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When Do Politicians Appeal Broadly? The Economic Consequences of Electoral Rules in Brazil[†]

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Electoral rules determine how voters' preferences are aggregated and translated into political representation. Using a regression discontinuity design, I contrast single- and two-round elections in Brazil. In two-round elections, the eventual winner must obtain at least 50 percent of the vote. I show that two-round elections provide incentives for candidates to secure a broader base of support and provide public goods more broadly. Candidates represent a more geographically diverse group of voters, public schools have more resources, and there is less variation in resources across schools. Effects appear to be driven by strategic responses of candidates rather than differential entry into races. (*JEL* D72, H41, H75, I21, I28, O15, O17)

Electoral rules determine how voters' preferences are aggregated and translated into political representation, and their design can lead to the election of representatives who represent broader or narrower constituencies. This is particularly important given the evidence that more inclusive political institutions are beneficial for long-term growth (Acemoglu and Robinson 2008).

By defining the coalitions that politicians need in order to win, electoral rules can generate incentives for politicians to appeal to broader groups of voters (Myerson 1993; Lizzeri and Persico 2005). This broad appeal can lead politicians to provide public goods with broader benefits (Persson and Tabellini 1999; Lizzeri and Persico 2001) or engage in less targeting of public spending to specific groups of voters (Milesi-Ferretti, Perotti, and Rostagno 2002; Genicot, Bouton, and Castanheira 2021). On the other hand, factors such as strategic voting can result in the Condorcet loser winning even with rules that appear to encourage broad representation (Bouton 2013). Despite this rich theoretical literature, there

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is limited causal evidence of the political impacts of electoral rules and how these translate into economic policy.

This article seeks to identify how one difference in electoral rules—namely, if elections feature a single round or two rounds—affects the extent to which politicians appeal to a broader constituency and how this, in turn, affects the level of public goods provided and the manner in which they are allocated across the electorate. Single- and two-round systems are the most widely used rules in democratic presidential elections (Bormann and Golder 2013), but endogenous selection of electoral institutions makes studying their causal effects difficult (Cusack, Iversen, and Soskice 2007). I take advantage of a policy in Brazil that assigns a municipality's electoral rule based on a threshold of 200,000 registered voters. Municipalities below this threshold elect their mayor in a single-round election, and municipalities above this threshold elect their mayor in a two-round election.

In a single-round election, voters vote once, and the candidate with the most votes wins. In a two-round election, voters vote once, and if no candidate receives a majority, they vote a second time to choose between the top two candidates. This difference generates two important distinctions. First, two-round elections require winners to attain a vote share above 50 percent. Second, the existence of a second round effectively limits the number of candidates (Lizzeri and Persico 2005). Even in the first round, the top candidate effectively only needs to be concerned with the runner-up, who either threatens victory in the first round or will be the opposition in the second. These distinctions can create incentives for candidates to secure a broader base of support in two-round elections (Bouton 2013; Bouton and Gratton 2015).[1] The intuition is that the rules in a two-round election make it more difficult for politicians to win with policies that appeal to a narrow group of voters. To the extent that politicians commit to their campaign promises, these policies can have economic consequences by reducing incentives for elected representatives to provide public goods supported by a narrow constituency.

To examine this intuition empirically, I exploit Brazil's threshold-based rule to assign the electoral rule for mayor in each election. I employ a regression discontinuity design across six municipal elections between 1996 and 2016. I estimate the causal effect of the electoral rule by comparing political and economic outcomes in municipalities just above the registered voter threshold with outcomes in those just below it. I obtain three main empirical results.

First, candidates in two-round elections receive broader geographic support, suggesting that two-round elections foster greater inclusiveness. Using highly disaggregated vote counts at each electoral section, I measure the geographic distribution of voters in the first round with (i) indices of voter concentration, to quantify the overall level at which voters for specific candidates are geographically concentrated; and (ii) the standard deviation of candidates' vote shares across electoral sections, to quantify a candidate-level measure of geographic concentration. In two-round municipalities, voters for specific candidates are less geographically concentrated,

[1] While not a focus of this paper, there is also a large literature arguing that two-round elections allow voters to vote more sincerely in the first round (see Bouton et al. 2022 for a review) and to better communicate their policy preferences to candidates (Piketty 2000).

corresponding to a 27.6–44.7 percent reduction from the level in single-round municipalities. The main impact of the electoral rule is on the candidates with a chance of winning—the top two candidates. These results are not due to the increased number of candidates in two-round elections.[2] Further, I find that increased inclusiveness does not only occur through representation but also through voter behavior. Voters are more engaged in the political process, casting significantly fewer blank and invalid ballots in two-round elections.

Second, once in office, politicians elected under two-round elections provide public goods differently—both in the level and distribution of the goods. I measure the provision of a local public good that can be geographically targeted and is controlled by the municipal government: public elementary education. In two-round municipalities, the level of resources present in public schools is 5.7–8.1 percentiles higher in the national distribution, and the standard deviation of these resources across schools is lower. The reduced variation is driven by schools with the fewest resources in the municipality benefiting the most from these additional resources.

Third, these differences in resources improve direct policy outcomes in two-round municipalities. Specifically, dropout rates are lower and literacy rates higher among cohorts of school age during the electoral term. On the other hand, improvements to broader, more downstream economic conditions—such as income, employment, and night lights—are limited.

Turning to mechanisms, I argue that the effects of two-round elections are not driven by differential selection of candidates. I do not find that different types of candidates enter the races, nor do they win. Neither candidates nor winners in two-round elections are observably different from those in single-round elections in terms of demographic characteristics, place of birth, educational attainment, or previous occupation. More candidates enter the race in two-round elections, and these additional candidates are often from smaller parties and have run in previous elections. However, these candidates are not more likely to win.

Instead, I argue that candidates face different strategic incentives in two-round elections, which lead them to adopt different strategies during campaigns. Geographic concentration of voters decreases between the first and second round of two-round elections, suggesting that candidates adjust their strategies between rounds to consolidate their voter bases.[3] Candidates in two-round elections also rely less on donations from corporations to finance their campaigns. To the extent that corporations represent narrower groups in the electorate, this implies that candidates adopt strategies that appeal more broadly to individuals rather than corporations.

[2] The effect of the electoral rule on the number of candidates is known as Duverger's Law, which states that single-round elections will lead to a two-party system while two-round elections will lead to a multi-party system. This has been formalized, and sometimes challenged, in recent literature (Osborne and Slivinski 1996; Cox 1997; Callander 2005; Fujiwara 2011; Bouton 2013; Bouton and Gratton 2015).

[3] There is empirical evidence from France that candidates in two-round elections adjust their strategies between rounds. Pons and Tricaud (2018) find that the qualification of a third candidate in the second round reduces the top two candidates' vote shares, indicating that when the third candidate is not present in the second round (as is always the case in Brazil), the top two candidates rally votes from the third candidate's supporters. Le Pennec (2020) analyzes candidates' manifestos between rounds and finds that candidates moderate their discourse in the second round compared to the first.

My results suggest that the different electoral rules between single- and two-round elections lead mayors to employ different strategies during their campaigns. These strategies result in mayors in two-round municipalities building less geographically concentrated constituencies. These political impacts translate into economic policy, as mayors subsequently allocate more public goods in a less geographically concentrated manner.

This paper adds to a growing empirical literature providing causal evidence on the impacts of local electoral rules. These studies, which compare proportional and single-round systems in addition to single- and two-round systems, have measured the impact on electoral outcomes and fiscal expenditures in Italy (Cipullo 2021), France (Eggers 2013), and Morocco (Pellicer and Wegner 2013), as well as Brazil (Fujiwara 2011; Chamon, Mello, and Firpo 2019). Of particular interest are Chamon, Mello, and Firpo (2019) and Bordignon, Nannicini, and Tabellini (2016), who compare single- and two-round elections in Brazil and Italy, respectively. Chamon, Mello, and Firpo (2019) use two-round elections as an instrumental variable for political competition, finding that greater competition in two-round municipalities shifts the composition of fiscal expenditures from current to investment expenditures and increases municipal school construction. By using a regression discontinuity design, my results consider the overall effect of two-round elections, which can operate through multiple channels, including through additional candidates. Bordignon, Nannicini, and Tabellini (2016) find more policy moderation in two-round municipalities, as measured by the volatility of a municipal tax rate across elections. My results complement both Chamon, Mello, and Firpo (2019) and Bordignon, Nannicini, and Tabellini (2016) by studying not only an aggregate policy outcome—the overall level of public goods provision—but also the allocation of this policy across the electorate. This paper's contribution is to provide evidence that electoral rules have economic consequences, both on the level of public goods provision and how these public goods are distributed.

More broadly, this paper connects to a literature on inequalities in the allocation of state resources. A large literature documents the role of political factors in creating these inequalities—in particular, how politicians politically favor certain subgroups, such as those of the same ethnicity or partisanship. A key insight that emerges is that there is less political favoritism when political institutions are stronger, elections are more competitive, and citizens are more broadly engaged in the electoral process (Fujiwara and Wantchekon 2013; Cascio and Washington 2014; Hodler and Raschky 2014; Burgess et al. 2015). Notably, Golden and Min (2013) emphasize the importance of policy responsiveness to voter preferences. Electoral rules serve as a key channel through which voter preferences are translated into policy outcomes. This paper demonstrates the role of another factor in political favoritism—the electoral rule—and the incentives it creates for politicians to broaden their appeal.

This paper is organized as follows: Section I describes the context. Section II describes the empirical strategy. Section III presents the results. Section IV discusses mechanisms. Section V concludes.

I. Institutional Context

A. *Municipal Politics and Public Goods*

Municipal governments in Brazil consist of an executive (a mayor, *prefeito*) and a legislative body (a council of legislators, *camara de vereadores*). Elections are at large and held for all municipal positions simultaneously every four years. Mayors in municipalities with less than 200,000 voters are elected through a single-round election, while in larger municipalities they are elected in a two-round election. In the two-round election in Brazil, if no candidate receives at least 50 percent of the votes in the first round, then a second round is held three weeks later between the top two candidates.[4] Legislators are elected through an open-list proportional system. Brazilian elections are a multiparty system, with over 30 political parties registered. Mayoral candidates are associated with a party and often a coalition of parties, which are formed prior to the election.[5]

Mayors have a broad mandate to provide public goods in education, health, and local infrastructure. Municipal revenue is a combination of state and federal transfers, which comprise the majority of revenues, and local revenues. Municipalities have considerable flexibility in spending these transfers.[6]

The majority of public goods spending is allocated through the annual budgetary process. Funds for specific public works and services can be allocated through bills submitted by the mayor or legislator. While all budgetary actions require joint approval by the mayor and legislature, mayors retain veto power and wield significant influence over the process.

As a result, mayors are important in determining how municipal funds are allocated. This study focuses on public goods provision in elementary education for several reasons. First, a large fraction of the municipal budget is allocated to education: in 2012, it represented 26.3 percent of the budget of municipalities in the sample. Second, municipal education is a geographically localized public good. This feature allows mayors to geographically target public goods provision. Third, unlike other public goods, for which municipalities share joint responsibility with the state or federal government, elementary education is almost entirely under the jurisdiction of the municipality.

B. *Voter Registration and Voting*

State electoral authorities register citizens to vote and maintain electoral rolls. Several features of Brazilian elections, mandated either in the federal constitution or by law, facilitate voter turnout on election day. First, voter registration is compulsory and must be completed at least 151 days prior to the election. Second, voting is

[4] In the sample, a second round occurs 57 percent of the time.

[5] Seats for the legislative council are allocated based on the number of votes received by candidates or parties in the coalition.

[6] Intergovernment transfers are either constitutional automatic transfers or discretionary transfers. Among the automatic transfers, only 30 percent of funds are earmarked, with municipalities only restricted to spending this percentage on health and education.

compulsory for all literate Brazilian citizens between 18 and 69 years of age.[7] Third, elections are held on the first Sunday in October, a day when few voters are at work.

When registering to vote, voters receive an electoral card. The electoral card contains the voter's unique electoral identification number, her assigned voting location, and her assigned electoral section, or a voting machine, within the location. Voting locations are assigned based on the home address the voter registers with and electoral sections are assigned based on the order registrations are received. Although voting locations are not assigned based on distinct geographical districts, voters are assigned the nearest voting location with capacity and typically keep this assignment until they change their home address. At least 60 days prior to the election, electoral authorities publish the allocation of sections among locations in the municipality, with the allocation largely based on the previous election.[8] Among the municipalities in the sample, there are, on average, 527 sections in a municipality and 369 voters registered in each section.

The timing of the announcement of the electoral rule for mayor has varied. In earlier elections, the electoral rule was announced three to four months prior to the election. In more recent elections, the number of registered voters has been regularly published, allowing the electoral rule to be known earlier.

II. Empirical Strategy

A. *Econometric Framework*

The 200,000-registered-voter-threshold rule for mayoral elections provides a natural candidate for a regression discontinuity design (RDD). Assignment of the electoral rule is determined by the running variable, the number of registered voters X_i. The assignment variable D_i takes on the value of $D_i = 0$ if $X_i < 200{,}000$ and $D_i = 1$ if $X_i \geq 200{,}000$.

Following Imbens and Lemieux (2008) and Calonico, Cattaneo, and Titiunik (2014), to estimate the treatment effect at the discontinuity, I use a local linear regression:

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

where for municipality i in election year t, γ_t is an election-year fixed effect and Y_{it} is the outcome of interest. Inclusion of election-year fixed effects approximates an ideal experiment where municipal elections are randomly assigned an electoral rule within a given election year. Equation (1) amounts to fitting two linear regressions using municipality-years to the left and to the right of the threshold. β_1 represents

[7] Voters can justify their absence to a local electoral office. Absent this justification, voters must pay a small fine, and those who fail to vote for three consecutive elections are prevented from accessing public services, such as obtaining a passport or government loans.

[8] Sections must maintain a minimum and maximum number of voters. The electoral code requires sections to have between 50 and 400–500 voters (depending on the location), although states may impose more stringent requirements. Up to election day, electoral authorities may temporarily reassign voters to sections, typically within the same voting location, on a discretionary basis (due to resource constraints, machine malfunctions, etc.). Electoral authorities cannot reassign more than 20 sections on a discretionary basis.

the estimate of the local average treatment effect. Standard errors are clustered at the municipality level.

Because the treatment effect is identified only at the threshold, equation (1) is estimated using municipality-years close to the threshold. The main analysis uses a 50,000-registered-voter window, but robustness is provided for other bandwidths as well as bandwidths selected using data-driven methods (Imbens and Kalyanaraman 2012; Calonico, Cattaneo, and Titiunik 2014).[9]

B. *Identification*

For β_1 to represent the causal effect of the electoral rule, the conditional expectation of the potential outcomes must be continuous at the threshold. In the following section I discuss identification of the RDD estimates.

Violations of Smoothness.—The smoothness assumption can be violated if the threshold choice is motivated by political or economic factors. There appears to be little evidence for this. The choice of 200,000 registered voters as the threshold was somewhat arbitrary and mainly reflected practical concerns regarding the cost of holding a second round (Fujiwara 2011; Chamon, Mello, and Firpo 2019). In addition, given that the threshold was set in the federal constitution in 1988, it is unlikely that politicians chose the threshold anticipating which municipalities would be above or below the threshold in 1996 and later.

A common issue that arises with population-based RDDs is compound treatment when policies other than the electoral rule also change discretely at the threshold (Eggers 2013). While a number of policies in Brazil are implemented using thresholds, these use population counts, which, while highly correlated, do not vary one-to-one with the number of registered voters. To the best of my knowledge, there are no other policies at 200,000 registered voters.[10]

A third possibility is whether municipalities selectively sort across the threshold. Practically, it is difficult for municipalities to do so, since voter registration is mandatory and handled by state electoral authorities. The estimated size of the discontinuity in the density of registered voters at the threshold does not provide evidence of sorting.[11]

[9] Because there is a skewed right tail of municipality sizes from a few large municipalities such as Rio de Janeiro, the data-driven methods would sometimes select bandwidths larger than the support—i.e., larger than 200,000. As a result, these bandwidths are calculated on a subset of elections that lies within the support and is symmetrical around the threshold: 0–400,000 voters.

[10] Two policy thresholds potentially lie close to 200,000 registered voters: a salary cap for local legislators at 300,000 inhabitants and a change in the size of the local legislature at 285,714 inhabitants. Online Appendix B.1 shows that the probability of being above or below these thresholds does not change discontinuously at 200,000 voters, indicating that any effect of these policies is balanced across the electoral-rule threshold. Online Appendix B.6 estimates placebo regressions and shows that the electoral outcomes are smooth at these population thresholds. Because legislator salaries and legislature size can have economic effects, placebo regressions for public goods outcomes are not meaningful.

[11] See online Appendix B.1. The size of the discontinuity in the density of elections at the threshold is 0.168 (p-value 0.392). The size of the discontinuity is estimated based on McCrary (2008).

The last possibility is that potential confounds change discretely at the threshold. I test this by estimating equation (1) on pretreatment characteristics of municipalities. I discuss this in detail in the following section.

Balance on Pretreatment Characteristics.—Since treatment (the two-round election) is determined by the number of registered voters, municipalities can move into treatment or be treated multiple times.[12] As a result, pretreatment can be defined as (i) prior to the introduction of the threshold rule in the 1988 constitution, and (ii) prior to the most recent election when the municipality was untreated, or prior to 1996 if the municipality was never untreated.[13] Outcomes for (i) are measured in the 1980 census. Outcomes for (ii) are measured either in the census prior to the most recent year in a single-round election (the 1991, 2000, or 2010 census) or in the 1991 census for municipalities that held a two-round election in 1996 (Instituto Brasileiro de Geografia e Estatística, 1980, 1991, 2000, 2010).

There is no significant treatment effect on nearly all outcomes measured prior to the 1988 constitution and prior to the most recent election in a single-round election (Table 1). I test for pretreatment imbalance on demographic and economic characteristics, income and demographic segregation, and income inequality.[14] Another concern is that there are factors that change discontinuously at the threshold and that affect which municipalities move into treatment and the length of treatment, but I do not find an imbalance in population growth.[15]

One exception is a large and significant effect on population density. However, there are several reasons that this is likely a false positive. First, much of the effect is driven by a single municipality.[16] There is no visual discontinuity, and the estimate is not robust (online Appendix B.1). Second, the estimate is not significant after Bonferroni adjusting the significance threshold for the number of hypotheses tested. Third, the estimates across the outcomes from the most recent single-round election are not jointly significant ($p = 0.136$).

This imbalance poses an issue if politicians are manipulating the composition and size of the electorate by, say, moving citizens or municipality borders. Such manipulation is unlikely to be motivated by the electoral rule, given that this effect is seen in 1980, prior to the introduction of the threshold rule. In addition, I find no discontinuities in the urbanization rate, changes in municipality area, or population growth across the threshold. Nevertheless, to restore identification in my baseline

[12] Municipalities can also move out of treatment. However, while municipalities do experience population decline, none move below the 200,000-voter threshold in my sample.

[13] The earliest electoral data available is from 1996, so I cannot observe whether municipalities are treated or untreated prior to 1996. Since 1992 is the only unobserved election after the 1988 constitution and only 45 municipalities have moved across the 200,000-voter threshold between 1996 and 2016, it is unlikely that many, if any, municipalities experienced multiple electoral rules between 1992 and 1996.

[14] Demographic characteristics include the share of the population 0–15 years of age. Economic characteristics include unemployment, literacy, and low income rate. Segregation is calculated using the entropy index, with census sectors as the geographic unit (see Section IID for the formula). This was calculated separately for income and demographics. For income, the groups are defined by bins of income relative to the minimum wage. For demographics, the groups are defined by sex, age, and literacy. Income inequality is measured using the Gini coefficient.

[15] There is also no discontinuity in the probability of being treated in the previous election. See online Appendix B.1.

[16] Dropping one outlier municipality (Carapicuíba, São Paulo, a municipality in the São Paulo metropolitan area) reduces the coefficient by 38.2 percent.

Table 1—Municipality Pretreatment Characteristics

Panel A. Characteristics measured prior to the 1988 constitution

	Percent illiterate	Percent low income	Unemployment rate	Population density
Two-round	−1.499	−0.751	0.044	−397.678
	(1.057)	(1.148)	(0.145)	(184.347)
Single-round mean	19.029	50.182	1.978	516.067
Observations	293	293	293	293
Municipalities	90	90	90	90

Panel B. Characteristics measured prior to most recent single-round election

	Percent illiterate	Income per capita	Percent low income	Unemployment rate
Two-round	0.014	−18.964	1.211	−0.652
	(0.590)	(33.328)	(2.290)	(1.114)
Single-round mean	7.409	646.917	36.123	11.671
Observations	295	295	295	295
Municipalities	91	91	91	91

	Municipal area change (percent)	Population growth (percent)	Population density	Population ages 0–15 (percent)
Two-round	−0.601	−0.468	−663.035	0.886
	(2.296)	(1.260)	(332.555)	(0.683)
Single-round mean	−3.003	2.863	1,013.396	29.386
Observations	231	295	295	295
Municipalities	74	91	91	91

	Income segregation	Demographic segregation	Gini coefficient
Two-round	−0.003	0.001	0.004
	(0.004)	(0.001)	(0.009)
Single-round mean	0.090	0.028	0.540
Observations	231	231	295
Municipalities	74	74	91

Notes: The table presents RD estimates on outcomes from the 1980 census (panel A) and outcomes either from the census prior to the most recent single-round election or from the 1991 census (panel B). *F*-stat for all treatment effects in panel B is jointly significant: 1.526 ($p = 0.136$). *Percent low income* is the fraction of households earning 0–50 percent of the minimum wage. *Municipal area change* and *Population growth* are the change in municipality area and population, respectively, from the prior census. *Population density* is population per square kilometer. *Population ages 0–15* is the fraction of the population between 0 and 15 years of age. *Income segregation* and *Demographic segregation* are segregation, respectively, of census tracts (measured using the entropy index). *Municipal area change*, *Income segregation*, and *Demographic segregation* are unavailable for the 1991 census. *Single-round mean* refers to the dependent variable mean for single-round municipalities within the bandwidth. The estimation method is local linear regression with election-year fixed effects and a 50,000-voter bandwidth.

specification, I control for population density measured in the census prior to the election, Z_{it}:

$$(2) \qquad Y_{it} = \beta_1 D_{it} + \beta_2 X_{it} + \beta_3 X_{it} \cdot D_{it} + \beta_4 Z_{it} + \beta_5 Z_{it} \cdot D_{it} + \gamma_t + \varepsilon_{it}.$$[17]

[17] Under randomization, including covariates increases precision by correcting for finite-sample imbalance (Imbens and Rubin 2015; Negi and Wooldridge 2021).

C. *Data Sources*

Electoral Data.—Data on municipal elections come from Brazil's electoral authority (Tribunal Superior Eleitoral 1996–2016). The data provide information on the candidates running, the party and coalition each candidate belongs to, and the number of votes received. The data encompass 6 municipal elections between 1996 and 2016, totaling 32,767 elections across 5,568 municipalities. 296 elections fall within the 50,000-registered-voter bandwidth: 33 in 1996, 43 in 2000, 46 in 2004, 55 in 2008, 55 in 2012, and 64 in 2016.

Electoral results are available for each electoral section (*seçao eleitoral*), allowing me to observe at a very fine level the number of votes each candidate receives.[18] I use this to measure the geographic distribution of voters for specific candidates at both an overall and candidate level (see Section IID on the measures used). Baseline results use votes from the first round of elections to allow comparability between single- and two-round elections.

Public Goods Provision in Schools.—To measure public goods provision in elementary education, I use the annual school census from the Ministry of Education (Ministério da Educação 1997–2016). I use the census to calculate the level of resources present in public municipal schools across two categories: equipment and infrastructure.[19] Equipment includes movable elements, such as the number of computers and availability of air conditioning. Infrastructure includes immovable elements, such as the number of classrooms, sanitation, and the availability of a library. Online Appendix A provides a full description of these categories. I construct for each school an index of resources, separately for equipment and infrastructure, by taking the principal component of the elements and computing the school's percentile rank within the country for each year.[20] I aggregate this into a municipality-level measure by taking the average percentile rank across schools in the municipality.

D. *Measuring Geographic Concentration of Voters*

In this section I use the following notation. In municipality m, there are K_m candidates and I_m electoral sections. The number of voters in the municipality and in each section is given by N_m and n_{im}, respectively. The fraction of voters for candidate k in the municipality and in each section is given by p_{mk} and p_{imk}, respectively.

Overall Geographic Concentration of Voters.—To measure the overall geographic concentration of voters, I use three indices from the racial segregation literature that

[18] Electoral results are available at the municipality level but unavailable at the electoral section level for 31 elections in 1996. Electoral section data for one election in 2008 (Rio Branco, in Acre) was dropped because the electoral section data was inconsistent with the municipality results.

[19] Data on equipment in schools is available in the 2000–2016 school census. Data on infrastructure in schools is available in the 1997–2016 school census.

[20] The PCA index is constructed by estimating the first principal component of the underlying variables for each year, using the national sample of public elementary schools. Because the variables in the school census vary from year to year, it is difficult to compare the raw PCA index across years. Calculating a school's percentile rank for each year allows for comparison across years.

measure multigroup spatial segregation: the coefficient of variation, the fractionalization index, and the entropy index. These indices are described in White (1986) and Reardon and Firebaugh (2002). The indices assume a value of one if there is full geographic concentration of voters or where each section contains voters for only one candidate. The indices assume a value of zero if there is full geographic dispersion of voters or where each section contains the same composition of voters as the municipality as a whole. The correlation between these indices is high: between 0.89 and 0.96, in my sample.[21]

The coefficient of variation, s_m, is defined as

$$s_m = \frac{1}{K_m - 1} \sum_{k=1}^{K_m} \sum_{i=1}^{I_m} \frac{n_{im}}{N_m} \frac{(p_{imk} - p_{mk})^2}{p_{mk}}. \tag{3}$$

s_m is interpreted as the square deviation of voter composition in electoral sections from voter composition in the municipality. Dividing by $K_m - 1$ keeps the index between zero and one. When each section has the same composition as the municipality, then $p_{imk} = p_{mk}$ and $s_m = 0$.

The fractionalization index, f_m, is defined as

$$f_m = \frac{\hat{f}_m - \bar{f}_m}{\hat{f}_m}, \quad \text{where} \quad \hat{f}_m = \sum_{k=1}^{K_m} p_{mk}(1 - p_{mk}), \tag{4}$$

$$\bar{f}_m = \sum_{k=1}^{K_m} \sum_{i=1}^{I_m} \frac{n_{im}}{N_m} p_{imk}(1 - p_{imk}).$$

$\hat{f}_m$ is the fractionalization in the municipality and $\bar{f}_m$ is the average fractionalization across electoral sections. Fractionalization, also known as the interaction index, measures the probability that two members within a population chosen at random are from different groups. There are two ways to interpret f_m. One, f_m is the average concentration across sections, normalized by the level in the municipality to keep the index between zero and one. Alternatively, f_m is the fraction of concentration in the municipality that is due to differences in voter composition between sections. When each section has the same concentration as the municipality, or when there are no differences between sections, then $\hat{f}_m = \bar{f}_m$ and $f_m = 0$. When each section contains only one type of voter, or when there are large differences between sections, then $\bar{f}_m = 0$ and $f_m = 1$.

The entropy index, h_m, is defined as

$$h_m = \frac{\hat{h}_m - \bar{h}_m}{\hat{h}_m}, \quad \text{where} \quad \hat{h}_m = -\sum_{k=1}^{K_m} p_{mk} \ln p_{mk}, \tag{5}$$

$$\bar{h}_m = -\sum_{k=1}^{K_m} \sum_{i=1}^{I_m} \frac{n_{im}}{N_m} p_{imk} \ln p_{imk}.$$

[21] The correlation between s_m and f_m is 0.956, between s_m and h_m is 0.892, and between f_m and h_m is 0.929.

$\hat{h}_m$ is the entropy in the municipality, and $\bar{h}_m$ is the average entropy across electoral sections. Entropy measures how far the population is from equal representation of all groups. The interpretation and range of values of h_m are the same as that of f_m.

Sensitivity of Overall Concentration to the Number of Candidates.—There is a concern that the indices may be mechanically related to the number of candidates. The discussion below derives from Reardon and Firebaugh (2002).

For the coefficient of variation, before dividing by $K_m - 1$, the index attains a maximum value of $K_m - 1$ and as a result depends on the number of candidates. Dividing by $K_m - 1$ removes this mechanical effect.

For the fractionalization index, $\bar{f}_m$ attains a maximum of $\hat{f}_m$ and $\hat{f}_m$ attains a maximum of $1 - 1/K_m$. For the entropy index, $\bar{h}_m$ attains a maximum of $\hat{h}_m$ and $\hat{h}_m$ attains a maximum of $\ln K_m$. As a result, before dividing by $\hat{f}_m$ and $\hat{h}_m$, these indices will depend on the number of candidates. Dividing by $\hat{f}_m$ and $\hat{h}_m$ removes part of the mechanical effect.

While these indices will not monotonically depend on the number of candidates, these indices may still be affected by the number of candidates. Section IIID performs several robustness exercises to address this concern.

Candidate-Level Geographic Concentration of Voters.—To capture a candidate-level measure of the spatial distribution of voters, I use the standard deviation in vote shares across electoral sections. This measure describes whether a candidate's supporters are spread across many areas or concentrated within a few sections. For candidates whose voters are spread across many areas in the municipality, the vote share will vary less across sections. The standard deviation in a candidate's vote share, σ_{mk}, is defined as

$$\sigma_{mk} = \left[\frac{1}{I_m - 1} \sum_{i=1}^{I_m} \left(p_{imk} - \frac{1}{I_m} \sum_{i=1}^{I_m} p_{imk} \right)^2 \right]^{1/2}. \tag{6}$$

Interpretation of Geography.—The concentration indices are geographic measures because they are calculated over unique spatial units. While electoral sections are arranged spatially, they do not necessarily represent unique locations, as several sections may be assigned to the same voting location. As a result, variation in the concentration indices may capture variation both across and within locations. While the locations of sections cannot be identified for most of the sample, in 2012 the median voting location contained five sections for the municipalities in the sample. Given that the procedure for assigning voters to sections is mandated in the federal electoral code, it is unlikely that variation in the concentration indices within locations changes discontinuously at the threshold.

To what extent does geographic composition represent the socioeconomic composition of voters? Income segregation is high among municipalities in the sample—on average, they rank in the 73.3 and 79.6 percentiles in the national distribution in the 2000 and 2010 censuses, respectively—suggesting there is a high level of

geographic sorting by income.[22] While imperfect, differences in the geographic composition of voters are likely to reflect differences in the socioeconomic composition of voters.

Using Only the Top Two Candidates.—I also calculate the indices using vote shares from the top two candidates only, i.e., assuming that only the top two candidates are in the race. The advantage is that this fixes the number of candidates and ignores the potential dilution of votes from lower-placed candidates. Note that by construction, the standard deviation of votes for the first-place candidate σ_{m1} is the same as that of the second-place candidate σ_{m2}.

III. The Effect of the Two-Round Election

In this section I investigate the effect of the two-round election and present three main results. One, candidates in two-round elections receive broader geographic support. Two, once in office, mayors elected under two-round elections provide more resources to schools and distribute these resources more equitably. In other words, these politicians are represented by a broader group of voters and in turn provide public goods at a higher level and more broadly. Three, these differences in public goods provision lead to improvements in downstream education outcomes.

A. *Geography of Votes*

Do candidates in two-round elections secure broader bases of support? I provide evidence that in two-round elections voters are less geographically concentrated overall and that it is the top two candidates who receive support from a geographically broader group of voters.

Geographic Concentration of Voters.—Overall, voters for specific candidates are less geographically concentrated in two-round elections, whether concentration is measured by the coefficient of variation, fractionalization index, or entropy index (Figure 1, panels A–C and Table 2, panel A). There is less variation in voter composition between electoral sections; or, in other words, the composition of voters in electoral sections is closer to the composition of voters in the municipality. Two-round elections experience a reduction of 44.7 percent of the single-round mean for the coefficient of variation (0.425 standard deviations, $p = 0.008$), 44.3 percent of the single-round mean for fractionalization (0.548 standard deviations, $p = 0.014$), and 27.6 percent of the single-round mean for entropy (0.397 standard deviations, $p = 0.078$).

Turning to support for each candidate, not all candidates obtain support from geographically broader constituencies. Voters for the top two candidates are less concentrated in two-round elections, but the concentration of voters for the third-

[22] Demographic segregation along age, sex, and literacy is lower: on average, municipalities in the sample rank in the 62.1 and 48.5 percentiles in the 2000 and 2010 censuses, respectively. Income and other demographic characteristics, such as race, are more salient in Brazil.

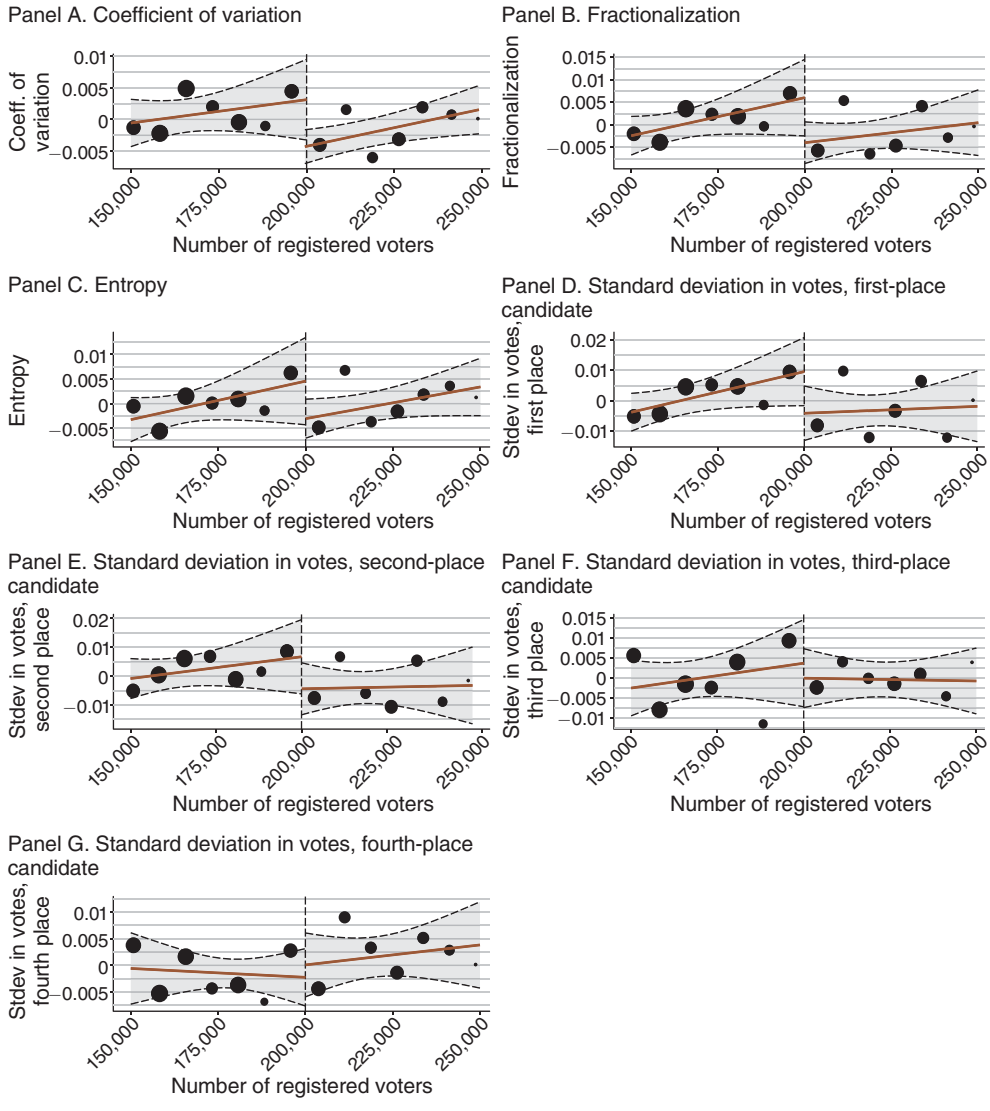

Figure 1. Regression Discontinuity Plots of Concentration of Voters

Notes: The figures plot the overall concentration of voters for specific candidates (panels A–C) and the standard deviation in a candidate's vote shares across electoral sections (panels D–G). Vote shares are from the first round. In each panel, each point plots an average value within a 7,500-voter bin. Variables on the vertical axes are residualized by population density and election-year fixed effects. The diameters of the points are proportional to the number of observations. Confidence intervals (dashed lines) represent the 95 percent confidence intervals of a local linear regression (solid red line) with standard errors clustered at the municipality level.

and fourth-place candidates is not significantly different (Figure 1, panels D–G and Table 2, panel B). Estimates for the top two candidates are similar in magnitude: the reduction in the variance of support across electoral sections is 20.8 percent of the single-round mean for the first-place candidate (0.474 standard deviations, $p = 0.021$) and 18.9 percent of the single-round mean for the second-place

Table 2—Effect on the Geographic Concentration of Voters

	Coefficient of variation	Fractionalization	Entropy	
Panel A. Concentration indices of voters for specific candidates				
Two-round	−0.009	−0.012	−0.008	
	(0.003)	(0.005)	(0.005)	
Potential bias	0.0008	−0.0002	−0.0923	
Single-round mean	0.019	0.027	0.030	
Observations	264	264	264	
Municipalities	89	89	89	
	First place	Second place	Third place	Fourth place
Panel B. Standard deviation in vote shares for each candidate				
Two-round	−0.017	−0.014	−0.005	0.004
	(0.007)	(0.008)	(0.007)	(0.004)
Potential bias	−0.0011	−0.0010	−0.0004	−0.0002
Single-round mean	0.080	0.075	0.042	0.023
Observations	264	264	251	216
Municipalities	89	89	89	84
	Coefficient of variation	Fractionalization	Entropy	Standard deviation of first-place candidate
Panel C. Using vote shares from top two candidates only				
Two-round	−0.015	−0.018	−0.013	−0.015
	(0.008)	(0.009)	(0.007)	(0.009)
Single-round mean	0.036	0.038	0.029	0.088
Observations	264	264	264	264
Municipalities	89	89	89	89

Notes: The table presents RD estimates on the overall concentration of voters for specific candidates (panel A) and the standard deviation in a candidate's vote shares for the first- through fourth-place candidate across electoral sections (panel B). Estimates are also presented for these outcomes calculated using vote shares from the top two candidates only (panel C). Vote shares are from the first round. Observations are at the election level. *Single-round mean* refers to the dependent variable mean for single-round municipalities within the bandwidth. *Potential bias* is the simulated effect on the outcome from having an additional candidate in every single-round election (see Section IIID). The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

candidate (0.430 standard deviations, $p = 0.087$). Estimates for the third- and fourth-place candidates are close to zero and insignificant.

To provide further support that the top two candidates drive the reduced concentration of voters in two-round elections, the effects on overall concentration are larger when calculating these measures using vote shares from the top two candidates only (Table 2, panel C).[23]

Voter Engagement.—The geographic pattern of support indicates that two-round elections lead to greater inclusiveness, as voters from more geographic areas are represented. I also find inclusiveness along another dimension: higher rates of voter

[23] While it is more intuitive to compare vote shares from the first round, the results are robust to using votes from the final round (the first round in single-round elections and the second round in two-round elections). See online Appendix B.2.

Table 3—Effect on Other Electoral Outcomes

	Turnout	Blank/invalid ballots	Number of candidates
Two-round	0.006	−3.821	1.273
	(0.008)	(1.670)	(0.339)
Single-round mean	0.843	16.524	4.604
Observations	296	296	296
Municipalities	92	92	92

Notes: The table presents RD estimates on other electoral outcomes. *Turnout* is the fraction of eligible voters who cast a ballot in the election. *Blank/invalid ballots* is the number of ballots (in thousands) that were either blank or voided. Outcomes are from the first round. Observations are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

engagement in two-round elections. While turnout is unaffected (which is expected, as turnout is mandatory in Brazil), the number of blank and invalid ballots is significantly lower ($p = 0.023$) in two-round municipalities (Table 3). Given evidence that these ballots are often cast by dissatisfied or disinterested voters (Gonzales, León-Ciliotta, and Martínez 2022), the reduction suggests that voters in two-round elections engage in the electoral process at higher rates.[24]

B. *Allocation of Municipal Resources*

I next investigate the impact on public goods provision. If politicians secure broader bases of support in two-round elections, they may also provide public goods differently once they are in office. I provide evidence that two-round elections impact both the level and distribution of resources in municipal schools.

There is a significant increase in resources in municipal schools in two-round municipalities compared to those in single-round municipalities (Figure 2, panels A–B and Table 4, columns 1–2). Schools in two-round municipalities are 8.1 ($p = 0.021$) and 5.7 ($p = 0.084$) percentiles higher in the national distribution of equipment resources and infrastructure resources, respectively. The coefficient on infrastructure resources is smaller and less significant; this may be because infrastructure is difficult to manipulate, as allocating new infrastructure requires more time and capital than allocating equipment.[25]

In addition to differences in the overall levels, resources are distributed more evenly across schools in two-round municipalities (Figure 2, panels C–D and

[24] Ballots can be invalid or blank for a number of reasons. For example, municipalities with more illiterate voters will have more blank and invalid ballots (Fujiwara 2015). Since the illiteracy rate is not discontinuous across the threshold and all municipalities used electronic voting by 2000 (which reduced the number of unintentional errors), I interpret the difference in the number of blank and invalid ballots as voter engagement. Gonzales, León-Ciliotta, and Martínez (2022) provide empirical evidence for this interpretation, as they find that forced electoral participation increases the number of blank and invalid ballots cast. Note that it is not possible to disentangle whether the effect is due to having additional candidates in two-round elections, as in Pons and Tricaud (2018), or due to other aspects of two-round elections.

[25] Empirically, infrastructure resources are less responsive to the electoral cycle (results available on request).

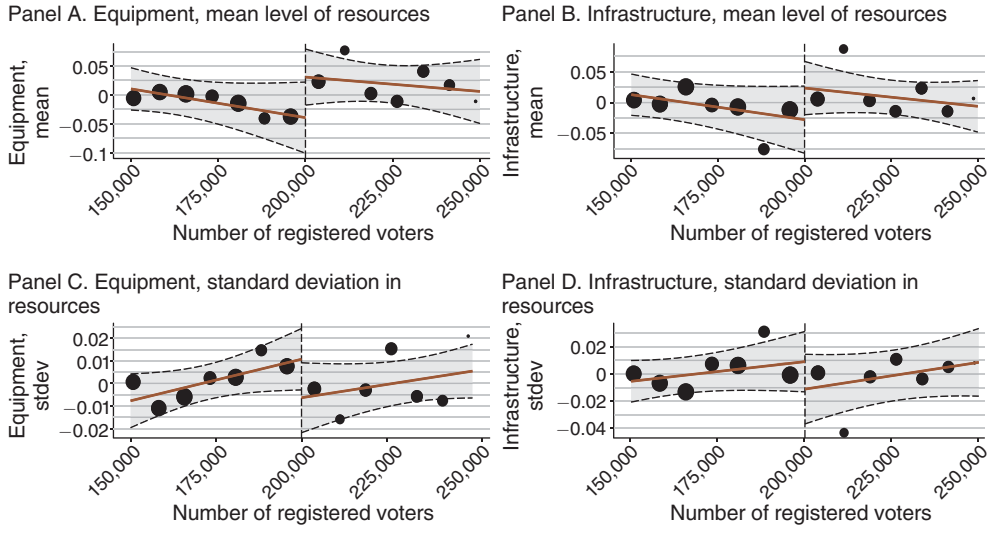

FIGURE 2. REGRESSION DISCONTINUITY PLOTS OF RESOURCES IN MUNICIPAL SCHOOLS

Notes: The figures plot the mean level of resources in schools (panels A–B) and the standard deviation in resources across schools (panels C–D). *Equipment* and *Infrastructure* are indices constructed by taking the first principal component of a school's equipment and infrastructure elements, calculating the school's percentile in the national distribution, and then averaging across schools in the municipality. In each panel, each point plots an average value within a 7,500-voter bin. Variables on the vertical axis are residualized by population density and election-year fixed effects. The diameters of the points are proportional to the number of observations. Confidence intervals (dashed lines) represent the 95 percent confidence intervals of a local linear regression (solid red line) with standard errors clustered at the municipality level.

TABLE 4—EFFECT ON RESOURCES IN MUNICIPAL SCHOOLS

	Mean level of resources		Standard deviation in resources	
	Equipment	Infrastructure	Equipment	Infrastructure
Two-round	0.081	0.057	−0.018	−0.021
	(0.035)	(0.033)	(0.009)	(0.016)
Single-round mean	0.738	0.731	0.121	0.157
Observations	820	912	820	912
Municipalities	79	79	79	79

Notes: The table presents RD estimates on the mean level (first two columns) and standard deviation (last two columns) in resources in municipal schools. *Equipment* and *Infrastructure* are indices constructed by taking the first principal component of a school's equipment and infrastructure elements, calculating the school's percentile in the national distribution, and then averaging across schools in the municipality. Observations are at the year level. *Single-round mean* refers to the dependent variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

Table 4, columns 3–4). The standard deviation in equipment resources across schools is 1.8 percentiles lower in two-round municipalities (14.7 percent of the single-round mean, $p = 0.060$). Although the estimate on the standard deviation in infrastructure resources is of a similar magnitude (-2.1 percentiles), the difference is not significant ($p = 0.190$).

Table 5—Effect on Resources in Schools at Different Parts of the Municipal Distribution

	Mean level of resources in schools at different quartiles			
	First quartile (bottom 25%)	Second quartile	Third quartile	Fourth quartile (top 25%)
Panel A. Equipment				
Two-round	0.082	0.066	0.069	0.038
	(0.035)	(0.037)	(0.042)	(0.029)
Single-round mean	0.652	0.733	0.781	0.856
Observations	700	728	760	748
Municipalities	74	75	74	73
Panel B. Infrastructure				
Two-round	0.116	0.102	0.056	0.013
	(0.046)	(0.047)	(0.035)	(0.021)
Single-round mean	0.540	0.689	0.814	0.914
Observations	776	764	784	780
Municipalities	75	74	75	75

Notes: The table presents RD estimates on the mean level of resources in schools at different parts of the municipal distribution. Dependent variables are the mean level of equipment (panel A) and infrastructure (panel B) elements, separated by quartiles. Quartiles are defined by the school's percentile in the municipal distribution in the year prior to the election. Observations are at the year level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

To better understand how resources are distributed, I estimate effects for schools at different parts of the distribution in the municipality. If the variance in resources is lower, schools with the least (most) resources in the municipality should have more (fewer) resources. I group schools into quartiles defined by each school's percentile in the municipal distribution prior to the election. I find that the increased level of resources is concentrated in schools located at the lower end of the distribution (Table 5). Schools in the bottom quartile are 8.2 percentiles higher in equipment resources ($p = 0.021$) and 11.6 percentiles higher in infrastructure resources ($p = 0.012$). Schools in the second quartile also experience gains, though smaller ones—6.6 percentiles in equipment resources ($p = 0.077$) and 10.2 percentiles in infrastructure resources ($p = 0.031$). There is no significant difference in resources in schools at the top of the distribution.

C. *Downstream Outcomes*

If mayors provide more public goods and distribute them more equitably, do these differences translate into downstream outcomes? I find, in two-round municipalities, improvements in education outcomes but limited effects on economic outcomes.

I measure four education outcomes. Using the school census, I measure the dropout, failing, and passing rates in municipal schools. Using the demographic census, I measure the literacy rate among cohorts who were of elementary school age during the electoral term. In two-round municipalities, drop-out rates are significantly lower—1.65 percentage points ($p = 0.014$) off a baseline of 3.21 percentage

Table 6—Effect on Downstream Municipal Outcomes

	Dropout rate	Failing rate	Passing rate	Elementary literacy rate
Panel A. Education outcomes				
Two-round	−1.649	−0.758	2.330	1.199
	(0.667)	(1.114)	(1.459)	(0.710)
Single-round mean	3.211	8.645	88.283	91.445
Observations	909	909	909	177
Municipalities	79	79	79	71
	Low-income rate	Income per capita	Unemployment rate	Night lights
Panel B. Economic outcomes				
Two-round	−5.186	64.667	−0.964	2.715
	(3.079)	(61.782)	(0.635)	(3.306)
Single-round mean	27.929	762.417	9.815	22.527
Observations	177	177	177	763
Municipalities	71	71	71	80

Notes: The table presents RD estimates on municipal education outcomes (panel A) and economic outcomes (panel B). *Dropout rate*, *Failing rate*, and *Passing rate* are the mean rates across schools in the municipality from the school census. *Elementary literacy rate* is the literacy rate of cohorts who are of elementary school age during the mayoral term from the 2000 and 2010 demographic censuses. *Low-income rate*, *Income per capita*, and *Unemployment rate* are from the 2000 and 2010 demographic censuses. *Low-income rate* is the fraction of households earning between 0 and 50 percent of the minimum wage. *Night lights* is the mean night lights level in the municipality from the 1997–2013 NOAA night lights series. Observations for the dropout rate, failing rate, passing rate, and night lights are at the year level. Observations for the elementary literacy rate, low-income rate, income per capita, and unemployment rate are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

points—and literacy rates are significantly higher among elementary cohorts: 1.20 percentage points ($p = 0.093$) off a baseline of 91.45 percentage points (Table 6, panel A). While differences in the failing and passing rates are not significant, the direction of the estimates suggests improvements in two-round municipalities: failing rates are lower and passing rates higher.

While improvements in education outcomes can lead to improvements in broader economic outcomes, I do not find that this is the case in two-round municipalities (Table 6, panel B). Using the demographic census, I measure the fraction of low-income households, income per capita, and the unemployment rate. The caveat is that these outcomes are measured two to ten years after the corresponding election, and the sample size may be underpowered to detect effects.[26] Using the NOAA night lights series (National Oceanic and Atmospheric Administration, 1997–2016a, 1997–2016b), I measure the mean night lights level in the municipality. The fraction of low-income households is significantly lower in two-round municipalities ($p = 0.094$), but given the size of the coefficient, this may be a false positive. In general, the direction of the estimates suggests that two-round elections lead to improved economic outcomes (income per capita is higher, unemployment is lower, and night lights are higher), but not significantly so.

[26] For the 1996 elections, outcomes are observed four years later in the 2000 demographic census. For the 2000, 2004, and 2008 elections, outcomes are observed ten, six, and two years later in the 2010 demographic census.

There are several reasons why I may not observe improved economic outcomes. The most obvious is that two-round elections have no effect on the broader economy in Brazil. Given the public goods that fall under municipal jurisdiction—namely, health, education, and local infrastructure—mayors' influence over outcomes such as income and employment may be limited. By the same token, improvements in economic outcomes due to mayors' influence may only occur in the long term. Outcomes such as income and night lights may take more than two to ten years to improve. An alternative story is that economic impacts are not experienced in aggregate but instead only among certain populations. For example, I find that increased school resources are concentrated in schools at the bottom of the distribution. This may explain the significant effect on the low-income rate, which reflects improved outcomes for the poorest households, and not on more aggregate economic outcomes.

D. *Robustness of the Main Results*

Bias in Measures of Concentration.—I address two potential sources of mechanical bias in the concentration indices.

One concern is the size of the parcels (here, electoral sections) used to calculate the indices. The number of voters assigned to each section is regulated by the electoral authority, so all sections should be of similar size. Empirically, the number of eligible voters and valid votes in each section varies smoothly across the threshold (results available on request).

A second concern, although potentially a mechanism, is the increased number of candidates in two-round elections (Table 3). As discussed in Section IID, while the indices may be affected by the number of candidates, the direction of bias is not monotonic. Nevertheless, I perform three robustness checks. One, while the number of candidates is an endogenous outcome, including it as a control does not affect the qualitative results (online Appendix B.2). Two, I simulate the effect of an additional candidate in all single-round elections.[27] For most measures, the estimated bias is small (the "Potential bias" row in Table 2). Third, using only the vote shares from the top two candidates does not substantially change the results (Table 2, panel C).

Calculating the Resource Index.—Using z-scores of each school's resources rather than the first principal component to construct the resource index does not affect the qualitative results (online Appendix B.3).[28]

RDD Design.—I investigate the robustness of my results to the regression discontinuity design (online Appendix B.5). The results are not driven by the choice

[27] The simulation adds to each electoral section a candidate who receives a vote share equal to the average vote share of last-place candidates (1.5 percent). A proportionate number of votes is taken from the other candidates to maintain the same total number of voters. The estimated bias is the change between the actual outcome and the simulated outcome.

[28] The z-score index is constructed by rescaling each of the underlying variables for each year, using the mean and standard deviation in the national sample of public elementary schools. The index is the sum of the rescaled components. These values are then aggregated to the municipality level by taking the average across schools in the municipality.

of bandwidth, whether fixed or chosen by a data-driven method. The estimates maintain similar magnitudes and significance for bandwidths out to 150,000 voters, although the estimates for the standard deviation in school resources decline and are not significant at larger bandwidths. The results are also similar when using alternative specifications—namely, dropping all controls, including only election-year fixed effects, including pretreatment characteristics as controls, and a quadratic specification—although the results are noisier for some specifications.

Placebo Tests.—I show that there are no discontinuities at placebo thresholds in registered voters (170,000; 180,000; 190,000; 210,000; 220,000; 230,000), indicating that the outcomes are relatively continuous at places where the treatment does not change (online Appendix B.6). The treatment effect is isolated to the actual threshold: there are no estimates with the same size and significance as at the actual threshold.

IV. Mechanisms

Broadly, there are two reasons why two-round elections can lead to different outcomes. Selection is one mechanism: different types of candidates may enter two-round elections, or different types of candidates may win two-round elections. Strategic incentives are another mechanism: candidates adopt different behaviors during the campaign and in office.[29] In this section, I explore these explanations and provide suggestive evidence that candidates' strategic responses explain a larger part of the effects of the two-round election.

A. *Selection in Candidates*

Candidates in two-round elections may have a broader group of supporters because there are different types of candidates in the electoral races. I do not find differences in the composition of candidates who enter two-round elections in terms of age, sex, educational attainment, state of birth, or occupational background (Table 7).[30] Nor do I find that candidates who win two-round elections are significantly different along these characteristics (Table 8). These results are suggestive, as zero effects and noisy effects cannot always be differentiated and candidates may differ along other characteristics that I am not able to measure. For example, estimates on the fraction of female candidates ($p = 0.109$), the fraction of candidates with a previous business occupation ($p = 0.505$), and the probability of a winner with a previous business occupation ($p = 0.188$) are large relative to the baseline.

[29] A third possibility is that voters behave differently, either through turnout or strategic voting. In Brazil, turnout is mandatory, and so not a major factor. Regarding strategic voting, Fujiwara (2011) finds that lower-place candidates receive higher vote shares in two-round elections and argues that voters behave less strategically. In this paper, I interpret the strategic responses of candidates as an equilibrium outcome that can arise from the electoral rule directly or indirectly through the electoral rule's impact on voter behavior.

[30] Candidate demographic characteristics and occupational backgrounds are unavailable for most elections in 1996.

Table 7—Effect on Characteristics of Candidates

	Age	Female	University degree	Born same state
Panel A. Demographic characteristics of candidates				
Two-round	0.470	−0.065	−0.045	−0.070
	(1.514)	(0.040)	(0.048)	(0.049)
Single-round mean	49.955	0.129	0.796	0.784
Observations	264	263	263	263
Municipalities	89	89	89	89

	Public sector	Technical	Business
Panel B. Previous occupations of candidates			
Two-round	−0.008	0.014	0.017
	(0.063)	(0.059)	(0.025)
Single-round mean	0.466	0.388	0.047
Observations	263	263	263
Municipalities	89	89	89

Notes: The table presents RD estimates on the average characteristics of candidates. *Born same state* is whether the candidate was born in the same state as the election. *Public sector* includes occupations such as elected positions, the judiciary, and workers in public administration. *Technical* includes occupations such as scientists, technicians, and artists. *Business* includes occupations such as administrative positions, workers in commerce and services, and business owners. *Age* is the average age of candidates and *Female*, *University degree*, *Born same state*, and previous occupation are the fraction of candidates with these traits. Observations are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

Table 8—Effect on Characteristics of Winners

	Age	Female	University degree	Born same state
Panel A. Demographic characteristics of winners				
Two-round	0.160	−0.027	0.023	−0.041
	(2.763)	(0.087)	(0.100)	(0.071)
Single-round mean	51.608	0.112	0.832	0.789
Observations	264	263	263	263
Municipalities	89	89	89	89

	Public sector	Technical	Business
Panel B. Previous occupations of winners			
Two-round	−0.088	0.069	0.059
	(0.137)	(0.131)	(0.044)
Single-round mean	0.534	0.348	0.043
Observations	263	263	263
Municipalities	89	89	89

Notes: The table presents RD estimates on the characteristics of winners. *Born same state* is whether the candidate was born in the same state as the election. *Public sector* includes occupations such as elected positions, the judiciary, and workers in public administration. *Technical* includes occupations such as scientists, technicians, and artists. *Business* includes occupations such as administrative positions, workers in commerce and services, and business owners. *Female*, *University degree*, *Born same state*, and previous occupation are indicator variables. Observations are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

Nevertheless, there is not strong evidence that candidates and winners in two-round elections are significantly different along observable characteristics.

There are differences in political affiliation among candidates who enter the races. In two-round elections there are more candidates from small parties ($p = 0.032$) and who previously ran as mayoral candidates ($p = 0.038$), but they are not more likely to win (Table 9).[31] Incumbent candidates are less likely to win two-round elections, but this result is not robust to other bandwidths; nor do the treatment effects depend on the mayor's incumbency status (online Appendix B.7). Since smaller parties are more likely to appeal to narrower electorates, I argue that this cannot explain the reduced concentration in vote shares.

Why do small candidates enter two-round elections if they are not more likely to win? One possible motivation is to build support for subsequent elections, which explains why more candidates with previous campaign experience enter. A second motivation is to gain positions in the elected administration. Third, candidates may seek to influence the top candidates' platforms, either through the political competition they pose or through direct bargaining. The following section considers how top candidates' strategies may be influenced in two-round elections.[32]

B. *Strategic Responses by Candidates*

I find limited evidence for selection as a mechanism. Instead, I argue that candidates face different strategic incentives in two-round elections and that these incentives lead candidates to adjust their strategies during the campaign. I provide two pieces of suggestive evidence.

One, I find that the concentration of voters for the top two candidates decreases between the first and second rounds (Table 10).[33] This decrease in concentration suggests that candidates use the period between rounds (three weeks) to adjust their strategies and rally voters who supported eliminated candidates, a strategy documented in Pons and Tricaud (2018) and Le Pennec (2020).

Two, using data on campaign finances,[34] I document that candidates in two-round elections finance their campaigns differently (Table 11). Campaign donations reflect the strategies candidates adopt to appeal to voters, providing one indication that candidates face different incentives. Candidates in two-round elections receive fewer donations, both on average and between the top two candidates. While the estimates on total donations and donations from individuals are noisy, there is a significant decrease in donations candidates receive from corporations, particularly among the top two candidates. To the extent that corporations represent a narrower swath of the electorate, this pattern suggests that candidates in two-round elections are appealing to broader groups in the electorate.

[31] While it is difficult to ideologically categorize political parties in Brazil in local elections, online Appendix B.7 shows that extremist, noncentrist parties are also not more likely to win the election.

[32] The political competition channel is explored in a theoretical framework in online Appendix C.

[33] This is not a causal effect; I estimate a regression that compares concentration in the first round with concentration in the second round using all elections holding two rounds.

[34] I use data from the 2004–2012 elections. Data on campaign finances are not available before the 2004 elections. I exclude the 2016 elections, as a new campaign finance law was passed in 2016 banning donations from corporations.

Table 9—Effect on Political Affiliation of Candidates

	Previous candidacy	Incumbency	Small party	PT party	Governor's party
Panel A. All candidates					
Two-round	0.539	−0.182	0.721	0.038	−0.084
	(0.259)	(0.155)	(0.334)	(0.100)	(0.136)
Single-round mean	1.652	0.752	2.535	0.636	0.584
Observations	263	263	296	296	263
Municipalities	89	89	92	92	89
Panel B. Winner only					
Two-round	−0.144	−0.201	−0.008	−0.022	−0.012
	(0.130)	(0.115)	(0.127)	(0.094)	(0.120)
Single-round mean	0.621	0.410	0.369	0.187	0.242
Observations	263	263	296	296	263
Municipalities	89	89	92	92	89

Notes: The table presents RD estimates on political affiliation of candidates (panel A) and winners (panel B). *Previous candidacy* is whether the candidate ran in a previous mayoral election. *Incumbency* is whether the candidate held the position of mayor in a previous term. *Small party* is any party that is not one of the top five parties by national membership. *PT party* is whether the candidate is from the *Partido dos Trabalhadores*. *Governor's party* is whether the candidate is from the party of the incumbent state governor. *Previous candidacy*, *Incumbency*, and *Governor's party* are unavailable for the 1996 elections. Dependent variables are either the number of candidates with that characteristic (panel A) or an indicator for the winner having that characteristic (panel B). Observations are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

Table 10—Effect on Concentration of Voters between Rounds

	Coefficient of variation	Fractionalization	Entropy	Standard deviation of first-place candidate
Second round	−0.012	−0.012	−0.010	−0.013
	(0.001)	(0.001)	(0.001)	(0.001)
First-round mean	0.049	0.049	0.037	0.103
Observations	432	432	432	432
Municipalities	77	77	77	77

Notes: The table presents estimates from a regression comparing the first-round results with the second-round results using the full sample of elections that held two rounds. Observations are at the election level. *First-round mean* refers to the dependent variable mean for the first round of all two-round municipalities holding a second round. The estimation method is a standard regression with *second round* as the regressor and election-year fixed effects. Standard errors are clustered at the municipality level.

V. Conclusion

In two-round elections, voters vote twice: once on a full set of candidates and a second time between the top two candidates. This rule ensures that the eventual winner obtains at least 50 percent of the vote. Theoretically, this can lead politicians to appeal to a broader group of voters and to provide public goods more broadly. This paper investigates this argument using data on electoral results and resource provision in public schools in Brazilian municipalities. Using a regression discontinuity design, I compare outcomes in elections holding a single round with outcomes

Table 11—Effect on Campaign Donations Received by Candidates

	Total	From individuals	From corporations
Panel A. Average donation amount per candidate			
Two-round	−0.225	−0.491	−0.673
	(0.286)	(0.310)	(0.404)
Single-round mean	12.844	10.742	11.782
Observations	154	154	154
Municipalities	71	71	71
Panel B. Total donation amounts among top two candidates			
Two-round	−0.074	−0.614	−1.023
	(0.335)	(0.492)	(0.541)
Single-round mean	14.053	11.717	12.846
Observations	154	154	154
Municipalities	71	71	71

Notes: The table presents RD estimates on the log donation levels, in Brazilian reals, received by candidates. Donors are identified as *Individual* and *Corporation* depending on whether the donor filed an individual or corporate identification number. Observations are at the election level. *Single-round mean* refers to the dependent-variable mean for single-round municipalities within the bandwidth. The estimation method is a local linear regression with election-year fixed effects and a 50,000-voter bandwidth. Population density is included as a control separately across the cutoff. Standard errors are clustered at the municipality level.

in elections holding two rounds. Candidates in two-round municipalities are represented by a geographically broader group of voters, and once in office, provide more resources and more evenly distribute resources to schools. These impacts result in improved education outcomes in two-round municipalities. I find evidence suggesting that candidates adjust their strategies rather than that different types of candidates enter the races.

If two-round elections lead to positive outcomes, why are they not more widely used? There are several potential counterarguments. First, it may be costly for voters to vote twice. In Brazil, turnout is lower in the second round compared to the first round. Uncovering the reasons for this will better quantify the costs of two-round elections. Second, there may be opposition to implementing two-round elections by the elite, which is consistent with evidence I find that those at lower parts of the distribution benefit. Identifying these barriers is crucial to understanding the process of political reform. Finally, two-round elections may result in better outcomes only when the electorate is fragmented. The average single-round election in Brazil has 4.6 candidates running. In this context, incentivizing candidates to include small groups in the coalition may lead to better outcomes. This may not translate to contexts where the electorate is composed of two large groups. Future empirical work on these issues can greatly advance our understanding of electoral systems.

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