

2019

Partisanship and Local Fiscal Policy: Evidence from Brazilian Cities

Raphael Gouvea

Institute for Applied Economic Research and University of Massachusetts Amherst

Daniele Girardi

Economics Department, University of Massachusetts Amherst

Follow this and additional works at: https://scholarworks.umass.edu/econ_workingpaper



Part of the [Economics Commons](#)


Recommended Citation

Gouvea, Raphael and Girardi, Daniele, "Partisanship and Local Fiscal Policy: Evidence from Brazilian Cities" (2019). *UMass Amherst Economics Working Papers*. 264.

<https://doi.org/10.7275/14301401>

This Article is brought to you for free and open access by the Economics at ScholarWorks@UMass Amherst. It has been accepted for inclusion in Economics Department Working Paper Series by an authorized administrator of ScholarWorks@UMass Amherst. For more information, please contact scholarworks@library.umass.edu.

Partisanship and local fiscal policy: evidence from Brazilian cities.*

Raphael Gouvêa[†]  Daniele Girardi[‡]

Abstract

We study the role of partisanship in shaping local fiscal policy in Brazilian cities in the 2004-2016 period. Using a regression-discontinuity design, we find no effect of left-wing mayors on the size of the city government nor on the allocation of spending across main budget categories (current spending, investment and personnel). We do find a modest, significant and robust positive effect of approximately 0.6 percentage points on the social expenditures share. We investigate mechanisms and find that re-election concerns and institutional constraints can explain the limited extent of spending allocation effects, while Tiebout-competition does not seem to play a role.

*We thank the Department of Economics, University of Massachusetts Amherst, for financial support through the Chair's Summer Research Fund. Raphael Gouvêa acknowledges financial support from CAPES (grant 0846/2014-07). We thank, without implicating, Arin Dube, Gilberto Tadeu Lima, Peter Skott and participants to the 2019 Analytical Political Economy Workshop for useful comments on previous versions of this paper.

[†]Institute for Applied Economic Research and University of Massachusetts Amherst.

[‡]Department of Economics, University of Massachusetts Amherst.

1 Introduction

Do political parties matter when it comes to governing cities? The classical Downsian model predicts policy convergence between parties (Downs, 1957). However, convergence can fail when parties/candidates are ideologically motivated and represent different constituencies (Alesina, 1987, 1988; Besley and Coate, 1997). While these broad theoretical considerations apply to all government levels, municipalities present some specificity, due to their different political, legal and economic environment (Peterson 1981; Ferreira and Gyourko 2009, pp. 401-403). A recent literature has studied this issue empirically in the US and other industrialized countries, finding that partisan control of a city government has some effect on local policies and outcomes in some European countries (Pettersson-Lidbom, 2008; Fiva et al., 2018), but not in the US (Ferreira and Gyourko, 2009).

This paper studies the effect of partisanship on local fiscal policy in the context of a developing country. Specifically, we study Brazilian municipalities and estimate the effect of electing a left-wing mayor on various fiscal policy variables through a regression-discontinuity design.

Overall, our baseline results point to substantial (but not complete) fiscal policy convergence between political parties in Brazilian cities. We find no effect of left-wing mayors on the size of the city government nor on the allocation of spending across the main budget categories (current spending, investment and personnel). We do find a modest, significant and robust positive effect on the share of social expenditures. The (close) election of a left-wing mayor tends to raise the share of social expenditures by around 0.6 percentage points in our preferred RD specification.

We then explore potential mechanisms which may limit partisan effects in Brazilian cities. Mainstream parties may just not have fundamentally different ideological views when it comes to local fiscal policy. Or they may have different ideological views on this matter, but their policy space may be constrained. Previous literature and our reading of the institutional context suggest three possible types of constraints. The first is re-election concerns, consistent with models of Downsian competition with reputation-building (eg Enelow and Munger (1993), Besley and Case (1995)). The second is institutional rigidities regarding the tax revenue system and the allocation of public expenditures. The third is Tiebout-type competition among local jurisdictions, which previous studies have found to be important in bringing about policy convergence in US municipalities (Ferreira and

Gyourko, 2009).

We propose empirical tests for these explanations. To assess the role of re-election concerns, we restrict the analysis to ‘lame-duck’ mayors, who face a binding term limit and are therefore less constrained by electoral competition in pursuing their agenda. To test the ‘institutional constraints’ hypothesis, we exploit exogenous changes in these constraints provided by ‘oil windfalls’. In Brazil, a subset of oil-producing municipalities experience sharp fluctuations in revenues due to fluctuations in oil production and prices. If partisan effects are limited by institutional constraints, we would expect to find larger effects when these constraints are relaxed by oil-related revenue windfalls. Following the US literature (Ferreira and Gyourko, 2009), we test the ‘Tiebout-competition’ hypothesis by building a Herfindahl index, measuring the presence of potentially competing locations in the same local area, and test whether the impact of partisanship covaries with this index.

Our results suggest that none of these mechanisms explain the lack of partisan effects on the size of government. This suggests limited ideological differences between mainstream parties on this topic. This interpretation appears consistent with both survey evidence on the policy preferences of Brazilian politicians (Zucco and Power, 2018) and studies of the evolution of the policy proposals of the Workers’ Party (PT), the leading Brazilian left party (Campello, 2016).

However, institutional constraints and re-election concerns *do* appear to explain the limited extent of budget composition effects. In cities where institutional constraints are relaxed by oil windfalls, left-wing mayors raise the share of social expenditures by around 2.4 percentage points, a fourfold increase compared to the baseline results. Moreover, in this subsample we also find a marginally significant effect of around 1.4 percentage points on the share of personnel expenditures. We also find a larger effect on the share of social expenditures among ‘lame-duck’ mayors (0.97 percentage points versus 0.60 in the baseline). We do not find support for the Tiebout-competition hypothesis.

1.1 Related literature

Some recent papers have used regression-discontinuity designs (RDDs) to study the causal effect of political partisanship on city-level fiscal policy and other outcomes. These studies have pointed to significant effects of left-wing parties on the size and composition of the city budget in Nordic European countries (Norway and Sweden), but no effect of Democrat mayors in US cities. Little

evidence has been available so far on developing countries.¹

Specifically, Pettersson-Lidbom (2008) finds that left-wing city governments in Sweden increase the municipal budget, employ more workers, and reduce the local unemployment rate relative to conservative ones. Folke (2014) adapts the regression-discontinuity framework to study the role of small parties in proportional representation systems, finding large effects of party representation in Swedish municipal councils on immigration and environmental policy, but not taxes.

Fiva et al. (2018) estimate the effect of both government control and party representation in Norwegian cities. They find that a conservative city government lowers property taxes, but has no impact on spending allocations, and that an increase in the seats of left-wing parties leads to higher childcare spending and lower elderly care spending.

Ferreira and Gyourko (2009) find no partisan differences in policy outcomes between Democrat and Republican mayors in US cities. They investigate possible explanations, and find most support for Tiebout-competition among municipalities within metropolitan areas. This paper is the closest to ours in the sense of studying a majoritarian system in which a directly-elected mayor is the head of the city government.

A broader literature has studied partisan effects at the regional and national level on various outcomes. Lee et al. (2004) exploit close US congressional elections to show that the electoral strength of a legislator does not affect her voting behavior, suggesting that voters elect (rather than affecting) parties' policy positions. Leigh (2008) studies US States in the 1941-2002 period and finds partisan effects on post-tax inequality, unemployment, incarceration rates, minimum wages and welfare caseloads, but no impact on taxes, public employment and crime rates. Beland and Oloomi (2017) study the effect of the party affiliation of US Governors on fiscal policy, finding no effect on total spending but some differences in the allocation of funds, with Democratic governors allocating a larger share to health and education. In a related study, Beland (2015) finds that Democratic governors tend to cause reductions in racial gaps in employment and earnings. Other studies have looked at the effect of partisan electoral victories on financial markets (eg Snowberg et al. (2007); Girardi (2018)).

¹Most previous work on partisan effects in developing countries lacks a clear identification strategy, as the one provided by a RDD. It is hard, therefore, to rule out endogeneity biases in these previous work. For Brazil, for example, there are a few studies using panel regression with party dummies to study partisanship effects on fiscal policy at the local level (Sakurai, 2009; Sakurai and Menezes-filho, 2011; Sakurai and Gremaud, 2007).

2 Institutional Context

Brazil is a federal republic with three autonomous and independent administrative levels: the federal government, 27 states (including the federal district) and 5,570 municipalities. Brazilian municipalities have an executive and a legislative branch. The mayor is directly elected by plurality or majority rule and the city council by proportional rule. Local elections happen every 4 years in October and the elected mayor and city council start their mandate in January 1st of the following year. Municipal elections are always two year apart from federal and state elections, which happen at the same time. In municipalities with fewer than 200,000 voters, there is only one round for electing the mayor. In larger cities, there is a runoff between the two most voted candidates if none of them achieves an absolute majority in the first round. Mayors face a two-term limit.

Brazil is a case of multipartism, with 33 registered and roughly 14 effective parliamentary parties in 2016 (Nicolau, 2017; Gallagher and Mitchell, 2008).² Four parties, however, have played a major role in the period under study. The social-democratic, pro-Labor Worker's Party (PT) is dominant on the left and has won four consecutive presidential elections since 2002. The PT has moved towards the center during its bid to the 2002 presidential campaign (Campello, 2016). The leftist camp also includes smaller communist, socialist and green parties. PSDB and PMDB are the main center-right parties, while DEM/PFL is the most important party on the right (Zucco and Power, 2009, 2018). Appendix A provides the full list of parties that participated in the municipal elections we study.

The current constitution, enacted in 1988, promoted an important decentralization of the administrative structure, leading to an increase in the responsibility of city governments in the provision of public goods. The main areas under municipal responsibility are education (child care and primary), basic health services, provision of infrastructure in sanitation, transportation and urban planning.

Even though many expenditure categories have been decentralized to cities, tax collection continues to be rather centralized at the federal and state level. As a consequence, municipalities have relatively low self-financing capacity and are highly dependent on intergovernmental transfers,

² The effective number of parliamentary parties is a standard measure of political fragmentation in comparative politics, and is computed using the number of parties in parliament weighted by parties' vote shares (Laakso and Taagepera, 1979; Gallagher and Mitchell, 2008).

which accounted for 58 percent of all municipal revenues in 2016. Most of these revenues come from block-grant/earmarked transfer programs and a smaller share in the form of discretionary transfers. Since the enactment of the Law of Fiscal Responsibility in 2000, municipalities (as well as other levels of government) face strong restrictions in their levels of deficit and debt.

Importantly, in Brazilian municipalities (as well as at the federal and state level) the executive branch has a dominant role in crafting, approving and executing the budget. The role of the city council is mostly confined to amending limited parts of the budget bill crafted by the executive and, after spending has occurred, auditing and reviewing municipal spending. Moreover, given that in Brazil the budget law is not mandatory but just authoritative, the executive has large flexibility in deciding whether to execute or not each amendment approved by the city council (Alston et al., 2005; Albuquerque et al., 2013).

3 Data

We combine electoral results from the 2004, 2008 and 2012 Brazilian municipal elections with data on several public finance outcomes. Our sample includes 9,679 municipal elections for which we can calculate the left's margin of victory/loss (the running variable in our RDD) and have data on the fiscal policy outcomes of interest over the full post-election mayoral term.

3.1 Electoral results and partisanship

Data on municipal elections come from Brazil's Electoral Court - *Tribunal Superior Eleitoral* (TSE). We focus on the 2004, 2008 and 2012 elections, as data from previous elections is either unavailable or incomplete.³ From TSE, we obtain information on the candidate party, the composition of her coalition and the number of votes. With this information, we can compute the running variable in our RDD: the left's margin of victory/loss, defined as the vote share of the most voted left-wing candidate minus the vote share of the most voted non-left candidate. In case of a runoff, we use the runoff vote shares to compute the margin of victory. We use the Zucco and Power (2018) classification to determine the ideological stance of parties (left or non-left). When a party is not included in Zucco and Power (2018), we use other sources to assign party ideology. The partisanship

³As stated in the website of the data repository (accessed in April 2019).

classification is detailed in Appendix A.

3.2 Public finance

Public finance data come from Brazil’s National Treasury - *Secretaria do Tesouro Nacional* (STN). Municipalities report detailed information on expenditures and revenues to STN, which then publishes the dataset *Finanças do Brasil - Dados Contábeis dos Municípios* (FINBRA). We use total revenues and total expenditures per capita and as a share of GDP as our measures of government size. We also study how the allocation of expenditures among the main budget and functional categories is affected by party ideology. For the budget categories, we use current expenditures, personnel and investment as a share of total expenditures. Given the main areas under responsibility of municipalities, we study the allocation of functional categories in two groups: social and non-social expenditures. We define social expenditures as expenditures in health and sanitation, education and culture, and social welfare programs. Other expenditures are composed of housing and urban development, transportation and others, the latter being a residual group that includes all other functional categories.⁴ To create a sample of oil-windfall receivers (used in mechanisms’ analysis), we use information from the *Transferências Constitucionais* from STN. This database reports all non-discretionary transfers made by the central government to states and municipalities.

3.3 Sample selection and descriptive statistics

We take a number of steps to create our baseline sample. We start with all 16,685 municipal electoral results available in the TSE repository. We exclude 258 elections which occurred after the regular schedule. After computing the left’s margin of victory/loss, the running variable in our RDD, we are left with 10,789 elections. Concerning the outcome variables, even though FINBRA is an unbalanced panel dataset, it has a coverage rate of at least 93 percent of the municipalities per year. We exclude 580 observations from FINBRA because municipalities have not reported total revenues or total, current or capital expenditures. Moreover, we only keep observations for which we can observe all outcome variables over the full term. As a result, our baseline sample has 9,679 observations, where an observation is a municipality-election cycle. We supplement our data with

⁴The OECD includes pensions in the social expenditure categories (OECD, 2007). Given that pension expenditures are mostly pre-determined for the current mayor, as they are the result of hiring and wage setting decisions from previous administrations, we decide to include pensions in the residual group.

information on municipal GDP, population and a set of demographic and geographic covariates.

Table B.1 reports descriptive statistics, in the whole sample and in the sub-samples we use to analyze mechanisms. Even though these sub-samples select observations following different criteria (discussed in detail in next sections), overall they display similar characteristics as the baseline sample, except for geographic location.

4 Research design

We employ a regression-discontinuity design (RDD) (Hahn et al., 2001) to identify the effect of a mayor’s partisanship on local fiscal policy. Intuitively, we estimate a causal effect by comparing municipalities with closely-elected left-wing mayors with municipalities where the left-wing candidate barely lost the election. More formally, we test whether the expected values of our fiscal policy variables of interest display a discontinuity when the left margin crosses the victory threshold.

4.1 Regression-discontinuity specification

Our estimator of interest, which gives the effect of a left-wing mayor on fiscal policy variable y , is given by

$$\beta = \lim_{ml \downarrow 0} E[y|ml] - \lim_{ml \uparrow 0} E[y|ml] \quad (1)$$

where ml is the margin of victory/loss of the left candidate, defined as the difference between the vote share of the most-voted left-wing candidate and the vote share of the most-voted non-left candidate.

Our key identification assumption is that unobserved confounding factors – variables affecting both election probabilities and fiscal policy choices – do not ‘jump’ discontinuously around the threshold.⁵ This means that cities where the left candidate barely wins an election do not tend to be abruptly different from cities where the left barely loses. Under this ‘smoothness’ assumption, our RD estimator identifies the causal effect of a (closely-elected) left mayor on fiscal policy variables.

We estimate β through the following RD specification:

$$y_{it} = \beta \mathbb{1}\{ml_{it} > 0\} + f(ml_{it}) + \alpha_i + \tau_t + \varepsilon_{it} \quad (2)$$

⁵More precisely, the assumption is that counterfactual outcomes are continuous in the running variable.

where i and t index city and election year; y is a public finance variable measured as an average over the after-election mayoral term, that is, from year $t + 1$ until year $t + 4$ (see Section 2); ml is the left’s margin of victory/loss; $f(\cdot)$ is a potentially non-linear function that we approximate through kernel-weighted local linear regression; α_i and τ_t are city and year fixed effects.⁶ We use the Calonico et al. (2014) robust and bias-corrected estimator.

4.2 Design assessment

Results reported in Appendix C support the validity of our RD design. Figure C.1 shows that there is no evidence of electoral manipulation, as we do not find a discontinuity in the distribution of the running variable at the cutoff (McCrary, 2008). Table C.1 displays differences in pre-determined city characteristics between left and non-left mayors. The first column includes all elections in the sample, showing that in general cities electing a leftist mayor are different: they are more likely to be urban and in the northeast region, they have a lower share of white population, lower incomes and lower labor force participation. The subsequent columns show that these differences disappear if we restrict the comparison to progressively closer elections. Most importantly, column 6 estimates differences in pre-determined city characteristics using the same RD specification that we employ for estimating fiscal policy effects (equation 2), finding that all differences are both economically and statistically insignificant around the threshold.

5 Main results: impact of partisanship on local fiscal policy

This section presents our main results, which are reported in column 1 of Table 1 and displayed graphically in Figure 1. As explained in Section 4, all outcomes are measured as an average over the four years in office. Overall, we find no significant effect of left-wing mayors on the size of the city government nor on the allocation of expenditures across main economic categories (current spending, investment and personnel). We do find a modest significant positive effect on the social

⁶ We control for city and year fixed effects by first regressing y_{it} on city and year dummies, and then using residuals from this fixed-effects regression as the left-hand variable in our RD estimation. In the interest of efficiency, this ‘first-step’ fixed-effects regression uses the whole sample, including observations which are excluded from the RD estimation because the ‘left margin’ variable is not available. Our results do not change if we restrict the first-step regression to exactly the same sample used in the RD estimation. For details on this two-steps procedure, see Lee and Lemieux (2010, p. 331-333).

expenditures share.

5.1 Size of government

We proxy the size of city governments using their total revenues and expenditures, per capita and as a share of municipal GDP.

We find no significant partisan effects on the size of city government: there is no discontinuity at the threshold for any of the four proxies (top panel of Table 1, column 1; Figure 1, panels (a) to (d)).

[Insert Table 1 and Figure 1 here]

5.2 Budget composition

We now study how partisanship affects the allocation of resources. First, we look at the composition of expenditures across the main budget categories. Again, we find no significant effects: there is no evidence of discontinuities in the shares of current spending, personnel and investments in total spending (middle panel of Table 1, column 1; Figure 1, panels (e) to (g)).

Second, we look at the composition of expenditures across the main functions of government. We find a modest positive discontinuity in the share of social expenditures (Figure 1, panel (h)). The share of social spending is higher by 0.6 percentage points under a left-wing mayor, with $p < 0.01$ (bottom panel of Table 1, column 1). The ‘Health & Sanitation’ and ‘Education & Culture’ sectors account for most of this effect. All other categories seem to adjust to accommodate the increase in social expenditures: we do not find any single item among non-social expenditures which tends to be disproportionately penalized.

To assess how this reallocation translates into an increase in the level of social spending, we report results for the effect on social spending per capita (bottom panel of Table 1, column 1, and top panel of Figure 2). We find that social spending per capita increases by around 1 percent.

[Insert Figure 2 here]

In order to uncover dynamics, Appendix D reports results by year in office. The key finding is summarized in Figure 3: the effect on the social expenditures share increases gradually, reaching

0.95 percentage points in the last year in office, consistent with the idea that it takes time to reallocate resources. Effects on size of government and budget categories are confirmed to be small and insignificant for all years in office (Table D.1).

[Insert Figure 3 here]

5.3 Robustness tests

Appendix E reports robustness tests. Table E.1 restricts the analysis to progressively larger cities, up to the 90th percentile. Results are remarkably stable, indicating that heterogeneous effects by city size cannot account for our results. Table E.2 re-estimates equation 2 using alternative bandwidth selection criteria. Results are insensitive to bandwidth choice.

5.4 Falsification exercise

Given that we find a significant effect only on social expenditures, and that this effect is economically modest, we ask how likely it is to be the artifact of a tendency of our estimated RD specification to over-reject the null hypothesis, or of some failure of our identification assumptions. To assess this possibility, we perform the following falsification test. We randomly draw 200 placebo thresholds, a hundred from each side of the true threshold, and re-estimate equation 2 with the social expenditures share as the outcome variable and using a placebo threshold instead of the true threshold. In order to avoid mis-specification, we only include in the estimations observations from the same side of the true threshold. We consider only placebo thresholds that guarantee at least 25 observations in each side of the bandwidth to avoid biasing the the test against significant findings due to weak statistical power.

Figure 4 plots the distribution of t-statistics from the regressions using randomly-drawn placebo thresholds. There is no evidence of a tendency to find significant discontinuities away from the true threshold. Moreover, the t-statistics from our baseline estimations at the true threshold (vertical dashed lines) are in the tails of the distribution of placebo t-statistics and are consistent with levels of significance below 5 percent.

[Insert Figure 4 here]

Overall, the results presented in this section point to very limited partisanship effects in municipal fiscal policy. We now turn to the question of why this is the case and explore possible mechanisms that may help explain the substantial degree of fiscal policy convergence in Brazilian cities.

6 Mechanisms: what accounts for substantial fiscal policy convergence in Brazilian cities?

This section explores potential explanations for the rather limited partisan effects we have found. Our analysis below suggests that policy divergence in the allocation of spending is limited by both institutional constraints and re-election concerns. In contrast with previous evidence for the US, Tiebout competition does not seem to play a significant role in our sample.

6.1 Re-election concerns

Political competition is a natural candidate explanation for policy convergence, in the spirit of the classic Downsian model. We test this explanation by restricting the analysis to ‘lame-duck’ mayors, who cannot run for re-election because of term-limits. Of course, this does not eliminate political competition effects altogether – ‘lame-duck’ mayors may still care about their party/coalition winning the next election, or about their own reputation, for example in view of running for other offices – but it can weaken them significantly.

Results are consistent with re-election concerns playing a role in limiting budget composition effects (column 2 of Table 1 and middle panel of Figure 2). In the sub-sample with ‘lame-duck’ mayors, partisan effects on the share of social expenditures are indeed moderately larger than in the whole sample (0.97 versus 0.60 percentage points). Furthermore, in terms of social expenditures per capita, effects are almost three times larger in this subsample (3.3 versus only 1 percent in the baseline sample). In contrast, the effect on the size of the city government remains non-significant, suggesting that for this variable absence of partisan effects is not driven by re-election concerns.

6.2 Tiebout competition

Competition between cities within a geographical area (‘Tiebout competition’) may limit the policy space of mayors if residents can easily move to nearby cities (Tiebout, 1956; Peterson, 1981). Ferreira and Gyourko (2009) find evidence that this mechanism can explain policy convergence between Democrat and Republican mayors in US cities.

To test this hypothesis, we follow Ferreira and Gyourko (2009) in building a proxy for the intensity of Tiebout competition faced by each city in our sample. This measure of Tiebout competition is a Herfindahl index of the adult population (at least 16 years old) in each city within a commuting zone. This is calculated as the sum of the squares of the shares of population of the municipalities inside the same commuting zone (Ferreira and Gyourko, 2009, 417). A low value for this index indicates high Tiebout competition: many cities of small relative size within the same commuting zone; symmetrically, a high value signals low competition.

To assess whether Tiebout competition can explain our baseline results, we restrict the analysis to cities facing low Tiebout competition. Under the hypothesis that Tiebout competition explains policy convergence, we expect larger partisan effects in these cities. Column 3 of table 1 reports our RD specification in cities with below-median Tiebout competition (Herfindahl index above the median), while column 4 focuses on cities with Tiebout competition below the 25th percentile (Herfindahl index above the 75th percentile).

Overall, we do not find support for the Tiebout competition hypothesis. Effects on the size of government and on distribution among functional categories remain insignificant. Effects on the share of social expenditures get moderately larger in the sample with below-median Tiebout competition, but *smaller* in the sample with below-25th-percentile competition. Moreover, we find little effect on social expenditures per capita in both these subsamples (Table 1). These results are inconsistent with the Tiebout-competition hypothesis, which predicts that partisan effects should grow in size as the intensity of Tiebout competition gets lower.

6.3 Institutional constraints

As discussed in Section 2, Brazilian mayors appear to face strong *institutional constraints* on their fiscal policy decisions: laws regulating local public finance, limited tax capacity, and hardwired

expenditures. If binding, these constraints may help explain limited policy divergence.

To test this hypothesis, we look at cities and periods in which institutional constraints are exogenously relaxed by ‘oil-windfalls’: large increases in revenues due to royalties from oil production.⁷ If policy divergence is limited by institutional constraints, we expect to find larger effects (larger policy divergence) when these constraints are relaxed by oil windfalls.

Large oil windfalls are relatively common in our sample. Since the 1997 ‘New Oil Law’, companies must pay royalties amounting to 5 to 10 percent of oil output value, indexed at international prices. Most of these royalties are allocated to local governments, following rules that disproportionately benefit a set of “oil producer” municipalities (990 in our sample period). Moreover, both oil production and oil prices displayed large increases in the period we study, resulting in sudden substantial relaxations of the budget constraints of ‘oil producing’ cities.

Importantly, cities are relatively free in allocating revenues from oil royalties, with only two restrictions: these revenues cannot be directly used to hire new public employees on a permanent basis, nor to pay public debt. Caselli and Michaels (2013) show that on average (independent of partisanship) oil revenues tend to increase municipal spending on housing, urban development, transportation, education, health and transfers to households.

To identify a subsample of oil windfall receivers, we use the growth rate of average oil royalties received over an election cycle, *i.e.* over the mayoral term. We calculate this variable for each municipality-election observation and define our oil-windfall receivers sample as those observations above the median. In other words, a city-election enters our ‘oil-windfall subsample’ if there was a large (above-median) increase in revenues from oil royalties during the after-election mayoral term. This procedure leaves us with 1,335 observations. After we match these observations with our baseline sample, we are left with a subsample of 893 city-elections.

Partisan effects on the budget composition are indeed much stronger in the presence of oil windfalls (column 5 of Table 1 and bottom panel of Figure 2). The election of a left-wing mayor raises the share of social expenditures by 2.4 percentage points in this setting – a fourfold increase

⁷ Caselli and Michaels (2013, pp. 117-221 and online appendix) argue that oil windfalls are exogenous and provide details on the rules governing the allocation of oil royalties in Brazil. Note that our RD strategy does not rely crucially on oil windfalls being exogenous: contrary to Caselli and Michaels (2013), we do not compare cities receiving oil windfalls to cities not receiving them. Our comparison is between closely elected left and non-left mayors, within the subset of cities receiving oil windfalls. What we assume is that oil production and oil prices are not affected by a mayor’s fiscal policy choices and that heterogeneity in partisan effects between cities receiving oil windfalls and other cities are due to oil windfalls and not to other differences.

in the size of the effect relative to the baseline. The differential effect is even larger in terms of social expenditures per capita: 8 versus only 1 percent in the baseline. Left-wing mayors also increase the share of personnel expenditures by 1.4 percentage points in this subsample, even though this effect is significant only at the 10 percent level. Given that we do not find any effects for the other economic categories (current expenditure and public investment) and that cities cannot use oil revenues to hire new public employees, it seems likely that pay rises account for most of this effect.⁸

The effect on the overall size of the city government, however, remains null as in all other specifications and subsamples (column 5 of Table 1). This suggests that the absence of partisan effects on government size is not driven by the strong institutional constraints faced by Brazilian mayors, but rather by absence of underlying ideological differences between Brazilian mainstream parties on this topic, although of course we cannot rule out alternative explanations based on external constraints different from the ones we have been able to identify and measure.

6.4 Inference on differential impacts

We perform a simple bootstrap exercise to provide more information on the differential impacts presented in our mechanisms' analysis. For each mechanism, we estimate our RD specification separately in the subsample of interest and in the rest of the sample.⁹ We then take the difference between the two estimated effects and run 500 bootstrap replications to obtain standard errors (clustered by municipality) for this difference.

The main finding from this exercise is that the differential budget-composition effect for oil-windfall receivers is fairly precisely estimated, while the one for lame-duck mayors is noisier. Specifically, we find that the effect on the social expenditures share is higher by 1.95 percentage points for oil-windfall receivers relative to the rest of the sample, with a standard error of 0.99; it is higher by 0.52 points (s.e. 0.47) for lame-duck mayors and by 0.77 points (s.e. 0.41) in cities with Tiebout competition below median, but lower by 0.11 points (s.e. 0.41) in cities with Tiebout competition

⁸Even though we cannot directly test it, this interpretation seems in line with Caselli and Michaels (2013) results. When they study the effects of oil windfalls on measures of real outcomes in the areas where they find spending increases, the authors find no effect for most outcome variables. The only exception is the education sector where oil-related-revenues are associated with increases in the number of teachers and classrooms per capita.

⁹This procedure is equivalent to using interaction terms to obtain differences in group effects in a parametric regression. We perform a bootstrap exercise due to our non-parametric approach. Point estimates for differential effects from this exercise are not equal to differences between coefficients from different columns of Table 1, because here we take the difference between the subsample and the rest of the observations (excluding the subsample itself), rather than between the subsample and the whole sample.

below the 25th percentile. The differential effect on the share of personnel expenditures is 1.51 points (s.e. 0.93) for oil-windfall receivers.

6.5 Dynamics, robustness and placebo tests

Figure 3 and Appendices E and F report dynamic effects, robustness tests and placebo exercises for our mechanisms' analysis. Like baseline ones, results are robust to bandwidth selection criteria and pass a falsification exercise using placebo thresholds. Moreover, social spending effects increase gradually over time also in the 'lame duck' and 'oil-windfall' subsamples, and are larger compared to the baseline for each single year in office (Figure 3).

7 Conclusions

This is the first study to provide causally identified evidence about the influence of political partisanship on local fiscal policy in the context of a developing country. We study a large sample of Brazilian municipalities in the 2004-2016 period and employ a regression-discontinuity design, thus focusing on close mayoral elections.

We find no effect of left-wing mayors on the size of the city government, nor on the allocation of spending across the main budget categories (current spending, investment and personnel), but a small positive effect on the share of social expenditures. A left-wing mayor tends to raise the share of social expenditures by around 0.6 percentage points relative to a non-left mayor in our preferred RD specification. This reallocation results in a 1 percent increase in social spending per capita.

We then explore three potential mechanisms that may account for the lack of more substantial partisan effects. Our results suggest that re-election concerns and institutional constraints play a role in explaining the limited extent of budget composition effects. In cities that have their budget constraint relaxed by an 'oil windfall', the positive impact of a left-wing mayor on the share of social expenditures is four times larger than in the whole sample (around 2.4 percentage points), and there is also a positive impact on the relative share of personnel expenses. Also in the subsample of cities with 'lame-duck' mayors we find a larger effect on the share of social spending (0.97 percentage points, compared to 0.60 in the whole sample). These differential effects are even larger in terms of social expenditures per capita: on this variable we find a partisan effect of almost 8 percent

among oil-windfall receivers, and around 3 percent among ‘lame duck’ mayors, compared to just 1 percent in the whole sample. We find no empirical support for the Tiebout-competition hypothesis.

References

- C. Albuquerque, M. Medeiros, and P. H. Feijó. *Gestão de Finaças Públicas: fundamentos e práticas de planejamento, orçamento e administração financeira com responsabilidade fiscal*. Gestão Pública, 3rd edition, 2013. ISBN 8590627306.
- A. Alesina. Macroeconomic policy in a two-party system as a repeated game. *The Quarterly Journal of Economics*, 102(3):651–678, 1987. doi: 10.2307/1884222.
- A. Alesina. Credibility and policy convergence in a two-party system with rational voters. *The American Economic Review*, 78(4):796–805, 1988.
- L. Alston, M. Melo, B. Mueller, and C. Pereira. Who decides on public expenditures? a political economy analysis of the budget process: the case of brazil. Working Paper RE1-05-06, Inter-American Development Bank, September 2005.
- A. Baker and K. F. Greene. The Latin American left’s mandate: Free-market policies and issue voting in new democracies. *World Politics*, 63(1):43–77, 2011. ISSN 00438871. doi: 10.1017/S0043887110000286.
- L. P. Beland. Political parties and labor-market outcomes: Evidence from US states. *American Economic Journal: Applied Economics*, 7(4):198–220, 2015. ISSN 19457790. doi: 10.1257/app.20120387.
- L. P. Beland and S. Oloomi. Party Affiliation and Public Spending: Evidence From U.S. Governors. *Economic Inquiry*, 55(2):982–995, 2017. ISSN 14657295. doi: 10.1111/ecin.12393.
- T. Besley and A. Case. Incumbent behavior: Vote-seeking, tax-setting, and yardstick competition. *The American Economic Review*, 85(1):25–45, 1995.
- T. Besley and S. Coate. An economic model of representative democracy. *The Quarterly Journal of Economics*, 112(1):85–114, 1997.

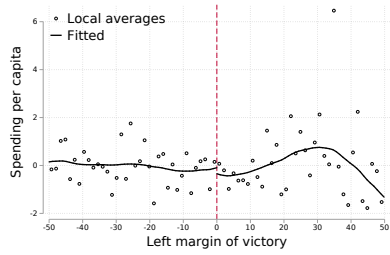
- S. Calonico, M. D. Cattaneo, and R. Titiunik. Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326, 2014. ISSN 00129682. doi: 10.3982/ecta11757.
- D. Campello. *The Politics of Market Discipline in Latin America: Globalization and Democracy*. Cambridge University Press, 2016. ISBN 9781107039254. doi: 10.1590/1981-382120160003000012.
- F. Caselli and G. Michaels. Do Oil Windfalls Improve Living Standards? Evidence from Brazil. *American Economic Journal: Applied Economics*, 5(1):208–238, 2013.
- M. D. Cattaneo, M. Jansson, and X. Ma. Simple Local Polynomial Density Estimators. *Working Paper*, 2018. URL <https://arxiv.org/abs/1811.11512v1>.
- A. Downs. *An Economic Theory of Democracy*. Harper and Row, 1957.
- J. M. Enelow and M. C. Munger. The elements of candidate reputation: the effect of record and credibility on optimal spatial location. *Public choice*, 77(4):757–772, 1993.
- F. Ferreira and J. Gyourko. Do Political Parties Matter? Evidence from U.S. Cities. *Quarterly Journal of Economics*, 124(1):399–422, 2009.
- J. H. Fiva, O. Folke, and R. J. Sørensen. The power of parties: Evidence from close municipal elections in norway. *The Scandinavian Journal of Economics*, 120(1):3–30, 2018. doi: 10.1111/sjoe.12229.
- O. Folke. Shades of brown and green: Party effects in proportional election systems. *Journal of the European Economic Association*, 12(5):1361–1395, 2014. doi: 10.1111/jeea.12103.
- M. Gallagher and P. Mitchell. *The Politics of Electoral Systems*. Oxford University Press, 1st edition, 2008. ISBN 9780199257560. doi: 10.1093/0199257566.001.0001.
- D. Girardi. Political shocks and financial markets: regression-discontinuity evidence from national elections. *UMass Amherst Economics Working Papers*, (8), 2018. URL <https://www.umass.edu/economics/publications/2018-08.pdf>.

- J. Hahn, P. Todd, and W. Klaauw. Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–209, 2001.
- IBGE. Produto Interno Bruto dos Municípios: ano de referência 2010. 2010.
- IBGE. Estimativas da população residente no Brasil e Unidades da Federação com data de referência 1° de julho de 2018. Technical report, IBGE, 2018.
- M. Laakso and R. Taagepera. “Effective” Number of Parties: a measure with application to West Europe. *Comparative Political Studies*, 12(1):3–27, 1979. ISSN 0010-4140.
- D. S. Lee and T. Lemieux. Regression discontinuity designs in economics. *Journal of Economic Literature*, 48(2):281–355, June 2010. doi: 10.1257/jel.48.2.281. URL <http://www.aeaweb.org/articles?id=10.1257/jel.48.2.281>.
- D. S. Lee, E. Moretti, and M. J. Butler. Do Voters Affect or Elect Policies? Evidence from the U. S. House. *The Quarterly Journal of Economics*, 119(3):807–859, 08 2004. doi: 10.1162/0033553041502153.
- A. Leigh. Estimating the impact of gubernatorial partisanship on policy settings and economic outcomes: A regression discontinuity approach. *European Journal of Political Economy*, 24(1): 256 – 268, 2008. ISSN 0176-2680. doi: <https://doi.org/10.1016/j.ejpoleco.2007.06.003>.
- J. McCrary. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714, 2008. ISSN 03044076. doi: 10.1016/j.jeconom.2007.05.005.
- J. Nicolau. Os quatro fundamentos da competição política no brasil (1994-2014). *Journal of Democracy em Português*, 6(1):83–106, 2017.
- OECD. The social expenditure database: An interpretive guide. Technical report, June 2007. URL https://stats.oecd.org/OECDStatDownloadFiles/OECDSOCX2007InterpretativeGuide_En.pdf.
- P. E. Peterson. *City limits*. University of Chicago Press, 1981.

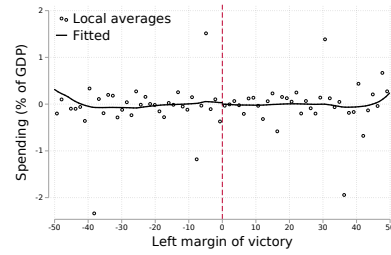
- P. Pettersson-Lidbom. Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach. *Journal of the European Economic Association*, 6(5):1037–1056, 2008.
- T. Power and C. Zucco. Brazilian Legislative Surveys (1990-2013), 2011. URL <https://doi.org/10.7910/DVN/T2PW7F>.
- S. N. Sakurai. Ciclos políticos nas funções orçamentárias dos municípios brasileiros: uma análise para o período 1990 - 2005 via dados em painel. *Estudos Econômicos (São Paulo)*, 39(1):39–58, 2009. ISSN 01014161. doi: 10.1590/S0101-41612009000100002.
- S. N. Sakurai and A. P. Gremaud. Political Business Cycles: evidências empíricas para os municípios paulistas (1989 – 2001). *Economia Aplicada*, 11(1):27–54, 2007.
- S. N. Sakurai and N. A. Menezes-filho. Opportunistic and partisan election cycles in Brazil : new evidence at the municipal level. *Public Choice*, 148(1-2):233–247, 2011. doi: 10.1007/s11127-010-9654-1.
- E. Snowberg, J. Wolfers, and E. Zitzewitz. Partisan impacts on the economy: evidence from prediction markets and close elections. *Quarterly Journal of Economics*, 122(2):807–829, 2007. doi: 10.1162/qjec.122.2.807.
- C. M. Tiebout. A pure theory of local expenditures. *Journal of political economy*, 64(5):416–424, 1956.
- C. Zucco and T. Power. Estimating ideology of Brazilian legislative parties, 1990-2005. *Latin American Research Review*, 44(1):218–246, 2009.
- C. Zucco and T. Power. Elite Preferences in a Consolidating Democracy: The Brazilian Legislative Surveys, 1990-2009. *Latin America Politics and Society*, 54(2):1–27, 2012. doi: <https://doi.org/10.1111/j.1548-2456.2012.00161.x>.
- C. Zucco and T. Power. Fragmentation Without Cleavages? Ideology, Policy Preferences, and Political Polarization in Brazil. *Working Paper*, 2018. URL <http://hdl.handle.net/10438/25940>.

Figure 1: Local fiscal policy indicators - baseline sample

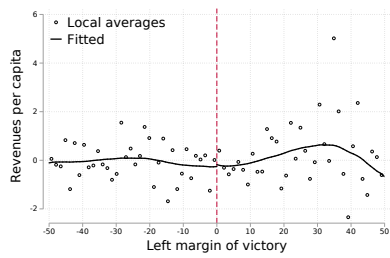
Size of government



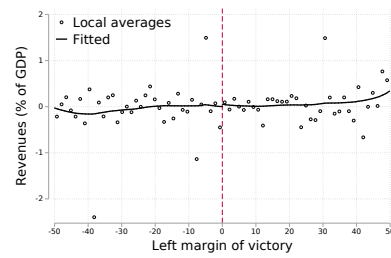
(a) Spending per capita



(b) Spending (% of GDP)

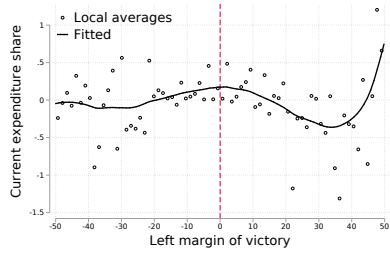


(c) Revenues per capita

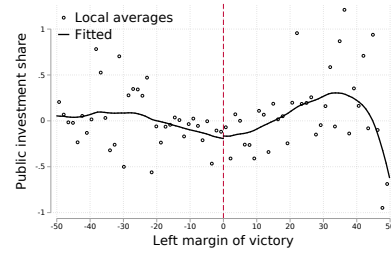


(d) Revenues (% of GDP)

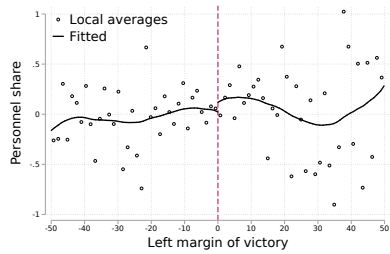
Expenditures composition (shares)



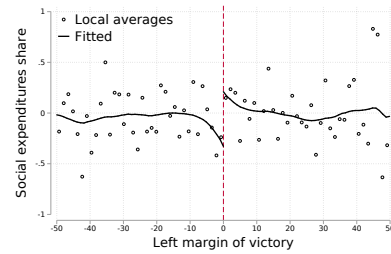
(e) Current expenditures



(f) Investment



(g) Personnel

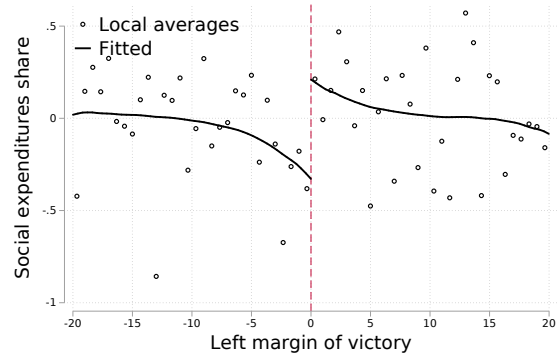
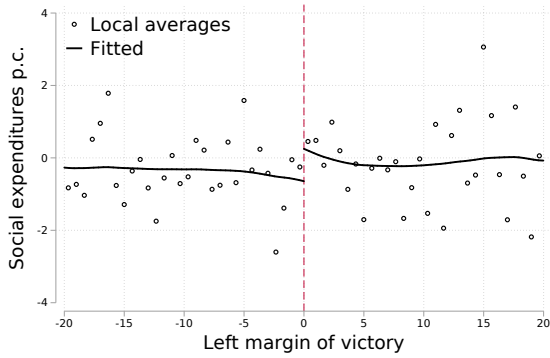


(h) Social expenditures

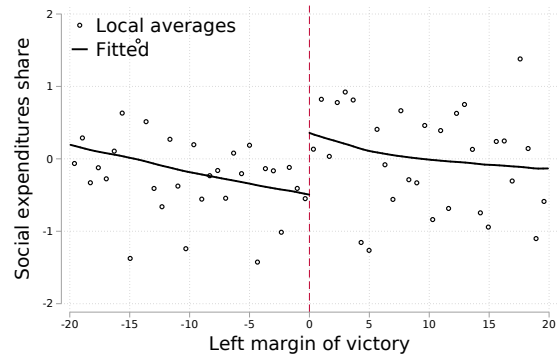
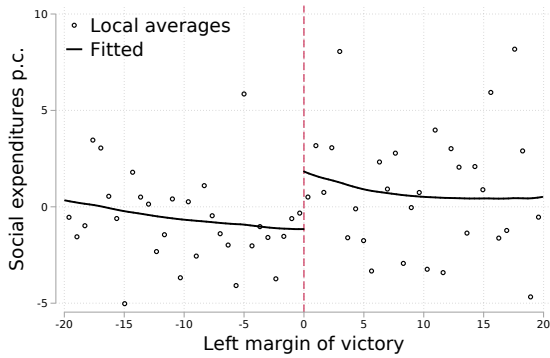
Notes: Visual presentation of our RD estimates of the effect of a left-wing mayor, reported in column 1 of table 1 and based on the specification in equation 2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel), with MSE-optimal bandwidth.

Figure 2: Effect of a left-wing mayor on social spending

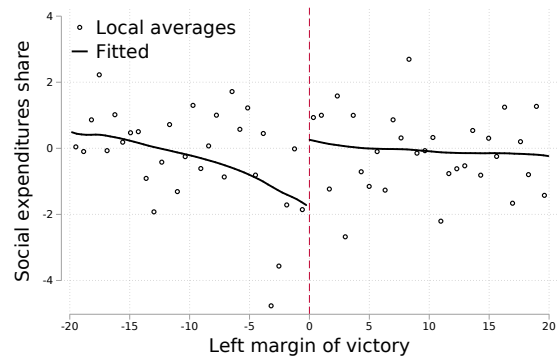
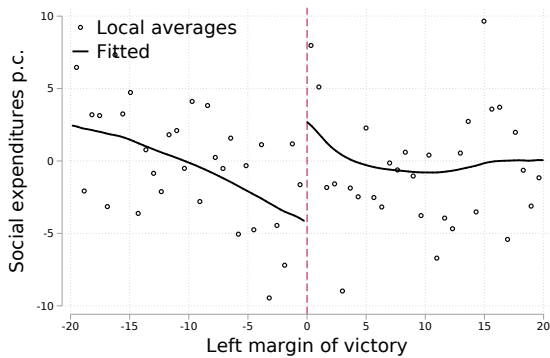
Baseline (whole sample)



'Lame-ducks' subsample

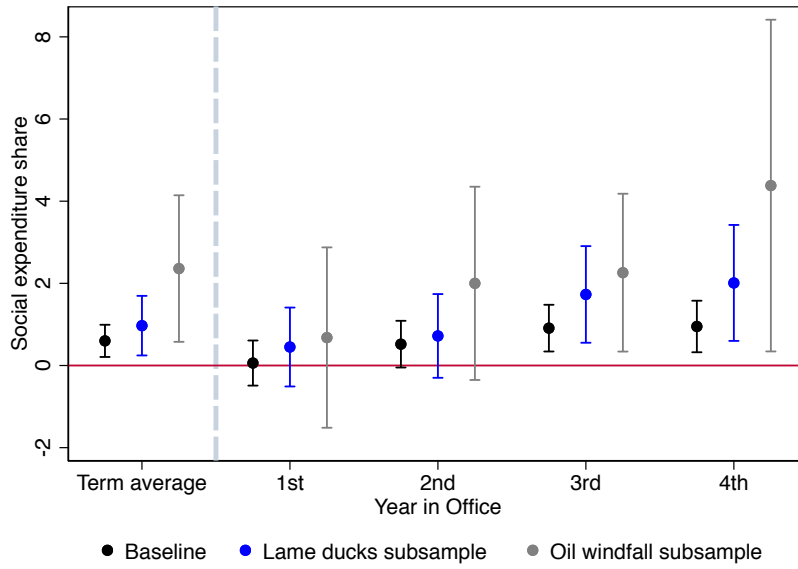


'Oil-windfalls' subsample



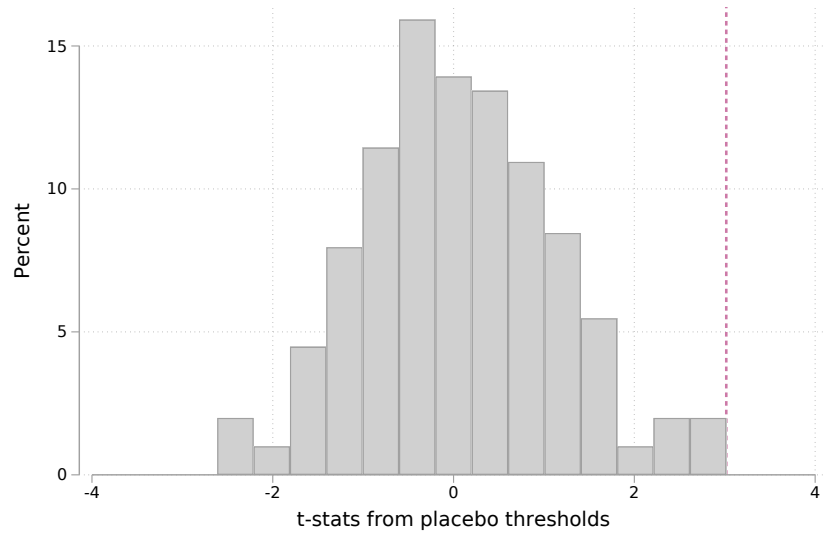
Notes: Visual presentation of our RD estimates of the effect of a left-wing mayor on social expenditures per capita (left) and as share of total expenditures (right) for each subsample, as reported in columns 1, 2 and 5 of table 1 and based on the specification in equation 2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel), with MSE-optimal bandwidth. See main text for definition and interpretation of the subsamples.

Figure 3: Social expenditure effects by year in office



Notes: Effect of a left-wing mayor on the share of social spending from our RD specification (equation 2), using the robust and bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Bars represent 95% confidence intervals from robust bias-corrected standard errors clustered by municipality.

Figure 4: Falsification test using placebo thresholds for the effect of a left-wing mayor on the social expenditures share – whole sample



Notes: Empirical distribution of t-statistics from our RD estimates (equation 2) of the effect of a left-wing mayor on the share of social spending, based on 200 randomly-drawn placebo thresholds, drawn separately on the left and on the right side of the true threshold (100 on each side), using only observations belonging to that side and with at least 25 observations on each side of the bandwidth. Vertical line = t-statistics obtained using the true threshold. The t-statistics are from the robust bias-corrected procedure of Calonico et al. (2014).

Table 1: RD estimates of the effect of a left-wing mayor

| Outcome | Baseline | Mechanisms | | | |
|---|------------------|------------------|------------------|------------------|------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Size of government: overall revenues and expenses | | | | | |
| Expenditure per capita | -0.21 (0.53) | 1.04 (1.18) | -1.19 (0.70) | -0.91 (0.90) | 2.52 (2.11) |
| Expenditure, % of GDP | -0.02 (0.21) | -0.70 (1.10) | -0.16 (0.33) | -0.56 (0.51) | -0.61 (0.46) |
| Revenue per capita | 0.16 (0.49) | 0.91 (1.14) | -0.41 (0.74) | 0.38 (1.10) | 1.87 (2.05) |
| Revenue, % of GDP | 0.09 (0.23) | -0.81 (1.11) | 0.06 (0.37) | -0.25 (0.57) | -0.74 (0.47) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | |
| Current Expenditure | -0.03 (0.18) | -0.29 (0.37) | -0.02 (0.23) | 0.33 (0.34) | 0.33 (0.60) |
| Public Investment | 0.05 (0.18) | 0.26 (0.36) | 0.11 (0.20) | -0.29 (0.34) | -0.39 (0.61) |
| Personnel | 0.09 (0.20) | -0.37 (0.41) | 0.06 (0.23) | 0.09 (0.33) | 1.44 (0.79) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | |
| Social Expenditures | 0.60 (0.20) | 0.97 (0.37) | 0.92 (0.27) | 0.45 (0.35) | 2.36 (0.91) |
| of which: | | | | | |
| Health & sanitation | 0.21 (0.14) | 0.75 (0.24) | 0.61 (0.21) | 0.49 (0.27) | 1.17 (0.56) |
| Education & culture | 0.19 (0.15) | 0.04 (0.25) | -0.00 (0.19) | -0.20 (0.30) | 1.10 (0.54) |
| Social welfare | 0.09 (0.05) | 0.22 (0.10) | 0.18 (0.07) | 0.12 (0.09) | 0.07 (0.19) |
| Other expenditures: | | | | | |
| Housing | -0.18 (0.13) | -0.37 (0.29) | -0.05 (0.18) | 0.02 (0.25) | -0.14 (0.51) |
| Transportation | -0.15 (0.09) | -0.16 (0.18) | -0.18 (0.12) | -0.11 (0.16) | -0.47 (0.20) |
| Other | -0.15 (0.20) | -0.63 (0.34) | -0.68 (0.29) | -0.33 (0.36) | -1.51 (1.04) |
| Social Expenditures per capita | 1.07 (0.60) | 3.28 (1.29) | 0.50 (0.77) | 0.10 (1.12) | 7.98 (2.71) |
| Observations (all) | 9679 | 2555 | 5418 | 3073 | 893 |
| Observations (effective) | 4898 | 1345 | 3173 | 2129 | 484 |
| Lame ducks subsample | | ✓ | | | |
| Tiebout < median subsample | | | ✓ | | |
| Tiebout < 25th pct subsample | | | | ✓ | |
| Oil windfall subsample | | | | | ✓ |

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Outcomes measured as 4-year averages over a mayoral term. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Online Appendix

Partisanship and local fiscal policy: evidence from Brazilian cities

Raphael Gouvêa and Daniele Girardi

Appendix A Partisanship classification

This appendix provides information on how we assign party ideology. To calculate the left margin of victory, we classify all parties that participated in the municipal elections of 2004, 2008 and 2012 as left or non-left (centrist, right or neither). This is not an easy task given that Brazil has one of the most fragmented party system in the world (Zucco and Power, 2018), with 33 registered parties in Brazil’s Electoral Court in 2018. Therefore, we base our classification using previous literature and only assign party ideology based on other sources for a few cases. In what follows we detail how candidate’s partisanship was coded.

First, we use Zucco and Power (2018, 2012, 2009)’s classification as our main source of party ideology. The classification is based on eight waves of the Brazilian Legislative Surveys (BLS) that have been carried out by the authors since the redemocratization of the country (Power and Zucco, 2011). The survey asks each legislator questions that require them to position themselves and all main parties in the political system on a “left-right” scale. Based on these answers, the authors create scores for each party in the “left-right” scale, where all parties to the left of PV (PV inclusive) in the 2017 survey is classified as left and to the right as non-left (Zucco and Power, 2018, p. 5). We classify 15 parties in this way.¹⁰

Second, we use Baker and Greene (2011) partisanship codes to classify other 12 parties. Baker and Greene (2011) provides scores in the left-right scale for all parties in Latin America that participated in a presidential election between 1995 and 2008.

Third, we follow Girardi (2018) and assign partisanship based on party international partisan association for all other cases. All parties affiliated to the Socialist International, Foro de São Paulo, Party of European Socialists or Progressive Alliance are coded as left. All the remaining parties are classified as non-left (centrist, right or neither).

Table A.1 reports the final classification with the respective source from which the party ideology was assigned.

¹⁰Even though party scores change for every survey-year, none of the parties switch from right to left (or the other way around) of PV score for the year 2017.

Table A.1: Party classification

| Leftist parties | | Non-leftist parties | |
|-----------------|-------------------------|---------------------|-------------------------|
| Party | Source | Party | Source |
| PV | Zucco and Power (2018) | DEM/PFL | Zucco and Power (2018) |
| PT | Zucco and Power (2018) | MDB/PMDB | Zucco and Power (2018) |
| PSOL | Zucco and Power (2018) | PP | Zucco and Power (2018) |
| PSB | Zucco and Power (2018) | PR | Zucco and Power (2018) |
| PPS | Zucco and Power (2018) | PRB | Zucco and Power (2018) |
| PDT | Zucco and Power (2018) | PSDB | Zucco and Power (2018) |
| PCdoB | Zucco and Power (2018) | PSL | Zucco and Power (2018) |
| PPL | Foro de São Paulo | PTB | Zucco and Power (2018) |
| PSTU | Baker and Greene (2011) | PRONA | Baker and Greene (2011) |
| PMN | Baker and Greene (2011) | PRP | Baker and Greene (2011) |
| PCO | Baker and Greene (2011) | PRTB | Baker and Greene (2011) |
| PCB | Baker and Greene (2011) | PSC | Baker and Greene (2011) |
| | | DC/PSDC | Baker and Greene (2011) |
| | | PODE/PTN | Baker and Greene (2011) |
| | | PTdoB | |
| | | PAN | |
| | | PHS | |
| | | PL | |
| | | PSD | |
| | | PTC | |
| | | PEN | |

Leftist parties: Partido Verde (PV), Partido dos Trabalhadores (PT), Partido Socialismo e Liberdade (PSOL), Partido Socialista Brasileiro (PSB), Partido Popular Socialista (PPS), Partido Democrático Trabalhista (PDT), Partido Comunista do Brasil (PCdoB), Partido Pátria Livre (PPL), Partido Socialista dos Trabalhadores Unificado (PSTU), Partido da Mobilização Nacional (PMN), Partido da Causa Operária (PCO), Partido Comunista Brasileiro (PCB). Non-leftist parties: Democratas/Partido da Frente Liberal (DEM/PFL), Movimento Democrático Brasileiro/Partido do Movimento Democrático Brasileiro (MDB/PMDB), Partido Progressista (PP), Partido da República (PR), Partido Republicano Brasileiro (PRB), Partido da Social Democracia Brasileira (PSDB), Partido Social Liberal (PSL), Partido Trabalhista Brasileiro (PTB), Partido da Reedificação da Ordem Nacional (PRONA), Partido Republicano Progressista (PRP), Partido Renovador Trabalhista Brasileiro (PRTB), Partido Social Cristão (PSC), Democracia Cristã/Partido Social Democrata Cristão (DC/PSDC), Podemos/Partido Trabalhista Nacional (PODE/PTN), Partido Trabalhista do Brasil (PTdoB), Partido dos Aposentados da Nação (PAN), Partido Humanista da Solidariedade (PHS), Partido Liberal (PL), Partido Social Democrático (PSD), Partido Trabalhista Cristão (PTC), Partido Ecológico Nacional (PEN).

Appendix B Descriptive Statistics

Table B.1: Descriptive Statistics

| | Baseline | | Lame Duck | | Tiebout < median | | Tiebout <25th perc. | | Oil windfall | |
|---|----------|--------|-----------|--------|---------------------|--------|------------------------|--------|--------------|--------|
| | mean | sd | mean | sd | mean | sd | mean | sd | mean | sd |
| Outcome variables | | | | | | | | | | |
| Size of government: overall revenues and expenses | | | | | | | | | | |
| Expenditure per capita | 8.11 | 69.08 | 10.88 | 65.05 | 10.47 | 68.99 | 10.06 | 68.43 | 10.72 | 70.52 |
| Expenditure, % GDP | 19.40 | 12.97 | 19.89 | 16.66 | 17.05 | 13.04 | 16.90 | 14.86 | 18.10 | 9.47 |
| Revenue per capita | 18.64 | 69.53 | 21.38 | 65.70 | 21.02 | 69.46 | 20.55 | 68.72 | 19.32 | 69.80 |
| Revenue, % GDP | 21.51 | 13.84 | 22.06 | 17.73 | 18.95 | 14.12 | 18.79 | 15.89 | 19.72 | 10.37 |
| Allocation of resources: budget categories (% of total expenditure) | | | | | | | | | | |
| Current Expenditure | 88.21 | 4.89 | 87.42 | 5.15 | 87.85 | 5.02 | 87.68 | 5.16 | 89.10 | 5.12 |
| Personnel | 48.12 | 7.14 | 47.48 | 7.05 | 47.66 | 6.93 | 47.44 | 7.20 | 49.57 | 7.59 |
| Public Investments | 10.19 | 4.86 | 10.97 | 5.13 | 10.53 | 4.98 | 10.70 | 5.10 | 9.06 | 5.17 |
| Allocation of resources: functional categories (% of total expenditure) | | | | | | | | | | |
| Social Expenditures | 59.45 | 8.41 | 59.09 | 8.46 | 58.43 | 8.16 | 57.70 | 8.20 | 60.63 | 8.62 |
| Health & sanitation | 24.14 | 5.27 | 23.95 | 5.16 | 24.10 | 5.48 | 23.94 | 5.57 | 22.69 | 5.23 |
| Education & culture | 31.50 | 8.26 | 31.30 | 8.18 | 30.67 | 7.91 | 30.08 | 7.62 | 34.60 | 8.35 |
| Social welfare | 3.81 | 1.71 | 3.84 | 1.74 | 3.66 | 1.67 | 3.68 | 1.69 | 3.34 | 1.55 |
| Housing | 9.01 | 4.76 | 9.42 | 4.89 | 9.20 | 5.04 | 9.29 | 5.13 | 10.60 | 5.07 |
| Transportation | 3.62 | 4.33 | 3.56 | 4.33 | 3.71 | 4.40 | 3.72 | 4.45 | 1.30 | 1.88 |
| Other | 27.92 | 7.42 | 27.94 | 7.64 | 28.66 | 7.51 | 29.29 | 7.61 | 27.48 | 7.99 |
| Demographics and geographic covariates | | | | | | | | | | |
| log(Population) | 957.15 | 116.95 | 975.40 | 129.92 | 976.96 | 139.04 | 958.36 | 121.23 | 1,035.32 | 124.28 |
| % Urban | 62.45 | 23.64 | 65.09 | 24.76 | 65.42 | 25.20 | 62.75 | 23.86 | 65.47 | 25.54 |
| % White | 51.13 | 24.95 | 54.34 | 25.01 | 53.09 | 25.45 | 52.38 | 24.98 | 39.70 | 21.79 |
| log(Earnings) | 602.34 | 55.64 | 614.01 | 53.51 | 614.91 | 51.40 | 600.81 | 57.07 | 601.47 | 54.08 |
| % Higher education | 2.42 | 2.18 | 2.66 | 2.32 | 2.65 | 2.34 | 2.35 | 2.13 | 2.36 | 2.50 |
| % Illiterate | 41.46 | 12.15 | 39.20 | 11.57 | 38.76 | 10.98 | 41.62 | 12.56 | 42.09 | 12.72 |
| Labor force participation | 54.92 | 8.45 | 55.94 | 8.09 | 56.10 | 8.05 | 54.98 | 8.87 | 55.08 | 6.58 |
| North | 0.08 | 0.27 | 0.10 | 0.30 | 0.11 | 0.31 | 0.07 | 0.25 | 0.00 | 0.07 |
| Northeast | 0.32 | 0.47 | 0.22 | 0.42 | 0.22 | 0.41 | 0.34 | 0.47 | 0.53 | 0.50 |
| South | 0.20 | 0.40 | 0.23 | 0.42 | 0.23 | 0.42 | 0.21 | 0.41 | 0.06 | 0.23 |
| Southeast | 0.33 | 0.47 | 0.38 | 0.48 | 0.34 | 0.48 | 0.32 | 0.47 | 0.41 | 0.49 |
| Midwest | 0.07 | 0.25 | 0.07 | 0.26 | 0.10 | 0.30 | 0.06 | 0.23 | 0.00 | 0.00 |
| Number of obs. | 9679 | | 2555 | | 5418 | | 3073 | | 893 | |

Notes: Outcome variables are from FINBRA-STN. All demographic and geographic covariates are obtained from Brazil's National Bureau of Statistics - *Instituto Brasileiro de Geografia e Estatística* (IBGE). Data on municipal GDP is from the publication *Produto Interno Bruto dos Municípios 2002-2016* (IBGE, 2010). Population comes from the 2000 and 2010 Census and from the publication *Estimativas da População* (IBGE, 2018) in non-census years. All other demographic variables come from the 2000 and 2010 Census.

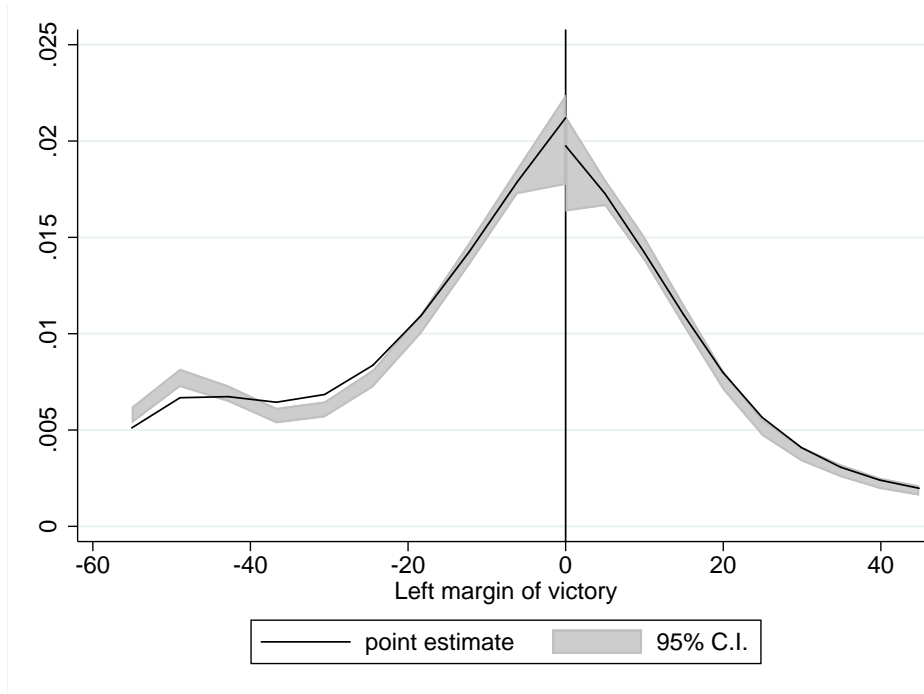
Appendix C Design assessment

Table C.1: Covariate balance tests: Difference in municipality characteristics between left and non-left mayors, by left margin of victory

| | All (1) | +/- 40 (2) | +/- 10 (3) | +/- 5 (4) | +/- 2.5 (5) | baseline RD (6) |
|---------------------------|------------------|------------------|------------------|------------------|------------------|--------------------|
| log(Population) | 19.80 (2.35) | 3.17 (2.52) | 1.38 (3.38) | -1.82 (4.57) | -2.96 (6.55) | -0.14 (0.47) |
| % Urban | 0.97 (0.47) | -0.15 (0.53) | -0.65 (0.74) | -1.29 (1.00) | -1.27 (1.42) | -0.24 (0.22) |
| % White | -3.09 (0.50) | -1.30 (0.57) | -0.69 (0.82) | -0.56 (1.13) | -0.03 (1.65) | -0.05 (0.20) |
| log(Earnings) | -1.84 (1.08) | -0.74 (1.24) | -0.07 (1.80) | -0.22 (2.47) | 0.89 (3.60) | -0.28 (0.65) |
| % Higher education | 0.09 (0.04) | 0.00 (0.05) | -0.03 (0.07) | -0.06 (0.09) | -0.09 (0.13) | -0.04 (0.03) |
| % Illiterate | 0.34 (0.24) | 0.14 (0.28) | -0.00 (0.40) | -0.02 (0.55) | -0.41 (0.78) | -0.13 (0.17) |
| Labor force participation | -0.53 (0.17) | -0.03 (0.20) | 0.18 (0.29) | 0.29 (0.40) | 0.42 (0.59) | 0.17 (0.16) |
| North | -0.00 (0.01) | -0.01 (0.01) | -0.00 (0.01) | 0.01 (0.01) | 0.01 (0.02) | 0.02 (0.02) |
| Northeast | 0.05 (0.01) | 0.02 (0.01) | 0.00 (0.02) | -0.01 (0.02) | -0.02 (0.03) | -0.05 (0.04) |
| South | -0.03 (0.01) | -0.02 (0.01) | -0.00 (0.01) | 0.01 (0.02) | 0.03 (0.03) | 0.03 (0.03) |
| Southeast | 0.01 (0.01) | 0.01 (0.01) | -0.00 (0.01) | -0.03 (0.02) | -0.02 (0.03) | -0.02 (0.03) |
| Midwest | -0.03 (0.00) | -0.01 (0.01) | 0.00 (0.01) | 0.01 (0.01) | -0.01 (0.02) | 0.01 (0.02) |
| Observations (all) | 16423 | 8689 | 3858 | 2060 | 1029 | 9619 |
| Observations (effective) | 16423 | 8689 | 3858 | 2060 | 1029 | 6560 |

Notes: Standard errors clustered by municipality. Column 6 employs our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city-year fixed effects.

Figure C.1: Tests of manipulation of the left margin of victory



Notes: The figure present visual evidence for the Cattaneo et al. (2018) manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the running variable at the cutoff. T-stat = -0.68; P-value = 0.49.

Appendix D Dynamic Effects

Table D.1: RD estimates of the effect of a left-wing mayor - dynamic effects baseline

| Outcome | Overall mandate | Dynamics | | | |
|---|------------------|------------------|------------------|------------------|------------------|
| | 4-year avg. | 1st year | 2nd year | 3rd year | 4th year |
| Size of government: overall revenues and expenses | | | | | |
| Expenditure per capita | -0.21 (0.53) | -0.02 (0.02) | -0.02 (0.02) | 0.21 (0.67) | -0.37 (0.73) |
| Expenditure, % of GDP | -0.02 (0.21) | 0.11 (0.15) | 0.16 (0.18) | -0.46 (0.72) | 0.01 (0.18) |
| Revenue per capita | 0.16 (0.49) | -0.02 (0.02) | -0.02 (0.03) | 0.31 (0.69) | 0.95 (0.94) |
| Revenue, % of GDP | 0.09 (0.23) | 0.30 (0.19) | 0.33 (0.20) | -0.38 (0.74) | 0.25 (0.21) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | |
| Current Expenditure | -0.03 (0.18) | 0.23 (0.27) | 0.22 (0.28) | -0.21 (0.23) | -0.24 (0.29) |
| Public Investment | 0.05 (0.18) | -0.21 (0.26) | -0.22 (0.25) | 0.24 (0.24) | 0.20 (0.29) |
| Personnel | 0.09 (0.20) | -0.00 (0.31) | 0.53 (0.28) | -0.08 (0.24) | -0.11 (0.28) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | |
| Social Expenditures | 0.60 (0.20) | 0.06 (0.28) | 0.52 (0.29) | 0.91 (0.29) | 0.95 (0.32) |
| of which: | | | | | |
| Health & sanitation | 0.21 (0.14) | -0.11 (0.22) | 0.10 (0.19) | 0.44 (0.21) | 0.58 (0.20) |
| Education & culture | 0.19 (0.15) | 0.16 (0.21) | 0.31 (0.22) | 0.14 (0.19) | 0.25 (0.20) |
| Social welfare | 0.09 (0.05) | -0.02 (0.08) | 0.10 (0.08) | 0.12 (0.07) | 0.09 (0.06) |
| Other expenditures: | | | | | |
| Housing | -0.18 (0.13) | -0.20 (0.20) | -0.14 (0.19) | 0.01 (0.16) | -0.31 (0.20) |
| Transportation | -0.15 (0.09) | -0.20 (0.14) | -0.24 (0.13) | -0.17 (0.12) | -0.11 (0.13) |
| Other | -0.15 (0.20) | 0.21 (0.24) | -0.07 (0.29) | -0.53 (0.29) | -0.54 (0.36) |
| Social Expenditures per capita | 1.07 (0.60) | -0.01 (0.01) | -0.01 (0.01) | 2.17 (0.80) | 1.32 (0.90) |
| Observations (all) | 9679 | 9679 | 9679 | 9679 | 9679 |
| Observations (effective) | 4898 | 4611 | 4726 | 4776 | 4804 |

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table D.2: RD estimates of the effect of a left-wing mayor - dynamic effects lameduck

| Outcome | Overall mandate | Dynamics | | | |
|---|------------------|------------------|------------------|------------------|------------------|
| | 4-year avg. | 1st year | 2nd year | 3rd year | 4th year |
| Size of government: overall revenues and expenses | | | | | |
| Expenditure per capita | 1.04 (1.18) | 0.03 (0.03) | 0.02 (0.04) | 0.74 (1.48) | 0.06 (1.55) |
| Expenditure, % of GDP | -0.70 (1.10) | 0.54 (0.36) | 0.15 (0.34) | -3.72 (4.19) | 0.34 (0.35) |
| Revenue per capita | 0.91 (1.14) | 0.02 (0.04) | 0.02 (0.05) | 1.28 (1.51) | 0.06 (1.52) |
| Revenue, % of GDP | -0.81 (1.11) | -0.03 (0.39) | 0.35 (0.39) | -3.90 (4.28) | 0.14 (0.39) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | |
| Current Expenditure | -0.29 (0.37) | -0.51 (0.52) | -0.20 (0.63) | -0.53 (0.52) | 0.02 (0.55) |
| Public Investment | 0.26 (0.36) | 0.52 (0.52) | -0.02 (0.67) | 0.35 (0.51) | -0.13 (0.55) |
| Personnel | -0.37 (0.41) | -0.59 (0.52) | 0.14 (0.55) | -0.52 (0.52) | -0.59 (0.58) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | |
| Social Expenditures | 0.97 (0.37) | 0.45 (0.49) | 0.72 (0.52) | 1.73 (0.60) | 2.01 (0.72) |
| of which: | | | | | |
| Health & sanitation | 0.75 (0.24) | 0.36 (0.39) | 0.76 (0.45) | 1.00 (0.45) | 1.48 (0.46) |
| Education & culture | 0.04 (0.25) | -0.09 (0.41) | -0.07 (0.35) | 0.23 (0.35) | 0.78 (0.58) |
| Social welfare | 0.22 (0.10) | 0.28 (0.14) | 0.20 (0.15) | 0.29 (0.14) | 0.18 (0.12) |
| Other expenditures: | | | | | |
| Housing | -0.37 (0.29) | -0.26 (0.40) | -0.30 (0.41) | -0.58 (0.41) | -0.32 (0.45) |
| Transportation | -0.16 (0.18) | -0.09 (0.27) | -0.12 (0.29) | 0.03 (0.26) | -0.37 (0.32) |
| Other | -0.63 (0.34) | -0.20 (0.51) | -0.54 (0.50) | -1.04 (0.65) | -1.91 (0.80) |
| Social Expenditures per capita | 3.28 (1.29) | 0.02 (0.02) | 0.02 (0.02) | 5.10 (1.90) | 1.61 (2.97) |
| Observations (all) | 2555 | 2555 | 2555 | 2555 | 2555 |
| Observations (effective) | 1345 | 1344 | 1316 | 1299 | 1211 |

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table D.3: RD estimates of the effect of a left-wing mayor - dynamic effects oil windfall

| Outcome | Overall mandate | Dynamics | | | |
|---|------------------|------------------|------------------|------------------|------------------|
| | 4-year avg. | 1st year | 2nd year | 3rd year | 4th year |
| Size of government: overall revenues and expenses | | | | | |
| Expenditure per capita | 2.52 (2.11) | -0.00 (0.05) | 0.02 (0.11) | 3.58 (2.91) | 3.42 (2.74) |
| Expenditure, % of GDP | -0.61 (0.46) | -0.17 (0.53) | -0.48 (0.74) | -0.84 (0.54) | -0.68 (0.73) |
| Revenue per capita | 1.87 (2.05) | 0.02 (0.08) | 0.01 (0.13) | 3.61 (2.25) | -0.22 (2.80) |
| Revenue, % of GDP | -0.74 (0.47) | 0.41 (0.60) | -0.84 (0.75) | -0.82 (0.58) | -1.43 (0.79) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | |
| Current Expenditure | 0.33 (0.60) | 0.08 (0.78) | 1.22 (0.95) | -0.84 (0.86) | 1.22 (0.96) |
| Public Investment | -0.39 (0.61) | -0.06 (0.84) | -1.21 (1.00) | 0.73 (0.83) | -1.27 (0.95) |
| Personnel | 1.44 (0.79) | 0.15 (0.89) | 1.68 (1.13) | 0.41 (1.09) | 2.47 (1.11) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | |
| Social Expenditures | 2.36 (0.91) | 0.68 (1.12) | 2.00 (1.20) | 2.26 (0.98) | 4.38 (2.06) |
| of which: | | | | | |
| Health & sanitation | 1.17 (0.56) | -0.34 (0.74) | 1.11 (0.76) | 1.61 (0.70) | 1.58 (0.86) |
| Education & culture | 1.10 (0.54) | 1.12 (0.85) | 0.96 (0.72) | 0.70 (0.54) | 2.17 (1.25) |
| Social welfare | 0.07 (0.19) | -0.12 (0.26) | -0.09 (0.31) | 0.02 (0.28) | 0.26 (0.23) |
| Other expenditures: | | | | | |
| Housing | -0.14 (0.51) | -0.02 (0.63) | -0.24 (0.79) | -0.42 (0.74) | -0.03 (0.72) |
| Transportation | -0.47 (0.20) | -0.44 (0.30) | -0.56 (0.38) | -0.27 (0.28) | -0.41 (0.28) |
| Other | -1.51 (1.04) | -0.23 (1.33) | -1.18 (1.34) | -1.53 (1.07) | -3.72 (2.29) |
| Social Expenditures per capita | 7.98 (2.71) | -0.01 (0.03) | 0.01 (0.04) | 6.59 (2.96) | 6.23 (2.95) |
| Observations (all) | 893 | 893 | 893 | 893 | 893 |
| Observations (effective) | 484 | 556 | 476 | 464 | 453 |

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Appendix E Robustness tests

Table E.1: RD estimates of the effect of a left-wing mayor - by city size

| Outcome | (1) | (2) | (3) | (4) | (5) |
|---|------------------|------------------|------------------|------------------|------------------|
| Size of government: overall revenues and expenses | | | | | |
| Expenditure per capita | -0.21 (0.53) | -0.50 (0.61) | 0.14 (0.73) | 0.68 (1.10) | 0.97 (1.73) |
| Expenditure, % of GDP | -0.02 (0.21) | -0.16 (0.26) | 0.02 (0.16) | 0.18 (0.18) | 0.16 (0.23) |
| Revenue per capita | 0.16 (0.49) | -0.12 (0.56) | 0.06 (0.63) | -0.18 (0.96) | 0.17 (1.39) |
| Revenue, % of GDP | 0.09 (0.23) | -0.05 (0.27) | 0.02 (0.15) | 0.06 (0.17) | 0.02 (0.23) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | |
| Current Expenditure | -0.03 (0.18) | 0.10 (0.19) | 0.05 (0.23) | -0.03 (0.33) | 0.01 (0.57) |
| Public Investment | 0.05 (0.18) | -0.01 (0.18) | 0.05 (0.22) | 0.18 (0.29) | -0.17 (0.64) |
| Personnel | 0.09 (0.20) | 0.22 (0.22) | 0.52 (0.28) | 0.32 (0.40) | 0.75 (0.67) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | |
| Social Expenditures | 0.60 (0.20) | 0.65 (0.23) | 0.56 (0.29) | 0.49 (0.41) | 0.50 (0.82) |
| of which: | | | | | |
| Health & sanitation | 0.21 (0.14) | 0.22 (0.17) | 0.06 (0.22) | 0.17 (0.33) | -0.16 (0.58) |
| Education & culture | 0.19 (0.15) | 0.27 (0.17) | 0.44 (0.23) | 0.25 (0.26) | 0.63 (0.47) |
| Social welfare | 0.09 (0.05) | 0.11 (0.06) | 0.03 (0.06) | 0.03 (0.09) | -0.10 (0.14) |
| Other expenditures: | | | | | |
| Housing | -0.18 (0.13) | -0.29 (0.16) | -0.31 (0.18) | -0.23 (0.24) | 0.22 (0.48) |
| Transportation | -0.15 (0.09) | -0.18 (0.10) | -0.21 (0.11) | -0.32 (0.15) | -0.21 (0.24) |
| Other | -0.15 (0.20) | -0.13 (0.25) | 0.04 (0.29) | 0.15 (0.42) | -0.39 (0.82) |
| Social Expenditures per capita | 1.07 (0.60) | 0.75 (0.66) | 1.20 (0.75) | 1.75 (1.12) | 1.92 (1.89) |
| Observations (all) | 9679 | 7259 | 4839 | 2419 | 967 |
| Observations (effective) | 4898 | 3847 | 2620 | 1283 | 541 |
| Baseline sample | ✓ | ✓ | ✓ | ✓ | ✓ |
| Population > 25th pct (6337) | | ✓ | | | |
| Population > median (13787.5) | | | ✓ | | |
| Population > 75th pct (30007.25) | | | | ✓ | |
| Population > 90th pct (73400.5) | | | | | ✓ |

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Outcomes measured as 4-year averages over a mayoral term. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

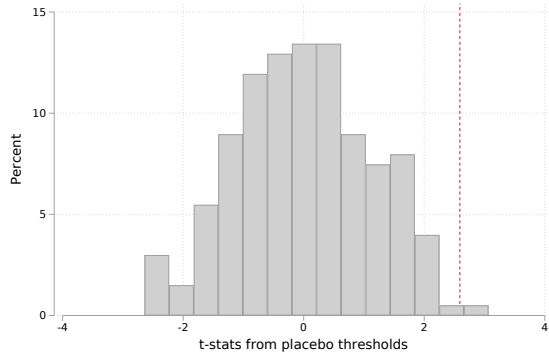
Table E.2: RD estimates of the effect of a left-wing mayor: robustness to alternative bandwidth selection

| Outcome | Baseline | | Lame Duck | | Tiebout < median | | Tiebout <25th perc. | | Oil windfall | |
|---|------------------|------------------|------------------|------------------|---------------------|------------------|------------------------|------------------|------------------|------------------|
| | MSE | CER | MSE | CER | MSE | CER | MSE | CER | MSE | CER |
| Size of government: overall revenues and expenses | | | | | | | | | | |
| Expenditure per capita | -0.21 (0.53) | -0.03 (0.57) | 1.04 (1.18) | 1.27 (1.16) | -1.19 (0.70) | -1.04 (0.75) | -0.91 (0.90) | -1.07 (0.95) | 2.52 (2.11) | 3.34 (2.22) |
| Expenditure, % of GDP | -0.02 (0.21) | 0.01 (0.20) | -0.70 (1.10) | -0.59 (1.10) | -0.16 (0.33) | -0.21 (0.36) | -0.56 (0.51) | -0.63 (0.59) | -0.61 (0.46) | -0.42 (0.46) |
| Revenue per capita | 0.16 (0.49) | 0.49 (0.53) | 0.91 (1.14) | 1.29 (1.14) | -0.41 (0.74) | -0.09 (0.82) | 0.38 (1.10) | 0.68 (1.21) | 1.87 (2.05) | 2.73 (2.17) |
| Revenue, % of GDP | 0.09 (0.23) | 0.20 (0.21) | -0.81 (1.11) | -0.76 (1.12) | 0.06 (0.37) | 0.05 (0.39) | -0.25 (0.57) | -0.28 (0.65) | -0.74 (0.47) | -0.48 (0.48) |
| Allocation of resources: budget categories (% of total expenditure) | | | | | | | | | | |
| Current Expenditure | -0.03 (0.18) | -0.02 (0.20) | -0.29 (0.37) | -0.20 (0.40) | -0.02 (0.23) | -0.01 (0.25) | 0.33 (0.34) | 0.38 (0.37) | 0.33 (0.60) | 0.46 (0.62) |
| Public Investment | 0.05 (0.18) | 0.06 (0.19) | 0.26 (0.36) | 0.16 (0.39) | 0.11 (0.20) | 0.08 (0.22) | -0.29 (0.34) | -0.36 (0.38) | -0.39 (0.61) | -0.45 (0.63) |
| Personnel | 0.09 (0.20) | 0.03 (0.22) | -0.37 (0.41) | -0.34 (0.45) | 0.06 (0.23) | 0.09 (0.25) | 0.09 (0.33) | 0.09 (0.36) | 1.44 (0.79) | 1.65 (0.84) |
| Allocation of resources: functional categories (% of total expenditure) | | | | | | | | | | |
| Social Expenditures | 0.60 (0.20) | 0.67 (0.22) | 0.97 (0.37) | 1.05 (0.40) | 0.92 (0.27) | 0.98 (0.31) | 0.45 (0.35) | 0.61 (0.39) | 2.36 (0.91) | 2.68 (0.99) |
| of which: | | | | | | | | | | |
| Health & sanitation | 0.21 (0.14) | 0.26 (0.16) | 0.75 (0.24) | 0.77 (0.27) | 0.61 (0.21) | 0.53 (0.24) | 0.49 (0.27) | 0.57 (0.31) | 1.17 (0.56) | 1.21 (0.59) |
| Education & culture | 0.19 (0.15) | 0.27 (0.16) | 0.04 (0.25) | -0.03 (0.27) | -0.00 (0.19) | 0.08 (0.21) | -0.20 (0.30) | -0.08 (0.33) | 1.10 (0.54) | 1.17 (0.59) |
| Social welfare | 0.09 (0.05) | 0.06 (0.06) | 0.22 (0.10) | 0.28 (0.11) | 0.18 (0.07) | 0.17 (0.08) | 0.12 (0.09) | 0.07 (0.10) | 0.07 (0.19) | 0.09 (0.21) |
| Other Expenditures: | | | | | | | | | | |
| Housing | -0.18 (0.13) | -0.19 (0.14) | -0.37 (0.29) | -0.43 (0.32) | -0.05 (0.18) | -0.02 (0.20) | 0.02 (0.25) | 0.00 (0.27) | -0.14 (0.51) | -0.24 (0.55) |
| Transportation | -0.15 (0.09) | -0.15 (0.10) | -0.16 (0.18) | -0.12 (0.20) | -0.18 (0.12) | -0.15 (0.13) | -0.11 (0.16) | -0.02 (0.18) | -0.47 (0.20) | -0.49 (0.21) |
| Other | 1.07 (0.60) | 1.44 (0.67) | 3.28 (1.29) | 3.79 (1.30) | 0.50 (0.77) | 0.90 (0.83) | 0.10 (1.12) | 0.38 (1.19) | 7.98 (2.71) | 8.22 (3.11) |
| Social Expenditures per capita | 1.07 (0.60) | 1.44 (0.67) | 3.28 (1.29) | 3.79 (1.30) | 0.50 (0.77) | 0.90 (0.83) | 0.10 (1.12) | 0.38 (1.19) | 7.98 (2.71) | 8.22 (3.11) |
| Observations (all) | 9679 | 9679 | 2555 | 2555 | 5418 | 5418 | 3073 | 3073 | 893 | 893 |
| Observations (effective) | 4898 | 3497 | 1345 | 1015 | 3173 | 2395 | 2129 | 1704 | 484 | 389 |

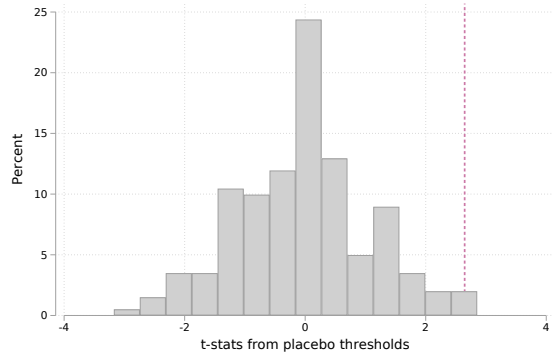
Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for city and year fixed effects. Outcomes measured as 4-year averages over a mayoral term. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Appendix F Placebo exercise by subsamples

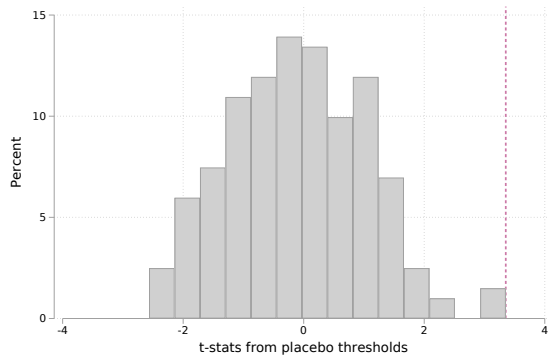
Figure F.1: Falsification test using placebo thresholds - effect on social expenditure share



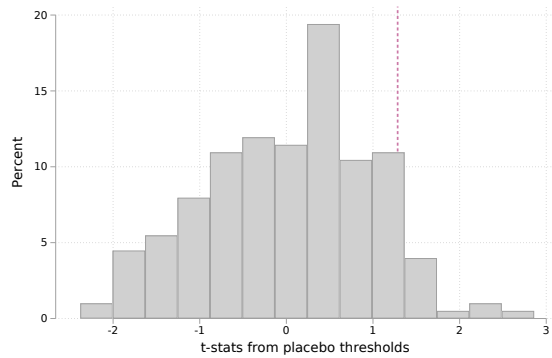
(a) Oil-windfall receivers



(b) Lame Ducks



(c) Tiebout competition < median



(d) Tiebout competition < 25th pct

Notes: Empirical distribution of t-statistics from our RD estimates (equation 2) of the effect of a left-wing mayor on the share of social spending, based on 200 randomly-drawn placebo thresholds, drawn separately on the left and on the right side of the true threshold (100 on each side), using only observations belonging to that side and with at least 25 observations on each side of the bandwidth. Vertical line = t-statistics obtained using the true threshold. The t-statistics are from the robust bias-corrected procedure of Calonico et al. (2014).