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

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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. We find a modest but robust positive effect of approximately 0.6 percentage points on the social expenditures share, which translates in a small (approximately 1 percent) increase in social expenditure per capita. The impact of left-wing mayors on social spending is stronger for lame-duck mayors and in cities receiving oil windfalls. These results suggest that Brazilian parties attempt to shape the allocation of municipal resources to favor their respective electoral bases but their ability to do so is severely limited by factors such as institutional constraints and re-election concerns.

Keywords: local fiscal policy; political economy; partisanship; Brazilian cities; regression-discontinuity design.

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1 Introduction

Do political parties matter when it comes to governing cities? Despite competitive pressures for platform convergence (Downs, 1957), policy differences can emerge when parties are ideologically motivated and represent different constituencies (Alesina, 1988). While these broad theoretical considerations apply to all government levels, municipalities present some specificities. Cities tend to be subject to more intense fiscal competition, stronger spatial sorting and tighter financial constraints, relative to higher levels of government (Ferreira and Gyourko, 2009, pp. 401-403). Moreover, the set of issues that are relevant for city governments is generally of a different nature and possibly less subject to partisan divide. It is therefore an open question whether the substantial degree of policy divergence that is often observed at the national and regional level may be observed in municipal governments as well. 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 nations (Pettersson-Lidbom, 2008; Fiva et al., 2018), while evidence from the US is mixed (de Benedictis-Kessner and Warshaw, 2016; Gerber and Hopkins, 2011; Ferreira and Gyourko, 2009).

In the context of developing economies and young democracies, theoretical predictions are even less clear-cut, and empirical evidence is lacking. A widespread view holds that local politics in these contexts tends to be dominated by patronage and personalistic attitudes, while weak parties seldom develop distinctive policy platforms and programmatic linkages to voters.[1]

This paper studies partisan effects in Brazilian cities. We estimate the effect of electing a left-wing mayor on municipal fiscal policy, using a regression-discontinuity design. We then test the role of re-election concerns, Tiebout competition, institutional constraints and the ideological composition of mayoral coalitions in determining the degree of policy divergence. We focus on mayors, rather than city councils, because in Brazilian cities the executive branch has a dominant role in crafting, approving and executing the municipal budget.

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

[1] We will discuss these issues and how they relate to our results in Sections 7 and 8.

spending, investment and personnel). We do find a modest but 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 four 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 (1995a)). The second 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). The third is institutional rigidities regarding the tax revenue system and the allocation of public expenditures. The fourth is the pre-electoral coalition-building process, which could lead to internally heterogeneous (and therefore ideologically ambiguous) multi-party coalitions in support of mayoral candidates.[2]

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. 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. 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. Finally, to test whether internally heterogeneous mayoral coalitions drive policy convergence, we test whether the impact of partisanship covaries with the ideological distance between the winning coalition and its opposition.

Our results suggest that none of these mechanisms explain the lack of partisan effects on the

[2] We thank an anonymous referee for suggesting this fourth hypothesis.

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, forthcoming) 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 2.2 percentage points, a more than threefold increase compared to the baseline results. We also find a larger effect on the share of social expenditures among 'lame-duck' mayors (around 1.3 percentage points versus 0.6 in the baseline). We find little support for explanations based on Tiebout-competition or ideologically heterogeneous coalitions.

The budget composition effects we find translate into changes in social expenditures per capita. The close election of a left-wing mayor increases social expenditures per capita by around 1 percent in the baseline sample, by around 3 percent in cities with a 'lame-duck' mayor and by more than 6 percent in cities experiencing oil windfalls.

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 in high-income countries. 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), while the evidence is mixed for Democrat (as opposed to Republican) mayors in US cities. Little evidence has been available so far on developing countries.[3]

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

[3]Most previous work on partisan effects on local policy in developing countries lacks a clear identification strategy, as the one provided by a RDD. It is hard, therefore, to discern causal partisan impacts from selection effects in these previous works. For Brazil, there are a few studies using panel regression with party dummies to study the relation between partisanship and fiscal policy at the local level (Sakurai, 2009; Sakurai and Menezes-filho, 2011; Sakurai and Gremaud, 2007).

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.

Studies of US cities provide a nuanced picture. Overall, they appear to point to null or very limited effects in small and medium-sized municipalities, but more substantial impacts in large cities. Specifically, Ferreira and Gyourko (2009) find no partisan differences in policy outcomes between Democrat and Republican mayors. They investigate possible explanations, and find most support for Tiebout-competition among municipalities within metropolitan areas. Similarly, Gerber and Hopkins (2011) find no major effects on policy outcomes in areas characterized by shared authority among different levels of government. They find, however, that Democrat mayors spend a smaller share of their budget on public safety, an area where mayors have higher sway. Differently from the previous two studies and focusing on larger cities (with more than 75,000 inhabitants), de Benedictis-Kessner and Warshaw (2016) find that electing a Democrat mayor leads to higher expenditures, which are financed by increased indebtedness, with no changes in revenues. These studies of US cities are the closest to our paper, in the sense of studying a majoritarian system in which a directly-elected mayor is the head of the city government.[4]

Ours 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. Moreover, we provide novel evidence on the important role of institutional constraints and re-election concerns in limiting policy divergence.

[4] A broader literature has studied partisan effects at the regional and national level on various outcomes. For example, Lee et al. (2004) use close US congressional elections to show that voters merely elect (rather than affecting) candidates' 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 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. Two recent studies have focused on US counties, finding relevant partisan effects on their fiscal policy, with Democratic legislators spending more (de Benedictis-Kessner and Warshaw, 2020), but no effect of sheriffs' partisanship on their law enforcement behavior (Thompson, 2020). Other studies have looked at the effect of partisan victories in national elections on financial markets (eg Snowberg et al. (2007); Girardi (2020)).

2 Institutional and Political Context

2.1 Institutional framework

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 years 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-terms limit.

Importantly for our research design, 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. The annual budget follows the civil calendar and must be approved before a new year begins, *i.e.* the budget for year t is approved in year $t-1$. This implies that mayors who are in their first term start their mandate with a budget crafted by the previous administration (Blöndal et al., 2003; Alston et al., 2005; Albuquerque et al., 2013).

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, primary and middle school), 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,

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.

2.2 Political parties and social cleavages

Brazil is a case of multipartism, with 33 registered and roughly 14 effective parliamentary parties in 2016 (Nicolau, 2017; Gallagher and Mitchell, 2008).[5] Four parties, however, have played a major role in the period under study, both at the national and local level. 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 MDB/PMDB are the main center-right parties, while DEM/PFL is the most important party on the right (Zucco and Power, 2009, forthcoming).[6]

Despite high fragmentation, the left-right divide is rather clear and highly relevant in Brazilian politics. For example, Zucco and Power (forthcoming) find that the optimal number of clusters for classifying federal legislators along several ideological dimensions is two: a left-wing and a conservative camp. In other words, despite the large number of parties, a binary left-right classification is able to capture most ideological variation in Brazilian politics.

Using various measures of polarization, Zucco and Power (forthcoming, p.18) also show that differences in policy preferences between left-wing and conservative legislators have been relevant and broadly stable or mildly increasing in our sample period (after a marked decrease in polarization between 1990 and 2002). Although Zucco and Power (forthcoming) survey federal legislators, and therefore their results do not necessarily apply identically to local politicians, it is worth noting that around 37% of the national legislators in their sample is a former mayor or vice mayor.

There is also evidence that left-wing and conservative parties represent different constituencies and that, at least since the early 2000s, lower income Brazilians constitute the left's electoral base.

[5] 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).

[6] Appendix A provides the full list of parties that participated in the municipal elections we study.

In his recent historical comparative analysis of national political, ideological and economic regimes, Thomas Piketty summarizes and interprets the Brazilian evidence as revealing "a *classist* party system emerging in the period 1989-2018 with important consequences for redistribution"(Piketty, 2020, p.953, our emphasis). Specifically, Gethin and Morgan (2018) show that throughout all our sample period (2004-2016), Brazilian lower income classes were substantially more likely than economic elites to vote for left-wing parties in national elections. For example, in the 2014 presidential election, the poorest 50% (in terms of income) were more likely to vote left than the richest 10% by 23 percentage points. This pattern is visible at the regional as well as the individual level, with poorer regions (particularly the Northeast) increasingly voting left, and wealthier ones (in particular the South) increasingly leaning conservative (Zucco, 2008; Gethin and Morgan, 2018). These class cleavages appear to be strongly linked to welfare policies directed to poor households that left parties (and the PT in particular) promoted at the national level (Gethin and Morgan, 2018; Zucco and Power, 2013; Zucco, 2008).[7]

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 8,943 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 for two reasons. First, data for previous elections in the main TSE statistical repository is incomplete.[8] Second, the fiscal outcome variables are not fully comparable in the pre-2002 period. Despite these challenges, in Appendix G we extend our sample to include the 1996 and 2000 elections by downloading electoral results from an old TSE repository and using an alternative definition of our outcome variables that allows consistency over

[7] The electoral rise of the leftist PT in the poorer Northeast region has also been linked to an intentional investment in local organizational facilities in the region (including the network of local branches), especially in regard to local elections (Van Dyck, 2014).

[8] This is clear from basic inspection of the data and is also stated in the TSE website (accessed on Sep 2020).

time.[9]

From TSE, we obtain information on the candidate's 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 (forthcoming) classification to determine the ideological stance of parties (left or non-left). When a party is not included in Zucco and Power (forthcoming), we use other sources to assign party ideology. The partisanship 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 expenditures per capita and as a share of GDP as our measures of government size. Variables expressed in per-capita terms are measured in constant 2016 Reais using the GDP deflator.

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.[10]

[9] The old TSE repository has all the key electoral variables for our study, but it is less precise than the main repository because it does not have detailed information about the status of the election or the candidates. See Appendix G for details. Both repositories were accessed in September 2020.

[10] Pension expenditure is not part of social spending, and is included in the residual category. Unlike public pensions paid by national governments, that constitute a form of social protection directed to the population, pension spending by municipal governments just includes pensions paid to former municipal employees. It is therefore best interpreted as deferred personnel compensation, rather than a form of social spending. It should also be noted that pension expenditures are mostly pre-determined for the current mayor, as they reflect past hiring and wage-setting decisions by previous administrations. Appendix G shows that main results are qualitatively similar when including pensions

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.

Data on federal transfers received through congress amendments to the federal budget, which will be used in assessing our research design, come from *SIGA-Brasil*, a website of the Brazilian Senate containing detailed information on the federal budget.

3.3 Municipal characteristics

We supplement our data with municipal characteristics obtained from Brazil's National Beareau of Statistics - *Instituto Brasileiro de Geografia e Estatística* (IBGE). 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 – median earnings, urbanization rate, race, labor force participation and education – come from the 2000 and 2010 Census. We also use information at the city level on the cash-transfer program Bolsa Família, obtained from *Ministério da Cidadania*.

3.4 Sample selection and descriptive statistics

We take a number of steps to create our baseline sample. We start with all 16,692 municipal electoral results available in the TSE repository. We exclude 256 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 9,944 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 only keep observations for which we can observe all fiscal policy variables over the full term. As a result, our baseline sample has 8,943 observations, where an observation is a municipality-election cycle.

Table 1 reports descriptive statistics for all our outcome variables, in our baseline 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 are representative of our baseline sample. The same happens with all other city characteristics, except geographic location, as reported in Appendix Table B.1.

paid to former employees into the social expenditure category.

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 precisely, 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 local average effect of a left-wing mayor on fiscal policy variable y, is given by

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

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.[11] This means that cities where the left candidate barely wins an election do not tend to be very different from cities where the left barely loses. Under this 'smoothness' assumption, our RD estimator identifies the average causal impact of a (closely-elected) left mayor on fiscal policy variables.

We estimate β through the following RD specification:

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

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; $f(.)$ 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.[12] We use

[11] More precisely, counterfactual outcomes are assumed to be continuous in the running variable.

[12] We control for city and year fixed effects by first regressing y_{it} on city and year dummies, and then using residuals

Table 1: Descriptive Statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	781.50	783.86	784.30	779.11	778.17
	(38.71)	(39.72)	(39.58)	(38.22)	(42.83)
Expenditure, % GDP	19.31	19.94	16.82	18.48	19.16
	(13.12)	(17.02)	(13.77)	(10.88)	(10.16)
Revenue per capita	791.85	794.16	794.35	789.22	787.04
	(40.09)	(41.19)	(40.67)	(39.88)	(42.42)
Revenue, % GDP	21.36	22.07	18.57	20.43	20.98
	(13.89)	(18.08)	(14.68)	(12.03)	(11.24)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	88.20	87.44	88.01	88.40	88.56
	(4.89)	(5.17)	(5.00)	(4.79)	(5.47)
Personnel	48.12	47.54	48.10	48.57	48.25
	(7.16)	(7.15)	(6.96)	(7.07)	(7.19)
Public Investments	10.20	10.94	10.38	9.97	9.58
	(4.85)	(5.14)	(4.95)	(4.68)	(5.56)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	59.56	59.24	59.25	59.88	59.66
	(8.49)	(8.57)	(7.99)	(8.67)	(8.06)
Health & sanitation	24.14	23.91	24.36	24.26	22.54
	(5.32)	(5.17)	(5.58)	(5.62)	(5.03)
Education & culture	31.64	31.52	31.20	31.88	33.57
	(8.32)	(8.31)	(8.07)	(8.39)	(7.58)
Social welfare	3.79	3.81	3.69	3.74	3.55
	(1.69)	(1.73)	(1.63)	(1.67)	(1.75)
Housing	8.98	9.43	9.55	8.86	10.87
	(4.79)	(4.94)	(5.06)	(4.84)	(5.11)
Transportation	3.55	3.44	3.20	3.28	1.44
	(4.33)	(4.26)	(3.92)	(4.19)	(2.23)
Other	27.90	27.90	28.01	27.98	28.03
	(7.51)	(7.77)	(7.34)	(7.86)	(8.01)
Left candidate margin	-10.05	-11.57	-9.43	-7.06	-12.13
	(27.67)	(31.13)	(28.57)	(24.29)	(31.82)
Observations	8943	2395	4158	3105	919

Notes: This table reports mean and standard deviation (in parenthesis) for the outcome variables and the left candidate margin of victory. Outcome variables are from FINBRA-STN and the margin of victory computed from the TSE electoral results. See Section 6 for the specific definition and motivation of each subsample. Summary statistics for covariates are in Appendix Table B.1.

the Calonico et al. (2014) robust and bias-corrected estimator.

4.2 Design assessment

To assess the validity of our RD design, we test for discontinuities in pre-determined covariates at the threshold. Table 2 displays differences in pre-determined city characteristics between cities with left and non-left mayors. The first column includes all elections, showing that in general cities electing a leftist mayor are different: they are much larger in terms of population, more likely to be urban, in the northeast region, and have a lower share of white population. These cities have also larger median earnings, but, at the same time, receive more conditional cash transfers (Bolsa Familia).

Given that Brazilian deputies tend to use federal budget amendments to reward cities that were important to their election, which in turn affects local voting behavior (Firpo et al., 2015), we also consider transfers received through these amendments as a potentially relevant covariate. We do not find any significant difference among cities with left and non-left mayors in this respect.

Table 2 shows that any difference in pre-determined characteristics disappears if we restrict the comparison to progressively closer elections. Most importantly, column 5 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.

Appendix Figure C.1 shows that there is no sign of systematic electoral manipulation in favor of or against left candidates: we do not find any discontinuity in the distribution of the running variable at the cutoff (McCrary, 2008). Following Caughey and Sekhon (2011, p.392), in Appendix Figures C.2 and C.3 we test for a discontinuity in the vote share of the incumbent mayor or the incumbent party at the threshold, in order to test for possible electoral manipulation (or other forms of sorting) by incumbents, and find none.

In Appendix D, we also test for discontinuities in candidates' characteristics between bare win-

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. See Lee and Lemieux (2010, p. 331-333) for details on this two-steps procedure, including the demonstration that no correction for the first step is needed when calculating standard errors.

Table 2: Difference in municipality characteristics between left and non-left mayors, by left margin of victory

	All	+/- 40	+/- 10	+/- 5	baseline RD
Labor market and demographic covariates					
log(Median earnings) ×100	2.06	-0.41	0.44	-0.08	0.53
	(0.64)	(0.74)	(1.06)	(1.43)	(0.54)
Labor force participation	-0.16	-0.03	0.15	0.12	0.25
	(0.18)	(0.21)	(0.31)	(0.42)	(0.18)
log(Population) ×100	27.90	3.57	3.45	-0.36	-0.04
	(2.60)	(2.72)	(3.69)	(4.90)	(0.47)
% Urban	1.38	-0.30	-0.30	-0.83	-0.31
	(0.51)	(0.57)	(0.79)	(1.07)	(0.23)
% White	-2.75	-1.78	-1.28	-1.62	-0.42
	(0.52)	(0.60)	(0.88)	(1.20)	(0.23)
% Higher education	0.33	0.02	-0.01	-0.03	-0.02
	(0.07)	(0.07)	(0.10)	(0.14)	(0.05)
% Illiterate	-0.00	0.33	0.01	0.17	-0.02
	(0.23)	(0.26)	(0.38)	(0.53)	(0.11)
Geographic indicators					
North	-0.00	-0.01	-0.01	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Northeast	0.04	0.02	0.01	0.01	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)
South	-0.01	-0.01	-0.00	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Southeast	-0.00	0.01	-0.00	-0.03	-0.03
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Midwest	-0.03	-0.01	0.00	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Other covariates					
log(Bolsa Familia households) ×100	9.56	7.83	1.26	2.07	-0.35
	(1.81)	(2.21)	(3.25)	(4.44)	(1.90)
log(Bolsa Familia receipts) ×100	10.82	8.32	1.77	2.76	-0.59
	(1.96)	(2.36)	(3.46)	(4.72)	(1.97)
Authorized amendments	0.17	-0.11	-0.21	-0.04	-0.15
	(0.12)	(0.16)	(0.16)	()	(0.24)
Executed amendments	0.04	-0.14	-0.15	-0.03	-0.01
	(0.07)	(0.13)	(0.10)	(0.07)	(0.15)
Observations (all)	16427	7849	3400	1809	8943
Observations (effective)	16427	7849	3400	1809	4608

Notes: Standard errors clustered by municipality. Both the number of households receiving Bolsa Familia and Bolsa Familia receipts are normalized by population to take into account city size. Transfers received through amendments are expressed as a share of city revenues. 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.

ners and bare losers within both political camps, in the spirit of Caughey and Sekhon (2011). We do not find evidence of sorting based on incumbency, education level or personal wealth. When not including city and time fixed effects in our specification, there is some discontinuity in campaign expenditures, with winning candidates spending more than losing candidates also at the threshold. However, when controlling for fixed effects as we do in our main analysis, these discontinuities in campaign expenditures shrink in size, becoming very small.[13]

5 Main results: impact of partisanship on municipal fiscal policy

This section presents our main results, which are reported in the first column of Table 3 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 the main economic categories (current spending, investment and personnel). We do find a modest but precisely estimated positive effect on the social 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 3, column 1; Figure 1, panel (a)).

5.2 Budget composition

We now study how partisanship affects the allocation of municipal resources. First, we look at the composition of expenditures across the main budget categories. Again, we find no significant effects:

[13] Evidence of discontinuities in campaign expenditures, among several other variables, has been found for a sample of US House elections by Caughey and Sekhon (2011). To our knowledge, this a novel result in the context of Brazilian elections. While this type of discontinuity would pose obvious problems for analyses estimating incumbency advantage effects, it is much less clear how the ability of the candidate to raise funds would affect his fiscal policy choices once elected. In other words, ability to raise campaign funds is a natural covariate when the outcome is ability to win elections, but not when it is the allocation of the city budget. Moreover, besides becoming very small after controlling for fixed effects, these positive discontinuities in campaign expenditures are completely absent in the subsample of municipalities receiving oil windfalls, which are the ones where we find strongest fiscal policy effects. Furthermore, as argued in de la Cuesta and Imai (2016, p.384) and Eggers et al. (2015, pp.267-270) in reference to similar results for US House elections, this type of sorting would need to be implausibly precise in order to introduce significant bias.

there is no evidence of discontinuities in the shares of current spending, personnel and investment in total spending (middle panel of Table 3, column 1; Figure 1, panel (b)). In Appendix E, we also look at the composition of revenues, finding no significant effect on the relative shares of municipal taxes, state transfers and federal transfers.

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 (b)). 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 3, first column).

All the main types of social expenditures display small average increases in their shares, with the 'Education & Culture' category increasing slightly more than 'Health & Sanitation' and 'Social Welfare'. 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. Indeed, when looking at sub-categories within other expenditures (housing, transportation, and all others), we find negative effects on all of them, but imprecisely estimated. This means that all other expenditures are on average reduced in relative size to make room for the relative increase in social spending, and there is large variation in how the 'burden' is distributed among other expenditure categories.[14]

This reallocation translates into a small positive effect on the level of social spending per capita, which is larger by around 1 percent under a left-wing mayor (bottom panel of Table 3, first column).

In order to uncover dynamics, Appendix F reports results by year in office. The key finding is summarized in Figure 2: the effect on the social expenditures share increases gradually, reaching 1 percentage point in the last year in office, consistent with the idea that it takes time to reallocate resources. Effects on size of government and other budget categories are confirmed to be small and insignificant for all years in office (Appendix Table F.1).

To provide a broader view of how partisan effects have evolved over time after Brazil's democratization, Appendix G extends our sample period backwards to the 1996 and 2000 elections and plots

[14]The 'Social expenditures' and 'Other expenditures' categories are exhaustive and mutually exclusive: they sum up to total expenditures. Therefore, the share of other expenditures decreases one-for-one with every increase in the social expenditures share. The effect on the other expenditures share and its standard error are thus necessarily of identical magnitude but opposite sign than those on the social expenditures share – that is why we don't report them in our results tables, given that the information is already fully contained in the coefficient on the social expenditures share.

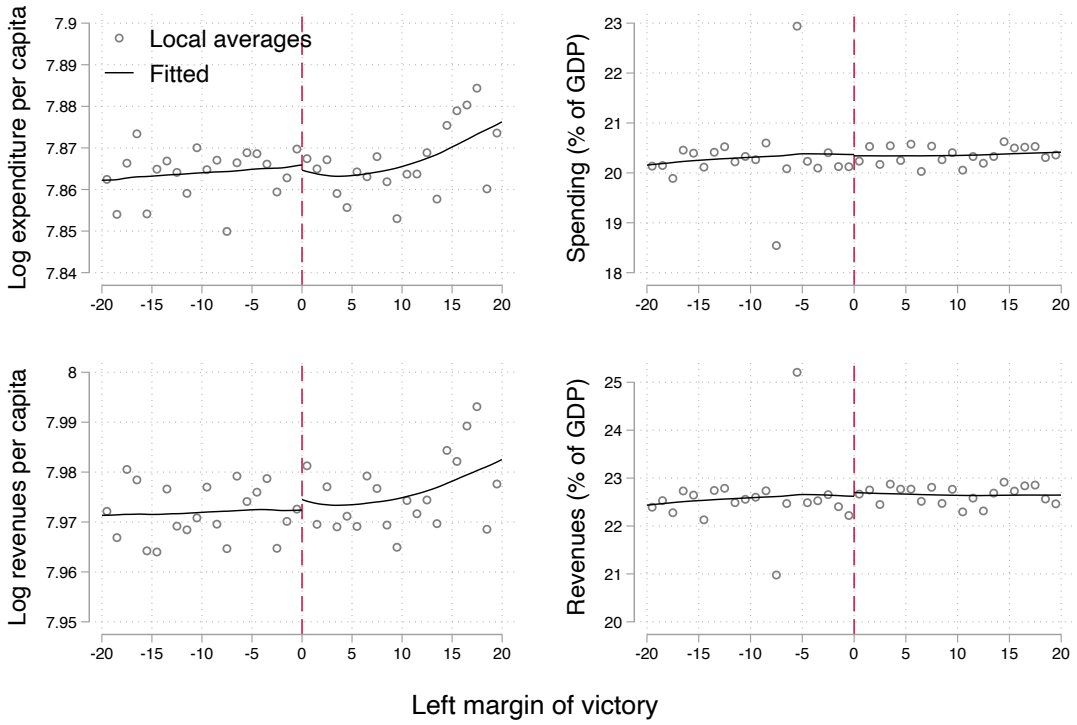
Table 3: RD estimates of the effect of a left-wing mayor

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of city government					
Expenditure per capita	-0.01	0.52	-1.24	0.23	2.26
	(0.56)	(1.27)	(0.85)	(0.82)	(2.06)
Expenditure, % of GDP	0.01	-1.05	-0.36	-0.02	-0.13
	(0.24)	(1.18)	(0.43)	(0.25)	(0.47)
Revenue per capita	0.39	0.59	-0.77	1.02	2.19
	(0.52)	(1.21)	(0.77)	(0.88)	(2.01)
Revenue, % of GDP	0.12	-1.14	-0.20	0.24	-0.21
	(0.25)	(1.20)	(0.42)	(0.27)	(0.49)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05	-0.39	-0.15	0.18	1.01
	(0.17)	(0.38)	(0.25)	(0.31)	(0.65)
Public Investment	0.09	0.40	0.23	-0.08	-0.96
	(0.16)	(0.37)	(0.26)	(0.31)	(0.63)
Personnel	-0.05	-0.81	-0.04	-0.08	-0.34
	(0.22)	(0.48)	(0.30)	(0.41)	(0.85)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64	1.27	0.71	0.91	2.19
	(0.21)	(0.40)	(0.30)	(0.39)	(0.87)
of which:					
Health & sanitation	0.18	0.69	0.38	-0.11	0.18
	(0.15)	(0.30)	(0.22)	(0.27)	(0.39)
Education & culture	0.24	0.31	0.12	1.00	0.84
	(0.16)	(0.30)	(0.19)	(0.30)	(0.49)
Social welfare	0.16	0.32	0.27	0.12	0.36
	(0.06)	(0.11)	(0.09)	(0.10)	(0.20)
Other Expenditures:					
Housing	-0.16	-0.36	0.05	-0.15	0.41
	(0.13)	(0.29)	(0.20)	(0.24)	(0.65)
Transportation	-0.18	-0.07	-0.24	-0.52	-0.73
	(0.09)	(0.20)	(0.13)	(0.19)	(0.29)
Other	-0.21	-0.86	-0.59	-0.22	-1.90
	(0.23)	(0.42)	(0.34)	(0.40)	(1.10)
Social Expenditures per capita	1.16	3.34	0.25	1.92	6.48
	(0.61)	(1.36)	(0.86)	(0.93)	(2.40)
Observations (all)	8943	2395	4158	3105	919
Observations (effective)	4408	1227	2367	1660	451

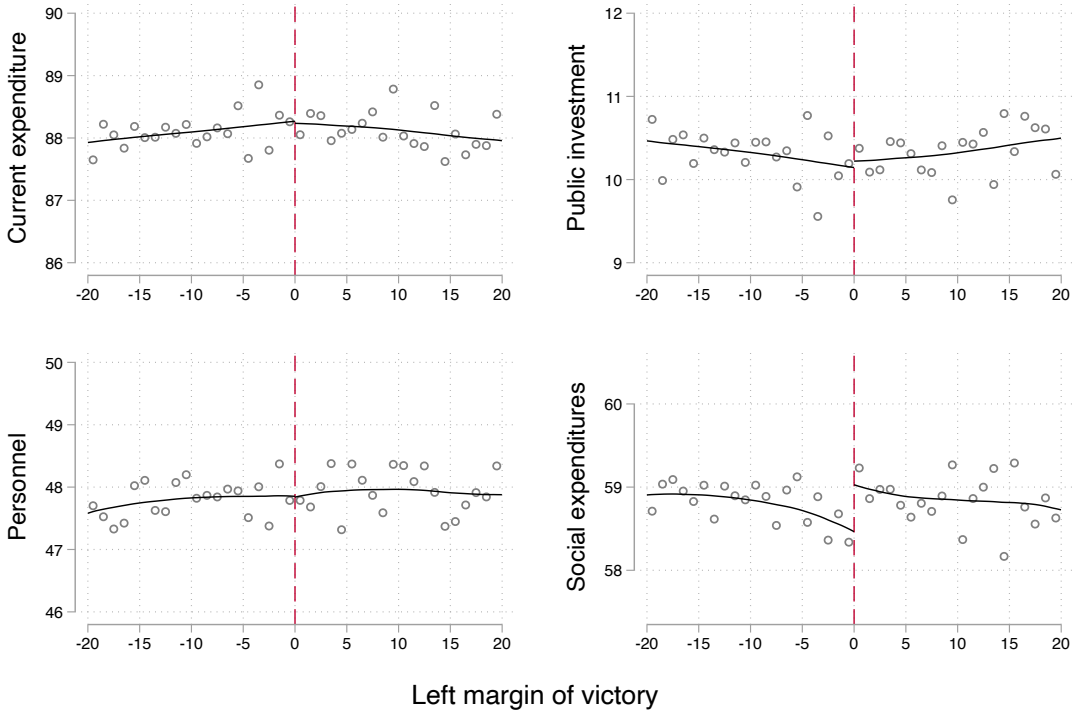
Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Figure 1: Local fiscal policy indicators - baseline (whole sample)

(a) Size of city government

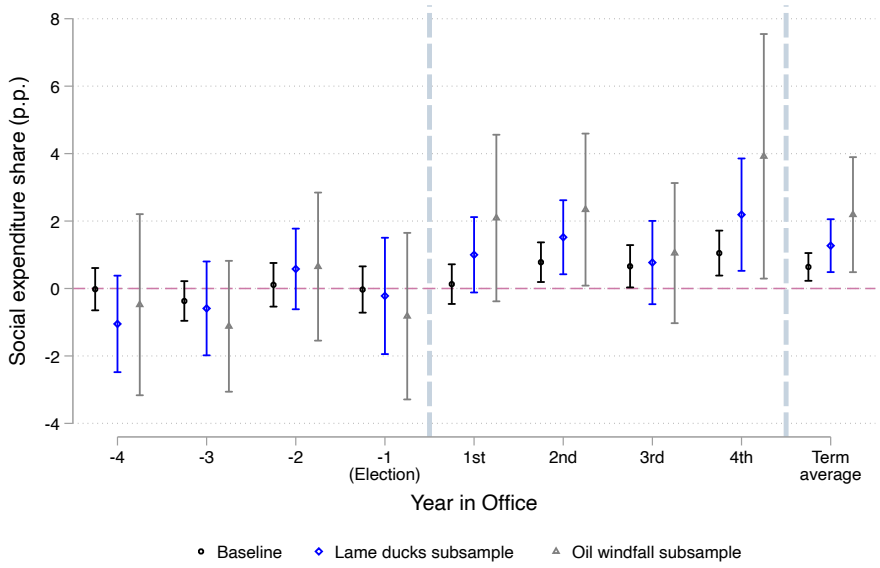


(b) Expenditures composition (shares)



Notes: Visual presentation of our RD estimates of the effect of a left-wing mayor, reported in column 1 of table 3 and based on the specification in equation 2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Per-capita variables are taken in logarithms. 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 the share of social spending, 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.

the effect of a left-wing mayor on the share of social spending by electoral cycle.[15] The effect on the share of social spending is positive in the entire time period, but appears much stronger in the 'boom years' 2004-2008. This appears consistent with the idea that left-wing mayors redistribute more when their financial constraints are relaxed, given that the 'boom years' were characterized by rising revenues due both to strong income growth and increasing oil royalties. We explore this idea in greater detail in the next section.

5.3 Robustness and falsification tests

Robustness tests Appendix H reports robustness tests. In the first column of Appendix Table H.1 we estimate effects on changes in fiscal outcomes rather than levels. In particular, for each outcome, we take the percentage-points difference between the election year ($t = -1$) and the fourth year of the term ($t = 4$). Our main results are qualitatively confirmed when using this approach.

[15] It is important to note that the fiscal and electoral data is not fully comparable pre-2004. For details, see Appendix G.

In Appendix Table H.2 we exclude the first year of the mandate from the term average. This test is informative because mayors have limited influence on the budget in the first year of their mandate, which was written by the previous administration (as discussed in Section 2.1). Results regarding the size of government and budget categories remain very small and not significant, while the effect on the share of social spending, as expected, becomes slightly larger (almost 0.8 percentage points).

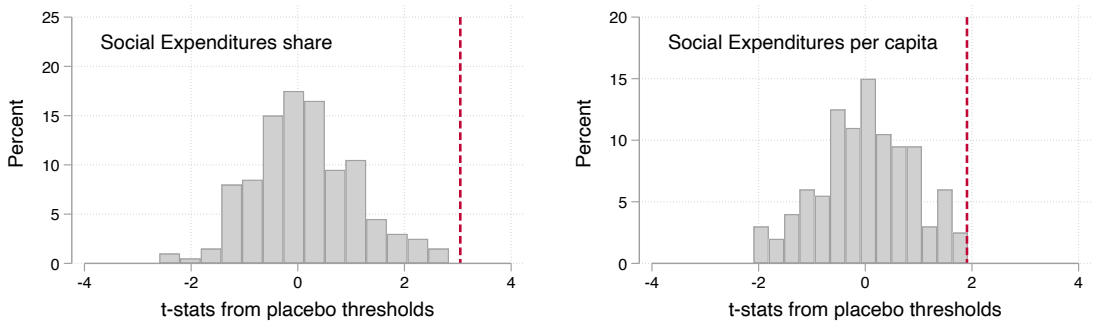
Appendix Table H.3 restricts the analysis to progressively larger cities, up to the 90th percentile. Results are overall rather stable, suggesting that heterogeneous effects by city size do not account for our results. Appendix Table H.4 re-estimates equation 2 using alternative bandwidth selection criteria. Results are insensitive to bandwidth choice.

Lagged outcomes As a falsification test, Figure 2 and the first column of Appendix Table F.1 report results for the 'effect' of a left-wing mayor on lagged (pre-election) outcomes. Reassuringly, we find no significant effects on lagged outcomes.

Placebo thresholds To assess whether our RD specification tends to over-reject the null hypothesis or suffers from some other failure of its identification assumptions, we perform a falsification test using placebo thresholds. We randomly draw 200 placebo thresholds, a hundred from each side of the true threshold, and re-estimate equation 2 with social expenditures (as a share of spending or per capita) 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 test against significant findings due to weak statistical power.

Figure 3 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 1 percent for the share of social expenditures and below 5 percent for social expenditures per capita.

Figure 3: Falsification test using placebo thresholds for the effect of a left-wing mayor on social expenditures



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 and social expenditure per capita, 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).

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

This section explores potential explanations for the rather limited extent of the 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. Moreover, we do not find any evidence that our results are driven by ideological heterogeneity within mayoral coalitions.

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.[16]

Results are consistent with re-election concerns playing a role in limiting budget composition effects (second column of Table 3 and middle panel of Figure 5). 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 (1.3 versus 0.6 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 (*microregião*). 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. The third column table 3 reports our RD specification in cities with below-median Tiebout competion (Herfindahl index above the median). In Appendix Table H.5, we restrict the sample to cities facing even lower competition, with Tiebout competition below the 25th percentile (Herfindahl index above the 75th percentile).

[16] This test follows a large previous literature that focuses on policy makers facing binding term limits in order to study the effect of weakening electoral competition pressures (e.g., Besley and Case (1995b); Ferraz and Finan (2011)). Previous studies suggest that 'lame-duck effects' are present among Brazilian mayors. In particular, Ferraz and Finan (2011) find that Brazilian lame-duck mayors are significantly more likely to engage in corruption than those facing re-election incentives.

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 subsamples. 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 2.1, 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.[17] 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 producing" municipalities. 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,

[17] Caselli and Michaels (2013, pp. 117-221 and online appendix) provide details on the rules governing the allocation of oil royalties in Brazil. Note that our RD strategy does not rely on the assumption that oil windfalls are exogenous to city-level characteristics and local shocks: we do not compare outcomes for cities receiving oil windfalls versus other cities. Our comparison is between closely elected left and non-left mayors, within the subset of cities which experienced an increase in oil windfalls. What we do assume here 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.

transportation, education, health and transfers to households, but have little effect on welfare-related outcomes, with the exception of the education sector.

To identify a subsample of oil windfall receivers, we use the growth of average oil royalties received over the mayoral term.[18] We calculate this variable for each 'oil-producing' municipality in our sample and define our oil-windfall receivers subsample as those observations above the median. In other words, a city-election enters our 'oil-windfall' subsample if it experiences a relatively large (above-median) change in revenues from oil royalties during the (after-election) mayoral term. In our baseline sample, 919 observations satisfy this criterion.

Partisan effects on the budget composition are indeed much stronger in the presence of oil windfalls (last column of Table 3 and panel (c) of Figure 5). The election of a left-wing mayor raises the share of social expenditures by 2.2 percentage points in this setting – a more than threefold increase in the size of the effect relative to the baseline. The differential effect is even larger in terms of social expenditures per capita: 6.5 versus only 1 percent in the baseline.

The effect on the overall size of the city government, as measured by municipal revenues and expenditures over GDP, however, remains essentially null as in all other specifications and subsamples (column 5 of Table 3). 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 we cannot rule out alternative explanations based on external constraints different from the ones we have been able to identify and measure.

6.4 Ideological convergence between mayoral coalitions

Another possible explanation for policy convergence is high ideological heterogeneity within political coalitions supporting mayors.[19] In some elections, conservative parties are part of the coalition supporting a left mayor, and vice versa. Heterogeneity within each coalition could lead to smaller ideological differences in policy platforms between different coalitions, therefore reducing policy divergence. Moreover, this channel could be stronger in close elections: when an election is expected

[18] To properly take into account city size, we scale the change in oil royalties by previous-period revenues.

[19] In Brazilian mayoral elections, coalition building happens before the election, presumably based on expected electoral returns, and aims to achieve mostly two things: i) get potential opponents out of the way and ii) transfer of electoral resources, especially fractions of free campaigning time on TV and radio (Limongi and Vasselai, 2018).

to be tight, there might be a stronger incentive for left and conservative candidates to try to secure the support of some party from the opposite ideological camp, in an attempt to maximize electoral returns.

To test this hypothesis, we calculate a measure of ideological distance between the competing coalitions in each election, and then assess how results vary with this index. If the small extent of policy divergence in our baseline results is due to a pre-electoral coalition-making process that leads to ideologically ambiguous coalitions, we would expect results to be different in elections in which the ideological distance between competing coalitions is large. We first compute a left-right ideology score for each mayoral coalition in our sample. This is equal to the weighted average of the Zucco and Power (forthcoming) left-right score for all parties in the coalition, with weights given by parties' vote shares in the previous city council election.[20] We then measure ideological distance as the difference in this left-right ideology score between the coalition supporting the most-voted left candidate and the coalition supporting the most-voted conservative candidate.

A first relevant fact is that, at least on average, there is a clear ideological demarcation between the competing coalitions that we study. In our average election, the coalition supporting the most-voted left-wing candidate is to the left of the coalition supporting the most-voted conservative candidate by around 0.37 points (median = 0.45), in a ideological score that ranges from -1 to +1. There is, however, substantial variability: the standard deviation for this distance is 0.42.

Moreover, coalitions supporting left mayors are clearly to the left of those supporting conservative mayors in the close elections we use for identification. To see this, we run our baseline RD specification, using the left-right ideology score for the elected mayor's coalition as the outcome variable and the left margin as the running variable. At the threshold, the ideology score for the winning coalition jumps to the left by 0.36 points (s.e. 0.02). This is shown in Figure 4. This is clearly inconsistent with the hypothesis that in close elections the competing coalitions tend to be ideologically indistinguishable.

Results reported in Table 3 and Appendix Table H.5 also provide little support for an explanation of our results based on ideological heterogeneity within coalitions. When restricting the analysis to elections in which the ideological distance between coalitions is larger (above median and above the

[20] Similar to our approach, Fujiwara (2015) and Power and Rodrigues-silveira (2019) also use weighted-averages of the Zucco and Power (forthcoming) left-right scores to measure, respectively, the ideological position of state legislatures and the electorally expressed ideology of Brazilian voters at the municipal level.

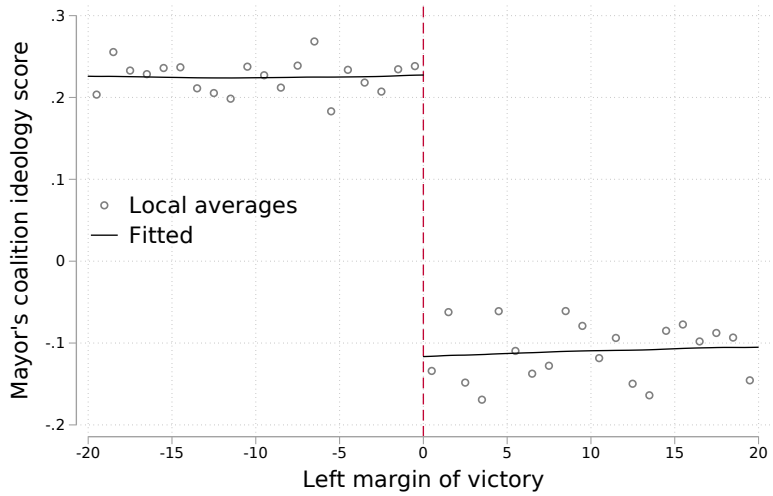


Figure 4: Ideology score for the coalition of the elected mayor

Notes: Left-right ideology score for the coalition supporting the elected mayor on the vertical axis (higher values indicate more right-wing coalitions). Margin of the left-wing candidate on the horizontal axis. See main text for the definition and construction of these two variables. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel) with MSE-optimal bandwidth.

75th percentile), results remain qualitatively similar to the baseline: there is no significant effect of left-wing mayors on the size of government; the effect on the social expenditures share remains small (only slightly larger than in the baseline) and actually decreases when passing from the median to the 75th percentile of ideological distance.

6.5 Inference on differential impacts

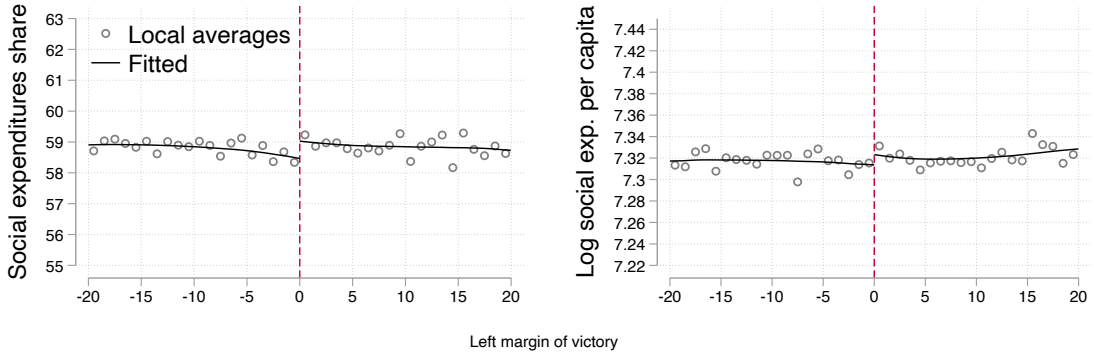
We perform a simple bootstrap exercise to provide more information on the differential impacts presented in our mechanisms' analysis. For each mechanism, we estimate our RD specification separately in the subsample of interest and in the rest of the sample. We then take the difference between the two estimated effects and run 500 bootstrap replications to obtain standard errors (clustered by municipality) for this difference.[21]

As shown in Table 4, the differential effects on social spending in cities with lame duck mayors and in those receiving oil-windfalls, both measured as a share of expenditures and per capita, are

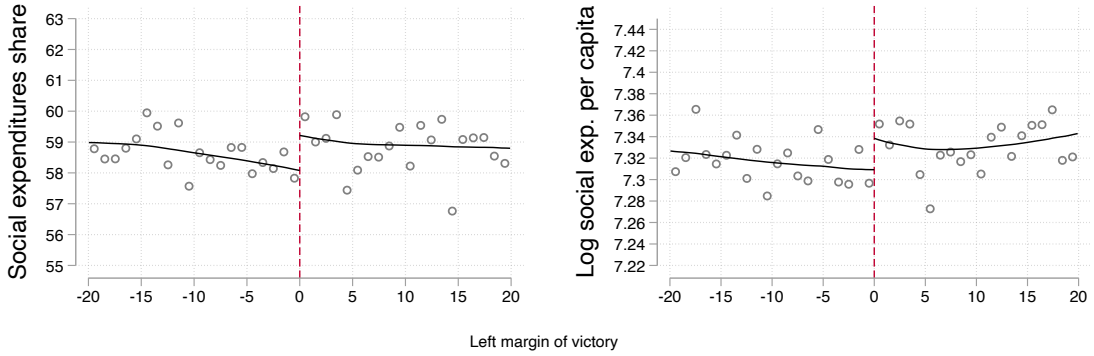
[21] This procedure is equivalent to using interaction terms to obtain differences in group effects in a parametric regression. We perform a bootstrap exercise due to our non-parametric approach. Point estimates for differential effects from this exercise reported in Table 4 are not equal to differences between coefficients from different columns of Table 3, because here we take the difference between the subsample and the rest of the observations, rather than between the subsample and the whole sample.

Figure 5: Effect of a left-wing mayor on social spending

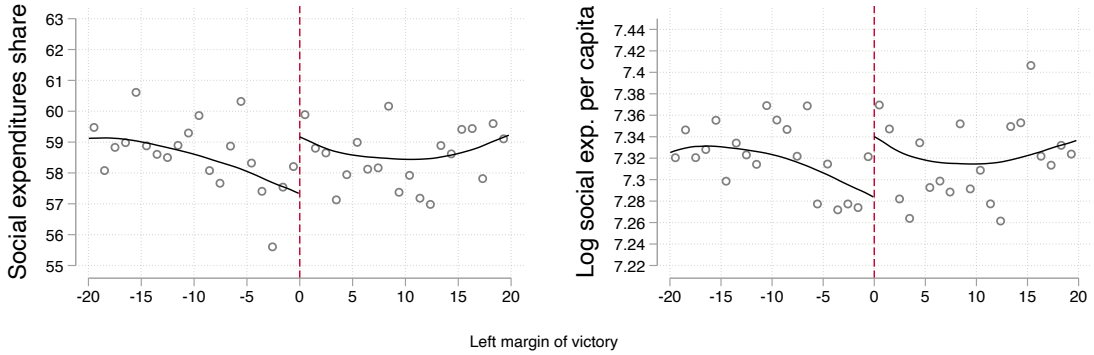
(a) Whole sample



(b) Cities with lame duck mayor



(c) Cities experiencing oil windfalls



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 3 and based on the specification in equation 2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Per-capita variables are taken in logarithms. 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.

Table 4: Differential effect on social expenditures in subsamples, relative to the rest of the sample

Outcome	Subsample					
	Lame duck	Oil Windfall	Tiebout competition		Ideology distance	
			< median	< 25th pct	> median	> 75th pct
Social expenditures share	0.91	1.74	0.41	-0.42	0.56	0.38
	(0.48)	(1.01)	(0.42)	(0.47)	(0.50)	(0.59)
Social expenditures per capita	3.09	5.74	-1.40	-2.24	0.75	-1.02
	(1.72)	(2.74)	(1.39)	(1.57)	(1.52)	(1.75)

Notes: For each subsample, this table reports the difference between the estimated effect in the subsample and in the rest of the sample. In each subsample, estimates are obtained 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. Standard errors clustered by municipality are obtained from 500 bootstrap replications. See Section 6.5 for more details on the procedure

all significant at least at the 10% significance level. Instead, effects in the subsamples with low Tiebout competition or high ideological distance between coalitions are not statistically different from the rest of the sample.

6.6 Dynamics, robustness and placebo tests for the mechanisms' analysis

Figure 2 and Appendixes F and H report dynamic effects, placebo exercises and robustness tests for our mechanisms' analysis.

Like baseline ones, results are qualitatively robust to differencing fiscal outcomes (Appendix Table H.1), excluding the first year of the term (Appendix Table H.2) and to alternative bandwidth selection criteria (Appendix Table H.4). When using differenced outcomes, however, the precision of the mechanisms' analysis decreases substantially (standard errors get wider). This is not surprising: in this robustness test we are considering only the final year of the term, which is likely to be noisier than the term-average, and at the same time the mechanisms' analysis employs smaller samples.

Also the results of the mechanisms' analysis pass the falsification exercise using placebo thresholds (Appendix Figure H.1). Moreover, there are no 'effects' on lagged (pre-election) outcomes (Appendix Tables F.2 and F.3). Social spending effects exhibit no pre-existing trend and increase gradually over time also in the 'lame duck' and 'oil-windfall' subsamples; they are larger compared to the baseline for each single year in office (Figure 2).

6.7 Welfare-related outcomes

In Appendix I, we estimate the effect of mayors' partisanship on a limited number of welfare-related outcomes. Specifically, we look at measures of infrastructure, human resources and overall performance of the municipal educational and health care systems. We also look at homicide rates given the high levels of violence observed in Brazil (Cerqueira et al., 2020).

This exercise should be interpreted as exploratory and taken with a grain of salt. First, the outcomes we use are very persistent and respond to policy with substantial time lags. Therefore, our research design might not be best suited to study them. Second, because of data availability, we are able to include a very limited number of outcomes, which provides only a partial and incomplete view of the possible socioeconomic effects of municipal policy.[22] Third, given the small size of the effect on each of the components of social spending (third panel of Table 3), any effect on individual welfare-related outcomes would plausibly be very small, and therefore hard to distinguish from noise in a finite sample.

With these important caveats in mind, Appendix Table I.2 does not find any robust effect on these outcomes over the mayoral term. Overall, there is some sign of positive effects of left-wing mayors on some education outcomes – in particular reductions in average class size and student-to-teachers ratios, and increases in progression rates in municipal schools – but these estimates are very imprecise and not statistically significant. The estimated effect on the infant mortality rate has a negative sign, but is again very imprecisely estimated and far from statistically significant. Effects on number of doctors, number of clinics and homicide rates have varying signs and are imprecisely estimated. For the reasons outlined above, we see these results as preliminary and not conclusive, and calling for further research.

7 Discussion

The most plausible explanation for our findings is that Brazilian left-wing mayors aim to redistribute municipal resources towards social spending in order to benefit the lower-income voters who

[22] Our research design based on close elections requires annual data available for all years of each election cycle or, at least, available in a regular schedule with information available for all cycles (for example, data on the third year of mayoral terms). Moreover, we need data that are representative at the city level. In Brazil, most surveys do not fit these two criteria, limiting the inclusion of a broader range of outcomes in this exercise.

constitute their electoral base, consistent with the available evidence on social cleavages in Brazilian politics (reviewed in Section 2.2).[23] Their ability to do so, however, appears to be severely limited both by institutional constraints and re-election concerns.

Viewed in this light, our results uncover some parallel between national and local political tendencies in Brazil. It is indeed widely recognized that, in the period we study, increasing social welfare spending has been a defining characteristic of the left-wing tenures in power at the federal level, and that financial constraints (mostly related to the global economic context) have determined the timing and generosity of welfare expansions (Campello, 2016).

Our results and the interpretation we have proposed are also consistent with Fujiwara (2015)'s analysis of the effects of electronic voting in Brazilian elections for state legislatures. Fujiwara (2015) finds that the introduction of electronic voting in the 1998-2002 period constituted a *de facto* enfranchisement of mainly poorer citizens. This caused the election of more left-wing state legislators, leading to a gradual increase in the share of social spending (in particular on public health care) but no effect on the size of the state government.

Although we find quite limited overall partisan effects on municipal fiscal policy, our mechanisms analysis does not support the widespread view that political parties in newly democratized developing countries lack distinctive economic policy preferences and programmatic linkages to voters. According to this view, parties in young democracies cannot rely on an already established social base, which would require both long time and substantial effort to develop. Therefore, they seek to win elections (especially local ones) by relying on clientelistic and charismatic appeals and self-interested local brokers. This strategy, in turn, inhibits the development of distinctive policy platforms and programmatic linkages to voters (Kitschelt and Wilkinson, 2007; Novaes, 2018). This view would predict complete absence of partisan effects, including when institutional constraints and re-election concerns are relaxed, contrary to our analysis of the mechanisms that limit policy divergence.

Of course, our results are not in contradiction with the view that local brokers are important in local politics, especially in developing countries, and that their strategic rent-seeking behavior can weaken political parties at the local level (as documented for example in Novaes (2018) for

[23] Of course, given that our results are based on a comparison between (closely-elected) left-wing and conservative mayors, this is equivalent to saying that conservative mayors shift resources away from social spending and towards their preferred uses.

Brazil and Camp (2017) for Argentina) and possibly also dilute their ideological identity. In fact, this might well be one of the mechanisms reducing policy divergence in our sample. However, our results are inconsistent with the view that Brazilian parties are completely free of distinctive policy preferences and programmatic linkages to voters as a result of this type of mechanism.

Another relevant question is why Tiebout competition within commuting zones does not seem to play a major role in limiting fiscal policy divergence in our sample (unlike, for example, in the case of US cities, as found by Ferreira and Gyourko, 2009). Our data does not allow to provide a conclusive empirical answer to this question, but some considerations are possible. Municipal public service provision and municipal taxation might be of second order importance for location choices, when compared to other factors such as living costs, earnings and employment opportunities.[24] For this reason, the degree of policy divergence that would be required to significantly affect location dynamics might be larger than what other (institutional and political) constraints can plausibly allow, thus making Tiebout competition a non-binding constraint for Brazilian mayors.

In principle, high moving costs might also contribute to make Tiebout competition toothless. However, even if we cannot directly observe these moving costs, internal migration in Brazil appears rather intense (de Lima Amaral, 2013).[25]

Finally, one might ask whether political alignment with the federal government could contribute to explain our results. There is evidence that municipalities with mayors affiliated with the coalition in power at the federal level receive more discretionary transfers (Brollo and Nannicini 2012; Meireles 2019, Chapter 2).[26] This might be relevant, especially given that during our sample period the leftist PT held the Presidency of the federal government, heading a center-left government coalition (with

[24] Indeed, the literature on internal migration in Brazil has generally identified real wage differentials and employment opportunities (together with geographical distance) as the key determinants of population movements (de Lima Amaral, 2013; Lameira et al., 2015). Two important caveats are that existing studies (a) do not focus specifically on movements within commuting zones and (b) do not explicitly compare the importance of these factors to that of municipal public service provision.

[25] An alternative explanation might be that Tiebout competition occurs across commuting zones as easily as within them, in such a way that the availability of alternative locations within the same commuting zone (which is what our measure, following the literature, captures) is not a relevant factor. For this to occur, however, moving within commuting zones would have to be at least as costly as moving across them. This appears unlikely, especially when taking not only monetary but also social costs into account, and inconsistent with available evidence. For example, Egger (2019) finds that Brazilian internal migrants tend to prefer towns closer to their origin, to minimize monetary and social moving costs.

[26] Brollo and Nannicini (2012) find that municipalities with mayors affiliated to the coalition (and especially the political party) of the Brazilian President receive more discretionary transfers for infrastructure projects in preelection years. Meireles (2019, Chapter 2) shows that cities aligned with a ministry, meaning that the mayor and the minister are from the same party, receive on average 25% more voluntary transfers from that specific ministry.

the only exception of the last three months of the 2016 mayoral term).

However, this mechanism can hardly explain our results. First, if political alignment with the federal government was driving our results, it would have caused a positive effect of left mayors on the size of government and, possibly, investment. However, we find little effect of left mayors on revenues and expenditures, both per capita and as a share of GDP. Indeed, when analyzing the composition of revenues, we find no significant effect of left-wing mayors on federal transfers received (Appendix E). A possible reason is that conservative parties have been part of the center-left coalition in power at the federal level during our sample period and held ministerial positions (including the vice presidency). Therefore, some conservative mayors in our sample might have benefited from the discretionary transfer effects documented in Brollo and Nannicini (2012) and Meireles (2019).Moreover, alignment with the federal government cannot explain the substantial differential effects found in our mechanisms analysis.

8 Conclusions

To study the role of partisanship in shaping local fiscal policy, this paper analyses a large sample of Brazilian municipal administrations in the 2004-2016 period. We employ a regression-discontinuity design, focusing on close mayoral elections.

We find no effect of left-wing mayors on the size of the city government, but a modest 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 four 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 more than three times larger than in the whole sample (around 2.2 percentage points). Also in the subsample of cities with 'lame-duck' mayors we find a larger effect on the share of social spending (1.3 percentage points compared to 0.60 in the baseline sample). These differential effects are even larger in terms of social expenditures per capita: on this variable we find

a partisan effect of almost 6.5 percent among oil-windfall receivers, and around 3.3 percent among 'lame duck' mayors, compared to just 1 percent in the whole sample. We find no empirical support for explanations based on Tiebout-competition or ideologically ambiguous electoral coalitions.

Our results, combined with available evidence on political cleavages in Brazil, suggest that Brazilian parties do attempt to shape the allocation of municipal resources to favor their respective constituencies, but their ability to do so is severely limited by institutional and budget constraints and re-election concerns. Our analysis of the factors limiting fiscal policy divergence in Brazilian cities suggests that, contrary to a widespread view, local politics in newly democratized developing countries can be characterized by distinct underlying policy preferences and programmatic linkages between voters and parties. Further studies might help determine whether Brazil is an exception or the rule from this point of view.

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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, forthcoming), 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 (forthcoming, 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, forthcoming, p. 5). We classify 15 parties in this way.[27]

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 (2020) 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).

[27] Even 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 reports the final classification with the respective source from which the party ideology was assigned.

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/CID	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/Cidadania (PPS/CID), 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 Covariates descriptive statistics

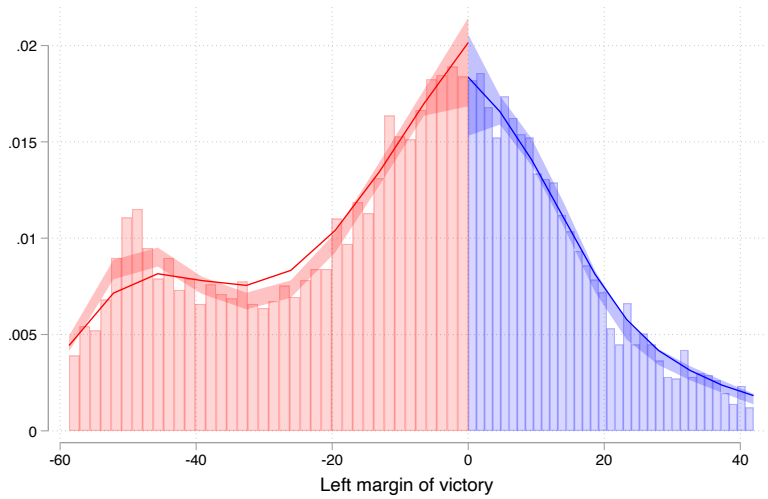
Table B.1: Covariates descriptive statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Labor market and demographic covariates					
Median earnings	806.89	809.02	873.48	822.26	786.95
	(269.84)	(286.18)	(294.44)	(280.56)	(305.44)
Labor force participation	54.87	54.78	55.37	55.02	54.07
	(8.43)	(8.79)	(7.59)	(8.45)	(6.93)
Population (in thousands)	43.24	46.50	65.47	58.98	67.47
	(253.47)	(217.32)	(342.65)	(373.28)	(247.91)
% Urban	62.55	62.73	67.39	63.85	65.72
	(23.82)	(24.25)	(23.67)	(24.08)	(24.84)
% White	50.93	51.62	52.07	50.42	42.31
	(25.03)	(25.05)	(24.37)	(25.34)	(21.87)
% Higher education	3.69	3.61	4.22	3.89	3.42
	(3.19)	(3.15)	(3.55)	(3.32)	(3.67)
% Illiterate	17.59	18.09	15.38	17.26	19.34
	(10.98)	(11.53)	(10.45)	(10.97)	(11.31)
Geographic covariates					
North	0.08	0.07	0.11	0.08	0.03
	(0.27)	(0.26)	(0.32)	(0.27)	(0.16)
Northeast	0.33	0.34	0.24	0.35	0.51
	(0.47)	(0.48)	(0.43)	(0.48)	(0.50)
South	0.21	0.22	0.19	0.23	0.06
	(0.41)	(0.41)	(0.39)	(0.42)	(0.24)
Southeast	0.32	0.31	0.40	0.28	0.41
	(0.47)	(0.46)	(0.49)	(0.45)	(0.49)
Midwest	0.07	0.06	0.06	0.05	0.00
	(0.25)	(0.23)	(0.24)	(0.22)	(0.00)
Other covariates					
Bolsa Familia (households)	2.16	2.37	2.89	2.69	2.98
	(7.00)	(7.38)	(9.66)	(8.98)	(7.45)
Bolsa Familia (receipts)	142.23	145.52	123.94	148.70	133.97
	(113.04)	(112.82)	(102.01)	(114.60)	(91.66)
Authorized amendments	1.56	1.60	1.82	1.75	1.26
	(6.65)	(6.08)	(7.68)	(7.85)	(2.33)
Executed amendments	0.64	0.80	0.76	0.74	0.66
	(4.90)	(4.66)	(5.88)	(6.34)	(1.76)
Number of obs.	8943	2395	4158	3105	919

Notes: This table reports mean and standard deviation (in parenthesis) for the covariate variables. Demographic and geographic covariates obtained from IBGE. Data on the conditional cash-transfer program Bolsa Família are from *Ministério da Cidadania*. Both the number of households receiving Bolsa Familia and Bolsa Familia receipts are normalized by population to take into account city size. Transfers received through amendments are expressed as a share of city revenues and obtained from *SIGA-Brasil*. See Section 6 for the specific definition and motivation of each subsample.

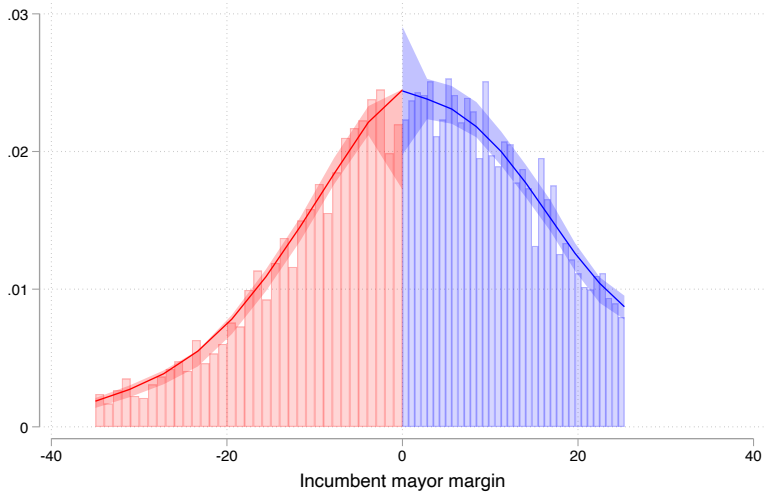
Appendix C Additional design assessment tests

Figure C.1: Test for manipulation of the running variable



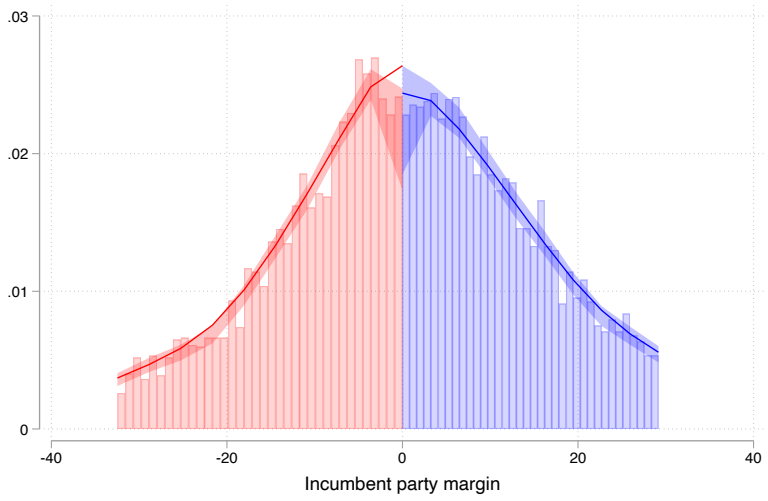
Notes: The figure presents visual evidence for the Cattaneo et al. (2018) manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the running variable at the cutoff. T-stat = -0.65; P-value = 0.51.

Figure C.2: Test for manipulation by incumbent mayors



Notes: The figure presents visual evidence for the Cattaneo et al. (2018) manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the incumbent margin at the cutoff. This test focuses on elections in which one of the candidates is the incumbent mayor. T-stat = 1.16; P-value = 0.25.

Figure C.3: Test for manipulation by incumbent parties



Notes: The figure presents visual evidence for the Cattaneo et al. (2018) manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the incumbent margin at the cutoff. This test focuses on elections in which one of the candidates is affiliated with the party of the incumbent mayor (either the incumbent herself or a different candidate from the same party). T-stat = 0.50; P-value = 0.62.

Appendix D Tests for balance in candidate characteristics

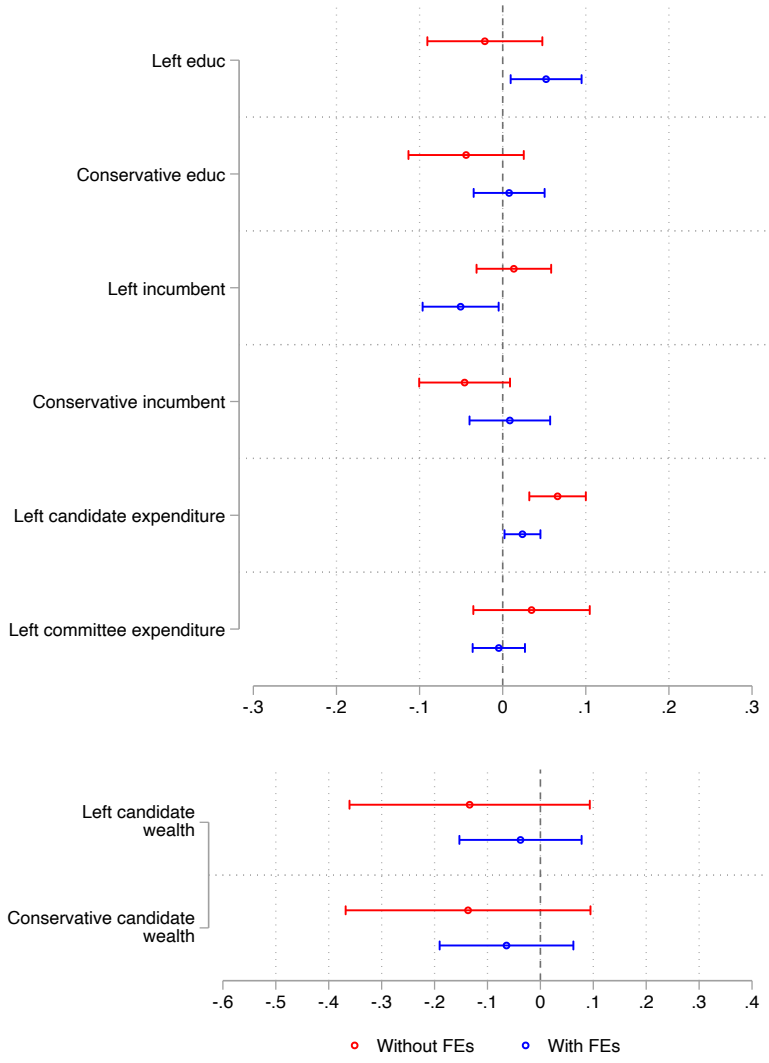
This appendix presents tests for discontinuities in candidates' characteristics around the threshold, in the spirit of Caughey and Sekhon (2011). In particular, we look at candidates' education level, incumbency status, self-reported personal wealth, and their campaign expenditures. For campaign expenditures, we look at both expenditures made by the individual candidate, and expenditures made by the party election committee on the candidate's behalf.[28]

Specifically, we estimate our RD specification (equation 2), again with the margin of the left-wing candidate as the running variable, but with the characteristics of both left and conservative candidates as the outcomes. In other words, this test compares close left-wing winners to close left-wing losers (and does the same for conservative candidates).

Results are reported in Figure D.1, for the baseline sample, both with and without controlling for city and time fixed effects. Overall, we find little evidence of discontinuities in candidates' education level, personal wealth and incumbency status. When not including city and time fixed effects, we do find some significant discontinuity in candidate expenditures: close winners tend to have spent more during the campaign than close losers. This is similar to what Caughey and Sekhon (2011) found in a sample of US House elections. However, this discontinuity in campaign expenditures becomes much smaller once we control for city and time fixed effects.

[28] Incumbency status is measured by a dummy variable equal to one if the candidate is the incumbent mayor; education level by a dummy equal to one if the candidate completed high school; campaign expenditures are normalized by the total expenditure of the two relevant candidates, therefore the relevant variable is the share of candidate expenditures made the left-wing candidate.

Figure D.1: Discontinuities in candidate characteristics around the threshold



Notes: The figure presents estimates from our RD specification (equation 2), with candidates' characteristics as outcome variables. 'Left educ' is a dummy for whether the left candidate has completed high school. 'Left incumbent' is a dummy for whether the left candidate is the incumbent mayor. Candidate and committee expenditures are measured as a share of the total expenditures of the two relevant candidates. Wealth is the log of self-reported wealth.

Appendix E Composition of revenues

Table E.1: RD estimates of the effect of a left-wing mayor on the composition of revenues

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
		Revenues, % GDP			
Total revenue	0.12	-1.14	-0.20	0.24	-0.21
	(0.25)	(1.20)	(0.42)	(0.27)	(0.49)
Municipal taxes	-0.02	-0.07	-0.10	0.05	-0.02
	(0.03)	(0.13)	(0.05)	(0.05)	(0.06)
Federal transfers	0.15	-0.44	0.05	0.09	-0.12
	(0.13)	(0.56)	(0.22)	(0.14)	(0.32)
State transfers	0.02	-0.18	-0.07	0.05	-0.01
	(0.06)	(0.23)	(0.11)	(0.08)	(0.15)
		Composition of revenues: revenue categories (% of total revenues)			
Municipal taxes	-0.17	0.13	-0.21	0.01	0.20
	(0.09)	(0.22)	(0.15)	(0.15)	(0.26)
Federal transfers	0.29	0.37	0.66	-0.22	0.36
	(0.19)	(0.44)	(0.28)	(0.33)	(0.78)
State transfers	-0.03	-0.04	0.03	0.28	0.13
	(0.14)	(0.29)	(0.23)	(0.27)	(0.52)
Observations (all)	8943	2395	4158	3105	919
Observations (effective)	5168	1786	3177	1679	366

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. Per-capita variables are taken in logarithms and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Appendix F Dynamic effects and pre-trends

Table F.1: Dynamic effects and pre-trends in the baseline sample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	-0.21	-0.01	1.05	0.07	0.18	-0.41
	(0.52)	(0.56)	(0.89)	(0.80)	(0.64)	(0.79)
Expenditure, % of GDP	0.40	0.01	0.34	0.14	-0.51	0.00
	(0.26)	(0.24)	(0.17)	(0.19)	(0.81)	(0.18)
Revenue per capita	-0.58	0.39	-0.01	1.38	-0.17	0.50
	(0.55)	(0.52)	(0.64)	(0.90)	(0.68)	(0.89)
Revenue, % of GDP	0.20	0.12	0.34	0.29	-0.33	0.35
	(0.25)	(0.25)	(0.19)	(0.20)	(0.75)	(0.22)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	-0.01	-0.05	0.10	0.20	-0.27	-0.33
	(0.24)	(0.17)	(0.26)	(0.30)	(0.27)	(0.30)
Public Investment	-0.01	0.09	-0.08	-0.17	0.37	0.27
	(0.24)	(0.16)	(0.26)	(0.30)	(0.27)	(0.29)
Personnel	-0.19	-0.05	-0.28	0.30	-0.02	-0.23
	(0.25)	(0.22)	(0.34)	(0.27)	(0.27)	(0.29)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.13	0.64	0.13	0.78	0.66	1.05
	(0.21)	(0.21)	(0.30)	(0.30)	(0.32)	(0.34)
of which:						
Health & sanitation	-0.16	0.18	-0.23	0.12	0.25	0.58
	(0.16)	(0.15)	(0.20)	(0.21)	(0.22)	(0.21)
Education & culture	0.08	0.24	0.32	0.47	0.09	0.35
	(0.16)	(0.16)	(0.22)	(0.23)	(0.22)	(0.24)
Social welfare	-0.07	0.16	0.09	0.19	0.18	0.13
	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)	(0.07)
Other expenditures:						
Housing	-0.00	-0.16	-0.18	-0.18	-0.12	-0.17
	(0.15)	(0.13)	(0.21)	(0.20)	(0.18)	(0.20)
Transportation	-0.24	-0.18	-0.16	-0.30	-0.25	-0.15
	(0.13)	(0.09)	(0.13)	(0.15)	(0.13)	(0.14)
Other	0.36	-0.21	0.16	-0.32	-0.11	-0.81
	(0.24)	(0.23)	(0.30)	(0.32)	(0.33)	(0.40)
Social Expenditures per capita	-0.23	1.16	1.23	2.42	0.96	1.01
	(0.65)	(0.61)	(0.98)	(1.10)	(0.81)	(0.92)
Observations (all)	8144	8943	8943	8943	8943	8943
Observations (effective)	5093	4408	3160	4565	5511	4166

Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table F.2: Dynamic effects and pre-trends in the lameduck subsample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	-1.58	0.52	3.59	1.40	-0.43	-0.50
	(1.09)	(1.27)	(1.97)	(1.81)	(1.57)	(1.64)
Expenditure, % of GDP	0.45	-1.05	0.63	0.15	-4.45	0.37
	(0.65)	(1.18)	(0.36)	(0.35)	(4.58)	(0.39)
Revenue per capita	-0.55	0.59	0.54	2.28	0.13	0.09
	(1.04)	(1.21)	(1.63)	(1.83)	(1.42)	(1.51)
Revenue, % of GDP	0.57	-1.14	0.11	0.23	-4.59	0.31
	(0.66)	(1.20)	(0.39)	(0.38)	(4.68)	(0.40)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	0.38	-0.39	-0.35	-0.32	-0.61	-0.32
	(0.42)	(0.38)	(0.56)	(0.59)	(0.55)	(0.55)
Public Investment	-0.28	0.40	0.49	0.16	0.51	0.27
	(0.43)	(0.37)	(0.56)	(0.66)	(0.54)	(0.59)
Personnel	0.49	-0.81	-1.14	-0.20	-0.93	-1.23
	(0.52)	(0.48)	(0.66)	(0.67)	(0.62)	(0.61)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.22	1.27	1.00	1.52	0.77	2.19
	(0.37)	(0.40)	(0.57)	(0.55)	(0.63)	(0.85)
of which:						
Health & sanitation	-0.13	0.69	0.28	0.69	0.55	1.12
	(0.29)	(0.30)	(0.39)	(0.45)	(0.48)	(0.45)
Education & culture	-0.06	0.31	0.27	0.57	0.18	0.97
	(0.30)	(0.30)	(0.48)	(0.42)	(0.43)	(0.63)
Social welfare	-0.07	0.32	0.43	0.38	0.19	0.21
	(0.13)	(0.11)	(0.15)	(0.17)	(0.14)	(0.12)
Other expenditures:						
Housing	-0.03	-0.36	-0.30	-0.52	-0.61	-0.05
	(0.35)	(0.29)	(0.46)	(0.42)	(0.42)	(0.43)
Transportation	-0.69	-0.07	0.07	0.25	0.06	-0.30
	(0.25)	(0.20)	(0.28)	(0.31)	(0.28)	(0.34)
Other	0.84	-0.86	-0.70	-1.26	-0.16	-2.00
	(0.44)	(0.42)	(0.57)	(0.56)	(0.70)	(0.87)
Social Expenditures per capita	-1.89	3.34	4.45	1.94	0.76	-0.66
	(1.36)	(1.36)	(2.04)	(2.67)	(2.52)	(3.16)
Observations (all)	2227	2395	2395	2395	2395	2395
Observations (effective)	1185	1227	857	1150	1266	1146

Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table F.3: Dynamic effects and pre-trends in the oil windfall subsample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	0.14	2.26	2.45	0.36	3.86	2.06
	(2.08)	(2.06)	(2.46)	(2.35)	(2.76)	(2.82)
Expenditure, % of GDP	-0.02	-0.13	0.85	-0.30	-0.37	-0.60
	(0.40)	(0.47)	(0.58)	(0.62)	(0.62)	(0.75)
Revenue per capita	1.52	2.19	2.79	-0.04	4.11	-0.76
	(2.64)	(2.01)	(2.63)	(2.75)	(2.24)	(2.60)
Revenue, % of GDP	0.05	-0.21	0.83	-0.57	-0.31	-0.87
	(0.47)	(0.49)	(0.61)	(0.58)	(0.65)	(0.83)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	0.49	1.01	0.79	1.36	-0.27	2.67
	(0.81)	(0.65)	(0.83)	(1.07)	(0.93)	(1.02)
Public Investment	-0.13	-0.96	-0.57	-1.19	0.05	-2.67
	(0.80)	(0.63)	(0.84)	(1.06)	(0.92)	(1.03)
Personnel	-0.35	-0.34	-0.29	0.37	-1.10	0.39
	(0.72)	(0.85)	(1.09)	(1.15)	(1.08)	(1.09)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.34	2.19	2.09	2.34	1.05	3.92
	(0.73)	(0.87)	(1.26)	(1.15)	(1.06)	(1.85)
of which:						
Health & sanitation	-0.92	0.18	0.95	1.00	0.42	1.16
	(0.55)	(0.39)	(0.97)	(0.68)	(0.64)	(0.75)
Education & culture	0.58	0.84	0.75	0.74	0.29	2.34
	(0.59)	(0.49)	(0.81)	(0.73)	(0.71)	(1.17)
Social welfare	0.08	0.36	0.39	0.33	0.36	0.33
	(0.24)	(0.20)	(0.24)	(0.30)	(0.27)	(0.28)
Other expenditures:						
Housing	0.45	0.41	0.45	0.46	0.03	0.45
	(0.64)	(0.65)	(0.80)	(0.97)	(0.78)	(0.92)
Transportation	-0.26	-0.73	-0.49	-0.59	-0.63	-0.87
	(0.28)	(0.29)	(0.34)	(0.37)	(0.35)	(0.38)
Other	0.15	-1.90	-1.79	-2.09	-0.29	-3.53
	(0.80)	(1.10)	(1.60)	(1.39)	(1.22)	(2.19)
Social Expenditures per capita	0.08	6.48	7.60	2.76	4.52	3.89
	(2.42)	(2.40)	(3.43)	(2.62)	(2.78)	(2.74)
Observations (all)	813	919	919	919	919	919
Observations (effective)	517	451	386	549	482	417

Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Appendix G Results by mayoral term and extended sample period

This section shows how the effect of a left-wing mayor on the share of social spending has varied over time. In this exercise, we extend our sample period backwards to the 1996 and 2000 electoral cycles, in order to provide a broader picture of how partisan effects evolved in time after Brazil's democratization. Extending our sample period backwards is challenging for two reasons, however, and results using pre-2004 data should be taken with caution.

First, TSE informs in the website of its main statistical repository that electoral data for pre-2004 elections are incomplete. Indeed, the raw files have data only on 116 cities for the 1996 election. However, data on the 2000 election seem complete as they contain information on 5,555 cities. For the 1996 election, we solve the problem by downloading data from an old repository from TSE. Even though this old repository has all the key variables for our study, it does not contain information about the status of the election (whether or not the election was judged irregular and redone outside the regular calendar) or the final status of the candidates registration (whether a candidate died, renounced or had any other irregularities), both of which we use in our sample selection procedure. Given that the data from the main repository seem complete for the 2000 elections and the fact that the old repository has important limitations, we decided to use data from the former for the 2000 election.

The second problem is that pre- and post-2002 fiscal data are not fully comparable. In particular, besides other issues, social spending cannot be calculated properly in the pre-2002 period, because welfare spending (which is part of our social spending variable) cannot be separated from pensions paid to former municipal employees (which are not). This second problem cannot be completely solved, which is the main reason why we don't include pre-2004 elections in our main analysis. Here, we adopt a second-best solution. We compute an alternative definition of social spending that can be calculated both pre- and post-2002. This measure is equal to our preferred definition of social spending, plus pensions paid to former municipal employees.

Figure G.1 displays the estimated effect of a left-wing mayor on the share of social spending by election cycle. We use both the proper definition of social spending (which excludes pensions to former municipal employees and is available only post-2002) and the 'second-best' one (which includes pensions and is available for all elections).

The effect on the share of social spending is positive in all periods and with both definitions, but it seems much stronger in the 'boom years' 2001-2008, and weaker pre-2001 and post-2008. The effect seems virtually null but very imprecisely estimated in the 1997-2000 period (that is, for mayors elected in 1996), but the large standard errors and the unavailability of the proper definition of social spending for that period suggest much caution in interpreting that result.

The fact that the effect is strongest in the 2001-2008 'boom years' appears consistent with the hypothesis that left-wing mayors redistribute more when financial constraints are relaxed: the boom years were characterized by rising revenues, caused both by strong income growth and the commodities boom. There does not appear to be any clear mapping between the intensity of partisan effects and the balance of power at the federal level. The left-wing PT held the presidency in the 2002-2008 period, in which partisan effects are stronger, but also in 2009-2016 (except for the second half of 2016), when the effect was weaker.

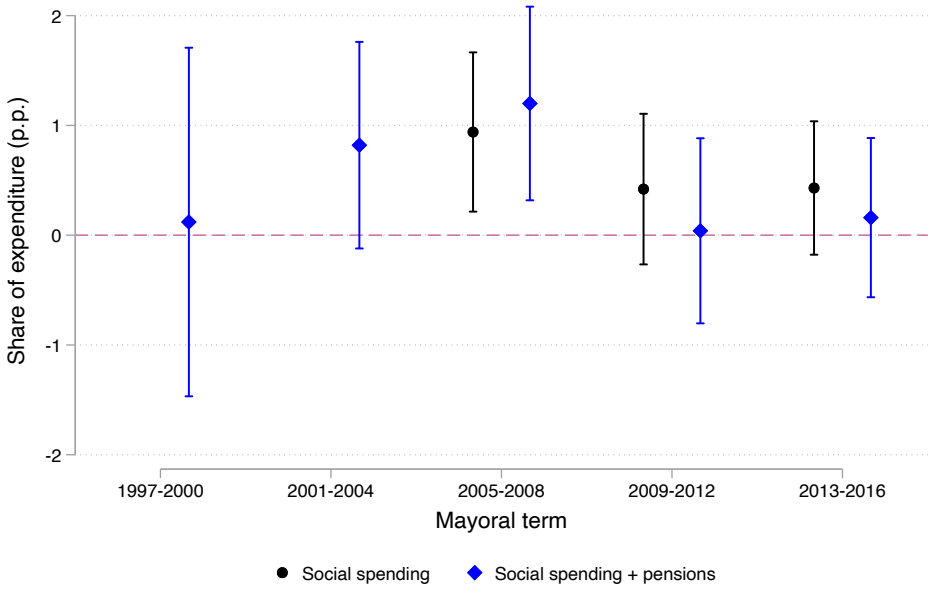


Figure G.1: Effect on the share of social expenditures, by mayoral term

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. The effect is estimated separately for each mayoral term, using our baseline definition of social spending (black) and an alternative definition that includes pensions paid to former municipal employees (blue). Bars represent 95% confidence intervals from robust bias-corrected standard errors clustered by municipality.

Appendix H Robustness tests

Table H.1: RD estimates of the effect of a left-wing mayor, using differenced outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	0.22	0.61	2.30	-3.02	0.86
	(1.45)	(2.62)	(2.23)	(2.18)	(4.13)
Expenditure, % of GDP	-0.03	0.50	-0.11	-0.97	-0.88
	(0.36)	(0.64)	(0.50)	(0.58)	(1.10)
Revenue per capita	1.40	0.36	0.13	0.79	-5.20
	(1.32)	(2.04)	(1.96)	(2.00)	(4.90)
Revenue, % of GDP	0.29	0.25	-0.35	-0.27	-1.88
	(0.35)	(0.68)	(0.52)	(0.57)	(1.29)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.30	-0.87	0.13	0.00	4.10
	(0.58)	(0.94)	(0.80)	(0.88)	(2.02)
Public Investment	0.53	0.93	0.32	0.54	-3.69
	(0.60)	(0.99)	(0.81)	(0.87)	(2.04)
Personnel	0.39	-1.02	-0.00	0.64	3.59
	(0.54)	(0.96)	(0.77)	(0.90)	(1.88)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	1.31	2.44	2.54	2.76	3.14
	(0.61)	(1.48)	(0.84)	(1.07)	(2.64)
of which:					
Health & sanitation	1.28	0.98	1.77	1.53	2.61
	(0.41)	(0.80)	(0.54)	(0.67)	(1.35)
Education & culture	-0.07	1.96	0.43	1.03	0.81
	(0.39)	(1.09)	(0.59)	(0.66)	(1.79)
Social welfare	0.24	-0.05	0.38	0.27	-0.22
	(0.13)	(0.25)	(0.18)	(0.22)	(0.71)
Other expenditures:					
Housing	0.08	0.72	0.78	1.19	-0.67
	(0.40)	(0.86)	(0.62)	(0.67)	(1.65)
Transportation	0.09	0.13	-0.56	-0.48	0.09
	(0.28)	(0.69)	(0.38)	(0.44)	(0.57)
Other	-1.57	-3.28	-2.83	-3.59	-2.88
	(0.72)	(1.56)	(1.04)	(1.27)	(3.05)
Social Expenditures per capita	1.56	-1.42	4.70	-1.45	0.98
	(1.87)	(5.11)	(2.22)	(2.79)	(4.91)
Observations (all)	8502	2320	3969	2963	859
Observations (effective)	3881	1150	1919	1585	460

Notes: Estimates from our baseline RD specification (equation 2), using the bias-corrected procedure of Calonico et al. (2014) and controlling for year fixed effects. All outcomes taken as percentage points differences between the fourth year of the term and the election year. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table H.2: RD estimates of the effect of a left-wing mayor - excluding first year of mayor term

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	-0.24	0.22	-0.60	-0.93	2.22
	(0.56)	(1.31)	(0.81)	(1.32)	(2.22)
Expenditure, % of GDP	-0.11	-1.39	-0.47	-1.37	-0.42
	(0.31)	(1.55)	(0.58)	(1.18)	(0.54)
Revenue per capita	0.36	0.42	-0.44	-0.12	1.88
	(0.55)	(1.25)	(0.82)	(1.30)	(2.13)
Revenue, % of GDP	0.06	-1.45	-0.32	-1.21	-0.49
	(0.32)	(1.58)	(0.58)	(1.18)	(0.55)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.12	-0.37	-0.24	0.31	1.06
	(0.18)	(0.41)	(0.29)	(0.48)	(0.73)
Public Investment	0.20	0.34	0.31	-0.06	-1.04
	(0.19)	(0.41)	(0.29)	(0.41)	(0.72)
Personnel	0.00	-0.67	0.05	0.28	-0.27
	(0.22)	(0.51)	(0.33)	(0.46)	(0.91)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.78	1.28	0.89	0.29	2.29
	(0.24)	(0.42)	(0.33)	(0.46)	(1.00)
of which:					
Health & sanitation	0.30	0.86	0.53	0.15	0.85
	(0.17)	(0.33)	(0.24)	(0.35)	(0.53)
Education & culture	0.22	0.46	0.12	-0.04	0.98
	(0.17)	(0.31)	(0.20)	(0.34)	(0.63)
Social welfare	0.16	0.27	0.28	0.13	0.36
	(0.06)	(0.11)	(0.10)	(0.12)	(0.25)
Other expenditures:					
Housing	-0.12	-0.39	0.06	0.25	0.44
	(0.14)	(0.33)	(0.25)	(0.35)	(0.75)
Transportation	-0.19	-0.02	-0.32	-0.15	-0.75
	(0.11)	(0.23)	(0.15)	(0.22)	(0.33)
Other	-0.39	-1.07	-0.67	-0.37	-1.90
	(0.27)	(0.53)	(0.39)	(0.52)	(1.24)
Social Expenditures per capita	0.77	2.83	0.94	-0.27	7.34
	(0.58)	(1.41)	(0.97)	(1.54)	(2.85)
Observations (all)	8943	2395	4158	2081	919
Observations (effective)	4972	1319	2696	1366	460

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 3-year averages over a mayoral term (excluding the first year of the term). Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table H.3: RD estimates of the effect of a left-wing mayor - by city size

	Baseline	Subsamples: Population higher than ...			
		25th pct	median	75th pct	90th pct
		6,594	14,196	31,209	76,530
Size of government: overall revenues and expenses					
Expenditure per capita	-0.01	-0.19	0.12	0.95	1.15
	(0.56)	(0.62)	(0.77)	(1.18)	(1.88)
Expenditure, % of GDP	0.01	-0.16	-0.05	0.11	0.02
	(0.24)	(0.27)	(0.16)	(0.19)	(0.21)
Revenue per capita	0.39	0.32	-0.12	0.21	0.18
	(0.52)	(0.60)	(0.70)	(1.14)	(1.67)
Revenue, % of GDP	0.12	-0.03	-0.06	0.01	-0.02
	(0.25)	(0.29)	(0.15)	(0.19)	(0.21)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05	0.13	0.04	-0.18	-0.32
	(0.17)	(0.21)	(0.25)	(0.34)	(0.56)
Public Investment	0.09	-0.04	0.14	0.34	0.17
	(0.16)	(0.21)	(0.25)	(0.32)	(0.63)
Personnel	-0.05	0.10	0.31	0.34	1.19
	(0.22)	(0.23)	(0.29)	(0.44)	(0.74)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64	0.75	0.51	0.34	0.68
	(0.21)	(0.25)	(0.29)	(0.40)	(0.91)
of which:					
Health & sanitation	0.18	0.17	0.05	0.17	-0.43
	(0.15)	(0.16)	(0.22)	(0.33)	(0.62)
Education & culture	0.24	0.36	0.41	0.16	0.94
	(0.16)	(0.19)	(0.22)	(0.27)	(0.51)
Social welfare	0.16	0.18	0.06	0.08	-0.03
	(0.06)	(0.06)	(0.06)	(0.09)	(0.14)
Other expenditures:					
Housing	-0.16	-0.28	-0.30	-0.01	0.46
	(0.13)	(0.16)	(0.20)	(0.28)	(0.54)
Transportation	-0.18	-0.25	-0.23	-0.35	-0.21
	(0.09)	(0.10)	(0.12)	(0.18)	(0.25)
Other	-0.21	-0.15	0.05	-0.08	-0.88
	(0.23)	(0.28)	(0.31)	(0.47)	(1.00)
Social Expenditures per capita	1.16	1.30	1.36	2.26	1.91
	(0.61)	(0.71)	(0.86)	(1.33)	(2.10)
Observations (all)	8943	6707	4471	2235	894
Observations (effective)	4408	3714	2393	1137	420

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. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. In the heading, numbers below the percentiles are the corresponding population thresholds. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table H.4: RD estimates of the effect of a left-wing mayor: robustness to alternative bandwith selection

	Baseline		Subsamples							
			Lame Duck		Tiebout < median		Ideology distance > median		Oil windfall	
Outcome	MSE	CER	MSE	CER	MSE	CER	MSE	CER	MSE	CER
Size of government: overall revenues and expenses										
Expenditure per capita	-0.01	0.05	0.52	0.99	-1.24	-1.07	0.23	-0.07	2.26	3.35
	(0.56)	(0.61)	(1.27)	(1.25)	(0.85)	(0.91)	(0.82)	(0.91)	(2.06)	(2.22)
Expenditure, % of GDP	0.01	0.08	-1.05	-1.01	-0.36	-0.38	-0.02	-0.19	-0.13	0.04
	(0.24)	(0.21)	(1.18)	(1.21)	(0.43)	(0.47)	(0.25)	(0.39)	(0.47)	(0.48)
Revenue per capita	0.39	0.62	0.59	1.10	-0.77	-0.68	1.02	1.03	2.19	3.12
	(0.52)	(0.58)	(1.21)	(1.21)	(0.77)	(0.83)	(0.88)	(1.02)	(2.01)	(2.15)
Revenue, % of GDP	0.12	0.22	-1.14	-1.09	-0.20	-0.27	0.24	0.13	-0.21	0.09
	(0.25)	(0.23)	(1.20)	(1.23)	(0.42)	(0.48)	(0.27)	(0.42)	(0.49)	(0.50)
Allocation of resources: budget categories (% of total expenditure)										
Current Expenditure	-0.05	-0.12	-0.39	-0.47	-0.15	-0.17	0.18	0.10	1.01	1.18
	(0.17)	(0.18)	(0.38)	(0.41)	(0.25)	(0.27)	(0.31)	(0.34)	(0.65)	(0.68)
Public Investment	0.09	0.17	0.40	0.45	0.23	0.29	-0.08	-0.05	-0.96	-1.11
	(0.16)	(0.18)	(0.37)	(0.40)	(0.26)	(0.29)	(0.31)	(0.34)	(0.63)	(0.66)
Personnel	-0.05	-0.12	-0.81	-0.85	-0.04	-0.15	-0.08	-0.19	-0.34	-0.46
	(0.22)	(0.24)	(0.48)	(0.52)	(0.30)	(0.33)	(0.41)	(0.45)	(0.85)	(0.91)
Allocation of resources: functional categories (% of total expenditure)										
Social Expenditures	0.64	0.75	1.27	1.39	0.71	0.71	0.91	1.00	2.19	2.21
	(0.21)	(0.24)	(0.40)	(0.44)	(0.30)	(0.33)	(0.39)	(0.43)	(0.87)	(0.92)
of which:										
Health & sanitation	0.18	0.15	0.69	0.83	0.38	0.22	-0.11	-0.14	0.45	0.75
	(0.15)	(0.17)	(0.30)	(0.32)	(0.22)	(0.25)	(0.27)	(0.30)	(0.42)	(0.47)
Education & culture	0.24	0.37	0.31	0.32	0.12	0.11	1.00	0.97	0.83	0.88
	(0.16)	(0.18)	(0.30)	(0.32)	(0.19)	(0.21)	(0.30)	(0.35)	(0.50)	(0.54)
Social welfare	0.16	0.11	0.32	0.30	0.27	0.27	0.12	0.09	0.36	0.35
	(0.06)	(0.07)	(0.11)	(0.12)	(0.09)	(0.10)	(0.10)	(0.11)	(0.20)	(0.21)
Other Expenditures:										
Housing	-0.16	-0.19	-0.36	-0.40	0.05	0.12	-0.15	-0.07	0.41	0.43
	(0.13)	(0.14)	(0.29)	(0.32)	(0.21)	(0.23)	(0.24)	(0.26)	(0.65)	(0.70)
Transportation	-0.18	-0.18	-0.07	-0.01	-0.24	-0.21	-0.52	-0.58	-0.73	-0.75
	(0.09)	(0.10)	(0.20)	(0.21)	(0.13)	(0.15)	(0.19)	(0.21)	(0.29)	(0.31)
Other	1.16	1.51	3.34	4.19	0.25	0.43	1.92	1.83	6.48	7.10
	(0.61)	(0.67)	(1.36)	(1.39)	(0.86)	(0.93)	(0.93)	(1.04)	(2.40)	(2.64)
Social Expenditures per capita	1.16	1.51	3.34	4.19	0.25	0.43	1.92	1.83	6.48	7.10
	(0.61)	(0.67)	(1.36)	(1.39)	(0.86)	(0.93)	(0.93)	(1.04)	(2.40)	(2.64)
Observations (all)	8943	8943	2395	2395	4158	4158	3105	3105	919	919
Observations (effective)	4408	3136	1227	929	2367	1810	1660	1209	451	360

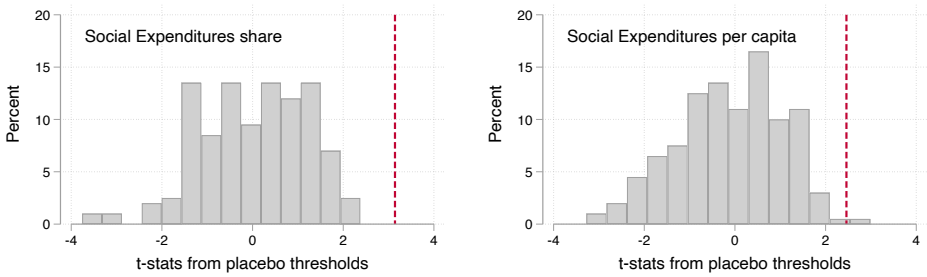
Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table H.5: RD estimates of the effect of a left-wing mayor in the Tiebout and ideology distance subsample

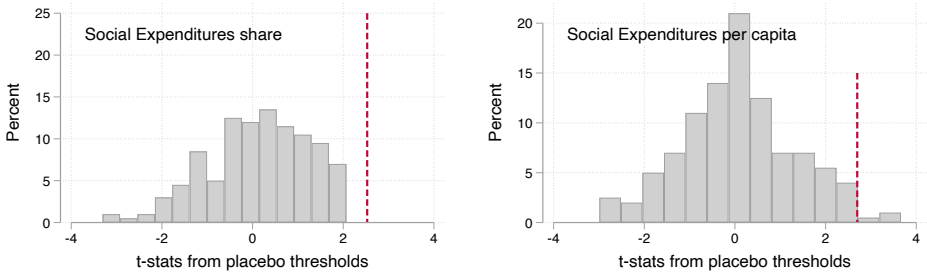
	Baseline	Tiebout competition		Ideology distance	
		< median	< 25th pct	> median	> 75th pct
Size of city government					
Expenditure per capita	-0.01	-1.24	-1.21	0.23	-0.90
	(0.56)	(0.85)	(1.22)	(0.82)	(1.13)
Expenditure, % of GDP	0.01	-0.36	-1.10	-0.02	0.43
	(0.24)	(0.43)	(0.89)	(0.25)	(0.40)
Revenue per capita	0.39	-0.77	-0.44	1.02	0.70
	(0.52)	(0.77)	(1.20)	(0.88)	(1.73)
Revenue, % of GDP	0.12	-0.20	-0.94	0.24	0.64
	(0.25)	(0.42)	(0.88)	(0.27)	(0.58)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05	-0.15	0.36	0.18	0.16
	(0.17)	(0.25)	(0.38)	(0.31)	(0.45)
Public Investment	0.09	0.23	-0.36	-0.08	0.05
	(0.16)	(0.26)	(0.43)	(0.31)	(0.43)
Personnel	-0.05	-0.04	0.32	-0.08	0.13
	(0.22)	(0.30)	(0.43)	(0.41)	(0.63)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64	0.71	0.20	0.91	0.78
	(0.21)	(0.30)	(0.41)	(0.39)	(0.49)
of which:					
Health & sanitation	0.18	0.38	-0.02	-0.11	-0.36
	(0.15)	(0.22)	(0.31)	(0.27)	(0.38)
Education & culture	0.24	0.12	-0.00	1.00	1.22
	(0.16)	(0.19)	(0.31)	(0.30)	(0.43)
Social welfare	0.16	0.27	0.15	0.12	0.14
	(0.06)	(0.09)	(0.11)	(0.10)	(0.14)
Other Expenditures:					
Housing	-0.16	0.05	0.07	-0.15	0.06
	(0.13)	(0.20)	(0.31)	(0.24)	(0.37)
Transportation	-0.18	-0.24	-0.15	-0.52	-0.13
	(0.09)	(0.13)	(0.18)	(0.19)	(0.24)
Other	-0.21	-0.59	-0.08	-0.22	-0.59
	(0.23)	(0.34)	(0.47)	(0.40)	(0.53)
Social Exp. per capita	1.16	0.25	-0.74	1.92	0.77
	(0.61)	(0.86)	(1.37)	(0.93)	(1.36)
Observations (all)	8943	4158	2081	3105	1545
Observations (effective)	4408	2367	1347	1660	814

Notes: Estimation of equation 2, using the Calonico et al. (2014) procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

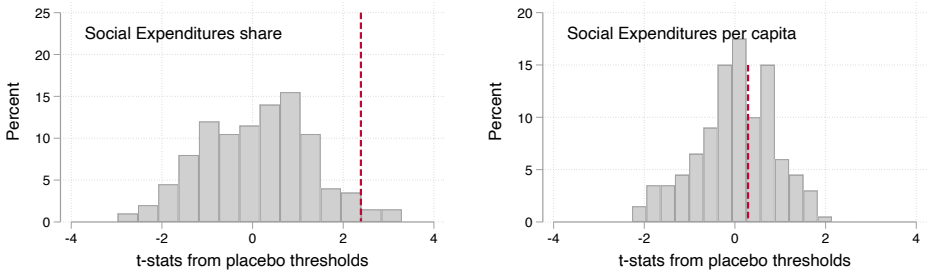
Figure H.1: Falsification test using placebo thresholds - effect on social expenditures, subsamples



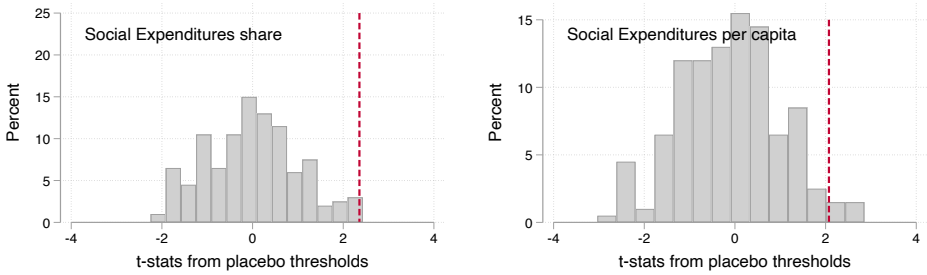
(a) Cities with a lame-duck mayor



(b) Cities experiencing oil windfalls



(c) Cities facing below-median Tiebout competition



(d) Cities with above-median coalition ideology distance

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 and social expenditure per capita, 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).

Appendix I RD estimates on welfare-related outcomes

To provide a broader picture of how partisanship affects policy in Brazilian cities, in this appendix we look at welfare-related outcomes provided by municipalities. First, we discuss the data sources and define the variables we use. Then, Appendix Table I.1 reports summary statistics and Appendix Table I.2 presents the regression discontinuity results.

Educational Outcomes

To assess the impact of partisanship on welfare-related outcomes in the area of education, we create two types of indicators using data from the *Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira* (INEP).

The first type of indicators is related to the infrastructure and human resources provided by city governments. We proxy infrastructure by the number of child care facilities, preschools and primary schools per a hundred thousand residents. For primary schools, we also compute the average classroom size, which is defined as the total number of students enrolled in 1st to 5th grades in municipal schools by the total number of classrooms available in the municipal school system for these grade levels. The (enrolled) students-to-teachers ratio is our measure of human resources available in the municipality. All these indicators are obtained from the School Census, an annual survey of all schools in Brazil, and only use data from municipal schools.

The second type of indicators attempts to capture the overall performance of the municipal educational system. In this case, we use data from the *Índice de Desenvolvimento da Educação Básica* (IDEB), which is an index created in 2005 to monitor student achievement and progression flows at primary and lower secondary education. IDEB assigns a score between 0 and 10 to individual schools and school systems. The index consists of two subindices: test scores and grade progression. The IDEB test scores are based on standardized average test scores on math and Portuguese from SAEB and Prova Brasil, which are national standardized exams administered every two years since 2005. These exams are taken by students in the last year of primary school, middle school and high school from schools with at least 20 students enrolled in that particular grade-level. The IDEB progression rate is defined as the inverse of the average time to complete a grade level and is obtained from the School Census (Fernandes, 2007).

IDEB is only available for the first and third year of a mayoral term. Given that it would be

really hard for mayors to affect any of these outcomes in the first year after the election, we use all indicators measured at the mayors 3rd year in office. As we pointed out in Section 2, cities are responsible for child care, primary and middle school education in Brazil. We focus on primary schools (1st to 5th grades), however, as many cities in our sample do not have middle schools in the municipal school system.

Health Outcomes

Similarly to education, we use two type of indicators to assess the impact of partisanship on welfare-related health outcomes. Also in this case, we use measures related to infrastructure and human resources and an overall measure of performance of the health care system in the city.

We proxy infrastructure in the municipal health care system by the number of clinics per a hundred thousand residents. We compute this outcome for clinics providing basic services (low complexity) and the total number of clinics. We do not use hospitals for two reasons. First, as discussed in Section 2, cities are responsible for the provision of basic health services. Second, most cities in our sample do not have a municipal hospital. We use two measures of human resources. The number of teams of the Family Health Program (ESF) in the city and the total number of doctors working for the municipal health-care system. ESF targets prevention and provision of basic health through the use of professional health-care teams directly intervening at the community level. Each team is assigned a predetermined number of families and focus on the provision of counseling, prevention, orientation related to recovery, and advice for fighting frequent diseases and for overall health protection in the community. It is important to note that ESF is run by the federal government and implementation requires voluntary adhesion of the municipal administration (Rocha and Soares, 2010). Both measures are scaled by a hundred thousand residents. To capture the overall performance of the municipal health-care system, we use the infant mortality rate defined as the number of infant deaths for every thousand live births.

All welfare-related outcomes of the health care system are obtained from DATASUS, a database from the Ministry of Health. All outcomes are computed as the mayoral term average, except for ESF which is measured at the last year in office due to data availability.

Law Enforcement Outcomes

Almost fifty eight thousand homicides were registered in Brazil in 2018 (the last year for which this statistic is available), which corresponds to an homicide rate of 27.8 homicides per hundred thousand people. The homicide rate was even higher in the previous four years (Cerqueira et al., 2020). We thus assess the overall performance in the law enforcement area using the homicide rate at the city level obtained from *Atlas da Violência* from the Institute for Applied Economic Research (IPEA).

Table I.1: Welfare-related outcomes – descriptive statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Education outcomes					
Student-teachers ratio	22.75	22.60	23.23	22.91	26.07
	(13.05)	(9.87)	(9.85)	(10.83)	(13.51)
Average classroom size	23.40	23.27	23.84	23.52	26.01
	(12.19)	(8.47)	(8.38)	(9.86)	(12.70)
Teachers, per 100K res.	352.98	351.00	342.17	341.68	353.47
	(158.59)	(163.78)	(152.74)	(156.78)	(139.77)
Schools, per 100K res.	92.40	92.74	79.93	89.05	105.78
	(90.70)	(92.23)	(87.54)	(86.78)	(86.72)
Child Care, per 100K res.	25.50	26.60	23.41	24.25	27.66
	(34.51)	(35.97)	(30.88)	(32.59)	(35.51)
Prepresch, per 100K res.	71.83	72.67	60.98	68.94	82.13
	(71.52)	(70.67)	(64.16)	(66.99)	(71.12)
IDEB test scores	5.18	5.18	5.28	5.17	4.92
	(0.88)	(0.87)	(0.88)	(0.87)	(0.83)
IDEB progression rate	0.89	0.89	0.90	0.89	0.84
	(0.09)	(0.08)	(0.08)	(0.08)	(0.10)
IDEB index	4.65	4.66	4.77	4.65	4.20
	(1.10)	(1.08)	(1.09)	(1.09)	(1.08)
Health outcomes					
Clinics (basic), per 100K res.	74.14	73.60	72.35	73.74	65.64
	(44.72)	(45.43)	(46.36)	(46.52)	(38.39)
Clinics (total), per 100K res.	118.07	118.02	121.79	120.57	98.95
	(81.71)	(79.82)	(85.11)	(85.71)	(64.33)
ESF team, per 100K res.	7.18	6.64	7.33	7.02	7.21
	(8.90)	(8.72)	(8.92)	(8.52)	(8.37)
Doctors, per 100K res.	44.90	46.07	54.94	47.06	53.78
	(53.31)	(53.80)	(63.60)	(55.34)	(66.94)
Infant mortality rate	15.38	15.21	15.05	15.16	15.90
	(6.94)	(6.76)	(6.47)	(6.52)	(6.15)
Law enforcement outcomes					
Homicide rate	20.18	20.05	22.03	21.07	24.53
	(16.41)	(16.27)	(17.27)	(16.75)	(18.71)

Notes: This table reports mean and standard deviation (in parenthesis) for welfare-related outcomes. Data on education outcomes are from INEP. Data on health outcomes are from DATASUS. Homicides rate are from IPEA. The number of observations available for each welfare-related outcome is presented in Appendix Table I.2. See Section 6 for the specific definition and motivation of each subsample.

Table I.2: RD estimates of the effect of a left-wing mayor on welfare-related outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
		Education outcomes			
Student-teachers ratio	-1.51	0.35	-1.08	-1.67	-2.72
	(1.05)	(2.13)	(1.49)	(1.70)	(3.34)
Average classroom size	-2.26	-2.91	-2.93	-3.95	-3.75
	(1.31)	(2.21)	(1.71)	(2.06)	(3.61)
Teachers, per 100K res.	0.32	-2.21	0.70	2.29	3.46
	(1.16)	(2.24)	(1.67)	(2.24)	(3.81)
Schools, per 100K res.	-3.43	1.90	-3.01	-4.41	0.90
	(1.27)	(2.70)	(1.76)	(2.15)	(3.68)
Child Care, per 100K res.	-1.49	7.80	0.87	-3.71	-6.80
	(2.66)	(5.40)	(3.52)	(4.26)	(8.14)
Pre Schools, per 100K res.	-1.45	-0.74	-6.70	0.07	-4.13
	(1.87)	(3.79)	(3.08)	(3.12)	(5.78)
Observations (all)	8872	2370	4125	3086	918
Observations (effective)	5298	1280	2306	1620	489
IDEB progression rate	0.33	0.39	0.78	0.61	2.01
	(0.38)	(0.70)	(0.50)	(0.51)	(1.20)
Observations (all)	8292	2221	3926	2905	901
Observations (effective)	3841	1070	1710	1894	390
IDEB test scores	-0.35	-0.01	-0.49	0.34	1.70
	(0.40)	(0.83)	(0.67)	(0.71)	(1.16)
IDEB overall index	-0.02	0.30	0.01	1.21	3.18
	(0.57)	(1.02)	(0.71)	(0.92)	(1.69)
Observations (all)	8211	2196	3894	2875	900
Observations (effective)	4385	1206	2531	1632	444

Table I.2: RD estimates of the effect of a left-wing mayor on welfare-related outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Health outcomes					
Clinics (basic), per 100K res.	0.80	-1.21	3.73	-0.20	-2.23
	(1.54)	(2.88)	(2.15)	(2.20)	(3.13)
Clinics (total), per 100K res.	0.34	-2.54	2.15	-0.05	-4.64
	(1.60)	(3.35)	(2.23)	(2.56)	(4.04)
Observations (all)	8937	2395	4155	3104	919
Observations (effective)	3998	985	1874	1765	455
ESF team, per 100K res.	-2.62	0.36	-0.70	-6.50	-2.79
	(2.88)	(6.52)	(4.52)	(5.58)	(8.96)
Observations (all)	4120	1069	2151	1522	517
Observations (effective)	2579	532	1275	843	282
Doctors, per 100K res.	-0.60	3.67	-13.39	4.06	-9.61
	(3.37)	(6.81)	(5.57)	(5.55)	(10.27)
Observations (all)	8033	2132	3860	2850	860
Observations (effective)	4379	1132	1617	1726	357
Infant mortality	-2.75	-1.95	0.60	2.87	-6.14
	(2.26)	(3.86)	(2.92)	(3.42)	(5.68)
Observations (all)	8656	2313	4065	3012	914
Observations (effective)	4525	1357	2114	1565	472
Law enforcement outcomes					
Homicide rate	-4.76	2.06	5.56	5.27	-6.00
	(2.93)	(5.53)	(3.90)	(3.95)	(8.18)
Observations (all)	8187	2181	3895	2884	895
Observations (effective)	3849	1003	2004	1764	471

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. Education outcomes measured in the 3rd year in office. Homicide rates and health outcomes, except for number of ESF teams, measured as 4-year averages over a mayoral term. ESF teams measured in the 4th year in office. All welfare-related outcome variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.