Political Competition and the Provision of Early Childhood Education and Care: Evidence from Brazil

Raquel Tebaldi* Alysson Lorenzon Portella†

August 2, 2022

Abstract: The objective of this paper is to investigate whether higher political competition induces the expansion of public early childhood education and care (ECEC) services using Brazil as a case study. Public ECEC services are provided mainly by Brazilian municipalities and although enrolment for children aged 0 to 3 is not mandatory, it has increased continuously in the last two decades, with large heterogeneity across municipalities. In addition, electoral rules in Brazil establishes a dual-ballot system only for municipalities with more than 200,000 registered voters. This provides an exogenous variation in political competition that enables us to evaluate its impact on ECEC provision through a regression discontinuity design. We find that average estimated municipal crèche net enrolment rates are around 3 percentage points larger in municipalities with the dual-ballot. ECEC expenditure levels are also higher in these municipalities.

Keywords: Childcare; Daycare; Political Competition; Electoral Rules

JEL Classification: H72, H75, I25

*UNU-MERIT, Maastricht, Netherlands - tebaldi@unu.merit.edu
†Insper, Sao Paulo, Brazil - alyssonlp1@insper.edu.br

1 Introduction

What are the determinants of early childhood education and care (ECEC) services in developing countries? ECEC provision has the potential to improve human capital impacting several life dimensions (Currie, 2001; Heckman et al., 2013; Berlinski and Schady, 2015; García et al., 2020; Evans and Kosec, 2012) and to increase female labor participation (Mateo Díaz and Rodriguez-Chamussy, 2016; Brilli et al., 2016). However, existing estimates indicate that over 40 percent of children who need these services do not have access to them worldwide, and the biggest share of this coverage gap (80 percent of the children without access) is found in low and middle income countries (Devercelli and Beaton-Day, 2020). Although these services have been expanding in developing countries and access to quality ECEC by all girls and boys is among the Sustainable Development Goals that governments should aim for by 2030, little is known about the institutional factors that enable their expansion.

The objective of this paper is to evaluate how political competition impacts ECEC provision in the Brazilian context. Brazil's expansion of ECEC services is an interesting case study to assess how political competition can influence social policies in Latin American democracies for two reasons. First, Brazilian electoral rule establishes that the election of mayors should be based on a dual-ballot electoral system only in municipalities with more than 200,000, while municipalities below this threshold have only a single-ballot. This creates an exogenous variation in political competition that we exploit via an regression discontinuity design. It has been shown that the dual-ballot system increases the incentives for elected politicians to appeal to the broadest possible voter base when compared to simple majority systems (Fujiwara, 2011; Chamon et al., 2019; Chin, 2021). This happens because where only one round of voting is conducted, voters may be more inclined to vote strategically, whereas voters in dual-ballot systems have more room for expressing their sincerest preferences in the first round of voting (Fujiwara, 2011). This also impacts the number of candidates that enter these races (Chamon et al., 2019) and thus the contestability (Bartolini, 1999, 2000) of dual-ballot races is increased.

Second, the provision of public ECEC is a clear responsibility of municipal govern-

ments in Brazil and has been expanding unevenly across municipalities. Because school attendance is only mandatory after age four, there is considerable variation in the provision of these services for the younger group aged below four across Brazilian municipalities. Coupled with the exogenous change brought about by electoral rules, this allow us to credibly estimate local causal effects of political competition on the supply of ECEC services by Brazilian municipalities.

Two studies have used the same empirical strategy to assess the impact of dual-ballot elections on different policy areas and election outcomes. Chamon et al. (2019) investigate the effects on fiscal policy outcomes and find that mayors in dual-ballot systems invest more in capital and less in current expenditures and more construction of schools. They highlight the role of a higher number of candidates, less concentration of votes among the front-runners and a higher share of votes directed to the third placed and lower candidates as the operating mechanisms. Chin (2021) highlights instead that mayors in two-rounds are represented by a more geographically diverse and engaged electorate, which would explain why once in office they tend to invest more in municipal education and distribute resources more equally across schools.

In this article, we estimate the impact of increased political competition on two broad outcomes of ECEC services: i) coverage, defined as net enrollment of children aged 0 to 3 in municipal childcare centres (*crèches*); and ii) municipal public expenditures on these services. We use the discontinuity in electoral rules at the 200,000 voters cutoff to compare these outcomes between municipalities above and below this threshold, including in the analysis all municipal elections from 1996 to 2016. Our main findings suggest that municipalities that elect their mayors in a dual-ballot system display higher average levels of ECEC coverage and expenditure during the mayoral mandate. Average crèche net enrollment rates are 3.3 percentage points (pp) larger in municipalities with higher electoral competition. This is a sizable effect, considering that average net enrollment in crèches is around 8% in our sample of municipalities across the whole period. In terms of average expenditure, we find that municipalities just above the threshold spend between 0.02 and 0.07pp more on crèches than the municipalities just below it, although this

estimate is imprecise and varies with specification. This is again a considerable effect in face of an average expenditure of around 0.14% of the municipal GDP, depending on the exact definition of expenditures we use. Our complementary results and robustness checks reinforce our conclusion that increased political competition drives higher expansions in ECEC services.

We contribute to the literature that aims at understanding the determinants of ECEC provision in developing countries by providing credible evidence that political competition can have a causal impact on the expansion of these services. The few existing quantitative studies of the political economy determinants of ECEC provision look at the context of higher income countries (Bonoli and Reber, 2010; Curran, 2015; Hieda, 2013; Neimanns, 2021). These studies do not tend to focus on political competition as an explanatory variable, but rather on women's representation in politics and partisanship (left- versus right-wing governments). Studies looking at the context of lower income countries generally rely on qualitative methods and highlight the difficulty in making this policy area a priority, either because civil society is not significantly mobilised around it or due to policy and cultural legacies which weaken support for the public provision of childcare (Faur, 2011; Neuman and Powers, 2021). Wampler et al. (2019) also investigate whether political competition, measured as margins of victory, can affect the provision of ECEC in Brazil, but do not find any impact across the universe of Brazilian municipalities. Instead, they highlighting the role of participatory institutions (such as policy councils and town hall meetings), left-wing governments and state capacity considerations to explain ECEC outcomes.

Our study also contributes to the literature on the political determinants of social policy and social expenditure expansion in Latin America (Gouvêa and Girardi, 2021; Niedzwiecki and Pribble, 2017; Altman and Castiglioni, 2019). When focusing particularly in education policies, we see that mixed results emerge. On the one hand, some studies support the electoral competition hypothesis in the context of Mexico (Hecock, 2006) and Uruguay (Azar, 2021), as well as the importance of voter enfranchisement to increase education expenditure in Brazil (Schneider et al., 2019). On the other hand,

Bursztyn (2016) finds that income is an important mediator of this relation in Brazil through observational and experimental data, as poorer voters do not tend to reward incumbents for investing more in education. Most of these studies tend to focus on the mandatory provision of education starting in primary school. Our paper provides a novel contribution to this literature by focusing on a non-mandatory stage of education, for children aged 0 to 3.

This paper proceeds with the following structure: Section 2 describes the institutional context of both ECEC policy as well as the electoral system in Brazil while Section 3 presents the empirical strategy and data. Section 4 presents the main results and Section 5 further contains robustness checks. Section 6 concludes.

2 Institutional context

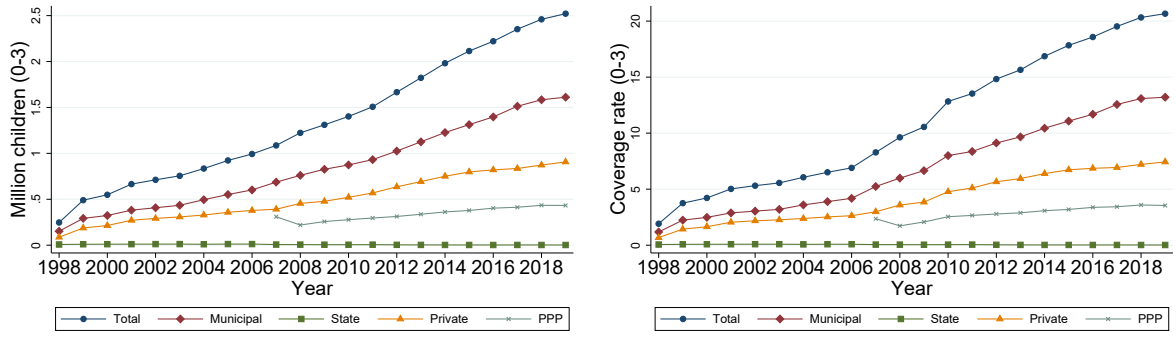
In Brazil, early childhood education and care (ECEC) provision is a responsibility of municipalities and is divided into daycare/crèches for children aged 0 to 3 and pre-school for children aged 4 to 5. Municipalities are the main public service providers, but ECEC public provision also encompasses services provided by the state and federal levels. A national-level fund (FUNDEB[1]) for basic education in Brazil (from ECEC to high school) redistributes funds generated from tax revenues to municipalities based on the number of students, minimum amounts per student, and maximum allocation rates per state (Brazil, 2013b). At the national level, there has been a clear push to improve public ECEC provision in recent years via several policy measures, including *Proinfância*, a programme financing school construction, a reduction in the age of mandatory education from 6 to to 4 in 2013 (thus making pre-school mandatory) and the establishment of coverage targets in the National Education Plan made into law in 2014 (Brazil, 2013a; MoE; FNDE).

Although access to ECEC is enshrined in the Constitution, stark inequalities remain in terms of children's access to this service throughout the country. Even if education

[1] *Fundo de Manutenção e Desenvolvimento da Educação Básica e de Valorização dos Profissionais da Educação*

from age 0 is a constitutionally guaranteed right, enrolment of children between 0 to 3 years old is more dependent on the demand from families, as mandatory enrolment is required only from four-year-olds (since 2013). Furthermore, while there has also been plenty of space for expansion as evidenced by the low levels of enrolment and increasing demand, we also find expressive territorial and income inequality in access to services (Gomes, 2017; IBGE, 2018).

Figure 1 illustrates the evolution in the enrolment rates and the number of students aged 0 to 3 in crèches from 1998 to 2019 in Brazil, while Figure 2 shows how coverage rates vary between municipalities. These numbers have been steadily growing throughout the period. In sum, ECEC in Brazil is a policy with clear attribution of responsibility, with space to expand that has seen increasing demand over time. As attendance for children under the age of four is not mandatory, variation in terms of the crèche enrolment outcomes across municipalities make this policy an interesting case study.



(a) Total enrolment

(b) Net coverage rate

Figure 1: Evolution in the number of students enrolled in crèche by type of service provider in Brazil

Note: Authors' estimation based on data from the Brazilian School Census (INEP). Total enrolment numbers encompass all types of service providers, both public and private. PPP refers to private-public partnerships and is included in the numbers for private enrolment.

Regarding Brazil's electoral system, the 1988 Constitution establishes that those municipalities with less than 200,000 registered voters have their mayors elected under a plurality voting system (single ballot) and that those with more voters have the possibility of a runoff election (dual ballot) if no candidate obtains more than half of the valid votes in the first round (Brazil, 1988). Since then, mayoral elections have been held every four years across Brazilian municipalities. Mayors are thus elected for four-year terms

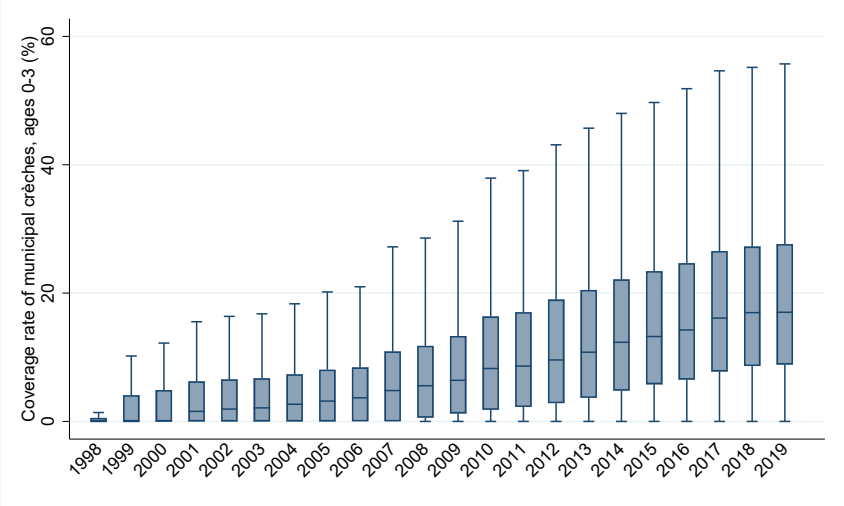


Figure 2: Distribution of Creche coverage rates across municipalities

Note: Authors' estimation based on data from the Brazilian School Census (INEP). Coverage rates are computed at the municipality level and consider only municipal providers. The boxplot excludes outlier municipalities with net coverage rates above the upper "whisker".

and since 2000 incumbent mayors can run for re-election for one consecutive term.

Some studies have shown that the behavior of politicians and voters are affected by the possibility of a second round. Fujiwara (2011) finds that voters behave strategically by casting fewer votes in the third-place candidate and concentrating votes in the top two candidates when elections are single ballot. Chamon et al. (2019) shows that there are more candidates in dual-ballot elections, and that candidates elected in these system manage municipalities funds differently. Chin (2021) argues that dual-ballot systems create incentives for candidates to appeal to a broader electoral base, which results in resources being distributed more equally across groups of interest. Based on these findings, we hypothesize that mayors elected in dual-ballot system will expand ECEC services more rapidly, either because they channel more funds to social policies or because they must appeal to a broader base, in our context low-income women who are most benefited by these services.

3 Empirical strategy and Data

3.1 Empirical Strategy

The paper's identification strategy follows Chamon et al. (2019) and is based on the fact that the size of the electorate in each municipality determines whether that municipality is subject to a dual-ballot system for their mayoral elections. For municipalities that are subject to this rule, meaning those with over 200,000 registered voters, higher political competition levels are found in terms of higher number of candidates and lower concentration of votes in the front-runners, which in turn affect policy decisions.

As explained by Chamon et al. (2019) and also tested by Fujiwara (2011) in the context of Brazil, the theoretical explanation for the increase in political competitiveness due to the dual-ballot rule can be explained by Duverger's law, which posits that single-ballot voting tends to favour a two-party system (Duverger, 1954). This rule thus provides an exogenous increase in the level of competitiveness as demonstrated by Chamon et al. (2019) in their regression discontinuity study focusing on municipalities between 100,000 and 300,000 registered voters and their fiscal policy outcomes from the elections from 1996 to 2004. The authors found that, when compared to single-ballot municipalities, municipalities over the threshold of 200,000 voters investment more and have lower current expenditures. Chin (2021) also uses the same discontinuity in electoral rules to show how the dual-ballot system pushes candidates to secure a broader voter base. She shows that municipalities across the threshold not only displayed higher investment in primary school resources, but they were also distributed more evenly across schools.

Following these papers, we uses the same empirical strategy, focusing on the effects of higher electoral competition on the provision of ECEC services. Our time horizon encompasses all elections from 1996 to 2016. We use a local linear regression specification to estimate the treatment effect effect at the discontinuity (Imbens and Lemieux, 2008; Calonico et al., 2014).

$$Y_{it} = \beta_1 D_{it} + \beta_2 X_{it} + \beta_3 X_{it} \cdot D_{it} + \gamma_t + \varepsilon_{it} \tag{1}$$

Where i indexes municipalities in electoral years t between 1996 and 2016. The variable Y_{it} is the outcome of interest corresponding to each electoral race. The variable X_{it} is the running variable and D_{it} correspond to treatment assignment, respectively the number of voter and whether it is larger than 200,000. We also control for election-year fixed effects, τ_t.

The local average treatment effect is given by the coefficient β_1. Our preferred specification relies on a 50,000 voter window around the threshold, using a uniform kernel. However, we also estimate treatment effects using 100,000 window and a quadratic specification for the running variable below and above the threshold. As robustness checks, we use data-driven methods to select bandwidths, combined with a triangular kernel Calonico et al. (2014) We consider either the case of symmetric and asymmetric bandwidth below and above the cutoff. Standard errors are clustered at the State level in all cases.

We describe our data and how we constructed the outcomes variables more thoroughly in the following section. For now, we note that we measure the outcomes in two ways. First, we consider the average of the outcome variable across the four years of the elected mayor's mandate, following the practice in studies of the political economy of social policies of looking at the outcomes associated with the previous election results (Fried, 2012; González, 2017; Gouvêa and Girardi, 2021; Schady, 2000).[2] Therefore, in the case of a mayor elected in the 2000 election, we consider that municipality's average outcomes between 2001 and 2004. Second, we also consider the outcome at the last year of the mandate, which is the following election year. In the example above, of the mandate starting in 2001, this would mean outcomes measured in 2004.

3.2 Data

The dataset used in this analysis contains municipal Brazilian elections between 1996 until 2016. Electoral data was obtained directly from the Brazilian Superior Electoral

[2] 1998 to 2000; 2001 to 2004; 2005 to 2008; 2009 to 2012; 2013 to 2016; and 2017 to 2020. For coverage rates, we take the average of the first mandate starting in 1998 and we take the average of the last mandate without data from the year 2020.

Court and cross-referenced and complemented with datasets used in previous studies (Chamon et al., 2019; Nicolau; Power and Rodrigues-Silveira, 2019; TSE).

Data on municipal budgetary allocation was obtained from the treasury (SICONFI) for the years 2004 to 2019 and from SIOPE[3] from 2011 (FNDE). Data on municipal GDP was obtained from IBGE and was limited to the period from 2002 to 2019. Detailed population data by age group, used to construct net enrolment rates, was obtained from DATASUS up to 2012 and from ABRINQ Foundation between 2013 and 2019 (ABRINQ; DATASUS). Socioeconomic data was obtained from the 2000 and 2010 Censuses (IBGE). Finally, school enrolment data was obtained from the annual School Census carried out by INEP (INEP). Considering that our outcomes of interest were net enrolment rates, we only used data starting from 1998, when the age of students attending crèches started being reported up to 2019.

It is important to note that the paper faces some data limitations, as not all years were available for each variable[4]. Furthermore, the paper is also limited by the fact that data collection on ECEC coverage and expenditure outcomes started in the late nineties, so we do not observe pre- and post-democracy scenarios.

Our outcomes of interest are municipal crèche coverage rates and ECEC expenditure rates. Therefore, we estimated net ECEC enrolment rates in crèches for children aged 0 to 3 using data from the school census for the enrolment numbers and data from DATASUS and from ABRINQ foundation for the total population per age group. Our main analysis focuses on crèche outcomes, but we also report results for preschool outcomes in the Appendix. For the expenditure outcomes, we use two different categories, budgeted expenditures from FINBRA and paid expenditures from SIOPE. [5] Each expenditure measure correspond to a different stage of execution of the expenditures. Budgeted expenditures refer to the first stage of contracting the services. Paid expenditures, the final stage, are expenditures that have been paid to service providers after verification of

[3] *Sistema de Informações sobre Orçamentos Públicos em Educação.*

[4] We estimated coverage rages between 1998 and 2019 and expenditure rates from 2004 to 2018.

[5] When constructing expenditure indicators from FINBRA we use the total amount budgeted for ECEC over municipal GDP and when using SIOPE data we estimated paid crèche and preschool expenditures over GDP considering current and capital expenditures.

service delivery according to what was agreed in the contracts.[6] The advantage of using SIOPE data in complement to data from FINBRA is that it distinguishes between expenditures on crèche and preschool as well as between total expenditures and expenditures without considering funds from FUNDEB. Therefore, each measure of expenditure adds a more specific information: ECEC budgeted expenditures comprise all expenditure in both crèche and preschool; crèche paid expenditures encompass all expenditures at this specific level; and crèche paid expenditures excluding FUNDEB funding further specifies those expenditures that did not come from this federal sources. Municipal GDP data was obtained from IBGE. Finally, we also include a variable for per capita crèche expenditure using the total annual paid expenditures over the total population aged 0 to 3. We use the implicit GDP deflator and adjust this variable to correspond to 2016 prices.

Table 1 displays the descriptive statistics of our outcome variables of interest, restricting the sample to municipalities between 100,000 and 300,000 registered voters and splitting it at the 200,000 voters cutoff. Descriptive statistics for the combined sample, political variables and other covariates can be seen in the Appendix, respectively in Tables A1, A2 and A3.

Table 1: Descriptive Statistics: main outcome variables, mandate averages

	Control			Treatment		
	N	Mean	SD	N	Mean	SD
Number of voters	523	140374.77	27435.19	182	242147.90	28082.22
Municipal creche net enrolment	523	8.60	9.14	182	9.09	10.02
ECEC budget by GDP	368	0.41	0.33	142	0.48	0.36
ECEC budget per capita	368	1645.73	1649.90	142	2082.30	1841.75
Creche expenditure by GDP	285	0.14	0.12	110	0.14	0.11
Creche expenditure by GDP, exclud. FUNDEB	273	0.09	0.07	104	0.11	0.10

Note: our main outcome variables of interest are the estimated crèche municipal enrolment rates and the estimated ECEC and crèche expenditure rates. This table reports descriptive statistics for the mandate averages of each outcome variable. Sample is restricted to municipalities with between 100,000 and 300,000 voters in the elections from 1996 to 2016.

3.3 Validation of the RD design

Prior analyses exploiting the same empirical strategy have conducted a series of tests to gauge its appropriateness. Figure 3 plots the density of elections around the cutoff.

[6]More information available at:http://www.portaltransparencia.gov.br/pagina-interna/603453-dicionario-de-dados-execucao-da-despesa

Like Chin (2021) and Chamon et al. (2019), we find no evidence of manipulation of the size of the electorate using the manipulation test based on density discontinuity as proposed by Cattaneo et al. (2018). We also do not know of any other policy that coincides with the threshold of 200,000 voters as established by the electoral rules.

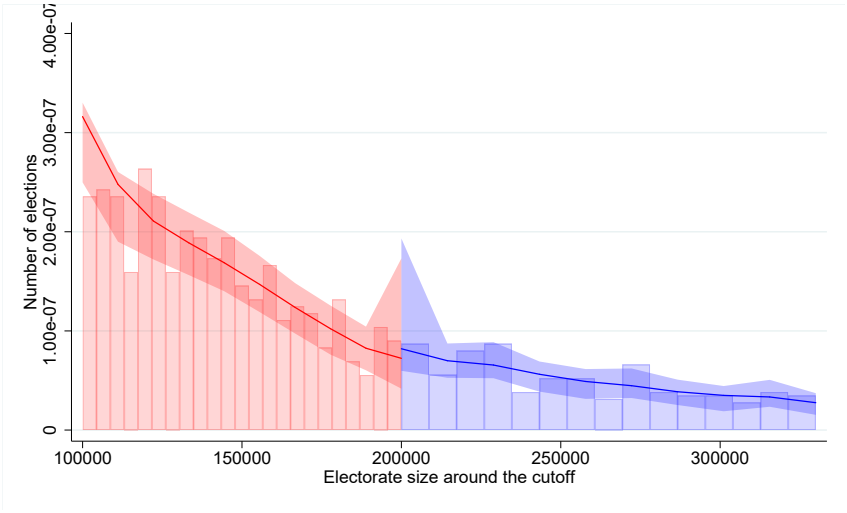


Figure 3: McCrary Test

Notes: Graphs plot the number of elections around the threshold using local-polynomial density estimators as proposed by Cattaneo et al. (2018). Sample is restricted to municipalities below 400,000 registered voters.

As part of the validation of the regression discontinuity design, it is important to verify if treatment and control units are similar in terms of observable covariates that should not be affected by treatment. This can be done by checking units' characteristics before treatment assignment and by checking post-treatment characteristics in the form of placebo outcomes (Cattaneo et al., 2020).

On Table A3, in the Appendix, we present descriptive statistics for control and treatment municipalities for the following variables: illiteracy rate (population from 15 to 64); average schooling years; Gini coefficient (based on total household income); average monthly total household income; female population; rural population; non-white population; share of the population aged 0 to 3; share of the population aged 4 to 5; average number of children per household; average dependency ratio; average household size; labour participation rate; female labour participation rate; and population density. Figure 4, reports the t-statistic associated with the treatment assignment when replacing the dependent variable in Equation 1 with the aforementioned variables from the closest

Census (either the 2000 or 2010). No relevant discontinuity in variables which may affect the outcomes of interest emerge at the discontinuity threshold, except for an effect in terms of population density.

Chin (2021) also checked the balance on municipalities' characteristics prior to the 1988 Constitution (using data from the 1980 Census). The only difference found is in terms of population density, which is then included in the author's model specifications.[7] Adding a control for population density does not alter our main results. We also include this control in results reported in the Robustness Checks section when using the same model specification as Chin (2021). Finally, Chamon et al. (2019) also analyses control and treatment municipalities' characteristics using data from the 2000 Census for races up to 2004 and finds no significant differences between the treatment and control groups.

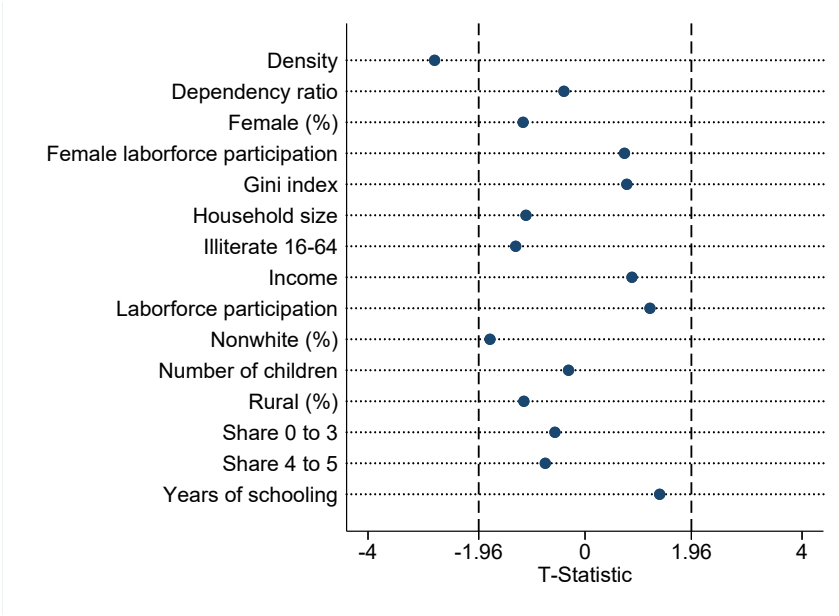


Figure 4: Balance tests, closest Census

Note: T-statistic of the treatment assignment using the regression specified in Equation (1) and replacing the dependent variable by the variable of interest taken from the Census 2000 for elections between 1996 and 2004 or from the Census 2010 for elections between 2008 and 2016 as placebo outcomes. We use local linear regression in a 50,000 window around the threshold with a uniform kernel. Standard errors are clustered at the State level.

[7] See Chin (2021), page 14. The author also discusses why this result is likely a false positive.

4 Results

In this section we present our results for crèche enrolment and expenditure rates, showing that municipalities across the 200,000 voter threshold present enrolment rates around 3 percentage points (pp) higher as well as higher expenditure rates measured as percentage of municipal GDP or per capita. We also present results related to political outcomes of interest, showing that the threshold increases the number of candidates entering municipal races, as also found by Chamon et al. (2019) and Chin (2021).

4.1 Creche Outcomes

Our main results, reported in Table 2, present results of our estimates with a 50,000 and 100,000 bandwidth around the threshold of 200,000 voters fitting a local polynomial of the electorate size of order 1 and 2. The results demonstrate that municipalities subjected to the dual-ballot voting system present average net municipal crèche enrolment rates over each political mandate 3 percentage points (pp) higher. We also find a positive impact of the threshold on average ECEC budgeted expenditure rates of around 0.15 pp (which includes both crèche and preschool), as well as specifically on crèche paid expenditure rates, between 0.06 pp for expenditures other than those financed from FUNDEB to 0.07 pp for total crèche expenditures in the specification in column (1). Crèche per capita annual expenditures are also higher across the threshold, around 460 to 520 Brazilian reais per child at 2016 prices.

Visual evidence of the discontinuity is also supported by figures 5 and 6 These results are relevant considering the low average values of ECEC enrolment and expenditure in the municipalities in the sample, as reported in Table 1.

To further investigate the process by which municipalities increase the coverage rates of crèches, we estimate the effect of dual-ballots on the number of institutions that provide crèche services and the number of children between 0-3 attending crèche per institution. Table 3 shows the estimated treatment effects for these outcomes variables. We find both evidence of a higher number of municipal crèches and a higher number of students per institutions. However, these estimates are imprecise and only significant when we

Table 2: Effect of dual-ballot on creche outcomes

	50,000 bandwidth		100,000 bandwidth	
	Poly. order = 1	Poly. order = 2	Poly. order = 1	Poly. order = 2
	(1)	(2)	(3)	(4)
Net enrolment in municipal creches	3.277**	3.125	3.409	3.412**
	(2.63)	(1.27)	(1.64)	(2.59)
Observations	297	297	705	705
Creche expenditure per capita	461.8**	291.9	354.1	520.2**
	(2.76)	(1.71)	(1.36)	(2.42)
Observations	172	172	395	395
Creche payments as share of GDP	0.0702*	0.0394	0.0460*	0.0603**
	(2.05)	(1.10)	(1.94)	(2.10)
Observations	172	172	395	395
Creche expenditures excl. FUNDEB (% GDP)	0.0642**	0.0637**	0.0257*	0.0652**
	(2.57)	(2.49)	(1.88)	(2.53)
Observations	164	164	377	377
ECEC budget as share of GDP	0.133	0.196	0.153***	0.116
	(1.24)	(1.54)	(3.25)	(1.25)
Observations	220	220	510	510

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are four-year averages of each mandated. All estimates include election-year fixed effects. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

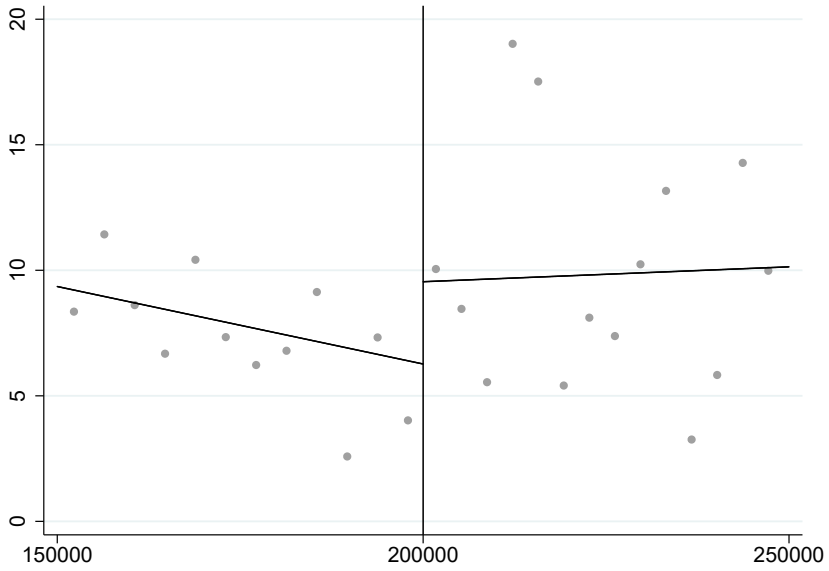
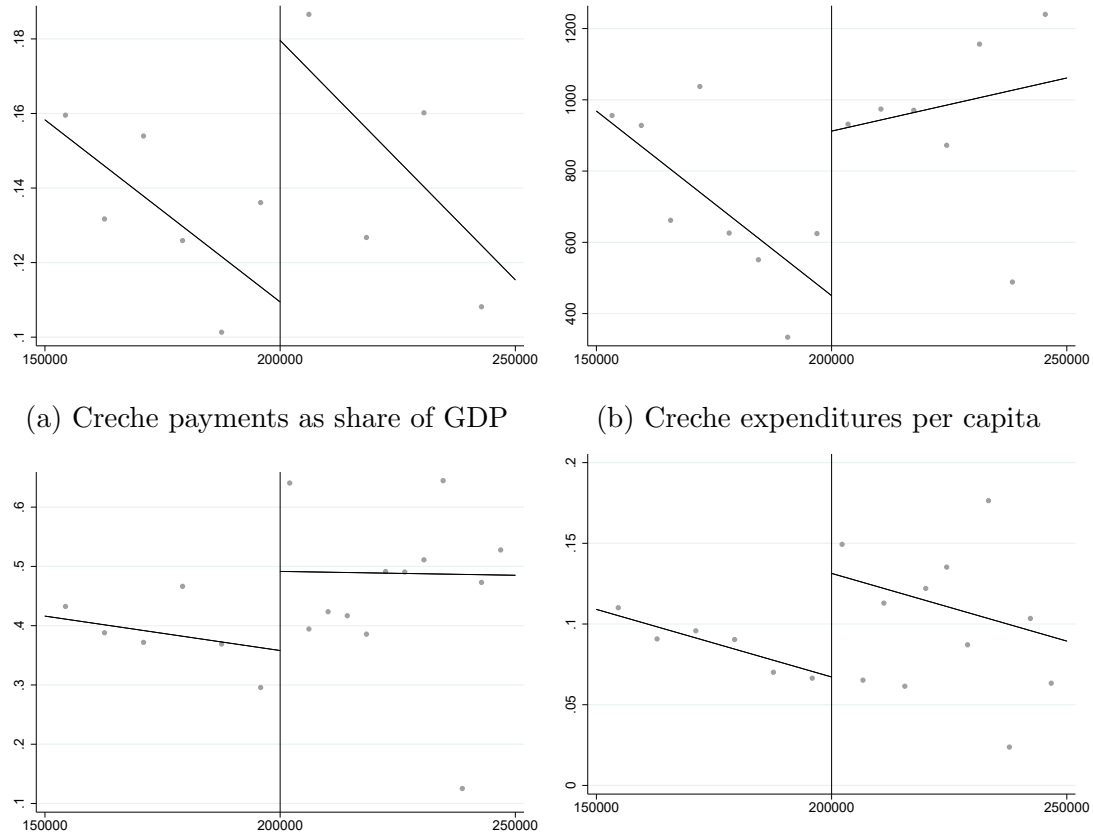


Figure 5: The impact of dual-ballot on net municipal creche enrolment

Notes: This figure shows graphically the effect of dual-ballots on net enrolment in municipal creches. We use a linear specification and a 50,000 bandwidth around the 200,000 voters cutoff. The estimate include State fixed-effects. The dependent variable is a four-year mandate average.

estimate the treatment effect in a 100,000 voter window around the cutoff and use a linear specification (column 3).

We also investigate whether there is some heterogeneity in treatment effects with respect to time periods. To do so, we split the sample into two parts: for the three



(a) Creche payments as share of GDP

(b) Creche expenditures per capita

(c) ECEC budget as share of GDP

(d) Creche expenditure as share of GDP, excluding FUNDEB

Figure 6: The impact of dual-ballots on creche expenditure measures

Notes: This figure shows graphically the effect of dual-ballots on expenditures on creches. We use a linear specification and a 50,000 bandwidth around the 200,000 voters cutoff. The estimate include State fixed-effects. The dependent variable are four-year mandate average of (a) creche payments as share of GDP; (b) Creche expenditures per capita; (c) ECEC budget as share of GDP; and (d) Creche expenditure as share of GDP, excluding FUNDEB.

Table 3: Effect of dual-ballot on number of institutions

	(1)	(2)	(3)	(4)
	50,000 bandwidth		100,000 bandwidth	
	Poly. order = 1	Poly. order = 2	Poly. order = 1	Poly. order = 2
Number of municipal creches	4.364	3.780	7.097**	2.891
	(1.12)	(0.78)	(2.25)	(0.62)
Observations	297	297	705	705
Students per municipal creche, 0-3 years old	7.401	9.979	10.02**	6.702
	(1.15)	(0.79)	(2.57)	(0.68)
Observations	291	291	677	677

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are four-year averages of each mandated. All estimates include election-year fixed effects. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

elections between 1996 and 2004, and the three elections between 2008 and 2016. As we have mentioned before, not all outcomes of interests have data available for all time

periods. Therefore, we only estimate these effects for net municipal crèche enrolment and ECEC budget as a share of GDP. Moreover, for the latter we only have data available after 2004 and we can only estimate effects of this election for that variable. Results are shown in Table 4. We find that our positive results seem to be driven by the most recent period. None of the estimates for the first period are statistically different from zero, and some point estimates are in fact negative, suggesting that the insignificant result is not the result of small sample sizes. For the second period, however, estimated treatment effects are all positive and most of them are significant at the 5% confidence level.

Table 4: Effect of dual-ballot on creche outcomes, split sample

	Before 2004				After 2004			
	50,000 bandwidth		100,000 bandwidth		50,000 bandwidth		100,000 bandwidth	
	Order = 1	Order = 2	Order = 1	Order = 2	Order = 1	Order = 2	Order = 1	Order = 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net municipal creche enrolment	0.722	-0.347	1.977	0.628	6.082***	7.312**	4.495	6.621**
	(1.487)	(2.132)	(1.229)	(1.950)	(2.101)	(3.290)	(3.630)	(2.471)
Observations	123	123	304	304	174	174	401	401
ECEC budget as share of GDP	-0.0663	-0.210	0.00864	-0.0983	0.201	0.373*	0.195***	0.187
	(0.115)	(0.202)	(0.0991)	(0.111)	(0.139)	(0.179)	(0.0592)	(0.117)
Observations	46	46	113	113	174	174	397	397

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are four-year averages of each mandated. All estimates include election-year fixed effects. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Restricting the analysis to election years instead of mandate averages lead to similar results in terms of crèche enrolment and expenditure rates. Results can be found in Table A4, in the Appendix. This could be interpreted as evidence of how the political cycle and proximity to elections may also play a role in ECEC provision (but particularly crèche in the case of Brazil), like other studies have shown for other policy areas (Couyoumdjian and Londregan, 2012; Hernández et al., 2019; Penfold-Becerra, 2007; Simpser et al., 2016; Tribin, 2020).

Finally, since our focus is on municipal-level elections, our expectation is that municipal enrolment rates (as opposed to state or federal provision or private rates) would be impacted at the threshold. Indeed, we only find significant results for municipal and public net rates of enrolment.[8] We also do not find any effect on the supply of private crèches nor on public and private partnerships (PPP). Including the services provided by

[8]Public rates encompass municipal, state and federal levels of provision.

PPP together with municipal and all public institutions increases the size of the treatment effect, although only by a small margin. These results are reported on table A5, in the Appendix.

4.2 Political Outcomes

We also use the same methodology to assess the local impact of the dual ballots in terms of various political outcomes: the number of candidates entering each race; the share of votes going to the candidates other than the front-runners; voter turnout rate; and characteristics of the candidates (gender, race, education, ideology). These results are summarized in Figure 7.

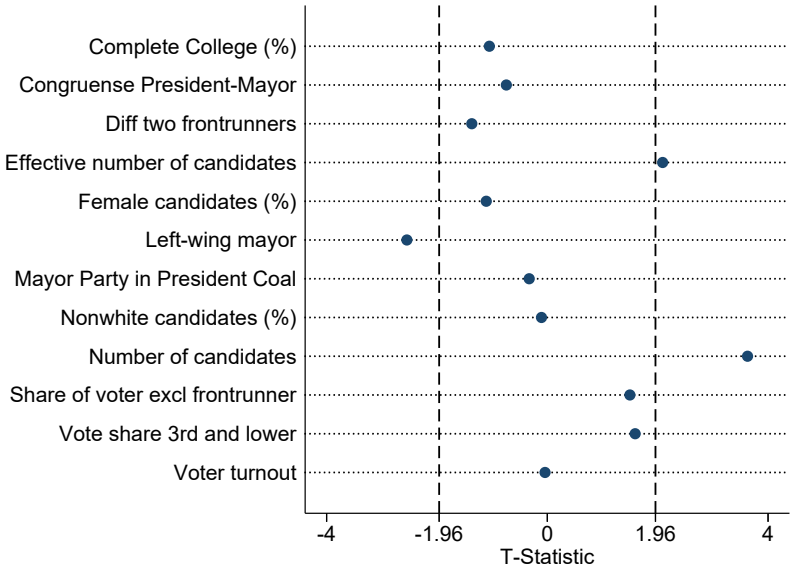


Figure 7: Effect of dual-ballot on political variables

Notes: We report the T-statistic of the local treatment effect. We estimate these effects using local linear regression in a 50,000 window around the threshold with a uniform kernel (Eq. 1). Standard errors are clustered at the State level. Variables expressed as percentages refer to characteristics of the candidates (gender, education, race). Effective number of candidates follows the procedure proposed by Laakso and Taagepera (1979). Share of Votes excluding frontrunners is equal to 100% minus the share of votes of the first two placed candidates. For municipality with only one or two candidates in a mayoral race this equals to zero. Left-wing mayor is a binary variable which takes the value of 1 when the party of the elected mayor is a left-wing party following the classification proposed by Gouvêa and Girardi (2021).

As expected, the threshold has a significant impact in terms of the number of candidates, meaning that more candidates enter the races. These are signs of increased competitiveness of these elections and relate to the dimension of contestability as proposed

by Bartolini (1999, 2000). The intuition behind the difference in number of candidates between both election systems is that where results are decided based on a single round of voting, voters will tend to concentrate their votes on the two front-runners, meaning they will predominantly adopt a strategic voting behaviour rather than express their sincerest political preferences (Fujiwara, 2011), which would then discourage greater numbers of candidates of entering electoral races in single rounds. This in turn would thus impact the dimension of contestability of political competition as theorized by Bartolini (1999), who explains that in lower contestability environments: "the knowledge of the existence of such barriers to victory may act as a barrier to entry for newcomers and as a margin of non-adaptation and non-responsiveness by the existing represented parties/candidates" (pg. 457). That is, in single rounds incumbents would have less incentives to respond to the electorate's demands compared to dual-ballot systems considering the greater barriers for entry.

Chin (2021) also considers this aspect and suggests that while more candidates enter two-round elections, they are not necessarily more likely to win and may thus do so to build experience for future elections, or to gain positions within the elected administration or even to influence the platforms of the front-runners through their competition or bargaining. However, the author finds that the effect of the two-round elections is better explained by looking more closely at the characteristics of the electorate and behaviours of the top candidates, including the following mechanisms: 1) top two candidates in two round election systems tend to receive support from a broader group of voters; 2) candidates in two-round elections use less campaign financing from corporations (which can be assumed to represent narrower interests within the society); 3) while voter turnout is not impacted by the threshold, voters in dual-ballot systems cast less blank or invalid votes, which can be interpreted as a sign of greater voter engagement. These factors would thus combine to prompt candidates in two-round elections to appeal more broadly to all voters, including minority groups. In that sense, our results concerning public ECEC provision is also in line with these findings, as it is considered a social policy that benefits women and children, particularly from poorer households.

We do not find an impact in terms of the education of candidates or turnout of voters, but we do find an unexpected impact of the threshold on the partisanship of elected mayors, which suggests that municipalities with the dual-ballot system tend to elect left-wing mayors less than in control municipalities. We follow Gouvêa and Girardi (2021) to classify left-wing parties. When it comes to the role of ideology, previous studies in the area of the political economy of social policies in Latin America tend to either find support for left ideology as a driver (Huber and Stephens, 2012; Sugiyama, 2011) or find that where right-wing incumbents expand these policies they do so because of high levels of political competition and/or social mobilization, as these policies are typically not at the core of their political agendas (Niedzwiecki and Pribble, 2017; Garay, 2016). Our finding of greater municipal ECEC provision in an environment of greater political competition, even if these municipalities are slightly more right-leaning, is thus in line with these previous findings.

5 Robustness Checks

This section presents the results of the following robustness checks: using data-driven approaches to the estimation of the main results and restricting the sample to different discontinuity ranges to test the robustness of the results to different bandwidths. We show that our main results in terms of average crèche coverage rates remain significant throughout these tests, but our results in terms of expenditure rates are less robust. We also conduct placebo tests with the threshold set at different points of electorate size and, as expected, none of the outcome variables of interested are impacted.

5.1 Different bandwidths

The results for enrolment rates are robust to data-driven bandwidth selection approaches (Calonico et al., 2014), as shown in Table 5, whether using a symmetrical (Panel A) or allowing for an asymmetrical window (Panel B) around the threshold. Not all of our expenditure measures are robust to this estimation, although all coefficients

remain positive.

Table 5: Main results using data-driven approach to bandwidth selection

	Net enrolment in municipal crèches	Paid crèche expenditure p.c.	Paid crèche expenditure as share of GDP	Paid crèche expenditure Excluding FUNDEB as share of GDP	ECEC budget as share of GDP
	(1)	(2)	(3)	(4)	(5)
Panel A: Same bandwidth					
RD Estimate	3.354**	379.143	0.051*	0.032	0.135
Stand. Error	[1.702]	[259.504]	[0.030]	[0.031]	[0.157]
Robust 95% CI	[.001 ; 6.742]	[-103.034 ; 877.963]	[-.04 ; .105]	[-.051 ; .075]	[-.188 ; .422]
Robust p-value	0.050	0.122	0.383	0.718	0.452
BW Loc. Poly. (h)	54287.46	110520.52	100725.70	125645.81	94274.16
BW Loc. Poly. (h)	54287.46	110520.52	100725.70	125645.81	94274.16
Eff. Obs. Left	206	355	287	488	336
Eff. Obs. Right	118	116	110	120	139
Panel B: Different bandwidth					
RD Estimate	3.302*	260.816	0.051*	0.030	0.128
Stand. Error	[1.805]	[253.217]	[0.030]	[0.032]	[0.155]
Robust 95% CI	[-.044 ; 6.707]	[-190.533 ; 753.077]	[-.034 ; .111]	[-.044 ; .085]	[-.197 ; .407]
Robust p-value	0.053	0.243	0.296	0.536	0.497
BW Loc. Poly. (h)	80011.76	158954.52	87103.93	94114.46	112763.29
BW Loc. Poly. (h)	48136.45	101456.03	102710.19	145419.55	92485.18
Eff. Obs. Left	369	1274	235	249	489
Eff. Obs. Right	109	111	113	128	135

Notes: This table reports estimates of the effect of dual-ballot on creche outcomes. Optimal bandwidth are obtained based on the data-driven approach by Calonico et al. (2014). Panel (a) uses the same bandwidths below and above the cutoff, while Panel (b) uses different bandwidth in each side of the cutoff. We follow the same work to obtain 95% confidence intervals and p-values. All estimates include election year fixed-effects and standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Figures 8 and 9, we also investigate how the size of treatment effects and confidence intervals are sensible to bandwidth choice. In this Figures, we plot the estimated treatment effects for each symmetric window around the threshold of 200,000 voters, starting at 20,000 up to 200,000 voters, increasing by intervals of 1,000 voters, and always using linear specifications for the running variable. These figures also plot the estimated effect and confidence intervals using the data-driven approach of Calonico et al. (2014), either considering the bandwidth that minimizes Mean Squared Error (MSE) and the one that minimizes Coverage Error (CER).

The results are mostly robust the size of the bandwidth selected. In Figure 8, bandwidths between 30,000 up to around 100,000 voters lead to very similar point estimates and confidence intervals above zero, including for the robust MSE and CER data-driven approaches. The results on effects on expenditure variables are less robust. Point estimates are generally stable, although they vary within some ranges. Moreover, the confidence intervals using robust data-driven methods are much larger than the standard confidence intervals obtained by naive estimators.

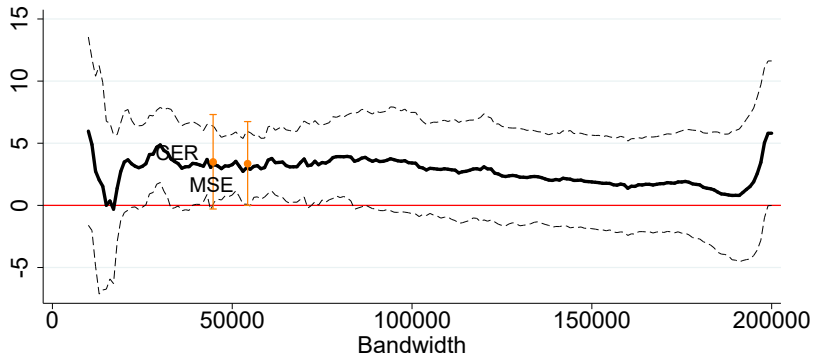
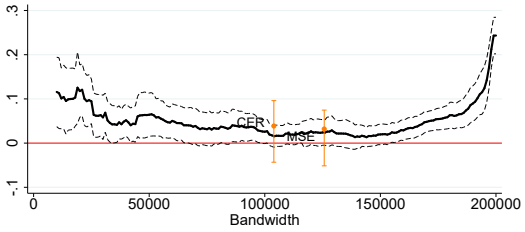
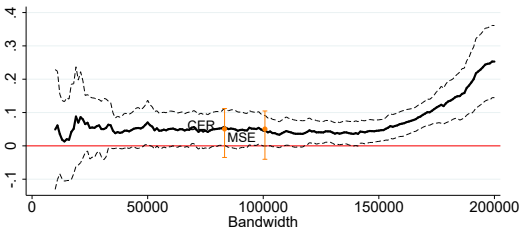


Figure 8: Different bandwidth for net creche enrolment

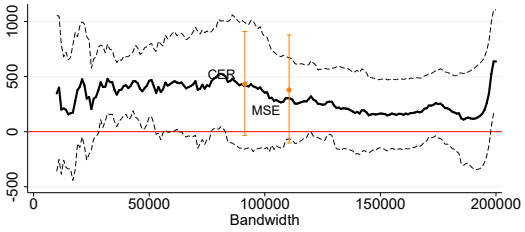
Notes: This figures plot the estimated treatment effects and confidence intervals for several symmetric bandwidth around the 200,000 cutoff in registered voters. It also plots the estimated effects and confidence intervals using symmetric bandwidths chosen using the data-driven criteria that minimizes Mean Squared Errors (MSE) and Coverage Errors (CER). All estimates are based using local linear regression following Equation 1. Standard errors are clustered at the State level.



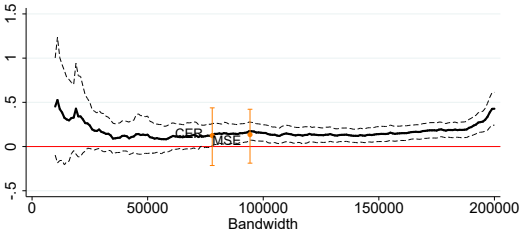
(a) Creche expenditure by GDP excluding FUNDEB



(b) Creche paid expenditure by GDP



(c) Creche expenditure per capita



(d) ECEC budget by GDP

Figure 9: Different bandwidth for avg. expenditure variables

Notes: This figures plot the estimated treatment effects and confidence intervals for several symmetric bandwidth around the 200,000 cutoff in registered voters. It also plots the estimated effects and confidence intervals using symmetric bandwidths chosen using the data-driven criteria that minimizes Mean Squared Errors (MSE) and Coverage Errors (CER). All estimates are based using local linear regression following Equation 1. Standard errors are clustered at the State level.

5.2 Placebo cutoffs

Another robustness check consists of estimating local treatment effects at placebo cutoffs. To do, we replace the original threshold of 200,000 registered voters by threshold

between 150,000 and 250,000. The specification uses 50,000 symmetric bandwidth and linear running variable. Figure 10 show the results for net enrolment in municipal creches. Apart from the estimate at exactly the original thresholds, all other confidence intervals include zero treatment effects. One case of placebo cutoff is at the 210,000 registered voters, in which the point estimate is larger than that estimated at the 200,000 threshold and the confidence interval marginally covers null effects. We believe these results to be driven mostly by municipalities above the cutoff that have particularly high enrolment rates, as can be seen in Figure 5.

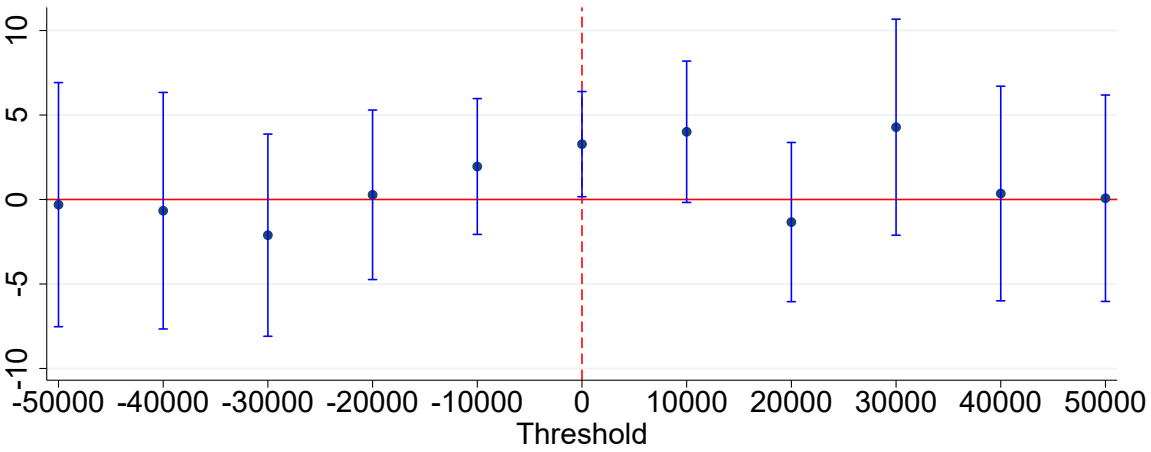


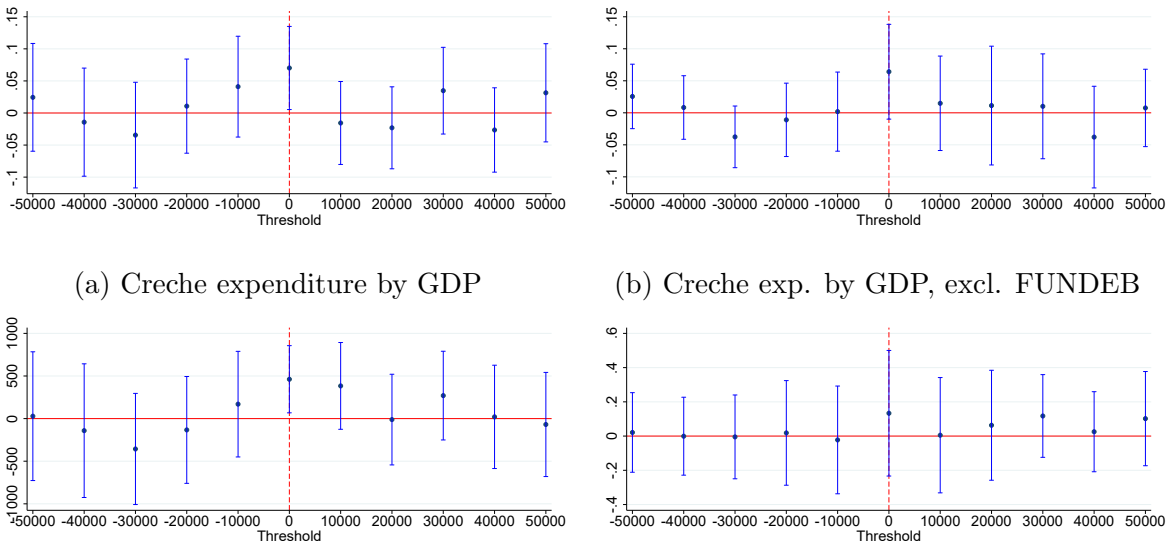
Figure 10: Placebo cutoffs for avg. net creche enrolment

Note: This Figure present estimates of the local treatment effect on net enrolment in municipal crecres using alternative thresholds of the running variable for treatment assignment. The original cutoff of 200,000 is represented by 0, meaning no change, while values in the right represent increases in the cutoff and values to the left reductions. Treatment effects are estimated based on Equation 1, using symmetric bandwidths of 50,000, linear specification for the running variable, and uniform kernel. Standard errors are clustered at the State level.

Figure 11 present the robustness of our results on expenditure outcomes to placebo cutoff. Again, we find no significant treatment effect estimate at alternative thresholds.

5.3 Population Density

The last robustness exercise we conduct concerns the inclusion of population density as an addition control in our estimate. We do so because our validation tests on the balance on pre-treatment covariates (Figure 4) has detected that population density is larger in municipalities just below the cutoff, which might have some influence on the



(a) Creche expenditure by GDP

(b) Creche exp. by GDP, excl. FUNDEB

(c) Creche expenditure per capita

(d) ECEC budget by GDP

Figure 11: Placebo cutoffs for expenditure variables

Note: These Figures present estimates of the local treatment effect on expenditure outcomes using alternative thresholds of the running variable for treatment assignment. The original cutoff of 200,000 is represented by 0, meaning no change, while values in the right represent increases in the cutoff and values to the left reductions. Treatment effects are estimated based on Equation 1, using symmetric bandwidths of 50,000, linear specification for the running variable, and uniform kernel. Standard errors are clustered at the State level.

provision of ECEC services. Therefore, the specification in Equation 1 is extended to include a population density interected with the treatment assignment variable.

Results are presented in Table 6. We can see that most of the point estimates do not vary considerably when we include this control variable, although most become more imprecise. In particular, the treatment effect for creche enrolment rates reduces to around 2.5pp when we consider a window of 50,000 voters, but change only marginally if we include the larger window of 100,000. The estimate for expenditure outcomes are mostly not significant. The most robust results concerns creche expenditures excluding FUNDEB as share of GDP, which remains close to the original estimates of around 0.06pp larger in treated municipalities

6 Conclusions

This paper has exploited a regression discontinuity design to investigate whether higher levels of political competition are associated with higher levels of ECEC provision

Table 6: Effect of dual-ballot on main outcomes - Controlling for Population Density

	(1)	(2)	(3)	(4)
	50,000 bandwidth		100,000 bandwidth	
	Poly. order = 1	Poly. order = 2	Poly. order = 1	Poly. order = 2
Net enrolment in municipal creches	2.612*	2.331	3.463*	3.438**
	(1.74)	(0.91)	(2.05)	(2.09)
Observations	295	295	700	700
Creche expenditure per capita	326.0	127.0	311.8	457.8**
	(1.39)	(0.63)	(1.31)	(2.26)
Observations	172	172	394	394
Creche expenditure as share of GDP	0.0483	0.0186	0.0305	0.0342
	(1.36)	(0.64)	(1.25)	(1.16)
Observations	172	172	394	394
Creche exp. excluding FUNDEB (% GDP)	0.0592**	0.0567**	0.0254	0.0659*
	(2.18)	(2.22)	(1.56)	(2.03)
Observations	164	164	376	376
ECEC budget as share of GDP	0.126	0.181	0.148***	0.0999
	(1.26)	(1.65)	(2.92)	(1.03)
Observations	220	220	508	508

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are measured at the end of the mandate in the next election. All estimates include election-year fixed effects and population density interacted with the treatment dummy. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

in Brazil. Our findings suggest that ECEC policies are impacted by the electoral rule that introduces a dual-ballot voting system at the 200,000 voter threshold in Brazilian municipalities.

This threshold has been found to be associated with higher levels of ECEC coverage: average municipal crèche net enrolment rates were found to be around 3 pp higher across the threshold. This is a relevant increase considering the low levels of the mean net municipal crèche and enrolment in our sample across the whole period, of around 8%. These results are robust to different discontinuity ranges and model specifications. In terms of average expenditure rates we also found that the municipalities with dual-ballot have higher levels of estimated ECEC budgeted expenditure rates, which encompass both crèche and preschool. However this result was not as robust as the ones specifically on crèche expenditure rates, which we found to be also higher across the threshold. These outcomes, however, were mostly measured at more recent years. In the future, once more data is available, it would be interesting to revisit this analysis.

In general, our study highlight the relevance of political economy factors in determining the expansion of ECEC services, which have been shown to play an important

role in child development and has been set as an important SDG. Jointly with results from Chamon et al. (2019) and Chin (2021), it suggests that expanding the political representation of minority groups, in our case, women and children, might be essencial in assuring that government meet target on the coverage of these services for all boys and girls.

Our study is restricted to quantitative indicators of ECEC provision. Given the importance of quality of childcare provision to achieve positive outcomes (Duncan et al., 2022), further analyses could be dedicated to investigating the impact of political economy factors on service quality.

References

ABRINQ. Estratificação da população estimada pelo IBGE segundo faixas etárias. URL https://observatoriocrianca.org.br/cenario-infancia/temas/populacao/1048-estratificacao-da-populacao-estimada-pelo-ibge-segundo-faixas-etarias?filters=1,1626.

D. Altman and R. Castiglioni. Determinants of Equitable Social Policy in Latin America (1990–2013). *Journal of Social Policy*, pages 1–22, Oct. 2019. ISSN 0047-2794, 1469-7823. doi: 10.1017/S0047279419000734. URL https://www.cambridge.org/core/product/identifier/S0047279419000734/type/journal_article.

P. Azar. Politics as a determinant of primary school provision: the case of Uruguay. *Cliometrica*, 2021. doi: 10.1007/s11698-021-00228-3. URL https://www.scopus.com/inward/record.uri?eid=2-s2.0-85104132238&doi=10.1007%2fs11698-021-00228-3&partnerID=40&md5=89d836960d9c326dbfa241d213d3b03c.

S. Bartolini. Collusion, Competition and Democracy: Part I. *Journal of Theoretical Politics*, 11(4):435–470, Oct. 1999. ISSN 0951-6298. doi: 10.1177/0951692899011004001. URL https://doi.org/10.1177/0951692899011004001. Publisher: SAGE Publications Ltd.

S. Bartolini. Collusion, Competition and Democracy: Part II. *Journal of Theoretical Politics*, 12(1):33–65, Jan. 2000. ISSN 0951-6298. doi: 10.1177/0951692800012001002. URL https://doi.org/10.1177/0951692800012001002. Publisher: SAGE Publications Ltd.

S. Berlinski and N. Schady. The early years: Child well-being and the role of public policy, 2015.

G. Bonoli and F. Reber. The political economy of childcare in oecd countries: Explaining cross-national variation in spending and coverage rates. *European Journal of Political Research*, 49(1):97–118, 2010.

Brazil. Constituição, 1988. URL http://www.planalto.gov.br/ccivil_03/constituicao/constituicao.htm.

Brazil. L12796, 2013a. URL http://www.planalto.gov.br/ccivil_03/_Ato2011-2014/2013/Lei/L12796.htm.

Brazil. FUNDEB - Manual de orientação, 2013b. URL http://mpeduc.mp.br/mpeduc/www2/controle_social/arquivos/manual_fundeb_2013.pdf.

Y. Brilli, D. Del Boca, and C. D. Pronzato. Does child care availability play a role in maternal employment and children's development? evidence from italy. *Review of Economics of the Household*, 14(1):27–51, 2016.

L. Bursztyn. Poverty and the Political Economy of Public Education Spending: Evidence from Brazil. *Journal of the European Economic Association*, 14(5):1101–1128, Oct. 2016. ISSN 15424766. doi: 10.1111/jeea.12174. URL https://academic.oup.com/jeea/article-lookup/doi/10.1111/jeea.12174.

S. Calonico, M. D. Cattaneo, and R. Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326, 2014.

M. D. Cattaneo, M. Jansson, and X. Ma. Manipulation Testing Based on Density Discontinuity. *The Stata Journal*, 18(1):234–261, Mar. 2018. ISSN 1536-867X. doi: 10.

1177/1536867X1801800115. URL https://doi.org/10.1177/1536867X1801800115. Publisher: SAGE Publications.

M. D. Cattaneo, N. Idrobo, and R. Titiunik. *A Practical Introduction to Regression Discontinuity Designs: Foundations*. Elements in Quantitative and Computational Methods for the Social Sciences. Cambridge University Press, 2020. doi: 10.1017/9781108684606.

M. Chamon, S. Firpo, J. M. d. Mello, and R. Pieri. Electoral rules, political competition and fiscal expenditures: regression discontinuity evidence from brazilian municipalities. *The Journal of Development Studies*, 55(1):19–38, 2019.

M. Chin. When do politicians appeal broadly? the economic consequences of electoral rules in brazil. *IMF Working Papers*, 2021(227):A001, 2021. doi: 10.5089/9781513595795.001.A001. URL https://www.elibrary.imf.org/view/journals/001/2021/227/article-A001-en.xml.

J. P. Couyoumdjian and J. B. Londregan. Cultivating votes in rural chile. *Legislative Studies Quarterly*, 37(3):355–388, 2012.

F. C. Curran. Expanding downward: Innovation, diffusion, and state policy adoptions of universal preschool. *UMBC Faculty Collection*, 2015.

J. Currie. Early childhood education programs. *Journal of Economic perspectives*, 15(2): 213–238, 2001.

DATASUS. População Residente - Brasil. URL http://tabnet.datasus.gov.br/cgi/deftohtm.exe?ibge/cnv/popbr.def.

A. E. Devercelli and F. Beaton-Day. *Better Jobs and Brighter Futures: Investing in Childcare to Build Human Capital*. World Bank, Dec. 2020. doi: 10.1596/35062. URL http://elibrary.worldbank.org/doi/book/10.1596/35062.

G. Duncan, A. Kalil, M. Mogstad, and M. Rege. Investing in early childhood development in preschool and at home. 2022.

M. Duverger. *Political parties*. Wiley, New York, 1954.

D. Evans and K. Kosec. *Early child education: Making programs work for Brazil's most important generation*. World Bank Publications, 2012.

E. Faur. A widening gap? the political and social organization of childcare in argentina. *Development and Change*, 42(4):967–994, 2011.

FNDE. Portal do FNDE - Proinfância. URL https://www.fnde.gov.br/programas/proinfancia.

B. J. Fried. Distributive Politics and Conditional Cash Transfers: The Case of Brazil's Bolsa Familia. *World Development*, 40(5):1042–1053, May 2012. ISSN 0305750X. URL http://login.ezproxy.ub.unimaas.nl/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=1294666&site=ehost-live&scope=site.

T. Fujiwara. A Regression Discontinuity Test of Strategic Voting and Duverger's Law. *Quarterly Journal of Political Science*, 6(3-4):197–233, Dec. 2011. ISSN 15540634. doi: 10.1561/100.00010037. URL http://www.nowpublishers.com/article/Details/QJPS-10037.

C. Garay. *Social Policy Expansion in Latin America*. Cambridge University Press, 2016. doi: 10.1017/9781316585405.

J. L. García, J. J. Heckman, D. E. Leaf, and M. J. Prados. Quantifying the life-cycle benefits of an influential early-childhood program. *Journal of Political Economy*, 128 (7):2502–2541, 2020.

A. V. A. Gomes. Educação infantil no PNE 2014-2024: acesso, equidade e qualidade. Technical report, Câmara dos Deputados, Brasília, 2017. URL https://www2.camara.leg.br/atividade-legislativa/estudos-e-notas-tecnicas/publicacoes-da-consultoria-legislativa/areas-da-conle/tema11/educacao-infantil-no-pne-2014-2024-acesso-equidade-e-qualidade-1.

L. González. Electoral Competition and Social Spending in the Argentine Provinces. *Journal of Politics in Latin America*, 9(1):91–124, Apr. 2017. ISSN 1866-802X, 1868-4890. doi: 10.1177/1866802X1700900104. URL http://journals.sagepub.com/doi/10.1177/1866802X1700900104.

R. Gouvêa and D. Girardi. Partisanship and local fiscal policy: Evidence from brazilian cities. *Journal of Development Economics*, 150:102637, 2021.

J. Heckman, R. Pinto, and P. Savelyev. Understanding the mechanisms through which an influential early childhood program boosted adult outcomes. *American Economic Review*, 103(6):2052–86, 2013.

R. Hecock. Electoral competition, globalization, and subnational education spending in Mexico, 1999-2004. *American Journal of Political Science*, 50(4):950–961, 2006. doi: 10.1111/j.1540-5907.2006.00226.x. URL https://www.scopus.com/inward/record.uri?eid=2-s2.0-33748760096&doi=10.1111%2fj.1540-5907.2006.00226.x&partnerID=40&md5=2f31fbf139ac2486ff328982c75c63e4.

M. R. R. Hernández, P. M. Reyes, and M. M. Bárcenas. Presidential elections and public expenditure in social development in Mexico, 1995 – 2016. *Ensayos Revista de Economía (Ensayos Journal of Economics)*, 38(1):41–60, May 2019. ISSN 2448-8402. doi: 10.29105/ensayos38.1-2. URL http://ensayos.uanl.mx/index.php/ensayos/article/view/125. Number: 1.

T. Hieda. Politics of childcare policy beyond the left–right scale: Post-industrialisation, transformation of party systems and welfare state restructuring. *European Journal of Political Research*, 52(4):483–511, 2013.

E. Huber and J. D. Stephens. *Democracy and the Left: Social Policy and Inequality in Latin America*. University of Chicago Press, Sept. 2012. ISBN 978-0-226-35655-6. Google-Books-ID: Rk0bYjb_NYsC.

IBGE. Censo Demográfico | IBGE. URL https://www.ibge.gov.br/estatisticas/sociais/populacao/9662-censo-demografico-2010.html?=&t=microdados.

IBGE. Síntese de Indicadores Sociais: uma análise das condições de vida da população brasileira. Technical Report 39, IBGE, Rio de Janeiro, 2018. URL https://biblioteca.ibge.gov.br/visualizacao/livros/liv101629.pdf.

G. W. Imbens and T. Lemieux. Regression discontinuity designs: A guide to practice. *Journal of econometrics*, 142(2):615–635, 2008.

INEP. Resultados. URL https://www.gov.br/inep/pt-br/areas-de-atuacao/pesquisas-estatisticas-e-indicadores/censo-escolar/resultados.

M. Laakso and R. Taagepera. "Effective" Number of Parties: A Measure with Application to West Europe. *Comparative Political Studies*, 12(1):3–27, Apr. 1979. ISSN 0010-4140. doi: 10.1177/001041407901200101. URL https://doi.org/10.1177/001041407901200101. Publisher: SAGE Publications Inc.

M. Mateo Díaz and L. Rodriguez-Chamussy. Childcare policies: Key for female labor participation. 2016.

MoE. PNE - Plano Nacional de Educação - Plano Nacional de Educação - Lei n° 13.005/2014. URL http://pne.mec.gov.br/18-planos-subnacionais-de-educacao/543-plano-nacional-de-educacao-lei-n-13-005-2014.

E. Neimanns. Preferences, vote choice, and the politics of social investment: Addressing the puzzle of unequal benefits of childcare provision. *Journal of Social Policy*, pages 1–20, 2021.

M. J. Neuman and S. Powers. Political prioritization of early childhood education in low-and middle-income countries. *International Journal of Educational Development*, 86:102458, 2021.

J. Nicolau. Dados eleitorais do Brasil. URL http://jaironicolau.github.io/deb/.

S. Niedzwiecki and J. Pribble. Social Policies and Center-Right Governments in Argentina and Chile. *Latin American Politics and Society*, 59(3):72–97, 2017.

ISSN 1531426X. URL http://login.ezproxy.ub.unimaas.nl/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=1671476&site=ehost-live&scope=site.

M. Penfold-Becerra. Clientelism and Social Funds: Evidence from Chavez's Misiones. *Latin American Politics and Society*, 49(4):63–84, 2007. ISSN 1531426X. URL http://login.ezproxy.ub.unimaas.nl/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=0957344&site=ehost-live&scope=site.

T. J. Power and R. Rodrigues-Silveira. Replication Data for: Mapping Ideological Preferences in Brazilian Elections, 1994-2018: A Municipal-Level Study. May 2019. doi: 10.7910/DVN/5P03UL. URL https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/5P03UL. Publisher: Harvard Dataverse type: dataset.

N. R. Schady. The Political Economy of Expenditures by the Peruvian Social Fund (FONCODES), 1991-95. *American Political Science Review*, 94(2):289–304, June 2000. ISSN 00030554. URL http://login.ezproxy.ub.unimaas.nl/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=0549763&site=ehost-live&scope=site.

R. Schneider, D. Athias, and M. Bugarin. Does Enfranchisement Affect Fiscal Policy? Theory and Empirical Evidence on Brazil. *Economics of Governance*, 20(4):389–412, Dec. 2019. ISSN 14356104. URL https://mu.idm.oclc.org/login?url=https://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=1802054&site=ehost-live&scope=site.

SICONFI. Siconfi - Secretaria do Tesouro Nacional (STN) - Versão: 2.0 Contas Anuais. URL https://siconfi.tesouro.gov.br/siconfi/pages/public/consulta_finbra/finbra_list.jsf.

A. Simpser, L. Duquette-Rury, J. A. H. Company, and J. F. Ibarra. The Political Economy of Social Spending by Local Government: A Study of the 3×1 Program

in Mexico. *Latin American Research Review*, 51(1):62–83, 2016. ISSN 1542-4278. doi: 10.1353/lar.2016.0013. URL https://muse.jhu.edu/article/617801.

N. B. Sugiyama. The diffusion of Conditional Cash Transfer programs in the Americas. *Global Social Policy*, 11(2-3):250–278, Dec. 2011. ISSN 1468-0181. doi: 10.1177/1468018111421295. URL https://doi.org/10.1177/1468018111421295. Publisher: SAGE Publications.

A. Tribin. Chasing votes with the public budget. *European Journal of Political Economy*, 63:101875, June 2020. ISSN 01762680. doi: 10.1016/j.ejpoleco.2020.101875. URL https://linkinghub.elsevier.com/retrieve/pii/S0176268020300239.

TSE. Pesquisas Eleitorais. URL https://www.tse.jus.br/hotsites/pesquisas-eleitorais/index.html.

B. Wampler, N. B. Sugiyama, and M. Touchton. *Democracy at Work: Pathways to Well-Being in Brazil*. Cambridge University Press, Sept. 2019.

7 Appendix

Table A1: Descriptive Statistics: main outcome variables, mandate averages, whole sample

	Full Sample				
	Mean	SD	Min	Median	Max
Number of voters"	166648	52414.75	100087.00	153027	299309.00
Municipal creche net enrolment	8.7	9.37	0.00	5.5	46.00
ECEC budget by GDP	.43	0.34	0.00	.38	2.77
ECEC budget per capita	1767	1714.86	0.00	1422	12475.80
Creche expenditure by GDP	.14	0.12	0.00	.11	0.81
Creche expenditure by GDP, exclud. FUNDEB	.095	0.08	0.00	.075	0.53

Note: our main outcome variables of interest are the estimated crèche municipal enrolment rates and the estimated ECEC and crèche expenditure rates. This table reports descriptive statistics for the mandate averages of each outcome variable. Sample is restricted to municipalities with between 100,000 and 300,000 voters in the elections from 1996 to 2016.

Table A2: Descriptive statistics, political variables

	Control			Treatment			Full Sample				
	N	Mean	SD	N	Mean	SD	Mean	SD	Min	Median	Max
Number of candidates	444	4.39	1.44	165	5.59	1.78	4.72	1.63	2.00	5.00	13.00
Effective number of candidates	444	2.55	0.68	165	2.77	0.86	2.61	0.74	1.00	2.49	5.70
Vote share 3rd and lower	444	15.93	12.16	165	20.40	13.19	17.14	12.60	0.00	15.55	56.24
Female candidates (%)	440	11.11	15.39	163	11.41	13.23	11.19	14.83	0.00	0.00	75.00
Nonwhite candidates (%)	93	27.26	28.67	43	29.42	26.41	27.94	27.90	0.00	21.11	100.00
Complete College (%)	440	70.08	23.98	163	72.72	21.02	70.79	23.23	0.00	75.00	100.00
Congruense President-Mayor	519	0.58	0.24	182	0.59	0.23	0.58	0.24	0.03	0.66	1.00
Mayor Party in President Coal	521	0.70	0.46	182	0.69	0.46	0.70	0.46	0.00	1.00	1.00
Share of voter excl frontrunner	521	0.90	0.03	182	0.91	0.03	0.90	0.03	0.72	0.91	0.95
Diff two frontrunners	521	0.05	0.05	182	0.05	0.05	0.05	0.05	0.00	0.03	0.28
Voter turnout	521	0.85	0.04	182	0.85	0.03	0.85	0.04	0.69	0.85	0.95
Left-wing mayor	444	0.42	0.49	165	0.42	0.49	0.42	0.49	0.00	0.00	1.00

Table A3: Descritive statistics, pre-treatment covariates

	Control			Treatment			Full Sample				
	N	Mean	SD	N	Mean	SD	Mean	SD	Min	Median	Max
Dependency ratio	503	0.63	0.09	166	0.60	0.08	0.62	0.09	0.46	0.60	1.00
Income	513	61756.49	21950.92	178	73602.06	22171.75	64807.88	22594.63	19927.17	62761.79	136781.98
Number of children	513	8.07	3.28	178	7.46	3.05	7.91	3.23	2.60	7.51	26.28
Gini index	513	0.53	0.06	178	0.53	0.05	0.53	0.06	0.39	0.54	0.66
Household size	513	5.10	0.57	178	4.95	0.49	5.06	0.55	4.10	4.94	7.52
Years of schooling	513	4.44	0.78	178	4.99	0.76	4.58	0.81	2.16	4.61	7.51
Nonwhite (%)	513	42.10	24.57	178	39.04	21.98	41.31	23.95	3.75	38.66	87.38
Rural (%)	513	8.88	8.68	178	5.50	5.55	8.01	8.12	0.00	5.42	48.55
Female (%)	513	50.63	1.02	178	51.04	1.06	50.73	1.05	46.09	50.65	53.75
Laborforce participation	513	62.36	3.44	178	63.04	3.18	62.54	3.39	54.79	62.54	71.63
Female laborforce participation	513	40.76	5.15	178	43.35	4.44	41.43	5.10	22.87	41.61	53.40
Illiterate 16-64	513	12.40	7.42	178	9.42	4.69	11.63	6.95	2.73	9.50	45.61
Share 0 to 3	503	8.97	1.32	166	8.59	1.27	8.87	1.32	5.57	8.73	12.72
Share 4 to 5	503	4.60	0.67	166	4.37	0.63	4.54	0.67	3.00	4.50	6.66
Density	513	735.24	1527.26	178	1311.85	2311.79	883.77	1779.16	2.98	299.39	9944.20

Table A4: Effect of dual-ballot on outcomes at election years

	(1)	(2)	(3)	(4)
	50,000 bandwidth		100,000 bandwidth	
	Poly. order = 1	Poly. order = 2	Poly. order = 1	Poly. order = 2
Net enrolment in municipal creches	2.934*	1.496	3.656*	2.521
	(2.07)	(0.78)	(2.03)	(1.63)
Observations	233	233	563	563
Creche expenditure per capita	786.1***	291.4	464.6	684.3*
	(3.15)	(0.72)	(1.33)	(1.99)
Observations	103	103	240	240
Creche payments as share of GDP	0.0694*	-0.00477	0.0470*	0.0516
	(1.91)	(-0.09)	(1.92)	(1.25)
Observations	103	103	240	240
ECEC budget as share of GDP	0.0188	-0.0282	0.0906	-0.0373
	(0.27)	(-0.30)	(1.48)	(-0.44)
Observations	196	196	464	464
Creche expenditures excluding FUNDEB (% GDP)	0.0410	0.00358	0.0124	0.0291
	(1.01)	(0.11)	(0.60)	(0.69)
Observations	95	95	218	218

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are measured at the end of the mandate in the next election. All estimates include election-year fixed effects. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Effect of dual-ballot on other creche outcomes

	50,000 bandwidth		100,000 bandwidth	
	Poly. order = 1	Poly. order = 2	Poly. order = 1	Poly. order = 2
Net enrolment in State creches	-0.00770	-0.0329	0.0134	-0.0195
	(-0.14)	(-0.40)	(0.29)	(-0.34)
Observations	297	297	705	705
Net enrolment in public and PPP	3.430**	3.396	3.278	4.299**
	(2.24)	(0.89)	(1.10)	(2.76)
Observations	220	220	514	514
Net enrolment in public creches	3.273**	3.104	3.429	3.392**
	(2.59)	(1.26)	(1.62)	(2.53)
Observations	297	297	705	705
Net enrolment in private creches	0.663	0.704	-0.272	0.962
	(1.15)	(0.96)	(-0.44)	(1.36)
Observations	297	297	705	705
Net enrolment in PPP creche	-0.655	-0.393	-0.913*	-0.143
	(-1.38)	(-0.64)	(-1.76)	(-0.19)
Observations	220	220	514	514
Net enrolment in municipal and PPP	3.585**	3.963	3.354	4.416***
	(2.46)	(1.09)	(1.11)	(2.91)
Observations	220	220	514	514
Gross enrolment in municipal creches	3.160	2.972	3.155	3.187
	(1.61)	(1.05)	(1.11)	(1.63)
Observations	297	297	705	705

Note: The RDD estimates use bandwidths of 50,000 and 100,000 around the cutoff of 200,000 voters and fit local polynomials of order 1 and 2 in columns (1) and (3) and (2) and (4), respectively. All estimates use uniform kernel. The dependent variables are four-year averages of each mandated. All estimates include election-year fixed effects. Standard errors are clustered at the State level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$