Eleições em dois turnos, competição política e gasto público

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Resumo

Nesse trabalho, busco responder a duas perguntas: (1) Eleições em dois turnos, comparadas a eleições de turno único, aumentam a competição política? (2) O aumento de competição política relacionado às eleições em dois turnos causa indiretamente incremento na quantidade dos bens providos pelo Estado? Exploramos a descontinuidade advinda da atribuição de eleições em dois turnos apenas aos municípios brasileiros com mais de 200 mil eleitores e aplico um desenho de regressão descontínua *sharp* para responder empiricamente ambas as questões. Os resultados indicam que eleições em dois turnos geram aumento de competição política, evidenciado por um efeito médio local de tratamento positivo de 0,4 candidato efetivo e 5 pontos percentuais na parcela de votos recebidas pelos candidatos classificados em terceiro lugar ou pior. No entanto, não encontro evidências conclusivas de um efeito das eleições em dois turnos sobre provisão de bens pelo Estado, pois não identifico descontinuidades em gastos em (1) saúde e saneamento, (2) educação e cultura, (3) investimento público e (4) previdência e assistência sociais, expressos tanto em nível quanto em percentual do gasto total.

Palavras-chave: eleições em dois turnos, gasto social, gasto público, competição política, política local, Brasil.

Abstract

This paper addresses two questions: (1) Do dual-ballot elections (DBE) intensify political competition compared to single-ballot elections (SBE)? (2) Do DBE indirectly augment the level or quality of publicly provided goods due to an increase in political competition? I exploit the discontinuity in the assignment of DBE for mayoral elections only to Brazilian municipalities with more than 200,000 voters to give empirical answers to these questions using a sharp regression discontinuity design. Our results show a positive local average treatment effect on political competition: 0.4 effective candidate and 5 percentage points on the vote share of third and lower placed candidates. Nonetheless we do not find conclusive evidences of any DBE effect on public spending as there are no discontinuities in expenses in (1) health and sanitation, (2) education and culture, (3) public investment, and (4) pensions and social assistance (as levels or shares of total expenses).

Abstract: runoff elections, social spending, public spending, political competition, local politics, Brazil.

1. Introduction

Elections are the most important formal mechanism of popular control of government in representative democracies. Nonetheless, although the political consequences of electoral rules are an established research area in Political Science, the impact of these institutions on public policies and welfare are not well understood. I investigate these topics with two questions: (1) Do dual-ballot elections (DBE)[2], compared to their single-ballot counterparts (SBE)[3], increase political competition? (2) Do DBE, by raising political competition, indirectly cause an augmentation of the levels of publicly provided goods[4]? I exploit the discontinuity in the assignment of DBE rule for mayoral elections in Brazilian municipalities with more than 200 thousand voters as a source of exogenous variance in order to give empirical answers to these questions.

A notorious statement about the political implications of electoral rules is Duverger's Law: simple majority SBE benefit two-party systems and DBE favor multipartism (Duverger, 1954). The reasoning for this prediction is that SBE create incentives for strategic voting, as the rational choice for a voter would be to cast a ballot for one of the two candidates with the highest probabilities of winning the poll, rather than her ideally preferred but inviable one. However, if there is the possibility of a second round, a strategic voter may find it suitable to vote for a third candidate during the first turn in order to maximize the probability that the third option gets to the second round. In equilibrium, parties would be encouraged to enroll their candidates in the election, and political competition should increase.

There are theoretical reasons to suppose political competition makes politicians more responsive to public welfare as well. Ferejohn (1986) affirms political competition motivates incumbents to provide public goods because the voters punish politicians they judge as bad performers with no reelection. Besley & Burgess (2002) propose a game theoretic model in which opportunistic incumbents put more effort in providing public

[2] Dual-ballot is also called runoff elections.
[3] Single-ballot elections are also called plurality, simple majority or first-past-the-post.
[4] Throughout the paper, I use "publicly goods" as a synonym to publicly provided goods. Samuelson (1954) defined a "collective consumption goods" as a good that is consumed by all individuals in the same quantity. Following this definition, most Economists understand a good is "public" if its consumption by someone does not diminish the consumption of anybody else (non-rivalry), and if none can be excluded from its consumption (non-excludability). "Publicly provided goods" are goods provided by the state, even if they do not correspond technically to the former definition. For the sake of brevity, I use both terms as synonyms.

services if they have lower advantages in winning elections[5] (i.e., if political competition is higher) because users of those services are inclined to vote for the reelection of incumbents when they notice the public goods they need are supplied. Given this theoretical background, we have two hypothesis for our theme of concern.

Hypothesis 1. DBE increase political competition in comparison with SBE.
Hypothesis 2. Political competition increments due to DBE raise the supply of public goods.

Previous studies use the fact that only municipalities above the threshold of 200,000 voters have runoff elections for mayors, while the smaller municipalities have single-ballot plurality elections, as a natural experiment and validate the previsions of Duverger's Law and Hypothesis 1 for Brazil. Chamon, Mello & Firpo (2009) use a Regression discontinuity design (RDD) with linear and quadratic functions and weighted least squares (WLS), and a sample of 1996, 2000, and 2004 elections and show that around the threshold of 200,000 voters there is an increase of 34% on vote concentration measured by the number of effective candidates[6] (from the average of 4.67) and of 131% in the vote share of third or lower placed candidates (from the average of 18.35%).

Fujiwara (2011) shows there is increased competition due to runoff elections with the sample of 1996, 2000, 2004, and 2008 elections and linear and quadratic function specifications. The vote share of third and lower placed candidates increase from 15% to 23% around the 200,000 threshold, and the vote share of fourth and lower placed candidates increase from 4% to 8%. This paper gives a technical contribution to that literature by enlarging the sample and including 2012 elections. This procedure is

[5] The measure they use to empirically study the effect of political competition is the opposite of the difference between the shares of seats of the Congress party (the dominant party) and the second biggest party in the Indian Congress for each state. India is a parliamentary democracy; most members of the lower house (*Lok Sabha*) are elected by plurality in single-member districts; and most members of the upper house (*Rajya Sabha*) are elected indirectly by state and territorial assemblies. Only one third of the positions of the upper house are subject to elections every two years. The authors do not give any reason to suppose this measure of legislative competition correspond to the theoretical idea of electoral uncertain support for incumbents of their model.

[6] The number of effective candidates is a widely used measured of political competition in Political Science. Its definition is the inverse of the sum of squares of share of votes received by each candidate and it is equal to the inverse of Herfindahl-Hirschman index.

important because it enables me to assess previous results that lacked statistical significance many times.

The relation between political competition and public policies is more controversial. Many studies find positive consequences of political competition on welfare: increased calamity relief expenditures and public food distribution in Indian states (Besley & Burgerss, 2002); higher school enrollment rate and free immunizations (Arvate, 2013), higher expenditures in public investment, and lower levels of current and payroll expenses in Brazilian municipalities (Chamon, Mello & Firpo, 2009); higher public investment in infrastructure, higher economic growth rates and higher incidence of pro-growth policies (low taxes and right-to-work laws) in American states (Besley, Persson & Sturm, 2010); and larger social expenditures in a cross-section with 18 industrial democracies (Hicks & Swank, 1992). Nevertheless, Cleary (2007) does not find any relation between political competition and water or sewage coverages among Mexican municipalities, possibly because of voters' lack of information, clientelistic relations or expenses in visible but questionable public works. Boulding & Brown (2013) discover a negative correlation between social spending and political competition in Brazilian municipalities, which they explain by budgetary issues. Municipalities with strong budgetary constraints are not able to provide adequate level of public goods and rotate election winners because of permanent electoral dissatisfaction.

These studies generally use OLS, panel analysis, and instrumental variables to identify the effects of political competition. The weakness of these approaches is the possibility of bias due to correlation between omitted variables and the error term or the instrument. Although Chamon, Mello & Firpo (2009) use RDD techniques that mitigate this issue, WLS is very sensitive to manipulation of treatment in RDD settings. They also use and a fuzzy design to instrument political competition with dual-ballot rule. To be a valid instrument, DBE should affect public spending only through political competition, an untested assumption[7].

[7] Bordignon, Nannicini & Tabellini (2016) find another possible causal pathway for Italian municipalities, where DBE moderates political extremism and policy volatility. If most voters are moderate but their bliss policies are closer to those of extreme parties and electorates than to the center of policy space, runoff elections make it possible for moderate parties to reach the second turn without compromising their platforms with alliances and bargaining with extreme parties.

Brazilian municipalities are a good case to study because there are several indicators that DBE rule is indeed a quasi-random sources of exogenous variation: voter enlisting and elections are a responsibility of Judiciary branches independent from mayors; turnout is high; voting is mandatory; and voting machines count polls. Control and treatment groups seem to be comparable as there are no discontinuities in individual party entry probability, turnout and registration rates, candidates' occupational skills or educational quality levels, and share of seats received by each party in local legislators' elections (Fujiwara, 2011). I provide tests that refuse the hypothesis of manipulation of treatment assortment and do not refuse quasi-random assignment as well.

To test Hypothesis 2, differently from Chamon, Mello & Firpo (2009) who use outcomes during election year or one year before the election, I use the average of four categories of public spending during the three years after as a dependent variable: (1) health and sanitation, (2) education and culture, (3) public investment, and (4) pensions and social assistance (as levels or shares of total expenses). My results thus mitigate the impact of electoral cycles (Sakurai & Menezes-Filho, 2010) on the estimates and refer to medium-run trends along the mandate, not just during the election years.

Our results validate Hypothesis 1 as there is a positive local average treatment effect of 0.4 effective candidate and 5 p.p. on the vote share of third and lower placed candidates, but the reduced form estimates on public spending that test Hypothesis 2 are inconclusive. Although DBE raise political competition, there is no evidence that this positive variation is reflected in higher levels of public spending. The next session details the empirical strategy used for identification; section 3 summarizes the data used in the sample; section 4 exhibits the tests of quasi-randomness and main results; and section 5 concludes.

2. Empirical Strategy

Brazilian more than 5,000 municipalities are the smallest autonomous level of government. Each municipality has a single mayor (*prefeito*), elected every four years in regular predefined calendars. The Brazilian Constitution of 1988 (art. 29, II) establishes mandatory runoff elections for mayor office in municipalities with more than 200,000 voters when the first-turn winner candidate achieves a plurality smaller than 50% of

valid votes[8]. For municipalities under that threshold, plurality single-ballot elections apply for mayor office.

Electoral rules are federal. First turn elections are held in October, and second turn polls in November. All municipal elections for mayors use plurality rule, with no electoral district subdivisions. Moreover, elections are regulated and organized by a branch of the Judiciary System (*Justiça Eleitoral*[9]), composed of a federal entity, *Tribunal Superior Eleitoral* (TSE), and 27 state entities, *Tribunais Regionais Eleitorais* (TRE), that execute mayoral elections and register the headcount of voters.

Given that in Brazil, a Judiciary branch independent of local mayors organizes elections, voting is mandatory, and attendance is higher than 80% in most municipalities (Fujiwara, 2011), the discontinuity in SBE-DBE provides a credible source of exogenous variation to estimate the impact of electoral rules in political competition and policy outcomes. It is very unlikely that municipalities manipulate the headcount of voters or polls results because that would require large-scale frauds. In addition, since 1996, vote casting and poll counting use voting machines massively.

We can consider Brazilian mayoral runoff elections as a quasi-random treatment in order to estimate local average treatment effects on political competition, directly, and on policy outcomes, in reduced form, using RDD. For both cases, we use the following estimating equation:

$$Y_{it} = \sum_{k=0}^{p} \delta_k (P_{it}^*)^k + D_{it} \sum_{k=0}^{p} \gamma_k (P_{it}^*)^k + \sum_{t=1}^{4} \alpha_t d_t + \sum_{j=1}^{25} \beta_j d_{ji} + \varepsilon_{it}$$

For all variables, the subscripts i and t represent municipality and election year counters, respectively. Y_{it} is a dependent variable of political competition or public policy outcome, $P_i^* = P_i - 200{,}000$ is the number of voters centered at the 200,000 cutoff, D_{it} is a treatment dummy that indicates 1 if $P_i \geq 200.000$, d_t are time dummies and d_{ji} are the state dummies that indicate in which state the municipality is located[10],

[8] Votes cast to parties or candidates are considered valid. This definition thus excludes votes in blank or in non-existent candidates.
[9] Brazilian Electoral Justice.
[10] Brazil has 27 federated units which are 26 states and 1 federal district (DF). Because the only city of DF is Brasília, the federal capital city, and it has a governor, not a mayor; it is not included. In order to avoid perfect multicollinearity, only 25 state dummies are included.

ε_{it} is a random disturbance, $p \in \{1,2,3\}$ is the polynomial order selected to serve as the functional form of the RDD. Sometimes, with due disclaimer, state dummies were excluded from the regression. Besides that, P_i must be restrict to the subsample close to the neighborhood of the cutoff. We formalize this requisite in the following equation, in which h can assume the values 25,000; 50,000; and 75,000 for different specifications.

$$P_i \in [200.000 - h, 200.000 + h]$$

If the treatment assignment is truthfully quasi-random and the specification is correct, the local average treatment effect is unbiasedly identified by the estimated parameter $\widehat{\gamma_0}$. The inference for all regressions considers robust clustered standard errors in municipalities to correct for temporal autocorrelation in each municipality.

For the dependent variable (Y_{it}), in different estimations we use the number of effective candidates and the vote share of third or lower placed candidates as proxies for political competition[11], and current, payroll, public investment, health and sanitation, education and culture, and social assistance and pensions expenses, both in logarithm and as a share of total expenses. Spending is a convenient proxy for public goods provision because it is under relative direct control of the mayor and it is not as much contaminated by state capacity, bureaucratic management, and lagged variable concerns as indicators of direct public service provision. However, a weak point of this measure is that it does not correspond necessarily to population welfare induced by government action. Ideally, to measure social spending, we would be able to consider the effects on health, education and social assistance isolated, but that breakup is not possible before 2002. In order to verify the plausibility of the assumption of quasi-random assignment, we test the existence of discontinuities using the same RDD on per capita income, Gini coefficient, female and rural population share, transfer revenues and total revenues.

In every regression concerning public policy outcomes, the value of the dependent variable on election year t is the average of that variable in the three years following the election. With this procedure, we mitigate the impact of electoral cycles on public policy by not considering the election year itself. That procedure avoids the capture of

[11] The vote share of third or lower placed candidates is a proxy related to the idea that DBE allow strategic voters to cast votes to candidates with the third highest probability of passing the first turn, as discussed in the introductory section of this paper.

raises of expanses during the electoral year for reelection purposes. As the reduced form concerns the indirect impact of runoff on policy outcomes through political competition, it is important to use outcomes posterior to the election year, in order to avoid reverse causality. Because elections are only held in intervals of four years, the averaging of the variable also mitigates the impact of public policy volatility on the estimates in order to capture medium-run relationships. This contribution differentiates our studies from Chamon, Mello & Firpo (2009), because they use the level of dependent variable during the election year itself.

3. Data

Our sample consist of data on elections, policy, economic and political variables on Brazilian municipalities covering the 5 mayoral mandates subsequent to 1996, 2000, 2004, 2008, and 2012 elections. Data on spending and revenues is available from the online database *Finanças do Brasil - Dados Contábeis dos Municípios*, from the National Treasury (*Secretaria do Tesouro Nacional*). Data on electoral results and voters headcount are available from *Tribunal Superior Eleitoral* in the online database *Repositório de Dados Eleitorais*. Data on income per capita, Gini coefficient, female and rural population shares used for quasi-random assignment tests are available from Brazilian Census for years 1991, 2000 and 2010, from *Instituto Brasileiro de Geografia e Estatística*. We assign the closest anterior value on Census to data on each election year, i.e. electoral data on 1996, 2000, 2004, 2008, and 2012 are matched to Census data on 1991, 2000, 2000, 2000, and 2010, respectively. We matched data between different databases using the municipalities and states names. Spending and revenue data were deflated to 2016 prices using official inflation (*Índice de Preços ao Consumidor Amplo*).

The number of municipalities in Brazil oscillates slightly because of emancipation or fusion of small municipalities out of the neighborhood of the cutoff. In 2010, there were exactly 5,564 municipalities listed in the national Census. Table 1 displays some descriptive statistics for Brazilian municipalities. Panel A includes the entire population of municipalities, Panel B includes only those for which the electorate is between 125,000 and 200,00 voters and Panel C only those for which the electorate is between 200,000 and 275,000. In the specifications with the widest bandwidths, the group of

municipalities on Panels B and C are equivalent to control and treatment groups respectively.

Table 1. Summary statistics

Variable	Mean	Std. dev.	Minimum	Maximum
Panel A: All municipalities				
Per capita income (R$)	349.72	212.35	33.24	2043.74
Gini coefficient	0.532	0.072	0.270	0.920
Female population share	0.493	0.015	0.189	0.544
Rural population share	0.423	0.244	0.000	1.000
Number of voters	22,824	146,834	501	8,619,170
Vote share of 3rd and lower placed	0.146	0.119	0.000	0.573
Number of effective candidates	2.170	0.548	1.000	6.153
Transfer revenues (R$mi.)	46.10	241.00	1.46	17300
Total revenues (R$mi.)	68.90	608.00	1.59	51700
Total expenses (R$mi.)	66.70	596.00	1.83	50000
Current expenses (R$mi.)	58.40	513.00	1.34	43300
Payroll expenses (R$mi.)	51.20	422.00	0.00	33600
Investments (R$mi.)	6.65	59.50	0.00	4860
Education and culture exp. (R$mi.)	17.50	124.00	0.00	10300
Health and sanitation exp. (R$mi.)	16.40	124.00	0.00	9020
Social assistance and pensions exp. (R$mi.)	5.51	92.20	0.00	8900
Panel B: Municipalities with more than 125,000 and less than 200,000 voters (Control group)				
Per capita income (R$)	641.06	219.81	204.43	1161.68
Gini coefficient	0.531	0.055	0.390	0.670
Female population share	0.511	0.008	0.494	0.532
Rural population share	0.050	0.059	0.000	0.290
Number of voters	155,848	21,362	125,081	199,692
Vote share of 3rd and lower placed	0.159	0.116	0.000	0.427
Number of effective candidates	2.515	0.640	1.150	4.416
Transfer revenues (R$mi.)	334.00	168.00	7.02	1340
Total revenues (R$mi.)	507.00	281.00	25.20	2600
Total expenses (R$mi.)	489.00	256.00	27.10	2230
Current expenses (R$mi.)	427.00	219.00	25.60	2070
Payroll expenses (R$mi.)	379.00	185.00	23.30	1500
Investments (R$mi.)	51.20	49.60	1.54	571
Education and culture exp. (R$mi.)	128.00	73.40	1.95	674
Health and sanitation exp. (R$mi.)	131.00	80.20	4.25	553
Social assistance and pensions exp. (R$mi.)	29.90	20.00	0.93	123

(Continued)

Table 1. Summary statistics *(Continued)*

Variable	Mean	Std. dev.	Minimum	Maximum
Panel C: Municipalities with more than 200,000 and less than 275,000 voters (Treated group)				
Per capita income (R$)	760.49	277.24	271.63	1866.58
Gini coefficient	0.535	0.052	0.420	0.620
Female population share	0.514	0.007	0.499	0.532
Rural population share	0.033	0.037	0.000	0.182
Number of voters	233,637	21,548	200,203	274,256
Vote share of 3rd and lower placed	0.196	0.133	0.000	0.498
Number of effective candidates	2.706	0.845	1.122	4.899
Transfer revenues (R$mi.)	484.00	231.00	105.00	1330
Total revenues (R$mi.)	802.00	404.00	197.00	2610
Total expenses (R$mi.)	765.00	363.00	197.00	2270
Current expenses (R$mi.)	672.00	326.00	169.00	2040
Payroll expenses (R$mi.)	588.00	276.00	114.00	1770
Investments (R$mi.)	77.60	55.00	7.13	324
Education and culture exp. (R$mi.)	186.00	98.40	45.10	747
Health and sanitation exp. (R$mi.)	210.00	119.00	28.70	596
Social assistance and pensions exp. (R$mi.)	60.10	43.40	3.32	193

We can notice that municipalities in both treated and control groups are much richer than national average, but evenly unequal. That is not astonishing as most Brazilian municipalities are much smaller than those considered in our restricted sample. Some of the observations have a share of votes for third or lower placed candidate equal to zero because those municipalities had elections with less than three candidates. A similar situation occurs if the number of effective candidates is equal to one, which means that the same candidate received all votes. These outliers are not elements of our groups of interest. Apparently, on average, the treatment and control groups are similar in Gini coefficient and female populations shares, but different in other economic, fiscal and political aspects. I will explore these comparisons further in the next session.

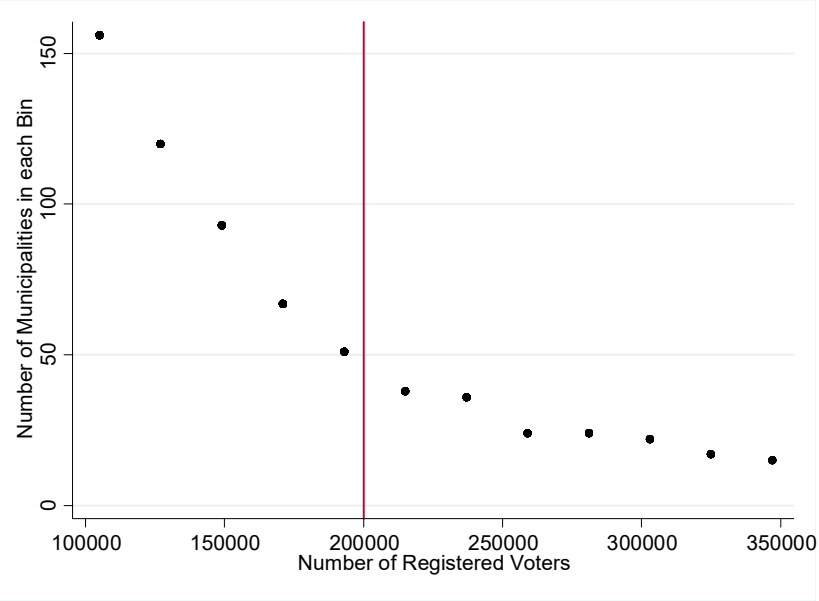
The missing data in any of the databases and in any of the years never surpasses 5% of the population and are very mostly concentrated at the smallest cities, so that should not impact my estimates.

4. Results

The idea of quasi-random assignment is that as the treatment assignment is exogenous, treatment and control groups should be similar in every single aspect *ex ante* (i.e. before treatment). That condition could be compromised if municipalities were able to use strategic manipulation of the treatment for whatever reason, for examples, if mayor were able to avoid the registration of voters to prevent the change to DB elections.

We test if there is evidence of manipulation with the exercise prescribed by McCrary (2008). If strategic manipulation happened, it would probably provoke a jump in the number of observations combined in bins of voters around the threshold. In Figure 1, we see that is not the case.

Figure 1. Distribution of electorate size.



We also perform the regression of the number of voters in each bin (of 22,000 voters) with the RDD described in section 2. The estimated coefficients are numerically small and change signs in different specifications, so there is no strong evidence of strategic manipulation. Throughout the paper, we present the mean of dependent variable in SB group (electorate between 175,000 and 200,000 voters) for each regression to clarify the relative size of treatment effect. To make this exercise easier to

read, we always present these means at variable levels or share (if the dependent variable is a percentage), never in logarithm.

Table 2. Test of manipulative assortment of treatment

Specification	SB mean	Linear	Linear	Linear	Quad.	Quad.	Quad.
Bandwidth		25.000	50.000	75.000	25.000	50.000	75.000
Dependent variable		(1)	(2)	(3)	(4)	(5)	(6)
Number of municip.	56,819	-0.601	-0.0376	4.186***	-1.843	-4.699***	-3.646**
in each bin		(1.692)	(1.471)	(1.124)	(1.616)	(1.674)	(1.517)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		111	233	383	111	233	383

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.
Robust standard errors clustered at municipality levels in parenthesis. Each numbered column represents the treatment effect (of a change from SB to DB) on the dependent variable in a different polynomial regression with specified bandwidth. SB mean is the mean of dependent variable among municipalities with 175,000 to 200,000 voters.

Table 3 presents the treatment effect for a number of economic variables: per capita income, Gini coefficient, female population share, rural population share, transfer revenues and total revenues. The regressions control for year fixed effects. As the coefficients estimated for the third degree term of the cubic specifications are not significant and results are qualitatively similar, we omit this specification. If the treatment is indeed quasi-random, we should not find discontinuities between the groups on these variables. It is especially important to test for transfer revenues differences because they represent flows from other federative entities that account for more than 3/5 of total revenues of municipalities in the control group. Overall, we find evidence that municipalities in the two groups are similar and comparable on average without any risk of selection bias.

Table 3. Tests of quasi-randomness

Specification	SB mean	Linear	Linear	Linear	Quad.	Quad.	Quad.
Bandwidth		25.000	50.000	75.000	25.000	50.000	75.000
Dependent variable		(1)	(2)	(3)	(4)	(5)	(6)
Per capita income	649.04	68.17	72.42	65.97*	-28.65	35.52	40.43
(R$)		(71.34)	(45.35)	(38.28)	(94.17)	(66.02)	(62.71)
Gini coefficient	0.533	-0.00575	-0.00418	-0.00338	0.0120	-0.00649	-0.0114
		(0.0176)	(0.0116)	(0.00993)	(0.0230)	(0.0156)	(0.0135)
Female population	0.513	-0.00334	-0.00169	0.000481	-0.00244	-0.00260	-0.00333
Share		(0.00266)	(0.00173)	(0.00161)	(0.00357)	(0.00243)	(0.00212)
Rural population	0,049	-0.0129	-0.0127	-0.0105	0.00559	-0.0208	-0.0176
Share		(0.0205)	(0.0134)	(0.0128)	(0.0214)	(0.0189)	(0.0147)
Log of transfer	327.526.655	-0.113	0.0595	0.0685	0.00661	-0.107	-0.0236
revenues (R$)		(0.160)	(0.0839)	(0.0698)	(0.271)	(0.133)	(0.105)
Log of Total	512.396.860	-0.0239	0.0979	0.0801	0.0732	-0.0308	0.0342
revenues (R$)		(0.148)	(0.0887)	(0.0690)	(0.241)	(0.127)	(0.108)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		111	233	381	111	233	381

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Robust standard errors clustered at municipality levels in parenthesis. Each numbered column represents the treatment effect (of a change from SB to DB) on the dependent variable in a different polynomial regression with specified bandwidth. SB mean is the mean of dependent variable among municipalities with 175,000 to 200,000 voters, and it is always displayed in level (not in logarithm).

Table 4 presents the treatment effect on political competition variables. Panel A controls for year fixed effects and Panel B for year and state fixed effects. Most specifications estimate a positive effect of the transition to DB, or, if negative, numerically close to zero, overall in concordance with Duverger's Law, but the estimate is not significant in all the specifications. Because RD designs are very sensitive to functional form specification and the selected bandwidths generate small samples and most estimates are close to 0.4 on the number of effective candidate and 5 p.p. on the vote share of third or lower placed candidate, we can assume there is evidence of a positive treatment effect. Figure 2 permit the graphical visualization of these discontinuities.

Table 4. Treatment effects on political competition

Specification	SB mean	Linear	Linear	Linear	Quad.	Quad.	Quad.
Bandwidth		25.000	50.000	75.000	25.000	50.000	75.000
Dependent variable		(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Controlled by year fixed effects							
Numer of effective	2,481	0.282	0.421*	0.369**	0.441	0.405	0.478*
Candidates		(0.318)	(0.218)	(0.171)	(0.429)	(0.327)	(0.257)
Vote share of 3rd	0,161	0.0331	0.0519	0.0592**	0.0592	0.0529	0.0608
and lower placed		(0.0548)	(0.0353)	(0.0273)	(0.0805)	(0.0553)	(0.0435)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Controlled by year and state fixed effects							
Numer of effective	2,481	-0.0180	0.0506	0.0550**	-0.0433	0.0355	0.0554
Candidates		(0.0698)	(0.0346)	(0.0261)	(0.0971)	(0.0610)	(0.0421)
Vote share of 3rd	0,161	-0.0356	0.417*	0.354**	-0.134	0.391	0.473*
and lower placed		(0.377)	(0.212)	(0.165)	(0.519)	(0.350)	(0.246)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		89	193	324	89	193	324

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.
Robust standard errors clustered at municipality levels in parenthesis. Each numbered column represents the treatment effect (of a change from SB to DB) on the dependent variable in a different polynomial regression with specified bandwidth. SB mean is the mean of dependent variable among municipalities with 175,000 to 200,000 voters.

Figure 2. Treatment effect on political competition.

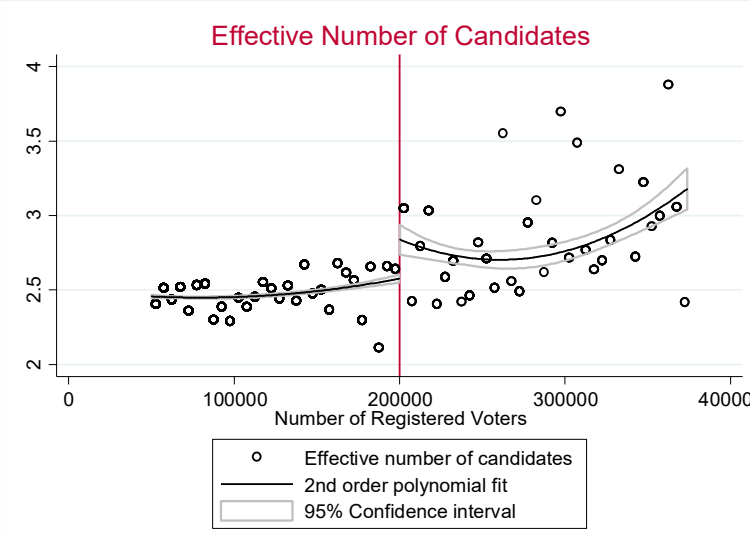
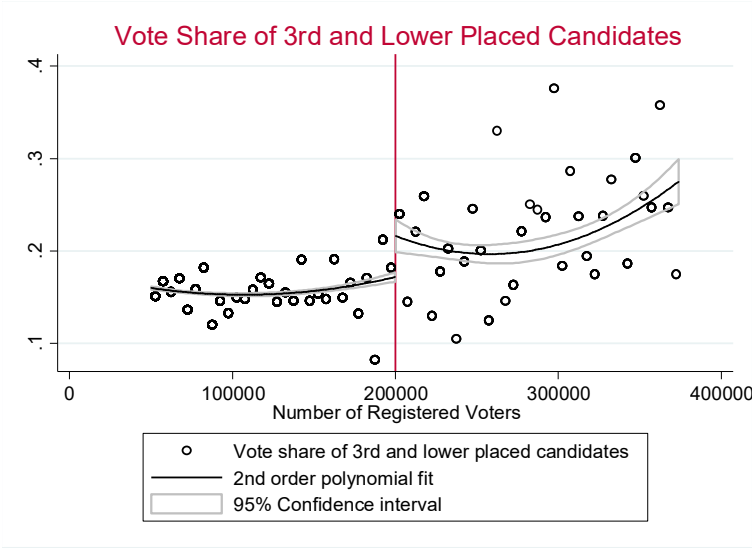


Figure 3. Treatment effect on political competition *(Continued)*.



Now we proceed to the reduced-form estimates of the treatment effect on public spending on Table 5, which controls for year fixed effects, and Table 6, which includes year and state fixed effects. The dependent variables are current, payroll, investment, health and sanitation, education and culture, and social assistance and pensions spending, both in logarithm and as a percentage of total expenditures.

Table 5. Reduced-Form Estimates of Treatment Effect on Public Spending

Controlled for Year Fixed Effects

Specification	SB mean	Linear	Linear	Linear	Quad.	Quad.	Cubic
Bandwidth		25.000	50.000	75.000	25.000	75.000	25.000
Dependent variable		(1)	(2)	(3)	(4)	(5)	(6)
Log of current exp.	438.806.895	-0.181	-0.0136	0.0215	-0.0801	-0.0734	-0.160
(R$)		(0.160)	(0.0900)	(0.0647)	(0.268)	(0.102)	(0.318)
Current expanses	0.879	-0.00947	-0.00729	-0.00500	0.00212	-0.00863	0.0652
(share of total exp.)		(0.0235)	(0.0115)	(0.00973)	(0.0370)	(0.0138)	(0.0421)
Log of payroll exp.	386.278.821	-0.207	-0.0190	0.0316	-0.102	-0.0906	-0.111
(R$)		(0.157)	(0.0935)	(0.0720)	(0.262)	(0.0992)	(0.303)
Payroll expenses	0.778	-0.0272	-0.0102	-0.000130	-0.0132	-0.0174	0.0964**
(share of total exp.)		(0.0270)	(0.0146)	(0.0150)	(0.0422)	(0.0174)	(0.0464)
Log of investiments	44.752.595	-0.136	0.0674	0.0917	-0.114	0.0334	-0.831
(R$)		(0.264)	(0.132)	(0.102)	(0.382)	(0.169)	(0.509)
Investiments	0.101	0.00360	0.00279	0.00102	-0.00453	0.00508	-0.0658
(share of total exp.)		(0.0222)	(0.0113)	(0.00965)	(0.0364)	(0.0138)	(0.0463)
Log of education and	126.091.703	-0.124	0.0187	0.00386	0.163	-0.0522	0.0122
culture exp. (R$)		(0.208)	(0.0978)	(0.0680)	(0.314)	(0.119)	(0.372)
Education and culture	0.261	0.0188	0.00882	-0.00558	0.0667**	0.00520	0.0676*
(share of total exp.)		(0.0213)	(0.0136)	(0.0121)	(0.0299)	(0.0164)	(0.0377)
Log of health and	126.091.703	-0.289	-0.00330	0.0760	-0.359	-0.121	-0.591
sanitation exp. (R$)		(0.238)	(0.129)	(0.0982)	(0.368)	(0.151)	(0.419)
Health and sanitation	0.248	-0.0213	0.00528	0.0146	-0.0657	-0.0104	-0.0747
(share of total exp.)		(0.0299)	(0.0154)	(0.0145)	(0.0452)	(0.0197)	(0.0471)
Log of social assist.	29.427.495	-0.0918	0.0433	0.186	-0.0541	-0.0741	-0.303
and pensions exp. (R$)		(0.266)	(0.171)	(0.124)	(0.481)	(0.186)	(0.565)
Social assistance and	0.067	0.00294	0.000849	0.00730	-0.00444	-0.00309	-0.0134
pensions (share of total)		(0.0126)	(0.00797)	(0.00665)	(0.0207)	(0.00930)	(0.0212)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		111	233	381	111	381	111

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Robust standard errors clustered at municipality levels in parenthesis. Each numbered column represents the treatment effect (of a change from SB to DB) on the dependent variable in a different polynomial regression with specified bandwidth. SB mean is the mean of dependent variable among municipalities with 175,000 to 200,000 voters, and it is always displayed in level (not in logarithm).

Table 6. Reduced-Form Estimates of Treatment Effect on Public Spending Controlled for Year and State Fixed Effects

Specification	SB mean	Linear	Linear	Linear	Quad.	Quad.	Cubic
Bandwidth		25.000	50.000	75.000	25.000	75.000	25.000
Dependent variable		(1)	(2)	(3)	(4)	(5)	(6)
Log of current exp.	438.806.895	-0.0699	0.0823	0.0619	-0.00458	0.00932	-0.245
(R$)		(0.148)	(0.0915)	(0.0707)	(0.249)	(0.112)	(0.297)
Current expanses	0.879	-0.0162	-0.0103	-0.00612	-0.00811	-0.0138	0.0552
(share of total exp.)		(0.0198)	(0.0112)	(0.0103)	(0.0327)	(0.0143)	(0.0338)
Log of payroll exp.	386.278.821	-0.104	0.0700	0.0746	-0.0371	-0.0113	-0.198
(R$)		(0.145)	(0.0932)	(0.0761)	(0.242)	(0.110)	(0.286)
Payroll expenses	0.778	-0.0410*	-0.0184	0.000249	-0.0310	-0.0257	0.0860**
(share of total exp.)		(0.0232)	(0.0135)	(0.0149)	(0.0395)	(0.0173)	(0.0429)
Log of investiments	44.752.595	-0.0172	0.146	0.112	-0.0445	0.100	-0.859**
(R$)		(0.231)	(0.129)	(0.116)	(0.343)	(0.166)	(0.388)
Investiments	0.101	0.00431	0.00202	-6.56e-05	-0.00455	0.00464	-0.0573*
(share of total exp.)		(0.0194)	(0.0110)	(0.0103)	(0.0307)	(0.0143)	(0.0342)
Log of education and	126.091.703	-0.158	0.0741	0.0443	0.121	0.00229	-0.194
culture exp. (R$)		(0.185)	(0.101)	(0.0794)	(0.297)	(0.124)	(0.372)
Education and culture	0.261	-0.0251	-0.00528	-0.00711	0.0255	-0.00613	0.0342
(share of total exp.)		(0.0245)	(0.0169)	(0.0147)	(0.0239)	(0.0190)	(0.0326)
Log of health and	126.091.703	-0.133	0.100	0.0713	-0.275	-0.00804	-0.696*
sanitation exp. (R$)		(0.215)	(0.125)	(0.101)	(0.313)	(0.158)	(0.365)
Health and sanitation	0,248	-0.0165	0.00464	0.00354	-0.0785*	-0.00699	-0.102**
(share of total exp.)		(0.0305)	(0.0170)	(0.0160)	(0.0397)	(0.0219)	(0.0455)
Log of social assist.	29.427.495	0.126	0.196	0.266**	0.341	0.0463	-0.306
and pensions exp. (R$)		(0.266)	(0.170)	(0.132)	(0.487)	(0.197)	(0.532)
Social assistance and	0.067	0.0127	0.00637	0.0114	0.0200	0.00210	-0.00364
pensions (share of total)		(0.0148)	(0.00905)	(0.00789)	(0.0252)	(0.0104)	(0.0269)
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Observations		111	233	381	111	381	111

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.
Robust standard errors clustered at municipality levels in parenthesis. Each numbered column represents the treatment effect (of a change from SB to DB) on the dependent variable in a different polynomial regression with specified bandwidth. SB mean is the mean of dependent variable among municipalities with 175,000 to 200,000 voters, and it is always displayed in level (not in logarithm).

The results on Table 6 show that the treatment indirect effect on those variables usually have no numerical pattern and are not significant. The effect education and culture spending as a share of total expenditures is significant in specifications 4 and 6, it is numerically small and loses its significance once we control for state fixed effects on Table 6. The significant results in the cubic polynomial suggest the effect is opposed

to those that Chamon, Mello & Firpo (2009) preconize, because the expanses investment diminish and the expanses in payroll increase. The effect on two areas of social spending (education and culture, and health and sanitation) is also negative. Altogether, the reduced-form estimates of DB on public policy outcomes are quite ambiguous and lack statistical significance.

Figure 3. Treatment effect on public spending.

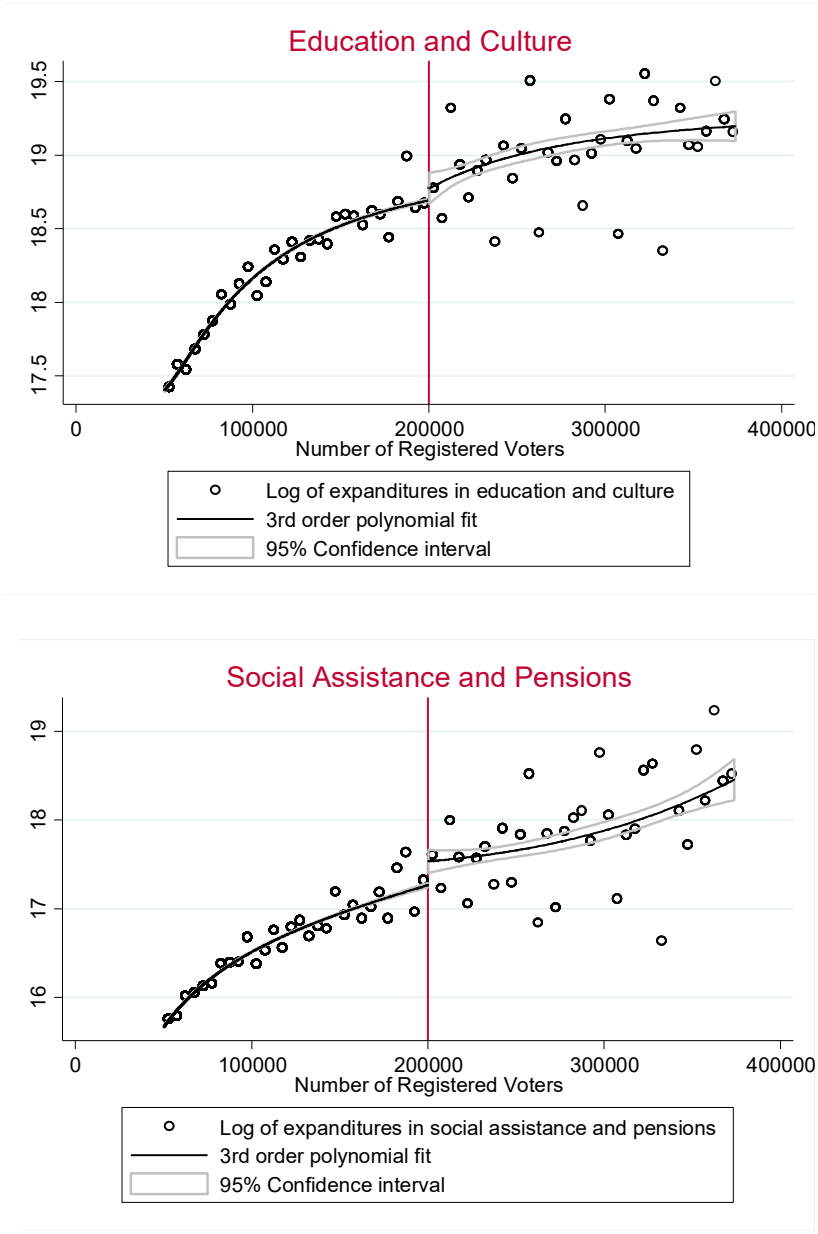
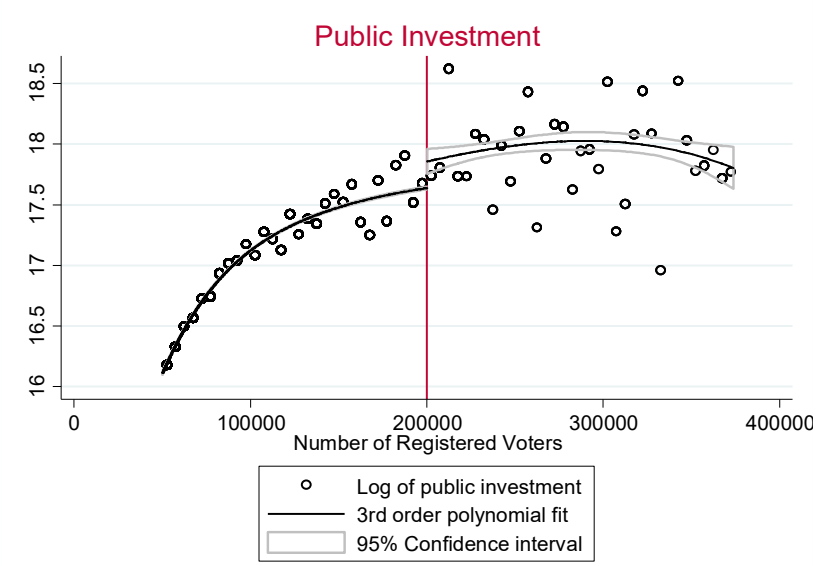


Figure 3. Treatment effect on public spending *(Continued)*.



5. Conclusion

The paper exploited the discontinuity on the assignment of dual-ballot election to Brazilian municipalities with electorates over 200,000 voters to estimate the impact of electoral rules on political competition and public spending with a RDD design. Our results present evidence that the local average treatment effect on the effective number of candidates is 0.4 (from a 2.5 baseline) and 5 p.p. on the vote share of third or lower placed candidate (from 16%). These results are smaller than those of Fujiwara (2011). Although these results are not new, they provide a robustness check for the conclusions of previous work to the enlargement of sample size.

The reduced form estimates of DBE on current, payroll, investment, health and sanitation, education and culture, and social assistance and pensions expenses, however, are inconclusive and insignificant. This lack of impact in expenditures should be considered with caution. On one hand, public spending may be not a good proxy for welfare, because of differences of efficiency, costs and quality of publicly provided services and goods. On the other hand, the results of Chamon, Mello & Firpo (2009) may also indicate that DBE and political competition have short-run impacts on public goods provision related to electoral cycles which this study does not consider. If these effects exist, they seem to be mitigate in medium-term trends over mandates.

Further extensions could test the effects of DBE on public policy indicators of direct service provision or policy efficiency.

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