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Voting for Quality? The Impact of School Performance Information on Electoral Outcomes

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Voting for Quality?

The Impact of School Performance Information on Electoral Outcomes *

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Abstract

We use data from polling stations and public primary schools to estimate the electoral effects of making school quality information available to voters. We exploit the introduction of a school-level accountability system in Brazil that provides, for the first time, information about school quality and exploit variation in schools affected by the policy. 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. These effects are mostly driven by the unpredicted component of school quality and are larger for 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 voters react to information on public service delivery (Khemani et al., 2016). Another set of papers focus on how improving citizens' access to information affects voters' choices, but most of the literature has focused on information about corrupt practices and politicians' characteristics rather than the quality of public services (Ferraz and Finan, 2008; Banerjee et al., 2011; Chong et al., 2014; Bobonis et al., 2016; Larreguy et al., 2018).

This paper fills this gap by examining whether voters react to information about school quality. Upon the introduction of a new school-level accountability system in Brazil, the Federal Government started a nation-wide standardized test score program that allowed for the comparison of learning outcomes across different schools and jurisdictions. We use the staggered implementation of this policy across types of schools to examine the effects of disclosing information about school quality on voting for the incumbent mayor, who is responsible for the provision of primary public education. We use a differences-in-differences strategy that compares votes at polling stations located in schools that were tested in 2007 and had their school quality index disclosed before the 2008 municipal election with polling stations at which voters did not receive information about local school quality before the election due to their ineligibility.

We find that a one standard deviation increase in the 2007 School Quality Index, disclosed before the 2008 election, is associated with a 1.3 percentage point increase in the proportion of votes for the incumbent, which corresponds to 2.5% of the baseline proportion of votes for the incumbent in non-informed polling stations. Consistent with models that predict

[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).

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. We also use the fact that there were two sets of test scores released before the 2008 municipal elections (i.e, 2005 and 2007 scores) to assess if performance in changes matters more for electoral performance than that in levels. We do find that an increase in school quality between 2005 and 2007 is related with an increase in the vote-share of the incumbent. However, we find that the effect is mostly driven by the level of 2007 performance, which means that past performance is not a particularly relevant reference point to voters.

These findings suggest that information about school quality induces some voters to change their voting patterns. 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. First, we explore the fact that two quality indexes were released before the 2008 election: in 2007 (relative to 2005 performance) and in 2008 (relative to 2007 performance). We use the 2005 Quality Index as a proxy for voters' priors for the quality of public schools as it was the first time such information became available in Brazil. Second, we predict 2007 performance using not only the 2005 Quality Index, but also an index of the quality of infrastructure of schools, socio-economic characteristics of students and the education of parents. The shortcoming of these two proxies is that they do not allow us to control for trends in priors, as we do not have test scores for the control group. To overcome this we also proxy for voters' priors using the infrastructure index, which captures the quality of infrastructure in each school.

When we estimate the effects depending on voters' priors, we do not find voting patterns to vary according with previous school performance. We do find, however, that all the reward to good performance in 2007 seems to come from the unpredicted component of school quality, which is consistent with the hypothesis that the Quality Index contains relevant and novel information about learning outcomes. We also find that the reward for good performance is lower in places where school infrastructure is better.

The accountability literature often points out to two challenges when trying to engage citizen participation by providing information about politicians or public services. First, there is evidence that 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 examine whether these two factors play a role in our setting, we look for evidence of misattribution and we decompose the effect of good performance on the incumbents' vote-share by citizens' level of schooling. To test for misattribution, we look at voting for mayors in polling stations that are located in state schools, which are not managed by local governments and thus could be thought of as a placebo test. Although the magnitude of the coefficients is smaller, we do find some evidence of misattribution. With respect to schooling levels of parents we find evidence that, in polling stations located at schools that have a higher proportion of parents with completed high school, the reward for good performance is larger.

One concern with our empirical strategy is that schools that receive an index of school quality might have different counterfactual trends in voting for the incumbent than those that are not evaluated. The criteria that determine whether or not a given school receives the objective index of quality are based on size of school and cohort and location of these institutions. Because mayors in Brazil face a two consecutive term limit, we cannot directly assess whether trends in the proportion of votes for the incumbent mayors in our sample are parallel between treatment and control groups before the release of the school quality measure. What we can do is test the evolution over time of different outcomes in informed and non-informed groups. When we do so we find that the evolution of voting and school outcomes between these groups looks similar over time.

Another concern is that schools in our treatment and control groups look somewhat different. Although similarity between these groups is not necessary for our identification assumption to hold, one might worry that this might lead to unobserved differential trends in voting for the incumbent even in the absence of the disclosure of school quality information. To address this concern, we assess the robustness of our findings to the use of alternative comparison groups. First, we restrict the control group to schools that were not evaluated before the 2008 elections, but that received the index of performance in either 2009 or 2011. These schools are mostly rural schools that were included in the standardized tests from 2009 onward. Second, because schools in the treatment group are on average larger than those in the control group, as a robustness check we restrict both treatment and control groups to polling stations located at schools that have between 50 and 300 students enrolled in grades 1-5 in 2004. Finally, we re-weight observations in the control group using the propensity score, which we estimate by regressing the probability of treatment on school characteristics such as infrastructure, class size, dropout rate, and passing rates in 2004. Our results are robust to all these alternative specifications.

Our findings relate to studies that examine how information affect public service deliv-

ery. First, there is a large number of studies that focus on citizen report cards and whether it can induce monitoring and demand for accountability (Banerjee et al., 2010; Björkman and Svensson, 2009; Björkman-Nyqvist et al., 2014). This literature's main focus is on what World Bank (2004) calls the short route of accountability, through which citizens hold providers directly responsible for the quality of the services delivered by them. Evidence on whether or not citizens use new information to hold politicians electorally accountable for delivery of public services is scarcer. Second, this paper relates directly to studies that test whether politicians are held accountable for voters' access to public goods and services (Harding, 2015; de Kadt and Lieberman, 2015). There is growing evidence about the importance of improving citizens' access to information about politicians and their actions. Among these studies, there are papers that focus on information about malfeasant behavior (Ferraz and Finan, 2008; Larreguy et al., 2018; Chong et al., 2014). Other studies look at informing voters about the characteristics of the candidates disputing an election (Banerjee et al., 2011; Kendall et al., 2015).

We know 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. In many contexts, attributes of political systems, such as widespread clientelism, may obstruct the ability of informed citizens to demand better performance from governments (See Keefer and Khemani (2005), Keefer and Vlaicu (2008) and (Bursztyn, 2016)).

On related work Romero et al. (2017) find that voters electorally reward legislators in favor of an education reform in Liberia when they receive information that this reform increased school quality. Although these legislators are responsible for approving the reform, school management is not among their attributions while in office. Therefore, we build on these findings by studying a setting in which the politicians running for office are directly responsible for managing public schools[2].

2 Institutional 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 consti-

[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.

tution 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] 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 should 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.

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 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

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

[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

editions of the evaluation. There were specific eligibility criteria that a school had to fullfil in order to have an IDEB score made public: i) have enough students enrolled in the relevant grade (the cutoff was 20 students); ii) located in urban area; iii) have more than ten students taking the standardized tests in Portuguese and Mathematics for the relevant grade.

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]. Therefore, citizens become aware of the quality of the schools near them either through the press, because they have children who go to these schools, or their community is involved with neighboring schools. Some schools even display their IDEB grade in a place of large visibility, although this is not a general rule across the country.

2.2 Municipal elections in Brazil

Municipalities in Brazil are governed by a mayor elected every 4 years through a first-past-the post system.[6] Once elected, mayors can serve up to two consecutive terms. After which they have to 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 explore made information about school quality publicly available for the first time in 2007 and then again in 2008, right before the 2008 municipal election. Our sample comprises of 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 the 2005 data, but it was only released to the public in 2007. For our sample of mayors that took office in 2005, a large part of this score cannot be attributed to their policies. Thus we use the 2005 score released in 2007 as setting a benchmark for voters with respect to school quality. The 2007 IDEB score was processed faster and released in

[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).

[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.

July 2008, three months before the municipal election having the potential to affect voters' perceptions and choices.

The official political campaigns in the elections we study in this paper began in 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 his 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 adds.

3 Data and sample

We put together a dataset that links electoral data from polling stations to the administrative data from municipal schools where polling stations are located. To do this, we use data from *Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira* (INEP) and from *Tribunal Superior Eleitoral* (TSE). These are the official government offices responsible for educational and electoral statistics in Brazil. From INEP we have data on schools' names, characteristics, student characteristics, IDEB scores, and standardized test scores named *Prova Brasil*. 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.

We match the data using a fuzzy matching procedure to link polling stations and schools using their names. 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 improve the matching procedure we first identify which polling stations are located at schools[8] We are able to match 75% of these polling stations to the administrative records of schools. We present a more detailed description of the matching procedure in Appendix

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

[8] In practice, this means having a prefix or suffix that is frequently used in the names of education institutions. For example, *EM* is often short for *Escola Municipal*, which are municipal schools.

A.1.

Up to this point, we do not make any restriction to which schools are paired to polling stations. After the matching procedure, we restrict our sample to the pair polling station-school 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's 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 to the pairs school-polling station we lose some municipalities from the sample because some municipalities only have polling stations at private schools or the identification of polling stations change between 2004 and 2008 and we are unable to match them to electoral results. In Appendix A.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 the ones included in the analysis.

We define treatment and control groups according to whether the municipal school where each polling station is located had a performance score assigned to it prior to the 2008 municipal elections. In Table 1 we show characteristics of polling stations related to their location (i.e, school characteristics) and voters' characteristics[9]. The poor infrastructure of schools is widespread. 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, they have low levels of schooling and approximately half of them voted for the incumbent in the previous election.

To investigate if our effects are heterogeneous with respect to voters' schooling, we use data from the surveys that are answered by students at the moment they take the standardized tests named *Prova Brasil*. These are the tests in Portuguese and Mathematics that are used in the construction of the IDEB quality index. The *Prova Brasil* survey provides information about the largest education level attained by each student's parents. By definition, students

[9]Note that voters' characteristics are from the 2008 municipal elections

of schools from the control group do not answer this survey neither in 2005 nor in 2007, since they are not included in the IDEB evaluation. However, a considerable number of schools in the control group are included in this evaluation in 2009. Thus, we use the data from the 2009 survey to input the information on parents' schooling for both treatment and control group observations.

4 Empirical Strategy

We use the introduction of the school-level accountability system in Brazil as a natural experiment to estimate the effect of information regarding the quality of public schools on voter behavior. We use a differences-in-differences approach to compare the proportion of votes received by the incumbent mayor running for reelection between these groups before and after the information release.

We first estimate the effect of information about the quality of schools on voting using the following equation:

$$\text{votes}_{ijt} = \alpha + \beta_1 \text{info}_{ijt} + \zeta_{jt} + c_i + \epsilon_{it} \tag{2}$$

in which votes_{ijt} is the vote share received by the mayor at the polling station i located in area j at election t and info_{ijt} is a dummy that indicates whether the voters at polling station i in region 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 in voting between rural and urban areas to account unobserved trends across these areas.

The identification of β_1 relies on two assumptions. First, informed and non-informed schools must have similar trends on electoral outcomes prior to the information release. The second assumption is that these groups remain, on average, the same over time. The standard test of the first assumption would be to compare the electoral outcomes of the candidates in our sample when they run for mayor prior to 2008. Since reelection for positions in the executive power is limited to one consecutive term in Brazil, we are unable to directly test the assumption of parallel pre-trends between treatment and control groups.

We can, however, look at pre-trends for the proportion of votes for specific parties to examine whether there were significant changes in municipal and presidential elections from 1998 to 2006. We focus on the worker's party (PT) because it had a candidate running in

all the presidential elections. The municipal election we consider was held in 2004. The 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. Finally, we complement the analysis of pre-trends in electoral outcomes assessing the pre-trends in school outcomes, since this is another margin in which treatment and control groups might differ.

We plot pre-treatment trends in Figure 1. 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.

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 2. The dimension in which these groups vary the most is school size. Although our identification strategy does not formally require them to be similar in this dimension, in the robustness section we test if our results are robust to changes in our sample that seek to improve the similarity of schools in treatment and control groups.

The first part of Table 2 shows that the infrastructure in schools that have an IDEB score before 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 2 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 2 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 2 shows that electoral outcomes are not very different between treatment and control groups. The difference in voter turnout between groups is very small. However, we do observe in Table 2 that voters who don't receive information about the

quality of the public primary schools near them are more inclined to vote for the PT candidate in the 2006 presidential elections. We address this in our robustness section as well. Although the statistics we present in Table 2 indicate that our treatment and control groups are somewhat different in their mean characteristics, this is not crucial to our identification strategy.

While equation 2 estimates an average effect, the reaction of voters might depend on the type of information provided and their priors about the quality of public schools. Following Ferraz and Finan (2008), we interact the treatment variable info_{ijt} with a measure of performance and estimate the following equation:

$$\text{votes}_{ijt} = \beta_1 \text{performance}_{ij} \cdot \text{info}_{ijt} + \beta_2 \text{info}_{ijt} + \zeta_{jt} + c_i + \nu_{ijt} \tag{3}$$

where $performance_{ij}$ is a measure of school performance in school i region j. We first use the IDEB 2007 score as a measure of performance. 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 performance as being, repectively, above/below the state median or among the top/bottom 20% best schools in our sample.[10]

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

$$\begin{aligned} \%\text{votes}_{ijt} =& \beta_1 \text{performance}_{ij} \cdot \text{info}_{ijt} + \beta_2 \text{info}_{ijt} + \gamma_1 \text{prior}_{ij} \cdot \text{performance}_{ij} \cdot \text{info}_{ijt} + \\ & \gamma_2 \text{prior}_i \cdot \text{info}_{ijt} + \zeta_{jt} + c_i + \nu_{ijt} \end{aligned} \tag{4}$$

The specification in equation 4 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

[10] Because we do not observe performance 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 performance.

what is the effect of disclosing positive information about the quality of schools when voters have high or low expectations about the performance of students in these institutions.

We use two different approaches to construct proxies for priors about public education. First, we use the Quality Index 2005 to capture priors about the performance of schools in 2007. Since the Quality Index 2005 collected information about student performance only a few months after the incumbent mayors in our sample took office, it is reasonable to assume that this information should only set a standard to what the performance of a given school should look like in the Quality Index 2007. Second, we predict performance using the observed characteristics of schools. One limitation of the first and second approaches is that we cannot control for differential trends in priors between treatment and control groups. Therefore, our third proxy for priors uses observable characteristics of schools (e.g, having a science lab) to create an index which we call *Infrastructure Index*. Such index captures the idea that schools with more resources should also be more effective in promoting student learning. As a reality check, we also construct an equipment index, which also proxies for resources available at the school, but these resources should be unobservable to parents and other stakeholders (e.g, having a copy machine or Internet connection).

5 Results

5.1 School quality information and electoral outcomes

Our empirical specification examines whether school quality information affect the voting patterns of mayoral elections. In Table 3 we begin by comparing voting outcomes in polling stations that received and did not receive information about school quality. Column (1) shows no statistically significant difference 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 additional 2.1 percentage points in the incumbents' vote-share.

We next examine whether extreme results and relative performance matter for voters by splitting schools 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 leads to a 2.14 percentage point ($\approx$ 5%) increase on the proportion of votes of the incumbent while being among the worst performers generates a punishment of 1.54 percentage points in terms of the incumbent vote share.

We also consider that voters may make yardstick comparisons between local communities to overcome political agency problems (Besley and Case, 1995). In this context, the school quality index might not be easy to interpret, but people see the information that the school in their local community is better evaluated than schools in other places as being good news. Thus, in column (4) we test if performance relative to the other schools in the same state matters to voters. We find that being among the top 20% performers in the state is associated with a 1.51 percentage point increase in the vote share of the incumbent. The average Quality Index 2007 for the schools that are overall bottom performers is 2.61. The average Quality Index 2007 for schools that are among the bottom performers in their state is 3.07, which 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 might 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.

5.2 Do voters respond to levels or changes?

In the previous section 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 4 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 (notice 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.

5.3 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 5, 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.[11] We then break down the 2007 Quality Index into a predictable component and a surprise component where surprise is the unpredicted part of the Quality Index.[12]

In column 2 of Table 5 we include the predicted quality index and the actual quality index in the regression and find that it is the surprise component of the 2007 Quality Index that leads to an electoral reward for good performance. One concern with the results we present in columns 1 and 2 of Table 5 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

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

[12] 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.

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 5 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 equipments that school has is less observable to the local community than the overall infrastructure of the school.

One question that comes up from results presented above is whether voters attribute actually attribute variation in the quality of schools to the correct politician. When students took the standardized tests used to construct the 2005 Quality Index, the incumbent mayors in our sample had only a few months in office, while in 2007 they had been managing municipal schools for almost three years. In column 1 of Table 6 we show that 2007 performance is the component that drives the relationship between school quality and votes for the incumbent mayor. We also do a placebo test comparing the electoral outcome of the incumbent mayor in polling stations located near *state* public schools that were informed and not informed about performance before the 2008 elections. Since these state schools are managed by governors, not mayors, we would not expect to see more or less votes for the incumbent mayor depending on their performance in the absence of misattribution. In columns 2-6 of Table 6 we show that voters in state-level schools ranked in the bottom and top seem to reward and punish mayors suggesting that there might be some misattribution, in line with findings of Romero et al. (2017).

5.4 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 priors about the politician in office (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.

The 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 that did not take part on the IDEB in 2007, but that did participate in *Prova Brasil* in 2009. We then use the 2009 data on parents' education as if it was the 2007 cohort assuming that students' background is similar for students in these two cohorts.

We group schools according to the proportion of parents that have completed high school (we split them in terciles). In Table 7 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 7 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.

Overall, we find evidence that voters do respond to information about the delivery of public education in their local community. The magnitude of this response, however, is arguably small. The largest estimate we find is an approximate 2 percentage point increase in the vote-share of the incumbent running for reelection when the school at which citizens cast their vote is in the top 20% performers in our sample. This response represents about 4% of the mean proportion of votes received by the incumbent in control group polling stations in the baseline 2004 election.

6 Robustness checks

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.

First, note that many schools in our control group actually received an IDEB score in 2009 and/or 2011. Before 2009 only urban schools were eligible to participate in the IDEB evaluation. Therefore, 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 8 that our results remain unchanged from making such sample restriction.

Second, we address the concern that schools that did receive an IDEB score are, on average, larger than those that did not receive such measure of performance before 2008. We restrict the size of schools in our sample by removing the ones with less than 50 and more than 300 students on 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 9. We find results that are qualitatively the same as the estimates from our main specification. In columns 1 and 4 of Table 9 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 9 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.

7 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 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. To avoid comparing candidates that are different in non-observable dimensions, we look only at municipalities in which the same politician runs for office in both the baseline 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 in about 1.33 percentage points after the information release. 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.

While our results suggest that many voters update their beliefs about the mayor and change their votes, the magnitude of our estimates suggest a limited role of school quality information to make politicians more accountable. Based on our estimates, we conclude that information that is released from a school accountability system may not be enough to allow voters to select better politicians and make politicians accountable at the polls for their educational policies.

In part this can be due to difficulties of low-educated voters to process news on school quality indexes or to 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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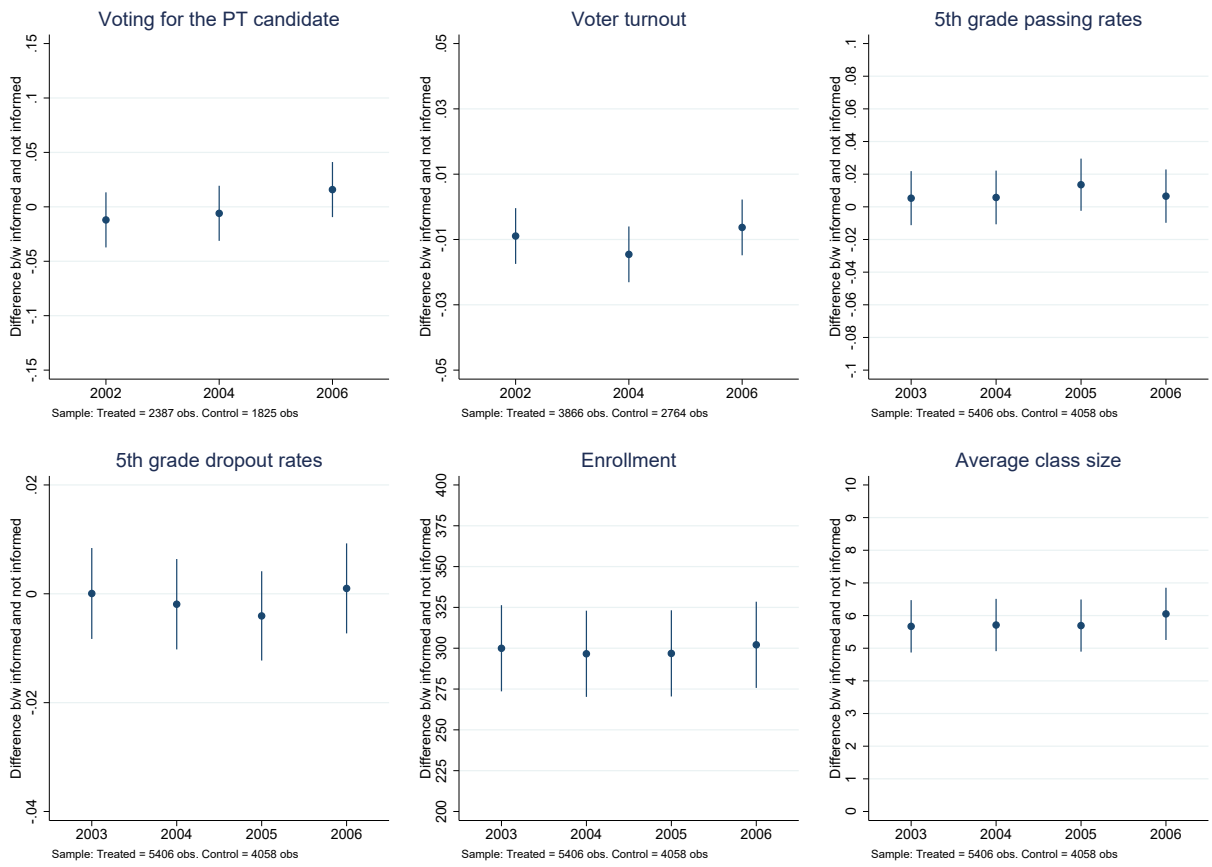

Figure 1: Pre-treatment trends: Electoral and school outcomes

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.

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	240.34	197.53	6.00	1,695.00
Dropout rates	0.06	0.12	0.00	8.44
Passing rates	0.82	0.22	0.00	8.50
Voters' characteristics				
% Age ≤ 24	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
% Middle school dropout	0.73	0.15	0.09	0.99
% High school dropout	0.19	0.09	0.00	0.52
% High school graduate	0.08	0.08	0.00	0.72
% votes for the incumbent	51.20	13.86	1.39	100.00
Incumbents' characteristics				
% 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
% Middle school dropout	0.07	0.26	0.00	1.00
% High school dropout	0.09	0.28	0.00	1.00
% High school graduate	0.28	0.45	0.00	1.00
% Bachelor's degree	0.55	0.50	0.00	1.00

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: Characteristics of polling stations in treatment and control groups

	(1)		(2)		(3)	(4)		(5)		(6)
	Rural					Urban				
	Control		Treatment		Diff	Control		Treatment		Diff
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
School characteristics										
Teacher's room	0.31	0.46	0.40	0.49	-0.09*	0.43	0.50	0.74	0.44	-0.31***
Computer lab	0.07	0.25	0.13	0.34	-0.06**	0.10	0.31	0.37	0.48	-0.26***
Science lab	0.02	0.13	0.02	0.14	-0.00	0.01	0.10	0.05	0.23	-0.04***
Library	0.22	0.41	0.31	0.47	-0.10**	0.31	0.46	0.50	0.50	-0.19***
Number of computers	0.56	1.82	0.93	2.24	-0.37*	1.04	2.86	3.74	6.21	-2.70***
Number of teachers	8.61	6.35	13.87	8.16	-5.25***	7.75	5.41	20.62	13.66	-12.87***
Enrollment (1-5 grades)	110.23	83.08	216.70	128.01	-106.47***	105.57	76.08	339.64	203.12	-234.08***
Average class size	22.06	9.15	26.50	8.42	-4.44***	18.29	7.98	24.66	7.69	-6.36***
Dropout rate 5th grade	0.07	0.10	0.07	0.08	0.00	0.05	0.09	0.05	0.08	0.00
Passing rate 5th grade	0.77	0.19	0.77	0.14	0.00	0.80	0.18	0.81	0.15	-0.01
Voters' characteristics										
Age < 25	0.24	0.07	0.25	0.10	-0.00	0.22	0.08	0.24	0.12	-0.02**
$24 <$ Age < 60	0.61	0.06	0.61	0.08	-0.00	0.63	0.07	0.64	0.09	-0.00
Age ≥ 60	0.15	0.05	0.14	0.05	0.00	0.15	0.05	0.13	0.06	0.02***
% Male	0.52	0.03	0.50	0.03	0.02***	0.51	0.04	0.49	0.03	0.02***
Middle school dropout	0.82	0.09	0.79	0.10	0.03***	0.72	0.14	0.66	0.15	0.06***
High school dropout	0.14	0.07	0.15	0.07	-0.02**	0.20	0.08	0.23	0.08	-0.03***
High school graduate	0.04	0.04	0.05	0.04	-0.01***	0.08	0.07	0.11	0.09	-0.03***
Electoral outcomes										
% votes PT 2006	0.64	0.21	0.65	0.18	-0.02	0.55	0.20	0.55	0.18	0.01
% votes PSDB 2006	0.33	0.20	0.31	0.18	0.02	0.39	0.19	0.39	0.17	-0.00
Turnout in 2006	0.82	0.07	0.80	0.07	0.02***	0.83	0.07	0.82	0.06	0.00
Observations	3797		151		3948	307		5427		5734

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.

Table 3: The impact of information about the quality of public schools on the vote-share of the incumbent mayor running for reelection

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: The impact of information about the quality of public schools on the vote-share of the incumbent mayor running for reelection: other measures of perfomance

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 5: How voters' priors relate to the effect of informing them about school quality on the vote-share of the incumbent running for reelection

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***	1.05**	1.59***	1.02***
	(0.45)	(0.49)	(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)			
Informex x Predicted Quality Index 2007		0.49		
		(0.58)		
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.

Table 6: Are incumbent mayors also held accountable for the performance of schools when they are not managed by them? Assessing mis-attribution.

Dependent variable: % votes (0-100)	(1)	(2)	(3)	(4)	(5)
		Placebo w/ state schools			
Informed	0.11	-0.18	-0.18	0.34	0.17
	(0.99)	(0.75)	(0.75)	(0.80)	(0.80)
Informed x Quality Index 2007	1.40***		0.90**		
	(0.45)		(0.36)		
Informed x Quality Index 2005	0.00				
	(0.44)				
Informed x (Quality Index 2007 in Top 20%)				-0.48	
				(0.95)	
Informed x (Quality Index 2007 in Bottom 20%)				-2.16**	
				(0.93)	
Informed x (Quality Index 2007 in State Top 20%)					-0.36
					(0.95)
Informed x (Quality Index 2007 in State Bottom 20%)					-1.40
					(0.92)
Mean % votes in the control group in 2004	51.3	51.2	51.2	51.2	51.2
Obs informed	5578	2597	2597	2597	2597
Obs not informed	4104	766	766	766	766
Post x Rural	Yes	No	No	No	No

Notes: We examine if there is mis-attribution in the process of holding incumbent mayors accountable for the quality of public schools. In the results reported in column 1, we assess whether, conditional on 2007 performance, voters hold the incumbent mayor accountable for the 2005 Quality Index results. Since 2005 standardized tests were taken by students approximately one year after the incumbent took office, it is arguable that these results do not reflect her ability as manager of the municipal public school system. In columns 2-6, we show the impact of information about the quality of state schools on the vote-share of the incumbent, since state schools are not managed by local governments. To give the best shot of each coefficient being statistically different than zero, non-robust standard errors are reported in parenthesis in columns 2-6. In column 1, standard errors are clustered at the school level. We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100. The variable Quality Index 2007 is normalized to have zero mean and a one-point standard deviation in the relevant sample. Top and bottom performers are obtained from ranking the quality index of schools in our treated sample. State top/bottom performers are obtained by ranking schools in the state, irrespective of being or not in our final sample.

Table 7: Heterogeneous effects 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 8: 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

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.

Table 9: 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 Appendix

A.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 thousand 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[13]. 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[14]. From the 37 thousand polling stations that we consider as being located at schools in municipalities included in our sample, about 23 thousand receive a score of one, which indicates a perfect match.

[13] 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.

[14] 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

A.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[15]. 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 10 that they are similar. The p-values we report on Table 10 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[16]. We report the results of this mean comparison for the characteristics of voters at each polling station in Table 11, 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 12. 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 three last rows of Table 12 show that, in terms of average class size

[15] 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).

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

Table 10: Comparing cities included and excluded from the final sample

	(1)		(2)		(3)
	Excluded from sample		Included in sample		Diff
	Mean	SD	Mean	SD	
City characteristics					
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	0.03	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
Incumbents' characteristics					
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	45.41	9.05	46.32	9.56	0.91*
Observations	758		2270		3028

Notes: This table compares cities included and excluded from our final sample in the process of linking the administrative data from schools to that of polling stations. Characteristics of cities come from the 2000 Demographic Census. The education variables for the city are defined as the proportion of citizens with a certain school level divided by the total population, which is why they do not sum 1. Characteristics of incumbents come from the TSE registry of candidates for the 2004 election.

and educational outcomes such as passing rates and dropout rates, schools included or not in our final sample are not too different.

Table 11: Comparing polling stations included and excluded from the final sample

	(1)		(2)		(3)
	Excluded from sample		Included in sample		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

Notes: This table compares characteristics of voters in polling stations included and excluded from our analysis. Polling stations that are schools, but are outside of our sample fall into one of the following categories: i) Were matched to a state public school or a private one; ii) Were not matched to any school; iii) The school they were matched to has an IDEB grade for 2005, but not for 2007 or vice-versa. Data of the characteristics of voters in each polling station comes from TSE.

Table 12: Comparison of municipal schools included and excluded from our final sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Excluded	Included	Diff	Excluded	Included	Diff
	Mean	Mean		Mean	Mean	
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

Notes: Municipal schools that are out of our sample are the ones that are not matched to polling stations. We restrict the school included in this comparison according to the same criteria schools must fit to enter our final sample. They must be active both in 2004 and 2008 and have students enrolled in 5th grade in both years.