Punishing Mayors Who Fail the Test:

How Do Voters Respond to Information about Educational Outcomes?

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Abstract*

This paper explores the electoral effects of providing information on the educational outcomes of municipal schools when the mayor is running for reelection. We designed and implemented an experiment in Chile whereby we sent 128,033 letters to voters in 400 randomly selected polling stations. The letters included information on past test scores for local public schools (levels and changes), and either average or maximum outcomes for comparable municipalities. We find that information on educational outcomes affects turnout, which translates almost one to one into votes for the incumbent mayor. Voters respond to educational results in levels and to letters that have average results as a benchmark. The results are concentrated in polling stations with bad educational outcomes, which reduce turnout and thus votes for the incumbent, especially if such outcomes come as news to voters. The results appear to be stronger in contexts where information is scarcer. Overall, when provided with information they care about, voters punish mayors who fail the test. JEL Codes: D72, H75, I25.

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Introduction

The idea that citizens can punish underperforming politicians at the polls is considered a key feature of democracy. However, poor governance abounds around the world, hindering the possibilities for development, and it often does with electoral support. Past studies have hypothesized that this is because citizens lack the necessary information to adequately assess politicians. Several experimental papers study this hypothesis, often with a focus on corruption (e.g. Ferraz and Finan 2008; de Figuereido et al. 2011; Chong et al. 2015; Larreguy et al. 2020), and mainly in low-income countries (e.g. Banerjee et al. 2011; Humphreys and Weinstein 2012; Adida et al. 2016; Buntaine et al. 2018; for reviews of this question, see Olken and Pande 2013; World Bank 2016; Pande 2020), with mixed results. The lack of consistent findings in the literature raises the question of whether information is more impactful in other areas, or whether it depends on how information is provided.

Thus, the paper's first objective is to explore the electoral effect of providing voters with information about local government outcomes—in this case, educational outcomes—in an emerging economy with a consolidated democracy where local governments oversee the management of public schools and voters identify the provision of education as one of the main priorities of mayors. Informing voters about educational outcomes is especially important because of the key role of education in development and relevant informational frictions in this area. For instance, parents are usually misinformed about schools' results (see World Bank 2020 and Allende et al. 2019 for recent evidence for the world and Chile, respectively). To this end, we designed and implemented a large-scale randomized intervention that involved sending 128,033 voters a letter containing information about educational outcomes (test scores) of public schools in their municipality under the current mayor.

I second objective is to better understand what types of information generally matter to voters. For this purpose, we focus on four main dimensions. First, each letter presents information on two indicators: test scores in levels (corrected to account for socioeconomic differences) and changes in test scores as a measure of improvement over the mayor's tenure. Second, we also varied the yardstick used to compare the municipality's outcomes for both indicators by randomly assigning whether voters received the average or best-performing municipality's results as a benchmark. The maximum treatment is a more stringent benchmark, but it is *a priori* unclear whether this makes it more effective. While raising citizens' expectations increases the chances of sanctioning poor performance (Gottlieb 2016; Cruz et al. 2021), providing yardsticks that are overly high may be counter-productive, as it may reinforce the underlying heterogeneity in individual outcomes (e.g. Nguyen 2007). Third, informing about good and bad outcomes may have different effects, which is consistent with loss aversion (e.g. Kahneman and Tversky 1979). Fourth, we further study how information affects electoral behavior depending on voters' prior beliefs about school outcomes. We expect information to have stronger effects when it provides news to voters, i.e., when it differs from their priors (Arias et al. 2018). Thus, we study in detail the effects of these four dimensions of information to understand what matters to voters when they cast their vote.

Our sample contains 59 large, urban municipalities in Chile in which the mayor is running for reelection. The sample includes 22,385 polling stations with an average of 334 registered voters each. We randomize the treatments at the polling station level, considering strata based on the municipality and each polling station's gender composition. We allocated 200 stations to the "average" benchmark and 200 to the "maximum" benchmark, leaving 21,985 polling stations in the control group.

We have four main sets of results. First, information matters: being informed of relative performance affects turnout, which translates almost one-to-one into votes for the incumbent. The results are especially strong when the educational results are bad, reducing turnout and votes for the incumbent. In other words, voters punish mayors for poor educational outcomes by not turning out to vote for them. The size of the effect is important: moving from the 75th to the 25th percentile in educational performance reduces turnout by about 1.24 percentage points and support for the incumbent by around 1.12 percentage points. Second, not all information matters equally: our results are concentrated in the group that received average performance letters, suggesting that overly stringent standards may not be relevant for voters. Voters also respond to educational results in levels, rather than to changes in results, and bad results have much stronger effects than good ones.

The third set of results reveals that priors matter: we use a fine-grained proxy for perceptions of schools quality at the polling station level taken from a universal survey of parents and find that our results are driven by information that comes as "news," i.e where there exists a mismatch between voters' priors and the information provided to them. Moreover, consistent with our previous results, we find that voters react more strongly to "bad news" (i.e. priors of good performance but new information suggesting otherwise). Fourth, while due to the limited number of municipalities in the sample we lack the statistical power to identify statistically significant heterogeneities in the treatment effects, our results seem to be mainly driven by polling stations in poor municipalities and those with lower levels of incumbent campaign spending, which suggests that the effects are stronger where information is scarcer.

It is worth noting that our main specifications control for fixed effects at the municipality level so that all the identification of the effects comes from two sources of variation: i) within-municipality variation of whether voters received the letters and ii) the content of the report cards, which varies across municipalities. While the first source is randomly allocated, the second is not, since it depends on the outcomes of public schools, which are not exogenous. Thus, we present a set of robustness exercises in which we add interactions of treatment status with other 11 variables (unrelated to the content of the letters) that capture between-municipality variation in other dimensions. We find that the main estimated effects are unaffected by including these interactions. Thus, suggesting that the results are truly related to the content of the report cards.

Our paper makes two main contributions to the literature. First, we identify the causal impacts of giving voters performance information in terms of outcomes generated by local politicians—not just inputs. Our study is one of the first experimental papers to provide information on measures of outcomes of locally elected politicians; in particular, regarding education performance. A small number of papers have exploited the disclosure of public school outcomes in Brazil and present quasi-experimental evidence of its effects on local elections (Dias and Ferraz 2019; Firpo et al. 2017; Toral 2016). Ajzenman and Durante (2021) study how the quality of the infrastructure of the school where citizens were assigned to vote , influenced their support for their mayor he ran for president. Relatedly, de Kadt and Lieberman (2017) report negative correlations between public service delivery and support for the incumbent, also in a quasi-experimental setting.[1] Our experiment directly measures the effect of information about educational quality on the vote choice of the offices who oversee public schools (mayors), in a large scale and in a context that allows exploring heterogeneities. In our setting, educational outcomes are among voters' main priorities (and are correlated with other outcomes at the municipality level), mayors have sufficient leeway to affect outcomes and voters are aware of this, and there are well-documented information frictions about educational outcomes.[2]

Second, we add to the literature on the conditions under which informational interventions are more likely to be an effective means for enhancing democratic accountability. We investigate whether voters respond more to information provided in levels or changes, and with different benchmarks. These are key topics because, from a conceptual perspective, voters' responses to information depend on which yardsticks they have available. We also investigate whether voters respond more strongly to good or bad outcomes, and to those that represent good or bad "news." It is even possible that some of the null results of information found in the literature (e.g. Dunning et al. 2019) arise out of providing types of information that voters do not care about, and not because people do not care about information overall. Our research helps understand the conditions under which informational interventions are more likely to be effective.[3]

The paper is organized as follows. Section 2 provides background information on Chile's electoral and education systems and describes the data. Section 3 presents the experimental

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[1] In a related result, Sandholtz (2022) finds that informing voters of the outcomes of a "successful" educational reform in Liberia, as well as the presidential and legislative candidates' stances on this policy, makes voters in competitive districts more likely to report voting for candidates from parties that supported the policy.

[2] Thus, we also contribute to the literature on information frictions to assess the quality of educational providers. While most of the previous papers have identified significant frictions at the school level (e.g., Allende et al., 2019 and World Bank, 2020), we identify effects of the provision of municipality-level information related to the provision by public schools. This is a relevant contribution because the market equilibrium of the provision of education may be significantly affected by the behavior of public schools that are typically a significant share of the market and provides a benchmark for the provision of other types of providers. See, for instance, Andrabi et al. (2013) for a paper showing how increases in the quality of public schools affect the provision of private schools in general equilibrium.

[3] As an additional contribution, our setting permits using more fine-grained data than previous studies. Intervening at the polling station level implies an advantage over most papers in the literature, which intervene at more aggregate levels, such as the precinct, slum, or village level (Banerjee et al. 2011; De Figueiredo et al. 2011; Chong et al. 2015; Gottlieb 2016; Arias et al. 2018). We also use a fine-grained polling-station-level measure of voters' prior beliefs about educational outcomes that provides within-municipality variation, which allows us to determine that "bad news" is more consequential.

design including the treatments, sample, randomization, balance tests, and the main estimating equation. Section 4 presents the main findings and robustness checks and extensions. Section 5 concludes.

2. Context and Data

2.1 Local Elections and Public Education in Chile

With a population of around 18 million people, Chile is organized at the local level into 345 municipalities. The mayor is the head of the local government, and her responsibilities include managing financial resources; providing municipal and national public goods in the municipality including public schools; and strategic planning including building permits, garbage collection, and implementing local health and education policies. Local elections are held every 4 years, and mayors are directly elected by a majoritarian system. Municipalities also have a council of 6–10 members, depending on their population, who are elected in a proportional system. As shown in Table 1, a large majority of mayors run for reelection (over 80%); over 60% are reelected. During our study period, Chilean politics was mostly organized around two main coalitions, Alianza and Concertación, and more than 70% of mayors belonged to one of these coalitions (see Appendix Figure 1). Thus, whenever we analyze political leanings, we focus on these coalitions.

Table 1: Incumbent Advantage

Local election	Percentage of mayors who run for reelection	Percentage of running incumbent mayors who are reelected
2004	88%	67%
2008	80%	63%
2012	84%	60%
2016	86%	73%

Notes: Percentage of mayors who run for reelection are calculated using the total number of mayors in the country (*N=345*). Source: SERVEL.

Two reforms were enacted in the 2010s that affected the 2016 election, which we examine here. First, in 2012 Chile moved from a system with mandatory voting and voluntary registration to one that has voluntary voting and automatic registration. Before the reform, 32% of the adult population (mostly young people) was not registered to vote, and there were separate polling stations for men and women. Beginning with the 2012 local elections, new mixed-gender stations were created; old stations with fewer than 350 voters were mostly filled with younger voters of the opposite sex. We exploit this enormous variation in polling stations' age and gender composition in the heterogeneity analysis.

In a second reform, the 2016 local elections were the first to be held after stringent restrictions of political campaigns were introduced in April 2016. The new law shortened the campaign period, reduced the limits for electoral spending and for contributions to each candidate, increased the transparency surrounding electoral campaign contributions and electoral spending, regulated political signage (where it can be placed and the size of posters and signs), and imposed higher sanctions for electoral offenses (Ministerio del Interior 2016). Since less information was available in this election than in previous elections, information was likely to be more valuable in 2016. Indeed, the Espacio Público and Ipsos surveys in 2017 revealed, respectively, that 45% and 50% of respondents reported there was less information about candidates and their programs than in previous campaigns. We explore the role of campaign spending in our heterogeneity analyses.

Chile has a school choice system with public and private providers with public funding, in which 35% of students attend public schools, 56% voucher schools, and 9% private schools that do not receive public funds (Mineduc 2018). Mayors oversee public schools, which are known as "municipal schools," and have considerable leeway to affect outcomes, including appointing the head of the municipal school system, setting the school system's goals, and assessing its performance yearly.[4] Previous research documents important differences in productivity at the municipal level (e.g., Gallego 2013; Allende et al. 2019) as well as the importance of transfers from local governments to finance municipal schools, on top of the voucher system used in the country (OECD 2017). Furthermore, a report by the Comptroller

[4] Under an ongoing reform, the management of local public schools is scheduled to transfer from the 345 municipalities to 70 school districts by 2025 (Ministerio de Educación 2017). However, implementation is currently delayed; the transition is expected to be finished in 2027 at the earliest.

General found that several municipalities misused funds allocated for targeted education vouchers.[5] This practice establishes a possible link between educational outcomes and fund mismanagement.

Since 1988, Chile has administered a yearly nationwide test (called the System of Measurement of the Quality of Education, or SIMCE) to assess the quality of education. SIMCE is administered to more than 90% of students, annually to 4th graders, and depending on the year, to 6th, 8th, or 10th graders. It includes math, Spanish, natural sciences, and history and social sciences.

Most parents are familiar with SIMCE, since almost 90% of them have to answer the associated parent survey at least once during a child's time in school. Although parents consider it a reliable instrument, they are generally misinformed about the scores of their child's school (Allende et al. 2019). Allende et al. (2019) present evidence that shows that parents are not fully informed of their children's school's results: providing information about schools' test scores changed treated households' primary school choices, which improved their children's educational outcomes 6 years after the intervention.

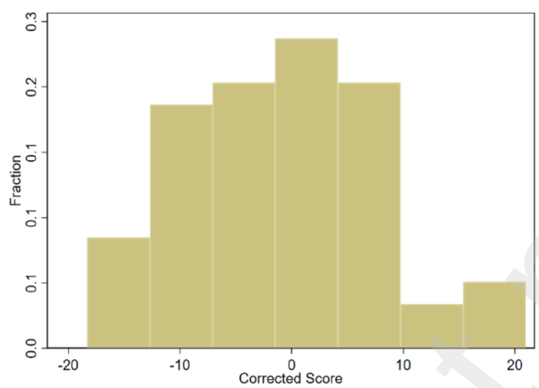
A survey revealed that 60% of respondents with a school-age child reported knowing the most recent SIMCE scores of their child's school, but when asked about the rough score, 80% did not actually know (CEP Survey, June-July 2011). Likewise, another survey revealed that 73% of parents know what the SIMCE is, but only 21% said they know the score of their child's school (CIDE 2006). Therefore, voters are unlikely to be precisely informed of education test scores at the municipality level. Furthermore, data from SIMCE parents' survey reveal great within-municipality variance in beliefs about public schools' quality, with a weak correlation between beliefs and quality as measured by SIMCE (see section 4.1).

In this paper, we report math and language results because these subjects are the only ones assessed annually. We only consider 4th grade results, since this is the only grade assessed annually during our study period, and because younger children usually go to schools near their homes; thus municipality plays a larger role in potential educational outcomes. The SIMCE results reveal there is great variation in public school results at the municipality level.

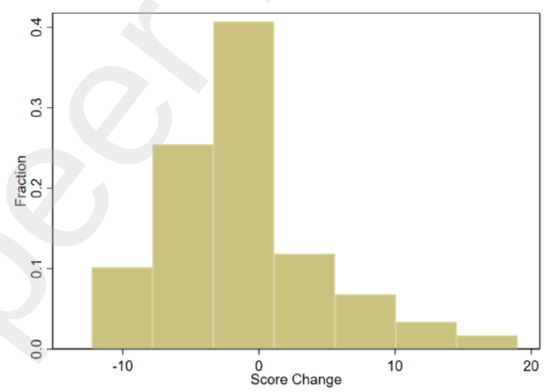
[5] See https://ciperchile.cl/wp-content/uploads/contraloria_nominacompleta.pdf.

Figure 1 presents the average test scores after controlling for socioeconomic status proxies for municipalities in both levels and changes with respect to the previous mayoral period.[6] There is a notably high level of heterogeneity across municipalities and over time. Finally, it is important to note that municipality-level educational results might be considered a proxy for the general quality of the mayor. There are strong correlations between SIMCE test scores and other measures of municipal performance, such as the risk of crime or the maintenance of parks (see Appendix Table 1).

Figure 1: Distribution of Main Educational Outcomes



(a) Corrected Score



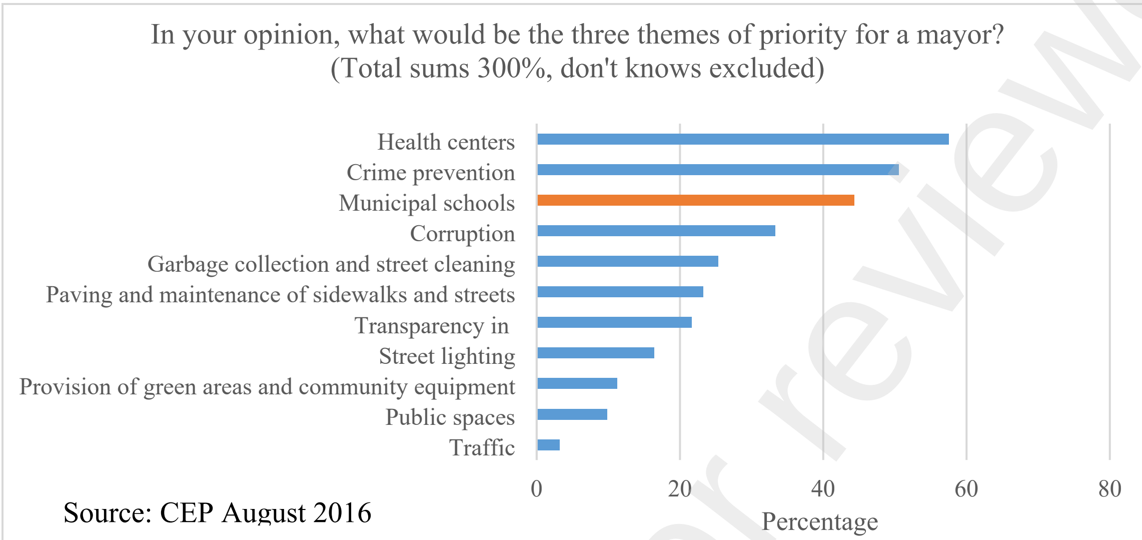
(b) Score Change

Notes: Distribution of outcomes at the municipal level. Corrected score controls for socioeconomic conditions, as explained in Section 3.1.

Education is a top priority issue in Chilean politics at both the national and local levels. For example, at the national level education ranked fourth on the country's main priorities for the government, and at the local level it ranked third among the priorities for a mayor, after health and crime prevention (see Figure 2). The priority given to municipal schools reveals that citizens already know that these are the mayor's responsibility, even if they are unaware of their performance.[7]

[6] SIMCE's scale has a mean of 250 points and a standard deviation of 50 points.
[7] This contrasts with the setting in Cruz et al. (2021), where voters had little information about the existence of the policy instruments on which politicians could exert effort. Therefore, information about their performance also informed participants of the existence of resources intended for the policy.

Figure 2: Voters' Priorities



We draw on two main sources for our electoral data. The first is the electoral roll, which is publicly available; it includes the names and addresses of all voters in each polling station. The second source is polling-station-level electoral results published by the National Electoral Office (SERVEL).

Chile is a great case for studying political accountability in education. Since the electoral roll is public, and election results are published at the polling station level, we can reach voters and study their behavior at a much smaller unit than in most studies. Meanwhile, public education is managed by local governments and voters consider it to be a main priority, and there is a reliable instrument that annually measures the quality of schools (SIMCE), which shows great variation across municipalities. While the public is generally familiar with SIMCE scores, previous evidence suggests that there are important information frictions in relation to educational outcomes at the school level (Allende et al. 2019).

2.2 Data

Our main data source is the Chilean electoral office, SERVEL. The polling-station-level data include the results of the 2016 local elections, and the age (in 5-year ranges) and gender composition of the polling stations.[8] The results for the 2012 local election are available for 94% of the polling stations; the rest of the polling stations were created after 2012 to include new voters who had turned 18 and those who changed municipalities between elections. We also use data from the 2013 and 2017 presidential elections as a robustness check.

The SIMCE test scores come from the Chilean Quality of Education Agency. The data used to correct for socioeconomic conditions—population, percentage of rural population, municipal students' vulnerability index, and the number of poor students in public schools—and come from the National System of Municipal Information (SINIM),[9] while the municipality type comes from SUBDERE (2005). The data used to measure voters' prior beliefs about the quality of education at the polling station level comes from the SIMCE parent surveys, and the Chilean Quality of Education Agency merged it with the electoral roll data.[10] Electoral campaign spending data comes from SERVEL. Table 2 presents selected summary statistics.

[8] The electoral law establishes that when two or more polling stations have fewer than 175 registered voters each, and no more than 350 in total, they may be merged. The electoral results of merged stations are calculated aggregately. Our sample contains 1,072 merged stations, which we treat as single stations. When a treated polling station is merged, we deal with the treatment variable as an intensity of treatment (i.e. 0.5 if one of two merged stations was treated). Merged stations in 2016 do not necessarily correspond to those from 2012. Thus, to construct the 2012 results for merged stations in 2016, we assume that the results in merged stations in 2012 were distributed evenly among the single stations that were merged, and then sum the results of the single stations that were merged in 2016. We demonstrate below that the results are robust to excluding merged stations.

[9] Available at www.sinim.gov.cl.

[10] Our measure of prior beliefs about education quality at the polling station level was constructed thanks to the collaboration of the Chilean Quality of Education Agency. We submitted the list of Chilean identification numbers by polling station, and they labeled them with fake ID numbers, linking parents with the encrypted IDs of students' SIMCE test scores.

Table 2: Summary Statistics

Panel A: Polling Station Level

	(1)				
	Observations	Mean	SD	p1	p99
Registered voters in 2016	22,385	333.93	21.85	256.00	439.00
Turnout in 2012 (%)	21,019	38.15	13.79	7.16	68.12
Incumbent Vote Share in 2012 (%)	21,019	19.72	8.62	3.14	42.66
Challenger Vote Share in 2012 (%)	21,019	11.82	5.93	1.17	27.86
Difference in Turnout 2016-2012	21,019	-8.13	7.09	-25.29	8.37
Difference in Incumbent Vote Share 2016-2012	21,019	-5.56	6.32	-20.78	9.27
Difference in Challenger Vote Share 2016-2012	21,019	-2.28	5.62	-14.26	14.60
Men's Share (%)	22,385	0.48	0.19	0.06	0.97
Age 18-30 (%)	22,385	25.00	28.47	1.17	98.54
Age 30-59 (%)	22,385	53.41	23.68	1.15	96.17
Age 60 and more (%)	22,385	21.59	15.80	0.00	76.90

Panel B: Municipality Level

	Observations	Mean	SD	p1	p99
Turnout in 2012 (%)	59	40.15	6.18	28.17	56.91
Incumbent Vote Share in 2012 (%)	59	20.83	5.21	11.74	37.28
Challenger Vote Share in 2012 (%)	59	11.98	4.04	4.42	20.87
Difference in Turnout 2016-2012	59	-8.03	3.11	-13.70	-0.48
Difference in Incumbent Vote Share 2016-2012	59	-5.68	5.05	-14.13	11.90
Difference in Challenger Vote Share 2016-2012	59	-1.54	4.77	-9.05	10.42
Corrected Score	59	-0.98	8.72	-18.31	20.98
Score Change	59	-1.51	5.99	-12.29	19.00
Vote Share Left (%)	58	42.70	14.45	9.48	73.46
Vote Share Right (%)	57	34.53	16.93	3.90	75.13

Notes: Table 2 presents the number of valid observations (column 1), mean (column 2), standard deviation (column 3), percentile 1 (column 4) and percentile 99 (column 5) for each variable in the study sample. Vote shares are calculated over registered voters so that they are comparable to turnout. Differences in variables between 2016 and 2012 are in percentage points. *Corrected Score* and *Score Change* were constructed based on information provided by SINIM. All other variables are calculated based on data from SERVEL. Turnout, vote shares and their differences, and demographics are expressed as percentages.

3. Research Design

3.1 The treatments

The intervention consisted of providing voters with information on the educational outcomes of local public schools to assess how that affected electoral outcomes in Chilean local elections in 2016. The information was provided in a letter that was sent to arrive about a week before Election Day (October 23). It was sent to all voters in 400 randomly selected polling stations (out of 22,385) in urban municipalities where the mayor was running for reelection. The letter included information on the test scores of local municipal schools in both levels and changes, as well as one of two benchmarks for both levels and changes—the outcomes of either the average municipality (average treatment, T^{ave} hereafter) or best municipality (maximum treatment, and T^{max} hereafter), which was randomized at the polling station level. We sent a total of 128,033 letters.

On average, polling stations have 334 registered voters, whose addresses are publicly available. *Correos de Chile*, the national post office, printed and mailed the letters.[11] They were delivered within a 5-business day window, starting on October 12, i.e. 11 days before Election Day.

The appendix presents two examples of the letters sent to voters (in Spanish), one for the average treatment and one for the maximum treatment (Appendix Figures 2 and 3). The letters included the voter's name and address. The heading of the letter read: *"Sunday, October 23 is Election Day. The municipality is responsible for the administration of municipal schools. These are the results of municipal schools in your municipality in the SIMCE 4th grade test, which measures learning outcomes."* Thus, in the first place, the letter informs the recipient of the upcoming election. It also reported two measures of relative performance of the voter's municipality, one for levels and one for changes. We provided information on these two dimensions because both are relevant to assessing a mayor's performance; this was confirmed in a pre-test of the letter.

For the *levels* of SIMCE results, we corrected the 4th grade test scores to reflect the value added by schools by controlling for a set of socioeconomic outcomes using the following equation:

$$SIMCE_i = \beta_0 + \beta_3 Pop_i + \beta_4 Pop_i^2 + \beta_5 Rur_i + \beta_6 Rur_i^2 + \beta_7 Poor_i + \beta_8 Poor_i^2 + \sum \beta_{9k} Type_k + e_i,$$

[11] *Correos de Chile* is an autonomous public firm, with a politically independent board.

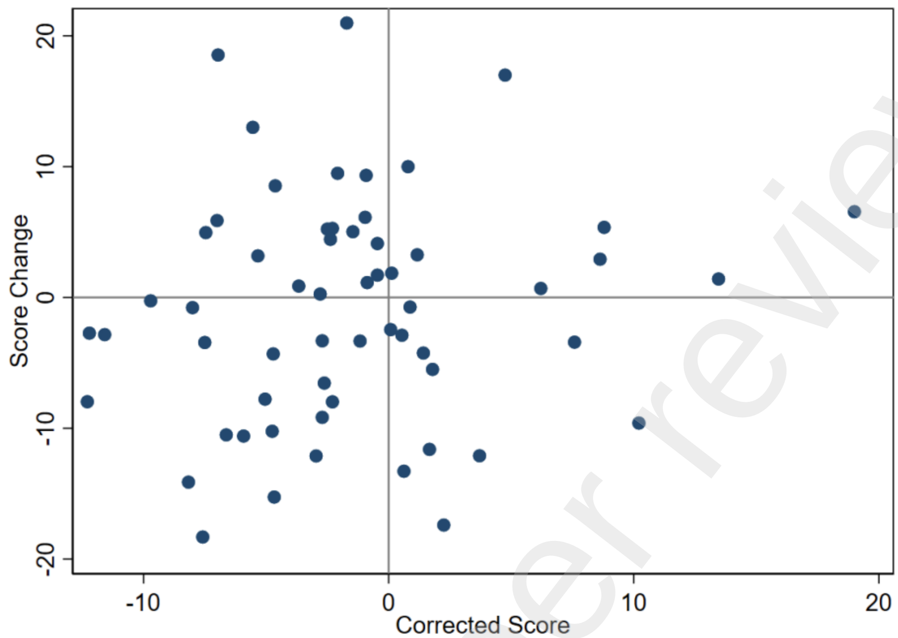
where, for municipality i, $SIMCE_i$ is the SIMCE score, Vul_i corresponds to the students' vulnerability index, Pop_i to population, Rur_i to the percentage of the population that lives in rural areas, $Poor_i$ to the number of poor students in public schools, and $Type_i$ to the municipality type according to the central government's classification k.[12] We proxy a school's "value-added" using the residuals of this regression, e_i. Residuals from this regression constitute a measure of the performance of local public schools after controlling for municipality socioeconomic and demographic characteristics to make it comparable across municipalities.[13] In the paper, we call this measure the "corrected score" (or simply, "score"). When we use this measure in the letter, the benchmark refers to "comparable municipalities."

For the *changes* in SIMCE results, we used 4th grade average scores from 2013 to 2015 (i.e. three-fourths of the current electoral period in 2016) and subtracted the average of the previous mayoral period (2009–2012).[14] This is a measure of improvement in the quality of education provided by the local government, and in the paper we call it "score change" (or Δ *Score*).

Figure 3 shows the distribution of SIMCE-corrected scores and score changes for the municipalities in the sample. It clearly illustrates that the two measures provide different types of information. Both measures are uncorrelated with the political coalition of the municipality's mayor (see Appendix Figure 4). Therefore, providing information on these measures did not favor any particular coalition.

[12] We follow the classification of municipalities developed by the Secretariat for Regional and Administrative Development (Subdere). Municipalities are classified into eight categories based on population, percentage of rural population, poverty rate, and the income share coming from the Municipal Common Fund (Subdere, 2005).
[13] Appendix Table 2 presents the results of the regression used to generate the residuals. The R^2 of the regression is 0.22.
[14] Data for the last year of the current mayoral period was not available at the time of the experiment.

Figure 3: Correlations between Corrected Scores and Changes in Scores for the mayoral term



The average treatment included a graph displaying the municipality's performance and that of the *average* (comparable) municipality (see Appendix Figure 2). The maximum treatment used a more demanding benchmark: it pitted the municipality's performance against that of the *best* (comparable) municipality (Appendix Figure 3). For both treatments, the graph offered a visual representation of the distance between the voter's municipality and the provided benchmark, and it was scaled so that it also provided an idea of the municipality's distance to the best and worst performing municipalities.

We pretested the letter with focus groups and in surveys focusing on people with a low educational level, in collaboration with an independent consulting agency (MANO A MANO Consulting). We tried several iterations until we were confident that people understood the information and seemed to care about it. We also confirmed that people trusted the source of the information cited in the letter (the Pontifical Catholic University of Chile).

3.2 Sample and Randomization

The study focuses on urban municipalities in which the mayor ran for reelection in 2016. We define urban municipalities as those with a population greater than 50,000 and with fewer than 20% of the population living in a rural area. We restrict our sample to urban municipalities for two reasons: education data is more reliable for larger municipalities, and they have more polling stations, making spillovers less of an issue.[15] Our sample contains 59 municipalities out of 345.[16] The municipalities in our sample have 101–1,168 polling stations, with an average of 379.

We assigned treatment at the polling station level, stratified by municipality and by the gender composition of polling stations (in terciles of the percentage who are female). Each municipality in the sample contains six or seven treated polling stations (3–4 in each treatment arm).

The randomization was implemented using two different dimensions to define the strata:

1. First, we treated at least three polling stations for each treatment in each municipality (the maximum integer divisor of 200—the total number of polling stations assigned to each treatment arm—among 59 is 3). Using this procedure, we assigned treatment status to 354 polling stations.
2. In each municipality, we define three strata based on the polling station's gender composition. One polling station from each treatment arm was assigned to each stratum in every municipality.
3. The 46 remaining stations were assigned according to the following procedure:

i) No more than one extra polling station was assigned to any municipality.

ii) We ensured balance across the three gender-composition strata defined at the national level, so that two of the gender strata received eight extra stations in each treatment arm, and one was randomly selected to receive only seven.

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[15] We excluded one additional municipality because the value-added estimations of test scores were not robust to different specifications. Formally, the condition was that the maximum difference across specifications could not exceed 10 test score points (i.e., 20% of a standard deviation of the score at the student level). The specifications considered at this stage excluded the different controls sequentially, keeping the students' vulnerability index across all specifications.

[16] The municipalities in the sample are: Alto Hospicio, Antofagasta, Buin, Calama, Calera, Cerrillos, Cerro Navia, Chiguayante, Chillán, Colina, Concepción, Concón, Copiapó, Coronel, Coyhaique, Curicó, El Bosque, Estación Central, Huechuraba, La Cisterna, La Florida, La Granja, La Reina, La Serena, Lo Barnechea, Lo Espejo, Macul, Maipú, Osorno, Padre Hurtado, Pedro Aguirre Cerda, Penco, Peñaflor, Peñalolén, Providencia, Pudahuel, Puente Alto, Puerto Montt, Punta Arenas, Quilicura, Quillota, Quilpué, Quinta Normal, Rancagua, Recoleta, San Antonio, San Bernardo, San Felipe, San Joaquín, San Miguel, San Pedro de la Paz, San Ramón, Temuco, Tomé, Valdivia, Valparaíso, Villa Alemana, Viña del Mar, and Vitacura.

iii) We ensured balance across educational performance at the municipality level. We classified municipalities into four groups according to whether their performance was above or below average in both level and changes. Each of these groups received the eight extra stations, except for one randomly selected group that received seven.

3.3 Estimation Framework

The treatments used in this paper create two differences sources of variation in terms of information provision to voters: (i) *within-municipality variation*: a (small) share of voters receive the report cards and (ii) *between-municipality variation*: the content of the reports varies across municipalities because public schools in different municipalities have different educational outcomes. The content of the treatment letters, i.e. the levels and changes of test scores, are part of the essence of the treatment: we expect voters' responses to depend critically on them. Thus, our main interest lies in the coefficients of the interactions between the treatments and the content provided in the letters ($Score_m$ and $\Delta Score_m$). This implies that the main estimation framework we use in the paper is represented by the following equation:

$$y_{sm} = a + g * T_{sm}^{ave} + b * T_{sm}^{max} + c * Score_m * T_{sm}^{ave} + d * Score_m * T_{sm}^{max} + e * \Delta Score_m * T_{sm}^{ave} + f * \Delta Score_m * T_{sm}^{max} + X'_{sm}\mu + t_m + u_{sm} \qquad (1)$$

where y is the baseline level of an outcome variable in polling station s in municipality m, T_{sm}^{ave} is a treatment indicator for receiving information on mayoral outcomes using average performance as the benchmark, T_{sm}^{max} is a treatment indicator for receiving information on maximum performance as the benchmark, *Score* is corrected SIMCE score (as defined in Section 3.1), Δ*Score* is the change in the mayoral term, X'_{sm} includes voting booth controls (fraction of voters in the 18-30 age range, fraction of voters in the 30-65 age range, fraction of voters older than 65 years old and municipality-gender strata fixed effects), and t captures municipality fixed effects. Coefficients g and b capture the causal effects for groups receiving the letter with the average and maximum treatments, respectively (i.e., when *Score* and *ΔScore* are equal to 0). In turn, c and e (d and f) capture causal effects related to the content of the letter in the average (maximum) treatment.

Notice that equation (1) includes municipality fixed effects (t_m) in order to isolate between-municipality variation. Thus, note that, while *Score* and *ΔScore* vary at the municipality level, the main coefficients of interest (*c, d* , *e*, and *f*) are estimated by a combination of random variation at the polling station level (T^{ave} and T^{max}) with between-municipality variation.

The last point poses an empirical challenge because, for people receiving the letter, we do not vary the content of the letter in a random way, as it varies at the municipality level (public school outcomes vary across municipalities). Thus, the between-municipality identification may be driven by confounding variables correlated with the content of the report cards. We deal with this issue in two ways in the empirical exercises. First, notice that if we find different results for coefficients *c* and *d*, it is highly unlikely that the identification comes from some confounding factor that varies at the municipality level and is unrelated to the actual contents of the cards. We check this point in the regressions. Second, we present a robustness exercise in which we add interactions of (T^{ave} and T^{max}) and 11 different municipality level variables to the estimated equation, capturing variation in a number of social, economic, and political dimensions.

As in any randomization, we now check balance in multiple dimensions. Notice that testing balance taking into account the sources of variation identified in equation (1) entails considering variation both within municipalities (i.e., T^{ave} and T^{max}) and across municipalities (in *Score* and *ΔScore*). Table 3 presents the results of the balance tests for different variables. Panel A considers age at the polling station level (the only demographic variables available to us, besides gender, which we used to define the randomization strata), in which we find no significant coefficient. Panel B presents the results for variables in the 2012 municipal election, which reveal that one coefficient (*c*) is different from zero for two key outcome variables, turnout and incumbent vote share. This indicates that the control and treatment groups are not balanced in this dimension, which combines a variable that varies within municipalities (T_{sm}^{ave}) with one that varies across municipalities (*Score*). The random treatment assignment resulted in treated polling stations in municipalities with higher average SIMCE scores having significantly lower turnout and votes for the incumbent. This imbalance does not translate into differences in the number of registered voters or the political leanings of electoral results.

Table 3: Balance Tests

Panel A: Demographics

	(1) Age 18-30	(2) Age 30-59
Average Treatment	2.727	-1.786
	(2.008)	(1.737)
Average Treatment × Corrected Score	0.272	-0.179
	(0.232)	(0.189)
Average Treatment × Score Change	0.388	-0.286
	(0.358)	(0.304)
Maximum Treatment	1.964	-1.785
	(2.089)	(1.783)
Maximum Treatment × Corrected Score	-0.041	0.022
	(0.184)	(0.167)
Maximum Treatment × Score Change	0.146	-0.098
	(0.385)	(0.336)
Observations	22,385	22,385
R-squared	0.179	0.124

Panel B: Previous Municipal Election Outcomes (2012)

	(1) Registered	(2) Turnout	(3) Vote Share Incumbent	(4) Vote Share Challenger	(5) Vote Share Left	(6) Vote Share Right
Average Treatment	2.309	-1.025	-0.512	-0.326	-0.347	-0.241
	(1.871)	(0.867)	(0.541)	(0.308)	(0.494)	(0.476)
Average Treatment × Corrected Score	0.131	-0.308***	-0.213***	-0.057	-0.064	0.044
	(0.253)	(0.111)	(0.072)	(0.040)	(0.056)	(0.047)
Average Treatment × Score Change	-0.072	-0.136	-0.067	-0.046	0.039	-0.081
	(0.160)	(0.161)	(0.110)	(0.054)	(0.079)	(0.087)
Maximum Treatment	2.581	-0.428	-0.432	0.129	0.212	-0.498
	(1.621)	(0.872)	(0.526)	(0.328)	(0.424)	(0.491)
Maximum Treatment × Corrected Score	-0.078	0.037	0.013	0.041	0.065	-0.064
	(0.183)	(0.095)	(0.057)	(0.037)	(0.042)	(0.050)
Maximum Treatment × Score Change	0.123	0.137	0.064	0.047	-0.063	0.042
	(0.181)	(0.161)	(0.113)	(0.053)	(0.066)	(0.086)
Observations	21,029	21,019	21,019	21,019	20,534	20,350
R-squared	0.235	0.377	0.468	0.496	0.795	0.872

Panel C: Previous Presidential Election Outcomes (2013)

	(1) Turnout	(2) Vote Share Incumbent Candidate Party	(3) Vote Share Challenger Candidate Party
Average Treatment	-0.855	-0.816	0.442
	(0.788)	(0.602)	(0.561)
Average Treatment × Corrected Score	-0.261**	-0.222***	-0.072
	(0.103)	(0.069)	(0.061)
Average Treatment × Score Change	-0.299**	-0.323***	0.001
	(0.141)	(0.123)	(0.122)
Maximum Treatment	-0.872	-0.235	-0.366
	(0.792)	(0.626)	(0.469)
Maximum Treatment × Corrected Score	0.040	-0.007	0.016
	(0.080)	(0.054)	(0.052)
Maximum Treatment × Score Change	0.171	0.064	-0.049
	(0.141)	(0.150)	(0.083)
Observations	21,109	18,479	18,479
R-squared	0.343	0.654	0.640

Panel D: Subsequent Presidential Election Outcomes (2017)

	(1) Turnout	(2) Vote Share Incumbent Candidate Party	(3) Vote Share Challenger Candidate Party
Average Treatment	-0.772	-0.067	-0.360
	(0.640)	(0.613)	(0.375)
Average Treatment × Corrected Score	-0.160**	-0.128***	-0.056
	(0.071)	(0.048)	(0.042)
Average Treatment × Score Change	-0.205	-0.204	-0.077
	(0.132)	(0.161)	(0.078)
Maximum Treatment	-1.007	-0.245	-0.648*
	(0.647)	(0.538)	(0.340)
Maximum Treatment × Corrected Score	0.019	-0.028	0.028
	(0.071)	(0.050)	(0.038)
Maximum Treatment × Score Change	0.112	0.002	0.033
	(0.119)	(0.133)	(0.056)
Observations	20,557	17,743	17,743
R-squared	0.363	0.650	0.578

Notes: Panels A, B, C and D present the estimation of equation 1 (excluding demographic controls) for different sets of variables. In *Panel A*, demographic characteristics at the polling station level include: share of registered voters aged 18–30 (column 1) and 31–59 (column 2).

In *Panel B*, electoral outcomes of the previous municipal election: number of registered voters in the polling station (column 1), turnout rate (column 2), vote shares of the incumbent (column 3), challenger (column 4), left-wing coalition (column 5) and right-wing coalition (column 6).

In *Panel C*, electoral outcomes from the previous presidential election (2013): turnout rate (column 1), and vote shares of the incumbent mayor's party (column 2) and the challenger's party (column 3).

In *Panel D*, electoral outcomes of the subsequent presidential election (2017): turnout rate (column 1), and vote shares of the incumbent mayor's party (column 2) and the challenger's party (column 3).

All estimates include municipality fixed effects. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

To determine whether these findings apply only to the 2012 municipal election or more broadly, we also estimate equation (1) but for the 2013 presidential election.[17] We present the results in Panel C. Again, we find that the estimates of c for turnout and the incumbent's share are negative and have a point estimate very similar to the ones observed for the 2012 municipal election. Finally, Panel D presents the results for the 2017 presidential election. Certainly, it is unlikely that a letter received in 2016 informing voters about the quality of municipal schools in the context of local elections affected the presidential election the following year. Indeed, we again find a negative and significant effect. Thus, our randomly selected treatment polling stations appear to have had a stable feature (those in municipalities

[17] We construct a proxy for the share of support for the incumbent mayor in the presidential election by imputing the support for the presidential candidate from the mayor's political coalition. For this purpose, we focus on municipalities with an incumbent mayor from the two main political coalitions in Chile. We classify presidential candidates into these two coalitions, and define voting for the incumbent coalition as voting for a presidential candidate from the same coalition as the incumbent mayor.

with higher SIMCE scores had lower turnout and votes for the incumbent) that was present in two elections before the treatment and one presidential election afterwards: we were truly unlucky. Overall, the control and treatment polling stations do not seem different in most regards, except for a systematic imbalance in the non-experimental elections. These results suggest we are in the presence of parallel trends and therefore we can estimate the following equation:

$$\Delta y_{sm} = \alpha + \theta * T_{sm}^{ave} + \kappa * T_{sm}^{max} + \beta * Score_{m} * T_{sm}^{ave} + \gamma Score_{m} * T_{sm}^{max} \\ + \sigma \Delta Score_{m} * T_{sm}^{ave} + \Psi * \Delta Score_{m} * T_{sm}^{max} + X'_{sm}\mu + \tau_m + \varepsilon_{sm}, \quad (2)$$

which follows the same structure as equation (1) but with change in y between 2016 and 2012 (instead of y) as the left-hand-side variable, and with a vector of controls (including the age structure of the polling station).[18]

The estimates from equation (2) correspond to the intention to treat estimates, as they capture the effect of sending the letter to voters in the relevant polling stations. Note that some of the addresses may have been incorrect, and some of the letters may have been received by other persons or simply not read.[19] In addition, we are aware of a limited degree of spillovers.[20]

[18] Since we use two municipal elections, this is equivalent to a difference-in-differences estimator, except that the control variables are not in changes (we lack data on all controls for both years, but they are likely to be relatively stable).

[19] During the 2016 election there was a public controversy between the Electoral Office and the Civil Registrar Service, which provides the Electoral Office with information on address changes, because 3.5% of registered voters were the subject of involuntary electoral address changes. See http://www.t13.cl/noticia/politica/registro-civil-atribuye-cambios-involuntarios-domicilio-electoral-ajustes-informaticos. Unfortunately, the Chilean Postal Service was unable to provide data on the actual delivery of letters to treated voters. In an attempt to quantify the relevance of this phenomenon, we use data from other sources to document that address changes are rare in Chile: 78% of the population in the sample municipalities has lived in the same house for at least 5 years, and 58% have done so for at least 10 years (Minvu 2015). Importantly, there are no relevant differences in these figures by the municipality's income level or by gender, although differences in age are somewhat larger (Minvu 2015 and Casen 2017). The relative similarity across key variables reduces concerns about possible biases due to selective take-up. Finally, the electoral roll likely correctly reflected a relevant share of household mobility. In the treated municipalities, an average of 9% of 2012 voters changed their registration to another municipality between 2012 and 2016. All in all, these exercises suggest that the partial take-up of the treatment due to changes in address is probably a minor issue and is not strongly related to voters' observable characteristics.

[20] We sent the letters to a small share of polling stations in each municipality (0.6–7%, mean of 1.8%) and just a few days before the election to avoid spillovers. Still, we are aware that in a few municipalities the report cards were disseminated through social networks. There must have been some word-of-mouth dissemination as well, especially members of the same household, who generally vote at different polling stations. This implies that some voters in the control group may have actually received the treatment. However, only 9% of Chilean people often follow political issues in social networks, and 60% say they never talk about politics with either family or friends (CEP 2018). An analysis of a sample of 40,000 tweets during the period of our intervention using the Harvard CGA Geotweet Archive revealed no tweets referring to our experiment.

Both phenomena—i.e. possible problems with take-up and spillovers—imply the estimates are not the same as the effects of the actual letter, although the spillovers are probably not large enough to importantly affect the estimation (Sävje et al. 2019). In any case, both problems imply attenuation bias, meaning that estimates from equation (2) probably correspond to lower bounds of the true effects of the treatment.

A final concern regarding the identification of effects is incumbent mayors' reaction to the provision of information. However, we do not think this is a relevant threat for three reasons. First, incumbent mayors could have responded to the intervention by buying votes in treated polling stations, which would counteract the effects of the information shock, as found by Cruz et al. (2018). A strong vote-buying response seems unlikely as this is an uncommon strategy: only 5.5% of Chileans report having been offered a material benefit in exchange for their vote, ranking next to last in the Americas (Faughnan and Zechmeister 2011). Second, mayors could have targeted their campaigns to treated voters. However, mayors' responses are less likely in our case than in a village-level intervention, as in Cruz et al. (2018), since mayors would have had trouble identifying the 0.6–7% of voters who received the letter, as they could live anywhere in the municipality. Finally, the letters started to arrive just a week before Election Day, and Chilean law prohibits campaigning within 3 days left of an election, which left little time to react.

4. Results

This section presents our main results, focusing on three dependent variables at the polling station level—voter turnout, support for the incumbent, and support for the main challenger[21]—all measured as the share of total registered voters in the polling station. Next, we present a few robustness checks, extensions, and heterogeneity exercises.

[21] Appendix Table 3 shows that the treatments have no effect on the number of invalid votes. This implies that support for *all* the challengers can be recovered as the difference in the effect on turnout minus the effect on incumbent support.

4.1 Main Results

Table 4 presents the main results. We analyze the effects on voter turnout in columns (1) to (5). We take a step back from equation (2) for ease of interpretation: instead of estimating all the effects included in the equation, we start by estimating the direct effects of sending the letters without considering the impact of their content (i.e. assuming that $\theta=\kappa$ and $\beta=\gamma=\sigma=\Psi=0$). We do not find a statistically significant effect. In column (2) we allow for different effects of T^{max} and T^{ave} (still assuming that $\beta=\gamma=\sigma=\Psi=0$). While the effect of T^{ave} seems to be stronger, neither of the effects is statistically significant. The null findings for any of the letters *per se* are consistent with the fact that, depending on the municipality's outcomes, the letters convey very different messages that, on average, do not change voters' behavior. These results mimic Ferraz and Finan (2008), who find that audits have no effect on average, it depends on what information the audits give.

Next, we study the effects of T^{ave} in column (3), considering both its direct effects and the effects of the letter's content. We find that voters react to information about the corrected score by increasing turnout as SIMCE scores increase, which is significant at the 99% level (with no significant direct or indirect effects through the score change). The case of T^{max}, reported in column (4), is different, but again we find no effects. The results of the effects of T^{ave} are confirmed when we estimate all the elements of equation (2) together in column (5). Overall, these results imply that the extensive margin of voter turnout responds to the educational outcomes of the incumbent mayor's tenure, although voters only react to this information when provided with an average benchmark and levels of test scores (not changes).

Table 4: Main Results

	Turnout					Incumbent Share					Challenger Share				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Any Treatment	0.345					0.193					0.050				
	(0.299)					(0.197)					(0.165)				
Average Treatment		0.622	0.620		0.620		0.266	0.333		0.335		0.210	0.147		0.147
		(0.395)	(0.400)		(0.400)		(0.264)	(0.280)		(0.280)		(0.236)	(0.256)		(0.256)
Maximum Treatment		0.064		0.042	0.048		0.182		0.153	0.156		-0.090		-0.019	-0.017
		(0.436)		(0.431)	(0.431)		(0.293)		(0.299)	(0.299)		(0.233)		(0.244)	(0.244)
Average Treatment × Corrected Score			0.112***		0.111***			0.098***		0.098***			0.008		0.008
			(0.041)		(0.041)			(0.031)		(0.031)			(0.027)		(0.027)
Average Treatment × Score Change			-0.058		-0.058			-0.010		-0.010			-0.040		-0.040
			(0.073)		(0.073)			(0.052)		(0.052)			(0.048)		(0.048)
Maximum Treatment × Corrected Score				-0.013	-0.012				-0.014	-0.012				0.005	0.005
				(0.048)	(0.048)				(0.033)	(0.033)				(0.026)	(0.026)
Maximum Treatment × Score Change				-0.002	-0.003				-0.009	-0.009				0.047	0.047
				(0.090)	(0.090)				(0.068)	(0.068)				(0.043)	(0.043)
Observations	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019
R-squared	0.396	0.396	0.396	0.396	0.396	0.623	0.623	0.623	0.623	0.623	0.667	0.667	0.667	0.667	0.667

Notes: This table presents the results of estimating several variations of equation (2) for differences between the 2016 and 2012 municipal elections. Columns (1) to (5) present estimates on differences in turnout, columns (6) to (10) report estimates on differences in the incumbent vote share, and columns (11) to (15) present estimates for differences in the challenger vote share.

Columns (1), (6) and (11) have as the main regressor a dummy variable equal to 1 if the polling station was assigned to any treatment. Columns (2), (7) and (12) have as the main regressors one dummy variable equal to 1 if the polling station was assigned to the Average Treatment, and a second dummy equal to 1 if the polling station was assigned to the Maximum Treatment. Columns (3), (8) and (13) have as the main regressors the Average Treatment dummy plus interactions of this dummy with both the corrected score variable and the change in score. Columns (4), (9) and (14) have as the main regressors the Maximum Treatment dummy plus interactions of this dummy with both the corrected score variable and the change in score. Finally, columns (5), (10) and (15) present the results of estimating equation (2), so they include as the main regressors both treatment dummies and their interactions with corrected score and score change.

All estimates include municipality fixed effects. Controls include the gender and age composition of the polling station. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Columns (6) to (15) present the results for the treatments' effects on support for the incumbent and main challenger. This allows us to decompose the effect on the extensive margin into support for the two main candidates. The results for incumbent support mainly match the effect on turnout, also significant at the 99% level, which implies that almost all the effects on the extensive margin translate into more or fewer votes for the incumbent, with no clear effect on support for the main challenger. It is remarkable that support for the challenger does not change after receiving news about the performance of the current mayor, suggesting that local elections featuring an incumbent mayor serve more as a referendum on the mayor's performance than as a contest between the mayor and the challenger.[22]

[22] One interpretation of these results relates to the contrasting findings in Ferraz and Finan (2008) and Chong et al. (2015) about the effect of information about corruption audits on voter turnout and incumbent support.

Our results on the electoral effects of information are both statistically and economically significant. A 10-point increase in the corrected SIMCE score (note that 14 municipalities in our sample experienced score changes of at least 10 points during the previous mayoral term) on average raises turnout by 1.1 percentage points and support for the incumbent by 0.98 points. These are important effects: in two of the 59 municipalities in our sample, the margin of victory was less than 2 percentage points, while in six it was less than 3 points. Also recall that, due to limited take-up and spillovers, our estimates are lower bounds.

Interestingly, the fact that we only find effects for the interaction of T^{ave} with corrected SIMCE (and not for the interaction of the same variable with T^{max}) suggests that the results are not driven by interactions of T^{ave} with some unobserved variable that varies at the municipality level.

We further test the relevance of potential confounding variables that interact with treatment to explain our results in Table 5. Here we present the specifications for turnout and incumbent share presented in columns (5) and (10) of Table 4 (in column (1) of both panels) and then we add interactions of T^{ave} and T^{max} with 11 different municipality-level variables including population size, the share of students in public schools, municipality income (per-capita), whether the mayor belongs to the right wing coalition, vulnerability index, number of poor students in public schools, public health centers per capita, maintained parks per capita, expenditure in parks per capita, an index of crime risk, and incumbent candidate's electoral expenditure.[23] Results show that the point estimates of the interactions of T^{ave} and corrected SIMCE (and in most cases also their significance) do not change, suggesting that our results are not driven by confounding factors but truly by the content of the letters.

While the former paper finds that where voting is mandatory, this information has no effect on turnout, the latter identifies a significant effect on turnout where voting is voluntary. Thus, one possibility is that the strong effect on turnout we find is due to our voluntary voting context.

[23] We do not include the exercises for the challenger's vote share to save space.

Table 5: Robustness: Including Interactions with Municipality-Level Controls

Panel A: Total Participation

	Baseline	Population	Share students in public schools	Income	Right Wing mayor	Vulnerability Index	Number of poor students	Public health centers per capita	Mantained parks per capita	Expenditure in parks per capita	Crime	Electoral expenditure by incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Average Treatment	0.620	0.450	0.440	1.001	0.766	-0.988	0.034	0.620	1.156	1.451	1.034	0.132
	(0.400)	(0.619)	(1.105)	(0.707)	(0.500)	(3.221)	(0.752)	(0.400)	(0.872)	(0.919)	(0.792)	(0.897)
Maximum Treatment	0.048	-0.933	-0.554	-0.885	0.531	7.790**	-0.761	0.048	-1.349	-1.519	1.536**	0.277
	(0.431)	(0.662)	(1.152)	(0.775)	(0.510)	(3.679)	(0.866)	(0.431)	(1.246)	(1.052)	(0.740)	(1.089)
Average Treatment × Corrected Score	0.111***	0.111***	0.110***	0.117***	0.114***	0.116***	0.108***	0.111***	0.096**	0.092**	0.108***	0.109***
	(0.041)	(0.041)	(0.042)	(0.042)	(0.042)	(0.043)	(0.041)	(0.041)	(0.043)	(0.043)	(0.042)	(0.042)
Maximum Treatment × Corrected Score	-0.012	-0.018	-0.018	-0.026	-0.005	-0.041	-0.017	-0.012	0.008	0.018	-0.026	-0.011
	(0.048)	(0.049)	(0.048)	(0.049)	(0.048)	(0.050)	(0.049)	(0.048)	(0.048)	(0.049)	(0.046)	(0.047)
Average Treatment × Score Change	-0.058	-0.062	-0.055	-0.057	-0.053	-0.067	-0.071	-0.058	-0.049	-0.041	-0.050	-0.057
	(0.073)	(0.075)	(0.075)	(0.074)	(0.072)	(0.073)	(0.076)	(0.073)	(0.074)	(0.074)	(0.074)	(0.073)
Maximum Treatment × Score Change	-0.003	-0.026	0.009	-0.018	0.019	0.046	-0.022	-0.003	-0.010	-0.008	0.027	-0.003
	(0.090)	(0.088)	(0.094)	(0.090)	(0.087)	(0.095)	(0.090)	(0.090)	(0.090)	(0.091)	(0.089)	(0.090)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,019	21,019	21,019	20,066	21,019	21,019	21,019	21,019	20,921	19,446	21,019	21,019
R-squared	0.396	0.396	0.396	0.391	0.396	0.396	0.396	0.396	0.395	0.417	0.396	0.396

Panel B: Incumbent Vote Share

	Baseline	Population	Share students in public schools	Income	Right Wing mayor	Vulnerability Index	Number of poor students	Public health centers per capita	Mantained parks per capita	Expenditure in parks per capita	Crime	Electoral expenditure by incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Average Treatment	0.335	0.401	0.184	0.629	0.496	-0.989	0.200	0.335	1.241*	0.597	0.588	-0.054
	(0.280)	(0.444)	(0.788)	(0.523)	(0.354)	(2.141)	(0.548)	(0.280)	(0.640)	(0.789)	(0.458)	(0.574)
Maximum Treatment	0.156	-0.038	-0.063	-0.383	0.289	4.396	-0.093	0.156	0.101	-0.797	0.417	0.699
	(0.299)	(0.455)	(0.893)	(0.644)	(0.354)	(3.005)	(0.585)	(0.299)	(0.977)	(0.902)	(0.547)	(0.673)
Average Treatment × Corrected Score	0.098***	0.098***	0.096***	0.101***	0.101***	0.102***	0.097***	0.098***	0.083***	0.102***	0.096***	0.096***
	(0.031)	(0.031)	(0.030)	(0.031)	(0.031)	(0.031)	(0.030)	(0.031)	(0.032)	(0.032)	(0.031)	(0.031)
Maximum Treatment × Corrected Score	-0.012	-0.013	-0.015	-0.021	-0.011	-0.028	-0.014	-0.012	-0.006	-0.005	-0.015	-0.010
	(0.033)	(0.033)	(0.034)	(0.032)	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)	(0.034)	(0.033)	(0.032)
Average Treatment × Score Change	-0.010	-0.009	-0.008	-0.001	-0.005	-0.017	-0.013	-0.010	-0.004	-0.009	-0.006	-0.009
	(0.052)	(0.052)	(0.052)	(0.051)	(0.050)	(0.052)	(0.053)	(0.052)	(0.052)	(0.052)	(0.053)	(0.052)
Maximum Treatment × Score Change	-0.009	-0.014	-0.005	-0.004	-0.003	0.018	-0.015	-0.009	-0.013	-0.006	-0.004	-0.009
	(0.068)	(0.069)	(0.071)	(0.073)	(0.067)	(0.073)	(0.070)	(0.068)	(0.068)	(0.070)	(0.069)	(0.069)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,019	21,019	21,019	20,066	21,019	21,019	21,019	21,019	20,921	19,446	21,019	21,019
R-squared	0.623	0.623	0.623	0.590	0.623	0.623	0.623	0.623	0.623	0.639	0.623	0.623

Table 6 presents the results for an asymmetric specification in which we allow the effects to be different for "good" and "bad" educational results, defined as whether they are above or below zero (recall that the corrected score has a mean of zero).[24] The results imply that relaying bad results has a much stronger effect than communicating good results. Reporting a corrected SIMCE score of -10 in the treatment letter generates a 1.7-percentage-point reduction in turnout, which translates almost one-to-one into less support for the incumbent. These results are more than 50% greater than the effects estimated above for the symmetric specification, which assumes that good and bad information has equal effects. The effects of reporting a positive corrected score are not statistically different from zero. These results imply that voters respond much more to bad results than to good ones, as has been found elsewhere (Cruz et al. 2018). This finding is consistent with the fact that individuals tend to adjust their expectations upward faster than downward (Duesenberry 1949; Burchardt 2005; Ward 2015), and that they tend to be more averse to losses than to corresponding gains (Kahneman and Tversky 1979).

[24] The asymmetric specification is of the form:

$$y_{sm} = \alpha + \theta * T^{ave}_{sm} + \kappa * T^{max}_{sm} + \beta_1 * Score^{+}{}_m * T^{ave}_{sm} + \beta_2 * Score^{-}{}_m * T^{ave}_{sm} + \gamma_1 Score^{+} * T^{max}_{sm} + \gamma_2 Score^{-} * T^{max}_{sm} + \sigma_1 \Delta Score^{+}{}_m * T^{ave}_{sm} + \sigma_2 \Delta Score^{-}{}_m * T^{ave}_{sm} + \Psi_1 * \Delta Score^{+} * T^{max}_{sm} + \Psi_2 * \Delta Score^{-} * T^{max}_{sm} + X'_{sm}\mu + \tau_m + \varepsilon_{sm}$$

Where $Score^{+}{}_m$ and $\Delta Score^{+}{}_m$ correspond to $Score_m$ and $\Delta Score_m$, respectively, if $Score_m$ and $\Delta Score_m$are nonnegative, and 0 otherwise; and $Score^{-}{}_m$ and $\Delta Score^{-}{}_m$ correspond to $Score_m$ and $\Delta Score_m$, respectively, if $Score_m$ and $\Delta Score_m$are negative, and 0 otherwise.

Table 6: Asymmetric Effects

	Turnout			Incumbent Vote Share			Challenger Vote Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Intercepts									
Average Treatment	1.191*	0.326	0.826	0.747*	-0.049	0.346	0.521	0.200	0.538
	(0.618)	(0.631)	(0.787)	(0.418)	(0.397)	(0.511)	(0.383)	(0.399)	(0.516)
Maximum Treatment	-0.094	0.229	0.080	0.048	0.417	0.296	0.233	-0.386	-0.098
	(0.755)	(0.685)	(0.794)	(0.486)	(0.469)	(0.518)	(0.387)	(0.374)	(0.475)
Interactions with Levels									
Average Treatment × Corrected Score$^+$	0.035		0.041	0.038		0.043	-0.042		-0.041
	(0.074)		(0.073)	(0.061)		(0.062)	(0.048)		(0.049)
Maximum Treatment × Corrected Score$^+$	0.009		0.009	0.005		0.004	-0.040		-0.037
	(0.094)		(0.094)	(0.062)		(0.063)	(0.049)		(0.050)
Average Treatment × Corrected Score$^-$	0.171**		0.178**	0.151***		0.151***	0.047		0.052
	(0.077)		(0.079)	(0.052)		(0.052)	(0.054)		(0.055)
Maximum Treatment × Corrected Score$^-$	-0.032		-0.033	-0.030		-0.030	0.051		0.048
	(0.097)		(0.101)	(0.065)		(0.068)	(0.051)		(0.051)
Interactions with Changes									
Average Treatment × Score Change$^+$		0.016	-0.001		0.083	0.069		-0.051	-0.057
		(0.115)	(0.119)		(0.086)	(0.084)		(0.092)	(0.092)
Maximum Treatment × Score Change$^+$		-0.041	-0.039		-0.064	-0.063		0.126	0.123
		(0.142)	(0.143)		(0.127)	(0.128)		(0.079)	(0.079)
Average Treatment × Score Change$^-$		-0.087	-0.118		-0.063	-0.091		-0.026	-0.027
		(0.138)	(0.137)		(0.079)	(0.079)		(0.081)	(0.081)
Maximum Treatment × Score Change$^-$		0.034	0.038		0.046	0.050		-0.035	-0.040
		(0.182)	(0.186)		(0.123)	(0.126)		(0.077)	(0.077)
Observations	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019	21,019
R-squared	0.396	0.396	0.396	0.623	0.623	0.623	0.667	0.667	0.667

Notes: This table present estimates of our asymmetric version of equation (2), presented in footnote 21 Columns (1) to (3) report estimates of differences in turnout between the 2016 and 2012 municipal elections, while columns (4) to (6) present estimates for the differences in the incumbent vote share. Columns (7) to (9) present estimates for the differences in the challenger vote share.

Corrected Score$^+$ is defined as $\max\{\text{Corrected Score}, 0\}$, while Corrected Score$^-$ is defined as $\min\{\text{Corrected Score}, 0\}$. Score Change$^+$ is defined as $\max\{\text{Score Change}, 0\}$, while Score Change$^-$ is defined as $\min\{\text{Score Change}, 0\}$

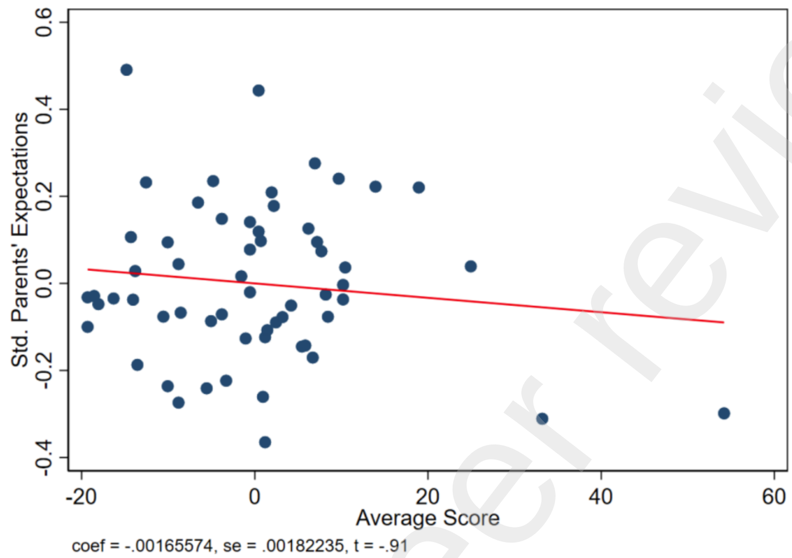
Our main regressors are the Average and Maximum Treatment dummies, and interactions with Corrected Score$^+$, Corrected Score$^-$, Score Change$^+$ and Score Change$^-$.

All estimations include municipality fixed effects, plus controls for the gender and age composition of the polling station. Robust standard errors in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 4: School Quality: Correlation between Prior Beliefs and Outcomes

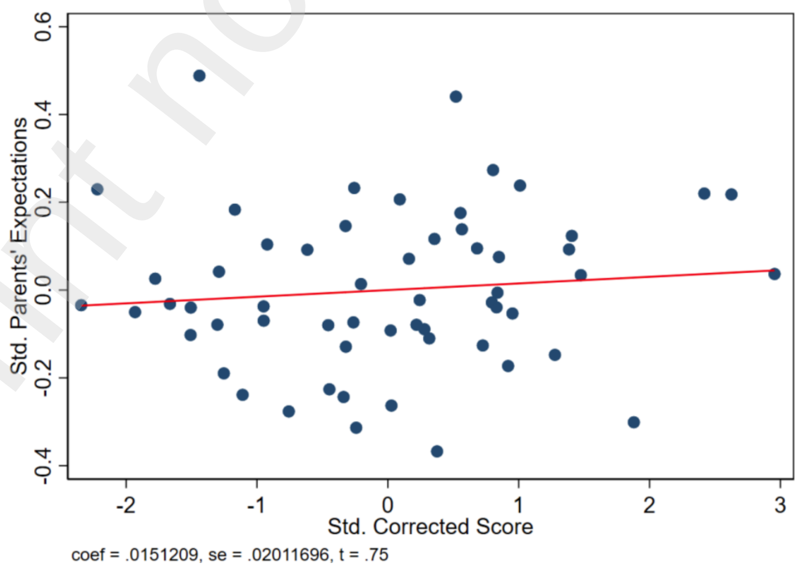
(A)

Parents' beliefs and SIMCE at the municipality level



(B)

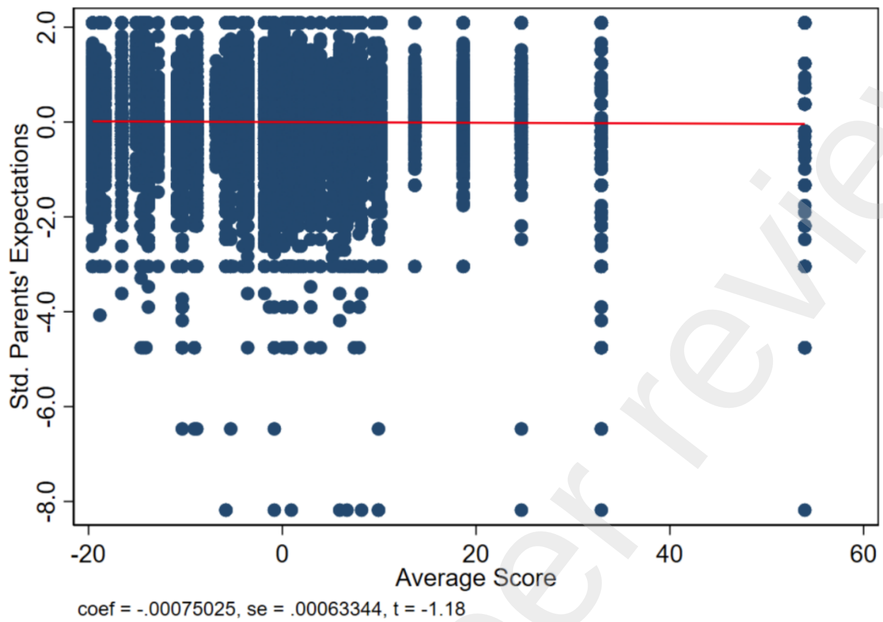
Parents' beliefs and corrected SIMCE at the municipality level



Notes: Standardized Parents' Expectations is the standardized average of parents' expectations about publicly financed schools according to data from the SIMCE parents' survey at the polling station level. Each dot represents one municipality. The slope represents the ordinary least squares coefficient of regressing the average Parents' Expectations Index at the municipality level on the corrected score.

(C)

Parents' beliefs and SIMCE at the polling-station level



Delving deeper, several prior studies argue that information should more strongly affect behavior when it provides "news," i.e. information that is not incorporated in voters' prior beliefs (e.g., Arias et al. 2018; Gallego et al. 2020). The information provided in the letter may have come as a surprise for some voters, but a confirmation for others. We expect the former case to have stronger effects on outcomes if the letter provides additional information. This is important if voters have heterogeneous priors about the quality of education across and within municipalities. We construct a proxy for prior beliefs about the quality of publicly financed schools from a universal survey administered to parents of school-aged children included in the SIMCE package.[25] Figure 4 plots expectations regarding the quality of publicly financed schools (standardized) against SIMCE scores (A) and corrected SIMCE (B) at the municipality level. It illustrates a slight, but *negative* correlation between beliefs and SIMCE score, which turns slightly positive when controlling for socioeconomic variables. Moreover, panel (C) reveals that parents' beliefs about quality at the polling-station

[25] Since there is a high correlation between expectations about the quality of public schools and private schools that are publicly financed (Allende et al. 2019), we include all publicly financed schools in this measure to increase the number of observations per polling station.

level vary greatly within-municipality. This evidence suggest voters are very poorly informed about public schools' quality, consistent with prior research (Allende et al. 2019).

To test the hypothesis of heterogeneous impacts driven by voters' priors, we extend equation (2) by adding interactions of the treatment dummies with a measure of prior beliefs at the polling station level.[26] We define four categories: a) high expectations, high performance, b) high expectations, low performance, c) low expectations, high performance, and d) high expectations, low performance. High expectations is a dummy that takes a value of 1 if prior beliefs are above the median of our sample. High performance is a dummy that takes a value of 1 if the corrected score is above 0, the mean. These four categories summarize the potential effects of whether the information constitutes "news" or not. We expect the effects of the information treatment to be stronger when it provides news (i.e. when priors are updated by the information provided *ex post*). We allow the direct and indirect treatment effects to be different for each group. Figure 5 displays the effects of the score residuals, and Appendix Table 4 presents the estimates for all coefficients.[27] The left panels present cases with no news (i.e. high expectations and high results or low expectations and low results), and the right panels cases in which voters' priors are different from the results relayed in the letters.

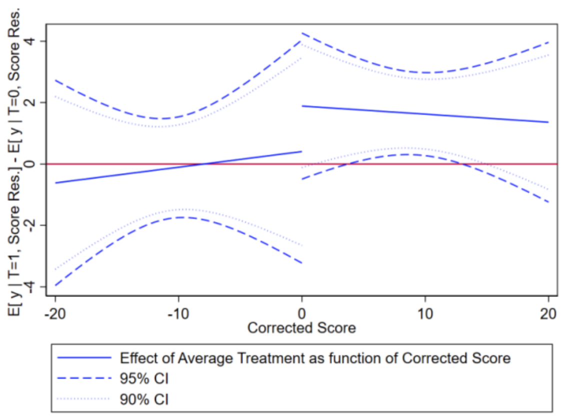
[26] We use data provided by the Chilean Quality of Education Agency to calculate measures of average priors for publicly financed schools at the polling station level. We weight the regressions including priors with the number of parents observations by polling station. Of the country's 17,546 polling stations, the average number of observations per station is 7.45, with a median of 7, percentile 5 of 1, percentile 95 of 16, and a maximum of 33; 805 stations (4.6%) have no observations.

[27] The estimating equation is :

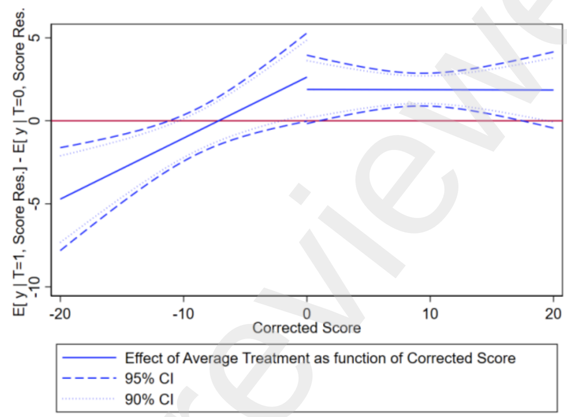
$$\begin{aligned}\Delta y_{sm} = \alpha + \theta * T_{sm}^{ave} + \kappa * T_{sm}^{max} + \beta_1 * Score^{++}{}_m * T_{sm}^{ave} + \beta_2 * Score^{--}{}_m * T_{sm}^{ave} + \beta_3 * Score^{+-}{}_m * T_{sm}^{ave} \\ + \beta_4 * Score^{-+}{}_m * T_{sm}^{ave} + \gamma_1 Score^{++} * T_{sm}^{max} + \gamma_2 Score^{--} * T_{sm}^{max} + \gamma_3 Score^{+-} * T_{sm}^{max} \\ + \gamma_4 Score^{-+} * T_{sm}^{max} + \sigma \Delta Score_m * T_{sm}^{ave} + \Psi * \Delta Score * T_{sm}^{max} + X'{}_{sm}\mu + \tau_m + \varepsilon_{sm}\end{aligned}$$

Where $Score^{++}{}_m$ corresponds to the high-expectations, high-performance category; $Score^{--}{}_m$ corresponds to the low-expectations, low-performance category; $Score^{+-}{}_m$ corresponds to the high-expectations, low-performance category; and, $Score^{-+}{}_m$ corresponds to the low-expectations, high-performance category.

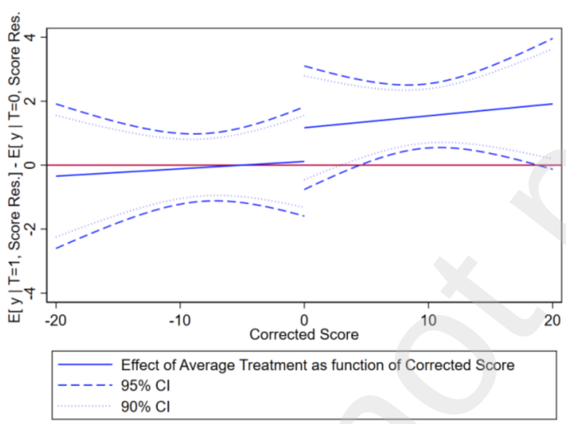
Figure 5: Effects of the Corrected Score Conditional on Prior Beliefs



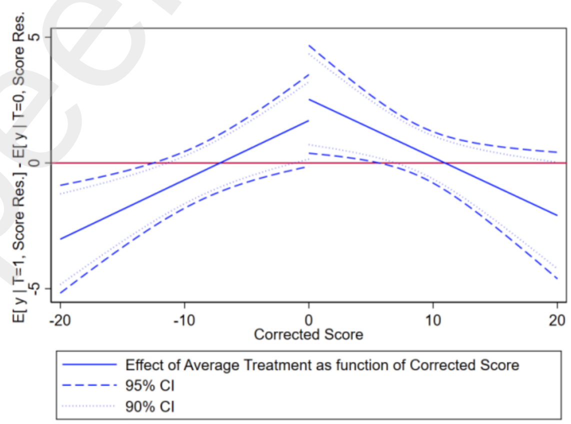
(a) Turnout, No News



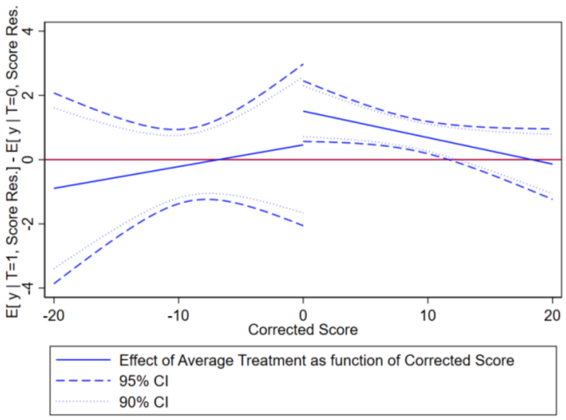
(b) Turnout, News



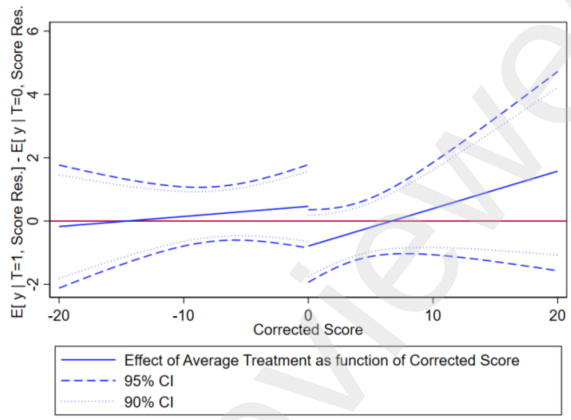
(c) Incumbent vote share, No News



(d) Incumbent vote share, News



(e) Challenger vote share, No News



(f) Challenger vote share, News

Notes: Figures 5a to 5f present estimates of the variation of equation (2) presented in footnote 26. The figures are based on the coefficients presented in Table A4, and present the effects of the Average Treatment conditional on the delivery of information that is "No News" (below-average expectations and low performance, or above-average expectations and high performance) or "News" (below-average expectations and high performance, or above-average expectations but low performance).

The upper panel (Figures 5a and 5b) illustrates the impact of the information on turnout, the center panel (Figures 5c and 5d) the effect on the incumbent vote share, and the bottom panel (Figures 5e and 5f) the impact on the challenger vote share.

The left-hand panels display the impact of the Average Treatment of "No News" (i.e. no mismatch between the information provided and voters' expectations about school performance), while the right-hand panels show the impact of "News" (i.e. mismatch between the information provided and voters' expectations about school performance).

Analytical confidence intervals (with 90 and 95% coverage) were calculated using estimated robust standard errors. Observations are weighted by the number of voters who had children enrolled in publicly funded schools in each polling station.

The results reported in Figure 5 suggest that the treatment letter does not have significant effects when it does not constitute "news" to the recipient. When the treatment provides bad news (high priors and low performance), it has much stronger electoral effects than positive news. Having high expectations and reporting a SIMCE score of -10 implies a reduction of 3.68 p.p. in turnout, and of 2.36 p.p. in support for the incumbent. These results are about 2–3 times those of the original symmetric specification (Table 4), and about twice those of the asymmetric case that distinguished between good and bad outcomes, but not between what is news and what is not (Table 6). Again, the effects on the extensive margin (turnout) do not translate into clear patterns of support for the challenger. In addition, the effects of good news are not clear. These results are again consistent with loss aversion and show that when voters' prior beliefs are taken into account, the greatest responses come not only from bad results, but from "bad news."

In summary, these results imply that voters react to information about educational outcomes, specifically to information on levels and when using an average benchmark. This reaction operates mainly through the extensive margin, increasing turnout and support for the incumbent mayor, and has no effect on support for challengers. We also observe that the reactions seem to be asymmetric in the sense that voters react more strongly to negative than to positive outcomes, especially when these outcomes represent new information. When provided with information they care about, and especially when such information implies "bad news," voters do punish mayors who fail the test.

Finally, we discuss two robustness checks of our previous results. First, our estimates in first differences rely on the assumption of parallel trends. We formally test this assumption in Table 7 by running a placebo regression using differences for the presidential elections between 2013 and 2017. The specification is the same as in our main equation but using the differences in the outcomes of the 2017 and 2013 presidential elections on the left side. As expected, if our identification assumptions are correct, we find no effects for the interaction of the treatment with the corrected score. Second, the results are robust to excluding merged polling stations (in Appendix Table 5).

Table 7: Placebo Exercises

	Presidential Elections (2017-2013)			Municipal Elections (2016-2012)
	(1) Turnout	(2) Incumbent Party Sh.	(3) Challenger Party Sh.	(4) Registered Voters
Average Treatment	0.242	0.892**	-0.935***	-2.510
	(0.343)	(0.381)	(0.362)	(2.083)
Average Treatment × Corrected Score	0.079	0.062	0.024	-0.098
	(0.050)	(0.053)	(0.036)	(0.272)
Average Treatment × Score Change	0.059	0.105	-0.117*	0.094
	(0.054)	(0.070)	(0.069)	(0.285)
Maximum Treatment	0.249	-0.050	0.044	-2.495
	(0.376)	(0.361)	(0.356)	(1.750)
Maximum Treatment × Corrected Score	-0.038	-0.019	-0.004	0.014
	(0.043)	(0.040)	(0.037)	(0.252)
Maximum Treatment × Score Change	-0.035	-0.010	0.048	-0.339
	(0.062)	(0.073)	(0.065)	(0.278)
Observations	19,426	16,803	16,803	21,029
R-squared	0.254	0.459	0.422	0.161

Notes: This table presents two sets of placebo exercises. Columns (1) to (3) present the results of estimating equation (2) (excluding demographic controls) on three outcome variables for the next presidential election (in 2017), while column (4) includes one placebo exercise related to the 2016 municipal election. Column (1) presents estimates for the differences in turnout between the 2017 and 2013 presidential elections. Column (2) reports estimates for differences in the incumbent party's vote share between the 2017 and 2013 presidential elections. Column (3) includes estimates for the differences in the challenger party's vote share between the 2017 and 2013 presidential elections. Column (4) presents estimates for the differences in the number of registered voters at the polling station level between the 2016 and 2012 municipal elections.

All estimations include as the main regressors both treatment dummies and their interactions with corrected score and score change. All estimates also include municipality fixed effects. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.2 Heterogeneity Analyses

We analyze whether the estimates of our main coefficient of interest, β (the effect of the SIMCE score for polling stations treated with the average treatment), are different for polling stations with different types of voters (Table 8, Panel A) and for those located in municipalities with different characteristics (Table 8, Panel B).[28] The motivation of these exercises is to shed light on the mechanisms behind our main results. Overall, we lack the power to identify statistically significant differences by the type of polling station or type of municipality. However, the differences in point estimates suggest relevant heterogeneities across several dimensions, which are large and consistent and may still indicate that some groups have stronger effects—which therefore may help us understand some of the mechanisms driving the results. Appendix Table 6 presents correlations among all the variables we use to study heterogeneous effects.

[28] We only report the effects for β after estimating the complete specification in equation (2). We also report average treatment effects as a reference in Table 7 in each panel.

Table 7: Summary of Heterogeneous Effects

Panel A: Polling-station-level heterogeneities

	(1) Turnout	(2) Incumbent vote share	(3) Challenger vote share
No Heterogeneity			
Average Treatment × Corrected Score	0.111***	0.098***	0.008
	(0.041)	(0.031)	(0.027)
Age			
Younger Voters	0.062	0.051	0.000
	(0.048)	(0.033)	(0.036)
Older Voters	0.088*	0.088**	-0.027
	(0.050)	(0.044)	(0.031)
p-value Difference	[0.533]	[0.214]	[0.478]
Gender			
Greater Share of Women	0.127**	0.083**	0.041
	(0.053)	(0.039)	(0.043)
Greater Share of Men	0.109**	0.116***	-0.023
	(0.055)	(0.040)	(0.036)
p-value Difference	[0.882]	[0.616]	[0.310]
Incumbent Support			
Lower Incubent Supp.	0.093	0.077*	-0.014
	(0.077)	(0.043)	(0.035)
Higher Incubent Supp.	0.090***	0.058*	0.048*
	(0.034)	(0.030)	(0.029)
p-value Difference	[0.591]	[0.724]	[0.114]

Panel B: Municipality-level heterogeneities

	(1) Turnout	(2) Incumbent vote share	(3) Challenger vote share
No Heterogeneity			
Average Treatment × Corrected Score	0.111***	0.098***	0.008
	(0.041)	(0.031)	(0.027)
Incumbent Political Coalition			
Concertacion	0.122**	0.078*	0.011
	(0.057)	(0.043)	(0.032)
Alianza	0.098	0.098**	0.023
	(0.072)	(0.050)	(0.055)
p-value Difference	[0.797]	[0.760]	[0.855]
Municipality's share of students in public schools			
Lower share for public schools	0.089	0.077	0.017
	(0.066)	(0.052)	(0.043)
Higher share for public schools	0.130**	0.113***	0.008
	(0.053)	(0.035)	(0.035)
p-value Difference	[0.627]	[0.556]	[0.874]
Municipality's average income			
Lower income municipalities	0.128***	0.107***	0.022
	(0.045)	(0.032)	(0.030)
Higher income municipalities	0.011	0.042	-0.079
	(0.091)	(0.076)	(0.055)
p-value Difference	[0.249]	[0.429]	[0.109]
Municipality size			
Smaller municipalities	0.125***	0.106***	0.001
	(0.046)	(0.035)	(0.029)
Larger municipalities	0.044	0.061	0.081
	(0.130)	(0.082)	(0.099)
p-value Difference	[0.561]	[0.610]	[0.436]
Incumbent electoral spending			
Lower electoral spending	0.134***	0.111***	0.015
	(0.047)	(0.035)	(0.030)
Higher electoral spending	-0.034	-0.005	-0.040
	(0.129)	(0.091)	(0.082)
p-value Difference	[0.224]	[0.231]	[0.525]

Notes: This table presents estimates of the heterogeneous effects of the interaction of our Average Treatment and the corrected SIMCE score (β in equation 2), over several polling station and municipality characteristics.

Panel A presents heterogeneous effects for polling station characteristics, while Panel B does so for municipal characteristics. Column (1) presents the results when the outcome variable is the difference in turnout between 2016 and 2012, column (2) when the outcome variable is the difference in the incumbent vote share between 2016 and 2012, and column (3) when the outcome variable is the difference in the challenger's vote share between 2016 and 2012.

As a benchmark, the first subpanels of Panels A and B show the main equation's estimation of the coefficient of interest (i.e. the interaction between the Average Treatment dummy and the corrected score).

The second subpanel of Panel A ("Age") estimates equation (2) separately for polling stations under or over the median average voter age. The third subpanel of Panel A ("Gender") estimates equation (2) separately for polling stations under or over the median fraction of men among registered voters in the polling station. The fourth subpanel of Panel A ("Incumbent Support") estimates equation (2) separately for polling stations under or over the median incument vote share in the previous municipal election (2012).

The second subpanel of Panel B ("Incumbent Political Coalition") estimates equation (2) separately for polling stations in municipalities with an incumbent affiliated with Concertación (left-wing coalition) and Alianza (right-wing coalition). The third subpanel of Panel B ("Municipality's share of students in public schools") estimates equation (2) separately for polling stations in municipalities under or over the median fraction of students in public schools (considering the distribution by municipality). The fourth subpanel of Panel A

("Municipality's average income") estimates equation (2) separately for polling stations in municipalities under or over the median average municipality income. The fifth subpanel of Panel B ("Municipality size") estimates equation (2) separately for polling stations in municipalities under or over the median municipality population. The sixth subpanel of Panel B ("Incumbent electoral spending") estimates equation (2) separately for polling stations in municipalities under or over the median incumbent electoral spending.

All estimations include gender and age composition as controls. Robust standard errors in (round) parentheses. p-value of the hypothesis of equal coefficients for both groups in [squared] parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

We begin by estimating the heterogeneities in polling stations with different average ages,[29] gender compositions,[30] and support for the incumbent mayor in the 2012 election.[31] The results by gender and age suggest no relevant differences We also test for differential effects for various combinations of age and gender but found no clear patterns (not shown due to space constraints). Finally, we do not find that the effects are stronger depending on the incumbent's vote share in 2012.

Panel B presents heterogeneous effects by municipal characteristics, which are more illuminating. The treatment effects differ little based on the incumbent's political leaning. In addition, the results seem stronger in municipalities with higher public-school enrollment, as expected, because in these areas more voters are directly affected by the performance of public schools. The results by income level, despite showing non-significant differences, suggest stronger effects for poorer municipalities. This is consistent with previous evidence that implies that poorer voters may lack educational outcome information (as documented in Allende et al. 2019), although notice that income levels are highly correlated with public school enrollment -with lower income children being more likely to attend public schools. The effects are also stronger in smaller municipalities and especially in those in which the amount of (official) electoral spending in the incumbent's campaign is relatively low. One possible explanation for this could be that our experiment is more salient when the incumbent's campaign efforts are more limited. This result is consistent with research documenting the effects of campaign spending on incumbent advantage (e.g., Avis et al. 2020).

[29] We have the share of voters in each polling station in the following age intervals: 18–29, 30–59, 60–80. We use this information to create an index for the average age in each station.
[30] We split the sample into polling stations above and below the median in each dimension.
[31] We split the sample into stations with more or less than 50% of the votes for the incumbent mayor in the 2012 election (note that usually there are more than two candidates in the election).

Overall, the heterogeneity results suggest that the information intervention is most useful for voters from poor municipalities, and when incumbents spend less on their campaigns. This probably relates to contexts with more information frictions, although our findings could also reflect a greater demand and appreciation for mayoral outcomes; additional research is needed.

5. Conclusions

Citizens must often decide who to vote for without having much information about politicians' performance. This challenge is even more pressing when trying to deal with complex outcomes of the politicians' actions, which require both having benchmarks and processing raw information to assess the value added of their job. Our paper examines the electoral effects of providing information about the quality of educational provision during incumbent mayors' term in office. We designed and implemented a randomized large-scale experiment in Chile, where local governments oversee public schools, there is good information about educational outcomes, and the electoral roll is public. We sent 128,033 letters to voters with information on the performance of local public schools in test scores (levels and changes), alternating between two yardsticks (average and maximum educational performance).

Our results demonstrate that being informed about better relative performance increases turnout, which translates almost one-to-one into support for the incumbent. We find that the effects are concentrated in letters that use average performance as the yardstick, and that voters react much more strongly to outcomes in levels than in changes. The results are especially strong when the educational results are bad, reducing turnout and votes for the incumbent, more so if the bad results come as a surprise compared to voters' prior beliefs about educational quality. These results seem to be driven by effects in municipalities that are poor and have low levels of electoral spending by the incumbent. Taken together, these findings indicate that providing voters with information that matters to them about the outcomes of key policies controlled by politicians *does* affect their electoral behavior: voters punish mayors who fail the test.

It is worth noting, however, that we find a series of zero effects on electoral outcomes: no effects of information on score changes, no effects from the maximum treatment, no effects from good results or good news, and no effects on support for the challenger. From a policy perspective, this suggests that it is difficult to use information to affect voters' behavior. Indeed, the coexistence in our paper of relevant effects for certain types of information with no effects for others suggests that many null findings in previous research may have arisen *not* because voters do not care about information or policy outcomes, but because they did not care about the information provided in those settings.

Moreover, the fact that our significant effects only operate through votes for the incumbent and have no effect on voting for the main challenger implies that political competition has a limited ability to hold incumbents accountable. And since our effects operate via turnout, bad news is demobilizing, which may also be problematic.

In terms of policy evaluation and policy implications, our experiment incurred a cost of USD$0.34 per voter contacted. This is an upper bound, since information campaigns via social networks, key actors, or public signage could exploit spillovers to make the intervention more cost effective. While more research is needed, our results show that although not all information matters for voters, adequate information can generate an important response in the polls, hopefully improving both governance and education.

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APPENDIX

Table A1: Correlations of Educational Outcomes and Other Municipality Outcomes

	(1)	(2)	(3)	(4)
Dimension:	Number of public health centers	Crime risk	Mantained parks	Expenditures in park maintenance
D	0.235**	-11.13***	0.791**	1.026**
	(0.121)	(2.120)	(0.371)	(0.487)
$\log(Pop)$	-2.301**	-0.596*	-1.831***	-2.263***
	(0.952)	(0.789)	(0.616)	(0.780)
$\log(Pov)$	-2.892	-2.139	-1.463	-1.660
	(1.955)	(1.660)	(1.774)	(2.118)
R-squared	0.04	0.13	0.06	0.08
Observations	296	340	332	240
Spearman correlation	0.16***	-0.28***	0.13**	0.09

Notes: This table presents estimates of the following regression:

$$SIMCE_m = \alpha + \beta D_m + \gamma \log(Pop_m) + \delta \log(Pov_m) + \varepsilon_{sm} \ ,$$

where D are different proxies for other outcomes produced in municipality m, Pop is total population, and Pov is the poverty rate. The purpose is to control for size and income effects to better identify the correlation between SIMCE and other measures. The Spearman rank presents the correlation between SIMCE and D (after partialling out the effects of population and poverty).

Table A2:

Table A2: Value Added Regression

VARIABLES	(1) SIMCE score
Students' Vulnerability Index	-1.036**
	(0.469)
Students' Vulnerability Index^2	0.00534*
	(0.00322)
Rural Population	30.93***
	(8.734)
Rural Population^2	-25.15***
	(8.694)
Poor Students in Public Schools	-0.00149**
	(0.000687)
Poor Students in Public Schools^2	2.04e-08
	(1.29e-08)
Population	0.000126**
	(6.03e-05)
Population^2	-1.02e-10*
	(5.36e-11)
Type of Municipality FE:	Yes
Observations	343
R-squared	0.223

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3: Invalid Votes Share

	Change between 2016 and 2012	
	(1)	(2)
Average Treatment	0.058	0.049
	(0.083)	(0.083)
Average Treatment × Corrected Score	0.006	0.005
	(0.007)	(0.007)
Average Treatment × Score Change	0.016	0.015
	(0.015)	(0.015)
Maximum Treatment	-0.041	-0.046
	(0.095)	(0.095)
Maximum Treatment × Corrected Score	-0.010	-0.009
	(0.009)	(0.008)
Maximum Treatment × Score Change	-0.029	-0.028
	(0.018)	(0.019)
Controls	No	Yes
Observations	21,019	21,019
R-squared	0.181	0.186

Notes: This table presents estimates of the main equation on differences of the share of invalid votes (blank or null votes) between 2016 and 2012 at the polling station level. Column 1 does not include controls, while column 2 includes the gender and age composition as controls. All estimates include municipality fixed effects. ${}^{*}p < 0.1$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$

Table A4: Asymmetric Effects Conditional on Prior Beliefs

	(1) Turnout Change	(2) Incumbent Vote Share Change	(3) Challenger Vote Share Change
Expectations / Performance			
High / High			
Average Treatment	1.887	1.170	1.508***
	(1.214)	(0.985)	(0.481)
Average Treatment × Corrected Score	-0.026	0.037	-0.082*
	(0.107)	(0.088)	(0.046)
Low / Low			
Average Treatment	0.406	0.116	0.459
	(1.854)	(0.872)	(1.284)
Average Treatment × Corrected Score	0.051	0.023	0.068
	(0.157)	(0.085)	(0.127)
Low / High			
Average Treatment	1.894*	2.532**	-0.790
	(1.051)	(1.094)	(0.585)
Average Treatment × Corrected Score	-0.002	-0.231**	0.118
	(0.099)	(0.107)	(0.095)
High / Low			
Average Treatment	2.633*	1.686*	0.465
	(1.355)	(0.931)	(0.673)
Average Treatment × Corrected Score	0.368***	0.236***	0.032
	(0.129)	(0.084)	(0.070)
Mejora			
Average Treatment × Score Change	0.048	0.024	0.041
	(0.074)	(0.067)	(0.038)
Observations	16,741	16,741	16,741
R-squared	0.370	0.709	0.700

Notes: This table present the coefficients displayed graphically in Figure 3. They are estimates of the main equation, but allowing heterogeneous effects based on four groups:

i. No News with bad performance: Polling stations with voters with low expectations on average, in municipalities with low performance
ii. No News with good performance: Polling stations with voters with high expectations on average, in municipalities with high performance
iii. News with bad performance: Polling stations with voters with high expectations on average, in municipalities with low performance
iv. News with good performance: Polling stations with voters with low expectations on average, in municipalities with high performance

For these four different groups, we generate dummies that we interact with the Average Treatment dummy and its interaction with corrected score. We also include as a control the interaction between the Average Treatment dummy with the change in score.

Column 1 presents results for the difference in turnout between 2016 and 2012, column 2 for the difference in the Incumbent vote share between 2016 and 2012, and column 3 for the difference in the Challenger vote share between 2016 and 2012.

Observations are weighted by the number of voters who had children enrolled in publicly-funded schools. All estimations include the gender and age composition of the polling stations as controls. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A5: Main Equation for Non-merged Polling Stations

	Turnout					Incumbent Share					Challenger Share				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Any Treatment	0.288					0.218					0.060				
	(0.292)					(0.197)					(0.167)				
Average Treatment		0.522	0.552		0.553		0.219	0.295		0.298		0.190	0.142		0.142
		(0.389)	(0.397)		(0.397)		(0.262)	(0.278)		(0.278)		(0.235)	(0.256)		(0.256)
Maximum Treatment		0.050		0.053	0.058		0.217		0.193	0.196		-0.072		0.006	0.008
		(0.433)		(0.431)	(0.431)		(0.292)		(0.297)	(0.297)		(0.235)		(0.245)	(0.245)
Average Treatment × Corrected Score			0.117***		0.117***			0.098***		0.097***			0.011		0.011
			(0.041)		(0.041)			(0.030)		(0.030)			(0.027)		(0.027)
Average Treatment × Score Change			-0.040		-0.040			-0.003		-0.003			-0.033		-0.033
			(0.070)		(0.070)			(0.051)		(0.051)			(0.048)		(0.048)
Maximum Treatment × Corrected Score				0.001	0.003				-0.007	-0.006				0.011	0.011
				(0.047)	(0.047)				(0.032)	(0.032)				(0.026)	(0.026)
Maximum Treatment × Score Change				0.006	0.006				-0.011	-0.011				0.050	0.049
				(0.088)	(0.088)				(0.069)	(0.069)				(0.043)	(0.043)
Observations	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544	20,544
R-squared	0.402	0.402	0.402	0.402	0.402	0.628	0.628	0.628	0.628	0.628	0.669	0.669	0.669	0.669	0.669

Notes: This table presents the result of estimating equation 2 for differences between the 2016 and the 2012 municipal elections, for the subsample of non-merged polling stations. Columns 1 to 5 present estimates on differences in turnout; columns 6 to 10 present estimates on differences in the incumbent vote share; columns 11 to 15 present estimates for the differences in the challenger vote share.

Columns 1, 6 and 11 have as main regressor a dummy variable equal to one if the polling station was assigned to any treatment. Columns 2, 7 and 12 have as main regressors one dummy variable equal to one if the polling station was assigned to the Average Treatment, and a second dummy equal to one if the polling station was assigned to the Maximum Treatment. Columns 3, 8 and 13 have as main regressors the Average Treatment dummy plus interactions of this dummy with the corrected score variable and also with the change in score. Columns 4, 9 and 14 have as main regressors the Maximum Treatment dummy plus interactions of this dummy with the corrected score variable and also with the change in score. Finally, columns 5, 10 and 15 present the results of estimating Equation 2, so they include as main regressors both treatment dummies and their interactions with corrected score and Score change.

All estimates include municipality fixed effects. Controls include the gender and age composition of the polling station. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A6: Correlation between Municipal Characteristics

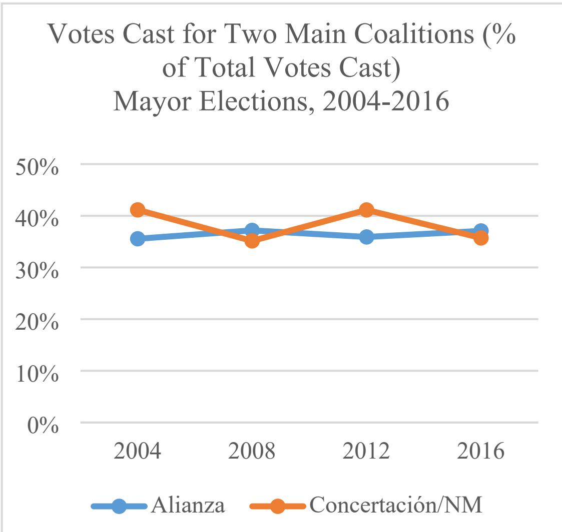
	Incumbent Party is Concertacion	Share Public Schools	Average Income	Municipality size	Incumbent Electoral Spending	Average polling station Men Share	Average polling station Age	Average polling station Incumbent Support	Share New polling stations
Incumbent Party is Concertacion	1.000								
Share Public Schools	0.047	1.000							
Average Income	-0.231*	-0.301**	1.000						
Municipality size	-0.107	-0.021	-0.051	1.000					
Incumbent Electoral Spending	-0.018	-0.201	0.175	0.392***	1.000				
Average polling station Men Share	0.147	0.418***	-0.443***	-0.141	-0.349***	1.000			
Average polling station Age	0.242*	0.038	0.289**	-0.254*	0.257*	-0.132	1.000		
Average polling station Incumbent Support	-0.029	-0.168	0.219*	-0.237*	-0.068	-0.283**	-0.042	1.000	
Share New polling stations	-0.322**	-0.238*	-0.200	0.390***	-0.054	-0.078	-0.812***	0.062	1.000

Notes: This table presents the correlations between municipality characteristics. The average variables are non-weighted averages over polling stations within each municipality. *N=59*. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

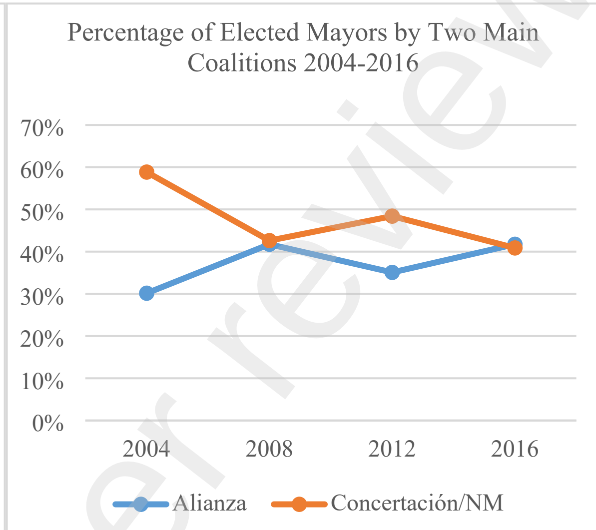
Appendix Figure 1

Support for the Major Coalitions in Mayoral Elections

Panel A



Panel B

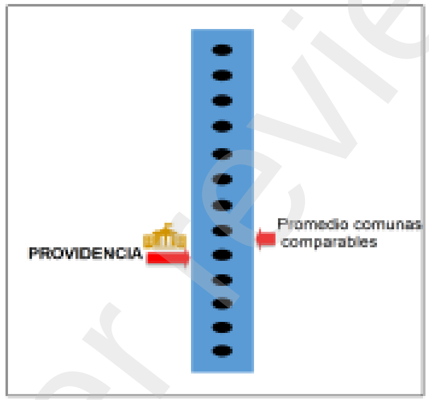


Appendix Figure 2

Letter with Average Treatment (Example)

El domingo 23 de octubre son las elecciones municipales. El municipio es responsable de la administración de las escuelas municipales. Estos son los resultados de las escuelas municipales de su comuna en la prueba SIMCE de 4º básico, que mide logros de aprendizaje.

¿Cómo le fue a PROVIDENCIA respecto a comunas comparables?



¿Mejoró o empeoró PROVIDENCIA en este período municipal?

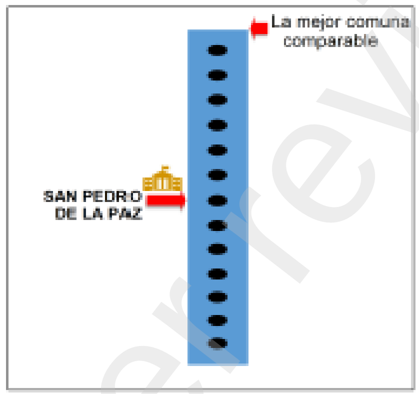
Esta carta informativa es parte de un proyecto de investigación académica coordinado por el profesor Francisco Gallego de la Pontificia Universidad Católica de Chile. Para preguntas o información adicional contactarse con [illegible].

Appendix Figure 3

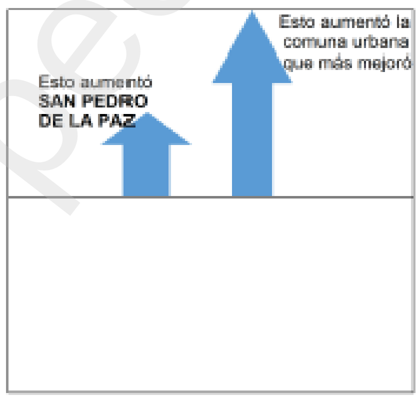
Letter with Maximum Treatment (Example)

El domingo 23 de octubre son las elecciones municipales. El municipio es responsable de varias tareas relacionadas con la comuna, como la administración de las escuelas municipales. Estos son los resultados de las escuelas municipales de su comuna en este período municipal en la pruebas SIMCE de 4^o básico, que mide logros de aprendizaje

¿Cómo le fue a SAN PEDRO DE LA PAZ respecto a comunas comparables?



¿Mejoró o empeoró SAN PEDRO DE LA PAZ en este período municipal?



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Appendix Figure 4

Correlation between SIMCE Corrected Scores and Changes in Scores, by Political Coalition