

Gouvêa, Raphael Rocha; Girardi, Daniele

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DEPARTMENT OF ECONOMICS

Working Paper

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Daniele Girardi

Working Paper 2019-06



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

Raphael Gouvêa[†] and Daniele Girardi[‡]

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Abstract

We study the role of political parties in shaping local fiscal policy in the context of 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 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 possible mechanisms which could bring about substantial fiscal policy convergence between political parties in Brazilian cities. We exploit oil-related revenue windfalls to explore the role of institutional constraints, and build an index of Tiebout competition to measure the role of the latter. We find support for the institutional constraints hypothesis in explaining the limited extent of spending allocation effects, and little support for the Tiebout-competition hypothesis.

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

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

1 Introduction

Do political parties matter when it comes to governing cities?¹ The classical Downsian model predicts policy convergence between different parties (Downs, 1957). However, policy 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 levels of government, the municipal level presents some specificity, due to its 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 context of 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).

In this paper we focus on the role of political parties in shaping 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. In order to deal with the endogeneity of electoral outcomes, we employ a regression-discontinuity design. Intuitively, this means that we focus on the behavior of closely-elected mayors of different political affiliations.

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 the mechanisms which may bring about substantial fiscal policy convergence in Brazilian cities. Brazilian 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 two possible types of constraints. The first is institutional rigidities regarding the tax revenue system and the allocation of public expenditures. The second is Tiebout-

¹We use the terms “city” and “municipality” interchangeably and, when referring to “local governments”, we always mean city/municipal governments (and not state governments).

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 discriminating between these different explanations. 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, oil prices, and the corresponding royalties. 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. In other words, we test whether different parties allocate these ‘windfall revenues’ differently. 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 neither institutional constraints nor Tiebout competition explain the lack of partisan effects on the size of government. This suggests limited differences in policy preferences 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 *do* appear to explain the limited extent of redistribution towards social expenditures and the lack of effects on the relative share of different budget categories. Left-wing mayors who have their budget constraint relaxed due to exogenous increases in revenues from oil production 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 significant and substantial effect on the share of personnel expenditures – around 1.4 percentage points in our preferred specification, and much higher (around 4.4 percentage points) in year of the next election. We do not find any support for the Tiebout-competition hypothesis.

1.1 Related literature

A few recent papers have used a regression-discontinuity design (RDD) 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.²

Specifically, [Pettersson-Lidbom \(2008\)](#) finds that left-wing city governments in Sweden tend to increase the size of the municipal budget by 2-3 percent, tend to employ 4 percent more workers, and to reduce the local unemployment rate by about 7 percent, relative to conservative city governments. More recently [Folke \(2014\)](#), also focusing on Swedish municipalities, adapts the regression-discontinuity framework in order to study the role of small parties in proportional representation (PR) systems, and finds large effects of party representation in municipal councils on immigration policy and environmental policy, but not on tax policy.

[Fiva et al. \(2018\)](#) estimate both the effect of government control and of party representation in the municipal council, in the context of Norwegian cities. They find that a conservative city government leads to lower 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.

While the studies discussed above are the closest to this paper, given their focus on municipal governments and fiscal policy outcomes, a broader literature has studied partisan effects at the regional and national level, on a range of outcomes. [Lee et al. \(2004\)](#) exploit close US congressional elections to show that the degree of electoral strength of a legislator does not affect her voting behavior, consistent with the idea that voters elect (rather than affect) parties' policy positions. [Leigh \(2008\)](#) studies US States in the 1941-2002 period. He finds that under Democratic governors post-tax inequality, unemployment and incarceration rates tend to be lower, while minimum wages

²Most previous work on partisan effects on developing countries use a regression framework, lacking a clear identification strategy as the one provided by the RD design. It is hard, therefore, to rule out endogeneity biases in these previous work. For Brazil, for example, there are a few studies using regressions (panel data) with party dummies to study partisanship of fiscal policy at the local level ([Sakurai, 2009](#); [Sakurai and Menezes-filho, 2011](#); [Sakurai and Gremaud, 2007](#)).

and welfare caseloads tend to be higher, but there are no differences in many other policy variables and outcomes, including tax rates, public employment, and crime rates. [Beland and Oloomi \(2017\)](#) study the effect of the party affiliation of US Governors on fiscal policy variables, 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, relative to Republican ones. Several papers look at the effect of partisan electoral victories on financial markets. For example, [Snowberg et al. \(2007\)](#) use prediction market data to show that the election of a Republican President in the US tends to raise stock market valuations by 3-4 percent; [Girardi \(2018\)](#) uses a large sample of close national elections and finds that left-wing victories cause a substantial short-term decrease in stock market valuations and in the value of the domestic currency, while government bond yields display little reaction.

2 Institutional Context

Brazil is a federal republic with three autonomous and independent administrative levels: the federal government, states and municipalities.³ Each Brazilian municipality has an executive and a legislative branch. The mayor is elected by majority rule and members of the city council by proportional rule. Local elections happen every 4 years in October and the elected mayor and city council members 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. In 1997, Congress passed the Reelection Amendment, allowing chiefs of the executive branches to run for reelection one time.

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

³The number of municipalities in Brazil has varied over the years. Currently, there are 5,570 municipalities.

transportation and urban planning.⁴ Even though many expenditure categories have been decentralized to cities, tax collection continues to be very centralized at the federal and state level. As a consequence, municipalities have relatively low self-financing capacity and are highly dependent on intergovernmental transfers, which accounted for 58% 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 restrictions in the level of deficits and debt.

3 Data

We combine electoral results from the 2004, 2008 and 2012 Brazilian municipal elections with data on several public finance outcomes. The resulting sample includes 9,679 municipal elections for which we can calculate the left’s margin of victory/loss (the running variable in our regression-discontinuity design) 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 Brazilian municipal elections come from Brazil’s Electoral Court - *Tribunal Superior Eleitoral* (TSE). We focus on the 2004, 2008 and 2012 elections as data from the 1996 and 2000 elections are incomplete.⁵ 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 regression discontinuity design: 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 the party ideology. The partisanship classification is detailed in [Appendix A](#).

⁴All these attributions are defined in Article 30 of the constitution. States are responsible for secondary education, non-basic health services, police and firefighter services, and infrastructure projects that involve many municipalities.

⁵As stated in the [website](#) of the data repository (accessed in April 2019).

3.2 Public finance

We complement the electoral data with information on public finance from Brazil’s National Treasury - *Secretaria do Tesouro Nacional* (STN). Municipalities report detailed information on expenditures and revenues to STN, which makes the information available in the dataset *Finanas do Brasil - Dados Contbeis dos Municpios* (FINBRA). We use total revenues and total expenditures per capita and as a share of GDP as our measures of the size of government. 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.⁶ To create a sample of oil windfall receivers, that we discuss in further detail in section 6.2, we use information from the *Transferncas Constitucionais* from STN. This database reports all non-discretionary transfers made by the central government to states and municipalities.

3.3 Municipal characteristics

We supplement our data with municipal characteristics obtained from Brazil’s National Bureau of Statistics - *Instituto Brasileiro de Geografia e Estatstica* (IBGE). Data on municipal GDP is from the publication *Produto Interno Bruto dos Municpios 2002-2016* (IBGE, 2010). Population comes from the 2000 and 2010 Census and from the publication *Estimativas da Populao* (IBGE, 2018) in non-census years. All other demographic variables – urbanization rate, race, labor force participation and education – come from the 2000 and 2010 Census.⁷

⁶The OECD includes pensions in the social expenditure categories (OECD, 2007). Given that expenditures in pensions are mostly pre-determined to 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.

⁷We use the Datazoom package, developed by the Department of Economics at PUC-Rio, to harmonize the definition of the variables labor force participation and education between the two census.

3.4 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 for the 2004, 2008 and 2012 elections. We then exclude 147 municipalities in 2008 and 111 in 2012, that had new elections after the regular schedule due to, for example, the death of a candidate or detection of fraud in the election. Since the left-wing margin of victory is the running variable in our regression discontinuity design, there must be at least one left and one non-left candidate in the ballot for that election to be included in our estimation sample. This condition is not met in 5,638 elections; of these, 1,293 cases had only left-wing candidates and 4,345 only centrist or right-wing candidates. Overall, we are left with 10,789 elections for which we can calculate the left margin.

In terms of our outcome variables, not all municipalities report information to STN every year. Even though FINBRA is an unbalanced panel dataset, it has a high coverage with a minimum of 5,172 municipalities in 2014 and maximum of 5,536 municipalities in 2006. We exclude 580 observations from FINBRA for which municipalities have reported a missing value or zero for 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. At the end, our baseline sample has 9,679 observations, where an observation is a municipality-election cycle.

Table 1 reports descriptive statistics in the baseline sample for our outcome variables, as well as other demographic and geographic covariates. It also shows descriptive statistics in the sub-samples we use to discuss possible mechanisms driving policy convergence. As we can see from the table, even though these sub-samples select observations following different criteria (discussed in detail in next sections), overall they shows similar results to the baseline, except in terms of the main geographic location of the municipalities.

4 Research design

We employ a regression-discontinuity (RD) design to identify the effect of a mayor's partisanship on local fiscal policy, thus focusing on close elections. Under plausible assumptions, our RD design allows to estimate a local average treatment effect by exploiting quasi-random variation around the victory threshold, thus avoiding the biases due to endogeneity of electoral outcomes. Standard

preliminary tests find no evidence of electoral manipulation, nor of differences in pre-determined covariates around the threshold, thus supporting the validity of our identification assumptions.

4.1 Regression discontinuity specification

Our RD approach (Hahn et al., 2001) exploits variation around the threshold that determines electoral victory. 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 precisely, we define the left margin of victory/loss in an election 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. We then test whether the expected values of our fiscal policy variables of interest display a discontinuity when the left margin crosses the 0 threshold. Formally, 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.

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.⁸ 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:

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

where i and t index city and election year; y is a public finance variable measured 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. We use the Calonico et al. (2014) robust and bias-corrected estimator.

⁸More precisely, the assumption is that counterfactual outcomes are continuous in the running variable.

4.2 Manipulation tests and covariate balance

To assess the validity of our RD design, we perform two standard types of tests: density tests to assess electoral manipulation (Figure 1); covariate-balance tests to verify whether pre-determined city characteristics are indeed similar around the threshold (Table 2). Results from both tests support the validity of our RD design.

The density test consists in assessing whether there is any discontinuity in the distribution of the running variable itself (the left margin) at the cutoff. Such a discontinuity would be consistent with electoral manipulation (McCrary, 2008). We employ the testing procedure of Cattaneo et al. (2018). Reassuringly, Figure 1 shows that there is no evidence of such a discontinuity.

Table 2 looks at differences in pre-determined city characteristics between cities that elect a left mayor and cities that elect a non-left mayor. The first column includes all elections in the sample, showing that in general cities electing a left 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 will employ for estimating fiscal policy effects (equation 2), finding all differences to be insignificant (both economically and statistically) around the threshold.

5 Main results: impact of partisanship on local fiscal policy

In this section we present and discuss our main results on the impact of partisanship on local fiscal policy. Overall, we find no significant effect on the size of the city government nor on the allocation of expenditures across the main economic categories, but we do find a modest significant positive effect of (closely) electing a left-wing mayor on the budget share of social expenditures.

5.1 Size of government

We proxy the size of city governments using their total revenues and expenditures. To account for differences in city size, we use total revenues and expenditures per capita and as a share of municipal GDP as our outcome variables in equation 2. Results are reported in the top panel of

table 3. Column 2 reports our preferred specification, which controls for city and year fixed effects and measures the outcome as an average over the four years in office.

We find no significant partisanship effects on the size of city government over the mayoral term (average over the four years in office). As clearly seen in figure 2, which displays graphically the results from our baseline specification, there is no discontinuity at the threshold for all our four measures of city government size.

Looking at averages over the term may hide dynamic effects, if mayors change their fiscal stance when the next election is approaching, as predicted by a political cycle hypothesis (Klein and Sakurai, 2015; Sakurai and Menezes-filho, 2008). ‘Opportunistic’ fiscal cycles may differ based on partisanship. To explore this possibility, we also report results using the size of government in the third or fourth year of the mayoral term as the outcome variable (columns 3 and 4 in Table 3). Also in the third and fourth years of the term we find no effect on the size of government.

5.2 Expenditures composition

We now study how partisanship affects the allocation of resources in two ways. 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 share of current spending, personnel and investments in total spending as shown in figure 2 and the middle panel of table 3.

Second, we look at the composition of expenditures across the main functions of government. As we discussed in section 2, the main areas under municipal responsibility are education, health and urban infrastructure (housing, urban planning and transportation). Figure 2 clearly shows a significant, but modest, increase in the share of social expenditures. As reported in column 2 of table 3, the share of social expenditures is higher by 0.6 percentage points under a left-wing mayor. The difference becomes larger when a new election approaches, reaching almost 1 percentage point in the fourth year. Table 3 also shows that the changes are mostly related to spending in the Health & Sanitation and Education & Culture categories, with the former playing a larger role in the third and fourth years. Moreover, all categories of other expenditures 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.

5.3 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 3 plots the distribution of t-statistics from the regressions using the randomly drawn placebo thresholds. As we can see, there is little evidence of a tendency to find significant discontinuities away from the true threshold. If that was not the case, the ‘smoothness’ assumption which underlies our RD design would be in question. Moreover, the t-statistics from our baseline estimations at the true threshold (vertical dashed line) are in the tails of the distribution of placebo t-statistic and are consistent with levels of significance below 5%.

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 very limited partisan effects we have found. Overall, the evidence seems to suggest that limited policy divergence in the allocation of spending is explained by external constraints. However, different from previous evidence for the US, where the urban setting has been found to discipline partisanship at the local level, in Brazil policy

convergence seems to be driven by public finance regulations.

6.1 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. Table 4 estimates our RD specification in cities with below-median Tiebout competition (Herfindahl index above the median); Table 5 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 (Table 4), but *smaller* in the sample with below-25th-percentile competition (Table 5). This is inconsistent with the Tiebout-competition hypothesis, which would predict that partisan effects should grow in size as the intensity of Tiebout competition gets lower.

6.2 Institutional constraints

As discussed in section 2, Brazilian mayors face a set of rules and regulations that constrain their fiscal policy decisions, and cities have limited self-financing capacity. Testing whether policy diver-

gence is limited by institutional constraints – laws regulating local public finance and pre-determined commitments – requires a setting in which cities have these constraints relaxed by exogenous factors. We exploit a unique characteristic of the oil sector in Brazil to explore this hypothesis. We re-estimate our RD specification in a sub-sample of ‘oil windfall’ receivers: cities that receive large exogenous increases in revenues due to payments of royalties from oil production. 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.

Oil production increased significantly in Brazil over the two decades after the enactment of the new Oil Law in 1997. From 2000 to 2018 (the years for which we use oil royalties payments to identify oil windfalls), production increased from near 466 to 977 million barrels a year. The new Oil Law was also important for our identification strategy because it increased the percentage that companies must pay in royalties to the government from 5% to 10% of output value and indexed the reference price to international prices, while before a subsidized domestic price was used. Royalties are collected by the national treasury and then transferred to other government administrations.

Most of these royalties are allocated to local governments following rules that disproportionately benefit a small number of “oil producer” municipalities.⁹ In fact, only 990 cities received direct payments of oil royalties between 2004 and 2016.¹⁰ But receiving royalties is only a necessary condition to characterize revenues windfalls. After all, what we want to capture is a sample of municipalities that had their budget constraints relaxed. Therefore, we need to look at situations where oil revenues increase by a large amount during the mayoral term. We identify these cases using the growth rate of the 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. This procedure leaves us with 1,335 observations. After we match these observations with our baseline sample, we are left with a sample of 893 observations.

It is important to clarify that cities face only two restrictions on the use of these oil royalties: they cannot use them to hire new public employees on a permanent basis or pay public debt (with

⁹In addition to cities, royalties are distributed to other federal entities and state governments.

¹⁰We call them “direct payments” because all municipalities in Brazil receive oil royalties through a fund that receives 8% of the total amount of royalty payments and distributes 80% of these resources to municipalities following the rules of the *Fundo de Participação dos Municípios* (FPM). For details on FPM and its role in shaping municipal fiscal policy, see [Corbi et al. \(2019\)](#).

the exception of debts to the federal government). If that was not the case, our assumption that institutional constraints are alleviated by oil royalties would not hold. Indeed [Caselli and Michaels \(2013\)](#) show that on average oil revenues tend to increase municipal spending on housing, urban development, transportation, education, health and transfers to household.

While [Caselli and Michaels \(2013\)](#) answer the question “what does the local government do with oil revenues?”, we ask whether fiscal policy changes along partisan lines in cities that receive oil windfalls. In other words, we test whether mayors allocate these windfall revenues differently based on partisan affiliation. The results in [table 6](#) suggest that this is indeed the case. For our preferred specification (average over 4-year term with city-time fixed effects), the close election of a left-wing mayor leads to an increase of the share of social expenditures in the order of 2.4 percentage points, a fourfold increase compared to the baseline sample. The result is even larger for the fourth year of the term, when the increase in share of social expenditures in total expenditures reaches 4.3 percentage points.

[Table 6](#) also shows that the (close) election of a left-wing mayor during an ‘oil-windfall’ increases the share of personnel expenditures by 1.4 percentage points, even though the effect is significant only at the 10% significance level. In the year of the election, however, this effect is larger, in the order of 4.4 percentage points, and significant at the 5% 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 most of the difference in spending that we find is due to pay raises. Even though we cannot provide more direct evidence, 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.

As shown in the top panel of [Figure 6](#), however, the effect on the overall size of the city government remains null. 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.

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 2002-2016 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 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.

We explore potential mechanisms that may account for the lack of more substantial partisan effects. Following the literature on US cities (Ferreira and Gyourko, 2009), we build a Herfindahl index to assess empirically the role of Tiebout competition among nearby municipalities. In the Brazilian context, institutional constraints may also be relevant: city governments are constrained by pre-committed expenditures, balanced-budget rules, and overwhelming reliance on transfers from the central government. We exploit oil-related revenue windfalls which relax these institutional constraints, in order to assess their relevance.

Our results suggest that institutional constraints play a relevant role in explaining the lack of more substantial partisan effects on the allocation of spending. 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 expenditure is three times larger (around 2.4 percentage points), and there is also a positive impact on the relative share of personnel expenses, especially during the last year of the mayoral term. We find no empirical support for the Tiebout competition hypothesis.

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Tables

Table 1: Descriptive Statistics

	Baseline		Tiebout < median		Tiebout <25th perc.		Oil windfall	
	mean	sd	mean	sd	mean	sd	mean	sd
Outcome variables								
Size of government: overall revenues and expenses								
Expenditure per capita	8.11	69.08	10.47	68.99	10.06	68.43	10.72	70.52
Expenditure, % GDP	19.40	12.97	17.05	13.04	16.90	14.86	18.10	9.47
Revenue per capita	18.64	69.53	21.02	69.46	20.55	68.72	19.32	69.80
Revenue, % GDP	21.51	13.84	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	87.85	5.02	87.68	5.16	89.10	5.12
Personnel	48.12	7.14	47.66	6.93	47.44	7.20	49.57	7.59
Public Investments	10.19	4.86	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	58.43	8.16	57.70	8.20	60.63	8.62
Health & sanitation	24.14	5.27	24.10	5.48	23.94	5.57	22.69	5.23
Education & culture	31.50	8.26	30.67	7.91	30.08	7.62	34.60	8.35
Social welfare	3.81	1.71	3.66	1.67	3.68	1.69	3.34	1.55
Housing	9.01	4.76	9.20	5.04	9.29	5.13	10.60	5.07
Transportation	3.62	4.33	3.71	4.40	3.72	4.45	1.30	1.88
Other	27.92	7.42	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	1,035.32	124.28
% Urban	62.45	23.64	65.09	24.76	65.42	25.20	65.47	25.54
% White	51.13	24.95	54.34	25.01	53.09	25.45	39.70	21.79
log(Earnings)	602.34	55.64	614.01	53.51	614.91	51.40	601.47	54.08
% Higher education	2.42	2.18	2.66	2.32	2.65	2.34	2.36	2.50
% Illiterate	41.46	12.15	39.20	11.57	38.76	10.98	42.09	12.72
Labor force participation	54.92	8.45	55.94	8.09	56.10	8.05	55.08	6.58
North	0.08	0.27	0.10	0.30	0.11	0.31	0.00	0.07
Northeast	0.32	0.47	0.22	0.42	0.22	0.41	0.53	0.50
South	0.20	0.40	0.23	0.42	0.23	0.42	0.06	0.23
Southeast	0.33	0.47	0.38	0.48	0.34	0.48	0.41	0.49
Midwest	0.07	0.25	0.07	0.26	0.10	0.30	0.00	0.00
Number of obs.	9679		5418		3073		893	

Table 2: 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

Standard errors clustered by municipality. Estimates using the baseline RD specification (equation 2) use the bias-corrected procedure of Calonico et al. (2014) and controls for city-year fixed effects.

Table 3: RD estimates of the effect of a left-wing mayor

Outcome	(1)	(2)	(3)	(4)
Size of government: overall revenues and expenses				
Expenditure per capita	0.67 (4.70)	-0.21 (0.53)	0.21 (0.67)	-0.37 (0.73)
Expenditure, % of GDP	-0.12 (0.80)	-0.02 (0.21)	-0.46 (0.72)	0.01 (0.18)
Revenue per capita	1.91 (4.86)	0.16 (0.49)	0.31 (0.69)	0.95 (0.94)
Revenue, % of GDP	0.21 (0.88)	0.09 (0.23)	-0.38 (0.74)	0.25 (0.21)
Allocation of resources: budget categories (% of total expenditure)				
Current Expenditure	-0.23 (0.32)	-0.03 (0.18)	-0.21 (0.23)	-0.24 (0.29)
Public Investment	0.17 (0.31)	0.05 (0.18)	0.24 (0.24)	0.20 (0.29)
Personnel	-0.09 (0.50)	0.09 (0.20)	-0.08 (0.24)	-0.11 (0.28)
Allocation of resources: functional categories (% of total expenditure)				
Social Expenditures	0.08 (0.62)	0.60*** (0.20)	0.91*** (0.29)	0.95*** (0.32)
of which:				
Health & sanitation	-0.10 (0.34)	0.21 (0.14)	0.44** (0.21)	0.58*** (0.20)
Education & culture	0.07 (0.61)	0.19 (0.15)	0.14 (0.19)	0.25 (0.20)
Social welfare	0.08 (0.12)	0.09 (0.05)	0.12 (0.07)	0.09 (0.06)
Other expenditures	-0.08 (0.62)	-0.60*** (0.20)	-0.91*** (0.29)	-0.95*** (0.32)
of which:				
Housing	-0.51 (0.34)	-0.18 (0.13)	0.01 (0.16)	-0.31 (0.20)
Transportation	0.39 (0.32)	-0.15* (0.09)	-0.17 (0.12)	-0.11 (0.13)
Other	0.07 (0.53)	-0.15 (0.20)	-0.53* (0.29)	-0.54 (0.36)
Observations (all)	9679	9679	9679	9679
Observations (effective)	4944	4898	4776	4804
Average over 4-year term	✓	✓		
Third year only			✓	
Fourth year only				✓
Year and city F.E.		✓	✓	✓

Robust bias-corrected standard errors clustered by municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: RD estimates of the effect of a left-wing mayor in cities facing low Tiebout Competition (Herfindahl index above median)

Outcome	(1)	(2)	(3)	(4)
Size of government: overall revenues and expenses				
Expenditure per capita	-6.60 (6.25)	-1.19* (0.70)	-0.39 (0.93)	-0.73 (0.90)
Expenditure, % of GDP	-0.60 (0.99)	-0.16 (0.33)	-0.77 (1.21)	-0.15 (0.23)
Revenue per capita	-5.48 (6.21)	-0.41 (0.74)	0.17 (1.13)	-0.15 (1.22)
Revenue, % of GDP	-0.17 (1.12)	0.06 (0.37)	-0.22 (1.32)	0.09 (0.27)
Allocation of resources: budget categories (% of total expenditure)				
Current Expenditure	-0.34 (0.40)	-0.02 (0.23)	-0.38 (0.32)	0.04 (0.36)
Public Investment	0.22 (0.38)	0.11 (0.20)	0.35 (0.30)	0.00 (0.32)
Personnel	-0.39 (0.55)	0.06 (0.23)	-0.14 (0.30)	0.00 (0.35)
Allocation of resources: functional categories (% of total expenditure)				
Social Expenditures	0.14 (0.77)	0.92*** (0.27)	1.05*** (0.39)	1.38*** (0.43)
of which:				
Health & sanitation	0.49 (0.46)	0.61*** (0.21)	0.67** (0.26)	0.85*** (0.27)
Education & culture	-0.45 (0.73)	-0.00 (0.19)	-0.04 (0.27)	0.34 (0.25)
Social welfare	0.05 (0.15)	0.18*** (0.07)	0.26*** (0.10)	0.15** (0.08)
Other expenditures	-0.14 (0.77)	-0.92*** (0.27)	-1.05*** (0.39)	-1.38*** (0.43)
of which:				
Housing	0.04 (0.50)	-0.05 (0.18)	-0.04 (0.26)	-0.18 (0.28)
Transportation	0.23 (0.44)	-0.18 (0.12)	-0.23 (0.19)	-0.15 (0.18)
Other	-0.29 (0.66)	-0.68** (0.29)	-0.39 (0.35)	-0.94** (0.41)
Observations (all)	5418	5418	5418	5418
Observations (effective)	3075	3173	2933	3217
Average over 4-year term	✓	✓		
Third year only			✓	
Fourth year only				✓
Year and city F.E.		✓	✓	✓

Robust bias-corrected standard errors clustered by municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Tiebout competition measured by the Herfindahl index of the adult population (at least 16 years old) in each city within a commuting zone.

Table 5: RD estimates of the effect of a left-wing mayor in cities facing low Tiebout Competition (Herfindahl index above 75th percentile)

Outcome	(1)	(2)	(3)	(4)
Size of government: overall revenues and expenses				
Expenditure per capita	-7.65 (8.36)	-0.91 (0.90)	-0.74 (1.35)	-0.55 (1.24)
Expenditure, % of GDP	-0.24 (1.41)	-0.56 (0.51)	-1.63 (1.94)	-0.32 (0.28)
Revenue per capita	-5.30 (8.28)	0.38 (1.10)	1.97 (1.96)	-0.68 (1.61)
Revenue, % of GDP	0.61 (1.61)	-0.25 (0.57)	-1.23 (2.40)	-0.36 (0.34)
Allocation of resources: budget categories (% of total expenditure)				
Current Expenditure	0.26 (0.48)	0.33 (0.34)	-0.05 (0.46)	0.29 (0.45)
Public Investment	-0.59 (0.53)	-0.29 (0.34)	0.08 (0.46)	-0.36 (0.48)
Personnel	0.31 (0.84)	0.09 (0.33)	-0.04 (0.43)	0.00 (0.48)
Allocation of resources: functional categories (% of total expenditure)				
Social Expenditures	-0.80 (1.04)	0.45 (0.35)	0.65 (0.50)	0.53 (0.45)
of which:				
Health & sanitation	-0.10 (0.61)	0.49* (0.27)	0.73** (0.37)	0.60* (0.34)
Education & culture	-0.78 (0.98)	-0.20 (0.30)	-0.22 (0.39)	-0.21 (0.35)
Social welfare	0.04 (0.21)	0.12 (0.09)	0.16 (0.13)	0.17* (0.10)
Other expenditures	0.80 (1.04)	-0.45 (0.35)	-0.65 (0.50)	-0.53 (0.45)
of which:				
Housing	-0.63 (0.64)	0.02 (0.25)	0.12 (0.36)	-0.07 (0.37)
Transportation	0.37 (0.60)	-0.11 (0.16)	0.04 (0.28)	-0.22 (0.21)
Other	1.11 (0.90)	-0.33 (0.36)	-0.67 (0.47)	-0.42 (0.54)
Observations (all)	3073	3073	3073	3073
Observations (effective)	1717	2129	1703	1863
Average over 4-year term	✓	✓		
Third year only			✓	
Fourth year only				✓
Year and city F.E.		✓	✓	✓

Robust bias-corrected standard errors clustered by municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Tiebout competition measured by the Herfindahl index of the adult population (at least 16 years old) in each city within a commuting zone.

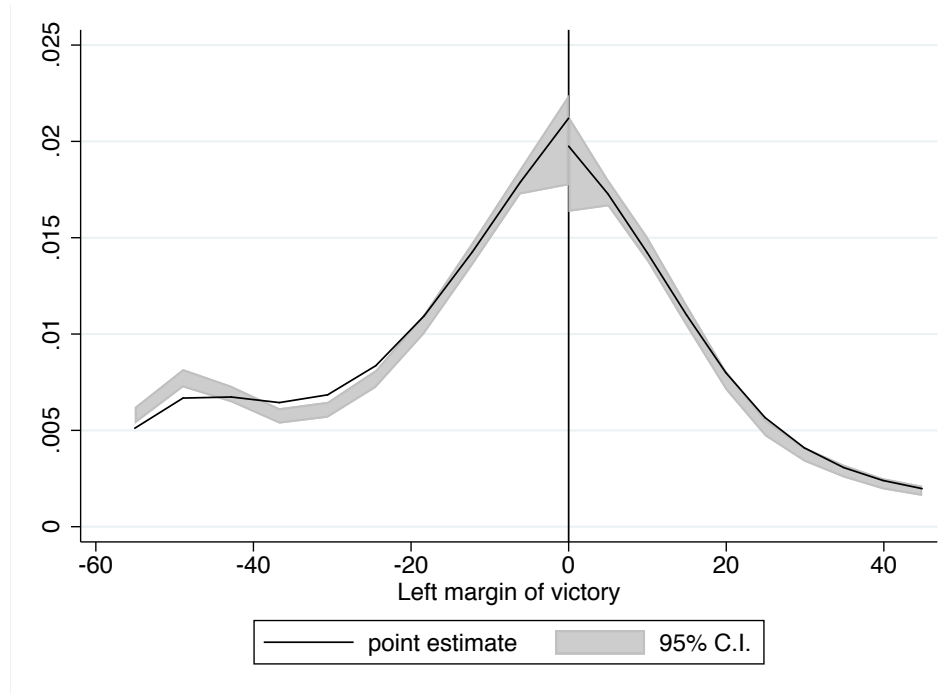
Table 6: RD estimates of the effect of a left-wing mayor in cities that received oil-windfall revenues

Outcome	(1)	(2)	(3)	(4)
Size of government: overall revenues and expenses				
Expenditure per capita	3.92 (14.84)	2.52 (2.11)	3.58 (2.91)	3.42 (2.74)
Expenditure, % of GDP	-1.14 (2.13)	-0.61 (0.46)	-0.84 (0.54)	-0.68 (0.73)
Revenue per capita	3.56 (14.20)	1.87 (2.05)	3.61 (2.25)	-0.22 (2.80)
Revenue, % of GDP	-1.11 (2.35)	-0.74 (0.47)	-0.82 (0.58)	-1.43* (0.79)
Allocation of resources: budget categories (% of total expenditure)				
Current Expenditure	-0.77 (1.00)	0.33 (0.60)	-0.84 (0.86)	1.22 (0.96)
Public Investment	0.55 (0.99)	-0.39 (0.61)	0.73 (0.83)	-1.27 (0.95)
Personnel	0.07 (1.70)	1.44* (0.79)	0.41 (1.09)	2.47** (1.11)
Allocation of resources: functional categories (% of total expenditure)				
Social Expenditures	3.58* (2.08)	2.36*** (0.91)	2.26** (0.98)	4.38** (2.06)
of which:				
Health & sanitation	0.54 (1.18)	1.17** (0.56)	1.61** (0.70)	1.58* (0.86)
Education & culture	2.60 (1.79)	1.10** (0.54)	0.70 (0.54)	2.17* (1.25)
Social welfare	0.46 (0.38)	0.07 (0.19)	0.02 (0.28)	0.26 (0.23)
Other expenditures	-3.58* (2.08)	-2.36*** (0.91)	-2.26** (0.98)	-4.38** (2.06)
of which:				
Housing	0.16 (1.10)	-0.14 (0.51)	-0.42 (0.74)	-0.03 (0.72)
Transportation	0.31 (0.37)	-0.47** (0.20)	-0.27 (0.28)	-0.41 (0.28)
Other	-3.99* (2.30)	-1.51 (1.04)	-1.53 (1.07)	-3.72 (2.29)
Observations (all)	893	893	893	893
Observations (effective)	486	484	464	453
Average over 4-year term	✓	✓		
Third year only			✓	
Fourth year only				✓
Year and city F.E.		✓	✓	✓

Robust bias-corrected standard errors clustered by municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figures

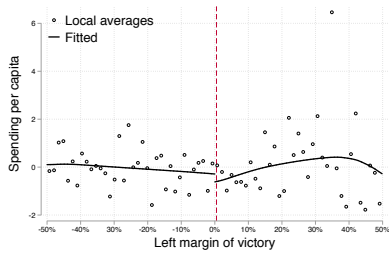
Figure 1: Tests of manipulation of the left margin of victory



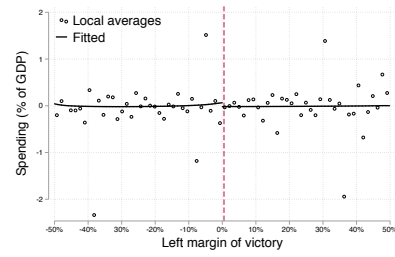
Notes: The figure present visual evidence for the [Cattaneo et al. \(2018\)](#) manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the running variable at the cutoff. T-stat = -0.68; P-value = 0.49.

Figure 2: Local fiscal policy indicators - baseline sample

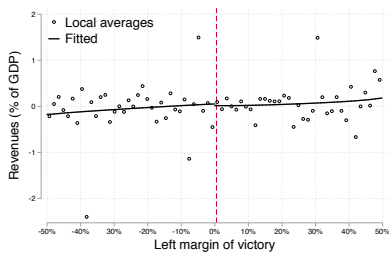
Size of government



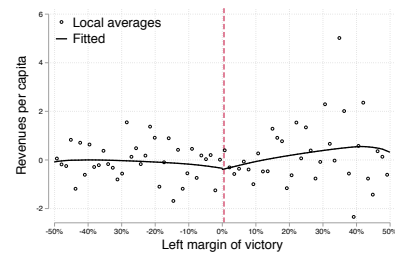
(a) Spending per capita



(b) Spending (% of GDP)

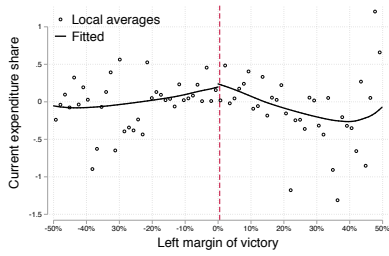


(c) Revenues (% of GDP)

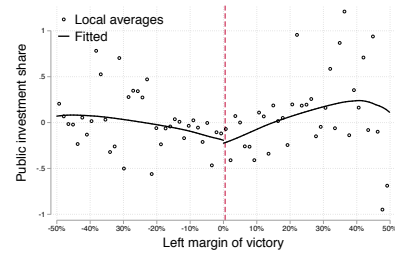


(d) Revenues per capita

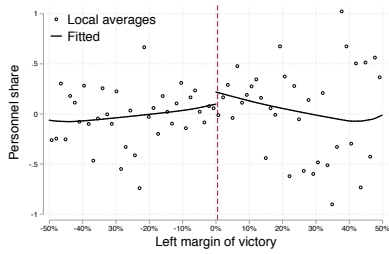
Composition (spending shares)



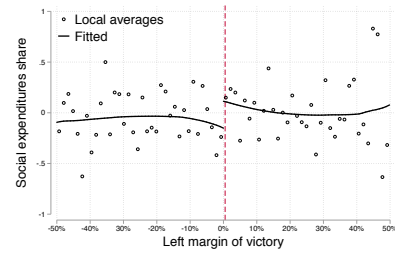
(e) Current expenditures



(f) Investment



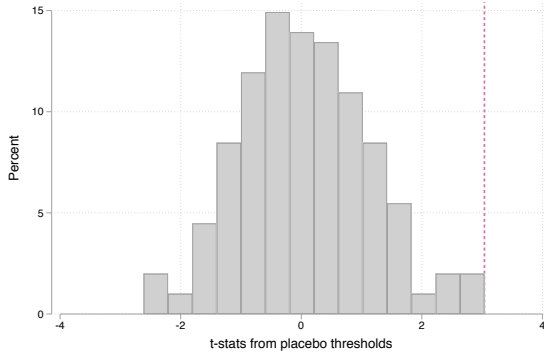
(g) Personnel



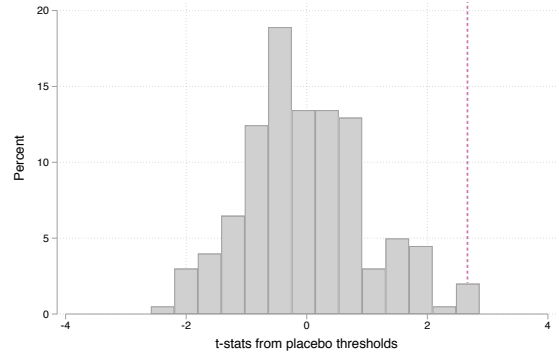
(h) Social expenditures

Notes: Effect of the (close) election of a left-wing mayor. RD estimates (equation 2) using the bias-corrected procedure of Calonico et al. (2014). Outcomes are 4-year term averages. Specification controls for city-year fixed effects. Visual presentation of estimates from column 2 of table 3.

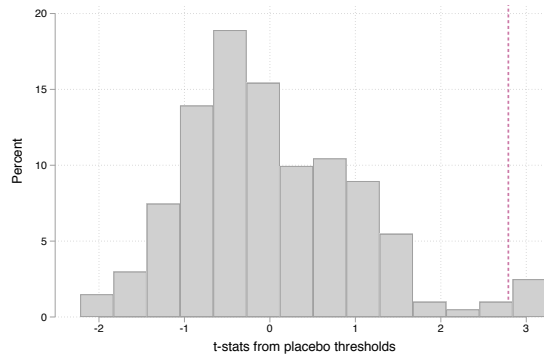
Figure 3: Falsification test using placebo thresholds



(a) Social Expenditure: 4-year average



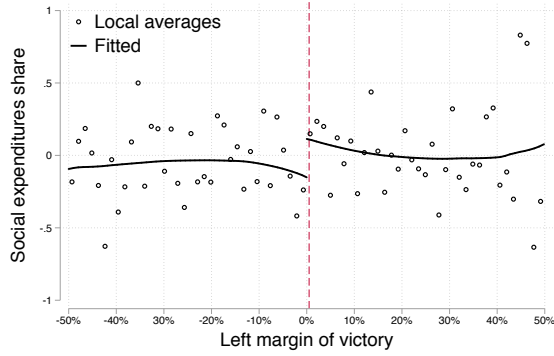
(b) Social Expenditure: 3rd year



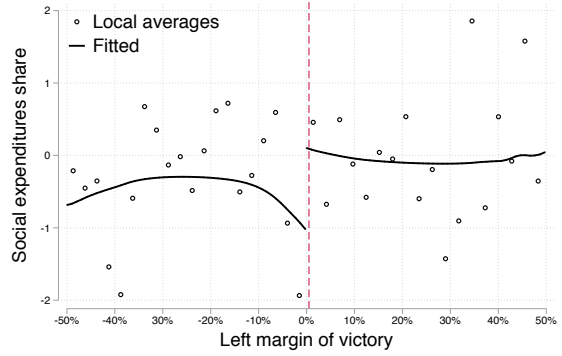
(c) Social Expenditure: 4th year

Notes: Empirical distribution of t-statistics from RD estimates (equation 2) based on 200 randomly draw placebo thresholds, drawn separately on the left and on the right side of the true threshold (a 100 on each side) using only observations belonging to that side with at least 25 observations in each side of the bandwidth. Vertical lines represent the t-statistics obtained using the true threshold. The t-statistics are from the robust bias-corrected procedure of [Calonico et al. \(2014\)](#).

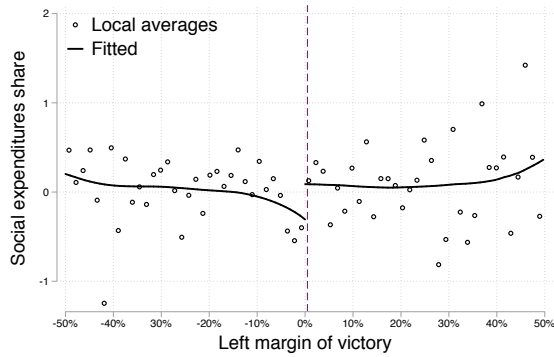
Figure 4: Social Expenditure effects in different samples



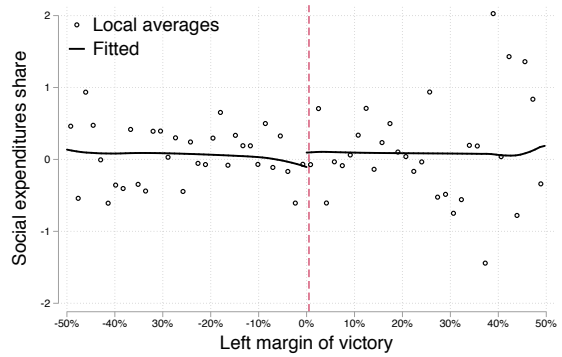
(a) Baseline



(b) Oil-windfall receivers



(c) Tiebout competition - median



(d) Tiebout competition - 25th perc.

Notes: Effect of the (close) election of a left-wing mayor. RD estimates (equation 2) using the bias-corrected procedure of [Calonico et al. \(2014\)](#). Outcomes are 4-year term averages. Specification controls for city-year fixed effects. Visual presentation of estimates from columns 2 of tables 3 to 6.

A Partisanship classification

This appendix provides information on how we assign party ideology. 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 34 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.

To calculate the left margin of victory, we classify all 35 parties that participated in the municipal elections of 2004, 2008 and 2012 into left or non-left (centrist, right or neither). We, then, take the difference in vote shares of the most-voted left candidate and the most-voted non-left candidate. We proceed in three steps.

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.¹¹

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 Sao 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 7 reports the final classification with the respective source from which the party ideology was assigned.

¹¹Even 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 7: Party classification

Leftist parties		Non-leftist parties	
Party	Source	Party	Source
PV	PZ	DEM(PFL)	PZ
PT	PZ	MDB(PMDB)	PZ
PSOL	PZ	PP	PZ
PSB	PZ	PR	PZ
PPS	PZ	PRB	PZ
PDT	PZ	PSDB	PZ
PcdoB	PZ	PSL	PZ
PPL	FS	PTB	PZ
PSTU	BG	PRONA	BG
PMN	BG	PRP	BG
PCO	BG	PRTB	BG
PCB	BG	PSC	BG
		PSDC	BG
		DC	BG
		PTN	BG
		PODEMOS	BG
		PTdoB	
		PAN	
		PHS	
		PL	
		PSD	
		PTC	
		PEN	

Notes: PZ (Zucco and Power, 2018); BG (Baker and Greene, 2011); FS(Foro de So Paulo).