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 prior to the 2016 municipal elections. The letters included information on past test scores for local public schools (levels and changes), and either average or maximum outcomes for comparable municipalities. Our findings do not reveal a relevant average impact of the letters, but when they contain poor educational outcomes, voter turnout decreases, translating almost one to one in decreases in 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 especially strong when poor educational outcomes come as bad news to voters. We also find spillover effects in the municipal council election. Overall, our findings suggest that voters hold politicians accountable when faced to certain (but not all) types of information on their performance. 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 Akyeampong, et al. 2023 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.

A 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 for the 2016 election includes 22,387 polling stations with an average of 329 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 21,987 polling stations in the control group. We sent the treatment letters to all registered voters in treated stations.

We have four main sets of results. First, information matters: being informed of poor relative performance affects turnout, which translates almost one-to-one into 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 the information provided from the 75^{th} to the 25^{th} percentile in educational performance is associated with a reduction in turnout of about 1.35 percentage points and in support for the incumbent of around 1.23 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 receiving bad results is correlated with much stronger responses than good ones.

The third set of results reveals that prior beliefs 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 better performance than actual results). Fourth, we also find suggestive evidence indicating that (i) 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; and (ii) there appears to be a partial transmission of information to the municipal council elections, with a similar pattern of results for council candidates politically aligned with the incumbent mayor.

It is important to note that the identification of the effects of the contents of the letters is based on two sources of variation: (i) the within-municipality variation in whether voters received the letters (and the treatment they received), and (ii) the content of the report cards, which varies across municipalities (note that our analysis controls for fixed effects at the municipality level). While the first source of variation is randomly allocated, the second is not, as it depends on the municipality's outcomes of public schools, which are subject to non-random factors. We provide several pieces of evidence supporting the connection between our results and the content of the report cards. First, the absence of results for the interaction between the outcomes and the maximum treatment indicates that the content of the letter is significant and that the results are not merely a byproduct of any omitted variable varying across municipalities. Second, we conduct a series of robustness checks where we include interactions of the treatment status with 11 additional variables. These variables, unrelated to the content of the letters, capture variations in other dimensions between municipalities. The primary estimated effects remain consistent even after accounting for these interactions, further reinforcing that the observed effects are attributable 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; Boas, Hidalgo, and Toral 2021). Ajzenman and Durante (2021) study how the quality of the infrastructure of the school where citizens were assigned to vote, influenced their support for the incumbent mayor then running 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] Boas, Hidalgo, and Toral (2021) also conducted a field experiment whereby they provided flyers informing about educational quality, with a sample of 3,200 individuals, and relying on a follow-up survey for voting records. Their experiment is consistent with their RDD using the disclosure of public-school outcomes, and shows that signals of school quality *decrease* electoral support for the incumbent because voters who do not value education perceive trade-offs with other services. Parents with children in municipal schools do respond as expected.

Our experiment directly measures the effect of providing information about educational quality on the vote choice of the offices who oversee public schools (mayors), in a large scale and using administrative electoral records (at the polling-station level). 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

[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 Akyeampong, et al., 2023), 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.

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 and even to potentially spill over to other indirectly related elections, as evidenced by our findings on local council elections, thereby enriching the literature.[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 design including the treatments, sample, randomization, balance tests, and the main estimating equation. Section 4 presents the main findings, 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; 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

[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.

reelected. During our study period, Chilean politics was mostly organized around two main coalitions, Alianza (center-right) and Concertación (center-left), and more than 70% of mayors belonged to one of these coalitions (see Appendix Figure A1). 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 study in this paper. First, in 2012 Chile moved from a system with mandatory voting and voluntary registration to one with voluntary voting and automatic registration. Before the reform, 32% of the adult population (mostly young people) were 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.[4]

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),

[4] The regulation applicable during our study period established that the Electoral Service assigned the newly registered voters or those who had changed their electoral address to the polling places according to their national ID number and without distinction of sex. First, they were assigned to the district's existing polling stations that had fewer than 350 registered voters, until they reached 350. Next, if needed, the remaining voters were assigned to new polling stations, also with a maximum of 350 voters per station (see Article 12, Law 20568, available at https://www.bcn.cl/leychile/navegar?idNorma=1035420&idParte=9232374&idVersion=2012-01-31).

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 survey in 2017 revealed, respectively, that 45% and 50% of respondents reported there was less information about candidates and their programs than in previous campaigns (Engel et al. 2018). 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.[5] Previous research documents important differences in productivity at the municipal level (e.g., Gallego 2013) 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 General found that several municipalities misused funds allocated for targeted education vouchers.[6] 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. Allende et al. (2019) show that parents are not fully informed of their children's school's results: providing information

[5] Under a 2017 reform, the management of local public schools is scheduled to transfer from the 345 municipalities to 70 school districts by stages (Ministerio de Educación 2017). Initially, the 70 school districts were supposed to be in operation by 2025, but implementation is delayed and after several problems in some of the implemented school districts, Congress has requested an evaluation, based on which the transfer of the remaining schools may be postponed or aborted (see here).

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

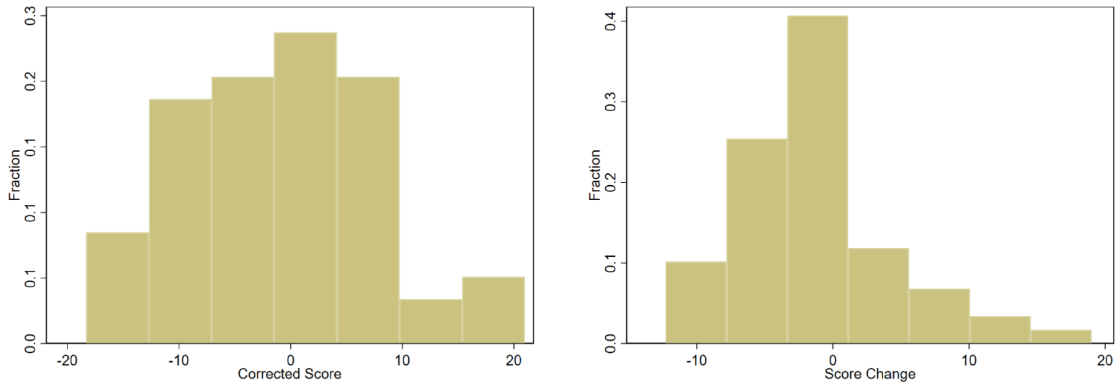
about schools' test scores changed treated households' primary school choices, which improved their children's educational outcomes 6 years after the intervention.

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 4.2).

In this paper, we use math and language results to report educational outcomes 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. 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.[7] 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 A1).

[7] The main motivation for the correction is to control for socioeconomic factors that influence educational outcomes and vary across municipalities. The correction is explained in detail in Section 3.1. SIMCE's scale has a mean of 250 points and a standard deviation of 50 points.

Figure 1: Distribution of Main Educational Outcomes



(a) Corrected Score

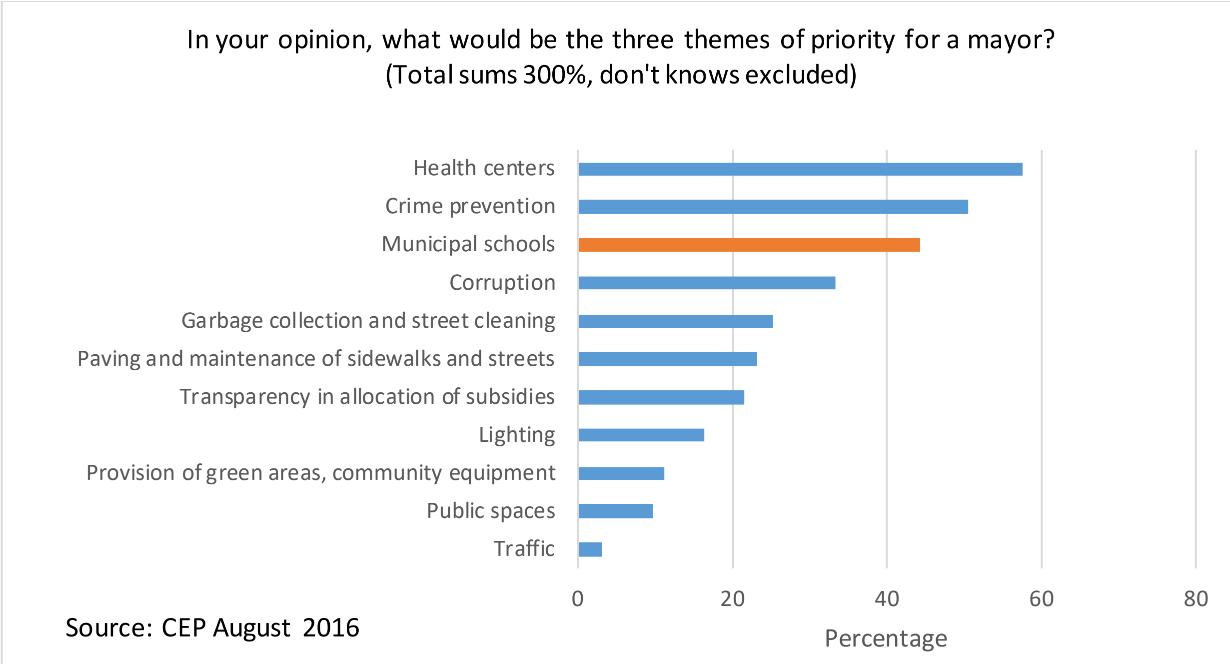
(b) Score Change

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.[8]

[8] 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 at the time of our study was publicly available and 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).

Thus, 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, as discussed above, previous evidence suggests that there are important information frictions in relation to educational outcomes at the school level.

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.[9] 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 presidential elections for some analyses.

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—come from the National System of Municipal Information (SINIM),[10] while the municipality type comes from SUBDERE (2005). Electoral campaign spending data comes from SERVEL. Table 2 presents selected summary statistics.

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.[11] 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. Of the country's 22,387 polling stations, the average number of observations per station is 7.17, with a median of 7, percentile 5 of 0, percentile 95 of 17, and a maximum of 40; 1,782 stations (8%) have no observations.

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[9] 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 (4.67% of the sample), which we treat as single stations. When a treated polling station is merged, we deal with the treatment variable as an intensity of treatment (e.g., 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 control for the number of stations in a merged station and our results are robust to excluding merged stations (results available upon request). We also dropped two polling stations that according to the official data had turnout rates over 100%.

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

[11] We are grateful to the Chilean Quality of Education Agency for their collaboration, which enabled us to construct a polling-station level measure of prior beliefs about education quality. Specifically, the question we use asked parents to grade the learning level of the students at their child's school. We expect these grades to closely correlate with parents' evaluations of the schools within the municipality.

Table 2: Summary Statistics

Panel A: Polling Station Level

	Full Sample Observations	Control Mean	SD	p1	p99
Registered voters in 2016	22,387	328.96	25.08	213.50	348.00
Turnout in 2012 (%)	21,018	37.71	13.79	6.57	67.91
Incumbent Vote Share in 2012 (%)	21,018	19.50	8.59	2.87	42.47
Challenger Vote Share in 2012 (%)	21,018	11.70	5.90	1.15	27.67
Turnout in 2016 (%)	22,387	29.55	10.03	9.36	54.95
Incumbent Vote Share in 2016 (%)	22,387	13.94	6.88	2.08	32.55
Challenger Vote Share in 2016 (%)	22,387	9.35	5.64	0.95	27.51
Men's Share (%)	22,387	48.23	18.96	5.79	96.57
Age 18-30 (%)	21,031	26.31	31.01	0.00	99.71
Age 30-59 (%)	21,031	54.18	25.31	0.00	99.13
Age 60 and more (%)	21,031	19.52	15.65	0.00	83.67

Panel B: Municipality Level

	Observations	Mean	SD	p1	p99
Registered voters in 2016	59	329.49	11.91	288.25	342.56
Turnout in 2012 (%)	59	39.72	6.17	27.66	56.43
Incumbent Vote Share in 2012 (%)	59	20.61	5.17	11.49	36.98
Challenger Vote Share in 2016 (%)	59	10.34	4.88	2.63	23.96
Men's Share (%)	59	48.31	1.28	44.47	52.56
Age 18-30 (%)	59	26.02	3.90	17.79	35.84
Age 30-59 (%)	59	54.46	1.83	51.05	61.08
Age 60 and more (%)	59	19.51	3.60	9.63	26.45
Challenger Vote Share in 2012 (%)	59	11.86	4.01	4.38	20.70
Turnout in 2016 (%)	59	31.72	6.71	21.16	49.57
Incumbent Vote Share in 2016 (%)	59	14.91	5.44	4.65	28.56
Corrected Score	59	-0.98	8.72	-18.31	20.98
Score Change	59	-1.51	5.99	-12.29	19.00

Notes: Corrected Score and Score Change were constructed based on information provided by SINIM. All other variables were provided by SERVEL.

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 registered voters in 400 randomly selected polling stations (out of 22,387) 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 329 registered voters, whose addresses were publicly available. *Correos de Chile*, the national post office, printed and mailed the letters.[12] 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 A2 and A3). 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.

Chilean municipalities face a wide range of socioeconomic conditions that can influence their educational performance, regardless of the mayor's efforts. Thus, for the *levels* of SIMCE results, we corrected the 4th grade test scores by municipality to reflect the value added by schools by controlling for a set of socioeconomic outcomes using the following equation:

$$SIMCE_i = \beta_0 + \beta_1 Vul_i + \beta_2 Vul_i{}^2 + \beta_3 Pop_i + \beta_4 Pop_i^2 + \beta_5 Rur_i + \beta_6 Rur_i^2 + \beta_7 Poor_i + \beta_8 Poor_i^2 + \sum \beta_{9k} Type_k + e_i, \quad (1)$$

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

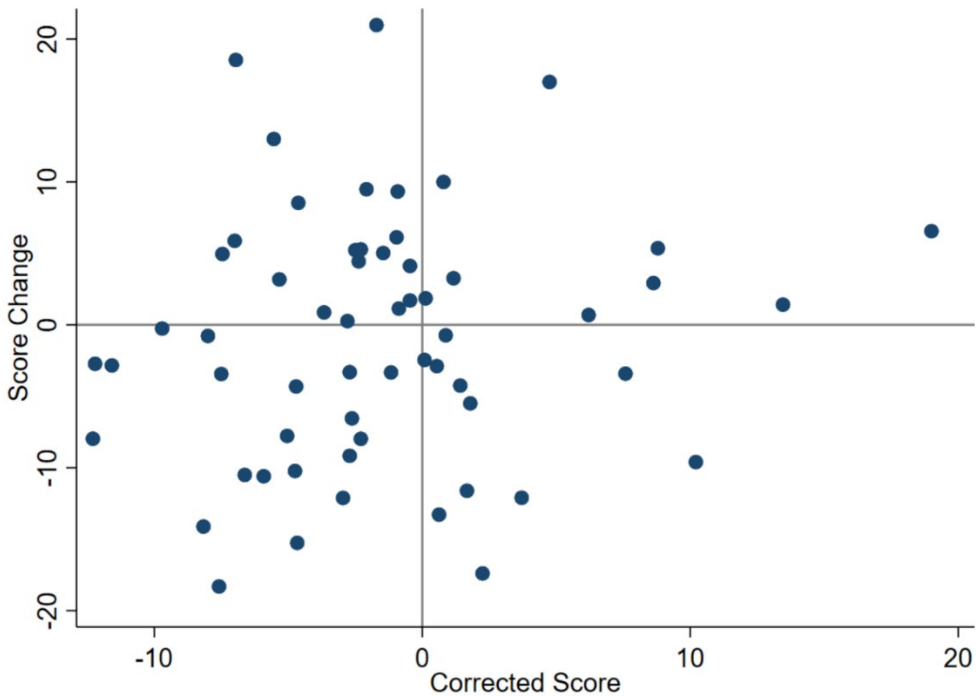
where, for municipality i, $SIMCE_i$ is the SIMCE score, Vul_i corresponds to the students' vulnerability index, Pop_i to population, Rur_i to the percentage of the population that lives in rural areas, $Poor_i$ to the number of poor students in public schools, and $Type_i$ to the municipality type according to the central government's classification k.[13] We proxy municipal schools' "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.[14] 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." The correlation between corrected and raw scores in our working sample is 0.8103.

For the *changes* in SIMCE results, we used 4th grade raw 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).[15] 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 $\Delta Score$).

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; indeed, their correlation is only 0.129. Both measures are uncorrelated with the political coalition of the municipality's mayor (see Appendix Figure A4). Therefore, providing information on these measures did not favor any particular coalition.

[13] 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).
[14] Appendix Table A2 presents the results of the regression used to generate the residuals, which includes all municipalities. The R^2 of the regression is 0.22. We use the term "comparable" municipalities to refer to the residuals that are net of the contributions of the sociodemographic variables to educational performance, as accounted in the previous equation. Given the complexity of explaining the logic behind a corrected score derived from such an equation to the public, we simply use the term "comparable" in the letters.
[15] 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 A2). The maximum treatment used a more demanding benchmark: it pitted the municipality's performance against that of the *best* (comparable) municipality (Appendix Figure A3). 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 a focus group 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.[16] We also confirmed that people trusted the source of the information cited in the letter (the Pontifical Catholic University of Chile).

[16] In these pretesting instances, we provided participants with letters containing figures similar to those finally sent (see Appendix Figures A2 and A3) and asked them to discuss their interpretation, ensuring they understood the information accurately.

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.[17] Our sample contains 59 municipalities out of 345.[18] The municipalities in our sample have between 101 and 1,176 polling stations, with an average of 389 stations per municipality.

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). Thus, we have 177 strata (59 municipalities times 3 terciles of percentage of female in the voting station). Each municipality in the sample contains six or seven treated polling stations (3–4 in each treatment arm), and we sent letters to all registered voters in these stations.

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. Within 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:

[17] 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.

[18] 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.

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

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

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 in each municipality 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:

$$y_{sm} = a * T_{sm}^{ave} + b * T_{sm}^{max} + c * Score_m * T_{sm}^{ave} + d * Score_m * T_{sm}^{max} + e * \Delta Score_m * T_{sm}^{ave} + f * \Delta Score_m * T_{sm}^{max} + X'_{sm}\mu + t_{sm} + u_{sm} \qquad (2)$$

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, 30-65, and 65 or more age ranges, and the number of stations in a merged station, which is 1 for non-merged stations), and t_{sm} captures strata fixed effects.

All the variables corresponding to vote shares are calculated based on the total number of registered voters, ensuring that their magnitudes are comparable. Coefficients a 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.[19]

Notice that given that strata are constructed based on the municipality and the polling station's gender composition, using strata fixed effects is more fine-grained that using municipality *and* gender fixed effects. Thus, the inclusion of strata fixed effects (t_{sm}) isolates between-municipality variation. Therefore, 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 (or e and f), 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 (2) 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 and gender at the polling station level (the only demographic variables

[19] We use robust standard errors in our estimates, reflecting the fact that treatments were allocated at the polling station level. Additionally, we also experimented with the use of standard errors clustered at the municipality level, given that the content of the letters varies at this level. Our findings remain robust under this adjustment (with slightly smaller standard errors). Results are available upon request.

available to us at this level), in which we only find a significant differences in the men's share for the average treatment.[20] Panel B presents the results for variables in the 2012, pretreatment, 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 of the two major coalitions (columns 5 and 6).

Table 3: Balance Tests

Panel A: Demographics

	(1) Age 18-30 (%)	(2) Age 30-59 (%)	(3) Men's Share (%)
Average Treatment	2.299	-1.853	-2.162***
	(2.368)	(1.988)	(0.673)
Average Treatment × Corrected Score	0.292	-0.207	0.069
	(0.270)	(0.217)	(0.083)
Average Treatment × Score Change	0.308	-0.334	-0.078
	(0.435)	(0.365)	(0.110)
Maximum Treatment	2.344	-2.213	-1.029
	(2.160)	(1.759)	(0.767)
Maximum Treatment × Corrected Score	0.005	0.018	-0.085
	(0.204)	(0.175)	(0.090)
Maximum Treatment × Score Change	-0.097	0.139	-0.226*
	(0.366)	(0.315)	(0.120)
Observations	21,031	21,031	22,387
R-squared	0.179	0.152	0.721
Control group mean	26.31	54.18	48.23

[20] This imbalance in men's share appears even though we stratified in the polling stations' gender composition. This may be due to the fact that, given the way that new voters were assigned to stations when automatic registration was introduced (see footnote 4), the distribution of the stations' men's share presents bunching on the extremes.

Panel B: Previous Municipal Election Outcomes (2012)

	(1) Registered Voters	(2) Turnout	(3) Incumbent's Share	(4) Challenger's Share	(5) Share Left	(6) Share Right
Average Treatment	2.805	-0.970	-0.470	-0.275	-0.291	-0.224
	(1.825)	(0.891)	(0.549)	(0.323)	(0.492)	(0.471)
Average Treatment × Corrected Score	0.113	-0.280**	-0.209***	-0.042	-0.089	0.059
	(0.228)	(0.115)	(0.074)	(0.042)	(0.055)	(0.050)
Average Treatment × Score Change	-0.327*	-0.158	-0.073	-0.051	0.049	-0.115
	(0.169)	(0.161)	(0.110)	(0.053)	(0.073)	(0.089)
Maximum Treatment	1.327	-0.378	-0.345	0.070	0.168	-0.448
	(1.921)	(0.860)	(0.528)	(0.309)	(0.423)	(0.468)
Maximum Treatment × Corrected Score	-0.113	0.009	-0.006	0.034	0.058	-0.051
	(0.227)	(0.093)	(0.057)	(0.035)	(0.042)	(0.050)
Maximum Treatment × Score Change	0.236	0.103	0.049	0.036	-0.033	0.024
	(0.196)	(0.154)	(0.110)	(0.050)	(0.069)	(0.083)
Observations	21,245	21,245	21,245	21,245	21,223	21,223
R-squared	0.334	0.377	0.464	0.493	0.825	0.867
Control group mean	335.82	37.89	19.57	11.74	41.33	37.35

Panel C: Previous Presidential Runoff Election Outcomes (2013)

	(1) Turnout	(2) Incumbent's Share	(3) Challenger's Share	(4) Share Left	(5) Share Right
Average Treatment	-0.957	-1.085*	0.031	0.655	-0.919
	(0.822)	(0.626)	(0.546)	(0.654)	(0.602)
Average Treatment × Corrected Score	-0.275***	-0.238***	-0.117	-0.135*	0.103
	(0.107)	(0.071)	(0.079)	(0.070)	(0.068)
Average Treatment × Score Change	-0.267*	-0.314**	-0.067	0.122	-0.139
	(0.145)	(0.124)	(0.091)	(0.123)	(0.118)
Maximum Treatment	-0.717	-0.192	-0.139	0.866	-0.811
	(0.806)	(0.666)	(0.549)	(0.607)	(0.553)
Maximum Treatment × Corrected Score	-0.028	-0.051	0.106	0.072	-0.069
	(0.092)	(0.064)	(0.071)	(0.066)	(0.060)
Maximum Treatment × Score Change	0.106	0.067	0.053	-0.033	0.045
	(0.144)	(0.158)	(0.092)	(0.102)	(0.094)
Observations	21,592	17,659	18,668	21,592	21,592
R-squared	0.378	0.612	0.543	0.527	0.557
Control group mean	40.81	21.86	19.38	59.16	38.35

Notes: Panels A, B, and C present the estimation of equation 2 (excluding demographic controls and number of stations in merged station) for different sets of variables. All shares of votes are calculated over total registered voters. 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 runoff (2013): 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 strata fixed effects, defined by municipality and the station's gender composition. 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 (2) but for the 2013 presidential election.[21] We present the results in Panel C of Table 3. 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. Thus, our randomly selected treatment polling stations appear to have had a stable feature (those in municipalities with higher SIMCE scores had lower turnout and votes for the incumbent) that was even present in one presidential election before the treatment was implemented: 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:[22]

$$\Delta y_{sm} = \theta * T_{sm}^{ave} + \kappa * T_{sm}^{max} + \beta * Score_{m} * T_{sm}^{ave} + \gamma Score_{m} * T_{sm}^{max} + \sigma \Delta Score_{m} * T_{sm}^{ave} + \Psi * \Delta Score_{m} * T_{sm}^{max} + X'_{sm}\mu + \tau_{sm} + \varepsilon_{sm}, \qquad (3)$$

which follows the same structure as equation (2) 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 same

[21] We construct a proxy for the share of support for the incumbent mayor in the presidential election runoff 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 and use the runoff, where only one candidate from each of the two major coalitions competed. This implies losing some observations because either the incumbent or the challenger at the local level were not part of the dominant national coalitions.

[22] We formally test for parallel trends in Appendix Table A3 by estimating a placebo difference-in-difference for the 2013 presidential vs. the 2012 municipal election combining the data presented in Panels B and C of Table 3. We discuss these results in Section 4.1.

variables mentioned in equation (2), i.e., the fraction of voters in the 18-30, 30-65, and 65 and more age ranges, the number of stations in a merged station, and strata fixed effects).[23]

The estimates from equation (3) 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.[24] In addition, individuals in treated polling stations could have shared the information in the letters with others, producing spillover effects.[25] Yet, even adopting a conservative approach, our data suggests that spillovers were quite limited: assuming all voters residing at the same address as a voter in a treated station were also treated, we find that the average number of treated individuals in control stations would be 1.06% for the average treatment and 1.14% for the maximum treatment, while at the 90th percentile, these figures are 2.48% and 2.72%, respectively. We were also aware of a limited degree of spillovers via social networks; however, an analysis of Twitter data indicates that such occurrences were rare.[26] 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.

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[23] 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).

[24] 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 us 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. For the municipalities included in our study, 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.

[25] We sent the letters to a small proportion of polling stations in each municipality (ranging from 0.5 to 6.3%, with an average of 2.4%) and did so just a few days before the election to minimize spillover effects.

[26] An analysis of a sample of 40,000 tweets during the period of our intervention, using the Harvard CGA Geotweet Archive, revealed no mentions of our experiment. It is important to note, however, that only 9% of Chileans frequently engage with political issues on social networks, and 60% report never discussing politics with family or friends (CEP 2018).

2019). In any case, both problems imply attenuation bias, meaning that estimates from equation (3) would correspond to lower bounds of the true effects of the treatment, should these problems prove to be significant.

A final concern regarding the identification of effects is potential incumbent mayors' reactions 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 results, focusing on three dependent variables at the polling station level—voter turnout, support for the incumbent, and support for the main challenger[27]—all measured as the share of the total registered voters in the polling station. We start by presenting the main results and robustness checks, then present exercises to better understand how voters respond to the content of the letters, and finish with two additional exercises: heterogeneity and spillovers to the municipal council's election.

[27] Appendix Table A4 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 (3). We take a step back from equation (3) 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 $\beta=\gamma=\sigma=\Psi=0$) and find that neither treatment affected turnout.

The null findings for any of the letters *per se* are important for two reasons. First, they allow assessing the average impact of the letters on outcomes, especially considering that the letters also informed voters about the upcoming election and may have prompted them to think about them. Second, these results are consistent with the fact that, depending on the municipality's outcomes, the letters convey very different messages that need not change voters' behavior on average. 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 estimate all the elements of equation (3) together 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 95% level (with no significant direct or indirect effects through the score change). We do not find effects for T^{max}. 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 in levels of test scores (not changes).

Table 4: Main Results

	Turnout			Incumbent Share			Challenger Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any Treatment	0.310			0.208			0.067		
	(0.293)			(0.196)			(0.163)		
Average Treatment		0.649	0.648		0.288	0.358		0.223	0.159
		(0.396)	(0.395)		(0.262)	(0.274)		(0.237)	(0.257)
Maximum Treatment		-0.031	-0.034		0.128	0.107		-0.092	-0.011
		(0.425)	(0.415)		(0.289)	(0.289)		(0.220)	(0.233)
Average Treatment × Corrected Score			0.104**			0.095***			0.004
			(0.042)			(0.031)			(0.027)
Average Treatment × Score Change			-0.054			-0.008			-0.039
			(0.072)			(0.052)			(0.048)
Maximum Treatment × Corrected Score			-0.002			-0.008			0.008
			(0.044)			(0.031)			(0.025)
Maximum Treatment × Score Change			0.001			-0.008			0.050
			(0.089)			(0.067)			(0.044)
Observations	21,018	21,018	21,018	21,018	21,018	21,018	21,018	21,018	21,018
R-squared	0.435	0.435	0.436	0.635	0.635	0.635	0.679	0.679	0.679
Control group mean	29.99	29.99	29.99	14.14	14.14	14.14	9.53	9.53	9.53

Notes: This table presents the results of estimating several variations of equation (3) for differences between the 2016 and 2012 municipal elections. Columns (1) to (3) present estimates on differences in turnout, columns (4) to (6) report estimates on differences in the incumbent vote share, and columns (7) to (9) present estimates for differences in the challenger vote share. All shares of votes are calculated over total registered voters. Columns (1), (4) and (7) have as the main regressor a dummy variable equal to 1 if the polling station was assigned to any treatment. Columns (2), (5) and (8) 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. Finally, columns (3), (6) and (9) present the results of estimating equation (3), so they include as the main regressors both treatment dummies and their interactions with corrected score and score change. All estimates include strata fixed effects, defined by municipality and the station's gender composition. Controls include the gender and age composition of the polling station, and the number of stations in a merged station. The "Control group mean" row provides averages of outcomes in 2016. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Columns (4) to (6) and (7) to (9) present the results for the treatments' effects on support for the incumbent and main challenger, respectively. 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, 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.[28]

Our results on the electoral effects of information are both statistically and economically significant. Receiving information of corrected SIMCE score that is 10 points higher (note that 14 municipalities in our sample experienced score changes of at least 10 points during the previous mayoral term) on average is correlated with an increase in turnout by 1.04 percentage points and support for the incumbent by 0.95 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, our estimates are probably lower bounds of the effects of information.

As previously discussed, a challenge in interpreting our results as causal is that the content of the report cards, which varies across municipalities, is not randomly allocated. We offer two pieces of evidence to support a causal interpretation for our results and the actual content of the report cards. First, the effects are observed only for the interaction of T^{ave} with corrected SIMCE (and not with T^{max}), suggesting that the results are not driven by interactions of T^{ave} with any unobserved variable that varies at the municipality level. Second, in Table 5, we further test the relevance of potentially confounding variables that may interact with treatment to explain our results. We test interactions with 11 municipality-level variables that are unrelated to the content of the letters and account for differences in other dimensions between municipalities: 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. In column (1) of Table 5 we present the specifications for turnout (panel A) and incumbent share (panel B) presented in columns (3) and (6) of Table 4, as a baseline, and then in each column we add the interactions of T^{ave} and T^{max} with one of the 11 possible

[28] 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. 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.

confounding variables.[29] The 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 findings are not driven by confounding factors but truly by the content of the letters. These results reinforce our interpretation of the observed effects as a causal effect of information on educational results.

Finally, our estimates in first differences rely on the parallel trends assumption. We formally test this assumption in Appendix Table A3 by running a regression using the differences for the presidential elections in 2013 and the previous municipal election in 2012. The specification mirrors that of our main equation but uses the 2013-2012 differences as the left-hand side variable. This is a direct test of parallel trends and we focus the discussion on the coefficient for the interaction between average treatment and corrected score, the relevant variable from our previous results. As it is noticeable, the coefficients for this interaction are small and not statistically significant for all our outcomes, thus confirming the main assumption of the diff-in-diff specification. This exercise also allows us to investigate whether our results for the 2016 municipal election are driven by mean-reversion, given the imbalance of the 2012 election. If mean reversion were an issue in our setting, we would expect an effect in the difference between the 2013 and 2012 elections; yet, in line with our identification assumptions, we find no effects for the interaction of the treatment with the corrected score. This supports the conclusion that our previous results are not driven by underlying different trends spuriously related to our treatment.

[29] 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	0.648	0.557	-0.082	0.018	0.794	-0.171	0.177	0.311	1.305	1.328	1.070	0.559
	(0.395)	(0.617)	(1.103)	(12.644)	(0.528)	(3.300)	(0.764)	(0.450)	(0.878)	(0.965)	(0.778)	(0.938)
Maximum Treatment	-0.034	-0.904	-0.850	-10.078	0.202	6.553*	-0.918	-0.025	-1.778	-1.301	1.567**	0.293
	(0.415)	(0.665)	(1.091)	(12.603)	(0.516)	(3.596)	(0.840)	(0.484)	(1.205)	(1.038)	(0.766)	(1.090)
Average Treatment × Corrected Score	0.104**	0.101**	0.093**	0.104**	0.104**	0.104**	0.099**	0.085**	0.086*	0.089**	0.099**	0.104**
	(0.042)	(0.042)	(0.044)	(0.043)	(0.043)	(0.044)	(0.042)	(0.041)	(0.045)	(0.045)	(0.043)	(0.043)
Maximum Treatment × Corrected Score	-0.002	-0.006	-0.010	-0.011	0.001	-0.023	-0.006	-0.015	0.024	0.017	-0.014	-0.001
	(0.044)	(0.045)	(0.045)	(0.044)	(0.045)	(0.046)	(0.045)	(0.046)	(0.043)	(0.046)	(0.043)	(0.043)
Average Treatment × Score Change	-0.054	-0.056	-0.041	-0.053	-0.048	-0.058	-0.063	-0.054	-0.044	-0.042	-0.047	-0.053
	(0.072)	(0.077)	(0.079)	(0.072)	(0.076)	(0.075)	(0.078)	(0.081)	(0.077)	(0.076)	(0.077)	(0.072)
Maximum Treatment × Score Change	0.001	-0.018	0.020	0.008	0.011	0.042	-0.019	-0.014	-0.009	0.002	0.028	0.001
	(0.089)	(0.092)	(0.099)	(0.088)	(0.093)	(0.100)	(0.093)	(0.079)	(0.093)	(0.096)	(0.094)	(0.089)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,018	19,459	19,459	21,018	19,459	19,459	19,459	18,163	19,361	17,886	19,459	21,018
R-squared	0.436	0.438	0.438	0.436	0.438	0.438	0.438	0.453	0.437	0.455	0.438	0.436
Control group mean	29.99	30.11	30.11	29.99	30.11	30.11	30.11	30.18	30.06	30.37	30.11	29.99

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	0.358	0.480	0.036	2.436	0.579	-0.513	0.292	0.337	1.360**	0.663	0.597	0.151
	(0.274)	(0.435)	(0.784)	(9.411)	(0.357)	(2.173)	(0.545)	(0.334)	(0.638)	(0.812)	(0.452)	(0.579)
Maximum Treatment	0.107	-0.014	-0.206	-7.072	0.185	3.869	-0.201	0.268	-0.075	-0.533	0.448	0.696
	(0.289)	(0.448)	(0.857)	(11.946)	(0.353)	(2.900)	(0.568)	(0.358)	(0.959)	(0.880)	(0.545)	(0.667)
Average Treatment × Corrected Score	0.095***	0.092***	0.088***	0.096***	0.094***	0.094***	0.091***	0.092***	0.076**	0.098***	0.090***	0.094***
	(0.031)	(0.031)	(0.032)	(0.031)	(0.032)	(0.032)	(0.031)	(0.032)	(0.033)	(0.032)	(0.031)	(0.031)
Maximum Treatment × Corrected Score	-0.008	-0.008	-0.011	-0.014	-0.007	-0.020	-0.009	-0.012	0.001	-0.004	-0.009	-0.005
	(0.031)	(0.031)	(0.033)	(0.029)	(0.031)	(0.031)	(0.031)	(0.032)	(0.029)	(0.032)	(0.032)	(0.030)
Average Treatment × Score Change	-0.008	0.005	0.010	-0.009	0.012	-0.001	0.001	-0.003	0.011	0.003	0.006	-0.008
	(0.052)	(0.054)	(0.055)	(0.051)	(0.053)	(0.054)	(0.055)	(0.058)	(0.054)	(0.055)	(0.055)	(0.052)
Maximum Treatment × Score Change	-0.008	-0.004	0.009	-0.003	-0.001	0.023	-0.008	-0.022	-0.004	0.006	0.005	-0.008
	(0.067)	(0.072)	(0.074)	(0.069)	(0.072)	(0.077)	(0.073)	(0.065)	(0.072)	(0.074)	(0.073)	(0.068)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,018	19,459	19,459	21,018	19,459	19,459	19,459	18,163	19,361	17,886	19,459	21,018
R-squared	0.635	0.647	0.647	0.635	0.647	0.647	0.647	0.647	0.646	0.661	0.647	0.635
Control group mean	14.14	14.03	14.03	14.14	14.03	14.03	14.03	13.95	13.96	14.22	14.03	14.14

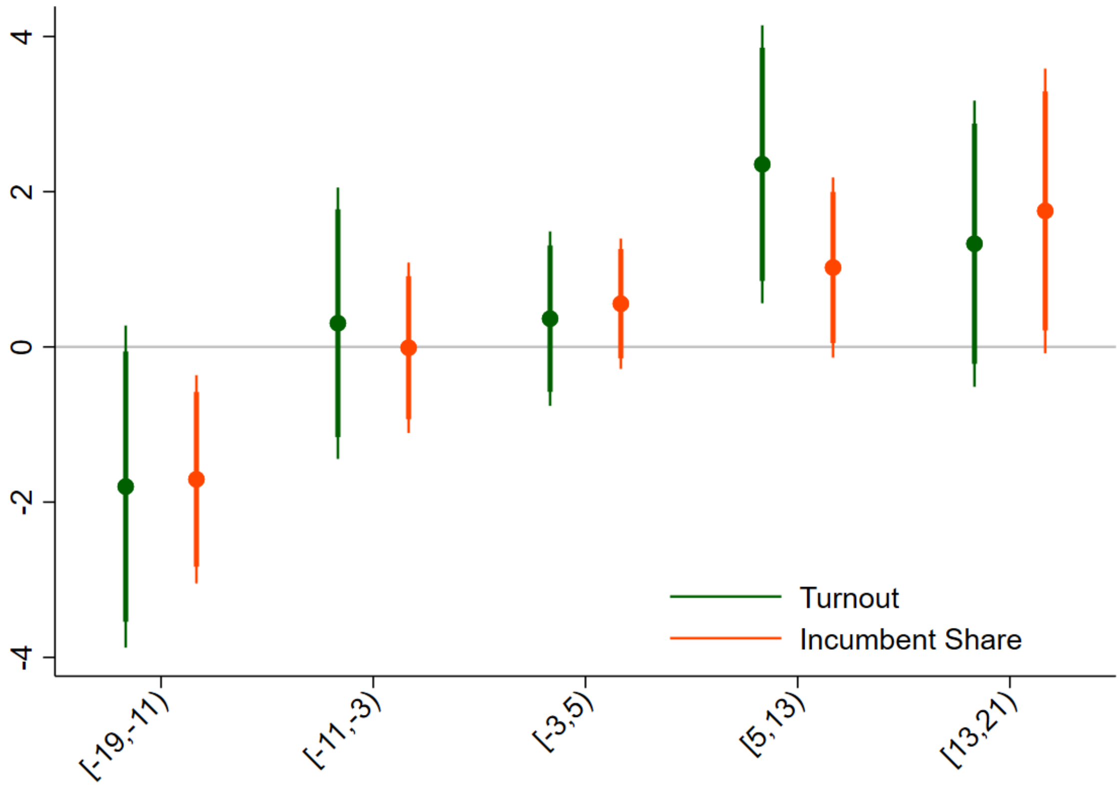
Notes: Panel (A) presents this robustness check for turnout and Panel (B) for the incumbent's share. In both cases, Column (1) presents the results for the estimation for the main effects from Table 4 (Columns 3 and 6, respectively), and then each column adds the interactions of T^{ave} and T^{max} with one possible municipality-level confounding variable: population, share of students in public schools, income, dummy variable for right-wing mayor, number of poor students, public health centers per capita, maintained parks per capita, expenditure in parks per capita, crime, and the incumbent's electoral expenditure. All estimates include strata fixed effects, defined by municipality and the station's gender composition. Controls include the gender and age composition of the polling station, and the number of stations in a merged station. The "Control group mean" row provides averages of outcomes in 2016. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.2 Whether and When Information Matter to Voters

We now proceed to conduct a series of exercises to further understand the mechanisms underlying the effects we have observed. First, we explore whether the effects of the contents of the report cards vary in a non-linear way depending on the educational outcomes reported. We begin with a graphical analysis, dividing the distribution of $Score$ into five bins, and presenting the effects of the interaction of $Score$ and T^{ave} on turnout and the incumbent's share.[30] Figure 4 displays the results. Although the interaction between $Score$ and T^{ave} is rarely significant (the sample size is much smaller), it depicts a pattern in which treatment effects are negative at lower scores, increase with higher scores, and then plateau as scores become sufficiently positive, suggesting a steeper slope for "bad" results (i.e., when $Score$ is sufficiently negative). This finding not only confirms our previous evidence but also suggests asymmetric effects based on these results.

[30] We omitted the results for score change, the interaction with T^{max}, and the challenger's share to save space. They basically show a set of null results.

Figure 4: Main effects for the interaction of average treatment with score, by bins of score



Note: This figure presents the results of estimating equation (3) for turnout and incumbent vote share but replacing the interaction between our Average Treatment dummy and Simce score, for an interaction between Average Treatment and a set of dummies for 8-SIMCE-point bins of Corrected SIMCE. All estimations include strata fixed effects, defined by municipality and the station's gender composition. Controls include the gender and age composition of the polling station, and the number of stations in a merged station.

Second, in Table 6 we formally test for asymmetric effects, allowing the effects to vary for "good" and "bad" educational results, defined based on whether the corrected score is above or below zero (recall that it averages zero). The specification is:

$$y_{sm} = \alpha + \theta * T_{sm}^{ave} + \kappa * T_{sm}^{max} + \delta_1 * T_{sm}^{ave} * \mathbf{1}(Score > 0) + \delta_2 * T_{sm}^{max} * \mathbf{1}(Score > 0) + \delta_3 * T_{sm}^{ave} * \mathbf{1}(\Delta Score > 0) + \delta_4 * T_{sm}^{max} * \mathbf{1}(\Delta Score > 0) + \beta_1 * Score^{+}{}_{m} * T_{sm}^{ave} + \beta_2 * Score^{-}{}_{m} * T_{sm}^{ave} + \gamma_1 Score^{+} * T_{sm}^{max} + \gamma_2 Score^{-} * T_{sm}^{max} + \sigma_1 \Delta Score^{+}{}_{m} * T_{sm}^{ave} + \sigma_2 \Delta Score^{-}{}_{m} * T_{sm}^{ave} + \Psi_1 * \Delta Score^{+} * T_{sm}^{max} + \Psi_2 * \Delta Score^{-} * T_{sm}^{max} + X'_{sm}\mu + \tau_{sm} + \varepsilon_{sm} \qquad (3)$$

where $\mathbf{1}(Score > 0)$ and $\mathbf{1}(\Delta Score > 0)$ are dummies that indicate positive values for *Score* and *Δ Score*, respectively. $Score^{+}{}_{m}$ 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. Consistent with the graphical analysis in Figure 4, 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 is associated with a 1.82-percentage-point reduction in turnout (significant at the 90% level), which translates almost one-to-one into less support for the incumbent (significant at the 99% level). These results are much bigger than the effects estimated above for the symmetric specification, which restricted good and bad information to have equal effects. The effects of reporting a positive corrected does not have a statistically significant slope. 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 asymmetry in individuals' behavior is consistent with the fact that people tend to adjust their expectations upward faster than downward (Duesenberry 1949; Burchardt 2005; Ward 2015), and that they tend to dislike losses more than they like equal gains (Kahneman and Tversky 1979).

Table 6: Asymmetric Effects

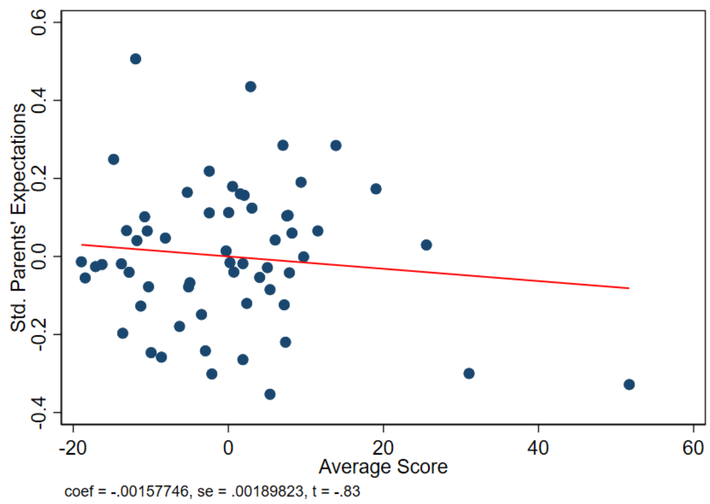
	Turnout	Incumbent Vote Share	Challenger Vote Share
	(1)	(2)	(3)
Intercepts			
Average Treatment	0.910	1.215	-0.438
	(1.534)	(0.914)	(0.864)
Maximum Treatment	0.355	-0.435	0.205
	(1.454)	(1.069)	(0.674)
Average Treatment $\times$ 1(Corrected Score$^+$)	-0.795	-0.948	0.503
	(1.353)	(0.912)	(0.855)
Maximum Treatment $\times$ 1(Corrected Score$^+$)	-1.290	-0.148	-0.548
	(1.477)	(0.983)	(0.716)
Average Treatment $\times$ 1(Score Change$^+$)	0.379	-0.923	1.336
	(1.448)	(0.878)	(0.838)
Maximum Treatment $\times$ 1(Score Change$^+$)	0.506	1.335	0.041
	(1.374)	(0.976)	(0.686)
Interactions with Levels			
Average Treatment $\times$ Corrected Score$^+$	0.103	0.086	-0.041
	(0.081)	(0.070)	(0.058)
Maximum Treatment $\times$ Corrected Score$^+$	0.109	0.049	-0.003
	(0.094)	(0.058)	(0.058)
Average Treatment $\times$ Corrected Score$^-$	0.182*	0.172***	0.024
	(0.102)	(0.060)	(0.072)
Maximum Treatment $\times$ Corrected Score$^-$	0.012	-0.029	0.071
	(0.131)	(0.090)	(0.062)
Interactions with Changes			
Average Treatment $\times$ Score Change$^+$	-0.005	0.139	-0.135
	(0.140)	(0.097)	(0.107)
Maximum Treatment $\times$ Score Change$^+$	-0.065	-0.138	0.119
	(0.148)	(0.141)	(0.093)
Average Treatment $\times$ Score Change$^-$	-0.129	-0.014	-0.126
	(0.188)	(0.113)	(0.096)
Maximum Treatment $\times$ Score Change$^-$	0.046	-0.036	-0.019
	(0.240)	(0.162)	(0.097)
Observations	21,018	21,018	21,018
R-squared	0.436	0.635	0.679
Control group mean	29.99	14.14	9.53

Notes: This table present estimates of our asymmetric version of equation (3). Columns (1) to (3) report estimates for the differences in turnout, the incumbent vote share, and the challenger's between the 2016 and 2012 municipal elections, respectively. $\mathbf{1}(Score > 0)$ and $\mathbf{1}(\Delta Score > 0)$ are dummies that indicate positive values for $Score$ and Δ $Score$, respectively. All shares of votes are calculated over total registered voters. 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 strata fixed effects, defined by municipality and the station's gender composition, plus controls for the gender and age composition of the polling station, and the number of stations in a merged station. The "Control group mean" row provides averages of outcomes in 2016. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 5: School Quality: Correlation between Prior Beliefs and Outcomes

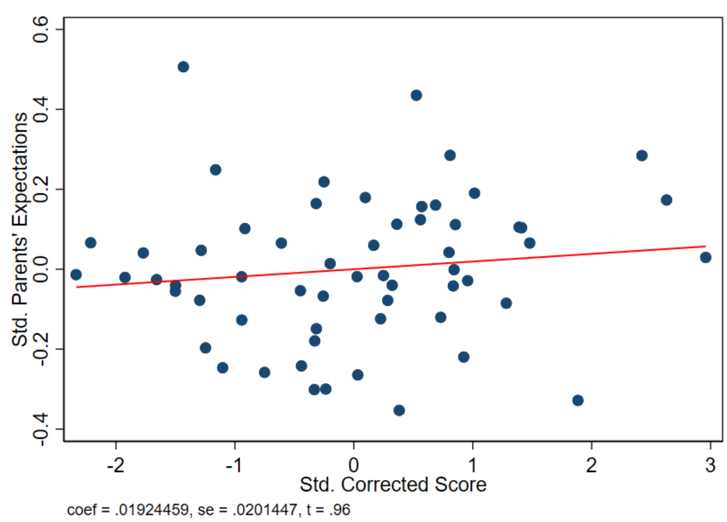
(A)

Parents' beliefs and SIMCE at the municipality level



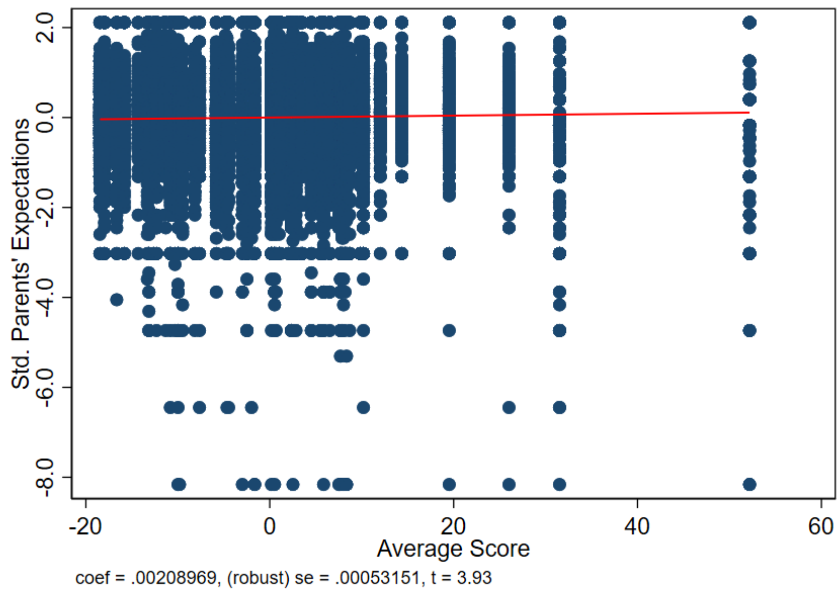
(B)

Parents' beliefs and corrected SIMCE at the municipality level



(C)

Parents' beliefs and SIMCE at the polling-station level



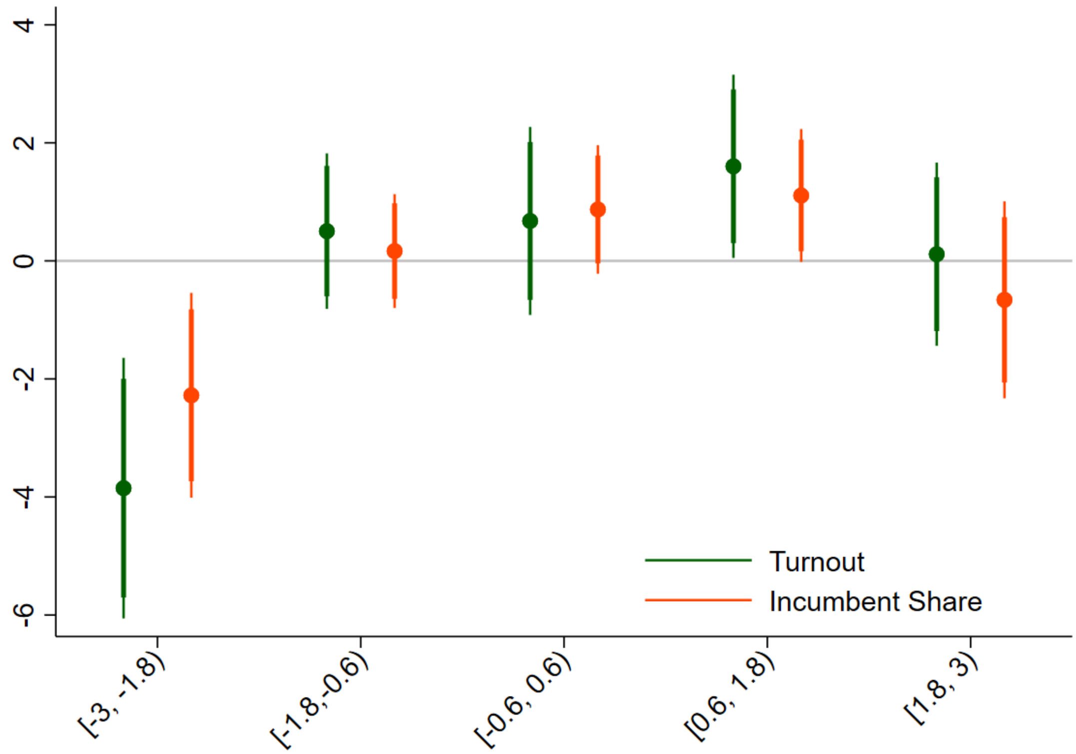
Notes: Standardized Parents' Expectations is the standardized average of parents' expectations about publicly financed schools according to data from the SIMCE parents' survey at the polling station level. Each dot represents one municipality. The slope represents the ordinary least squares coefficient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

Third, delving deeper into how voters react to information in educational results, 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 to some voters, but a confirmation to others. We expect stronger effects on outcomes if the letter provides new, 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 at the polling-station level from a universal survey administered to parents of school-aged children included in the SIMCE package, using the data provided and merged by the Chilean Quality of Education Agency.[31] Figure 5 plots beliefs about 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 level vary greatly within-municipality. This evidence suggests voters are very poorly informed about public schools' quality, consistent with prior research (Allende et al. 2019).

To investigate the hypothesis of heterogeneous impacts driven by the provision of *news* regarding education quality, we create a continuous "news" measure, which we calculate as the difference between the standardized actual educational outcomes reported in the letters and the standardized parents' prior beliefs about at the polling station level. We start presenting Figure 6, which displays the results for our flexible approach, which divides the distribution of *News* into five bins, and presents the effects of the interaction between $News$ and T^{ave} on turnout and the incumbent's vote share.[32] Given that the number of observations of prior beliefs per station is small and variable (see Section 2.2), we weight the regressions by the number of parents' observations at each polling station in both exercises.

[31] 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.
[32] As before, we omitted the results for score change, the interaction with T^{max}, and the challenger's share to save space. They basically show a set of null results.

Figure 6: Main effects for the interaction of average treatment with news, by bins of news



Note: This figure presents the results of estimating equation (3) for turnout and incumbent vote share, replacing the interaction between the Average Treatment dummy and Simce score with an interaction between Average Treatment and a set of dummies for 1.2-point bins of News. All estimations include strata fixed effects, defined by municipality and the station's gender composition. Controls include the gender and age composition of the polling station, and the number of stations in a merged station.

The results are consistent with our previous findings, suggesting an asymmetric effect with stronger responses to bad news that then stabilize as News become less negative. As News become sufficiently good, the effects cease to increase, implying that the slope of the relationship with News and outcomes is no longer positive.

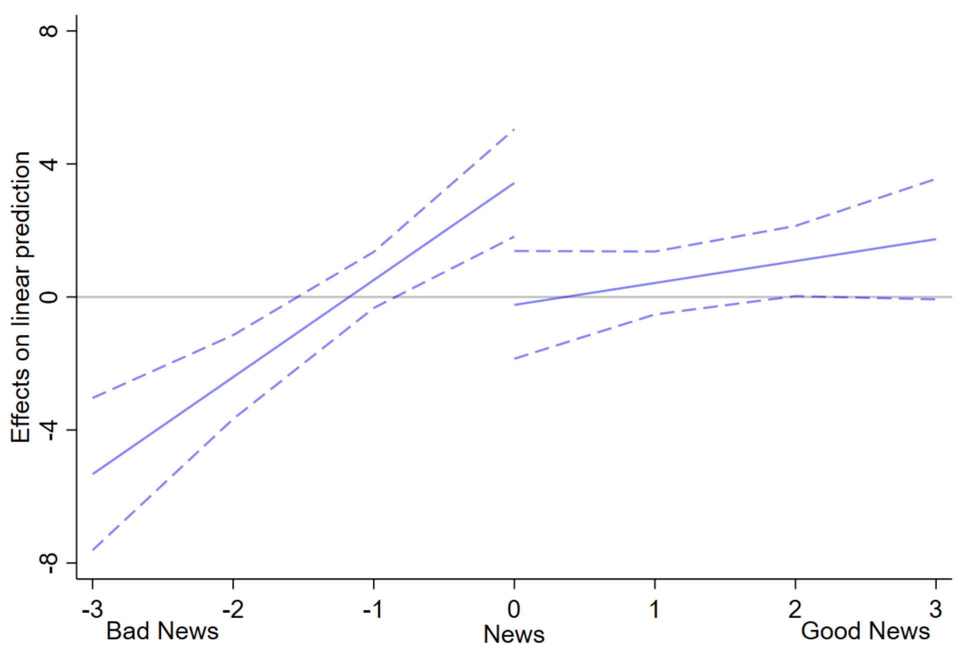
Then, we modify equation (3) to estimate the effects of this news measure interacted with the treatments, and allowing for heterogeneous effects based on whether the measure is positive or not (i.e., representing good vs. bad news). The estimating equation is:

$$\Delta y_{sm} = \theta * T_{sm}^{ave} + \kappa * T_{sm}^{max} + \delta_1 * T_{sm}^{ave} * \mathbf{1}(News > 0) + \delta_2 * T_{sm}^{max} * \mathbf{1}(News > 0) + \beta_1 * News^{+}{}_{sm} * T_{sm}^{ave} + \beta_2 * News^{-}{}_{sm} * T_{sm}^{ave} + \beta_3 * News^{+}{}_{sm} * T_{sm}^{max} + \beta_4 * News^{-}{}_{sm} * T_{sm}^{max} + X'_{sm}\mu + \tau_{sm} + \varepsilon_{sm} \qquad (4)$$

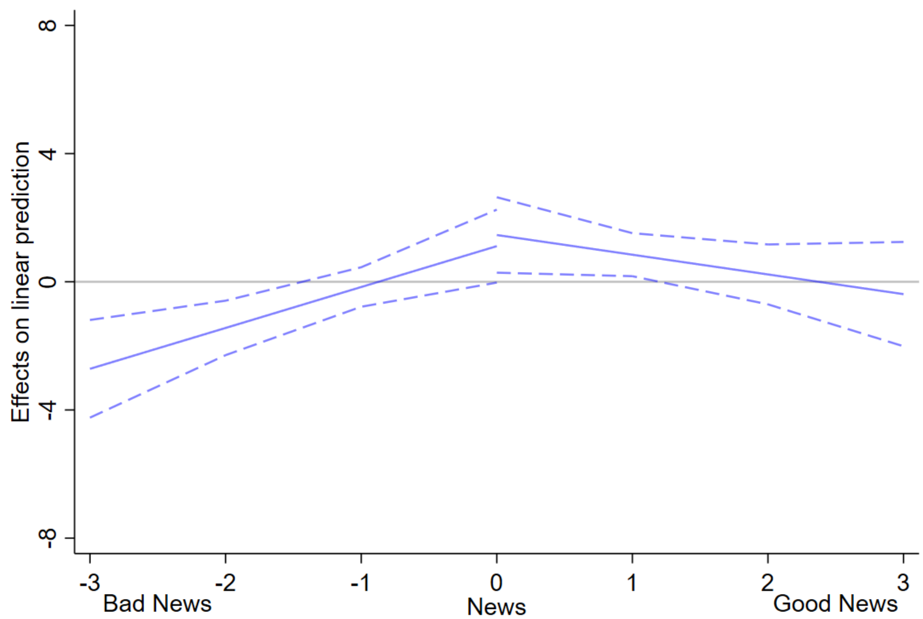
where $\mathbf{1}(News > 0)$ is a dummy that takes the value of 1 if News is positive. $News^{+}{}_{sm}$ corresponds to the measure of news for polling station s in municipality m when the news is positive and 0 otherwise. Similarly, $News^{-}{}_{sm}$ denotes the measure of news when the news is negative and 0 otherwise.

Figure 7: Marginal effect of average treatment depending on news

(A) Turnout



(B) Incumbent vote share



Note: These figures represent the marginal effect of the interaction of the continuous measure of news and the average treatment, as defined in equation (4), on turnout (panel A) and the incumbent's vote share (panel B). See Appendix Table A5.

Figure 7 displays how the effects vary along bad vs. good news (we do not report effects on the challenger share to save space), and Appendix Table A5 presents the estimates for all coefficients. These results suggest that there is positive slope when the news implicit in the treatment letter is bad, for both turnout and support for the incumbent. In turn, the effects for good news do not seem to vary in statistically significant way by the level of the shock. 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."

All in all, 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. 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 certain types of information, and especially when such information implies "bad news," voters do punish mayors who fail the test.

4.3 Additional Exercises

In this section, we conduct two additional analyses to examine (i) heterogeneity across municipalities to determine whether the effects of information are more pronounced in certain contexts and (ii) an extension concerning potential spillovers to the election of municipal council members.

First, 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 located in municipalities with different characteristics (Table 7).[33] The motivation of these exercises is to shed light on the mechanisms behind our main results. In particular, we study whether the information has different impacts in municipalities depending on their socioeconomic status, political outcomes, and educational markets. Overall, we lack the power to identify statistically significant differences, however, the differences in point estimates suggest relevant heterogeneities across several dimensions, which, although not statistically significant, are large and consistent and may still indicate that some groups have stronger effects.

The treatment effects differ little based on the incumbent's political leaning and on public-school enrollment. The results by income level suggest stronger effects for poorer municipalities, consistent with previous evidence that implies that poorer voters may lack educational outcome information (as documented in Allende et al. 2019). The effects also seem somewhat stronger in smaller municipalities (especially for the incumbent's share) and especially in those where 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

[33] We only report the effects for β after estimating the complete specification in equation (3). We also report average treatment effects as a reference. Appendix Table A6 presents correlations among all the variables we use to study heterogeneous effects.

more salient when the incumbent's campaign efforts are more limited, consistent with research documenting the effects of campaign spending on the incumbent's advantage (e.g., Avis et al. 2020).

Overall, the heterogeneity results, although not conclusive, 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; more research is needed.

Next, we explore whether the treatment influences the election of the municipality council. In Chile, these campaigns are strongly organized around parties and political coalitions, with councilors often running joint campaigns with mayoral candidates. Therefore, we examine the potential spillover effects of the report cards on these elections. It is important to note that the occurrence of such spillovers is not straightforward. On the one hand, the complexity of the election of councilors (where voters must choose among many lesser-known candidates for six to 10 slots) suggests that voters might rely on information from the mayoral election as for councilor voting (e.g., Ansolabehere et al. 2006). This implies that our treatment letters could also impact councilor elections (see Appendix Figures A5 and A6 for examples of the complexity of a councilor election ballot and campaign posters of councilors campaigning with mayors). However, on the other hand, the transmission of information to this different election may be limited, given the imperfect mapping between the responsibility of mayors and councilors in managing public education, and the more personalistic nature of the mayoral election.[34]

[34] Indeed, the partial correlation between turnout in the mayoral election and the councilors' election within the control group is 0.99. In turn, the correlation between support for the mayor and the councilors in the mayor's coalition is 0.67 for the control group. These figures suggest a high but not perfect alignment between the outcomes of both elections.

Table 7: Summary of Heterogeneous Effects by Municipality Type

	(1) Turnout	(2) Incumbent vote share	(3) Challenger vote share
No Heterogeneity			
Average Treatment × Corrected Score	0.104**	0.095***	0.004
	(0.042)	(0.031)	(0.027)
Incumbent Political Coalition			
Concertacion	0.114*	0.077*	0.006
	(0.063)	(0.046)	(0.034)
Alianza	0.103	0.094**	0.030
	(0.067)	(0.047)	(0.052)
p-value Difference	[0.904]	[0.799]	[0.699]
Municipality's share of students in public schools			
Lower share for public schools	0.098	0.080	0.021
	(0.060)	(0.050)	(0.040)
Higher share for public schools	0.092	0.093**	-0.010
	(0.060)	(0.038)	(0.038)
p-value Difference	[0.947]	[0.839]	[0.574]
Municipality's average income			
Lower income municipalities	0.119***	0.105***	0.016
	(0.045)	(0.033)	(0.030)
Higher income municipalities	0.017	0.051	-0.076
	(0.093)	(0.079)	(0.054)
p-value Difference	[0.323]	[0.529]	[0.137]
Municipality size			
Smaller municipalities	0.117**	0.108***	-0.008
	(0.049)	(0.037)	(0.029)
Larger municipalities	0.111	0.051	0.123*
	(0.113)	(0.069)	(0.073)
p-value Difference	[0.961]	[0.465]	[0.098]
Incumbent electoral spending			
Lower electoral spending	0.117**	0.105***	0.008
	(0.047)	(0.035)	(0.030)
Higher electoral spending	0.021	0.021	-0.021
	(0.135)	(0.093)	(0.083)
p-value Difference	[0.502]	[0.399]	[0.743]

Notes: This table presents estimates of the heterogeneous effects of the interaction of our Average Treatment and the corrected SIMCE score (β in equation 3), over several municipality 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. All shares of votes are calculated over total registered voters. As a benchmark, the first subpanel shows 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 ("Incumbent Political Coalition") estimates equation (3) separately for polling stations in municipalities with an incumbent affiliated with Concertación (left-wing coalition) and Alianza (right-wing coalition). The third subpanel ("Municipality's share of students in public schools") estimates equation (3) 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 ("Municipality's average income") estimates equation (3) separately for polling stations in municipalities under or over the median average municipality income. The fifth subpanel ("Municipality size") estimates equation (3) separately for polling stations in municipalities under or over the median municipality population. The sixth subpanel ("Incumbent electoral spending") estimates equation (3) separately for polling stations in municipalities under or over the median incumbent electoral spending. All estimations include gender and age composition as controls, and the number of stations in a merged station. Robust standard errors in (round) parentheses. p-value of the hypothesis of equal coefficients for both groups in [squared] parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Therefore, in Table 8 we examine whether the treatment effects spill over to the councilors' election, defining the incumbent's and challenger's share based on whether council candidates belong to the incumbent/challenger mayor's coalition (Alianza/Concertación).[35] Our discussion here primarily focuses on the coefficient of the interaction between $Score$ and T^{ave}. In line with our findings for mayors, we observe effects that increase with Simce scores, although they are only significant for support for the incumbent, at the 95% level and in the same order of magnitude. The coefficients of the interaction we are interested in suggest an impact between 64% and 70% of that in the mayoral elections (for the effects on turnout and incumbent support). The values being less than 1, yet positive and relatively substantial, align with our prior arguments and reveal a partial transmission of treatment effects to the councilor elections.

[35] We group electoral pacts according to whether they belonged to Concertación or Alianza. We lost the observations with incumbents/challengers from pacts not belonging to Alianza/Concertación because the pacts are not the same for the mayoral and council election, and the mapping becomes arbitrary. We restrict the sample to stations with data for all the outcomes.

Table 8: Main effect on municipality council's election

	Turnout			Incumbent Pact Share			Challenger Pact Share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any Treatment	0.342			0.169			0.319*		
	(0.290)			(0.193)			(0.170)		
Average Treatment		0.613	0.584		0.162	0.008		0.546**	0.635**
		(0.393)	(0.394)		(0.279)	(0.333)		(0.253)	(0.276)
Maximum Treatment		0.067	0.039		0.176	0.271		0.090	0.105
		(0.422)	(0.413)		(0.264)	(0.260)		(0.224)	(0.228)
Average Treatment × Corrected Score			0.066			0.066**			-0.001
			(0.045)			(0.032)			(0.028)
Average Treatment × Score Change			-0.050			-0.122**			0.051
			(0.073)			(0.062)			(0.049)
Maximum Treatment × Corrected Score			-0.019			-0.013			-0.011
			(0.045)			(0.031)			(0.021)
Maximum Treatment × Score Change			-0.008			0.072			0.016
			(0.091)			(0.053)			(0.044)
Observations	20,970	20,970	20,970	20,970	20,970	20,970	20,970	20,970	20,970
R-squared	0.420	0.420	0.420	0.655	0.655	0.655	0.766	0.766	0.766
Control group mean	29.93	29.93	29.93	11.24	11.24	11.24	8.35	8.35	8.35

Notes: This table presents the results of estimating several variations of equation (3) for differences between the 2016 and 2012 municipal council elections. Columns (1) to (3) present estimates on differences in turnout, columns (4) to (6) report estimates on differences in the incumbent vote share, and columns (7) to (9) present estimates for differences in the challenger vote share. All shares of votes are calculated over total registered voters. We only include observations for which we have data for all the outcomes.

Columns (1), (4) and (7) have as the main regressor a dummy variable equal to 1 if the polling station was assigned to any treatment. Columns (2), (5) and (8) have as the main regressors one dummy varia le 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. Finally, columns (3), (6) and (9) present the results of estimating equation (3), so they include as the main regressors both treatment dummies and their interactions with corrected score and score change. Incumbent's and challenger's shares are calculated by grouping council candidates' electoral pacts according to whether they belonged to Concertación or Alianza, and matching those coalitions to mayoral candidates electoral pacts. All estimates include strata fixed effects, defined by municipality and the station's gender composition. Controls include the gender and age composition of the polling station, and the number of stations in a merged station. The "Control group mean" row provides averages of outcomes in 2016. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

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 worse relative performance decreases 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 bad educational results come as a surprise compared to voters' prior beliefs about educational quality. These results seem stronger in municipalities that are poor and have low levels of electoral spending by the incumbent and have downstream effects to the election of the municipal council. 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 TABLES

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

Notes: This table presents a regression of SIMCE score on socio-economic characteristics at the municipality level for all municipalities with available data. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Placebo exercise: Differences between 2013 and 2012 elections

	Presidential Elections (2013-2012)		
	(1) Difference in Turnout	(2) Difference in Inc. Coalition Vote Share	(3) Difference in Chall. Coalition Vote Share
Average Treatment	0.422	-0.438	0.737**
	(0.458)	(0.500)	(0.359)
Maximum Treatment	-0.137	-0.077	-0.171
	(0.362)	(0.419)	(0.378)
Average Treatment × Corrected Score	0.038	0.064	-0.063
	(0.051)	(0.055)	(0.047)
Average Treatment × Score Change	-0.120	-0.225**	0.107
	(0.133)	(0.109)	(0.088)
Maximum Treatment × Corrected Score	-0.047	-0.043	0.044
	(0.040)	(0.052)	(0.046)
Maximum Treatment × Score Change	-0.060	0.015	-0.105
	(0.113)	(0.096)	(0.070)
Observations	14,672	14,672	14,672
R-squared	0.389	0.715	0.692
Control group mean	40.69	20.79	19.75

Notes: This table presents a placebo exercise for the differences between the 2013 presidential election and the 2012 local election. Column 1 has as outcome variable the difference in turnout between 2013 and 2012 in presidential elections, column 2 the difference in incumbent party vote share between the 2013 and 2012 presidential elections, and column 3 the difference in challenger party vote share between the 2013 and 2012 elections. All estimations include as main regressors both treatment dummies and their interactions with Corrected Score and Score change. We only include observations for which we have data for all the outcomes. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Invalid Votes Share

	Invalid votes	
	(1)	(2)
Average Treatment	0.056	0.043
	(0.083)	(0.083)
Maximum Treatment	-0.038	-0.046
	(0.090)	(0.090)
Average Treatment × Corrected Score	0.005	0.004
	(0.007)	(0.007)
Average Treatment × Score Change	0.015	0.014
	(0.016)	(0.015)
Maximum Treatment × Corrected Score	-0.007	-0.007
	(0.008)	(0.008)
Maximum Treatment × Score Change	-0.029	-0.029
	(0.018)	(0.019)
Controls	No	Yes
Observations	21,018	21,018
R-squared	0.187	0.195
Control group mean	1.17	1.17

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, over total registered voters. Column 1 does not include controls, while column 2 includes the gender and age composition as controls, and the number of stations in a merged station. All estimates include strata fixed effects, defined by municipality and the station's gender composition. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A5: Asymmetric Effects Conditional on Prior Beliefs

	Turnout	Incumbent Vote Share	Challenger Vote Share
	(1)	(2)	(3)
Main Effects			
Average Treatment	3.430***	1.113	1.767***
	(0.981)	(0.691)	(0.561)
Maximum Treatment	-1.626	-1.377	-0.283
	(1.493)	(0.865)	(0.689)
Average Treatment × 1(News ≥ 0)	-3.668***	0.348	-2.009**
	(1.391)	(0.996)	(0.816)
Maximium Treatment × 1(News ≥ 0)	1.431	0.886	0.650
	(1.760)	(1.019)	(0.936)
Good News			
Average Treatment × $News^+$	0.659	-0.615	0.114
	(0.600)	(0.501)	(0.400)
Maximum Treatment × $News^+$	-0.055	0.374	-0.331
	(0.530)	(0.312)	(0.399)
Bad News			
Average Treatment × $News^-$	2.919***	1.276***	1.328**
	(0.716)	(0.482)	(0.517)
Maximum Treatment × $News^-$	-0.903	-1.061	-0.009
	(1.102)	(0.713)	(0.469)
Observations	19,549	19,549	19,549
R-squared	0.449	0.675	0.684
Control group mean	30.38	14.37	9.57

Notes: This table present the coefficients displayed graphically in Figure 6, which come from estimating equation (4). "News" is the difference between the standardized actual educational outcomes reported in the letters and the standardized parents' prior beliefs about at the polling station level. "Good news" corresponds to a positive measure of news, as opposed to "Bad news."

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. All shares of votes are calculated over total registered voters.

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, and the number of stations in a merged station. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A6: Correlation between Municipal Characteristics, Expectations, and News

	Raw SIMCE Score	Corrected SIMCE	SIMCE change	Average polling station Men Share	Average polling station Age	Average polling station Incumbent Support	Parents' expectations	News	Share Public Schools	Average Income	Municipality size	Incumbent Electoral Spending	Incumbent Party is Concertacion
Raw SIMCE Score	1.000												
Corrected SIMCE	0.811***	1.000											
SIMCE change	-0.044	0.140	1.000										
Average polling station Men Share	-0.400***	-0.262*	-0.205	1.000									
Average polling station Age	0.201	0.063	-0.518***	-0.083	1.000								
Average polling station Incumbent Support	0.321**	0.155	0.203	-0.275**	-0.018	1.000							
Parents' expectations	-0.114	0.123	0.301**	0.241*	-0.204	0.293**	1.000						
News	0.834***	0.988***	0.095	-0.301**	0.095	0.112	-0.030	1.000					
Share Public Schools	0.101	0.186	-0.243*	0.443***	0.067	-0.167	0.002	0.187	1.000				
Average Income	0.621***	0.204	-0.040	-0.448***	0.291**	0.240*	-0.364***	0.261*	-0.292**	1.000			
Municipality size	0.081	0.076	0.132	-0.160	-0.412***	-0.266**	-0.252*	0.116	-0.067	-0.069	1.000		
Incumbent Electoral Spending	0.132	0.076	-0.018	-0.354***	0.087	-0.157	-0.278**	0.119	-0.227*	0.153	0.469***	1.000	
Incumbent Party is Concertacion	-0.172	-0.020	-0.255*	0.116	0.317**	0.055	0.106	-0.037	0.044	-0.245*	-0.336**	-0.006	1.000

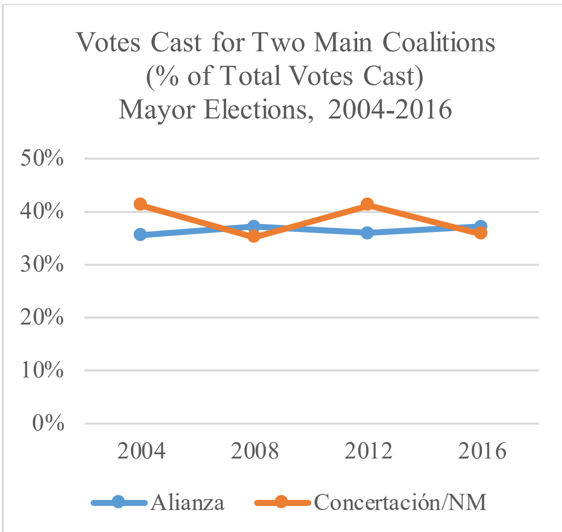
Notes: This table presents the correlations between municipality characteristics. The average variables are non-weighted averages over polling stations within each municipality. N=59. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

APPENDIX FIGURES

Figure A1

Support for the Major Coalitions in Mayoral Elections

Panel A



Panel B

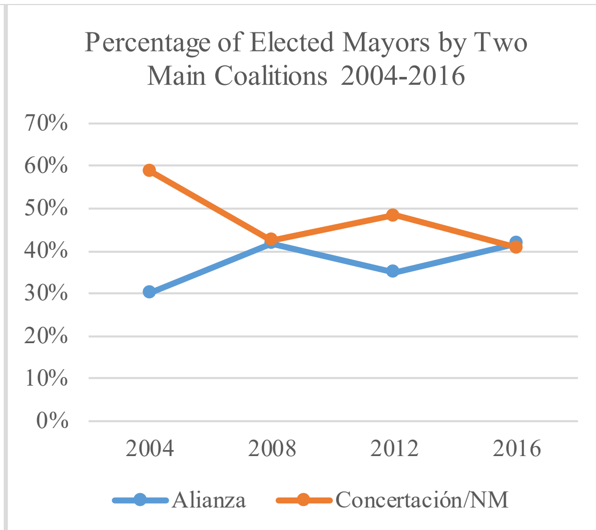
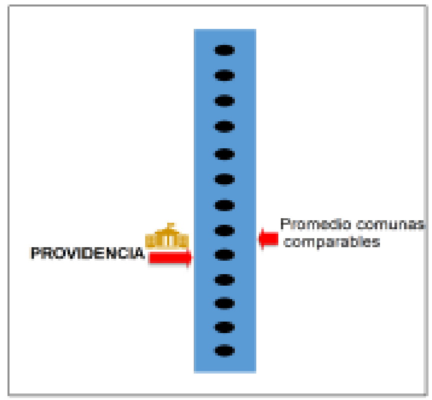


Figure A2

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?

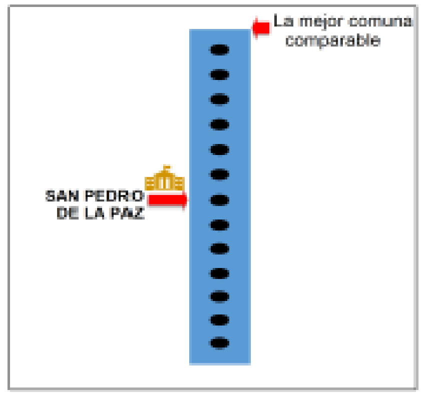
Esta carta informativa es parte de un proyecto de investigación académica coordinado por el profesor Francisco Gallego de la Pontificia Universidad Católica de Chile. Para preguntas o información adicional contactarse con [illegible].

Figure A3

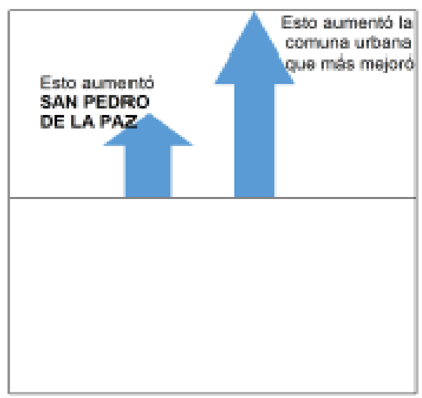
Letter with Maximum Treatment (Example)

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 4º 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?



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].

Figure A4

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

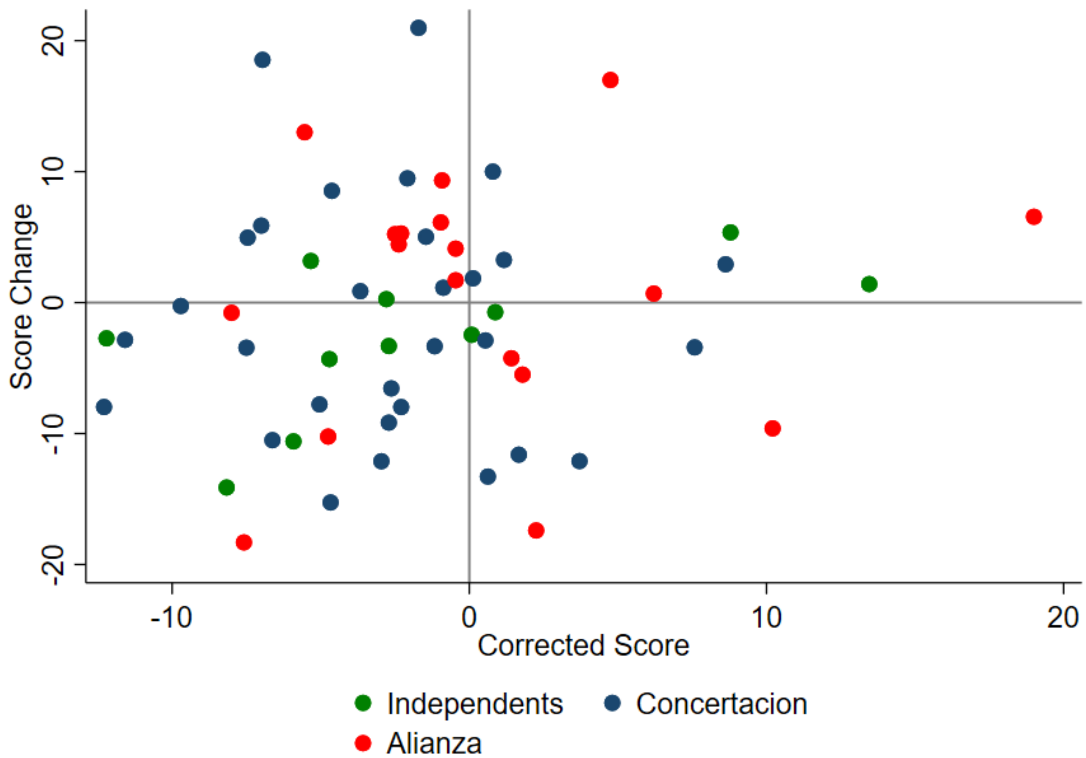


Figure A5

Ballot for the Councilors Elections (Example)

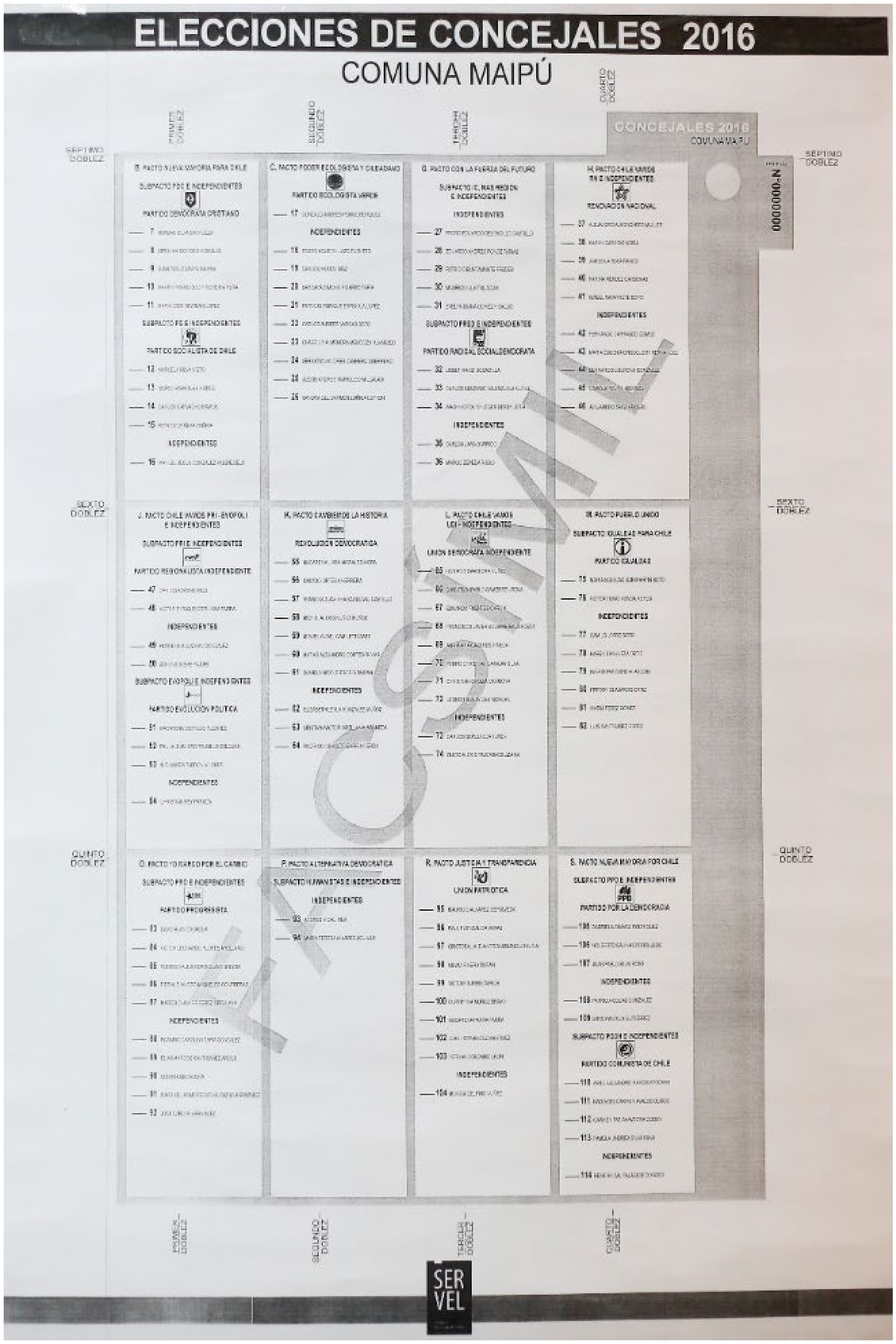


Figure A6

Councilors Campaign Posters (Examples)