Pontifícia Universidade Católica
do Rio de Janeiro

Marina Villas Boas Dias

Does Information on School Quality Affect Voting? Evidence from Brazil

Dissertação de Mestrado

Thesis presented to the Programa de Pós-graduação em Economia of PUC-Rio in partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor: Prof. Claudio Ferraz

Rio de Janeiro
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Prof. Claudio Ferraz
Advisor
PUC-Rio –

Prof. Gabriel Ulyssea
Departamento de Economia – PUC-Rio

Prof. Vladimir Ponczek
Escola de Economia de São Paulo – FGV-SP

Prof. Monica Herz
Graduate Coordinator
Centro de Ciências Sociais – PUC-Rio

Rio de Janeiro, March 30, 2017

Marina Villas Boas Dias

B.A., Economics, Universidade de Brasília (UnB), 2013.

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Abstract

Dias, Marina Villas Boas; Ferraz, Claudio (Advisor). **Does Information on School Quality Affect Voting? Evidence from Brazil**. Rio de Janeiro, 2017. 66p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This paper examines if voters act upon information about the quality of public service delivery. We explore a natural experiment in Brazil, which provided an objective measure of quality for some public schools, but not others. This creates variation in the availability of information to voters about the quality of schools inside a municipality. To use this variation, we look at polling stations located in municipal schools and compare electoral outcomes in mayoral elections in informed and non-informed groups of voters, before and after the information release. We find that, when the information received by voters is good news, the support for the incumbent increases. For the worst performers in our sample, providing information about school quality implies a decrease in the vote-share of the incumbent. We find that these effects are stronger in municipalities without local radios and/or newspapers. We do not find relevant heterogeneities when decomposing our effects according to schooling.

Keywords

Electoral accountability; School quality; Municipal elections;

Resumo

Dias, Marina Villas Boas; Ferraz, Claudio. **A informação sobre qualidade das escolas afeta resultados eleitorais? Resultados do Brasil**. Rio de Janeiro, 2017. 66p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Esse artigo examine se eleitores reagem a informação sobre a qualidade dos serviços públicos. Exploramos um experimento natural no Brasil, no qual divulgou-se uma medida objetiva de qualidade para algumas escolas públicas, mas não outras. Isso cria variação na quantidade de informação disponível dentro de um município. Para explorar essa variação, olhamos para locais de votação localizados em escolas municipais e comparamos resultados eleitorais em eleições municipais em grupos de eleitores informados e não informados, antes e depois da divulgação de informação. Os resultados indicam que, quando os eleitores recebem boas notícias, o apoio pelo incumbente aumenta. Para as escolas com pior desempenho em nossa amostra, divulgar informação sobre qualidade das escolas leva a uma redução na proporção de votos recebida pelo prefeito incumbente concorrendo a reeleição. Esses efeitos são maiores em municípios com menor cobertura de mídia local. Não há heterogeneidades interessantes com relação a escolaridade dos eleitores.

Palavras-chave

Accountability eleitoral; Qualidade das escolas; Eleições municipais;

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1
Introduction

Information is key for allowing voters to make politicians accountable for their actions and to select better candidates (1, 2). Many politicians get away with acting against the public interest when voters are poorly informed about their performance (3). This is especially true in developing countries where sources of information are scarce and independent media is not widespread (4). Thus, many theories predict that informing voters about the quality of public services should affect their choices (5). Whether voters will act upon being informed and how they will act depend not only on the availability of information, but on their previous beliefs and their preferences towards public service delivery.

In this paper, we examine if voters react to new information about the quality of public services. We study this in the context of public primary education provision, by exploring a natural experiment that made information about school quality available for some groups of voters, but not others. In 2005, Brazil introduced an accountability system to evaluate the performance of schools through standardized tests of Portuguese and Mathematics applied to students in the last grades of elementary, middle and high school. In 2007, the Ministry of Education created an objective measure of school performance called *Índice de Desenvolvimento da Educação Básica* (IDEB) and made the scores for schools and cities publicly available. But not every public school received an IDEB score.

Schools that were located in rural areas, had less than twenty students enrolled in the evaluated grade, or had less than ten of its students present on the standardized tests did not receive an IDEB score. Two factors generate variation in the availability of information about the quality of public education delivery. We look at the performance of schools run by local governments and relate this to the electoral outcomes of local politicians. First, we consider a framework in which citizens close to a school that is graded with an objective measure of performance are more exposed to this information shock. In this case, they pay attention to the average performance of the city to extract a signal about the quality of the mayor currently holding office, by looking at municipal IDEB scores. Second, we consider the case in which voters actually

use the scores of individual schools to update their priors about the incumbent politician. This may be especially appropriate if there is yardstick competition between local communities.

To explore these sources of variation, we look at polling stations located at schools to examine accountability at a local level. We use a difference-in-differences strategy to compare the proportion of votes received by the incumbent mayor running for reelection before and after the release of IDEB scores for informed and non-informed groups of voters. To avoid comparing candidates who are different in non-observable dimensions, our sample includes only municipalities in which the candidate runs in both the baseline and post-treatment elections.

We look at accountability at the local community level because citizens may have a better perception of the quality of services delivered near them. In fact, some experimental interventions are designed to induce improvements in service delivery through greater engagement and demand for accountability from citizens nearby (6). (4) argue that, without putting pressure on the politicians responsible for managing these services, participation may be too costly to citizens, and may fade away over time. We test if informing voters about the quality of the services delivered near them can create this demand for electoral accountability.

We find that there are heterogeneities depending on the type of information voters receive. In polling stations located at schools that perform well the proportion of votes of the incumbent increases between 0.4 and 1.9 p.p. (1-4%), with the release of information about school quality. We use different definitions of performance: i) the raw quality index; ii) meeting the target assigned to the school; iii) being in the top quintile of performance in our sample; iv) being above the median performance of the state where the school is located. For polling stations located at schools in the bottom quintile of performance in our sample in 2007, providing citizens with information about the delivery of public education leads to a decrease of about 2 percentage points (4.5%) on the vote-share of the incumbent.

These findings suggest that the information about school quality induces few voters to change their voting patterns. There are a few hypothesis for the small magnitude of our results. First, many voters may not care about public education delivery. This does not seem to be the case in Brazil. In the Latinobarómetro 2008 survey, 11% of respondents point out education problems as the most important problem in the country, behind only health problems (18.9%), unemployment (16.7%) and violence (12%). Still, we test if, in cities where people are more educated, our effects are different than the

effect we find, on average, for the entire sample. A second hypothesis is that voters can't process the information about school performance. In this case, we would expect that more educated citizens understand better the information and have a differentiated response to the news they receive. Finally, it is also possible that voters do not receive the information about IDEB scores. Thus, we decompose our effects according to media coverage. We do not find relevant heterogeneities in the effect of informing voters about the quality of public schools when decomposing our results by media coverage and citizens' schooling.

Another reason that might explain why voters do not respond strongly to the information about the quality of schools is that citizens may not know to whom attribute the responsibility for managing them. As a matter of fact, there is evidence that communities are often uninformed of what public services they are entitled to and what controls they have over these services (7, 8). (9) highlights that one of the contributions of his work is to acknowledge that attribution matters for accountability. The IDEB scores inform voters about the quality of schools, but does not inform them about who is responsible to manage these schools. Thus, verifiability of an outcome is not enough to make electoral accountability work (10).

These findings are related to two literatures in the political economy of development. First, this project relates to a literature about information and public service delivery. It examines, for instance, whether citizen report cards can induce monitoring and the demand for accountability (11, 6, 12). This literature's main focus is on what (13) 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, we relate to the electoral accountability literature. Some studies test if politicians are held accountable for voters' access to public goods and services (9, 14). 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 (15, 16, 17). Other studies look at informing voters of characteristics of the candidates disputing an election (18, 19). We know little about voters' reaction to positive and negative information about the quality of public services.

This work relates directly to two papers that use the same natural experiment in Brazil to investigate the effects of providing voters information on school quality. (20) compare electoral outcomes before and after the release

of the index of school quality at the municipal level. They find a positive effect of improvements in performance in the probability of reelection of the incumbent mayor. Since the comparison is among different municipalities, they lack a control group and thus face identification challenges that we address here. (21) also compares electoral outcomes in different municipalities, but he explores the fact that each municipality receives a target from 2007 onwards and looks at the discontinuity in electoral outcomes around the target's cutoff. Thus, the effect he estimates is that of signaling quality to voters. He finds that meeting the municipal target does not have an effect on the incumbent mayor's support. It may be the case that the city's performance as a whole or the event of meeting or not a target of quality is not exactly the piece of information voters extract from the evaluation of public schools. We add to this evidence by looking at information on school quality at a more disaggregated level than the municipality and by testing different dimensions of performance that voters may care about.

2
Empirical setting

2.1
Institutional background

2.1.1
Public education in Brazil

In Brazil, federal, state, and municipal governments are responsible for the delivery of public education. The system is divided in elementary, middle, and high school. Regarding primary education, municipalities are the main providers of elementary school, and they share with state governments the responsibility for managing middle schools. In fact, municipalities are the main providers of public primary education in Brazil. According to the 2004 School Census, which is the year of our pre-treatment election, 82% of public schools were ran by local governments. This means that 73% of students enrolled in public schools in 2004 studied at a municipal school. The rules that define the allocation of students into different municipal schools are defined by each local government. In some cities students are sorted according to where they live, in others they may choose which school they want to attend, for instance.

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 were administered to four grades of primary schools located in urban areas. In 1995, the standardized tests 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, and the characteristics of the tests themselves, made it difficult to compare the performance of schools over time.

In 2005, the system changed to evaluate all public schools located in urban areas. Two years after students took these tests, an objective measure of school quality, called *Índice de Desenvolvimento da Educação Básica*, (IDEB) was released, relative to each public schools' performance in 2005. We call

this IDEB 2005. Editions of IDEB occur every two years and every school received targets proposed by the Ministry of Education, which established a performance target from 2007 to 2021. These targets were defined with the final goal of reaching an average IDEB grade of 6 for the entire country in 2021. Schools with worse performance have goals that require a greater improvement, since the objective is also to reduce the inequality in the quality of schools.

The IDEB is computed by combining the performance on standardized tests with passing rates, according to the following equation[1]:

$$\text{IDEB}_{ji} = \text{N}_{ji} \cdot \text{P}_{ji} \tag{2-1}$$

in which N_{ji} is the average of the proficiency scores in Mathematics and Portuguese standardized to a value between 0 and 10 for institution j in year i and P_{ji} is the average percentage of children that advance from one grade to the next in that education cycle.

Not all public schools received an IDEB grade in the 2005 and 2007 editions of the evaluation. There are some eligibility criteria that a school had to fit in order to have an IDEB score released. Not having this grade usually occurs for the following reasons: i) not enough students are enrolled in the relevant grade (the cutoff is 20 students); ii) it is a rural school; iii) fewer than ten students took the standardized tests in Portuguese and Mathematics for the relevant grade. We explore the fact that not all schools have an IDEB grade to construct groups that receive and do not receive information about school quality because of the implementation of this accountability system.

When the IDEB grades 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[2]. 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 display their IDEB grade in a place of large visibility, but this is not a general rule across the country.

2.1.2

[1]Source: Instituto Nacional de Estudos e Pesquisas Educacionais (INEP)

[2]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.

Municipal elections in Brazil

Mayors are ultimately responsible for the quality of public primary schools. By law, politicians are only allowed to be reelected for positions in the executive power once. Therefore, they can only serve two consecutive terms as mayors. Because the natural experiment we explore made information about school quality publicly available in 2007, our sample comprises municipalities in which the mayor was elected for a first term in 2004 and ran for reelection in 2008.

The results of two editions of IDEB were released before the 2008 elections, which are relative to schools' performance in 2005 and 2007. The results of the 2005 edition of IDEB were released in 2007, and those of the 2007 edition were released in July 2008, three months before the municipal elections. Therefore, we use the 2004 and 2008 elections as pre and post-treatment periods, respectively.

The official political campaigns in the elections we study in this paper begun 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[3]. 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 outdoors, distribute fliers and visit local communities to speak directly to voters. With the release of IDEB 2007 scores so close to the elections, it is likely that this information was not only made public by the media, but also by incumbents and their opponents in their campaign adds.

2.2 Data and sample

We put together a unique dataset which links electoral data from polling stations to the administrative data from municipal schools where they 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, respectively. From INEP we have data on schools' names, characteristics and IDEB scores. From TSE we have data on electoral outcomes and the characteristics of politicians and voters.

[3](22) find a large effect of this type of TV advertising on electoral outcomes in Brazil.

We match electoral data from polling stations to the administrative data from the schools where they are located. First, we break the universe of polling stations in the 2008 elections in two based on being or not 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 Municial 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[4].

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 criteria to be in our final sample. 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. We believe our hypothesis of local accountability for elementary schools is more straightforward, and a larger of proportion of public schools are administered by municipal governments. The data used to determine which schools are in our final sample comes from the 2004 and 2008 School Census.

In the process of matching schools to polling stations and restricting the sample to the pairs school-polling station that make sense in our framework, we end up losing some municipalities that, *a priori*, should be represented in our 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

[4]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.

number of municipalities in our final sample is 2324. The reasons why we loose some cities from the matching procedure to our final sample are that all polling stations at one particular city may be matched only to private or public state schools, and that the number identifying polling stations may change between 2004 and 2008 and we are unable to match them to electoral results[5].

To check if cities that are left out of our analysis are different from those included in our final sample, we compare them in terms of their characteristics and those of the incumbents running for reelection. We show on Table 2.1 that they are similar. The p-values we report on Table 2.1 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.

We also compare the characteristics of polling stations and municipal schools that we leave out of our final sample[6]. We report the results of this mean comparison for the characteristics of voters at each polling station on Table 2.2, and we conclude that citizens in both groups are similar. 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 once again small. These differences are larger for the education variables we report on the last three lines of Table 2.2. Voters in our sample are, on average, less educated than voters left out of our sample. One explanation for this is that voters in rural areas are over-represented in our empirical setting. This comes from it being more likely for urban areas to have people voting in polling stations that are not municipal schools, because of the higher population density in these areas.

Finally, we show the comparison of schools in and outside our final sample on Table 2.3. 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 2.3 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.

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. Thus, the control or not informed group comprises polling stations linked to municipal schools that did

[5]We have to use polling station names only from 2008, because TSE does not have this dataset for earlier elections.

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

not receive an index of school quality. The treatment or informed group comprises polling stations linked to schools that had a measure of performance made public in 2005 and 2007[7]

On Table 2.4 we show characteristics of polling stations in terms of the schools where they are located and the people who vote there. The infrastructure of schools strikes as far from ideal. 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. On Table 2.4 we also show that the average school in our sample is not very large, both in number of teachers and in enrollment. Dropout rates are not very large, but the average passing rates suggests that a relevant number of students are retained on 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, there is one important detail to be kept in mind. The data we have about the characteristics of voters is obtained when they register to vote, which usually occurs when citizens are about eighteen years old, because this is the age for which voting becomes mandatory in Brazil. This explains why the mean proportion of high school graduates is so low in the last row of Table 2.4.

Finally, we show on Table 2.5 summary statistics for the IDEB scores of treated schools in our sample. There is a slight improvement on the performance of schools between 2005 and 2007. This is true not only for the average performance, but also for percentiles 25 and 75 of the score distribution. The range and variation of the index remain almost unchanged over time.

To investigate heterogeneities in the effects, we also use data from the 2010 Census and from the 2006 culture supplement of a survey named MUNIC, which profiles Brazilian municipalities. Both data sets come from the Brazilian Institute of Geography and Statistics (IBGE). We use the 2010 Census to get data of the proportion of high school graduates, the median household per capita income and the proportion of households connected to the Internet in each municipality. We use the culture supplement of MUNIC to get data about the presence of local radio stations in the city.

2.3

[7]Since we use the grades from both editions of IDEB in the regressions, to keep the number of observations constant among different specifications, we discard about 1 thousand schools in the treatment group that have an IDEB grade only in 2005 or 2007. This does not alter our results.

Empirical Strategy

We explore the institutional characteristics of the accountability of public education provision in Brazil as a natural experiment that allows us to identify the causal effect of providing information regarding the quality of public schools on voter behavior. Because of the nature of this information release, we are able to define treatment and control groups, which we name informed and non-informed groups of voters. 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.

First, we look at the effect of the disclosure of information about the quality of schools on voting using the following equation:

$$\%\text{votes}_{it} = \alpha + \beta_1 \text{info}_{it} + c_i + \lambda_t + \epsilon_{it} \tag{2-2}$$

in which $\%\text{votes}_{it}$ is the proportion of votes received by the mayor at the polling station i on election t and info_{it} is a dummy that indicates whether the voters at polling station i were informed about the quality of the public school nearby prior to election t. We include time fixed effects and also fixed effects at the level of the polling station, to control for aggregate effects and for unobserved characteristics which are constant over time.

The identification of β_1 relies on two identifying 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 ideal 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 in municipal and presidential elections from 1998 to 2006. We focus on the PT party because it had a candidate running in all the presidential elections. The two municipal elections we consider were held in 2000 and 2004. The presidential elections were held in 1998, 2002 and 2006. In order to increase the number of observations plotted in our pre-trends figures, when we look at the 2000 and 2004 municipal elections, a vote for the PT candidate actually means 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 IDEB.

We plot both pre-treatment trends on Figure 2.1. The trends shown on Figure 2.1 are similar, although not exactly parallel. Since we cannot test directly our first identification assumption, we also look at how the outcomes of schools in our treatment and control groups vary over time prior to the release of IDEB. This complements the trends presented on Figure 2.1 in the sense that we are checking if differences in outcomes of schools in the treatment and control groups could generate an effect on voting that confounds with the response we measure in this paper. Despite schools in each group being different in terms of size and infrastructure, their outcomes follow similar trends up to 2006[8].

We also compare the mean characteristics of schools, voters and electoral outcomes in treatment and control groups. We show the result of this comparison on Table 2.6. 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 2.6 shows that the infrastructure in schools that have an IDEB grade before 2008 is better than in schools that do not receive such grade. 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 2.6 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 grade before 2008.

In the second part of Table 2.6 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. In fact, 93% of schools of the control group are rural, while this proportion falls to 1% in the treatment group. The dataset that contains voters' characteristics is from the 2008 municipal elections. Ideally, we would like to know the difference between voters in treatment and control groups prior to the release

[8]For some years the hypothesis that trends are parallel is rejected at a 1% level, but the magnitude of the coefficients associated to the interaction of the year dummies with the treatment variable does not exceed 2 percentage points for the outcome passing rates and 0.7 p.p for the outcome dropout rates.

of IDEB, but we do not have such data.

The bottom of Table 2.6 shows that electoral outcomes are also different in polling stations located at treated schools. The difference in voter turnout between groups is small. However, we do observe in Table 2.6 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. Although the statistics we present on Table 2.6 indicate that our treatment and control groups are not statistically similar on level, this is not crucial to our identification strategy, which we discuss next.

While equation 2-2 estimates an average effect, the reaction of voters might depend on the type of information provided and their priors on the quality of public schools. Following (15), we interact the treatment variable info_{it} with a measure of performance, which leads to the estimation of the following equation:

$$\%\text{votes}_{it} = \beta_1 \text{performance}_i \cdot \text{info}_{it} + \beta_2 \text{info}_{it} + c_i + \lambda_t + \nu_{it} \qquad (2\text{-}3)$$

It is not obvious to us what would be *a priori* the best specification for the variable $performance_i$ on equation 2-3. (23) highlights that there is no clear evidence of which information about school quality is more effective, raw test scores or measures of value-added. (24) argues that providing citizens with information about performance is not enough, it is also necessary to provide them with reference points. She suggests that these reference points include general or relative performance standards.

We use as measure of performance the IDEB 2007 score, which is the raw measure of performance. We are unable to measure value added with our data, but we consider performance relative reference points. We treat the IDEB 2007 target assigned to each school as a general performance standard, and look at the effect of meeting or not the target. The relative performance point we consider is the score of other schools in the state where the polling station is located. We also look at top and bottom performers in the sample as a relative measure of performance. Finally, we use the 2005 score as a reference that anchors expectations, and thus test if the effect of information about school performance on the electoral outcomes of the incumbent varies according to voters' priors.

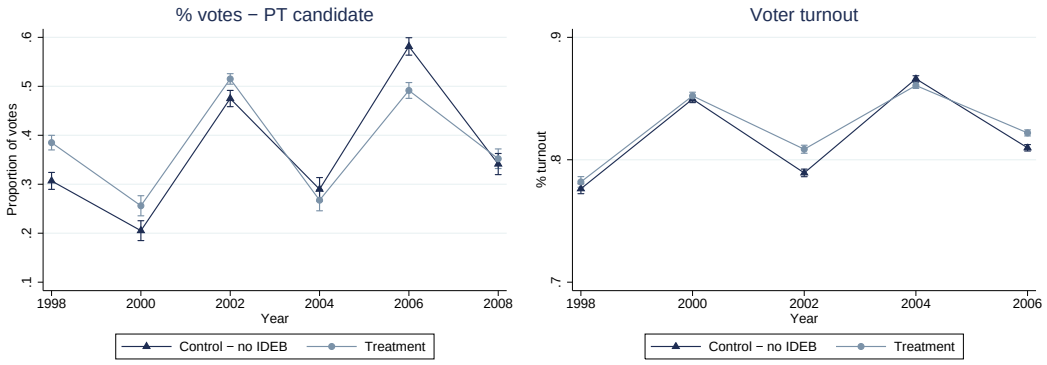

Figure 2.1: Pre-treatment trends: Electoral outcomes

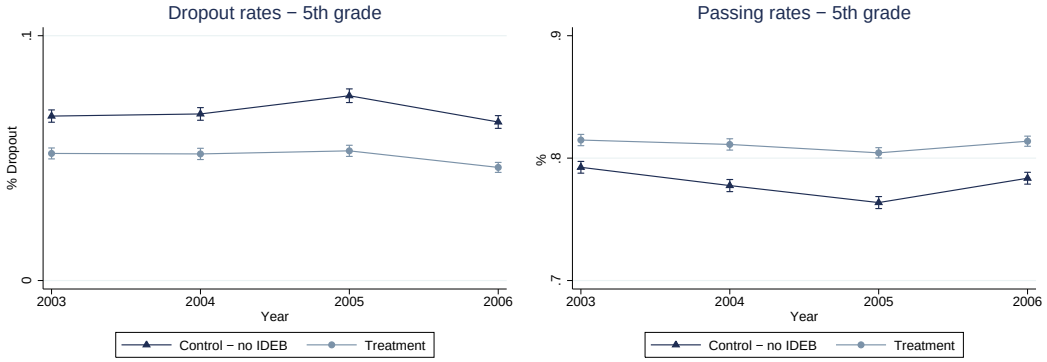

Figure 2.2: Pre-treatment trends: Schools' outcomes

Table 2.1: Comparing cities included and excluded from the final sample

	Included Mean	Excluded Mean	Pval
Characteristics of the city			
People in school	0.311	0.302	0.000
People in public school	0.288	0.285	0.192
People in school age	0.175	0.171	0.000
Male	0.507	0.512	0.000
Married	0.304	0.336	0.000
Middle school dropouts	0.309	0.307	0.464
High school dropouts	0.099	0.110	0.000
High school graduates	0.076	0.078	0.193
Median h.h per capita income	109.879	116.584	0.005
Characteristics of the incumbent			
Male	0.920	0.910	0.358
Married	0.873	0.870	0.911
Middle school dropouts	0.113	0.142	0.034
High school dropouts	0.110	0.134	0.090
High school graduates	0.334	0.350	0.404
College graduates	0.440	0.371	0.001
Age	46.170	45.856	0.437

Notes: Number of observations included in the final sample is 2311 and excluded is 719. 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.

Table 2.2: Comparing polling stations included and excluded from the final sample

	Not in sample		In sample		
	Mean	Sd	Mean	Sd	Pval
Number of voters	1,362.14	1,455.84	952.68	1,061.15	0.000
Age < 24	0.23	0.12	0.24	0.09	0.000
$24 <$ Age < 60	0.62	0.09	0.62	0.07	0.000
Age > 60	0.14	0.07	0.14	0.06	0.002
Male	0.50	0.05	0.51	0.04	0.000
Middle school dropouts	0.70	0.18	0.77	0.15	0.000
High school dropouts	0.20	0.10	0.16	0.09	0.000
High school graduates	0.10	0.10	0.06	0.07	0.000

Notes: Number of observations not in sample is 23965 and in sample is 13032. 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 2.3: Comparison of municipal schools included and excluded from our final sample

	Without IDEB					With IDEB				
	Not in sample		In sample			Not in sample		In sample		
	Mean	Obs	Mean	Obs	P-value	Mean	Obs	Mean	Obs	P-value
Rural	0.96	40,408	0.93	8,404	0.00	0.05	6,561	0.01	4,484	0.00
Teacher's room	0.07	40,389	0.20	8,401	0.00	0.61	6,556	0.77	4,483	0.00
Computer lab	0.01	40,387	0.04	8,401	0.00	0.26	6,554	0.38	4,480	0.00
Science lab	0.00	40,387	0.01	8,401	0.00	0.04	6,554	0.06	4,479	0.00
Library	0.04	37,721	0.14	7,818	0.00	0.36	6,382	0.51	4,384	0.00
Number of computers	0.06	40,408	0.33	8,404	0.00	2.31	6,561	3.98	4,484	0.00
Number of teachers	2.34	40,408	5.77	8,404	0.00	14.59	6,561	21.85	4,484	0.00
Enrollment in elementary grades	41.26	40,408	77.68	8,404	0.00	260.81	6,561	365.67	4,484	0.00
Average class size in primary school	20.92	40,408	21.09	8,404	0.15	24.55	6,561	25.29	4,484	0.00
Passing rate in 5th grade	0.79	33,780	0.82	7,744	0.00	0.83	6,048	0.83	4,406	0.13
Dropout rate in 5th grade	0.10	33,780	0.07	7,744	0.00	0.05	6,048	0.05	4,406	0.04

Notes: Municipal schools that are out of our sample fall into one of the following categories: i) Are not matched to any polling station; ii) Have and IDEB grade for 2005, but not for 2007, or vice-versa.

Table 2.4: Summary statistics

	Mean	Sd	Min	Max
School characteristics				
Teacher's room	0.40	0.49	0.00	1.00
Computer lab	0.16	0.37	0.00	1.00
Science lab	0.03	0.16	0.00	1.00
Library	0.27	0.45	0.00	1.00
N Computers	1.59	4.35	0.00	63.00
N teachers	11.40	12.13	1.00	216.00
Enrollment - El. School	178.13	189.15	2.00	1,695.00
Dropout rates - 5th grade	0.06	0.13	0.00	8.44
Passing rates - 5th grade	0.82	0.26	0.00	8.67
Voters' characteristics				
Age $\leq$ 24	0.24	0.09	0.00	0.78
24< Age < 60	0.62	0.07	0.20	0.93
Age > 60	0.14	0.06	0.00	0.46
Male	0.51	0.04	0.32	0.74
Middle school dropout	0.77	0.15	0.09	1.00
High school dropout	0.16	0.09	0.00	0.52
High school graduate	0.06	0.07	0.00	0.72

Notes: Number of observations is 13032, with some exceptions: teachers' room (13028), computer lab (13025), science lab (13024), library (12339), dropout rates (12285), and passing rates (11370). 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.

Table 2.5: Summary statistics of the Quality Index for treated schools included in our sample

	Quality Index 2005	Quality Index 2007
Mean	3.62	3.97
Std. Dev.	0.97	0.96
Min	0.10	1.10
Max	7.30	7.90
25th percentile	2.90	3.20
Median	3.60	4.00
75th percentile	4.30	4.70
Number of observations	4,550.00	4,550.00

Table 2.6: Characteristics of polling stations in treatment and control groups

	Control		Treatment		
	Mean	Obs	Mean	Obs	Pval
School characteristics					
Teacher's room	0.20	8,479	0.77	4,549	0.00
Computer lab	0.04	8,479	0.38	4,546	0.00
Science lab	0.01	8,479	0.06	4,545	0.00
Library	0.14	7,890	0.51	4,449	0.00
N Computers	0.33	8,482	3.95	4,550	0.00
N teachers	5.77	8,482	21.91	4,550	0.00
Enrollment - El. School	77.68	8,482	365.38	4,550	0.00
Avg class size	26.90	6,022	29.22	4,550	0.00
Dropout rates - 5th grade	0.07	7,814	0.05	4,471	0.00
Passing rates - 5th grade	0.78	7,221	0.81	4,149	0.00
Voters' characteristics					
Age $\leq$ 24	0.22	8,482	0.22	4,550	0.00
24< Age < 60	0.62	8,482	0.66	4,550	0.00
Age > 60	0.16	8,482	0.13	4,550	0.00
Male	0.52	8,482	0.48	4,550	0.00
Middle school dropout	0.84	8,482	0.67	4,550	0.00
High school dropout	0.12	8,482	0.22	4,550	0.00
High school graduate	0.04	8,482	0.11	4,550	0.00
Electoral outcomes					
% votes PT 2006	0.64	8,373	0.55	4,539	0.00
% votes PSDB 2006	0.32	8,348	0.39	4,539	0.00
% turnout 2006	0.82	8,373	0.82	4,539	0.00

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.

3
Results

3.1
Main results

In the theoretical model we have in mind, voters use the information about school quality to update their priors about the incumbent mayor. There are two ways we can think of equation 2-3. First, we can interact our treatment variable with measures of the average performance of the municipality. The intuition here is that, if voters are to update their prior about the incumbent mayors, they will consider the average quality of education this politician is providing. However, it is also possible that voters look only at the performance of the school near them. Examples for why this may occur are that they may distinguish better the signal component of IDEB scores from schools near them, or they may not receive the information of the average performance of all schools in their city. Therefore, we estimate equation 2-3 using measures of performance at the city and at the individual school levels.

3.1.1
The effects of information about school quality on the proportion of votes of the incumbent: city-level measures of performance

First, we present the results of the effect of informing voters about the quality of public education, that is, from the estimation of equation 2-2. We then present estimates of equation 2-3, in which we interact our treatment variable with different measures of the performance of the city on the IDEB evaluation, to see if the effect depends on whether the news about school quality received by voters is good or bad. The results we report in column 1 of Table 3.1 indicate that the effect of providing information about the delivery of public education on the incumbents' vote-share is, on average, 0.5 percentage points ($\approx 1\%$), which is small in magnitude and statistically equal to zero.

It seems natural to imagine that the response of voters to the information they receive about school quality depends on whether the school near them performs well or not in the evaluation. To account for this, we explore the fact that schools in our sample were evaluated twice between the 2004 and 2008 municipal elections to specify not only raw scores as measure of performance,

but to also use general and relative reference points as well as variation over time as indicators of school quality.

We first look at the IDEB 2007 grade as a measure of school performance, which was released only three months before the 2008 municipal elections, a period in which it is likely for voters to consume more news (25). In column 2 of Table 3.1 we show that a standard deviation increase on the average IDEB 2007 score of the city leads to an increase on the proportion of votes received by the incumbent of 1.32 percentage points ($\approx 2\%$). Although the IDEB grade is an objective measure of school quality, it is not obvious that voters should understand it. There are some dimensions of performance that may be more straightforward to interpret. First, we analyze if the incumbent has different electoral results when interacting our treatment variable with whether or not the school met its target for 2007, which we report in column 3 of Table 3.1. We find no effect on the vote-share of the incumbent either when the school performs well or when it performs badly.

We can also think that voters make yardstick comparisons between local communities to overcome political agency problems (26). Thus, we also test if relative performance matters to them. In this context, the intuition we have in mind is that the grades themselves may 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. We report the results when considering relative performance in columns 4 and 5 of Table 3.1. First, we test how voters respond to the release of IDEB scores when their city is among the best or worst performers in our sample. We find that, for cities that are among the top performers, the support for the incumbent at these polling stations does not change. When the average municipal school perform badly, the support for the incumbent decreases in 3.45 percentage points after the release of IDEB scores. The magnitude of the effect in this case is about 5%. Second, we find that receiving a score which is above the median of the state where the school is located has no effect on the proportion of votes of the incumbent running for reelection.

Overall, when looking at performance in light of the average score of the municipality on the IDEB evaluation, we find either non-response of voters to information about school quality or an effect over the vote-share of incumbents which is small in magnitude. One explanation for this may be that our assumption that voters should look at the quality index at the city level if they want to update their priors about the incumbent is wrong. Therefore, we test whether voters' response to information about school quality is different when looking at performance at the school level.

3.1.2
The effects of information about school quality on the proportion of votes of the incumbent: school-level measures of performance

In this section, we interact the treatment variable with measures of performance at the school level. We report the first interactions on Table 3.2. In column 1 of Table 3.2 we simply report once again the average effect of releasing information about school quality on the proportion of votes of the incumbent, which is approximately 1% and statistically equal to zero. Once again we first look at the IDEB 2007 grade as a measure of school performance. In column 2 of Table 3.2 we show that a standard deviation increase on the average IDEB 2007 score leads to an increase on the proportion of votes received by the incumbent of 1.4 percentage points ($\approx$ 2%). The effect of moving from the 25th percentile of IDEB 2007 to the 75th percentile, which is a 1.5σ increase on the IDEB 2007 score, is of about 3% the average of the control group on the baseline election.

Then, we look once again at general reference points and report these results on Table 3.2. We look at whether or not the school met its target for 2007 as a measure of performance relative to a general standard. We report in column 3 of Table 3.2 that meeting the quality index target implies an increase of 0.67 percentage points of the incumbents' vote-share, which is approximately 1% and statistically significant only at a 10% level. Missing the target has no effect on the proportion of votes of the incumbent.

We report the results when considering relative performance in columns 4 and 5 Table 3.2. First, we test how voters respond to the release of IDEB scores when the school near them is among the best or worst performers in our sample. We find that, for schools that are among the top performers, the support for the incumbent at these polling stations increases, on average, 1.87 percentage points, or about 3.5%. At polling stations located in schools that perform badly, the support for the incumbent decreases in 2.09 percentage points after the release of IDEB scores. The magnitude of the effect in this case is about 5%. Second, we find a 1.53 percentage point ($\approx$ 2%) increase in the proportion of votes the incumbent receives in his reelection attempt when the school where people vote receives a score which is above the median of the state where it is located.

3.1.3
Should city or school level performance matter?

We add performance at both the city and the school levels to our empirical equation to assess which of these dimensions matters more to voters.

We once again use the quality index for 2007 as measure of performance and report these results on Table 3.3. We show in column 2 of Table 3.3 that a one standard deviation increase on the average quality index at the school level implies an increase in 1.02 percentage points on the incumbents' vote-share, which is statistically different than zero at a 10% level. A one standard deviation increase on the average quality index at the city level has an effect of 0.44 percentage points on the proportion of votes of the politician running for reelection, which is statistically equal to zero.

Therefore, we consider our main results to be the ones that look at the performance of schools at an individual level to analyze the impact of receiving good or bad news about the quality of schools on the electoral outcomes of the politician currently holding office. To be sure that we are not missing the point on which dimension of performance matters to voters, we restrict our sample to cities in which one or two schools receive an IDEB score before 2008. We report these results in Appendix A.1, and we conclude that this restriction does not alter our findings in a meaningful way.

3.1.4
Does the effect vary according to voters' priors?

One reason for which the effect of releasing IDEB scores on the incumbents' electoral performance is small may be that voters' priors were very close to the actual performance of schools. Additionally, (30) make a point that differences in priors may explain the mixed findings of the literature that discusses information and electoral accountability. When looking at the effect of information about malfeasance on electoral outcomes, they interact their treatment variable with a variable that represents voters' priors, to test if their result remains unchanged.

We do something similar, but we do not have explicit information about voters' priors on school quality. We explore the fact that there were two events of information release about school performance prior to the 2008 elections to overcome this. When looking at the effect of the raw measure of performance that is given by IDEB 2007, we control for voters' priors with the triple interaction of the treatment variable with the 2007 quality index and the 2005 quality index. We are assuming here that the first release of IDEB scores fixes expectations about school quality. We report these results on Table 3.4.

In column 2 of Table 3.4 we show that allowing heterogeneous effects according to voters' priors does not change our results. A one standard deviation increase on the average IDEB 2007 score leads to a 1.41 percentage point increase on the vote-share of the incumbent, which is almost identical to

the point estimate of 1.40 we find when we do not control for the expectations of voters regarding the quality of schools. This indicates that our results are not driven simply by differences in voters' priors about the incumbent politician.

3.1.5 Heterogeneous effects

Overall, the results we have shown up to this point indicate that voters respond to information about public service delivery, but the magnitude of this effect is small. There are some reasons for which these effects may be small: i) voters may not receive the information about the IDEB scores of the school near them; ii) they do not understand the information they receive; iii) they do not take into account the delivery of public education when making their voting decisions. We decompose our effects in different ways to discuss each of these hypotheses.

There are two ways citizens may become aware of the IDEB scores near them. First, they may have children studying at the schools where they vote. Second, they may receive this information through the media. We find that voters reward incumbents for good performance and we would expect the magnitude of these effects to be larger where there are local radio stations and/or newspapers. The focus on local media comes from the evidence of other studies in the electoral accountability literature. (15) show that the sanctioning effect of auditing municipalities and revealing information about corruption to voters is larger where there is a broader presence of local media. (16) find that voters punish the party of malfeasant mayors only in electoral precincts covered by local radio stations. Thus, we look at the presence of local radio stations and newspapers in each city, and also at having or not Internet connection at home. In our context, the local media may be relevant to provide people with information about the results of IDEB in their community, other than in more aggregated levels. A similar line of thought can be applied to why Internet access may matter. There is a website only for searching IDEB grades and it might be the easiest way to learn about the performance of a specific school. Thus, we are using the proportion of households with an Internet connection as a proxy for Internet access.

We decompose our effect according to voters' schooling for two main reasons. 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. In fact, (18) run a field experiment in India in which they inform citizens about incumbent politicians' qualifications and performance in office and they find that, only for educated voters, receiving

this information leads to an increase in political knowledge. Second, more educated voters may value more the delivery of public education. Thus, they may be more likely to react to use the information on IDEB scores when deciding to cast a vote or not for the incumbent mayor running for reelection.

In Appendix A.2, we show how IDEB scores vary according to the presence of local media, voters' schooling and income. Overall, IDEB grades are larger in places with broader access to local media, with more educated voters and with higher median income. Since we do not want to confound the effect of these variables with the performance of schools, we once again take into account whether or not the information received by citizens is good news. We use the IDEB 2007 score to capture the performance of schools. We normalize this score to have mean of zero and standard deviation of one in each subsample.

We report the results of interacting school-level IDEB 2007 scores with the variables that capture the presence of local media on Table 3.5. In columns 1 and 2 of Table 3.5, we show that the magnitude of our effects is larger in municipalities that do not have a local radio. This is also the case if we define media coverage as the city having or not a local newspaper. In places where less households are connected to the Internet, the effect of a one standard deviation increase on the average IDEB 2007 grade is of 1.11 percentage points, or approximately 2%. However, in cities in which more households have an Internet connection the same effect is statistically equal to zero. We interpret this as an indication that what (28) call unmediated sources of information may be more important than the media to make citizens aware of the quality of schools near them.

We report the effect of releasing information about school quality on the vote share of the incumbent by quintiles of citizens' schooling on Table 3.6. For all quintiles of the proportion of high school graduates distribution, the effect of a standard deviation increase on the average IDEB 2007 score on the vote-share of the mayor is statistically equal to zero. The results we report on Table 3.6 are consistent with schools performing worse on the IDEB evaluation in cities with lower schooling levels. When taking this into account, we do not find that the response of voters to the release of IDEB scores is different in places where voters are more or less educated.

The results we show in this section suggest that the effect of providing voters with information about public education delivery varies according to media coverage. Our findings also suggest that, on the bottom quintiles of citizens' education, voters are negatively surprised by the performance of schools near them on the IDEB evaluation. When controlling for the initial

difference in the quality of the schools in cities with higher education levels, we find no differentiated response to increases on IDEB scores.

3.2 Robustness checks

We interpret our findings as an indication that some voters use the release of IDEB scores to update their priors about the mayor currently in office. When the information released is good news, the electoral support for the incumbent increases. However, the magnitude of our effects is small.

One concern with finding effects that seem small is that we are missing which piece of information matters most to voters. For instance, up to this point we focus on measures of performance that were released very close to the 2008 elections, which are the quality indexes for 2007. We make this choice based on evidence that voters pay more attention to news and information closer to election dates (25). Nevertheless, we still test if looking at IDEB scores for 2005, or even at variations over time, generates different findings. We report these results on Appendix A.3. Higher IDEB scores in 2005 or higher improvements between 2005 and 2007 imply on an increase on the vote-share of the incumbent. The same is true when looking at a simple indicator of improvement in performance over time. Having a negative variation on IDEB scores between 2005 and 2007 has no impact on the proportion of votes of the incumbent. These results are very similar to what we find when focusing on schools' performance in 2007.

Another concern with our empirical strategy comes from the trends on electoral outcomes prior to the 2008 elections. Although they are similar, there is a slight indication of mean reversion in voters' preferences. To test this, we add party trends to our specification. Since we only have two periods, this is an interaction of party dummies with a post-treatment indicator. In Appendix A.4 we show that our results remain basically unchanged after controlling for party-specific trends, which suggests that it is not likely for changes in preferences for specific parties in the treatment and control groups is driving our results. We also relax the parallel pre-trends assumption by adding trends specific to voters' characteristics and to the state where polling stations are located to our main specification. We report these results in Appendix A.5 and argue that this does not change our conclusions.

Finally, concerns with our empirical strategy may also come from the fact that schools in the treatment and control groups are different, especially in size. To account for this, we remove from our sample schools that have less than 50 or more than 300 students enrolled in grades 1-5. We then estimate

our results for this restricted sample and find effects that are even larger in magnitude than those computed for the entire sample, which we report in Appendix A.6.

Table 3.1: The impact of information about the quality of public schools on the vote-share of the incumbent mayor running for reelection: city-level measures of performance

% votes (0-100)	(1)	(2)	(3)	(4)	(5)
Informed	0.51	0.51		0.89	
	(0.77)	(0.77)		(0.92)	
Informed x City Quality Index 2007		1.32*			
		(0.68)			
Informed x (City Quality Index 2007 $\geq$ Target for 2007)			0.63		
			(0.83)		
Informed x (City Quality Index 2007 $<$ Target for 2007)			-0.13		
			(1.82)		
Informed x (City Quality Index 2007 in Top 20%)				0.62	
				(1.92)	
Informed x (City Quality Index 2007 in Bottom 20%)				-3.45**	
				(1.40)	
Informed x (City Quality Index 2007 $>$ State median)					0.69
					(1.11)
Informed x (City Quality Index 2007 $\leq$ State median)					0.30
					(0.94)
Number of polling stations	13,032	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550	4550
N not informed	8482	8482	8482	8482	8482
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3	51.3

Notes: i) Standard errors are clustered at the level of schools, since this is the level at which the information treatment is applied; ii) We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iii) Δideb = IDEB2007 $-$ IDEB2005; iv) We normalize IDEB 2007, IDEB 2005 and Δ ideb to each have a zero mean and a one point standard deviation;

Table 3.2: The impact of information about the quality of public schools on the vote-share of the incumbent mayor running for reelection: school-level measures of performance

% votes (0-100)	(1)	(2)	(3)	(4)	(5)
Informed	0.51	0.51		0.60	
	(0.34)	(0.34)		(0.40)	
Informed x Quality Index 2007		1.40***			
		(0.26)			
Informed x (Quality Index 2007 > Target for 2007)			0.67*		
			(0.39)		
Informed x (Quality Index 2007 ≤ Target for 2007)			0.21		
			(0.49)		
Informed x (Quality Index 2007 in Top 20%)				1.87***	
				(0.72)	
Informed x (Quality Index 2007 in Bottom 20%)				-2.09***	
				(0.62)	
Informed x (Quality Index 2007 > State median)					1.53***
					(0.43)
Informed x (Quality Index 2007 ≤ State median)					-0.45
					(0.43)
Number of polling stations	13,032	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550	4550
N not informed	8482	8482	8482	8482	8482
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3	51.3

Notes: i) Standard errors are clustered at the level of schools, since this is the level at which the information treatment is applied; ii) we scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iii) The quality index for 2007 is normalized to have zero mean and a one point standard deviation; iv) Quality index: $\mu = 3.97$ and $\sigma = 0.96$.

Table 3.3: The impact of information about school quality on the proportion of votes of the incumbent: city and school level measure of performance

% votes (0-100)	(1)	(2)
Informed	0.51	0.51
	(0.77)	(0.77)
Informed x School Quality Index 2007		1.02*
		(0.61)
Informed x City Quality Index 2007		0.44
		(0.80)
Number of polling stations	13,032	13,032
N informed	4550	4550
N not informed	8482	8482
Mean % votes in the control group in 2004	51.3	51.3

Notes: i) Standard errors are clustered at the city level; ii) The quality index for 2007 at the school and at the city level are normalized to have mean zero and a one point standard deviation;

Table 3.4: Controlling for voters' priors

% votes(0-100)	(1)	(2)
Informed	0.51	0.39
	(0.34)	(0.40)
Informed x Quality Index 2007	1.40***	1.41***
	(0.26)	(0.44)
Informed x Quality Index 2005		-0.034
		(0.43)
Informed x Quality Index 2007 x Quality Index 2005		0.15
		(0.24)
Number of polling stations	13,032	13,032
N informed	4550	4550
N not informed	8482	8482
Mean % votes in the control group in 2004	51.3	51.3

Notes: Standard errors are clustered at the school level. The quality indexes of 2005 and 2007 are normalized to have zero mean and a one point standard deviation.

Table 3.5: Heterogeneous effects in providing information to voters about public school quality on the proportion of votes of the incumbent running for reelection

	Radio		News		Internet	
	Yes	No	Yes	No	More	Less
% votes(0-100)	(1)	(2)	(3)	(4)	(5)	(6)
Informed	0.53	0.19	0.81	-1.14	-0.90	-0.69
	(0.87)	(1.33)	(1.15)	(0.72)	(1.07)	(0.72)
Informed x Quality Index 2007	1.27*	2.01**	0.94	0.98*	0.46	1.11**
	(0.71)	(0.90)	(0.84)	(0.54)	(0.84)	(0.51)
Number of polling stations	10,749	2,283	6,479	6,553	5,696	7,336
N informed	3907	643	3062	1488	3086	1464
N not informed	6842	1640	3417	5065	2610	5872
Mean % votes in control group in 2004	51	53	50.1	52.2	49.6	52.1

Notes: i) Yes and no radio mean, respectively, that the municipality has at least one local radio station or not, according to MUNIC 2006; ii) Yes and no newspaper mean, respectiveley that the municipality has at least one local newspaper or not; iii) More and less below Internet mean, respectively, that municipalities are below or above the median of proportion of households with Internet access in the 2010 Demographic Census; iv) Standard errors are clustered at the city level; v) We use school-level IDEB grades in these regressions; vi) IDEB grades are normalized, so its average is zero and the standard deviation is one in each subsample;

Table 3.6: Heterogeneous effects by citizens' schooling

	(1) % votes (0-100)
Informed x Quintile 1 of HS grads	-3.52***
	(0.96)
Informed x Quintile 2 of HS grads	-1.88*
	(1.08)
Informed x Quintile 3 of HS grads	0.19
	(1.22)
Informed x Quintile 4 of HS grads	-0.17
	(1.40)
Informed x Quintile 5 of HS grads	2.63*
	(1.43)
Informed x Quality Index 2007 x Quintile 1 of HS grads	0.62
	(0.91)
Informed x Quality Index 2007 x Quintile 2 of HS grads	0.92
	(0.89)
Informed x Quality Index 2007 x Quintile 3 of HS grads	0.46
	(0.92)
Informed x Quality Index 2007 x Quintile 4 of HS grads	0.52
	(1.14)
Informed x Quality Index 2007 x Quintile 5 of HS grads	0.019
	(1.07)
Number of polling stations	13,032
N obs treatment group	4550
N obs control group	8482
Mean % votes in the control group in 2004	51.3

Notes: i) We use data from the 2010 Census to construct the proportion of high school graduates for each municipality. Quintile 1 of HS grads means that the municipality where the polling stations is located is on the bottom of the distribution of proportion of high school graduates in our sample; ii) Standard errors are clustered at the city level; iii) We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iv) We use school-level IDEB scores in these regressions; v) IDEB grades are normalized, so that its average is zero and the standard deviation is one in each quintile;

4
Conclusion

This paper examines whether voters react to information about public service delivery. 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 politician before and after the information release in informed and non-informed groups of voters. 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 1.5% and 3%, depending on the measure of school performance we consider. In polling stations located at schools that rank in the bottom 20% of our sample, the support for the incumbent decreases in about 5% after the information release.

Overall, the magnitude of our effects suggests that the information about school quality causes few voters to change their voting patterns. To have a sense of the magnitude of the effect we measure, we compute the persuasion rates similar to (29) for our favorite specifications, which use school-level test scores. We assume that all parents from children in grades 1-5 in treated schools vote at that school. The purpose of this is to see what would be the largest switch in voting caused by the release of IDEB grades. We present the details of this computation in Appendix A.7.

The larger persuasion rates come from the interaction of the treatment variable with school-level IDEB grades being above or below the median of the state where the polling station is. For polling stations above the median, the release of IDEB scores implies that 17.8% of voters switch their votes in favor of the incumbent. For those below the median, the incumbent loses, on average, 4% of his support. Naturally, these numbers of the proportion of voters who switch their support for the incumbent are potentially overestimated, because we are being optimistic about the proportion of voters at each polling station that have children enrolled at the school where they are located. However, we

make the point that the small magnitude of our coefficients may actually imply a relevant effect of releasing information about the delivery of public education on the support for the incumbent mayor.

To test different explanations for the small magnitude of our coefficients, such as that voters do not receive the information about IDEB scores, do not care about the delivery of education, or do not understand the information that reaches them, we decompose our effects according to cities' local media coverage, education level of their citizens, and income. We do not find evidence of relevant heterogeneities in these dimensions. Thus, we do not find any clear indication of which of these hypotheses might explain why the response of voters to the information provided is small.

An alternative explanation for this is that voters do 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 hard to achieve (14). One limitation of our analysis is that we cannot fully understand why the effects we measure are small. Another limitation is that we cannot determine if voters are holding politicians accountable for the provision of public services, or simply using the information provided to them as a proxy for other things, such as their managerial ability.

Finally, there may be some concerns about our mechanism of local accountability and the validity of our definition of treatment and control groups. Our analysis implicitly assumes that voters live near where they vote and that their children go to school nearby. There is evidence in the literature of school choice that distance is one of the attributes most valued by parents (31, 32).

As to where people vote, we cannot ensure that citizens vote in the polling station closest to where they live. In Brazil, when people register to vote, the electoral zone to which they are assigned is determined by their address of residence, but they have the liberty to choose to vote at any polling station inside that specific zone. Thus, there may be citizens that live near a school assigned to our treatment group voting in a polling station located at a school in our control group and vice-versa. However, this potential contamination of treatment and control groups goes against finding any effect of the release of IDEB scores on the proportion of votes for the incumbent, by generating an underestimation bias in our coefficients of interest.

After finding an interesting response of voters to information about school quality, we raise the question of whether our effects are relevant enough to force local governments to try to improve the quality of schools. This would be key to defining if informing voters is enough to make what

(13) calls the long route of accountability work as a means of disciplining politicians to act in favor of the public interest. More than that, (4) propose using transparency and participation to make political incentives aligned with development objectives. Finally, assuming that the release of IDEB scores actually provides politicians with the right incentives to invest in education, leaves us with an appealing policy question, related to the kind of inequalities that are also being encouraged by the fact that some voters are better informed and/or care more about the quality of schools.

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A
Appendix

A.1
Cities with few treated schools

To close our discussion of which test scores (at the city or at the school level) capture more appropriately the effect we propose to measure, we restrict our sample to cities that have only one or two municipal schools evaluated by the national accountability system in 2005 and/or 2007. To apply this restriction we count not only municipal schools that are included in our sample, but also those that we leave out of the analysis.

We report the results of estimating our favorite specifications for this restricted sample on Table A.1. In Column 1 of Table A.1 we show that, for these cities, the average effect of releasing information about school quality on the incumbents' vote-share is statistically zero. This is qualitatively equivalent to our main findings, although the magnitude of the effect is larger.

When interacting our treatment variable with a raw measure of performance, which is the normalized school quality index of 2007, we find that a one standard deviation increase on the average school performance results in a 2 percentage points hike on the proportion of votes of the incumbent running for reelection. This is approximately 3.7% of the average vote-share of the incumbent in the control group prior to the release of information about school quality. This effect is also larger in magnitude than the one we find estimating the same regression for the whole sample. One explanation for this is that, in smaller cities, local communities are more engaged, or it is simply easier to observe performance. On the other hand, one could also argue that voters' priors about the quality of schools in these places should also be more precise relative to larger cities.

In column 3 of Table A.1 we show that the effect of meeting the performance target for 2007 on the vote-share of the incumbent is statistically equal to zero. The effect of missing the target is the same. Finally, in column 4 of Table A.1 we report the results of interacting our treatment variable with the relative measure of performance represented by an indicator of having a quality index in 2007 above or below the median of the state. Similarly to our

main findings, being above the state's median performance leads to an increase of 1.97 percentage points on the proportion of votes of the politician running for reelection.

In sum, restricting our sample to cities that have few treated schools does not change our results in a meaningful way. Thus, the small magnitude of our main findings does not seem to come from missing the point of which performance measures voters see as signaling information about the incumbent mayor.

A.2
How IDEB grades vary with citizens' schooling, access to information and income

We show on Table A.2 how IDEB grades vary in places with broader media coverage and more access to the Internet. In municipalities with at least one local radio and/or newspaper, IDEB scores are larger than where the presence of local media is scarcer or inexistent. In cities in which more people have Internet connection at home, schools also perform better in the IDEB evaluation.

On Table A.3, we show how IDEB grades vary when we divide our sample in quintiles of education. Our education variable is the proportion of people with a High School degree in the municipality. We see that, in cities with lower levels of education, schools perform worse on the IDEB evaluation.

A.3
Exploring the information of performance in 2005

In this section, we explore the performance of schools in 2005 to test if the effects are similar to the main results we report on Table 3.2 and to consider the impact of variation of performance over time on the vote-share of the incumbent. We show on Table A.4 that a standard deviation increase on the quality index of 2005 leads to an increase of 1.12 percentage points on the incumbents' vote-share, which is approximately 2%. This is similar to what we find when interacting our treatment variable with the quality index for 2007.

When looking at variations over time, we find that a standard deviation increase on the improvement of IDEB between 2005 and 2007 leads to 0.44 percentage point increase on the proportion of votes of the incumbent mayor running for reelection. This is less than 1% and is statistically different than zero only at a 10% level. Similarly, voters near schools that show an improvement in performance over time reward the incumbent, on average, with

a 0.71 percentage point increase on his vote-share, which is also significant only at a 10% level.

A.4
Considering mean reversion driven by voters' political preferences

Because our pre-trends figures may suggest that voters' political preferences in the control group might be driving our results, we add party trends to our main specification. Since we only have two periods, this is adding the interaction of party dummies with a post-treatment indicator to the regressions. We report these results on Tables A.5 and A.6 and they show that our conclusions remain.

A.5
Relaxing the parallel trends assumption

A typical concern with difference-in-differences estimators is confounding our treatment effect with a spurious mean reversion. We test the robustness of our results to adding voters' characteristics and state trends to our regressions. Since we only have two periods in our panel, we simply interact these variables with a post-treatment dummy. Voters' characteristics at each polling station are the proportion of people per age group and gender. Although we do have information about the proportion of voters by education levels, we do not use this in these interactions because we believe this variable has measurement error that is correlated with its true value, violating the classical measurement error hypothesis and generating a potential bias in all our coefficients[1].

We report these results on Tables A.7 and A.8. We show in columns 2, and 4 of Table A.7 that adding state trends to our regressions leads to a reduction of the estimated effects, and we can no longer reject the hypothesis that they are equal to zero. The same is true for adding state trends to the coefficients we report on Table A.8. Besides losing variation, state trends may be problematic to us because of the distribution of observations among treatment and control groups. We show on Table A.9 that the state of Amapá (AP) does not even have any observations in the control group in our sample, and other states have a very uneven distribution of observations between these groups. Thus, it is not obvious that restricting ourselves to within state comparison between

[1]People report there education level to the Supreme Electoral Court (TSE) when they register to vote at around age 18. At this age, many voters continue to educate themselves and achieve higher education levels. In polling stations where the average voter is more educated, the measurement error will be larger, thus being correlated with the true education levels we do not observe.

observations in the treatment and control group should make our results more reliable.

When adding voters' characteristics trends to our specification, most of our results remain unchanged. In column 1 of Table A.7 we show that the average effect of informing voters about school quality on the vote-share of the incumbent remains qualitatively the same, although the point estimate is even smaller in magnitude. Other coefficients on Table A.8 also decrease in magnitude after looking at within age and gender group effects. However, in general they remain qualitatively the same, which indicates that potentially confounding effects should not be captured by differences in voters characteristics.

A.6
Addressing the issue of mean reversion in the test scores of small schools

(33) point out two facts about variability in school-level mean test scores. First, that it is high among small schools. Second, this variability in test scores among small schools is not driven exclusively by heterogeneity among them. These schools are also much more likely to report large changes in mean scores over time, which raises a concern that our results might be driven by a spurious mean reversion. To address this 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. We show that treatment and control groups become more balanced in size after restricting our sample. 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.

We show the results of comparisons of means tests between treatment and control groups after dropping very large and very small schools from our sample on Table A.10. Cutting off schools based on their size implies losing half of the treatment and half of the control group. Overall, the difference in size of schools that are and are not treated becomes smaller after the restriction, but is still different than zero. This does not solve the problem of balance for the other variables as well, although being similar on average is not strictly required by our identification strategy.

We report our main results for the restricted sample on Table A.11. In column 1 of Table A.11 we show that the average effect of the information is actually statistically positive in the restricted sample. In this sample, informing voters about the quality of schools increases the proportion of votes of the incumbent in 1.26 percentage points, which is approximately 2%. When considering different measures of performance, our results remain either similar

or we see an increase in the magnitude of the effects in the restricted sample.

(34) also use the results of a placebo test as evidence of spurious mean reversion. They reproduce their treatment effects in periods prior to the treatment assignment. We can do a similar exercise looking at the effect of the information about the quality of state schools on the proportion of votes of the incumbent mayor. We do not look at prior elections because of the term limits for mayors in Brazil. We report the results of this exercise on Table A.12. We do not find evidence of spurious mean reversion when looking at the effect of releasing information about the quality of school run by Governors on the proportion of votes of the incumbent mayor running for reelection. One coefficient that is statistically different than zero is from the interaction of the treatment variable with IDEB 2007 grades. In this case, the magnitude of the effect is small, and about half of the same effect measured in our main regressions using municipal schools. When looking at the worst performers in the sample there is a decrease on the proportion of votes of the incumbent, which is statistically different than zero at a 5% level.

A.7
Persuasion rate

We compute persuasion rates similar to (29) to have a sense of the magnitude of our results. For now, we abstract of changes in voter turnout that might be induced by the release of information about school quality. In this case, the formula of the persuasion rate is given by:

$$f = \frac{v_T - v_C}{e_T - e_C} \frac{1}{1 - v_0} \tag{A-1}$$

in which v_T is voting in the treatment group v_C is voting in the control group, $e_T - e_C$ is the exposure to the message and v_0 is voting in the absence of the treatment, which is approximated by v_C. In our case, the voting variables are actually differences in the proportion of votes over time, such that $v_T - v_C$ is the coefficient of our regressions. We use $v_0 = v_{C,2008} - v_{C,2004}$.

We determine $e_T - e_C$ in the way of considering the maximum exposure possible in treatment and control groups. In our context, this means assuming that all parents of children studying in the school at which the polling station is located actually votes there. Thus, we multiply the number of children enrolled in the elementary grades of the school in question by two and divide it by the total of voters at that polling station[2]. Thus e_T and e_C are, respectively, the proportion of voters who are parents at polling stations from treatment and control groups.

[2]When this proportion is greater than one, we change it to one.

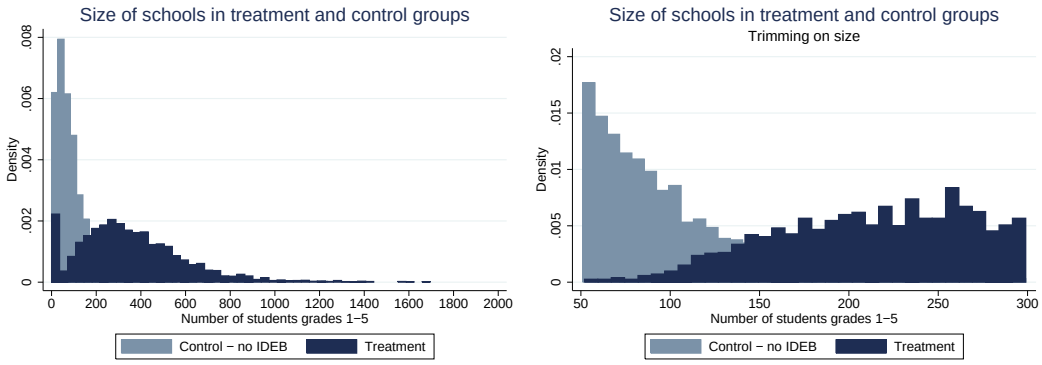

Figure A.1: Restricting the size of schools in the sample

Table A.1: Robustness checks: cities with only one or two treated municipal schools

% votes (0-100)	(1)	(2)	(3)	(4)
Informed	0.99	0.99		
	(0.80)	(0.80)		
Informed x IDEB 2007		1.99***		
		(0.61)		
Informed x (Quality Index 2007 > Target for 2007)			1.03	
			(0.97)	
Informed x (Quality Index 2007 ≤ Target for 2007)			0.92	
			(1.05)	
Informed x (Quality Index 2007 > State median)				1.97*
				(1.07)
Informed x (Quality Index 2007 ≤ State median)				0.18
				(0.96)
Number of polling stations	2,352	2,352	2,352	2,352
N informed	732	732	732	732
N not informed	1620	1620	1620	1620
Mean % votes in the control group in 2004	53.5	53.5	53.5	53.5

Standard errors are clustered at the school level. To define top and bottom performers, we compute percentiles of IDEB for treated schools in our sample and divide them in three groups: the top quintile, quintiles 2, 3 and 4 and the bottom quintile. The state median is computed relative to all schools that are evaluated by IDEB, and not only the ones included in our sample.

Table A.2: Ideb grades in municipalities with more or less media coverage

	Yes Mean	No Mean	Pval
Local radio			
IDEB 2005	3.45	3.65	0.00
IDEB 2007	3.80	4.00	0.00
Obs	643	3,907	
Local newspaper			
IDEB 2005	3.15	3.85	0.00
IDEB 2007	3.50	4.20	0.00
Obs	1,488	3,062	
Internet			
IDEB 2005	2.83	4.00	0.00
IDEB 2007	3.21	4.33	0.00
Obs	1,464	3,086	

i) Radio and newspaper variables come from the 2006 MUNIC survey. No and Yes in these cases means not having or having at least one local radio or newspaper in the municipality. ii) The Internet variable is the proportion of household with access to Internet in a given city according to the 2010 Demographic Census. No Internet and Yes Internet means, respectively, being below and above the median of proportion of household with Internet access;

Table A.3: Ideb grades by quintiles of education

IDEB 2007	Mean	SD	Min	Max	Obs
1st quintile	3.04	0.65	1.10	5.80	1,020
2nd quintile	3.25	0.76	1.40	5.60	1,132
3rd quintile	3.62	0.80	1.30	6.10	1,272
4th quintile	4.12	0.80	1.50	7.90	1,722
5th quintile	4.46	0.85	1.30	7.50	3,954

We determine quintiles of education based on the proportion of High School graduates in each municipality in the 2010 Demographic Census. This classification is at the city level, which justifies the difference in the number of observations among quintiles.

Table A.4: The effect of releasing information about school quality on the proportion of votes of the incumbent running for reelection: performance in 2005 and variation over time

% votes (0-100)	(1)	(2)	(3)	(4)
Informed	0.51	0.51		
	(0.34)	(0.34)		
Informed x Quality Index 2005	1.12***			
	(0.25)			
Informed x (Δ Quality Index)		0.44*		
		(0.26)		
Informed x (Quality Index 2007 $>$ Quality Index 2005)			0.71*	
			(0.38)	
Informed x (Quality Index 2007 $\leq$ Quality Index 2005)			0.027	
			(0.53)	
Number of polling stations	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550
N not informed	8482	8482	8482	8482
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3

Standard errors are computed at the school level. We normalize the quality index of 2005 to have a zero mean and a one point standard deviation. ΔQuality Index = Quality Index$_{2007}$ − Quality Index$_{2005}$. We also normalize Δ Quality Index. For Quality Index 2005, $\mu = 3.62$ and $\sigma = 0.97$.

Table A.5: Adding party trends to the main specification

% votes (0-100)	(1)	(2)	(3)	(4)	(5)
Informed	0.53	0.51	0.54		
	(0.34)	(0.34)	(0.34)		
Informed x Quality Index 2007		1.40***	1.51***		
		(0.26)	(0.25)		
Informed x (Quality Index 2007 > Target for 2007)				0.67*	0.73*
				(0.39)	(0.39)
Informed x (Quality Index 2007 ≤ Target for 2007)				0.21	0.13
				(0.49)	(0.49)
Number of polling stations	13,032	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550	4550
N not informed	8482	8482	8482	8482	8482
Party x Post	Y	N	Y	N	Y
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3	51.3

Table A.6: Adding party trends to the main specification

% votes (0-100)	(1)	(2)	(3)	(4)
Informed	0.60	0.49		
	(0.40)	(0.40)		
Informed x (Quality Index 2007 in Top 20%)	1.87***	2.36***		
	(0.72)	(0.70)		
Informed x (Quality Index 2007 in Bottom 20%)	-2.09***	-1.89***		
	(0.62)	(0.62)		
Informed x (Quality Index 2007 > State median)			1.53***	1.54***
			(0.43)	(0.43)
Informed x (Quality Index 2007 ≤ State median)			-0.45	-0.43
			(0.43)	(0.43)
Number of polling stations	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550
N not informed	8482	8482	8482	8482
Party x Post	N	Y	N	Y
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3

Table A.7: Robustness checks: adding voters' characteristics trends and state trends to our main specifications

% votes (0-100)	(1)	(2)	(3)	(4)
Informed	-0.037	-0.12	0.050	-0.058
	(0.40)	(0.37)	(0.40)	(0.38)
Informed x Quality Index 2007			1.32***	0.31
			(0.26)	(0.31)
Number of polling stations	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550
N not informed	8482	8482	8482	8482
Age and male x Post	Y	N	Y	N
State x Post	N	Y	N	Y
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3

i) Standard errors are clustered at the level of schools, since this is the level at which the information treatment is applied; ii) We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iii) We normalize IDEB 2007 to have a zero mean and a one point standard deviation; iv) Voters' characteristics trends are dummies for age, gender and schooling level. These variables refer to the 2008 municipal elections, which is the first period for which this data is available at the level of polling stations (Source: TSE);

Table A.8: Robustness checks: adding voters' characteristics trends and state trends to our main specifications

% votes (0-100)	(1)	(2)	(3)	(4)	(5)	(6)
Informed x (Quality Index 2007 > Target for 2007)	0.12	-0.17				
	(0.45)	(0.42)				
Informed x (Quality Index 2007 ≤ Target for 2007)	-0.34	-0.016				
	(0.53)	(0.51)				
Informed x (Quality Index 2007 > State median)			0.95*	0.47		
			(0.49)	(0.47)		
Informed x (Quality Index 2007 ≤ State median)			-0.91*	-0.62		
			(0.46)	(0.44)		
Informed					0.16	0.21
					(0.46)	(0.44)
Informed x (Quality Index 2007 in Top 20%)					1.67**	-0.33
					(0.72)	(0.76)
Informed x (Quality Index 2007 in Bottom 20%)					-2.03***	-1.08
					(0.62)	(0.66)
Number of polling stations	13,032	13,032	13,032	13,032	13,032	13,032
N informed	4550	4550	4550	4550	4550	4550
N not informed	8482	8482	8482	8482	8482	8482
Age and male x Post	Y	N	Y	N	Y	N
State x Post	N	Y	N	Y	N	Y
Mean % votes in the control group in 2004	51.3	51.3	51.3	51.3	51.3	51.3

i) Standard errors are clustered at the level of schools, since this is the level at which the information treatment is applied; ii) We scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iii) To compute top and bottom performers, we rank all treated schools in our sample according to their quality index; iv) The state median is computed relative to all treated schools in a state, regardless of being run by a local government or included in our sample; v) Voters' characteristics are the proportion of citizens at each polling station in age, gender and schooling level groups. These variables refer to the 2008 municipal elections, which is the first period for which this data is available at the level of polling stations (Source: TSE);

Table A.9: Distribution of observations between treatment and control groups by state

State	Treatment	Control	% Treatment	% Control
AC	25	17	0.60	0.40
AL	67	40	0.63	0.37
AP	4		1.00	
AM	22	96	0.19	0.81
BA	367	1,428	0.20	0.80
CE	316	1,273	0.20	0.80
ES	62	108	0.36	0.64
GO	145	80	0.64	0.36
MA	284	951	0.23	0.77
MT	73	70	0.51	0.49
MS	142	31	0.82	0.18
MG	563	798	0.41	0.59
PA	275	810	0.25	0.75
PB	158	111	0.59	0.41
PR	464	364	0.56	0.44
PE	213	284	0.43	0.57
PI	73	496	0.13	0.87
RJ	136	73	0.65	0.35
RN	87	150	0.37	0.63
RS	297	705	0.30	0.70
RO	22	44	0.33	0.67
SC	135	326	0.29	0.71
SP	526	78	0.87	0.13
SE	33	94	0.26	0.74
TO	61	55	0.53	0.47

Table A.10: Comparison of means between treatment and control groups for the sample restricted on the size of schools

	Control		Treatment		
	Mean	Obs	Mean	Obs	Pval
School characteristics					
Teacher's room	0.20	7,650	0.67	1,777	0.00
Computer lab	0.04	7,650	0.31	1,775	0.00
Science lab	0.01	7,650	0.04	1,775	0.00
Library	0.15	7,133	0.46	1,735	0.00
N Computers	0.33	7,653	2.71	1,778	0.00
N teachers	5.74	7,653	15.82	1,778	0.00
Enrollment - El. School	74.97	7,653	200.41	1,778	0.00
Avg class size	26.90	5,728	26.95	1,778	0.89
Dropout rates - 5th grade	0.07	7,110	0.05	1,734	0.00
Passing rates - 5th grade	0.77	6,562	0.80	1,592	0.00
Voters' characteristics					
Age $\leq$ 24	0.23	7,653	0.22	1,778	0.01
24< Age < 60	0.62	7,653	0.65	1,778	0.00
Age > 60	0.16	7,653	0.13	1,778	0.00
Male	0.52	7,653	0.49	1,778	0.00
Middle school dropout	0.84	7,653	0.68	1,778	0.00
High school dropout	0.12	7,653	0.21	1,778	0.00
High school graduate	0.04	7,653	0.10	1,778	0.00
Electoral outcomes					
% votes PT 2006	0.65	7,551	0.56	1,771	0.00
% votes PSDB 2006	0.32	7,528	0.38	1,771	0.00
% turnout 2006	0.82	7,551	0.82	1,771	0.08

We drop from our sample schools that have less than 50 and more than 300 students enrolled in grades 1-5.

Table A.11: Robustness checks: restriction on size

% votes (0-100)	(1)	(2)	(3)	(4)	(5)
Informed	1.26***	1.26***			1.68***
	(0.48)	(0.48)			(0.57)
Informed x Quality Index 2007		0.96***			
		(0.37)			
Informed x (Quality Index 2007 > Target for 2007)			1.62***		
			(0.55)		
Informed x (Quality Index 2007 ≤ Target for 2007)			0.54		
			(0.70)		
Informed x (Quality Index 2007 > State median)				1.70***	
				(0.61)	
Informed x (Quality Index 2007 ≤ State median)				0.86	
				(0.61)	
Informed x (Quality Index 2007 in Top 20%)					0.44
					(1.12)
Informed x (Quality Index 2007 in Bottom 20%)					-2.07**
					(0.88)
Number of polling stations	6,662	6,662	6,662	6,662	6,662
N informed	2025	2025	2025	2025	2025
N not informed	4637	4637	2808	4637	4637
Mean % votes in control group in 2004	51.5	51.5	51.5	51.4	51.4

i) This table refers to the sample in which we eliminate schools from the control group 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) The state median is computed relative to all schools evaluated by IDEB, regardless of being run by local governments or being in our sample; iv) Top and bottom performers are obtained from ranking the quality index of schools in our treated sample;

Table A.12: The impact of information about the quality of **state** public schools on the vote-share of the incumbent mayor running for reelection

% votes (0-100)	(1)	(2)	(3)	(4)	(5)
Informed	-0.18	-0.18			0.34
	(0.75)	(0.75)			(0.85)
Informed x IDEB 2007		0.90**			
		(0.36)			
Info x (Ideb 2007 $\geq$ Target 2007)			0.17		
			(0.78)		
Info x (Ideb 2007 $<$ Target 2007)			-1.14		
			(0.96)		
Info x (Ideb 2007 $>$ State median)				-0.20	
				(0.81)	
Info x (Ideb 2007 $\leq$ State median)				-0.14	
				(0.86)	
Informed x (Ideb 2007 in Top 20%)					-0.48
					(0.92)
Informed x (Ideb 2007 in Bottom 20%)					-2.16**
					(0.89)
Number of polling stations	3,363	3,363	3,363	3,363	3,363
N informed	2597	2597	2597	2597	2597
N not informed	766	766	766	766	766
% votes in the control group in 2004	51.2	51.2	51.2	51.2	51.2

i) To give the best shot of each coefficient being statistically different than zero, non-robust standard errors are reported in parenthesis; ii) we scale up the dependent variable, which is the vote-share of the incumbent, to vary from 0-100; iii) The state median is computed relative to all schools evaluated by IDEB, regardless of being run by local governments or being in our sample; iv) Top and bottom performers are obtained from ranking the quality index of schools in our treated sample;

Table A.13: Persuasion rates

	$v_T - v_C$	$e_T - e_C$	v_0	f
Informed	0.511	10.436	-1.104	0.048
Informed x (Ideb 2007 $>$ Ideb 2005)	0.711	10.328	-1.104	0.068
Informed x (Ideb 2007 $\leq$ Ideb 2005)	0.027	9.846	-1.104	0.003
Informed x (Ideb 2007 $>$ State median)	1.534	8.603	-1.104	0.176
Informed x (Ideb 2007 $\leq$ State median)	-0.448	11.038	-1.104	-0.040