estimating Equation (2). The top-coded regressions winsorize the observations with a dependent-variable value above the top or below the bottom 1% of the observations within the bandwidth. Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 27, [b] Optimal BW = 25, [c] Optimal BW = 98. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Number of Observations at the Threshold

	(1) Obs.	(2) Obs.	(3) Obs.	(4) Obs.	(5) Obs.
DirDem	0.0204	0.0168	0.0143	0.00376	0.0462
	(0.0197)	(0.0186)	(0.0190)	(0.0265)	(0.0426)
Observations	18164	27188	22636	9480	4916
Municipalities	1255	1596	1437	901	671
Bandwidth	Optimal[a]	1.5 x Opt.	1.25 x Opt.	.75 x Opt.	.25 x Opt.

Results from estimating Equation (2). The dependent variable is a dummy that takes the value of 1 if data are available for a (potential) observation and 0 if the data are missing. Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a]Optimal BW = 73 inhabitants. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effect of Political Parties on Policy under Representative Democracy

Panel A: First Stage: Effect of RW Winner on RW Mayor

	(1) RW Mayor	(2) RW Mayor	(3) RW Mayor	(4) RW Mayor	(5) RW Mayor	(6) RW Mayor
RW Winner	0.566***	0.550***	0.631***	0.542***	0.524***	0.568***
	(0.0315)	(0.0225)	(0.0569)	(0.0225)	(0.0181)	(0.0329)
Observations	18772	27717	9476	18873	27806	9549
Municipalities	2845	3471	1843	2946	3560	1916
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Panel B: Effect of RW Mayor on Log Expenditures

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Exp.	(5) Log Exp.	(6) Log Exp.
RW Mayor	0.0346	0.0217	-0.0210	0.0181	0.0283	0.0220
	(0.0542)	(0.0423)	(0.0757)	(0.0273)	(0.0207)	(0.0418)
Observations	33481	46776	17557	33401	46708	17459
Municipalities	3869	4478	2827	3789	4410	2729
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Panel C: Effect of RW Mayor on Log Revenues

	(1) Log Rev.	(2) Log Rev.	(3) Log Rev.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.
RW Mayor	0.0580	0.0276	-0.00799	0.0355	0.0110	0.0328
	(0.0571)	(0.0446)	(0.0799)	(0.0271)	(0.0207)	(0.0421)
Observations	31070	43917	16186	30984	43848	16090
Municipalities	3747	4351	2690	3661	4282	2594
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Panel D: Effect of RW Mayor on Deficit

	(1) Deficit	(2) Deficit	(3) Deficit	(4) Deficit	(5) Deficit	(6) Deficit
RW Mayor	-20.86**	-12.27	-30.53**	-12.75	-5.076	-17.45
	(9.082)	(7.941)	(13.08)	(10.98)	(9.388)	(18.34)
Observations	20816	30322	10667	20717	30237	10591
Municipalities	3094	3707	2072	2995	3622	1996
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[d]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Results from estimating Equation (4). The sample is restricted to elections where the PP and PSOE were the two most-voted parties. Each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at the municipality level, are in parentheses. Fixed effects are municipality and year fixed effects. The optimal bandwidth (BW) is based on Imbens and Kalyanaraman (2012). [a] Optimal BW = 9 percentage points, [b] Optimal BW = 17 percentage points, [c] Optimal BW = 15 percentage points, [d] Optimal BW = 10 percentage points. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Placebo Tests: Covariate Smoothness for "Effects of Political Parties on Policy under Representative Democracy"

Variable	(1) Mean	(2) Optimal BW Value	(3) Opt. BW Results	(4) .5 x Opt. BW Results
Votes Right (%)	51.6	11	-0.452	-1.948
		[N = 24142]	(0.981)	(1.345)
Votes Left (%)	30.6	10	1.255	2.154
		[N = 20787]	(0.936)	(1.321)
Votes Far Left (%)	3.2	9	-0.885*	-0.987
		[N = 15221]	(0.470)	(0.649)
Votes Difference (%)	31.0	12	0.208	0.115
		[N = 25609]	(1.039)	(1.422)
Votes Winner (%)	59.3	14	0.772	1.022
		[N = 29447]	(0.601)	(0.821)
Turnout (%)	78.3	11	-1.787**	-1.128
		[N = 22498]	(0.744)	(0.978)
Mean Age (years)	53.0	12	0.691	1.529*
		[N = 16313]	(0.679)	(0.889)
Young (%)	9.2	13	-0.998*	-1.796**
		[N = 18259]	(0.593)	(0.785)
Middle-Aged (%)	53.4	14	-0.396	-0.794
		[N = 19014]	(0.625)	(0.840)
Old (%)	37.3	20	1.557*	1.130
		[N = 25697]	(0.870)	(1.182)
Immigrants (%)	2.6	16	0.0441	0.460
		[N = 21368]	(0.637)	(0.948)
EU Immigrants (%)	45.4	14	3.253	1.357
		[N = 16898]	(3.259)	(4.400)

Column 1 shows the mean of the variables. Column 2 shows the optimal bandwidth value and number of observations for a placebo test that estimates the effect of RW Mayor on the corresponding variable, according to Equation (4). Columns 3 and 4 show the results for the placebo tests: each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. The optimal bandwidth (BW) is based on Imbens and Kalyanaraman (2012). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Effect of Political Parties on Policy under Direct Democracy

Panel A: Effect of RW Mayor on Log Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.	Log Exp.
RW Mayor	0.0610	0.0492	-0.117	-0.0637	-0.0596	-0.169
	(0.133)	(0.106)	(0.228)	(0.116)	(0.0832)	(0.252)
Observations	2035	3078	1025	2007	3045	1010
Municipalities	375	495	239	347	462	224
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[b]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Panel B: Effect of RW Mayor on Log Revenues

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Rev.	Log Rev.	Log Rev.	Log Rev.	Log Rev.	Log Rev.
RW Mayor	0.0596	0.0601	-0.0382	-0.0301	-0.0904	0.0261
	(0.136)	(0.108)	(0.230)	(0.111)	(0.0818)	(0.206)
Observations	1980	3004	997	1954	2972	982
Municipalities	368	489	235	342	457	220
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[c]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Panel C: Effect of RW Mayor on Deficit

	(1)	(2)	(3)	(4)	(5)	(6)
	Deficit	Deficit	Deficit	Deficit	Deficit	Deficit
RW Mayor	16.79	13.48	21.91	27.06	14.67	-56.13
	(21.90)	(18.66)	(27.26)	(36.01)	(23.01)	(50.19)
Observations	3540	5454	1764	3506	5406	1741
Municipalities	545	727	341	511	679	318
Fixed effects	NO	NO	NO	YES	YES	YES
Bandwidth	Optimal[d]	1.5 x Opt.	.5 x Opt.	Opt.	1.5 x Opt.	.5 x Opt.

Results from estimating Equation (4) for direct-democracy municipalities. The sample is restricted to elections where the PP and PSOE were the two most-voted parties. Each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at the municipality level, are in parentheses. Fixed effects are municipality and year fixed effects. The optimal bandwidth (BW) is based on Imbens and Kalyanaraman (2012). [a] Optimal BW = 33 percentage points, [b] Optimal BW = 33 percentage points, [c] Optimal BW = 58 percentage points. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Effect of Direct Democracy on the Variability of Policy

Panel A: Log Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$
DirDem	-0.0418**	-0.0300*	-0.00694	-0.0452	-0.0391	-0.0234	-0.0117
	(0.0204)	(0.0154)	(0.0384)	(0.0286)	(0.0364)	(0.0454)	(0.0220)
Observations	6333	9349	3130	11932	11932	11932	6333
Municipalities	782	975	538	1102	1102	1102	782
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal^{a}	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal^{a}
Mun. trends							Yes

Panel B: Log Revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\lvert\hat{u}_{rev,mt}\rvert$	$\lvert\hat{u}_{exp,mt}\rvert$	$\lvert\hat{u}_{rev,mt}\rvert$	$\lvert\hat{u}_{rev,mt}\rvert$	$\lvert\hat{u}_{rev,mt}\rvert$	$\lvert\hat{u}_{rev,mt}\rvert$	$\lvert\hat{u}_{rev,mt}\rvert$
DirDem	-0.0348	-0.0245	-0.00604	-0.0354	-0.0370	-0.0424	0.00512
	(0.0254)	(0.0165)	(0.0343)	(0.0261)	(0.0315)	(0.0391)	(0.0289)
Observations	4653	6774	2225	10625	10625	10625	4653
Municipalities	668	808	426	1047	1047	1047	668
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal^{b}	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal^{b}
Mun. trends							Yes

Panel C: Deficit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$	$\lvert\hat{u}_{def,mt}\rvert$
DirDem	-11.08	-12.61*	-8.971	-16.06	-2.256	-11.43	-4.158
	(7.592)	(7.363)	(9.212)	(12.00)	(14.30)	(16.41)	(13.91)
Observations	28337	34570	15225	34570	34570	34570	11958
Municipalities	1927	2239	1285	2239	2239	2239	1102
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal^{c}	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Optimal^{c}
Mun. trends							Yes

Results from estimating Equation (2) (columns 1 to 6) or Equation (3) (column 7). The dependent variable is the absolute value of the residuals obtained from previously estimating Equation (2) or Equation (3). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects, and the last column also includes municipality-specific trends. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. ^{a} Optimal BW = 14, ^{b} Optimal BW = 10. ^{c} Optimal BW = 71. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effects by Number of Terms under Direct Democracy

Panel A: Expenditures, Revenues, and Deficit

	(1) Log Exp.	(2) Log Exp.	(3) Log Exp.	(4) Log Rev.	(5) Log Rev.	(6) Log Rev.	(7) Deficit	(8) Deficit	(9) Deficit
DirDem	-0.0634*	-0.0638**	-0.107**	-0.0436	-0.0661**	-0.106**	-3.098	-2.188	4.623
	(0.0336)	(0.0291)	(0.0483)	(0.0317)	(0.0267)	(0.0451)	(11.27)	(12.00)	(12.25)
DirDem x N_2	-0.0557	-0.0277	0.0673	-0.0587	-0.0272	0.0436	-12.16	-14.92	-25.82*
	(0.0524)	(0.0377)	(0.0958)	(0.0506)	(0.0373)	(0.0879)	(12.60)	(12.01)	(13.72)
DirDem x N_3	-0.106	-0.0491	-0.0176	-0.0566	-0.0941*	-0.0679	-22.93	-28.11	-11.95
	(0.0830)	(0.0578)	(0.133)	(0.0793)	(0.0571)	(0.132)	(21.79)	(21.65)	(23.02)
DirDem x N_4	-0.0683	0.00848	-0.0240	-0.00572	-0.0170	0.00995	-1.626	-6.934	26.38
	(0.104)	(0.0786)	(0.125)	(0.108)	(0.0802)	(0.171)	(17.78)	(18.05)	(26.65)
Observations	11932	17646	5964	10625	16074	5542	34570	41761	20647
Municipalities	1102	1405	756	1047	1340	729	2239	2620	1556
Bandwidth	Optimal^a	1.5 x Opt.	.5 x Opt.	Optimal^b	1.5 x Opt.	.5 x Opt.	Optimal^c	1.5 x Opt.	.5 x Opt.

Results from estimating Equation (5). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. ^{a} Optimal BW = 27, ^{b} Optimal BW = 25, ^{c} Optimal BW = 98. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Effects of Direct Democracy on Subsequent Elections Behavior

Panel A: Effect of Direct Democracy at t on Election Variables at t

	(1) Votes Right	(2) Votes Right	(3) Votes Left	(4) Votes Left	(5) Votes FarLeft	(6) Votes FarLeft	(7) Turnout	(8) Turnout
DirDem	-0.649	-0.353	0.212	0.389	0.281	-0.297	-0.217	-0.376
	(0.724)	(1.028)	(0.585)	(0.807)	(0.244)	(0.400)	(0.521)	(0.674)
Observations	11493	5526	12300	6332	10133	4926	11929	5967
Municipalities	1086	727	1125	780	1111	741	1102	754
Bandwidth	Optimal^a	.5 x Opt.	Optimal^a	.5 x Opt.	Optimal^b	.5 x Opt.	Optimal^b	.5 x Opt.

Panel B: Effect of Direct Democracy at t on Election Variables at $t+1$

	(1) Votes Right	(2) Votes Right	(3) Votes Left	(4) Votes Left	(5) Votes FarLeft	(6) Votes FarLeft	(7) Turnout	(8) Turnout
DirDem	-0.686	-1.834	0.300	0.879	0.532*	0.464	0.327	-0.117
	(0.776)	(1.252)	(0.617)	(0.894)	(0.307)	(0.457)	(0.476)	(0.669)
Observations	9263	4390	12845	6414	9753	5039	10917	5231
Municipalities	979	654	1178	797	1090	742	1064	718
Bandwidth	Optimal^c	.5 x Opt.	Optimal^c	.5 x Opt.	Optimal^d	.5 x Opt.	Optimal^d	.5 x Opt.

Each column is a separate local linear regression with a uniform kernel. Standard errors, clustered at both municipality and the running variable, are in parentheses. Fixed effects are municipality and year fixed effects. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. ^{a} Optimal BW = 150, ^{b} Optimal BW = 102, ^{c} Optimal BW = 87, ^{d} Optimal BW = 47. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: Effect of Direct Democracy on Transfers from Upper-Level Governments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log Tr.	Log Tr.	Log Tr.	Log Tr.	Log Tr.	Log Tr.	Log Tr.
DirDem	0.0109	-0.0202	0.00613	-0.00143	0.00195	0.0496	-0.0229
	(0.0489)	(0.0435)	(0.0656)	(0.0577)	(0.0662)	(0.0768)	(0.0499)
Observations	12655	19064	6525	40550	40550	40550	11504
Municipalities	1163	1506	803	2630	2630	2630	1096
Specification	Linear	Linear	Linear	Order 3	Order 4	Order 5	Linear
Bandwidth	Optimal[a]	1.5 x Opt.	.5 x Opt.	Full	Full	Full	Opt.
Mun. trends							Yes

Results from estimating Equation (2) (columns 1 to 6) and Equation (3) (column 7). Each column is a separate regression with a uniform kernel. All regressions include municipality and year fixed effects, and the last column also includes municipality-specific trends. Standard errors, clustered at both municipality and the running variable, are in parentheses. I calculate the optimal bandwidth following Imbens and Kalyanaraman (2012)'s procedure. [a] Optimal BW = 31. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Figures

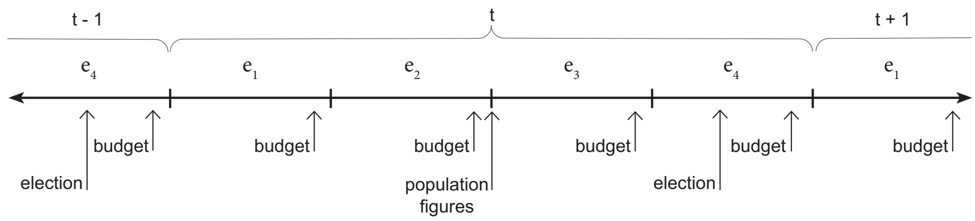
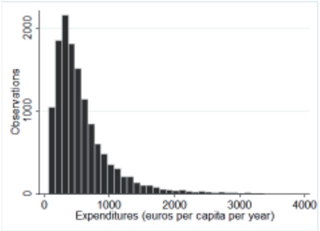
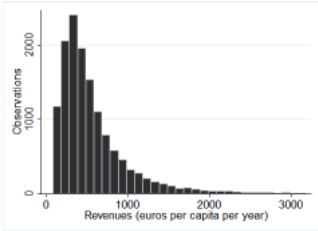


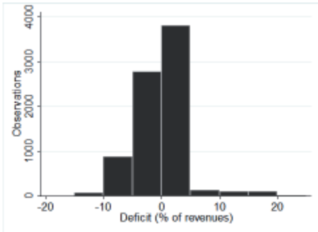
Figure A1: Timeline: each unit represents a year y.



(a) Expenditures

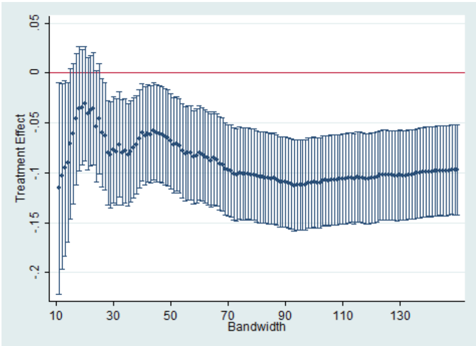


(b) Revenues

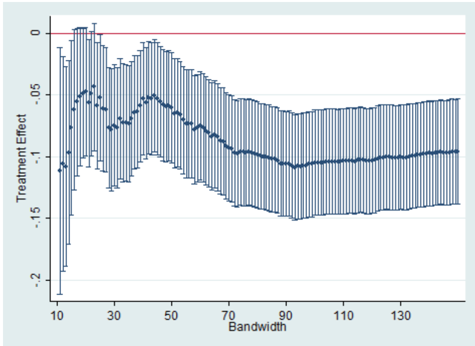


(c) Deficits

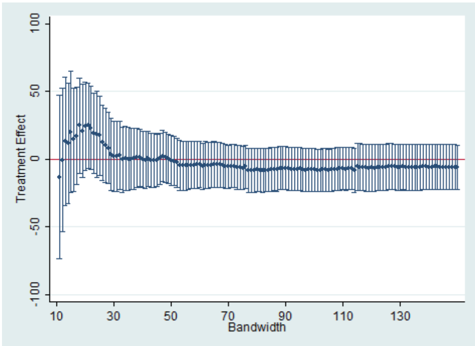
Figure A2: Histograms of expenditures, revenues, and deficits. An observation is a municipality-year. Observations above the 99^{th} and below the 1^{st} percentiles have been excluded to facilitate comprehension (90^{th} and 1^{st} for deficits). Bins are 100-euro wide (5 percentage points for deficit).



(a) Log Expenditures

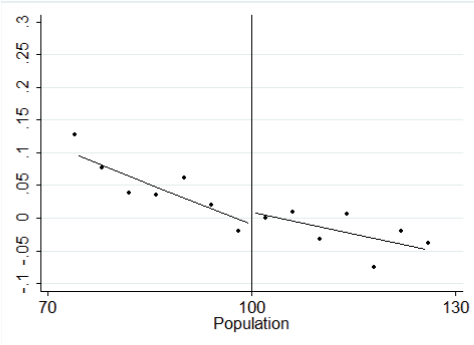


(b) Log Revenues

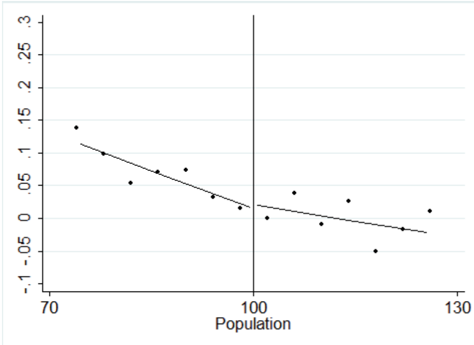


(c) Deficit

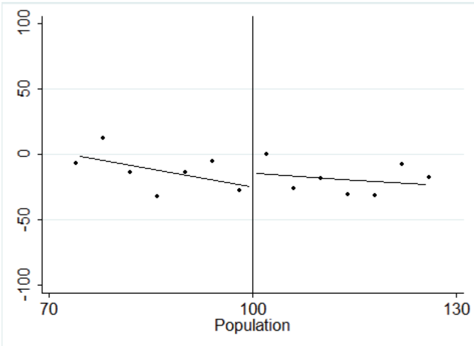
Figure A3: Robustness to Bandwidth Choice. Circles represent the estimated treatment effect, using different bandwidth choices (x-axis). Lines represent the 95% confidence interval (standard errors clustered at the municipality level). I report all possible bandwidths from 10 to 150 inhabitants. (The smaller sample sizes for bandwidths below 10 yield large confidence intervals—larger than .2, or around 20% of expenditures and revenues—for smaller bandwidths.)



(a) Log Expenditures (t-1)

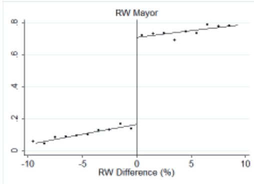


(b) Log Revenues (t-1)

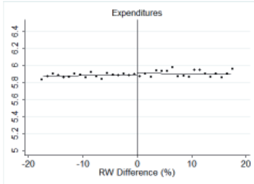


(c) Deficit (t-1)

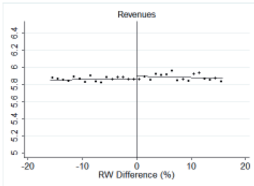
Figure A4: Placebo Tests: Effect of direct democracy on lagged expenditures, revenues, and deficit. I estimate $Outcome_{myt} = \alpha_m + \gamma_y + \sum_{j=100-OBW}^{100+OBW} \delta_j Population_{j,mt} + u_{myt}$, where $Population_{j,mt}$ is a dummy that indicates whether municipality m has population size j at term t. In the y-axis, I plot the estimated coefficients $\hat{\delta}_j$, averaged to 4-inhabitant-wide bins. I normalize the coefficients so that the average bin immediately to the right of the threshold takes the value of zero. The lines are linear fits on $\hat{\delta}_j$, fitted separately for observations above and below the threshold. I use the observations within the optimal bandwidth for $LogExpenditures_{myt}$, so that all graphs show the same range in the x-axis.



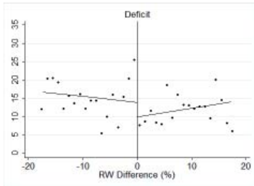
(a) Expenditures



(b) Expenditures



(c) Revenues



(d) Deficit

Figure A5: The effect of political parties on policy under representative democracy, following Equation (4). The x-axis shows the difference in the percent of votes between the PP and the PSOE in local elections. The sample is restricted to elections in which those two parties were the two most-voted parties. The bins are local averages of the dependent variables at 1-percentage-point intervals. The lines are fits of linear regressions run separately at both sides of the threshold.

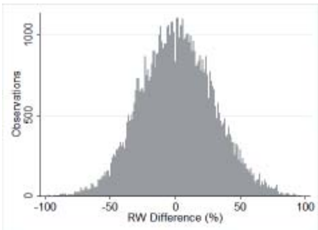


Figure A6: Histogram of RW Difference (difference in percentage points between the PP votes and the PSOE votes in local elections). An observation is a municipality-year. Bins are 1-percentage-point wide. The sample is restricted to elections in which the PP and the PSOE were the two most-voted parties.

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