Voting for Quality?

The Impact of School Performance Information on Electoral Outcomes *

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Abstract

We use the introduction of a school-level accountability system in Brazil to estimate the effects of public information on school quality. Reports revealing that schools are high in the test score distribution increase the incumbent mayor's vote share by about 2 percentage points. The revelation of low scores reduces incumbent mayor vote share by roughly the same margin, but also causes decreased class sizes and pupil-teacher ratios in subsequent years. These effects are larger in places with more educated voters.

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1 Introduction

Despite the wave of democratization that has swept low-income countries over the past decades, the quality of public services provided by most governments remains starkly low. Politicians often divert public resources for corruption, vote-buying, and clientelism at the expense of improvements in public goods and services (Huntington and Nelson, 1976; Kitschelt and Wilkinson, 2007; Olken and Pande, 2012). One explanation for these practices is that politicians respond to the preferences of poor voters who prefer direct transfers over public goods (Keefer and Khemani, 2005). An alternative explanation, however, points to the lack of information about politicians' actions as the cause for the failure of democracies to deliver public goods (Fearon, 1999; Besley, 2006; Pande, 2011).

A growing literature has examined whether increases in information affects the quality of public services.[1] Most of these studies, however, focus on the direct link between citizens and service providers, without taking into account how this accountability is mediated through elections (Khemani et al., 2016). A parallel body of research examines how voters respond to information about corrupt practices and politicians' characteristics (Ferraz and Finan, 2008; Banerjee et al., 2011; Chong et al., 2014; Bobonis et al., 2016; Larreguy et al., 2018). In contrast, little empirical work has studied how electorates react to information about the quality of public services. An exception is Cox et al. (2024), who show evidence that voters respond to information provided by researchers associated with a prestigious university. Whether voters react similarly to information provided by governments at scale is an open and important question.

This paper studies how voters react to public information about school quality, using the introduction of a nationwide school accountability program in Brazil. Starting in 2007, the federal government began to publicly release the school-level results of a new national standardized test program, creating for the first time a common benchmark for the comparison of learning outcomes across different schools and jurisdictions. The sudden and large-scale implementation of the program, as well as the conditions determining which schools would have their scores publicized, create a set of natural experiments allowing us to measure the causal effect of the school quality information on municipal election outcomes in 2008. Only schools with at least twenty students enrolled in fifth

[1] See World Bank (2004); Banerjee et al. (2010); Björkman and Svensson (2009); Björkman-Nyqvist et al. (2014); Keefer and Khemani (2014); Pradhan et al. (2014); Banerjee et al. (2018); Cilliers et al. (2021).

grade were eligible to have school-level fifth grade test scores publicly released.

We use a differences-in-differences strategy to compares votes at polling stations located in over 5,000 schools whose quality index was disclosed before the 2008 municipal election with polling stations at over 4,000 schools which were ineligible to have school quality information released. Mayors in Brazil are responsible for the provision of primary public education, so mayoral vote share is the focus of our analysis. We show that trends in incumbent party vote share, as well as school investments, were parallel in these two groups of schools in the years leading up to the revelation of the school quality information.

We find that the public release of school quality scores affected mayoral incumbent vote share in direct proportion to the level of school quality revealed. There is no significant average effect of information; the effects of positive and negative information more or less cancel one another out. Consistent with models that predict how voters update their beliefs based on the type of information, we find that incumbent vote shares are 2.14 percentage points higher for schools in the top 20% of our sample's distribution of school quality and 1.54 percentage points lower for schools in the bottom 20% of the distribution. We consider alternative measures of performance, and find consistent evidence of electoral rewards where schools performed well. These results are robust to a variety of alternative specifications and sample restrictions.

We also find that when the public release of information revealed poor performance, municipalities responded by increasing school investments. Municipal governments have discretion over the hiring of teachers in municipal public schools Akhtari et al. (2022). We show that pupil-teacher ratios and class sizes both fall sharply in schools revealed in the election year to be in the bottom quintile of the school quality index.

We corroborate our diff-in-diff results with a regression discontinuity design. Comparing 385 polling stations at schools near the 20-student class enrollment threshold which determines official eligibility for the program. We confirm that meeting the enrollment threshold has a large discontinuous effect on a schools information treatment status. As with the diff-in-diff results, the treatment effect depends on the content of the information revealed: where the news was good, incumbents got a boost at the polls.

We then examine the evidence for three potential mechanisms which could be driving our results: education, salience, and voters' priors. The accountability literature often points out two education-related challenges to engaging citizen participation through information provision. First, communities are often uninformed of what public services they are entitled to and how much control they have over these services (Pandey et al., 2011; Banerjee et al., 2018; Harding, 2015). Second, voters might not understand the information they receive (Banerjee et al., 2011). To whether voter education drives our results, we decompose the effect of information on good school performance on incumbent vote-share by citizens' level of schooling. Where more parents have completed high school, the reward for good performance is larger, suggesting a role for education in sharpening the accountability provided by information.

We next examine whether results are driven by the fact that education is especially salient because many voters vote in schools Ajzenman and Durante (2023). We expand our sample to include polling stations at non-school locations such as churches and community centers, associating them with their nearest school and testing the effect of the release of information about that nearest school on voting patterns. We find that the results are no different in non-school polling places: our results are not due to the special status of schools as frequent polling places, suggesting that information about other equally salient public services could potentially have similar electoral consequences.

We then examine the role of prior beliefs, using various proxies for voters' pre-treatment expectations of school performance. Following Arias et al. (2019) and Dunning et al. (2019), who highlight the role of priors for the interpretation of information treatments, we proxy for voters' priors about school quality in three different ways. One of these proxies leverages the previous round of the program's release of school quality information, based on data from 2005. Another relies on school infrastructure as a predictor of test scores. We do not find voting patterns to vary with these proxies for voters' priors.

Our paper's main contribution is to the literature on how information affects public service delivery. A large number of studies focus on citizen report cards and whether they can induce monitoring and demand for accountability (Banerjee et al., 2010; Björkman and Svensson, 2009; Björkman-Nyqvist et al., 2014; Cilliers et al., 2021). This literature's main focus is on what the 2004 World Development Report calls the "short route" of accountability, through which citizens hold providers directly re-

sponsible for service quality by exerting pressure through informal channels (World Bank, 2004). Evidence on the so-called "long route" of accountability — whether or not citizens use elections to hold politicians accountable for public services — is scarcer. The study most closely related to ours is that of Cox et al. (2024), in which researchers sent letters containing school performance information directly to voters' homes in a vast experiment in Chile. A similar program in Ohio found no effects of getting a bad School Report Card score on school board reelection rates (Kogan et al., 2016). Our paper builds on these studies by rigorously measuring the effect of information provided directly by a government at nationwide scale. It also relates to studies that test how voters respond to changes in access to public goods and services directly, as opposed to information about them (Harding, 2015; de Kadt and Lieberman, 2015; Leff Yaffe et al., 2025).

This paper also contributes to the literature on citizens' access to information about politicians and their actions. Among these studies, many focus on information about malfeasant behavior (Ferraz and Finan, 2008; Larreguy et al., 2018; Chong et al., 2014), while others focus on candidates' characteristics (Banerjee et al., 2011; Kendall et al., 2015). We know relatively little about voters' reaction to positive and negative information on the quality of public services. Even though there is evidence that parents respond to information about test scores by altering their choices (e.g. Hastings and Weinstein, 2008), it is not obvious that they will hold politicians accountable for the quality of public schools. There is evidence that voters exercise sophisticated judgment in predicting which policies will be personally advantageous for them when deciding how to vote (Méndez and Van Patten, 2025). However, in many political contexts, entrenched norms of clientelism may limit citizens' capacity or desire to demand better performance from governments (see Keefer and Khemani (2005), Keefer and Vlaicu (2008), Bobonis et al. (2022), and Bursztyn (2016)).

Finally, we build on the literature studying how voters react to the quality of public education. Biasi and Sandholtz (2025) find positive effects on incumbent vote share of a U.S. school reform which raised test scores. Meanwhile, Sandholtz (2023) finds that voters in Liberia punished the government for a reform which raised school quality on average. We build on these findings by showing that both voters and school investments respond directly to information, in a setting in which the responsibility for school administration is decentralized.[2]

[2]Firpo et al. (2017) and Toral (2016) also study how information about school quality affects electoral outcomes in Brazil, but at a more aggregated level and focusing on school-level targets. Boas et al. (2021) also examine electoral effects

2 Background

2.1 Public education in Brazil

In Brazil, municipalities are the main providers of elementary education, and they share with state governments the responsibility for managing middle schools. Brazil's constitution requires that 25 percent of all state and municipal taxes and transfers be spent on education and most resources used for this are transferred from state and federal governments[3]. Mayors can also exert effort in improving municipal public schools by applying for earmarked federal grants to construct new buildings, buy school supplies, etc. According to the 2004 School Census, which is the year of our pre-treatment election, 82% of public primary schools were run by local governments. This means that 73% of students enrolled in public schools in 2004 studied at a municipal school.

The accountability system of public education in Brazil was put in place in 1990 with the implementation of large-scale external evaluations. In the first edition, standardized tests in Portuguese, Math, and Science were administered to a sample of primary schools located in urban areas. In 1997, the national standardized test changed to be administered to the last grade of each education cycle: elementary school (5th grade), middle school (9th grade), and high school. At that time, a random sample of schools was selected to be evaluated at each period and all their students in the relevant grades were required to take these tests. This structure remained basically the same for the following ten years. Having only a sample of schools whose students take the standardized tests made it difficult to compare the performance of municipalities over time and make politicians accountable for educational outcomes.

2.2 School quality information: IDEB

In 2005 the system changed with the introduction of a standardized test named *Prova Brasil* aimed at evaluating all public schools located in urban areas every two years. These scores were used to create a school-quality index called *Índice de Desenvolvimento da Educação Básica* or IDEB. The index was computed at the school and municipality levels, allowing citizens to compare the educational performance of different local governments. The IDEB index was composed of tests scores and passing

of IDEB scores but focus on the effect of municipalities meeting arbitrary performance thresholds. They find that meeting these thresholds has negative effects on incumbent mayors' reelection.

[3]See Bruns et al. (2012) for more details on Brazil's education system.

rates and calculated combining the average performance on standardized tests in Portuguese and Math with average passing rates for each education cycle (i.e. 1st to 5th grade, 6th to 9th grade, and 1st to 3rd year of high-school). The IDEB index was calculated according to the following equation:

$$\text{IDEB}_{ij} = \text{N}_{ij} \cdot \text{P}_{ij} \tag{1}$$

where N_{ij} is the average proficiency score in Mathematics and Portuguese standardized to a value between 0 and 10 for school i in segment j and P_{ij} is the percentage of children that advanced from one grade to the next averaged for segment j.[4].

Two years after students took these tests, the IDEB quality index was released to the public for the first time. But not all public schools received an IDEB score in the 2005 and 2007 editions of the evaluation. There were specific eligibility criteria that a school had to fulfill in order to have an IDEB score made public: i) have at least 20 students enrolled in the relevant grade; ii) be located in urban area; iii) have more than ten students taking the standardized tests in Portuguese and Mathematics for the relevant grade.

The release of these school performance indicators is a major media event in Brazil. When the IDEB scores are released, the Ministry of Education holds a press conference and the topic of public school quality receives great attention from the Brazilian media. The media interprets this information and often ranks schools and municipalities according to their performance to investigate stories of success and failure.[5]. Media exposure therefore can make citizens aware of the quality of nearby schools even if they themselves do not have children who go to these schools. Some schools even display their IDEB grade in a place of large visibility, although this is not a general rule across the country.

[4]The first segment is composed of grades 1st to 5th while the second segment is composed of grades 6th to 9th. Test scores are only available for grade 5th and 9th

[5]When the IDEB 2015 results were released in 2016, the state of Ceará had 70 schools between the top 100 performers in the country and the policies adopted to improve the quality of these schools were widely discussed by the general media (e.g, https://g1.globo.com/ce/ceara/noticia/escolas-do-ceara-tem-o-melhor-indice-do-ideb-no-norte-nordeste.ghtml).

2.3 Municipal elections in Brazil

Municipalities in Brazil are governed by a mayor elected every four years through a first-past-the post system.[6] Once elected, mayors can serve up to two consecutive terms, after which they must leave office for at least one term if they desire to run again. Mayors are important political figures in Brazil. Each year, municipalities receive millions of dollars from federal and state governments to provide basic public services such as primary education, health care, and sanitation and the mayor sets the agenda for how resources are spent and allocated.

The natural experiment that we exploit made school quality information publicly available for the first time in 2007, then again in 2008, right before the 2008 municipal election. Our sample comprises municipalities in which the mayor was elected for a first term in 2004 and ran for reelection in 2008. The first IDEB school quality index was calculated using 2005 data, although it was only released to the public in 2007. As such, it conveys little information about the effect of the policies of mayors in our sample, who took office in 2005. In contrast, the IDEB score using 2007 data was processed much more quickly and released in July 2008, only three months before the municipal election. As such, we use the release of the 2007 IDEB score (in 2008) as our main treatment variable. In some specifications, we use the 2005 IDEB score (released in 2007) as a proxy for voters' priors on school quality.

The official political campaigns in the elections we study in this paper began on July 5 of the election year. Thus, candidates had approximately two and half months to publicize their platforms. In 2008, the beginning of the official campaign period coincided with the release of the IDEB 2007 results, which was also in July. For a period of 45 days prior to the elections, candidates have space for free publicity in the television and radio.[7] How much time each candidate has is based on how many seats the parties in her coalition have in the House of Representatives. A great deal of the information voters receive during the campaign comes from the media, but candidates also use billboards, distribute fliers and visit local communities to speak directly to voters. With the release of IDEB 2007 scores so close to the elections, this information was not only made public by the media, but also by incumbents and their opponents in their campaign ads.

[6] For municipalities with 200,000 or more registered voters, mayors must be elected with at least 50 percent of the votes or a second round runoff is held.

[7] Da Silveira and De Mello (2011) find a large effect of this type of TV advertising on electoral outcomes in Brazil.

3 Data and sample

We put together a dataset that links electoral data from polling stations to the administrative data on municipal schools where polling stations are located.

School quality data (INEP): The *Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira* (National Institute for Educational Studies and Research Anísio Teixeira) collects school data across all of Brazil. This is the source of our treatment variable, a dummy for whether a school had an IDEB score released in 2008. We also exploit the variation in the IDEB school quality scores themselves. As part of the surveys accompanying the standardized exam *Prova Brasil* — a component of IDEB scores — data is collected on the highest level of education attained by students' parents. We use this data to test for heterogeneous effects by voters' schooling.[8]

Electoral data (TSE): The *Tribunal Superior Eleitoral* (Superior Electoral Court) is responsible for all electoral data in Brazil. From TSE we have data on the characteristics of politicians running for office, electoral outcomes and characteristics of voters aggregated at the polling station level.[9]

School census (Censo Escolar): The *Censo Escolar* (School Census) is an annual survey of Brazil's schools, including detailed data on teachers, classes, students, infrastructure, and equipment. Participation in this survey is mandatory so coverage is nearly universal.

Merging procedure: Most polling stations are located at schools; we link the school data and electoral data using a fuzzy matching procedure on school names.[10] We first identify which polling stations are located at schools by searching for prefixes and suffixes frequently used in the names of education institutions. For example, *EM* is short for *Escola Municipal*, indicating municipal schools. This allows us to match, for instance, the school with name *EEEFM PE Ezequiel Ramin* with the polling station *EEEFM Padre Ezequiel Ramim* and the school *EMEFM Aldemir Lima Cantanhede* with the polling station *Esc. Aldemir de Lima Cantanhede*. We are able to match 75% of polling stations to the administrative records of schools. We present a more detailed description of the matching procedure in Appendix B.1.

[8] These data are publicly available at https://dadosabertos.mec.gov.br/.
[9] These data are publicly available at http://www.tse.jus.br/.
[10] In Section 7 we expand the sample to include polling stations not located at schools.

After the matching procedure, we restrict our sample to the polling station-school pair for which the school fits the following criteria. First, it must be a municipal school and have an active status in 2004 and 2008, which are, respectively, the year of our pre and post treatment elections. Second, it must be an elementary school with students enrolled in grades 1-5. This restriction derives from our focus on IDEB scores for 5th grade since it is the tested grade with a larger share of enrollment in schools that are fully managed by the municipal government.

In the process of matching schools to polling stations and restricting the sample we lose some municipalities where polling stations are only located at private schools, or where polling station names change between 2004 and 2008 making us unable to match them to electoral results. In Appendix B.2 we show that the cities, polling stations, and schools we lose in the process of linking electoral data to the administrative data from schools are not very different from those included in the analysis.

Table 1 shows characteristics of schools, voters, and incumbents.[11] School infrastructure tends to be quite poor. The lack of science labs in the schools in our sample may be justified by focusing our analysis in elementary schools, but the average proportion of schools with a teachers' room, a computer lab and a library is also small. We also show that the average school in our sample has about 240 students and 15 teachers. Dropout rates are on average 6 percentage points. The average passing rates suggests that a relevant number of students are retained before fifth grade. From the variability of the characteristics of schools, we infer that schools are more heterogeneous in terms of infrastructure and size than in outcomes. Regarding voters' characteristics, the table shows low levels of schooling. Incumbent mayors are overwhelmingly male, and more than half of them have a college degree. The average incumbent won election to their first term with 51% of the vote (only a plurality is required to win in most municipalities).

4 Empirical Strategy: Difference-in-differences

The implementation of a nationwide school-level accountability system in Brazil offers a natural experiment to estimate the effect of information about public service quality on voter behavior and subsequent public service investments. "Treatment" is defined at the school level, and consists of a school having its IDEB score publicly disclosed in 2007. In our first set of analyses we employ a

[11] Voters' characteristics are from the 2008 municipal elections

differences-in-differences approach, measuring how the difference in outcomes across treated and control schools changed after the information was released. Section 6 takes a regression discontinuity approach, comparing schools which were barely eligible vs. ineligible to have their scores publicly released.

The average effect of school quality information can be identified using the following equation:

$$\text{votes}_{ijt} = \alpha + \beta\,\text{Info}_{ijt} + \zeta_{jt} + c_i + \epsilon_{ijt} \tag{2}$$

in which votes_{ijt} is the vote share received by the mayor at the polling station i located in area type j at election t; and Info_{ijt} is a dummy that indicates whether the voters at polling station i in area type j were informed about the quality of the public school nearby prior to election t. Subscript j indicates whether the polling station is located in an urban or rural area. We include polling station fixed-effects and time-by-area fixed effects to control for unobservable characteristics at the polling-station level correlated with vote-shares, and differential trends between rural and urban areas to account for unobserved trends across these areas.

While Equation 2 estimates the average effect of the disclosure of information, the reaction of voters may depend on the information's content. Following Ferraz and Finan (2008), we interact the treatment variable Info_{ijt} with a measure of school quality and estimate the following equation:

$$\text{votes}_{ijt} = \beta\,\text{Info}_{ijt} + \theta\,\text{Quality}_{ij} \cdot \text{Info}_{ijt} + \zeta_{jt} + c_i + \nu_{ijt} \tag{3}$$

where $Quality_{ij}$ is a measure of school quality in school i in area type j. We first use the IDEB 2007 score as a measure of quality. We then consider other benchmarks of quality following Bruns et al. (2011) who highlight that there is no clear evidence of which information about school quality is used by parents, raw test scores or measures of value-added. Since we are unable to measure value added with our data, we also consider performance relative to reference points. We first consider the median IDEB 2007 score within a state as one possible standard for comparison. We also consider the distribution of scores within our sample as another reference point. Then, we construct indicators of good/bad quality as being, respectively, above/below the state median or

among the top/bottom 20% best schools in our sample.[12]

The identification of β in Equations 2 and 3 relies on two assumptions. First, informed and non-informed schools would have experienced the same average changes in outcomes absent the treatment (parallel trends). The second assumption is that the composition of the groups does not change in a way correlated with treatment over time.

To show the plausibility of the parallel trends assumption, we inspect the pre-treatment trajectories of the treatment and control groups for outcome variables where such an analysis is possible. Brazilian electoral law allows an incumbent mayor to run for re-election only once consecutively, so when the outcome variable is vote share for the mayor in office in 2008, there is no trend to compare. Instead we examine pre-trends (and treatment effects) for a related outcome variable: the vote share of the *party* of the 2008 incumbent mayor. Parties face no term limits and routinely field different candidates across cycles, so their electoral performance can be tracked repeatedly before and after the information shock.

Using election outcomes from 2000, 2004, and 2008, we set up an event-study regression of the form

$$\text{vote}_{ijt} = \sum_{k \in \{2000,2004,2008\}} \Big[\beta_k \left(\text{Info}_{ij} \times \mathbb{1}\{t = k\}\right) + \theta_k \left(\text{Info}_{ij} \times \text{Quality}_{ij} \times \mathbb{1}\{t = k\}\right)\Big] + \zeta_{jt} + c_i + \nu_{ijt}, \tag{4}$$

where 2004 serves as the omitted (baseline) year. As before, c_i are school fixed effects, ζ_{jt} control for area type trends, and standard errors are clustered at the school–station level. The coefficients β_k trace the average information treatment effect over time, while θ_k measure how that effect varies with a one–standard-deviation increase in the 2007 IDEB score. Finally, we conduct a similar analysis interacting Info_{ij} with dummies for whether the Quality_{ij} score is in the top or bottom 20%:

[12] Because we do not observe 2007 IDEB scores for the control group, we impute zeros for these observations. By doing this we are implicitly assuming that electoral outcomes do not have different counterfactual trends depending on quality.

$$\text{vote}_{ijt} = \sum_{k \in \{2000,2004,2008\}} \Big[\beta_k \left(\text{Info}_{ij} \times \mathbb{1}\{t = k\} \right) + \theta_k^1 \left(\text{Info}_{ij} \times \text{QualityTop20}_{ij} \times \mathbb{1}\{t = k\} \right) + \theta_k^2 \left(\text{Info}_{ij} \times \text{QualityBottom20}_{ij} \times \mathbb{1}\{t = k\} \right) \Big] + \zeta_{jt} + c_i + \nu_{ijt}. \quad (5)$$

The coefficients on pre-treatment years in these event-study specifications serve as tests of parallel trends for our main outcome variables — incumbent party vote share and school investments. For the outcome variable of vote share for the incumbent mayoral candidate herself in the 2008 election, we are unable to explicitly test for pre-trends as mayors are limited by law to a maximum of two consecutive terms. We therefore test for pre-trends in other related variables, finding no evidence of differential trends prior to the release of school quality information in 2008.

We also compare the mean characteristics of schools, voters and electoral outcomes in treatment and control groups. We show the result of this comparison in Table A1. The dimension in which these groups vary the most is school size. Although our identification strategy does not require that treatment and control groups be similar in levels, Section A.2 tests the robust of our results to sample limitations which increase the similarity of schools in treatment and control groups.

5 Results: Difference-in-differences

5.1 Effects on incumbent party vote share

Figure 1 shows that the revelation of school quality information affect incumbent parties' vote share in direct proportion to the level of school quality revealed by the information. Using data from two elections prior to the treatment, the figure plots point estimates and 95 % confidence intervals on the β_k and θ_k coefficients from Equation 4 (Panel A) and Equation 5 (Panel B). These denote the year-by-year difference between treated and control schools, and this difference interacted with $Quality_{ij}$ (Panel A) or Top/Bottom Quality quintiles (Panel B), respectively. Because the outcome variable is the vote share of the party in power 2008, we necessarily restrict the data to municipalities in which the party that won the 2004 election also contested the mayoral races in both 2000 and 2008. This yields a balanced panel of 800 municipalities and 2,723 polling-station observations that appear in

all three elections.

The figures show evidence in support of the parallel trends assumption. In both specifications, neither the Info_{ij} dummy nor its interactions with the IDEB quality index are on statistically different trends in the 2000-2004 pre-treatment period. In Panel (b), the 2000 coefficients for the $Info \times Top$ and $Info \times Bottom$ interactions are statistically indistinguishable from one another ($F = 1.21; p = 0.27$). This suggests that any differences in outcomes between groups after the revelation of the information is caused by the information itself and not by any pre-existing differences in trends.

Results from both specifications show that the effect of the information in the post-treatment 2008 election is a positive function of the school quality revealed. The overall average effect of information remains statistically indistinguishable from zero (though the point estimate is around 2 percentage points). In Panel (a), the coefficient on this main effect can be interpreted as the effect of the revelation of a Quality index of zero (i.e. the mean); in Panel (b), as the effect of the revelation of a Quality index in the middle three quintiles. However, the interaction with the continuous standardized quality index in Panel (a) is positive and statistically significant: a one-standard deviation increase in school quality raises the incumbent party's vote share by just over 1 percentage point. Another way to view this is by interacting the information dummy with indicators for whether the schools was in the top or bottom quintile, as in Panel (b). Here, the coefficient on $Info \times Top$ in the post-treatment election is about 2 percentage points and marginally significant ($p = 0.06$). In the context of Equation 5, in which the β_ks give the effect of information on the middle three quintiles of school quality, this implies that the effect of the revelation that a school was in the top quintile raised vote share by 2 percentage points over and above the effect of the information on the middle quintiles. The coefficient on $Info \times Bottom$ in 2008, meanwhile, is -1.23 but insignificant. The difference between θ_1 and θ_2 is significant ($F = 4.58; p = 0.03$). Information's positive effect on incumbent party vote share is increasing the how good the schools are revealed to be.

5.2 Effects on school investments

We next turn our attention to whether such information also leads to changes in school investments. We focus on two measures of school investments: Teacher-pupil ratios (TPR) and class size.[13] These

[13] Although in practice these two measures are closely correlated, they do not necessarily represent precisely the same variation; for example, schools could increase TPR without decreasing class size if they also provided teachers more prep

closely related measures represent one of the primary margins by which mayors can increase schooling investment: hiring more teachers Akhtari et al. (2022). Because these measures come from the School Census, they are measured every year, providing us with more pre-treatment periods in which to measure trends and more post-treatment periods for tracing the temporal dynamics of treatment effects.

Figures 2a and 2b show results from Equation 5 with Teacher-Pupil Ratio and Class Size as outcomes. In each case, treatment and control schools were on statistically identical trends prior to treatment, as were schools which would end up being revealed to be in the top and bottom quintile of the Quality index. After the revelation of school quality information in 2008, TPR in bottom-quintile schools jumped sharply, and class sizes fell. These effects increased throughout the mayoral term; by 2011, class sizes in bottom-quintile schools were one student smaller (4% of the 2007 control group mean of 25) relative to the common pre-treatment trend.

We also test for effects on other dimensions of school investments: access to water and access to sanitation. The results (as well as the detailed coefficients from Figure 2 on TPR and class size) can be found in Table 2. We do not find strong effects on these more infrastructure-based measures of school investments, consistent with teaching hiring being a more immediately responsive policy lever.

This pattern of results strongly suggests that the public dissemination of school quality information provoked compensating investments in places where performance was revealed to be substandard, and that electoral pressure is at least one channel for these changes. A good school report card was shown to help win votes; politicians (and parties) receiving bad report cards may plausibly have hoped to turn their fortunes around by the next election.

5.3 Incumbent politician vote share

We next turn our attention to vote share not for incumbent parties, but for incumbent politicians themselves. This analysis complements the results from Figure 1 on incumbent party vote share. One reason is that this is an outcome of great interest: incumbent mayors are the politicians to whom municipal school policy is most directly and visibly attributable, so understanding the role of school

time or shortened their workday.

quality information in their accountability is vital. Another is that the sample is larger because many more municipalities had an incumbent on the ballot in 2008 than met the stricter condition of having an incumbent party in 2008 which had also contested the 2004 and 2000 elections. Figure 1's analysis was based on a sample of 2,723 polling stations, while the analysis of incumbent vote share includes 9,682 polling stations.

However, because the law permits mayors to serve at most two consecutive terms in office, it is impossible to test pre-trends for this outcome across multiple electoral cycles. The absence of significant pre-trends in incumbent party vote share visible in Figure 1 may be somewhat reassuring, although the sample is somewhat different. Therefore, to bolster confidence in the assumption that the broader groups of treated and untreated municipalities were on similar trajectories prior to treatment, we test for pre-trends in other related electoral variables: the proportion of votes for the Workers' Party (PT), the dominant party of the period, in municipal and presidential elections from 1998 to 2006; voter turnout in the same set of elections; and the additional school outcomes 5th passing rates, dropout rates, and enrollment. The results are in Figure 3. None of these variables evolved differently prior to treatment, and most are statistically indistinguishable in levels (the exception is enrollment and class size; a mechanical difference due to the fact that treatment with the information revelation is determined by enrollments). In the analysis that follows, therefore, we present results from regressions according to Equation 2 and 3, stressing that the causal interpretation of the coefficients depends on the parallel trends assumption.

Table 3 shows the results. Column (1) compares voting outcomes in polling stations that received and did not receive information about school quality according to Equation 2, showing no statistically significant difference on average between incumbents' vote share of localities with and without information about school quality. This average effect does not take into account that some voters received positive information about the local school quality while others received negative information. In column (2) we present results from estimating equation 3 in which we interact our treatment variable with the IDEB education quality index released a few months before the election ("Quality index 2007"). The results show that an increase in one standard deviation on the school quality index is associated with an additional 1.30 percentage point increase on the vote-share of the incumbent mayor. Moving from percentile 25 of performance to percentile 75 in our sample

implies a 1.5 standard deviation increase in performance, which would translate into an additional 2.1 percentage points in the incumbent's vote-share.

We next examine whether extreme results and relative performance matter for voters, by interacting the treatment variables with indicators for whether the school was revealed to be in the top and bottom 20 percent. In column 3 of Table 3 we show that being among the top 20% performers in the sample raises incumbent vote share by 2.14 percentage point ($\approx$ 5%) more than being in the middle three quintiles, while being among the worst performers reduces the effect by 1.54 percentage points. These results are consonant with those from the analysis of incumbent party vote share shown in Figure 1; the coefficients on the interactions for extreme values of the quality index are slightly larger in absolute value, which may suggest that electoral accountability is greater for individual politicians who enacted school policies than for successor candidates running on the same party's ticket.

Voters may overcome political agency problems by making yardstick comparisons between local communities (Besley and Case, 1995). In this context, the school quality index might not be easy to interpret on its own, but rather provide a comparable benchmark against which voters can compare the schools of their local community with those in other nearby places. In column (4) we test whether performance relative to the other schools in the same state matters to voters, interacting treatment with dummies for being in the top or bottom quintile within the state rather than the overall distribution. We find that being among the top 20% of performers in the state is associated with a significant 1.51 percentage point increase in the vote share of the incumbent, while being revealed to be in the bottom 20% of the state seems to hurt incumbents no more than being average. This could be because voters do in fact interpret the school quality index in absolute terms, or because the relevant comparison group to most voters is the country rather than the state.[14]

[14]The average Quality Index 2007 for the schools that are overall bottom performers is 2.61, while the average Quality Index 2007 for schools that are among the bottom performers in their state is 3.07. This means that some of them perform quite better in absolute terms than the schools that are among the bottom performers of the sample as a whole. This could be why we do not find a significant effect of bad performance relative to the rest of the state on the vote-share of the incumbent.

6 Regression Discontinuity evidence

In this section, we provide evidence from a regression discontinuity analysis which supports the diff-in-diff findings of the foregoing section. As mentioned in section 4, the publication of IDEB in 2008 was not universal: a school was eligible only if it (i) enrolled at least 20 pupils in the relevant grade, (ii) was located in an urban area, and (iii) had at least ten students sitting the nationwide Portuguese and Mathematics examinations. These administrative rules generate sharp differences in information availability across otherwise similar schools. Here we exploit criterion (i): the discontinuity at the 20–student threshold in Grade 5.[15]

6.1 Empirical strategy: Regression discontinuity

Let x_{ij}^{2006} denote the number of 5$^{\text{th}}$–grade enrollments in 2006 for school (and associated polling station) i in area type j. We define the deterministic treatment predictor

$$Z_{ij}^{2007} \;=\; \mathbb{1}\{x_{ij}^{2006} \geq 20\},$$

which takes the value one for all schools that meet the IDEB–reporting threshold and zero otherwise.

In practice, although the IDEB regulation stated that only schools with $x_{ij}^{2006} \geq 20$ should have had their quality index disclosed, the rule was not necessarily perfectly enforced. The 20-student enrollment rule for Grade 5 eligibility does not perfectly exclude schools with fewer than 20 registered students in that grade, because some small schools operate multi-grade classrooms, where students from different grades are taught together and reported under combined enrollment figures. As a result, some schools with fewer than 20 enrolled fifth-graders nevertheless have enrollments greater than 20 for the class in which the fifth-graders sit, making them eligible to have their IDEB scores released. Because our running variable is 5th grade enrollments, this creates non-compliance on the left side of the cutoff. On the right side of the cutoff — urban schools with at least 20 Grade 5 enrollments recorded in 2006 — a few eligible schools did not have their scores released, but compliance is close to 100%, since these schools satisfied all administrative criteria for inclusion in the

[15] Municipalities have some discretion in whether to classify themselves as urban or rural, so criterion (ii) does not lend itself to a useful discontinuity for our purposes. And given that criterion (i) requires at least 20 students enrolled and nearly all enrolled students in the relevant grades sit the nationwide examinations, criterion (iii) is rarely binding.

IDEB release. We report ITT results, using the reduced-form treatment eligibility indicator Z_{ij}.

We choose the sample window h using the data–driven bandwidth selectors for local–polynomial RD point estimation and robust bias–corrected inference developed by Calonico et al. (2014), Calonico et al. (2018), Calonico et al. (2019), and Calonico et al. (2020).

We first verify the first stage by estimating

$$D_{ij}^{\text{info}} = \phi\, Z_{ij}^{2007} + f\left(x_{ij}^{2006}\right) + u_{ij}$$

within the Calonico–Cattaneo bandwidth h. A coefficient $\hat{\phi} \approx 1$ would imply near–perfect compliance, whereas $\hat{\phi} < 1$ quantifies the enforcement gap.

Figure 4 plots the share of urban public schools that report an IDEB score in 2007 against their 2006 fifth-grade enrollment. To the left of the 20 enrollments cutoff, the probability of reporting a score rises gradually with enrollment; about half of schools with 18–19 pupils disclose the IDEB. Immediately to the right of the threshold, however, the fitted red polynomial shows that publication rates jump to virtually full coverage, approaching 100 percent for schools with 20 or more students. This stark, discontinuous increase — roughly a 40-percentage-point jump — confirms that the 20-student rule generated an imperfect but strong link between enrollment and the likelihood of having an IDEB score.

Table 4 shows the effect of crossing the enrollment threshold on treatment under various specifications. Column (1) reports the discontinuity in the probability that a school in the local bandwidth actually had its IDEB score disclosed in 2007. Consistent with the administrative rule, the indicator for crossing the 20–pupil threshold ("≥ 20") raises the likelihood of publication by roughly 39 percentage points (79%) and is highly significant at the 1% level. Neither the level nor the slope of the running variable exhibits a statistically detectable kink at the cutoff. Column 2 shows that this is not different by the value of the Quality index. While Columns 1 and 2 look at schools which meet the condition of being located in an urban area, Columns 3 and 4 perform a placebo test looking at rural schools, where we should expect to see no IDEB scores because of the condition that IDEB scores only be reported in 2008 for urban schools. As expected, the level of IDEB score provision in this sample is essentially zero, as is the discontinuity at the enrollment threshold.

As before, we are interested in vote share for the incumbent mayor. We also consider the change in the incumbent mayor's vote share at polling station i region j between the 2004 and 2008 mayoral elections, defined as:

$$\Delta\%\text{votes}_{ijt} \;=\; \%\text{votes}_{ij}^{2008} - \%\text{votes}_{ij}^{2004}.$$

Because we restrict the sample to municipalities where the incumbent elected in 2004 ran for re-election in 2008, $\Delta\%\text{votes}_{ijt}$ captures the within-station swing in support for the same candidate, purging time-invariant local preferences. We also measure effects on the 2008 level of incumbent vote share, $\%\text{votes}_{ij}^{2008}$. As a placebo we re-estimate the specification using the level of 2004 vote share for the 2008 incumbent, $\%\text{votes}_{ij}^{2004}$; any discontinuity at the cutoff before the information shock would cast doubt on identification.

We interact the reduced-form treatment eligibility indicator Z_{ij} with the quality index and estimate the following local–polynomial reduced form within the Calonico–Cattaneo bandwidth h:

$$Y_{ijt} = \alpha + \gamma\, Z_{ij} + \delta\,(Z_{ij}\cdot x_{ij}) + \theta\,(Z_{ij}\cdot \text{Quality}_{ij}) + \rho\,(Z_{ij}\cdot \text{Quality}_{ij}\cdot x_{ij}) + f\big(x_{ij}\big) + \varepsilon_{ijt}. \tag{6}$$

Here Quality_{ij} is the standardized 2007 IDEB score (mean 0, s.d. 1) observed only when $Z_{ij} = 1$; we set $Z_{ij} = 0$ for schools below the threshold so that the interaction is defined. Function $f(\cdot)$ is a local linear polynomial in enrollments $x_{is} - 20$, estimated on either side of the cutoff, and all standard errors are clustered at the school/polling–station level.

The mean–squared–error–optimal half–bandwidths obtained from these procedures, subject to a minimum span of four enrollment units, imply a symmetric interval of $[16, 23]$ fifth–grade students around the 20–pupil cutoff. Accordingly, the estimation sample comprises all urban public schools with 2006 fifth–grade enrollments in that range, yielding $N = 385$ observations.

The causal interpretation of the RD rests on the assumption that the discontinuity in the treatment variable is not the result of manipulation, and that other discontinuities are not present at the same point. Figure A1 plots a histogram of fifth grade enrollments, showing that the density is smooth at the treatment threshold. Figure A2 shows balance of other school characteristics, demonstrating there is no discontinuity in school infrastructure or pedagogical equipment at the enrollment threshold. Taken together, these diagnostics indicate that observable school attributes are balanced

around the threshold, strongly suggesting that the discontinuity isolates exogenous variation in score disclosure. The regression discontinuity design therefore provides credible causal estimates which can complement the results from our diff-in-diff strategy presented in Section 5.

6.2 Results: Regression discontinuity

In Table 5, we compare voting outcomes in polling stations that were barely eligible to receive information about school quality to those barely not eligible to receive it. Columns (1) and (2) show a placebo test, looking at electoral outcomes in the (pre-treatment) 2004 mayoral race, with vote share of the (2008) incumbent as the dependent variable. Column (1) focuses solely on the discontinuity, while Column (2) interacts the discontinuity dummy with a continuous measure of the IDEB Quality index. In both columns, the point estimate on the discontinuity is negative, but imprecisely estimated. The coefficient on the interaction of treatment with the Quality Index 2007 is also insignificant, implying that the treatment is not correlated with electoral support in the pre-treatment period.

Columns (3) and (4) repeat the exercise with the dependent variable as incumbent vote share in the 2008 election. Column (4) interacts the information disclosure dummy with the Quality Index, allowing the treatment effect to vary linearly with the published score. Again, neither specification shows a significant effect of information on average, consistent with the results from Section 5. However, the effect again varies with the content of the Quality index. Schools more likely to have Quality Index made public, with one Standard Deviation higher index, experience a positive baseline shift of 5.7 p.p. (significant at the 10% level). Finally, Columns (5) and (6) use the 2008-2004 change in vote share (for the 2008 incumbent) as the dependent variable. The results are consistent with those of Columns (3) and (4), with the treatment effect rising 7.2 p.p. with a one Standard Deviation higher index (significant at the 10% level).

These results go hand-in-hand with the diff-in-diff results from Section 5. Although they are based on a much smaller sample, they use a wholly different causal identification strategy to produce estimates which are broadly consistent with those of the diff-in-diff outlines in Equation 3.

7 Mechanisms

There are many channels through which the revelation of information could affect electoral accountability for school quality. In this section we consider and furnish evidence on four potential mechanisms: voter education, voter priors, and polling station salience.

7.1 Do educated voters respond more to information?

The results presented in Section 5.1 show that voters reward and punish mayors according to the information released by the IDEB school quality index. In this section we ask whether more educated voters react differently to the release of information. First, it may be the case that more educated voters are more likely to understand the information that is being given to them and use it to update their beliefs about the politician in office (Besley and Burgess, 2002; Banerjee et al., 2011). Second, more educated voters may value more the delivery of public education (Bursztyn, 2016). Thus, they may be more likely to react to use the information to cast a vote for the incumbent mayor running for reelection.

Information about parents' education is typically collected through surveys that are applied to students together with standardized tests. Therefore, by definition we do not have this information for schools in our control group in 2007. We get around this by restricting our control group to schools which did not take part in the IDEB in 2007, but which did participate in *Prova Brasil* in 2009. We then use the 2009 data on parents' education as a proxy for the parental education in 2007, assuming that students' family background remains roughly constant over this timespan

We group schools according to the proportion of parents that have completed high school (we split them in terciles). In Table 6 we report the estimates from allowing the effect of information and the index of school quality vary according to terciles of parents' education. We normalize the educational score to have mean of zero and standard deviation of one in each tercile of citizens' schooling. In column (1) we use the schooling of mothers while in column (2) we use the schooling of fathers. In columns (1) and (2) of Table 6 we find that more educated voters are the ones to hold the incumbent mayor running for reelection accountable for the quality of the public school near them. The release of information about school quality does not seem to change the voting patterns in schools where only a small percentage of parents completed high school.

7.2 Salience: many polling stations are located at schools

Our main dataset combines (i) the TSE register of polling stations used in the 2004–2008 municipal election cycle with (ii) the annual school census, with municipal primary schools and their 2005-2007 *IDEB* scores. In Brazil, voters are allocated to the polling place nearest to their registered address, while children are generally enrolled in the nearest public school. Thus, in our baseline specifications in Figure 1 and Table 3 we restrict the analysis to polling stations that are physically located inside a public school.

However, this restriction could be consequential if people vote differently in schools than they do in other polling stations — or if school quality information has a different effect on voters voting in schools. Ajzenman and Durante (2023) show in Argentina that the quality of the school building that hosts the ballot box can influence electoral behavior by making deficiencies in public education visibly salient to voters at the moment of choice. In many Brazilian municipalities, ballot boxes are hosted at community centers, churches, or sports clubs. To test whether the electoral effect of information operates through the salience of voters' direct exposure to school facilities, we include non-school polling stations in the analysis, assigning each to the information of its nearest public school. The data processing necessary for this expansion of the sample is outlined in Section B.3; we include polling stations which we can classify with a high degree of confidence as located within 1 km of a school. We then test whether electoral effects on these voters casting ballots in non-schools are similar to those on voters casting ballots in schools.

By re–estimating the main regression on an expanded sample that includes polling stations located in non–school buildings, we can compare whether treatment effects vary across two groups: (i) voters who cast their ballots inside a public school and thus observe its facilities, and (ii) voters who cast their ballots elsewhere and are therefore not directly exposed to school infrastructure on Election Day. If the IDEB–related swing in incumbent vote share is similar in magnitude for both groups, the results would point away from an "on-site salience" channel and toward voters rewarding (or punishing) the incumbent for the quality of their children's proximate school. Conversely, a significant attenuation of the effect at non-school polling stations would provide evidence that direct visual exposure to school facilities amplifies the political impact of educational performance information.

Table 7 presents the results. In the combined sample of 14,084 observations (9,328 informed, 4,756 controls) the main-effect coefficient on *Informed* remains small and statistically insignificant, indicating that receiving an IDEB score on the average does not, by itself, shift votes. By contrast, the interaction with the quality index is positive and statistically significant, with a magnitude close to the specification with the main sample only. A one-standard-deviation improvement in the quality index raises the incumbent's vote share between 2004 and 2008 by about 1.6 p.p. in stations linked to schools with the IDEB score, relative to otherwise identical uninformed counterparts whose 2004 mean was 51.4%. School and station fixed effects absorb all time-invariant location characteristics, while the rural-specific trend controls for differential trajectories in those types of schools.

Column (2) adds full interactions with an indicator Main Sample$_{ij}$ (equal to one when the ballot box is *inside* a school). Neither the differential main effect (-0.12) nor the differential quality gradient (-0.82) is statistically distinguishable from zero, implying that the IDEB-driven swing is virtually identical whether voters cast their ballot within a school or in an external venue. (The point estimates are negative, implying that if anything the effect of information is higher in non-school polling stations.) Column (3) confirms this by estimating the model on the *non-school* subsample alone: the quality interaction remains 2.1 p.p. ($p < 0.01$).

Taken together, these results suggest that the mechanism is not the immediate salience of visible infrastructure at the polling place, but rather voters rewarding or punishing the incumbent for the educational performance of the local public school. This suggests that information about other public services (including those administered in places besides schools/polling stations) could have similar electoral effects.

7.3 Heterogeneity by prior beliefs

Finally we assess whether the effects of information disclosure depend on the priors about school quality that citizens hold. We interact Quality$_{ij}$ with different proxies for voters' priors about the quality of municipal schools in their local community using the following empirical specification:

$$\begin{aligned}\%\text{votes}_{ijt} =& \beta_1 \text{Quality}_{ij} \cdot \text{info}_{ijt} + \beta_2 \text{info}_{ijt} + \gamma_1 \text{prior}_{ij} \cdot \text{Quality}_{ij} \cdot \text{info}_{ijt} + \\ & \gamma_2 \text{prior}_i \cdot \text{info}_{ijt} + \zeta_{jt} + c_i + \nu_{ijt} \end{aligned} \tag{7}$$

The specification in equation 7 is motivated by the discussion that voters' reaction to the disclosure of information about politicians depends on what were their priors about their actions to begin with (Ferraz and Finan, 2008; Arias et al., 2019). Therefore, γ_1 captures what is the additional effect of disclosing positive information about the quality of schools when voters have high (relative to low) expectations about the performance of students in these institutions.

We use various approaches to proxy for voters' priors about public education. Our first approach uses the Quality Index 2005, released in 2007, as a measure of the information voters would already have had about school performance before the 2007 Quality Index was released in 2008. Since the Quality Index 2005 collected information about student performance only a few months after the incumbent mayors in our sample took office, voters in 2008 might plausibly have used it as a benchmark against which to assess the Quality Index 2007 information, rather than assigning credit or blame for it to the (then) brand-new mayor. As with the Quality Index 2007, we only have the Quality Index 2005 for treated schools; we assume it is zero for control schools.

Our second approach to studying voters' priors is to predict performance using the observed characteristics of schools. One limitation is that we cannot control for differential trends in priors between treatment and control groups. Therefore, we also create an alternate proxy for priors which uses observable characteristics of schools (e.g, having a science lab) to create an index which we call *Infrastructure Index*. This index captures the idea school resources are likely to correlate with student test scores. As a reality check, we also construct an equipment index, which also proxies for resources available at the school which are likely unobservable to parents and other stakeholders (e.g, having a copy machine or Internet connection).

7.3.1 Priors: Do voters respond to levels or changes?

In Section 5 we showed that voters react to information about levels of school quality. But voters might also care about the change in performance even in localities that have low performing schools. The unique setting of Brazil allows us to test for this. While the school accountability index was created in 2005 and was supposed to test students every two years, the first release of the IDEB index only occured in 2007 (relative to the 2005 sccore) while the second release occured in 2008 (relative to the 2007 score).

In Table 8 we test whether voters react to changes in the IDEB score for 2005 and 2007. Because not all schools that have a Quality Index for 2007 were also evaluated in 2005, we first replicate in columns (1) and (2) our main results for the sample of treated schools for which we have information about performance in both 2005 and 2007 (note that the control group remains the same) and show that the previous results are similar for this new sample of schools. In column (3) we test whether voters reward (or punish) schools that improve (or diminish) the score between the 2007 and 2008. While the coefficients go in the expected direction, the results are not statistically significant. In column (4) we interact our treatment indicator with the change in the school quality index and show that, similar to the estimates in column (3) the change is not associated with vote shares. In column (5) we interact our informed indicator separately with the level of school quality in 2007 and the change. Our results show that voters react more to the level (coefficient 1.4, standard error 0.27) rather than changes in the school quality index.

7.4 Priors: Do voters respond to surprises?

So far we have shown that information about school quality affects the vote share of the incumbent mayor, specially for schools in the top or bottom of the quality distribution. However, the release of information itself does not affect the incumbent electoral performance. We interpret these findings as suggestive evidence that, on average, voters are not surprised by the information they receive except when it changes their priors. To validate this interpretation, we next examine whether the electoral reward for good performance is larger when it is unexpected.

Since we do not observe voters' priors about school quality, we construct proxies for these priors. First, in column 1 of Table 9, we use the 2005 Quality Index as a proxy for voters' priors about school performance. We then interact the information dummy with the quality index of 2007 and the Quality index of 2005 to test whether voters respond more if the initial quality was low but the quality released in 2007 was high, for example. We find that the effect of the 2007 quality index does not depend on the 2005 index. Second, we create a "surprise" quality index as the deviation from the predicted quality index based on observable school characteristics including infrastructure and parents' education.[16] We then break down the 2007 Quality Index into a predictable component and

[16] For a related approach see Moretti (2011).

a surprise component where surprise is the unpredicted part of the Quality Index.[17]

In column 2 of Table 9 we include the predicted quality index and the actual quality index in the regression and find that both components have similar predictive power of the electoral reward for good performance. One concern with the results we present in columns 1 and 2 of Table 9 is that they may be confounded with differential trends in priors about school quality in informed and non-informed groups of voters. To be able to control for such trends, we use only variables that were observable to voters in both treatment and control groups to infer priors on school quality before disclosure of school performance. Thus, we construct two other proxies for voters' priors, based on pre-determined characteristics of schools. First, we build an infrastructure index, which should capture the quality of infrastructure of a school. The components of this index are indicators of principal's office, teacher's room, computer lab, science lab, sports court, playground, and library. The index is constructed by summing these indicators. Second, as a reality check, we construct an equipment index, which should capture only characteristics of a school's structure which we believe are less observable to voters than infrastructure characteristics. This index is constructed by taking the sum of the indicators of having at least one computer, television, printer, and copy machine.

In columns (3) and (4) of Table 9 we include the Z-score of these indexes in our regressions. In column (3) we find that the reward for good performance is larger when the infrastructure index is lower, which is consistent with expectations about quality being lower for these schools. We do not find the same pattern when considering the quality of schools' equipment as proxy for priors about quality, which we report in column (4). This is consistent with our view that the hardware that a school has are less observable to the local community than the overall infrastructure of the school.

8 Conclusion

This paper examines whether voters react to information about public service delivery. Differently from most of the existing literature that focuses on corruption and mismanagement, we address this question in the context of public education provision in Brazil. We explore a natural experiment which provided voters with information about the quality of some public schools, but not

[17] The variables we use to predict 2007 performance are the 2005 Quality Index, the number of students enrolled on 5th grade in 2008, the infrastructure index, the proportion of parents who have completed high school education or more, and socio-economic characteristics of the students in that school.

others. This allows us to construct treatment and control groups to compare the proportion of votes received by the incumbent politician before and after the information released. We set up this comparison in various complementary ways: using a diff-in-diff strategy and using a regression discontinuity; comparing party vote share across three election cycles or looking only at municipalities in which the same politician ran for office in the pre- and post-treatment elections.

We find that, when the information received by voters is good news, the proportion of votes of the incumbent increases between 1.3 to 2.62 percentage points ($\approx$ 3%-5%), depending on the measure of school performance we consider. But when the voters receive bad news in polling stations located at the bottom 20% of our sample, the support for the incumbent decreases by about 1.33 percentage points. We also find suggestive evidence that this electoral accountability effect comes from providing voters with information they cannot accurately predict before the release of IDEB scores.

We also find that policymakers respond to the release of information as well. Schools which learned they were in the bottom 20% of the IDEB score distribution responded by hiring teachers and reducing class sizes. This suggests that accountability measures not only have electoral effects, but also that they produce results in the desired direction.

One reason the magnitudes are not bigger could be the difficulties of low-educated voters to process news on school quality indexes or the fact that the value given to education varies across income levels. When we test if more educated citizens respond more to the release of information about school quality, we do find evidence that voters in the middle and top terciles of schooling are the ones who reward incumbents electorally when they receive positive information about the delivery of public education. The limited effects of information might also be due to voters' preferences for policies that improve their lives in the short-run rather than in the long-run as suggested by Bursztyn (2016). Voters might also not know to whom the responsibility for managing schools should be attributed, which is sometimes used as an argument for why accountability in the provision of education is hard to achieve (de Kadt and Lieberman, 2015). Future studies should examine these hypothesis and include electoral incentives in the discussion as improving the quality of education in poor and middle income countries is one of the most pressing issues in the development agenda.

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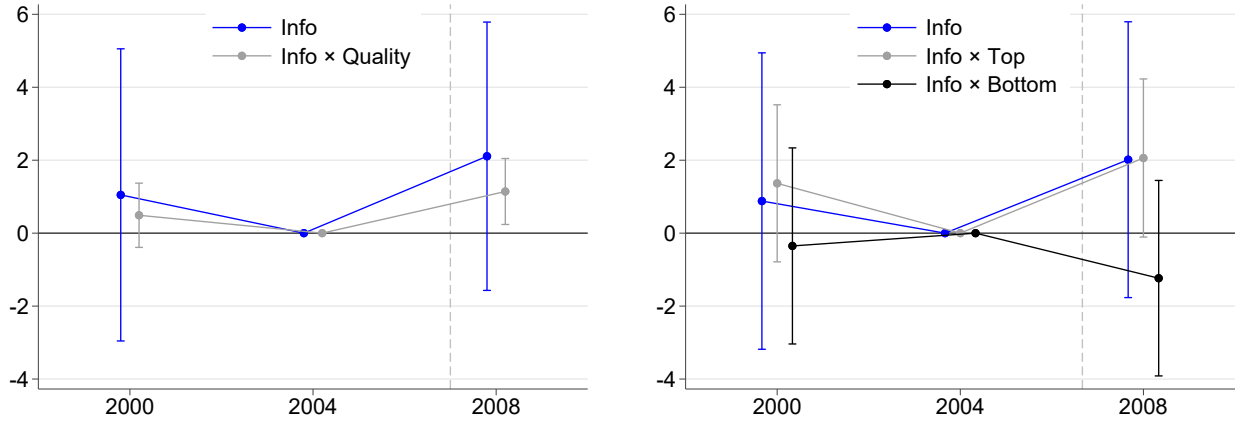
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Figure 1: The effect of school quality information on incumbent party vote share



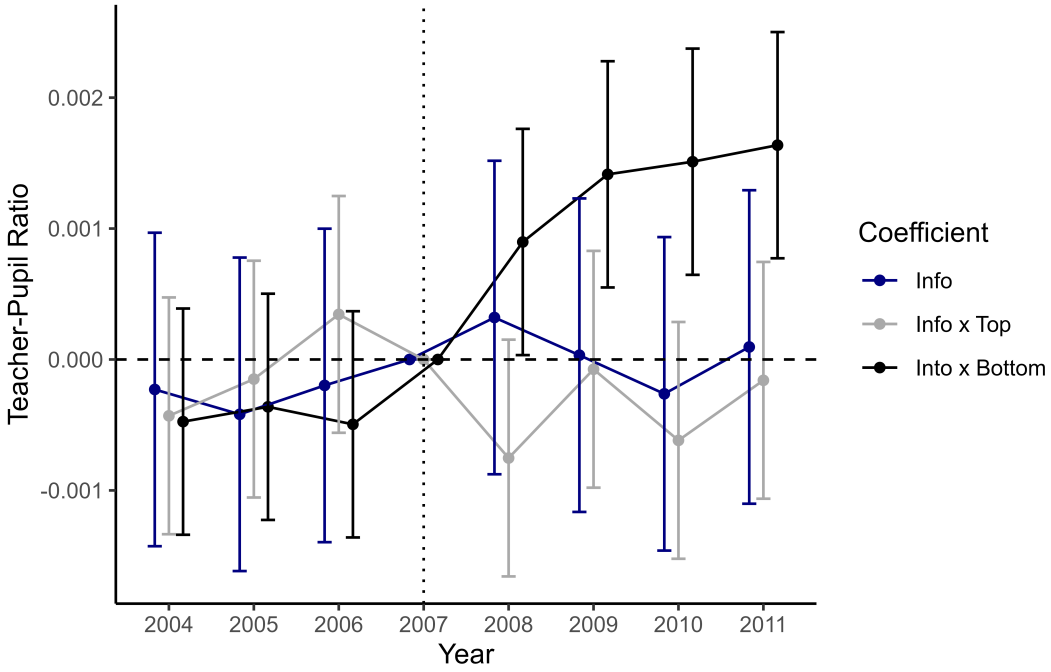
(a) Event study with continuous quality index

(b) Event study with top/bottom quality quintiles

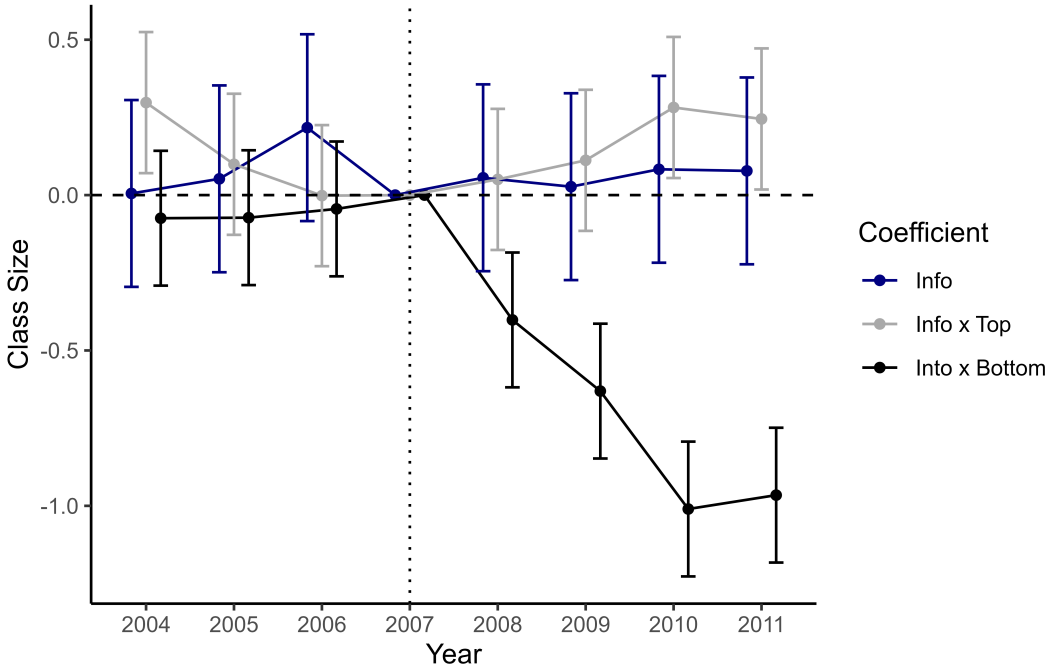
Panel (a) plots β_k coefficients (year $\times$ treatment interactions) and θ_k coefficients (year $\times$ treatment $\times$ continuous quality-index interactions) from Equation 4. Panel (b) plots β_k and θ_k^i from Equation 5, where θ_k^i are interactions of year $\times$ treatment with indicators for the top and bottom quintiles of the quality-index distribution. In both panels the outcome is municipal election vote share for the party in power at the time of the 2008 elections. The sample consists of polling stations located at schools in municipalities where the 2008 incumbent party fielded candidates in the 2000, 2004, and 2008 municipal elections.

Figure 2: The effect of school quality information on school investments

(a) Teacher-pupil ratios

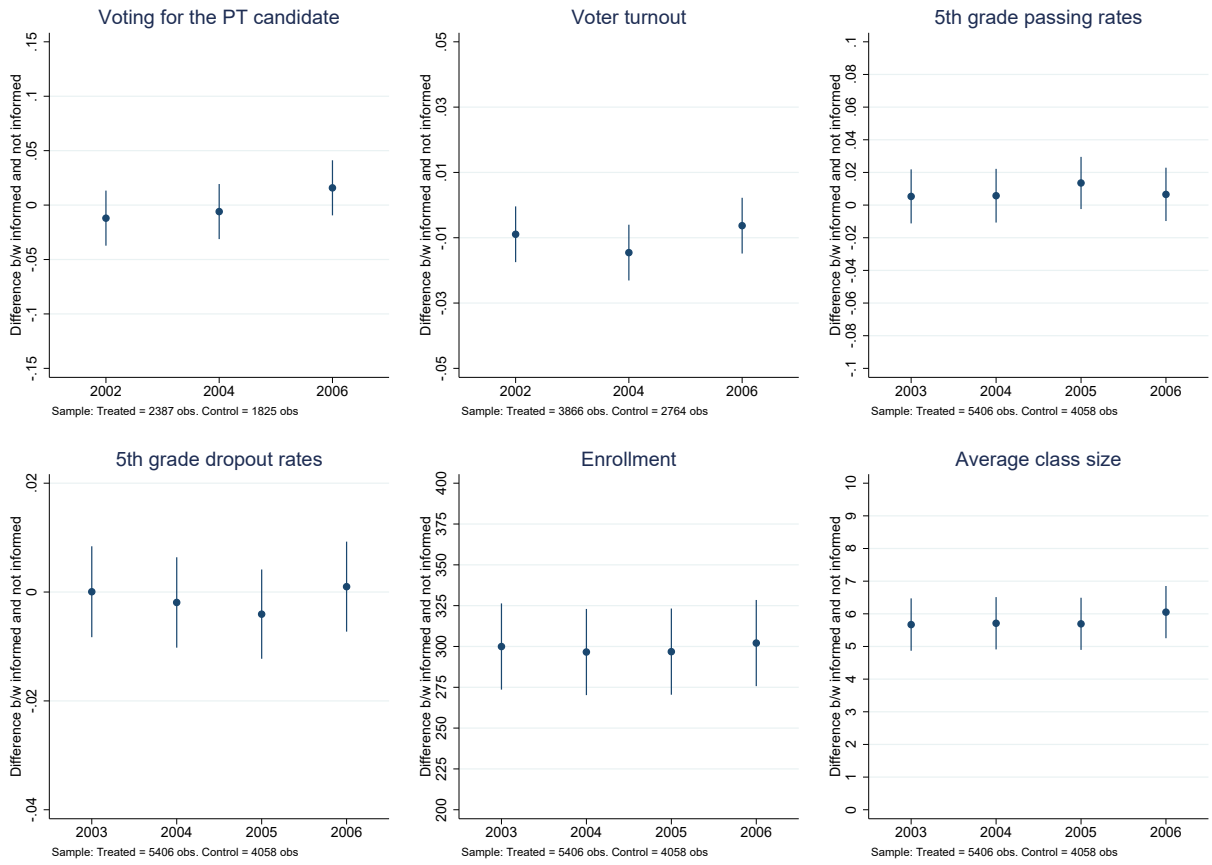


(b) Class size



Panel (a) plots β_k coefficients (year $\times$ treatment interactions) and θ_k^i coefficients (interactions of year $\times$ treatment $\times$ indicators for top and bottom quintile of the quality index distribution) from Equation 4, where the outcome variable is teacher-pupil ratio. Panel (b) plots these coefficients average class size as the outcome variable.

Figure 3: Pre-treatment trends: Electoral and school outcomes



This figure shows the evolution of the difference in pre-2008 electoral and school outcomes between treatment and control groups over time. We use voting for the PT candidate to proxy for electoral outcomes to maximize sample size, since 2004 is a municipal election year. Data for electoral outcomes comes from TSE, while data for school outcomes come from the School Census. To be conservative, none of the confidence intervals shown in this figure account for heteroskedasticity or serial correlation in the estimation of standard errors.

Figure 4: Share of urban schools with 2007 IDEB Score by 2006 enrollments

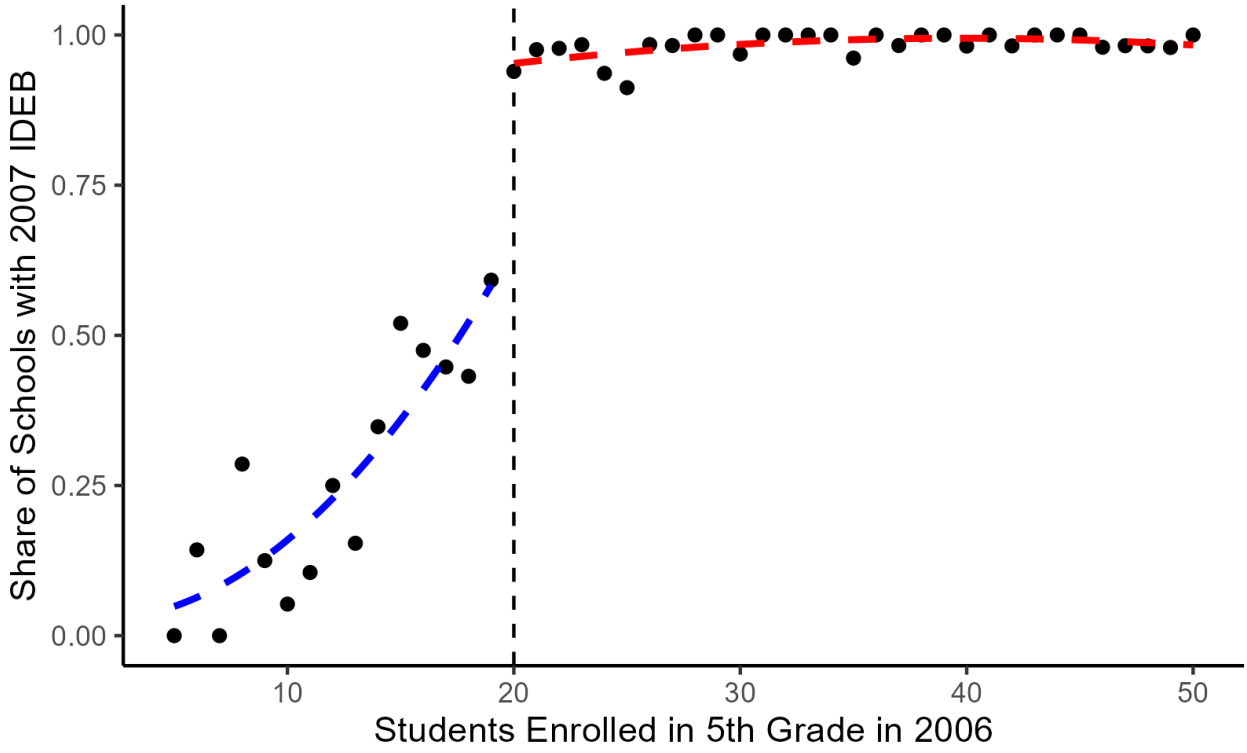


Table 1: Summary statistics

	Mean	SD	Min	Max
School characteristics				
Teacher's room	0.55	0.50	0.00	1.00
Computer lab	0.24	0.43	0.00	1.00
Science lab	0.04	0.19	0.00	1.00
Library	0.38	0.49	0.00	1.00
Number of computers	2.36	5.07	0.00	63.00
Number of teachers	15.40	12.55	1.00	216.00
Enrollment (1-5 grades)	240.34	197.53	6.00	1695.00
Average class size	23.46	8.46	3.60	96.00
Dropout rate 5th grade	0.06	0.12	0.00	8.44
Passing rate 5th grade	0.82	0.22	0.00	8.50
Voter characteristics				
Age < 25	0.24	0.10	0.00	0.78
$24 <$ Age < 60	0.63	0.08	0.20	0.93
Age ≥ 60	0.13	0.06	0.01	0.39
Male	0.50	0.04	0.32	0.74
Education attainment: Incomplete Middle School	0.73	0.15	0.09	0.99
Education attainment: Complete Middle School	0.19	0.09	0.00	0.52
Educaton attainment: High school degree	0.08	0.08	0.00	0.72
Incumbent characteristics				
Vote share x 100	50.80	15.71	0.50	100.00
Male	0.91	0.29	0.00	1.00
Married	0.86	0.60	0.00	5.00
Age	46.85	9.24	21.00	86.00
Education attainment: Incomplete Middle School	0.07	0.26	0.00	1.00
Education attainment: Complete Middle School	0.09	0.28	0.00	1.00
Education attainment: High School degree	0.28	0.45	0.00	1.00
Education attainment: College degree	0.55	0.50	0.00	1.00
Observations	19364			

Notes: School characteristics come from the 2004 School Census, which is the year of our baseline elections. IDEB scores come from a public dataset, which contains IDEB grades for each elementary school evaluated and is released by INEP. Voters' characteristics come from the TSE registry for the 2008 elections, which is the first year for which this data is available at the level of polling stations. The variable enrollment is for elementary school, i.e. grades 1-4 or 2-5. The variables passing rates and dropout rates are for fifth grade only. Incumbents' characteristics come from the TSE registry for the 2004 elections.

Table 2: The effect of school quality information on school inputs

Dependent Variables:	Teacher-Pupil Ratio	Class Size	Access to Water	Access Sanitation
Model:	(1)	(2)	(3)	(4)
Variables				
	(0.0002)	(0.0532)	(0.0024)	(0.0035)
2004 × Informed	-0.0002	0.0053	0.0050	-0.0083
	(0.0004)	(0.1406)	(0.0076)	(0.0118)
2005 × Informed	-0.0004	0.0523	-0.0006	0.0026
	(0.0004)	(0.1334)	(0.0068)	(0.0112)
2006 × Informed	-0.0002	0.2170	-0.0061	-0.0013
	(0.0004)	(0.1471)	(0.0047)	(0.0057)
2008 × Informed	0.0003	0.0557	0.0058*	-0.0056
	(0.0004)	(0.0959)	(0.0034)	(0.0065)
2009 × Informed	0.00003	0.0271	0.0043	-0.0045
	(0.0004)	(0.1128)	(0.0039)	(0.0084)
2010 × Informed	-0.0003	0.0830	0.00003	-0.0075
	(0.0005)	(0.1242)	(0.0048)	(0.0091)
2011 × Informed	0.0001	0.0779	0.0050	-0.0192*
	(0.0005)	(0.1311)	(0.0060)	(0.0101)
2004 × Informed x (Quality Index 2007 in Top 20%)	-0.0004	0.2977***	0.0097**	-0.0135*
	(0.0004)	(0.0936)	(0.0044)	(0.0078)
2005 × Informed x (Quality Index 2007 in Top 20%)	-0.0001	0.0992	0.0038	-0.0100
	(0.0003)	(0.0841)	(0.0040)	(0.0069)
2006 × Informed x (Quality Index 2007 in Top 20%)	0.0003	-0.0018	0.0041	-0.0009
	(0.0003)	(0.0706)	(0.0027)	(0.0042)
2008 × Informed x (Quality Index 2007 in Top 20%)	-0.0008**	0.0505	0.0052**	0.0013
	(0.0004)	(0.0618)	(0.0021)	(0.0045)
2009 × Informed x (Quality Index 2007 in Top 20%)	0.0001	0.1119	0.0005	-0.0046
	(0.0003)	(0.0750)	(0.0026)	(0.0061)
2010 × Informed x (Quality Index 2007 in Top 20%)	-0.0006*	0.2820***	-0.0011	-0.0010
	(0.0003)	(0.0815)	(0.0029)	(0.0065)
2011 × Informed x (Quality Index 2007 in Top 20%)	-0.0002	0.2450***	-0.0031	-0.0014
	(0.0004)	(0.0883)	(0.0031)	(0.0071)
2004 × Informed x (Quality Index 2007 in Bottom 20%)	-0.0005	-0.0744	-0.0080	-0.0048
	(0.0003)	(0.1196)	(0.0070)	(0.0087)
2005 × Informed x (Quality Index 2007 in Bottom 20%)	-0.0004	-0.0727	-0.0124**	-0.0036
	(0.0003)	(0.1092)	(0.0062)	(0.0079)
2006 × Informed x (Quality Index 2007 in Bottom 20%)	-0.0005*	-0.0445	-0.0032	-0.0009
	(0.0003)	(0.1001)	(0.0033)	(0.0040)
2008 × Informed x (Quality Index 2007 in Bottom 20%)	0.0009**	-0.4017***	-0.0057	0.0017
	(0.0004)	(0.0804)	(0.0040)	(0.0051)
2009 × Informed x (Quality Index 2007 in Bottom 20%)	0.0014***	-0.6307***	-0.0037	0.0095
	(0.0003)	(0.0967)	(0.0047)	(0.0064)
2010 × Informed x (Quality Index 2007 in Bottom 20%)	0.0015***	-1.010***	-0.0063	0.0109
	(0.0004)	(0.1055)	(0.0054)	(0.0068)
2011 × Informed x (Quality Index 2007 in Bottom 20%)	0.0016***	-0.9656***	-0.0138**	0.0042
	(0.0004)	(0.1106)	(0.0061)	(0.0073)
Mean in the control group in 2007	0.049	24.74	0.739	0.345
Observations	132,824	132,821	132,824	132,824
Year x Rural	Yes	Yes	Yes	Yes

Notes: This table shows the effect of the release of school quality information on four measures of school investment: Teacher-pupil ratio, class size, access to water, and access to sanitation. Year dummies are interacted with the treatment variable ('Informed') and dummies for whether the 2007 Quality Index was revealed to be in the top or bottom quintile.

Table 3: The effect of school quality information on incumbent mayor vote share

Dependent variable: % votes (0 - 100)	(1)	(2)	(3)	(4)
Informed	0.65	0.92	0.76	0.34
	(0.87)	(0.88)	(0.90)	(0.90)
Informed x Quality Index 2007		1.30***		
		(0.24)		
Informed x (Quality Index 2007 in Top 20%)			2.14***	
			(0.66)	
Informed x (Quality Index 2007 in Bottom 20%)			-1.54***	
			(0.57)	
Informed x (Quality Index 2007 in State Top 20%)				1.51**
				(0.62)
Informed x (Quality Index 2007 in State Bottom 20%)				0.34
				(0.61)
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3
Obs informed	5578	5578	5578	5578
Obs not informed	4104	4104	4104	4104
Post x Rural	Yes	Yes	Yes	Yes

Notes: This table shows the effect of providing voters with information about school quality on the vote-share of the incumbent running for reelection. In column 1, we show the average effect. In columns 2 to 5, we test for heterogeneities according to two different measures of school-level performance. Standard errors are clustered at the school (i.e, treatment) level. We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100. We normalize the Quality Index for 2007 to have a zero mean and a one point standard deviation. Top and bottom performers are determined relative to schools in the sample. Top and bottom performers in the state are determined relative to to all schools in the state, regardless of being or not in our final sample.

Table 4: RD first stage: The effect of crossing the enrollment threshold on the revelation of school quality information

	Urban		Rural (Placebo)	
≥ 20	0.390***	0.390***	0.018	0.021
	(0.070)	(0.070)	(0.018)	(0.017)
$\geq 20\times$ Quality index		-0.006		1.061
		(0.009)		(0.895)
Enrollment	0.036	0.036	-0.001	-0.001
	(0.034)	(0.034)	(0.001)	(0.001)
$\geq 20\times$ Enrollment	-0.022	-0.022	0.003	-0.001
	(0.035)	(0.036)	(0.007)	(0.006)
$\geq 20\times$ Quality index $\times$ Enrollment		0.001		-0.566*
		(0.003)		(0.290)
N	385	385	1,018	1,018
Mean (untreated)	0.491	0.491	0.002	0.002

Notes: This table shows the first-stage effect of crossing the fifth-grade 20-enrollment threshold on treatment. Dependent variable a binary for having a 2007 IDEB score publicized in 2008. The sample consists of polling stations at schools with an enrollment between 16–23 (inclusive) in 2006, in municipalities where the incumbent mayor was running for reelection. 'Quality index' is a standardized measure of the 2007 quality index for treated schools. Columns 1 and 2 include schools in urban areas; columns 3 and 4 include schools in rural areas as a placebo test.

Table 5: RD results: The effect of school quality information on incumbent vote share

	%votes (2004) (Placebo)		%votes (2008)		Δ %votes	
≥ 20	-3.120	-3.407	-4.166	-3.786	-0.610	0.060
	(2.674)	(2.653)	(3.182)	(3.218)	(3.508)	(3.485)
$\geq 20\times$ Quality index		-2.405		5.658*		7.218*
		(2.494)		(3.330)		(3.773)
Enrollment	-0.283	-0.283	0.317	0.296	0.393	0.393
	(1.008)	(1.011)	(1.047)	(1.052)	(1.203)	(1.206)
$\geq 20\times$ Enrollment	1.582	1.722	1.090	1.018	-0.349	-0.614
	(1.242)	(1.238)	(1.408)	(1.430)	(1.598)	(1.596)
$\geq 20\times$ Quality index $\times$ Enrollment		1.276		-1.475		-2.599*
		(0.904)		(1.258)		(1.387)
N	385	385	385	385	385	385
Mean (untreated)	50.582	50.582	50.934	50.934	0.352	0.352

Notes: This table shows the effect of school quality information on electoral outcomes. Columns 1 and 2 show vote share for the 2008 incumbent mayor in 2004, as a placebo test; Columns 3 and 4 show vote share for the incumbent in 2008; Columns 5 and 6 show the change in 2008 municipal vote share for the incumbent mayoral candidate relative to the same candidate's 2004 vote share. Dependent variable is measured on a scale from 0–100. The sample consists of polling stations at urban schools with an enrollment between 16–23 (inclusive) in 2006, in municipalities where the incumbent mayor was running for reelection. 'Quality index' is a standardized measure of the 2007 quality index for treated schools.

Table 6: The effect of school quality information on incumbent mayor vote share: Heterogeneity by citizens' schooling

Dependent variable: % votes (0 - 100)	(1) Mother's schooling	(2) Father's schooling
Informed x Tercile 1 of schooling	1.04	1.08
	(1.22)	(1.21)
Informed x Tercile 2 of schooling	0.33	-0.16
	(1.35)	(1.33)
Informed x Tercile 3 of schooling	3.87**	4.62***
	(1.71)	(1.78)
Informed x Quality Index 2007 x Tercile 1 of schooling	0.65	0.76
	(0.53)	(0.52)
Informed x Quality Index 2007 x Tercile 2 of schooling	0.89**	0.86**
	(0.39)	(0.38)
Informed x Quality Index 2007 x Tercile 3 of schooling	1.35***	1.04***
	(0.39)	(0.40)
Obs informed	5221	5221
Obs not informed	1867	1867
Post x Rural	Yes	Yes
Mean % votes in the control group in 2004	50.8	50.8

Notes: This table shows the effect of providing voters with information about school quality on the vote-share of the incumbent running for reelection, considering the heterogeneity in voters' schooling. We use data from *Prova Brasil 2009* to establish parent's schooling. This means we are restricting our control group sample to schools that are not assigned with a Quality Index before 2008, but enter the evaluation in 2009. *Prova Brasil* is the standardized test considered in the construction of the IDEB Quality Index. The terciles of schooling are determined based on the proportion of parents with at least a high school degree. We use school-level IDEB scores as the Quality Index in these regressions. IDEB scores are normalized, so that the average is zero and the standard deviation is one in each tercile.

Table 7: The effect of school quality information on incumbent mayor vote share: Expanded Sample

Dependent variable: %votes (0-100)	(1)	(2)	(3)
Informed	1.2	1.2	1.2
	(1.1)	(1.3)	(1.8)
Informed × Quality Index 2007	1.6***	2.1***	2.1***
	(0.40)	(0.76)	(0.77)
Informed × Main Sample		-0.12	
		(0.73)	
Informed × Quality Index 2007 × Main Sample		-0.82	
		(0.75)	
School	Yes	Yes	Yes
Polling Station	Yes	Yes	Yes
Post x Rural	Yes	Yes	Yes
Mean % votes in the control group in 2004	51.4	51.4	51.9
Obs informed	9328	9328	3750
Obs not informed	4756	4756	652

Notes: This table shows the effect of school quality information on 2008 vote share for the incumbent mayor. The sample consists of polling places which are schools, as well as to other polling places which can be located with a high degree of confidence within 1km of schools, in municipalities where incumbent mayors are running for reelection. The dependent variable varies from 0–100. We normalize the 2007 quality index to have a zero mean and a one point standard deviation.

Table 8: The effect of school quality information on the incumbent mayor vote share: Levels vs. changes

Dependent variable: % votes (0 - 100)	(1)	(2)	(3)	(4)	(5)
Informed	0.92	0.14		-0.03	0.14
	(.88)	(.99)		(.99)	(.99)
Informed x Quality Index 2007	1.30***	1.40***			1.40***
	(.24)	(.26)			(.27)
Informed x (Quality Index 2007 $>$ Quality Index 2005)			0.17		
			(1)		
Informed x (Quality Index 2007 $\leq$ Quality Index 2005)			-0.47		
			(1.1)		
Informed x (Δ Quality Index)				0.42	-0.00
				(.26)	(.27)
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3	51.3
Obs informed	5578	4467	4467	4467	4467
Obs not informed	4104	4104	4104	4104	4104
Post x Rural	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of providing voters with information about school quality on the vote-share of the incumbent running for reelection. In columns 1 and 2 we compare one of our main results when using the entire treatment sample to restricting it only to schools that received an IDEB score in both 2005 and 2007. In columns 3-5 we look for heterogeneities in voters' response to information about school quality using variables that represent the evolution of performance scores over time. Standard errors are clustered at the level of schools, since this is the level at which the information treatment is applied. We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100. We normalize the 2007 quality index and ΔQuality Index to have a zero mean and a one point standard deviation.

Table 9: The effect of school quality information on incumbent mayor vote share: Heterogeneity by citizens' priors

Dependent variable: % votes (0 - 100)	(1)	(2)	(3)	(4)
Informed	0.01	0.16	1.01	0.43
	(1.02)	(0.99)	(0.90)	(0.90)
Informed x Quality Index 2007	1.39***		1.59***	1.02***
	(0.45)		(0.28)	(0.30)
Informed x Quality Index 2005	-0.01			
	(0.44)			
Informed x Quality Index 2007 x Quality Index 2005	0.15			
	(0.24)			
Informed x Residual Quality Index 2007		1.05**		
		(0.49)		
Informed x Predicted Quality Index 2007		1.55***		
		(0.31)		
Informed x Zscore Infrastructure Index			-0.58	
			(0.45)	
Informed x Quality Index 2007 x Zscore infrastructure index			-0.82***	
			(0.27)	
Informed x Zscore Equipment Index				-0.21
				(0.46)
Informed x Quality Index 2007 x Zscore equipment index				0.05
				(0.32)
Number of informed polling stations	4467	4465	5578	5578
Number of not informed polling stations	4104	4104	4104	4104
Post x Rural	Yes	Yes	Yes	Yes
Post x Prior	No	No	Yes	Yes
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3

Notes: In this table we look at evidence of whether priors matter for voters' response to information about school quality. In column 1, we interact the performance in 2005 with the performance in 2007, assuming that the earlier release of the quality index fixes these priors. In column 2 we include an interaction between the treatment indicator and the predicted 2007 score, 2005 performance, the infrastructure index, school size, parents' schooling, and socio-economic characteristics of students to predict performance in 2007, which we denote by Predicted Quality Index 2007. In column 3 we interact 2007 performance with the infrastructure index, which captures observable characteristics of schools prior to the information release. In column 4, we interact 2007 performance with the equipment index, which we believe should capture less observable characteristics of schools. Standard errors are clustered at the polling station level. We re-scale the dependent variable to vary between 0 and 100.

Appendix

A Robustness checks

A.1 Difference-in-differences: parallel pre-trends

We focus on the worker's party (PT) because it had a candidate running in all the presidential elections in this period. The pre-treatment municipal election we consider was held in 2004, while presidential elections were held in 2002 and 2006. In order to increase the number of observations plotted in our pre-trends figures, when we look at the 2004 municipal elections, a vote for the PT candidate represents a vote for any candidate who is officially supported by the party PT. To ensure we are comparing electoral outcomes in the same places over time, we only keep polling stations that existed in all the years we consider and that had a candidate associated to the PT party in both municipal elections (in the Presidential elections this is true for all polling stations in the country). We also look at how voter turnout behaves before the release of the IDEB information.

We plot pre-treatment trends in Figure 3. We estimate the plotted coefficients allowing for differential trends in the electoral and school outcomes between urban and rural areas. The figures suggest that polling stations that received and did not receive information about school quality prior to 2008 had similar trends in both electoral and schooling outcomes.

A.2 Difference-in-differences: comparing more similar schools

We have shown evidence that incumbents are somewhat held accountable for the quality of local public education when voters receive information about the performance of the municipal school near them. A concern with the validity of this result is that schools that received an IDEB score before 2008 are different than those that were not evaluated before the elections. This section presents robustness checks that address such concerns.

Table A1 compares the mean characteristics of schools, voters and electoral outcomes in treatment and control groups. Some differences are significant, although most are small magnitude; the dimension in which these groups vary the most is school size.

The first part of Table A1 shows that the infrastructure in schools that have an IDEB score before

Table A1: Characteristics of polling stations in treatment and control groups

	Rural			Urban		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Treatment	Diff	Control	Treatment	Diff
School characteristics						
Teacher's room	0.31	0.40	-0.09*	0.43	0.74	-0.31***
Computer lab	0.07	0.13	-0.06**	0.10	0.37	-0.26***
Science lab	0.02	0.02	-0.00	0.01	0.05	-0.04***
Library	0.22	0.31	-0.10**	0.31	0.50	-0.19***
Number of computers	0.56	0.93	-0.37*	1.04	3.74	-2.70***
Number of teachers	8.61	13.87	-5.25***	7.75	20.62	-12.87***
Enrollment (1-5 grades)	110.23	216.70	-106.47***	105.57	339.64	-234.08***
Average class size	22.06	26.50	-4.44***	18.29	24.66	-6.36***
Dropout rate 5th grade	0.07	0.07	0.00	0.05	0.05	0.00
Passing rate 5th grade	0.77	0.77	0.00	0.80	0.81	-0.01
Observations	3797	151	3948	307	5427	5734
Voter characteristics						
Age $<$ 25	0.24	0.25	-0.00	0.22	0.24	-0.02**
24 $<$ Age $<$ 60	0.61	0.61	-0.00	0.63	0.64	-0.00
Age $\geq$ 60	0.15	0.14	0.00	0.15	0.13	0.02***
Male	0.52	0.50	0.02***	0.51	0.49	0.02***
Incomplete Middle School	0.82	0.79	0.03***	0.72	0.66	0.06***
Complete Middle School	0.14	0.15	-0.02**	0.20	0.23	-0.03***
High school degree	0.04	0.05	-0.01***	0.08	0.11	-0.03***
Observations	3797	151	3948	307	5427	5734
Election outcomes						
% votes PT 2006	0.64	0.65	-0.02	0.55	0.55	0.01
% votes PSDB 2006	0.33	0.31	0.02	0.39	0.39	-0.00
Turnout in 2006	0.82	0.80	0.02***	0.83	0.82	0.00
Observations	3746	150	3896	302	5408	5710

Notes: We use data from the 2004 School Census to test the difference in means of school characteristics. For voters' characteristics, the data comes from the 2008 municipal elections, which is the first year for which characteristics of voters are available at the level of polling stations. Electoral outcomes come from the 2006 presidential elections in Brazil.

2008 is better than in schools that do not receive such score. Treated schools are also larger in terms of number of students and number of teachers than those in the control group. On the other hand, while the outcomes of schools in both groups are statistically different, the magnitude of this difference is smaller relative to the other characteristics of schools. From Table A1 we see that schools in the control group have dropout rates slightly larger and passing rates smaller, on average, than schools that receive an IDEB score before 2008. In the second part of Table A1 we show that the characteristics of voters are different in polling stations located at schools that are or not evaluated by IDEB. Schooling levels are the variables that differ the most between both groups. This is consistent with rural schools being mainly in the control group.

The bottom of Table A1 shows that electoral outcomes are not very different between treatment and control groups, and the difference in voter turnout between groups is also very small and mostly insignificant. Although the statistics we present in Table A1 indicate that our treatment and control groups are somewhat different in their mean characteristics, this is not crucial to our identification strategy.

To address the concern that treated schools are, on average, larger than control schools, we restrict the size of schools in our sample by removing those with less than 50 or more than 300 students in grades 1-5. These cutoffs have the purpose of making schools in treatment and control groups more similar, but without losing too many observations in any of them. Alternatively, we account for the dispersion of school size in the treatment group by re-weighting observations based on the propensity score.

We report the results of trimming the sample and re-weighting observations based on the propensity score in Table A2. We find results that are qualitatively the same as the estimates from our main specification. In columns 1 and 4 of Table A2 we show that the average effect of informing voters about the quality of public municipal schools on the vote-share of the incumbent mayor running for reelection is larger in magnitude for the trimmed sample and when we re-weight observations by the propensity score, but still not statistically significant. In columns 2 and 3 of Table A2 we show that the reward and punishment for, respectively, good and bad performance is smaller in magnitude for the trimmed sample than that of our main findings. When we re-weight observations based on the propensity score we find that the reward (punishment) for good (bad) performance are virtually the same as in the main empirical estimates.

Another robustness check takes advantage of the fact that many schools in our control group received an IDEB score after the period of our main analysis, in 2009 and/or 2011. Initially only urban schools were eligible to participate in the IDEB evaluation, but this changed in 2009 and many rural schools in our sample started being evaluated from 2009 onwards. As a robustness check we restrict the control group to schools that did not receive an IDEB score before 2008, but that were evaluated in 2009 and/or 2011. We reproduce our preferred specification for this different sample and show in columns (1)-(4) of Table A3 that our results remain unchanged from making such sample restriction.

Table A2: Robustness checks: accounting for difference in size between schools in treatment and control groups

Dependent variable: % votes (0-100)	(1)	(2)	(3)	(4)	(5)	(6)
	Trimmed sample			Re-weighted estimates		
Informed	1.50	1.76*	1.84*	2.49	2.94*	2.77
	(0.98)	(0.99)	(1.03)	(1.78)	(1.77)	(1.80)
Informed x Quality Index 2007		1.07***			1.42***	
		(0.32)			(0.27)	
Informed x (Quality Index 2007 in Top 20%)			1.32			2.15***
			(0.93)			(0.78)
Informed x (Quality Index 2007 in Bottom 20%)			-1.89**			-1.72***
			(0.75)			(0.63)
Mean % votes in the control group in 2004	51.4	51.4	51.4	51.3	51.3	51.3
Obs informed	2987	2987	2987	4360	4360	4360
Obs not informed	3267	3267	3267	3522	3522	3522
Post x Rural	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (i) This table refers to the sample in which we drop from the sample schools which have less than 50 and more than 300 students enrolled in grades 1-5; (ii) Standard errors are clustered at the school level; The quality index for 2007 is normalized to have zero mean and a one point standard deviation; (iii) Top and bottom performers are obtained from ranking the quality index of schools in our treated sample.

A.3 Regression discontinuity: Balance tests

Figure A1 shows that the density of the running variable is smooth at the treatment threshold. It plots the histogram of fifth-grade enrollment in 2006, showing no visible spike or dip at the 20-student cutoff. A formal McCrary test confirms the visual impression: the log-density difference at the threshold is statistically indistinguishable from zero (p = 0.298), indicating that schools do not appear to manipulate enrollment to sort just above or below the policy rule.

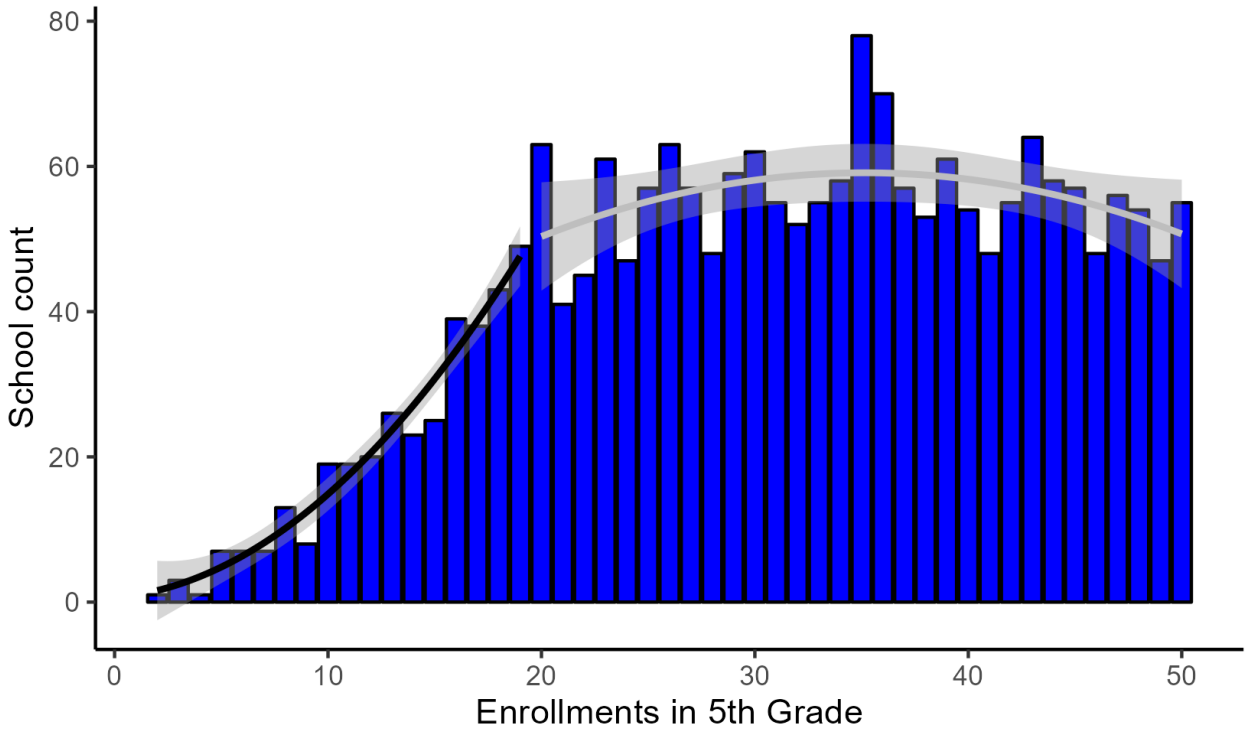
Figure A2 further supports the continuity of pre-treatment characteristics. The left panel shows that a normalized infrastructure index evolves smoothly with enrollment, and the local polynomial fits on either side of the cutoff line up without any discernible break. The absence of a statistically meaningful jump implies that basic infrastructure does not change discontinuously at the 20-student threshold. A similar pattern is observed for the normalized equipment index in the right panel looking at the relationship between enrollment and pedagogical equipment. The fitted curves are continuous at the cutoff, and the scatter shows no clustering that would suggest a sudden improvement (or deterioration) in pedagogical equipment among schools that barely qualify for IDEB

Table A3: Restricting the control group to schools with IDEB in 2009 or 2011

Dependent variable: %votes (0-100)	(1)	(2)	(3)	(4)
Informed	0.29	0.67	0.48	-0.01
	(1.09)	(1.10)	(1.12)	(1.11)
Informed x Quality Index 2007		1.30***		
		(0.24)		
Informed x (Quality Index 2007 in Top 20%)			2.13***	
			(0.66)	
Informed x (Quality Index 2007 in Bottom 20%)			-1.53***	
			(0.57)	
Informed x (Quality Index 2007 in State Top 20%)				1.50**
				(0.62)
Informed x (Quality Index 2007 in State Bottom 20%)				0.34
				(0.61)
Mean % votes in the control group in 2004	51.6	51.6	51.6	51.6
Obs informed	5578	5578	5578	5578
Obs not informed	1725	1725	1725	1725
Post x Rural	Yes	Yes	Yes	Yes

Notes: In this table we restrict the control group to the sample of schools that did not receive a Quality Index before 2008, but that were included in the evaluation either in 2009 or 2011. Standard errors are clustered at the school (i.e, treatment) level. We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100. We normalize the Quality Index for 2007 to have a zero mean and a one point standard deviation. Top and bottom performers are determined relative to schools in the sample. Top and bottom performers in the state are determined relative to to all schools in the state, regardless of being or not in our final sample.

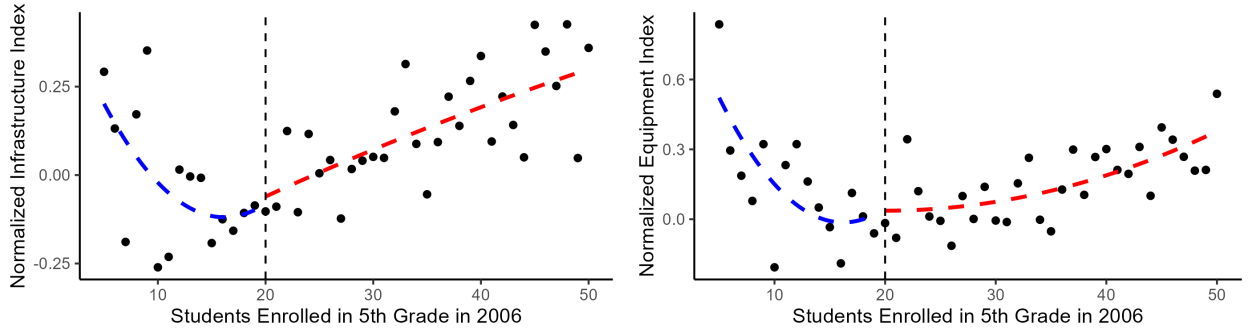
Figure A1: Number of urban schools by 2006 enrollments



This figure shows a histogram of fifth grade enrollment in urban schools in 2006.

disclosure.

Figure A2: Balance tests: infrastructure and equipment indexes in urban schools by 2006 enrollments



This figure plots indices of school infrastructure (Panel A) and pedagogical equipment (Panel B) against 2006 fifth grade enrollments.

B Data appendix

B.1 Matching polling stations to schools

We link administrative records of schools to voting records by using a fuzzy matching procedure on the names of schools and polling stations. First, we break the universe of polling stations in the 2008 elections in two based on whether or not they are located at a school. Polling stations are classified as being located at a school if their name indicates such. For example, *EMEF*, which stands for *Escola Municipal de Ensino Fundamental*, is a common abbreviation for the name of primary municipal schools. We then restrict our sample to polling stations located at schools and we match them to the schools' administrative data by name, which requires using a fuzzy matching procedure. This allows us to match, for instance, the school with name *EEEFM PE Ezequiel Ramin* with the polling station *EEEFM Padre Ezequiel Ramim* and the school *EMEFM Aldemir Lima Cantanhede* with the polling station *Esc. Aldemir de Lima Cantanhede*.

To assess if we are matching a relevant fraction of our sample, we first count how many polling stations are schools. We have around 93,000 polling stations in our sample and we estimate that 83% of them are located at schools. We exclude polling stations in municipalities that do not have an incumbent mayor running for reelection in 2008. From the remaining polling stations that are located at schools, we are able to match 75% of them to school administrative data.[18] The specifics of the matching procedure are chosen weighing the number of links with the number of mistakes made. We test different matching options and in each of them we count the number of observations successfully matched, the number of mistakes in the 50 matches with the worst matching score and the number of mistakes in a random sample of 300 observations with matching score smaller than one. We choose the specification that matches 80% of polling stations we consider are schools, has four mistakes among the fifty worst pairs and rate of less than 1% of mistakes among a random sample of 300 observations with matching score less than one.[19] From the 37,000 polling stations that we consider as being located at schools in municipalities included in our sample, about 23,000 receive a score of one, which indicates a perfect match.

[18] In the matching procedure we actually match 80% of them. However, polling stations that are matched to more than one school are excluded from our final sample, because they should be matched to only one school.

[19] We use the matchit command from Stata with the ngram circ 4 algorithm and root weights. Link to the package: https://ideas.repec.org/c/boc/bocode/s457992.html

B.2 Comparing cities, polling stations, and schools included and excluded from the analysis

In the procedure of linking polling station to schools, we lose cities, polling stations and schools that should be included in the analysis. We begin with 3047 cities in which there is an incumbent mayor running for reelection in 2008. In 3044 of them, we are able to classify at least one polling station as being a school. We are able to match at least one polling station with one school in 2999 of the remaining cities. Finally, the total number of municipalities in our final sample is 2270. The reasons why we lose cities are that all polling stations at one particular city may be matched only to private or public state schools, and that the identification of polling stations may change between 2004 and 2008 and we are unable to match them to electoral results[20]. The same happens for polling stations and schools.

To check if cities that are left out of our analysis are different from those included in our final sample, we compare the characteristics of the cities and of their incumbent mayors running for reelection. We show in Table B4 that they are similar. The p-values we report on Table B4 indicate statistical differences between these two groups of cities, but the magnitude of these differences is not very large. Overall, it does not seem that the loss of observations in the process of defining our final sample is particularly correlated with any characteristics of cities or politicians.

Next, we compare the characteristics of polling stations that we leave out of our final sample[21]. We report the results of this mean comparison for the characteristics of voters at each polling station in Table B5, and we conclude that citizens in both groups are almost identical. Although p-values indicate that the voters in polling stations included in our sample are different from citizens whose voting patters are not in our analysis, the magnitude of this difference is very small.

Finally, we show the comparison of schools in and outside our final sample in Table B6. Once again, observations in and outside of our sample are, in general, statistically different. The magnitude of this difference is larger for the variables that represent the infrastructure and size of schools. The

[20] In general, the electoral data is identified in the levels of cities, electoral zones, and ballots (which are called electoral sections in Brazil). Each polling station contains several ballots. We use a dataset provided by TSE with polling station names, electoral zone numbers and ballot numbers to create a unique identifier for polling stations in 2008. To match polling station numbers to other electoral data we use city, zone, and ballot codes as unique identifiers. Although uncommon, zone and ballot numbers may change between 2004 and 2008, and that makes us lose observations when putting together different datasets (e.g, matching polling station identifiers to electoral outcomes).

[21] For this comparison, we only look at polling stations located at municipal schools.

Table B4: Comparing cities included and excluded from the final sample

	(1) Excluded from sample		(2) Included in sample		(3) Diff
	Mean	SD	Mean	SD	
People in school	0.30	0.05	0.31	0.05	0.01***
Male	0.51	0.01	0.51	0.01	-0.01***
People in public school	0.29	0.05	0.29	0.05	0.00
Married	0.33	0.09	0.31	0.08	-0.03***
Median h.h. per capita income	0.02	0.02	0.01	0.01	-0.01***
People in school age	0.17	0.03	0.17	0.03	0.00
Middle school dropout	0.31	0.05	0.31	0.05	-0.00
High school dropout	0.11	0.06	0.10	0.06	-0.01**
High school graduate	0.07	0.04	0.08	0.05	0.00
Male	0.91	0.28	0.92	0.27	0.01
Candidate married	0.86	0.53	0.88	0.63	0.02
Middle school dropout	0.15	0.36	0.11	0.31	-0.04**
High school dropout	0.14	0.34	0.11	0.31	-0.03
High school graduate	0.37	0.48	0.33	0.47	-0.04*
Bachelor's degree	0.34	0.48	0.45	0.50	0.11***
Age	45.43	9.05	46.32	9.56	0.89*
Observations	760		2270		3030

Table B5: Comparing polling stations included and excluded from the final sample

	(1) Excluded from sample		(2) Included in sample		(3) Diff
	Mean	SD	Mean	SD	
Number of voters	1203.65	1408.52	1258.16	1143.98	-54.51***
Age < 25	0.23	0.11	0.24	0.10	-0.01***
$24 <$ Age < 60	0.62	0.09	0.63	0.08	-0.00***
% Male	0.51	0.05	0.50	0.04	0.01***
Age ≥ 60	0.14	0.06	0.13	0.06	0.01***
Middle school dropout	0.72	0.18	0.73	0.15	-0.01*
High school dropout	0.19	0.10	0.19	0.09	-0.00**
High school graduate	0.09	0.09	0.08	0.08	0.01***
Observations	27315		9682		36997

three last rows of Table B6 show that, in terms of average class size and educational outcomes such as passing rates and dropout rates, schools included or not in our final sample are not too different.

B.3 Expanding the sample to polling stations not at schools

Table 7 compares treatment effects in polling stations located at schools vs. other places. This section outlines the procedures used to expand the sample of polling stations for this analysis.

Table B6: Comparison of municipal schools included and excluded from our final sample

	(1) Excluded Mean	(2) Included Mean	(3) Diff	(4) Excluded Mean	(5) Included Mean	(6) Diff
Enrollment (1-5)	109.86	88.31	-21.55***	336.49	284.36	-52.13***
Teacher's room	0.32	0.20	-0.11***	0.73	0.62	-0.11***
Computer lab	0.07	0.04	-0.04***	0.36	0.26	-0.10***
Computers	0.60	0.29	-0.31***	3.69	2.40	-1.29***
Passing rate (5)	0.81	0.80	-0.01	0.83	0.83	-0.00
Dropout rate (5)	0.07	0.08	0.01**	0.05	0.05	0.00
Science lab	0.02	0.01	-0.01***	0.05	0.04	-0.01**
Library	0.22	0.13	-0.10***	0.50	0.35	-0.15***
Mean class size	21.77	22.20	0.43*	24.68	25.20	0.52**
Observations	4075	4700	8775	5500	3850	9350

B.3.1 Geolocating polling stations

The extension requires the precise geographic coordinates of all 2008 polling stations. Raw address strings provided by the TSE are notoriously messy: they embed phone numbers, neighborhood labels, and idiosyncratic abbreviations. We therefore developed an "address–scrubber" routine that standardizes the text in four steps. First, phone markers (e.g. `FONE:`, `TEL.`) and bus-stop tokens (`PARADA`) are removed, together with dangling commas or dashes. Second, a user-maintained dictionary deletes trailing neighborhood labels (`CENTRO`, `ZONA URBANA`, *etc.*). Third, explicit *"no number"* flags (`S/N`, `SN`) are recorded, and legitimate house numbers are extracted via regex-based tokens such as , `Nº` or `Nº`. Finally, the cleaned `Street` and numeric `Number` fields are re–squished and exported to the GEOCODEBR[22] API. Roughly one half of all polling stations are geolocated at the street-and-number level, and another 13 % at the street-only level; the remainder resort to neighborhood + ZIP-code matches.

B.3.2 Matching polling stations to schools

For every geocoded polling station we compute the great-circle distance to the universe of public schools and keep the closest one. 75 % of matches lie within 1.5 km, with a median distance of 0.74 km. This proximity suggests that the selected school is, with high probability, the facility most familiar to voters passing the ballot box. By linking each station to the IDEB score of its nearest

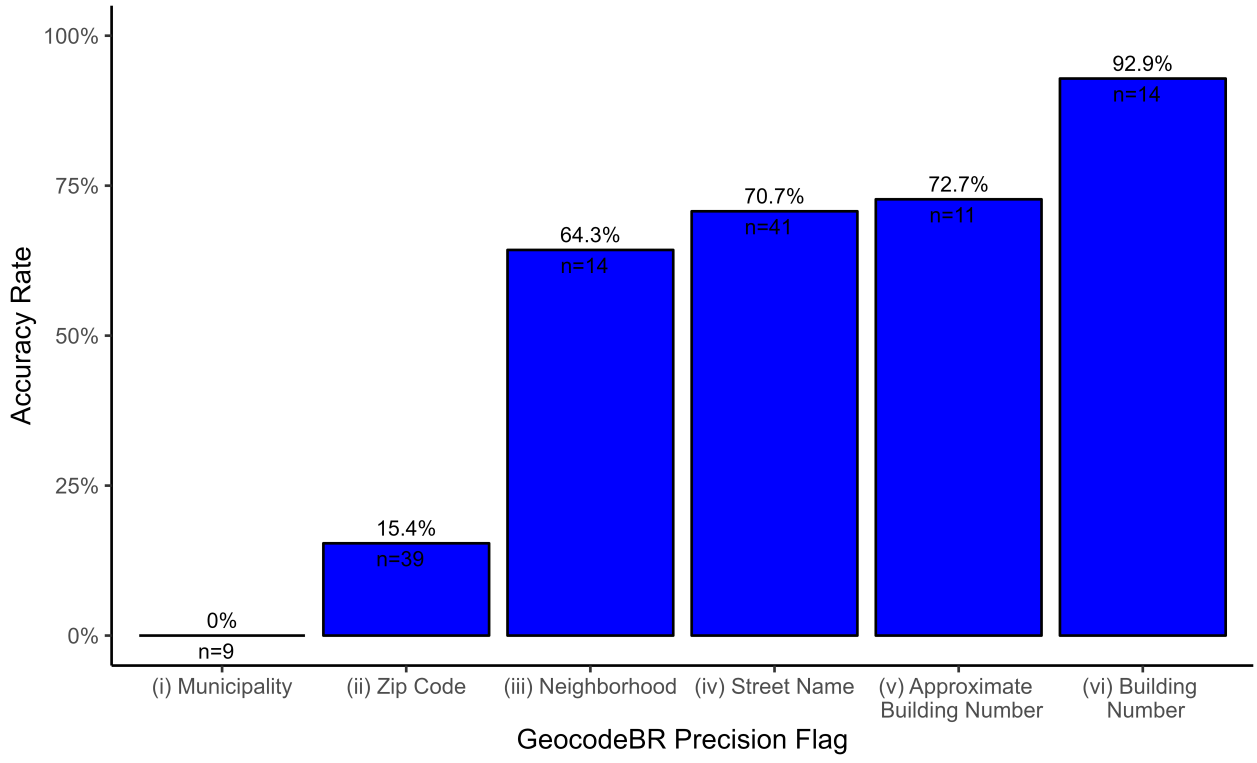
[22] https://cran.r-project.org/package=geocodebr.

school, we can test whether improvements in school infrastructure translate into higher incumbent vote shares through a *"visible facilities"* channel.

B.3.3 Data quality checks and remaining bias

The geospatial merge is not error-free. A manual audit of 150 randomly selected stations classifies 65 % of links as either *certainly correct* (30 %) or *within 1.5 km but name mismatch* (35 %), with the remaining 35 % clearly misplaced. Two patterns emerge. First, precision is over 70 % when the geocode relies on street name (and ideally building number). When the precision is at the neighbourhood level, it drops to 64 %, while zip code and municipality centroid precision provides an accuracy rate below 20 %. Thus, we will use only polling stations that were geolocated with precision level within "street name" or "street name and building number".

Figure B3: Geolocation accuracy rate by *GeocodeBR* precision category



The final estimation sample combines the main polling stations, located inside public schools, with stations situated elsewhere, but geocoded to lie within 1 km of a school, provided the geolocation precision is at least "street name".