Competence versus Priorities: Negative Electoral Responses to Education Quality in Brazil

Appendices

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A Regression discontinuity design

A.1 Media attention to IDEB

In order to get a quantitative measure of media attention, we use the search tool of written media content aggregator Factiva, to search for news stories in Portuguese from within Brazil mentioning IDEB, between the date when the data was published and the day of the elections. For 2016, this search returns 254 pieces of news, more than half of which also included the word "target" or "targets". This can be compared to a baseline of over 52,000 pieces in the Factiva database for that same period, i.e. about 0.49% of the news pieces in that period mention IDEB. For the dates between the publication of IDEB results and municipal elections in 2012, searching for news mentioning IDEB returns 494 pieces of news, 37% of which include the word "target" or "targets". For 2008, the search returns 239 pieces.

In addition to this hit count metric, we also read a systematic sample of 200 articles that mentioned IDEB during the 2008, 2012, and 2016 election period. Of these 200 articles, 118 articles discussed the performance of a specific municipality on the IDEB. Of these 118 articles, 56% discussed meeting or failing to meet the target. Discussing municipal performance specifically in connection to the target became more common in later elections: in 2016, for example, 70% of IDEB-focused articles discussed the target specifically.

This data supports the idea that media coverage of municipal performance discusses both levels of achievement and how it relates to federal targets. While Factiva covers a large number of sources, it does not cover the universe of Brazilian media, and it is thus hard to judge how much coverage these search hit totals represent. To get a benchmark, we look for news pieces about Bolsa Família, the federal government's highly visible and often discussed conditional cash transfer program. For the same period from the publication of IDEB results up to election day in 2016, Factiva returns 215 pieces of news

mentioning Bolsa Família versus 254 mentions of IDEB. Overall, it seems Brazilian written media pay significant attention to IDEB.

With respect to other media sources, the most direct piece of evidence on the salience of IDEB and IDEB targets specifically can be found in Varjão (2019). Varjão (2019) uses automated transcripts from 1/3 of all 2018 local community radio broadcasts in the states of São Paulo and Paraná to examine discussion of IDEB. The day after the release of IDEB, he found that 25% of radio stations discussed municipal performance.[1] Among those discussing municipal performance, 72% specifically discussed performance with respect to the federal targets. These findings from local media also suggest that federal targets are quite salient in media dicussions of IDEB.

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[1]Varjão (2019) notes that this may be a lower bound, due to imperfections in the automated transcriptions and the fact that recordings were unavailable from the day that the IDEB was released.

A.2 Quantitative measures of citizen attention to IDEB

We use Google Trends data on searches made from within Brazil as a proxy for citizen demand for information about IDEB. Figure A.1 below compares the relative frequency of searches for "IDEB" and a number of highly salient policy issues.
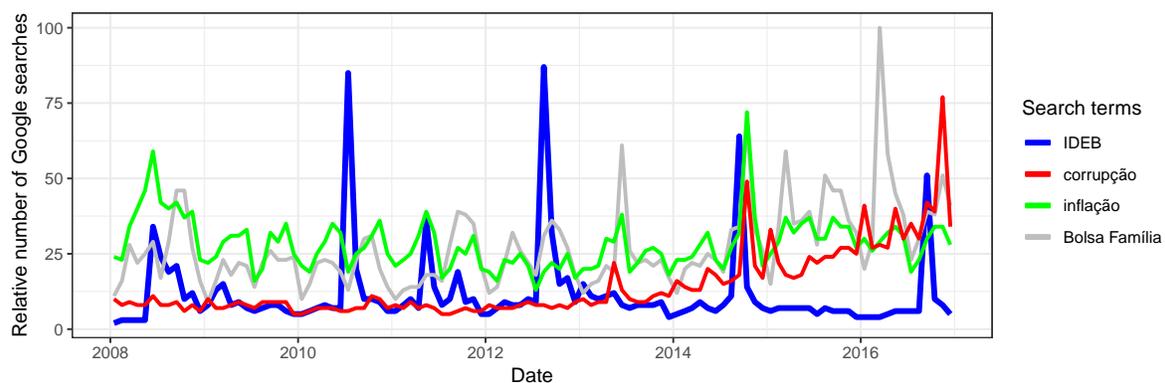
Figure A.1 – Relative frequency of Google searches in Brazil for the terms "IDEB", "corruption", "inflation", and "Bolsa Familia" from 2008 to 2016, by month. Data are from Google Trends.

A.3   Information release and election schedule
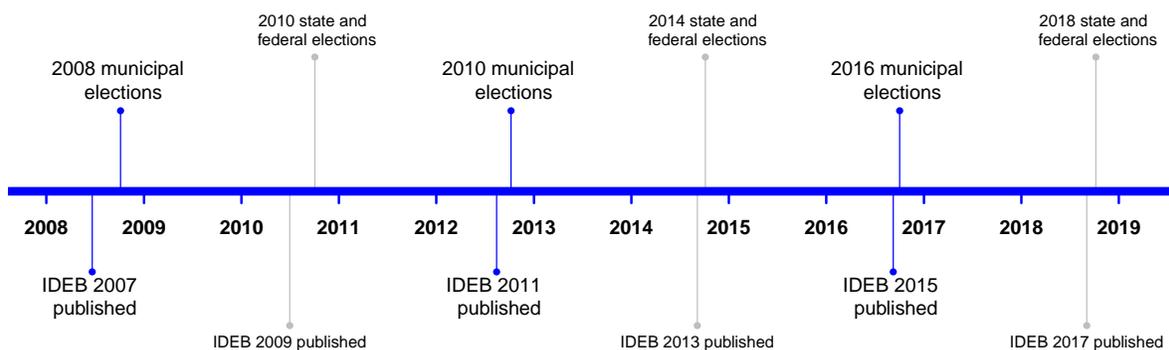
Figure A.2 – Timeline of the release of IDEB results and elections. See Table A.1 below for dates of each event.

ANRESC implemented	IDEB results published	First round of elections held	Time from results to elections
November 5-20, 2007	June 21, 2008	October 5, 2008	<18 weeks
October 19-30, 2009	July 1, 2010	October 3, 2010	<14 weeks
November 7-18, 2011	August 14, 2012	October 7, 2012	<8 weeks
November 11-21, 2013	September 5, 2014	October 5, 2014	<5 weeks
November 3-13, 2015	September 8, 2016	October 2, 2016	< 4 weeks
October 23-November 3, 2017	September 3, 2018	October 7, 2018	< 5 weeks

Table A.1 – Key dates in the IDEB and the electoral calendar. Municipal elections held in 2008, 2012 and 2016. State and federal elections held in years 2010, 2014 and 2018.

A.4 Continuity of the forcing variable

2007, 2011 and 2015

2007

2011

2015
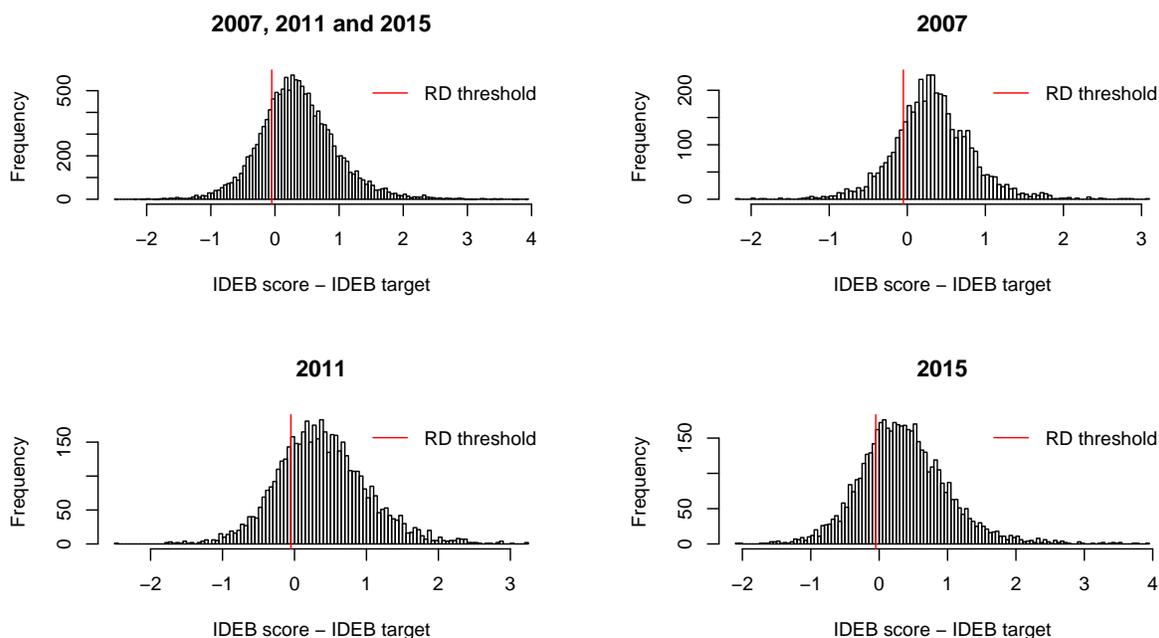
Figure A.3 – Histograms of the forcing variable, by test year.

While "a running variable with a continuous density is neither necessary nor sufficient for identification" (McCrary 2008, 701), it is important to consider possible ways teachers, directors and politicians could be manipulating the forcing variable. IDEB targets are impossible to manipulate. They were defined a priori following technical criteria and published at the beginning of the period. IDEB scores are themselves composed of two parts: passing rates and learning outcomes. Passing rates are the most obvious lever that school and municipality leaders could manipulate. However, boosting passing rates is likely to lead to a decrease in test scores (since students who would otherwise not pass generally get lower scores): the system is in fact designed to disincentivize this type of manipulation. Last, learning outcomes are under *limited* control of school administrators and teachers. IDEB is precisely targeted at measuring their capacity of "manipulating" this variable, i.e. boosting learning. But boosting learning is difficult, and even units that manage to achieve significant gains in learning may miss their target, particularly if they had been lagging behind. The key fact here is that while teachers, directors and
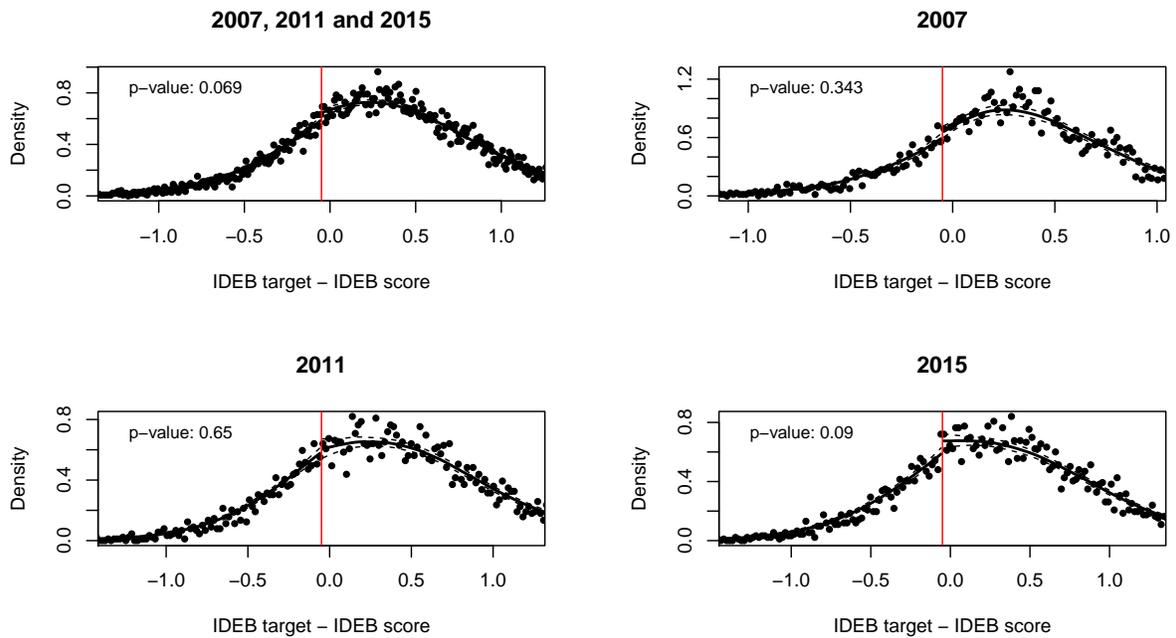
Figure A.4 – Density plots and results of the McCrary ([2008](#)) test for the forcing variable (IDEB score - IDEB target), by test year.

politicians may have some influence over the forcing variable, they cannot manipulate it *precisely*, which guarantees that, for municipalities around the threshold, treatment assignment is as-if-random (Lee and Lemieux [2010](#)).

A.5 Continuity of pre-treatment covariates

Covariate	RD estimate	Std. error	p value
Mayor vote share in previous election	0.009	0.010	0.366
Incumbent mayor ran	-0.032	0.039	0.411
Electoral concentration	-0.022	0.015	0.135
Incumbent belongs to PT	-0.018	0.025	0.452
Incumbent belongs to PSDB	0.019	0.029	0.519
Incumbent belongs to PMDB	-0.035	0.037	0.345
Incumbent party ran	0.002	0.039	0.961
Population (logged)	0.058	0.080	0.471
Share of population in poverty	-1.378	1.489	0.355
Share of municipal employees tenured	0.019	0.024	0.439
Local media stations	0.121	0.082	0.143

Table A.2 – Continuity of pre-treatment covariates.

*p<0.1; **p<0.05; ***p<0.01.

Note. The difference between municipalities where the IDEB target is met and those where it is missed is calculated using each covariate as the dependent variable in Equation 3, within the bandwidth specified by the Calonico, Cattaneo, and Titiunik (2014) algorithm. Standard errors are consistent for heteroskedasticity (HC1).

A.6 Data constraints and characterization of the RDD effective sample

While we start from a potential universe of 16,700 municipality-period observations (5,565 in 2008 and 2012, and 5,570 in 2016[2]), the RD design imposes a number of constraints in the data, detailed in Table A.3 below. With these constraints, the data effectively used for the RD models consist of 4,179 municipality-period observations where incumbent re-election is the dependent variable, and 2,902 where incumbent vote share is the dependent variable.[3] Most of the municipality-period observations that do not enter the RD data do so because they lack data on the dependent variable (since not all mayors are allowed to run for re-election) or data on the forcing variable (since not all municipalities receive both an IDEB score and an IDEB target).

Our results results are therefore valid for the subset of municipality-election observations where (i) there is a public IDEB score and target, (ii) the incumbent mayor is not barred from running (and, for the regressions of vote share, they actually run); (iii) the regular elections are valid and thus no supplementary elections are held; (iv) only one signal of municipal school quality is published; and (v) the IDEB score performance is sufficiently close to the target. While this set does not represent the whole population of municipalities in Brazil, it is the set for which it is meaningful to think of the causal impact of meeting the IDEB target on the electoral performance of the incumbent.

These constraints leave us with a subset of smaller and less competitive municipalities. Table A.4 characterizes the RDD effective sample in terms of the number of residents (logged), the percent of them who are poor (as defined by the federal statistics index), local political competitiveness (measured with a Herfindahl index of electoral concentration where numbers closer to 0 denote a more fragmented election and those closer to 1 a less

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[2]5 municipalities were created in 2013.

[3]The effective samples in Tables 1 and A5 are smaller because they exclude municipalities outside the RD bandwidth (i.e., where the performance of the municipality's school system was not sufficiently close to its target).

Constraint imposed	Resulting N	Notes
None	16,700	All municipal-year observations in 2008, 2012, 2016
IDEB score is published	↪ 15,110	IDEB scores required for measuring the forcing variable. The most common reason for municipalities not having an IDEB score is that schools must have at least 20 students enrolled in the corresponding grade for their ANRESC scores to be calculated.
IDEB target is published	↪ 14,240	IDEB targets required for measuring forcing variable The most common reason for municipalities not having an IDEB target is not having baseline IDEB scores with which the Ministry calculates targets.
Mayor can run	↪ 9,879	Mayor eligibility required for measuring dependent variable. Mayors who are in their second consecutive term are barred from running.
Mayor has unique ID	↪ 9,860	Mayor ID required for measuring dependent variable. The Supreme Electoral Court data does not report mayor ID in a few cases, likely due to data entry errors.
No supplementary election held	↪ 9,687	Supplementary elections are held months or years after regular elections, and thus far away from IDEB data being released. The Supreme Electoral Court does not report data from the original elections, in places where it reports results for a supplementary election.
Only one IDEB signal	↪ 4,179	A municipality receiving two signals (one for primary and one for for middle schools) is likely to confuse their interpretation by voters and media. Primary schools are consistently associated to municipal governments, whereas municipal middle schools are generally associated to state governments. Since some but not all municipalities receive two signals, it is not possible to do a two-dimensional RDD that could identify their effect.
Mayor ran	↪ 2,902	Mayor running required for measuring vote share

Table A.3 – Data constraints imposed on the RDD sample

competitive election), and the share of municipal workers who are tenured. Compared to all municipality-period observations, those that enter the RDD belong to less populous municipalities, with slightly lower incidence of poverty, and less electoral fragmentation.

	In RD sample	In bandwidth	In RD sample and bw
Population (logged)	−0.048***	0.037***	−0.013***
	(0.003)	(0.002)	(0.008)
Percent residents poor	−0.005***	0.000***	−0.002
	(0.000)	(0.000)	(0.001)
Electoral concentration	0.103***	−0.032*	0.036
	(0.025)	(0.018)	(0.052)
Share workers tenured	0.016	−0.027	−0.002
	(0.013)	(0.010)	(0.029)
Constant	0.774***	0.172***	0.280*
	(0.035)	(0.026)	(0.089)
Election fixed effects	Yes	Yes	Yes
Observations	16458	4161	16562
R-squared	0.059	0.011	0.019

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table A.4 – Characterization of the RDD effective sample. Dependent variable is a dummy for observations that enter the RD sample (versus all observations), the 0.39 bandwidth (versus those in the RD sample), and the RD sample and the 0.39 bandwidth (versus all observations). HC2 heteroskedasticity consistentstandard errors in parentheses.

A.7 Alternative outcome: Incumbent vote-share

	Linear	Linear	Robust	Robust
IDEB target met	-0.037*	-0.041*	-0.05*	-0.059**
	(0.022)	(0.022)	(0.028)	(0.03)
Election cycle fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	0.352	0.323	0.352	0.323
Observations	1113	1006	1113	1006

Table A.5 – Effect of Meeting the IDEB Target on Re-election of the Mayor

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1 in models 1-2, and nearest-neighbor in models 3-4.)
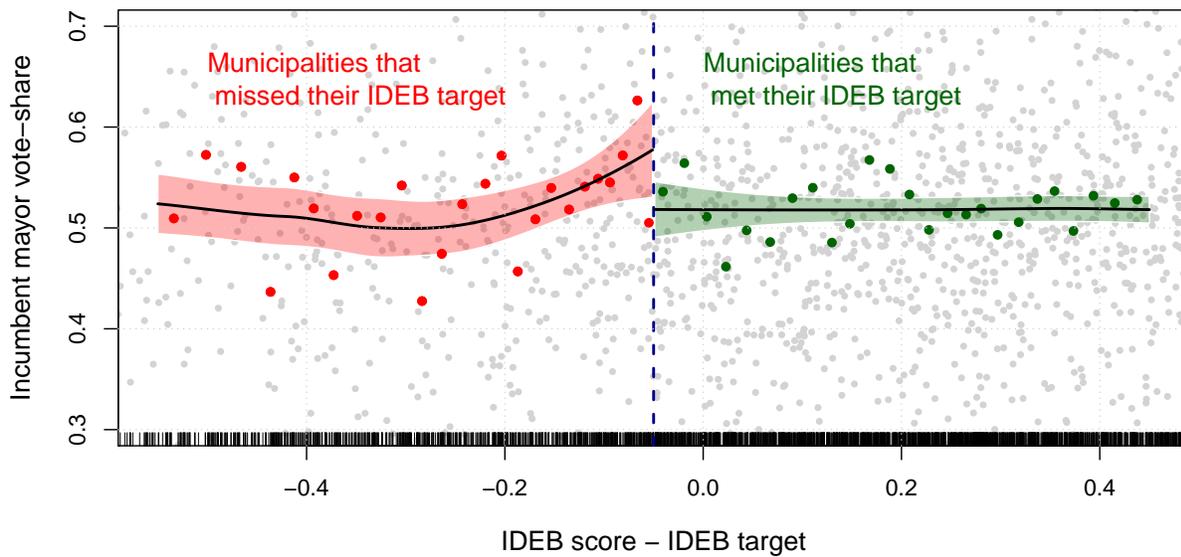
Figure A.5 – Relationship between meeting the IDEB target and vote share of the mayor. Grey dots are observations. Colored dots represent local averages for 50 equally-sized bins. Lines are loess regression lines estimated at both sides of the threshold with no controls. Shaded regions are their 95% confidence intervals.

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RD results of the effect of meeting the IDEB on the vote-share of the incumbent are of similar standardized magnitude than those for re-election repoted in Table 1, but noisier. This is most likely due to the decrease in power stemming from a lower number of observations (municipalities where the mayor was allowed to run but chose not to are dropped from this sample but not that of Table 1). One may however be concerned that part of the difference may be driven by municipalities' IDEB performance driving incumbents' decisions to re-run. This is unlikely given the calendar for registering electoral candidates in Brazil: only in 2008 was this deadline *after* the release of IDEB scores.[4] Still, we test for this possibility by running the RD regression with whether the incumbent ran as a dependent variable. Table A.6 shows statistically insignificant results.

	Linear	Linear	Robust	Robust
IDEB target met	-0.032	-0.032	-0.036	-0.041
	0.039	0.034	0.05	0.051
Election cycle fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	0.481	0.457	0.481	0.457
Observations	2120	2034	2120	2034

Table A.6 – Effect of Meeting the IDEB Target on Whether the Mayor Runs for Re-Election

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1 in models 1-2, and nearest-neighbor in models 3-4.)

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[4]Candidates had to register their candidacies by July 7 in 2008, July 5 in 2012, and August 15 in 2016.

A.8 Alternative bandwidths
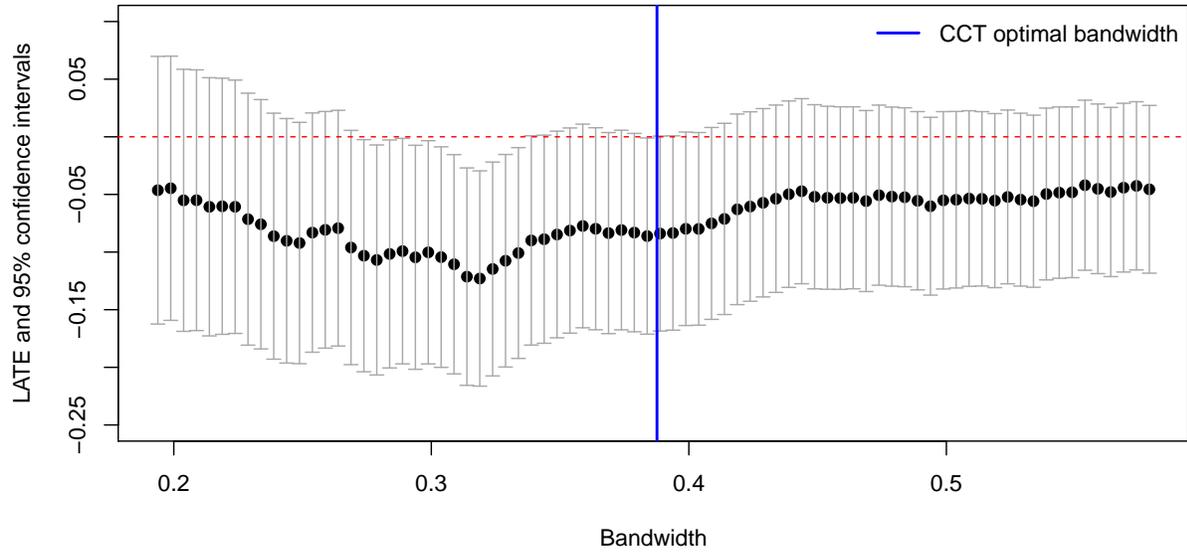
(a) Mayor re-election
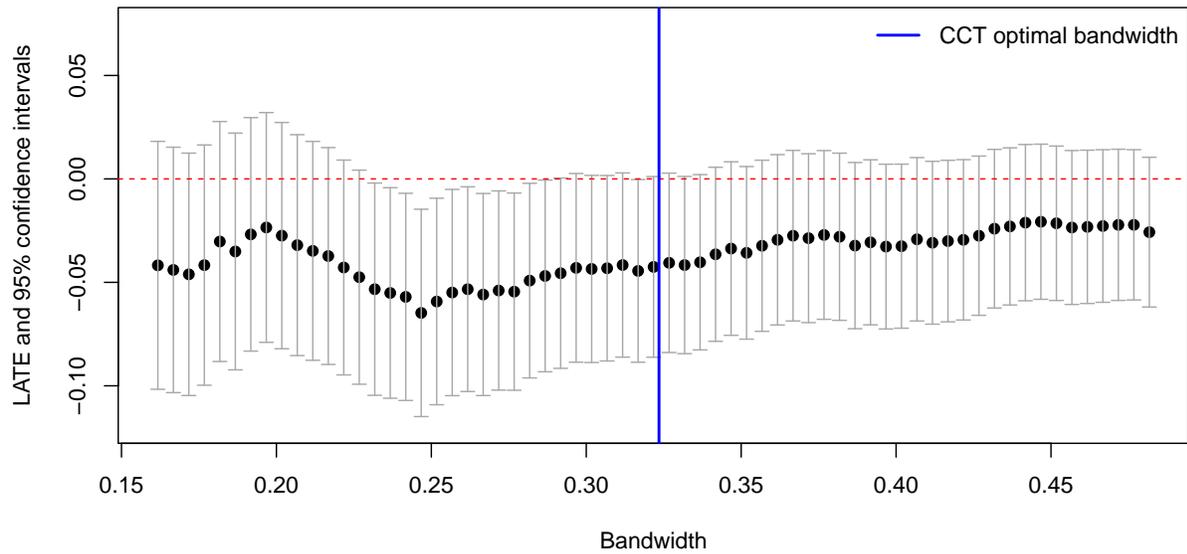
(b) Mayor vote-share

Figure A.6 – Robustness of the treatment effect shown in model 2 in Tables 1 and A5 (local linear with controls) to alternative bandwidths.

A.9   Alternative discontinuity thresholds as placebo tests
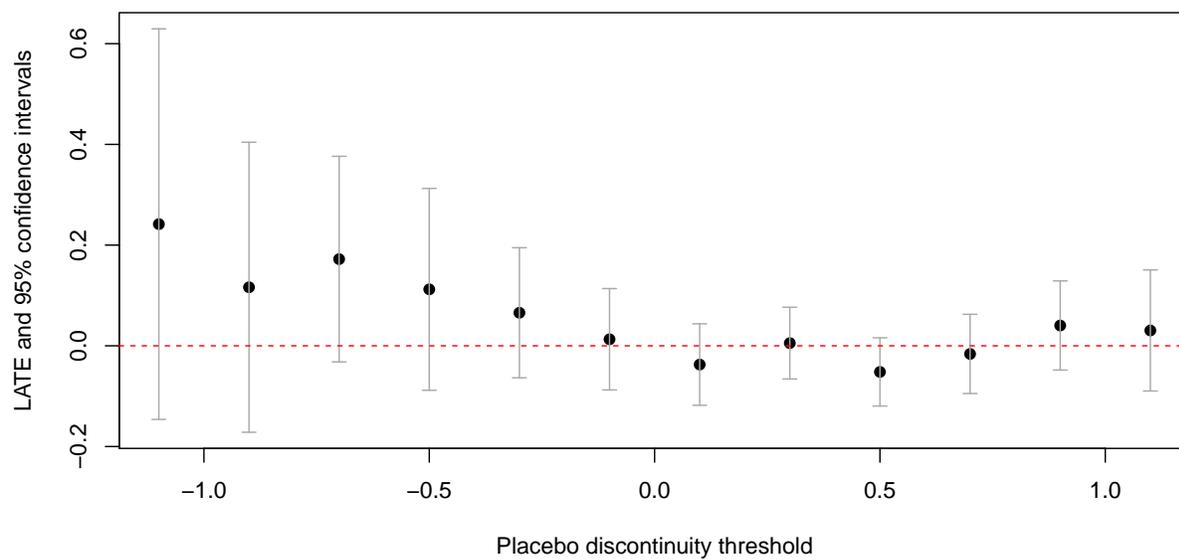
Figure A.7 – Placebo tests using alternative discontinuity thresholds for Model 2 in Table 1. For instance, placebo test using 0.5 as the discontinuity threshold replicates the analyses as if only municipalities with an IDEB score 0.5 points above the target had met their expected level of performance.

A.10   Alternative sample: no restrictions

As described in Section 4.1.1 and Appendix A6, our main results impose a number of constraints on the sample (observations where mayors are allowed to run, no supplementary elections are held, and municipalities receive only one IDEB signal). Here we present results lifting those constraints. Effects are smaller and less precisely estimated, but remain negative in all specifications.

	Linear	Linear	Robust	Robust
IDEB target met	-0.023	-0.029	-0.021	-0.022
	(0.022)	(0.021)	(0.028)	(0.029)
Election cycle fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	0.394	0.385	0.394	0.385
Observations	6296	6168	6296	6168

Table A.7 – Effect of Meeting the IDEB Target on Re-election of the Mayor – No constraints

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1 in models 1-2, and nearest-neighbor in models 3-4.)

	Linear	Linear	Robust	Robust
IDEB target met	-0.023*	-0.022*	-0.024	-0.026
	(0.012)	(0.012)	(0.016)	(0.016)
Election cycle fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Bandwidth	0.426	0.404	0.426	0.404
Observations	3270	3122	3270	3122

Table A.8 – Effect of Meeting the IDEB Target on Vote Share of the Incumbent – No constraints

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1 in models 1-2, and nearest-neighbor in models 3-4.)

A.11 Heterogeneity by municipal school enrolments

To test whether municipalities with more parents of children enrolled in municipal schools tend to have a more positive response to school quality signals, we examine heterogeneous local average treatment effects or HLATEs in the RD setup by whether the number of kids enrolled in municipal schools, as a share of the overall population, is high (i.e. in the upper quartile). To do so, we expand Equation 3 by including an indicator for high enrolments (H_{mj}) and its interactions with treatment, with the forcing variable, and with treatment and the forcing variable, so as to allow for separate slopes. We include the same covariates here as in the main specification (X).

$$Y_{mj} = \alpha + \beta_1 T_{mj} + \beta_2 \tilde{D}_{mj} + \beta_3 T_{mj} \tilde{D}_{mj} + H_{mj}(\lambda_1 + \lambda_2 T_{mj} + \lambda_3 T_{mj} \tilde{D}_{mj} + \lambda_4 \tilde{D}_{mj}) +$$
$$+ \sum_{g=2}^{3} \gamma_g I[g = j] + \sum_{k=1}^{K} \theta^k X_{mj}^k + \varepsilon_{mj} \tag{1}$$

Results, shown in Tables A.9 and A.10, suggest that in places where there is a larger number of voters with children enrolled in municipal schools the effect of meeting the IDEB target on the electoral performance of the mayor gets reversed, although this difference is not statistically significant.

	Linear	Linear
IDEB target met	-0.051	-0.093**
	(0.043)	(0.046)
IDEB target met x High enrolments	0.045	0.118
	(0.135)	(0.151)
Election cycle fixed effects	Yes	Yes
Controls	No	Yes
Bandwidth	0.545	0.388
Observations	2340	1772

Table A.9 – Effect of Meeting the IDEB Target on Re-election of the Mayor – Heterogeneity by School Enrolments

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1). High enrolments is an indicator for whether the number of children enrolled in municipal schools as a share of the overall population is in the upper quartile.

	Linear	Linear
IDEB target met	-0.025	-0.037*
	(0.019)	(0.022)
IDEB target met x High enrolments	0.001	0.048
	(0.058)	(0.071)
Election cycle fixed effects	Yes	Yes
Controls	No	Yes
Bandwidth	0.355	0.324
Observations	1629	1217

Table A.10 – Effect of Meeting the IDEB Target on Vote Share of the Mayor – Heterogeneity by School Enrolments

*p<0.1; **p<0.05; ***p<0.01.
Note. The bandwidth is the one determined by the algorithm of Calonico, Cattaneo, and Titiunik (2014). Standard errors in parentheses are consistent for heteroskedasticity (HC1). High enrolments is an indicator for whether the number of children enrolled in municipal schools as a share of the overall population is in the upper quartile.

B   Randomized control trial

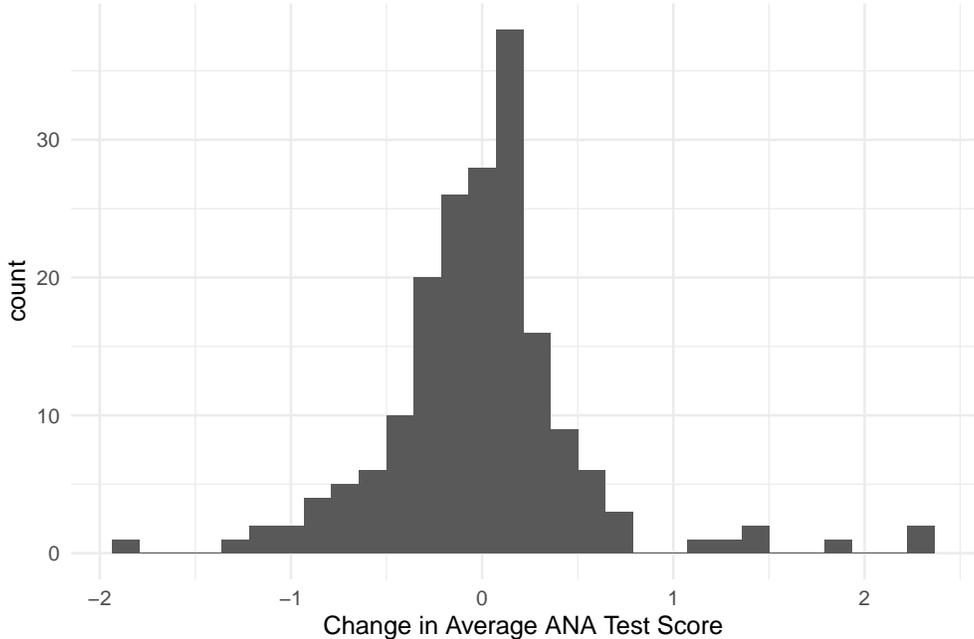## B.1   Distribution of ANA-based school quality scores

Figure B.8 – ANA distribution

B.2 Flyers Used in Experiment

Examples of the flyers used to deliver treatment information are contained in Figure B.9a (front side) and Figure B.9b (back side).

(a) Front of the flyer

(b) Back of the flyer
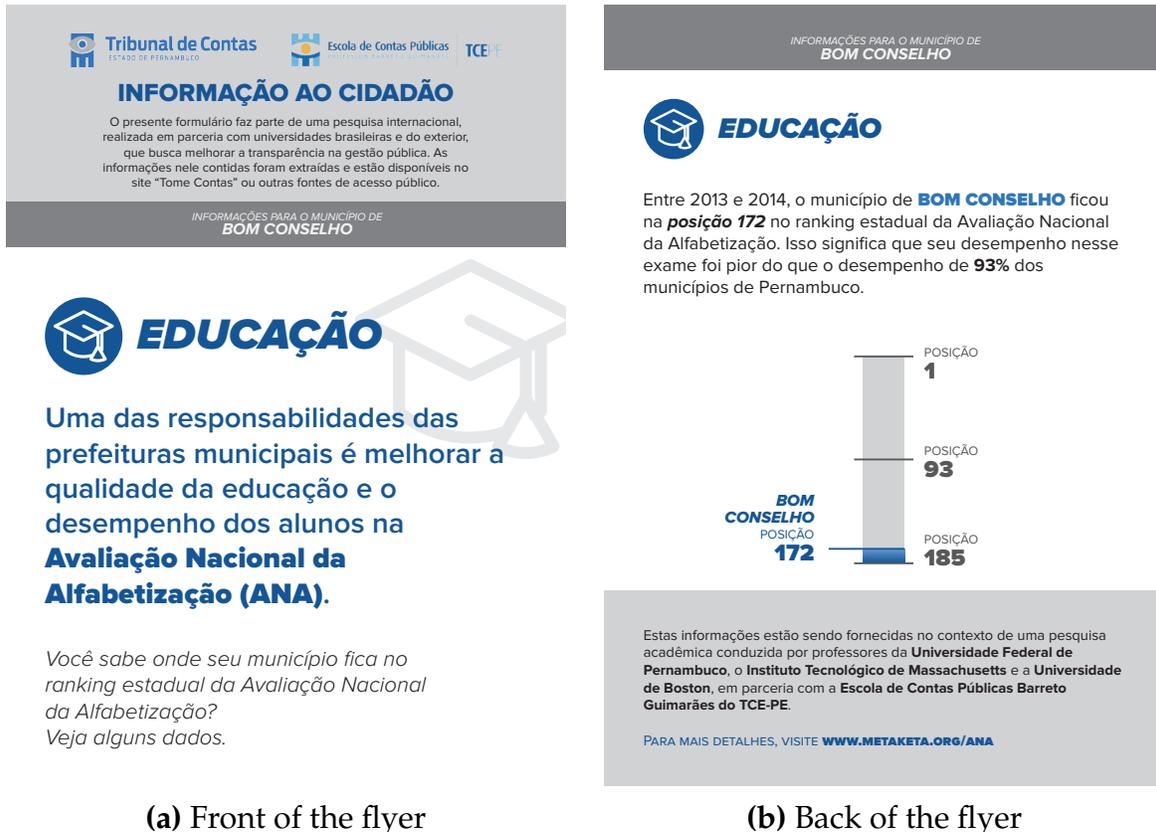
Figure B.9 – Example of flyers Distributed to Voters

B.3 Legality of the Intervention

Some scholars have questioned the legality of conducting electoral field experiments in Brazil that do not involve partnerships with parties or candidates, given strict regulations governing campaign advertising (Cunow and Desposato 2015; Desposato 2015). Doubts about the legality of interventions involving flyers spring from Article 38 of Law 9504 of September 1997, which governs elections. With respect to "the dissemination of electoral advertising via the distribution of leaflets, stickers, flyers, and other printed material," it holds that such items "shall be published under the responsibility of the party, coalition, or candidate." However, this law does not precisely define what counts as "electoral advertising." Those sections that come closest to a definition suggest that, to be considered electoral advertising, a message must explicitly ask for votes. Article 26, paragraph 2 states that: "The following are considered electoral expenditures, subject to registry and to the limits set by this Law ... direct and indirect advertising and publicity, via any medium of dissemination, intended to win votes." Likewise, Article 36-A, which governs campaigning prior to the official start date, holds that references to potential candidates "do not count as early electoral advertising as long as they do not explicitly ask for votes." Hence, a full reading of the law suggests that flyers that do not mention voting are not subject to the limits of Article 38 because they do not count as electoral advertising.

To check our interpretation of Law 9504, we submitted a request for clarification to Brazil's Superior Electoral Court (TSE). Their response quoted Articles 26 and 38, as cited above, indicating that these were the relevant portions of the law bearing on the question of the legality of flyers. However, they told us that they could not provide any analysis or interpretation of the law, and that for that purpose we should contact a specialist attorney.

A condition of our partnership with the State Accounts Court of Pernambuco was that they would have the opportunity to review and approve all study materials before they went to the field. We submitted drafts of the flyers, which were reviewed by a TCE-PE Councilor who is a former judge of the Regional Electoral Court of Pernambuco (TRE-

PE), as well as a Substitute Councilor who is a law professor. They requested several changes to the draft version which we implemented prior to conducting the study.

Based on our inquiries with relevant legal authorities and specialists, we concluded that our intervention did not violate Brazilian law.

B.4 Example Ballot

An example of the secret ballot used to measure vote field experiment is contained in Figure B.10.

PARA PREFEITO DE ABREU E LIMA

	NOME	NÚMERO	PARTIDO	
	KATIANA GADELHA	12	PDT	
	FLAVIO GADELHA	15	PMDB	
	PR. MARCOS JOSÉ	40	PSB	
	BRANCO / NULO			

26310   1

Figure B.10 – Secret Ballot for Measuring Vote Choice

B.5   Covariates

As explained in the main text, we use a lasso procedure to select covariates to include in our regression adjustment. The pre-specified covariates are listed in Table B.11. Most variables are originally likert-scale variables and we convert them to numeric interval variables. For categorical variables with many categories (`race`, `religion`, `muni_biggest_problem`), we collapse all categories with less than 5% of respondents into an "other" category and expand the variable to a set of dichotomous variables representing each possible value.

Variable	Definition
female	Female
Age	Age
politics_interest	Interest in politics
turnout_2012	Turnout in 2012 local elections
vote_2012	Voted for incumbent in 2012
turnout_2014	Turnout in 2014 national elections
vote_2014	Presidential candidate voter voted for in 2014
partisan	Identifies with a party
muni_biggest_prob	Most important problem in the municipality
politician_helped	Had received help from a politician
govt_eval_baseline	Evaluation of the municipal government at baseline
acc_eval_baseline	Evaluation of mayor's handling of the municipal accounts at baseline
uncertain_acc_baseline	Uncertainy over evaluation of handling of municipal accounts at baseline
edu_eval_baseline	Evaluation of mayor's handling of education
uncertain_edu_baseline	Uncertainty over evaluation of handling of education at baseline
tce_knowledge	Heard about the State Accounts Tribunal
ana_knowledge	Heard about ANA
child_school	Has a child in municipal school
confid_fedgov	Confidence in the federal government
confid_justice	Confidence in the Judiciary
confid_tce	Confidence in the State Accounts Tribunal
confid_muni	Confidence in the municipal government
acc_responsible	Degree to which mayor responsible for accounts status
edu_responsible	Degree to which mayor responsible for education
prob_vote_buying	Probability incumbent will attempt to buy vote
prob_vote_monitoring	Probability vote is not secret
prob_vote_count	Probability vote count is correct
acc_rejected_prior	Prior over whether municipal accounts are rejected
tce_prior_cert	Uncertainty about accounts status prior
ana_prior	Prior over ANA performance
edu_prior_uncert	Uncertainty over ANA prior
years_edu	Years of education
race	Race
religion	Religion
income	Income
relative_wellbeing	Perceived relative wellbeing
edu_rank1	Ranks handling of education as more important than handling of accounts.
acc_rank1	Ranks handling of account as more important than education.

Table B.11 – Covariates for the Pernambuco Field Experiment.

B.6 Experimental Results without Covariate Adjustment

	All	Parents	Not Parents	All
Treatment	−0.0317	−0.0489	−0.0145	−0.0128
	(0.0229)	(0.0529)	(0.0286)	(0.0279)
Treatment x Rank	0.0003	−0.0013	0.0008*	0.0009*
	(0.0004)	(0.0009)	(0.0005)	(0.0005)
Treatment x Rank x Parents				−0.0020**
				(0.0009)
Num. obs.	1709	525	1184	1709

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table B.12 – Experimental results without covariate adjustment. Experimental block coefficients are omitted. HC2 heteroskedasticity consistent standard errors in parentheses.

Results from the experiment without any covariate adjustment (aside from adjustment for experimental blocks) are presented in Table B.12.

B.7   Robustness to Other Interactions

	All	Parents	Not Parents
Treatment	-0.0534^{***}	-0.0708^{**}	-0.0422^{*}
	(0.0196)	(0.0355)	(0.0238)
Treatment x Rank	0.0003	-0.0007	0.0007^{*}
	(0.0003)	(0.0006)	(0.0004)
Num. obs.	1709	525	1184

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.13 – Experimental results with additional interactions. These estimates are from the regression models reported in Table 2 augmented with the interaction between the treatment and age, income, and years of education. HC2 heteroskedasticity consistent standard errors in parentheses.

In the discussion of the contrasting results of the experimental intervention on parents of children enrolled in municipal schools and non-parents, we note that parents tend to be younger, poorer, and less educated than non-parents. Given these differences, we control for these covariates by including them as additional interactions in the first three specifications found in Table 2. The findings that parents punish bad performers and that non-parents tend to punish good performers are robust to including these covariates as additional interactions, as shown in Table B.13. The point estimates in Table B.13 reported on the interaction between the intervention and performance on the ANA are almost identical to those reported in Table 2.

Similar to the concern about the correlates of having children in municipal school, another potential issue is that the the ranking of changes in ANA performance is correlated with other municipal attributes at may driving the observed heterogeneity in treatment effects. Specifically, one concern is that municipalities that performed well on the ANA in 2012 had only limited room for improvement in 2014. If this were the case, then high performing municipalities would be lower ranked on our ranking (which measures change) on average. To assess whether this may be driving the results, we control for the interaction of treatment and performance on the ANA exam in 2012. Results in Table B.14 show

	All	Parents	Not Parents
Treatment	−0.0536***	−0.0680*	−0.0429*
	(0.0196)	(0.0355)	(0.0239)
Treatment x Rank	0.0002	−0.0012	0.0008
	(0.0004)	(0.0007)	(0.0005)
Num. obs.	1709	525	1184

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table B.14 – Robustness of Experimental Results to Including Interaction between Treatment and Baseline ANA Performance. Covariates omitted. Covariates demeaned so treatment coefficient is estimated average treatment effect. HC2 heteroskedasticity consistent standard errors in parentheses.

that inclusion of this interaction has no appreciable effect on our results.

B.8   Heterogeneity by Priors

The result in Figure 2 might be explained by a correlation between rank and voter expectations. If voters in higher performing municipalities expected even better performance than the actual rank, while those in lower performing municipalities had properly calibrated expectations, then we would expect a negative interaction like the one we found. To test for this possibility, we estimate heterogeneity by the *gap* between voters' prior belief over their municipality's rank (measured at baseline) and the actual rank.

Results for treatment heterogeneity by the gap between voters expectations and reality can be found in Figure B.11. As is clearly evident from the plotted regression line, there is no substantive interaction between this gap and the effect of the treatment. The overall effect of treatment is negative, but this negative effect does not moderate among
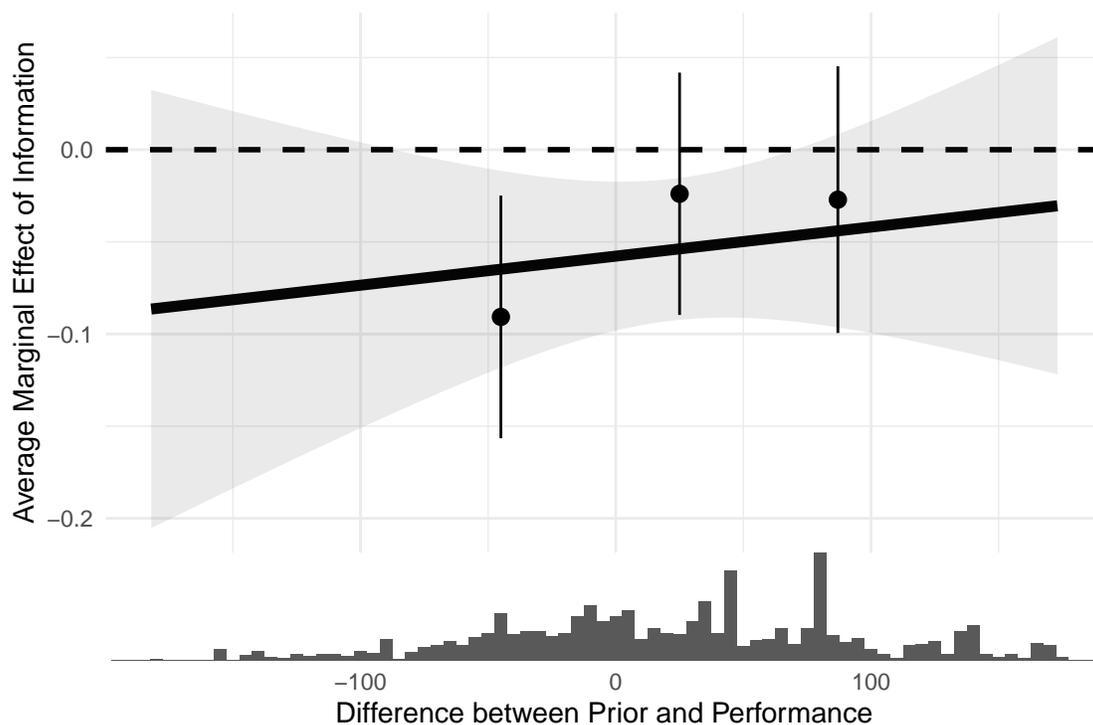
Figure B.11 – Effect of Treatment by Gap Between Voters' Prior and Municipal Performance. Negative values indicate worse performance than expected, while positive values indicate better performance than expected. 90% Confidence Intervals are shown. Histogram shows marginal distribution of the data by the gap.

voters who are positively surprised by the information. The tercile approach largely corroborates the linear interaction model, though there is some weak indication of a non-linearity with voters in the middle of the distribution punishing the incumbent slightly more than incumbents at the tails of the distribution.

B.9 Experiment Covariate Balance

Covariate balance is presented in Table B.15. All variables in Table B.11 are included, except for the non-ordinal categorical variables (race, vote_2014, religion, muni_biggest_prob). p-values calculated using permutation inference with 2000 permuted treatment realizations.

Variable	Mean Difference	SD	SE	Permutation p-value
partisan	0.0611	0.492	0.0214	0.0105
turnout_2012	-0.0432	0.352	0.0167	0.014
acc_rank1	-0.0331	0.438	0.0186	0.091
acc_eval_baseline	0.0911	1.31	0.0536	0.106
politician_helped	0.0266	0.402	0.0184	0.187
tce_knowledge	-0.0255	0.496	0.0216	0.27
politics_interest	0.0477	0.962	0.042	0.269
confid_muni	0.113	2.3	0.0965	0.267
acc_rejected_prior	-0.023	0.482	0.0174	0.194
turnout_2014	-0.0142	0.302	0.0141	0.332
confid_justice	0.0983	2.09	0.09	0.314
govt_eval_baseline	-0.0629	1.38	0.0573	0.278
vote_2012	0.0194	0.479	0.0201	0.349
edu_eval_baseline	0.049	1.26	0.0516	0.378
edu_rank1	-0.0164	0.437	0.019	0.398
prob_vote_monitoring	-0.0427	1.15	0.0478	0.378
uncertain_acc_baseline	-0.0418	1.29	0.0506	0.419
child_school	-0.013	0.461	0.0209	0.554
tce_prior_cert	-0.0169	0.602	0.0249	0.524
edu_prior_uncert	0.0147	0.568	0.0222	0.531
confid_fedgov	0.0522	2.12	0.0884	0.58
income	0.0355	1.46	0.0616	0.6
edu_responsible	-0.0387	1.64	0.0534	0.48
age	0.361	16.3	0.79	0.679
ana_knowledge	0.00753	0.427	0.0189	0.713
confid_tce	0.0318	1.99	0.0815	0.696
ana_prior	-0.854	53.6	2.08	0.685
years_edu	-0.0651	4.82	0.215	0.753
relative_wellbeing	0.0073	0.596	0.0268	0.799
female	-0.00524	0.5	0.0242	0.851
acc_responsible	0.0122	1.59	0.0522	0.811
uncertain_edu_baseline	-0.00465	1.35	0.0538	0.94
prob_vote_count	-0.00268	1.12	0.0388	0.945
prob_vote_count	-0.00268	1.12	0.0388	0.949

Table B.15 – Covariate Balance for the Pernambuco Experiment. The mean difference column reports estimated ATE for pre-treatment covariates specified in PAP. SD is standard deviation of variable. Permutation p-value is computed using 2000 permutations of the treatment variable. Most imbalanced variables are at the top of the table, as variables are ordered by the absolute value of the mean difference divided by the standard deviation.

B.10   Attrition

	(1)	(2)
Treatment	-0.003	-0.004
	(0.016)	(0.016)
Treatment x rank		0.000
		(0.000)
Treatment x vote_2012		-0.029
		(0.036)
Treatment x govt_eval_baseline		0.008
		(0.017)
Treatment x child_school		-0.015
		(0.038)
Treatment x confid_muni		0.009
		(0.010)
N	2047	2047

*** p < 0.01; ** p < 0.05; * p < 0.1.

Table B.16 – Correlation of Treatment with Attrition in the Pernambuco Experiment. These estimates are from a regression of an attrition indicator on treatment, covariates, treatment by covariate interactions, and block fixed effects. All covariates were demeaned. Coefficients on covariate main effects and block strata are omitted.

In the field experiment, 338 respondents were not re-interviewed. Differential attrition by treatment status could lead to biased estimates, so we check correlation of treatment with attrition. In the first column of Table B.16, we show that there is no statistically or substantively significant correlation between treatment and an attrition indicator. The second column shows results from a specification with interactions between the treatment variable and predictive covariates. Details on covariates can be found in Table B.11. We find no statistically significant interactions. Overall, we find no evidence of differential

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attrition.

B.11 Results for Pre-Registered Hypotheses

As mentioned in the main text, our analytical specification for the field experiment departs from that which was pre-registered with Evidence in Governance and Politics (EGAP) prior to analysis. In concert with the broader initiative of which this study was a part, our pre-specified approach conditions on whether information is "good news" or "bad news" (measured dichotomously) relative to a respondent's priors. By contrast, in the main text, we ignore priors and condition only on the municipality's continuous performance ranking.

For the treatment arm involving information about school performance, we pre-specified 23 distinct hypothesis tests. Results from these tests can be found in Table B.17. As shown in line 1 of the table, our main overall finding from the paper is similar when using the pre-specified approach. Information that is "good news" relative to priors reduces the probability of voting for the incumbent by 0.083, significant at the 0.05 level. By comparison, for respondents in the best tercile of ANA performance (ignoring priors), treatment information has an effect of -0.073, similarly significant at the 0.05 level. Hence, regardless of whether we condition on priors, respondents punish good performance.

We refer readers to the pre-analysis plan for complete details, but will describe here the main variables and subsets of the data used in each specification. The subset "Good News" refers to individuals who received information that is more positive than their priors (or equal to priors and better than or equal to the statewide median), while "Bad News" refers to individuals who received information that is worse than their priors (or equal to priors and worse than the statewide median). "All" refers to all individuals regardless of the content of information received.

The outcome variables are as defined as follows:

- `vote`: Equals 1 if the respondent reports voting for the incumbent, 0 otherwise (including abstention and blank/null votes).
- `turnout`: Equals 1 if the respondent reports voting in the 2016 election, 0 otherwise.

Family	Hypothesis	Outcome	Parameter	Subset	Estimate	SE	p-value
1	1a	vote	ana	Good News	-0.083	0.031	0.008
1	1b	vote	ana	Bad News	-0.037	0.026	0.146
2	2	vote	ana x uncertain_edu	Good News	0.001	0.001	0.243
2	2	vote	ana x uncertain_edu	Bad News	0.000	0.001	0.852
2	3	vote	ana x edu_rank1	Good News	-0.065	0.069	0.351
2	3	vote	ana x edu_rank1	Bad News	-0.048	0.060	0.426
2	4	vote	ana x edu_rank1 x uncertain_edu	Good News	-0.047	0.047	0.312
2	4	vote	ana x edu_rank1 x uncertain_edu	Bad News	-0.021	0.048	0.665
2	5a	vote	ana x child_school	Good News	0.009	0.068	0.893
2	5a	vote	ana x child_school	Bad News	-0.063	0.056	0.259
2	5b	vote	ana x child_school x uncertain_edu	Good News	0.051	0.052	0.329
2	5b	vote	ana x child_school x uncertain_edu	Bad News	0.057	NaN	NaN
3	7a	turnout	ana	Good News	-0.496	0.498	0.319
3	7b	turnout	ana	Bad News	0.020	0.013	0.111
4	8a	valid_vote	ana	Bad News	0.019	0.019	0.327
4	8b	valid_vote	ana x govt_eval	Bad News	0.011	0.030	0.713
4	9	edu_eval	ana	Good News	-0.009	0.082	0.908
4	9	edu_eval	ana	Bad News	-0.037	0.062	0.552
4	10	edu_eval	ana x uncertain_edu	Good News	-0.076	0.055	0.162
4	10	edu_eval	ana x uncertain_edu	Bad News	0.016	0.046	0.733
4	11	uncertain_edu_eval	ana	All	-0.019	0.032	0.560
4	12	info_import	ana	All	-0.046	0.028	0.100
4	13	ana_correct	ana	All	4.053	1.948	0.038

Table B.17 – Results for Pre-Registered Hypotheses. See pre-analysis plan for details.

- valid_vote: Using a secret ballot, equals 1 if the respondent reports having voted for any candidate for mayor, and 0 if s/he reports having abstained or cast a blank or null vote.

- edu_eval: The respondent's evaluation of the incumbent's management of schools at endline: (1) "excellent," (2) "good," (3) "regular," (4) "bad," or (5) "horrible."

- uncertain_edu_eval: The respondent's certainty about edu_eval: "very sure," (2) "sure," (3) "unsure," or (4) "very unsure."

- info_import: Equals 1 if the respondent said that "what I learned during the campaign about the mayor" was one of the three most important factors in their decision-making; 0 otherwise.

- ana_correct: The degree to which the respondent's post-treatment belief about the municipality's ANA ranking is accurate: 184 - |ana_posterior - ana_rank|.

 The independent and moderator variables are defined as follows:

- ana: Equals 1 for respondents assigned to receive information on municipal performance on the ANA, 0 otherwise.

- `uncertain_edu`: The respondent's certainty about the respondent's own evaluation of the incumbent's management of schools. Takes on integer values from 1 ("totally sure") to 7 ("not at all sure").

- `edu_rank1`: Equals 1 if respondent ranks education above financial management in importance, 0 otherwise.

- `child_school`: Equals 1 if the respondent has a child in municipal schools, 0 otherwise.

- `govt_eval`: The respondent's evaluation of the incumbent's administration at endline: (1) "excel- lent," (2) "good," (3) "regular," (4) "bad," or (5) "horrible."

C   Online survey experiment

C.1   Respondent recruitment via Facebook ads

We recruited respondents for the online survey experiment via Facebook ads, a well-established, low-cost method for subject recruitment in comparative politics (Boas, Christenson, and Glick 2020). Targeted Facebook users saw advertisements offering a chance to win 100 Brazilian reais (about 24 US dollars) for taking a survey, with a 1/100 probability of winning. To ensure a diverse sample, ads were targeted to respondents in distinct strata of region and age. We also excluded Brazil's 300 largest municipalities from our advertising campaign in order to recruit enough respondents from small and medium-sized municipalities, which make up the immense majority of the sample in the RDD and field experiment. Finally, to increase statistical power, we more heavily targeted states where a larger share of municipalities did not meet their IDEB target, which is the less common outcome. We aimed to recruit 3,000 Brazilians, and ended up surveying 3,118 respondents.

C.2 Survey experiment covariate balance

We examine balance in covariates by regressing each of them on the treatment indicator. We do this for the group as a whole, and for the two subsets of individuals in municipalities that met or did not meet their IDEB target. Table C.18, C.19 and C.20 present the results. Across all three comparisons treatment and control groups are balanced on covariates.

	Mean treatment	Mean control	SD	p value
region_north	-0.004	0.004	-2.865	0.435
region_centerwest	-0.001	0.001	-0.530	0.883
region_southeast	-0.011	0.010	-4.507	0.214
region_south	0.011	-0.010	5.300	0.133
priority_education	2.261	2.260	0.077	0.983
priority_healthcare	1.799	1.797	0.143	0.969
priority_economy	3.791	3.753	2.981	0.410
priority_socialassistance	3.928	3.962	-3.156	0.381
priority_security	3.222	3.228	-0.522	0.883
had_heard_of_ideb	0.701	0.695	1.476	0.683
age	-0.115	0.110	-1.841	0.614
female	0.002	-0.002	1.003	0.781
race_white	-0.008	0.008	-3.144	0.383
race_brown	0.012	-0.012	4.814	0.179
race_other	-0.004	0.004	-2.410	0.508
education	0.038	-0.036	3.310	0.362

Table C.18 – Covariate balance for the online survey experiment: All respondents

	Mean treatment	Mean control	SD	p value
region_north	-0.011	0.003	-5.465	0.222
region_centerwest	0.005	0.006	-0.674	0.876
region_southeast	0.024	0.040	-3.211	0.457
region_south	0.005	-0.017	5.489	0.192
priority_education	2.260	2.268	-0.699	0.870
priority_healthcare	1.798	1.799	-0.139	0.975
priority_economy	3.762	3.744	1.391	0.748
priority_socialassistance	3.916	3.959	-3.940	0.363
priority_security	3.264	3.229	2.931	0.486
had_heard_of_ideb	0.700	0.702	-0.497	0.908
age	0.026	0.568	-4.507	0.307
female	-0.001	-0.006	1.190	0.782
race_white	0.003	0.022	-3.904	0.364
race_brown	0.005	-0.019	4.820	0.260
race_other	-0.008	-0.004	-1.282	0.767
education	0.014	-0.042	2.504	0.561

Table C.19 – Covariate balance for the online survey experiment: Respondents in municipalities that had met their IDEB target

	Mean treatment	Mean control	SD	p value
region_north	0.012	0.006	2.005	0.758
region_centerwest	-0.012	-0.014	0.800	0.903
region_southeast	-0.087	-0.066	-4.848	0.469
region_south	0.024	0.006	4.251	0.513
priority_education	2.262	2.239	2.085	0.750
priority_healthcare	1.801	1.792	0.839	0.902
priority_economy	3.853	3.774	6.124	0.354
priority_socialassistance	3.954	3.971	-1.556	0.810
priority_security	3.130	3.223	-8.115	0.219
had_heard_of_ideb	0.704	0.675	6.449	0.335
age	-0.421	-1.072	5.131	0.427
female	0.009	0.008	0.255	0.969
race_white	-0.032	-0.030	-0.247	0.970
race_brown	0.027	0.007	4.206	0.523
race_other	0.004	0.024	-5.685	0.403
education	0.090	-0.020	4.917	0.470

Table C.20 – Covariate balance for the online survey experiment: Respondents in municipalities that had not met their IDEB target

C.3 Ranking of policy areas by respondents
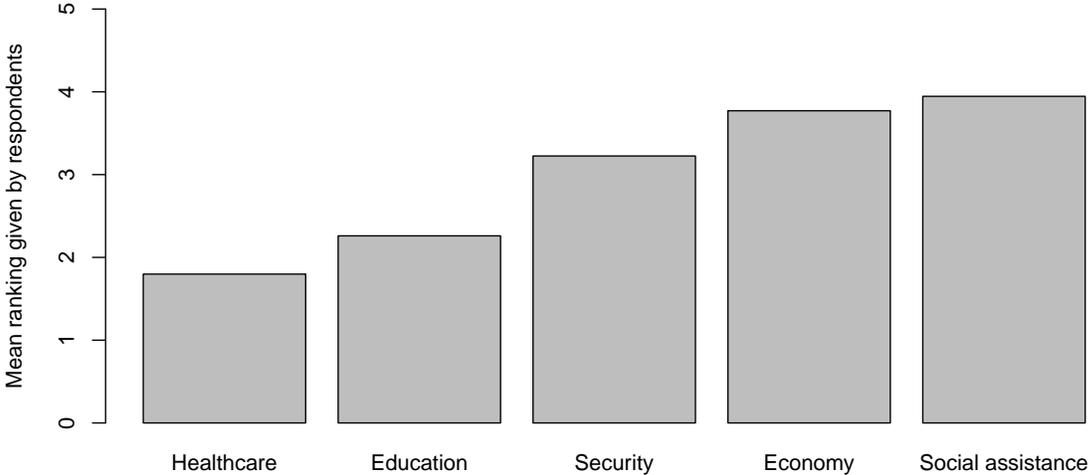
Figure C.12 – Mean ranking given by online survey respondents when asked to rank policy areas according to the priority they should be given by their municipal government

C.4 Additional, pre-specified results

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment positive	0.214***	0.125***	0.094**	0.120***	0.199***	0.124***	0.093**	0.149***
	(0.044)	(0.047)	(0.042)	(0.043)	(0.046)	(0.047)	(0.042)	(0.044)
Constant	2.422***	2.375***	2.372***	2.247***	2.486***	2.377***	2.414***	2.306***
	(0.031)	(0.032)	(0.029)	(0.030)	(0.032)	(0.032)	(0.029)	(0.030)
Observations	2172	2172	2172	2172	2172	2172	2172	2172
R^2	0.025	0.019	0.011	0.016	0.021	0.012	0.014	0.015

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.21 – Survey experiment results: Positive treatment (individuals in municipalities that met their IDEB target). Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment negative	−0.061	−0.040	−0.084	0.004	−0.160**	−0.047	0.008	−0.014
	(0.069)	(0.074)	(0.066)	(0.067)	(0.070)	(0.073)	(0.066)	(0.066)
Constant	2.214***	2.220***	2.276***	2.036***	2.298***	2.237***	2.257***	2.126***
	(0.049)	(0.053)	(0.047)	(0.048)	(0.050)	(0.052)	(0.047)	(0.048)
Observations	917	917	917	917	917	917	917	917
R^2	0.029	0.031	0.033	0.019	0.040	0.028	0.040	0.038

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.22 – Survey experiment results: Negative treatment (individuals in municipalities that did not meet their IDEB target). Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment positive	0.291***	0.176***	0.174***	0.200***	0.289***	0.189***	0.183***	0.214***
	(0.055)	(0.058)	(0.052)	(0.054)	(0.056)	(0.058)	(0.052)	(0.054)
Tr. pos. x Ed. low priority	−0.256***	−0.165*	−0.255***	−0.231**	−0.273***	−0.205**	−0.291***	−0.188**
	(0.095)	(0.100)	(0.090)	(0.093)	(0.098)	(0.101)	(0.091)	(0.094)
Education low priority	0.152**	0.028	0.079	0.041	0.166**	0.072	0.100	0.042
	(0.064)	(0.068)	(0.061)	(0.063)	(0.067)	(0.068)	(0.061)	(0.064)
Constant	2.376***	2.371***	2.351***	2.235***	2.433***	2.359***	2.386***	2.294***
	(0.038)	(0.040)	(0.036)	(0.037)	(0.039)	(0.040)	(0.036)	(0.038)
Observations	2172	2172	2172	2172	2172	2172	2172	2172
R^2	0.037	0.029	0.024	0.026	0.032	0.023	0.031	0.028

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.23 – Survey experiment results: Positive treatment (individuals in municipalities that met their IDEB target), by whether they give high or low priority to education. 'Education low priority' is an indicator for whether respondents give below-median priority to education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment negative	−0.084	−0.038	−0.034	−0.053	−0.183**	−0.016	−0.007	−0.037
	(0.085)	(0.092)	(0.082)	(0.082)	(0.086)	(0.090)	(0.081)	(0.082)
Tr. neg. x Ed. low priority	0.107	−0.015	−0.154	0.147	0.115	−0.098	0.055	0.068
	(0.149)	(0.161)	(0.144)	(0.145)	(0.152)	(0.159)	(0.143)	(0.145)
Education low priority	0.093	0.121	0.107	−0.089	0.073	0.117	0.128	−0.027
	(0.106)	(0.115)	(0.103)	(0.104)	(0.109)	(0.114)	(0.102)	(0.104)
Constant	2.180***	2.183***	2.248***	2.074***	2.262***	2.202***	2.217***	2.139***
	(0.061)	(0.066)	(0.059)	(0.059)	(0.062)	(0.065)	(0.059)	(0.060)
Observations	917	917	917	917	917	917	917	917
R^2	0.059	0.049	0.049	0.037	0.061	0.046	0.060	0.051

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table C.24 – Survey experiment results: Negative treatment (individuals in municipalities that did not meet their IDEB target), by whether they give high or low priority to education. 'Education low priority' is an indicator for whether respondents give below-median priority to education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

C.5 Results with weights to approximate the voter population in the RDD

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment positive	0.214***	0.113	0.077	0.103	0.184***	0.105	0.068	0.129*
	(0.065)	(0.069)	(0.063)	(0.065)	(0.068)	(0.069)	(0.062)	(0.066)
Constant	2.429***	2.378***	2.380***	2.256***	2.486***	2.376***	2.413***	2.312***
	(0.045)	(0.048)	(0.044)	(0.045)	(0.048)	(0.048)	(0.043)	(0.046)
Observations	2172	2172	2172	2172	2172	2172	2172	2172
R^2	0.060	0.067	0.039	0.060	0.036	0.059	0.027	0.054

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.25 – Survey experiment results: Positive treatment (individuals in municipalities that met their IDEB target). Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely. Data reweighted by education level according to census figures for municipalities within the RDD bandwidth (column 1 in Table D.31).

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment negative	−0.056	−0.015	−0.065	0.017	−0.130	−0.040	0.023	0.023
	(0.104)	(0.113)	(0.099)	(0.097)	(0.105)	(0.110)	(0.096)	(0.099)
Constant	2.233***	2.224***	2.293***	2.045***	2.303***	2.245***	2.269***	2.135***
	(0.075)	(0.081)	(0.071)	(0.069)	(0.075)	(0.079)	(0.069)	(0.071)
Observations	917	917	917	917	917	917	917	917
R^2	0.083	0.115	0.155	0.095	0.162	0.080	0.134	0.174

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.26 – Survey experiment results: Negative treatment (individuals in municipalities that did not meet their IDEB target). Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely. Data reweighted by education level according to census figures for municipalities within the RDD bandwidth (column 1 in Table D.31).

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment positive	0.287***	0.148*	0.153**	0.175**	0.262***	0.145*	0.146**	0.185**
	(0.076)	(0.082)	(0.074)	(0.076)	(0.081)	(0.082)	(0.073)	(0.078)
Tr. pos. x Ed. low priority	−0.245*	−0.106	−0.259**	−0.256*	−0.260*	−0.170	−0.270**	−0.180
	(0.137)	(0.147)	(0.131)	(0.136)	(0.145)	(0.146)	(0.130)	(0.138)
Education low priority	0.147	−0.022	0.065	0.042	0.156	0.046	0.086	0.017
	(0.093)	(0.100)	(0.090)	(0.093)	(0.099)	(0.100)	(0.089)	(0.095)
Constant	2.382***	2.390***	2.365***	2.250***	2.442***	2.371***	2.395***	2.308***
	(0.054)	(0.058)	(0.052)	(0.054)	(0.057)	(0.058)	(0.051)	(0.055)
Observations	2172	2172	2172	2172	2172	2172	2172	2172
R^2	0.124	0.108	0.110	0.135	0.090	0.115	0.103	0.112

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.27 – Survey experiment results: Positive treatment (individuals in municipalities that met their IDEB target), by whether they give high or low priority to education. 'Education low priority' is an indicator for whether respondents give below-median priority to education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely. Data reweighted by education level according to census figures for municipalities within the RDD bandwidth (column 1 in Table D.31).

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment negative	−0.079	−0.061	−0.036	−0.018	−0.167	−0.072	−0.005	−0.018
	(0.118)	(0.133)	(0.115)	(0.114)	(0.123)	(0.124)	(0.108)	(0.116)
Tr. neg. x Ed. low priority	0.119	0.096	−0.101	0.044	0.195	0.037	0.074	0.056
	(0.213)	(0.239)	(0.208)	(0.205)	(0.223)	(0.224)	(0.194)	(0.209)
Education low priority	0.057	−0.008	0.031	−0.039	−0.038	0.001	0.081	−0.006
	(0.153)	(0.171)	(0.149)	(0.147)	(0.160)	(0.160)	(0.139)	(0.149)
Constant	2.209***	2.233***	2.274***	2.068***	2.289***	2.250***	2.244***	2.163***
	(0.085)	(0.095)	(0.083)	(0.082)	(0.089)	(0.089)	(0.077)	(0.083)
Observations	917	917	917	917	917	917	917	917
R^2	0.215	0.183	0.239	0.167	0.227	0.223	0.270	0.244

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.28 – Survey experiment results: Negative treatment (individuals in municipalities that did not meet their IDEB target), by whether they give high or low priority to education. 'Education low priority' is an indicator for whether respondents give below-median priority to education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely. Data reweighted by education level according to census figures for municipalities within the RDD bandwidth (column 1 in Table D.31).

C.6   Results with a lower threshold to define respondents who give low priority to education

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment positive	0.239***	0.132***	0.129***	0.150***	0.235***	0.144***	0.115**	0.170***
	(0.048)	(0.050)	(0.045)	(0.047)	(0.049)	(0.050)	(0.046)	(0.047)
Tr. pos. x Ed. v. low priority	−0.285**	−0.184	−0.313**	−0.290**	−0.294**	−0.316**	−0.244*	−0.195
	(0.141)	(0.149)	(0.134)	(0.139)	(0.146)	(0.149)	(0.135)	(0.140)
Education very low priority	0.291***	0.212**	0.226**	0.196**	0.296***	0.336***	0.214**	0.179*
	(0.098)	(0.103)	(0.093)	(0.096)	(0.101)	(0.104)	(0.094)	(0.097)
Constant	2.393***	2.358***	2.350***	2.228***	2.456***	2.348***	2.397***	2.291***
	(0.033)	(0.035)	(0.031)	(0.032)	(0.034)	(0.035)	(0.031)	(0.033)
Observations	2172	2172	2172	2172	2172	2172	2172	2172
R^2	0.035	0.029	0.019	0.025	0.031	0.027	0.024	0.023

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table C.29 – Survey experiment results: Positive treatment (individuals in municipalities that met their IDEB target), by whether they give high or low priority to education. 'Education very low priority' is an indicator for whether espondents' place three or more policy areas as more important than education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

	The mayor invested a lot in:				The mayor improved the quality of:			
	Education	Healthcare	Social Assist.	Security	Education	Healthcare	Social Assist.	Security
Treatment negative	−0.093	−0.049	−0.074	0.005	−0.160**	−0.053	−0.014	−0.021
	(0.074)	(0.080)	(0.071)	(0.072)	(0.076)	(0.079)	(0.071)	(0.072)
Tr. neg. x Ed. v. low priority	0.157	−0.116	−0.207	0.059	−0.114	−0.152	−0.000	0.044
	(0.224)	(0.243)	(0.217)	(0.218)	(0.230)	(0.240)	(0.215)	(0.219)
Education very low priority	0.172	0.235	0.305**	0.097	0.291*	0.188	0.281*	0.118
	(0.158)	(0.172)	(0.153)	(0.154)	(0.162)	(0.169)	(0.152)	(0.155)
Constant	2.210***	2.207***	2.251***	2.035***	2.277***	2.237***	2.236***	2.121***
	(0.053)	(0.057)	(0.051)	(0.051)	(0.054)	(0.056)	(0.051)	(0.051)
Observations	917	917	917	917	917	917	917	917
R^2	0.070	0.052	0.055	0.049	0.061	0.052	0.066	0.053

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.30 – Survey experiment results: Negative treatment (individuals in municipalities that did not meet their IDEB target), by whether they give high or low priority to education. 'Education very low priority' is an indicator for whether espondents' place three or more policy areas as more important than education. Controls omitted. The dependent variable is respondents' agreement with the statement 'How much do you agree with the following statements about the mayor?'on a 1-4 scale (1 = disagree completely, 4 = agree completely.

CATE on beliefs that the mayor invested a lot of money in...

CATE on beliefs that the mayor improved the quality of...

CATE with 95% confidence interval

CATE with 95% confidence interval

Positive treatment | Education more valued
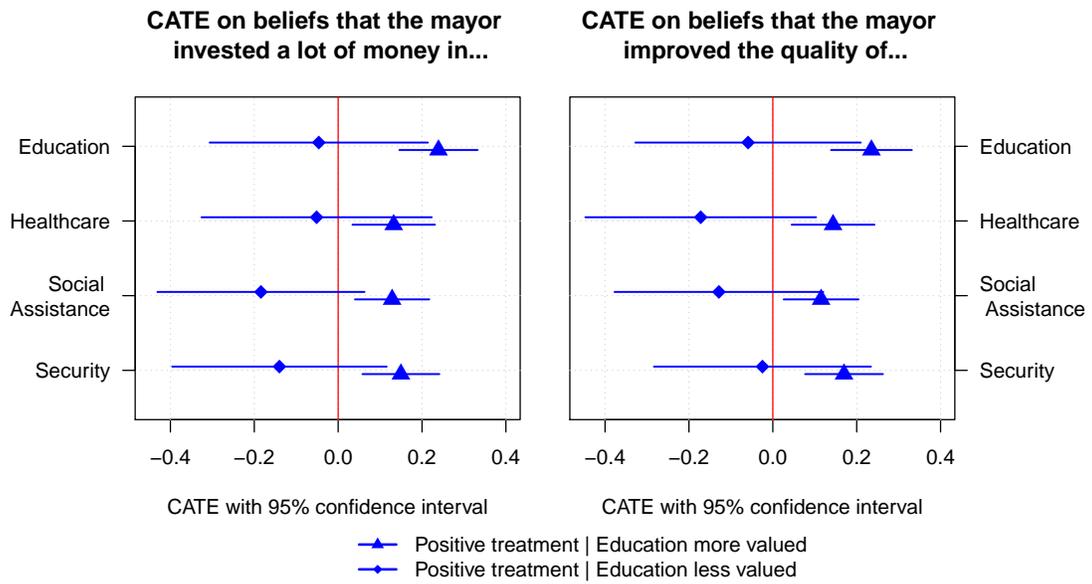Positive treatment | Education less valued

Figure C.13 – Conditional Average Treatment Effects (CATE) of receiving positive information about the quality of schools on respondents' agreement with statements about the mayor, by whether they give very low priority to education. Outcomes are measured in scales that go from 1 ("disagree completely") to 4 ("agree completely"). The "education more valued" group is composed of those who rank education among their top three priorities. The "education less valued" group is composed of those who rank at least three policy areas above education.

D   Additional Analysis

D.1   Null relationship between municipal education spending and school quality scores

To examine whether there is a relationship between municipal education spending and municipal primary school quality scores, we use official data from Brazil's National Treasury (FINBRA) and from the Ministry of Education. We first calculate the relative increase in spending and IDEB scores between 2013 and 2017 (dividing the increase by the baseline level), and regress the first on the second.

The resulting slope coefficient measures the association between spending in education and school quality indicators. By using a first-differenced model, we get rid of any time-invariant confounding. Results are presented in Table D.31 below. There is no statistically significant association between increases in spending and any of the measures of school quality improvement that we consider: whether the municipality met its IDEB target in 2017, the increase of IDEB scores in the period, the increase in the gap between the IDEB score and the IDEB target in the period, and the increase in IDEB scores relative to scores at baseline. The latter is our preferred specification, since it accounts for baseline levels of IDEB. Figure D.14 presents this null relationship from Model 4. All in all, the data suggest increases in spending are not correlated with increases in school quality.

Menezes Filho and Amaral (2009) reach a similar conclusion after analyzing the relationship between municipal spending in education school performance in 2005. Monteiro (2015) finds a null relationship between increased education spending and test scores, leveraging as an instrumental variable increases in municipal revenue stemming from oil royalties.

	2017 target met	Increase 2013 - 2017 in IDEB:		
		Score	Score - Target	Score, rel. to 2013
Education spending	0.0015	−0.0013	−0.0013	−0.0003
	(0.0017)	(0.0021)	(0.0021)	(0.0005)
Observations	4892	4658	4637	4658
R^2	0.0002	0.0001	0.0001	0.0001

$^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

Table D.31 – Relationship between municipal education spending (measured as increase from 2013 to 2017, relative to 2013), and performance of municipal primary education schools in IDEB.

Note. Intercept omitted from the table. HC2 heteroskedasticity consistent standard errors in parentheses.
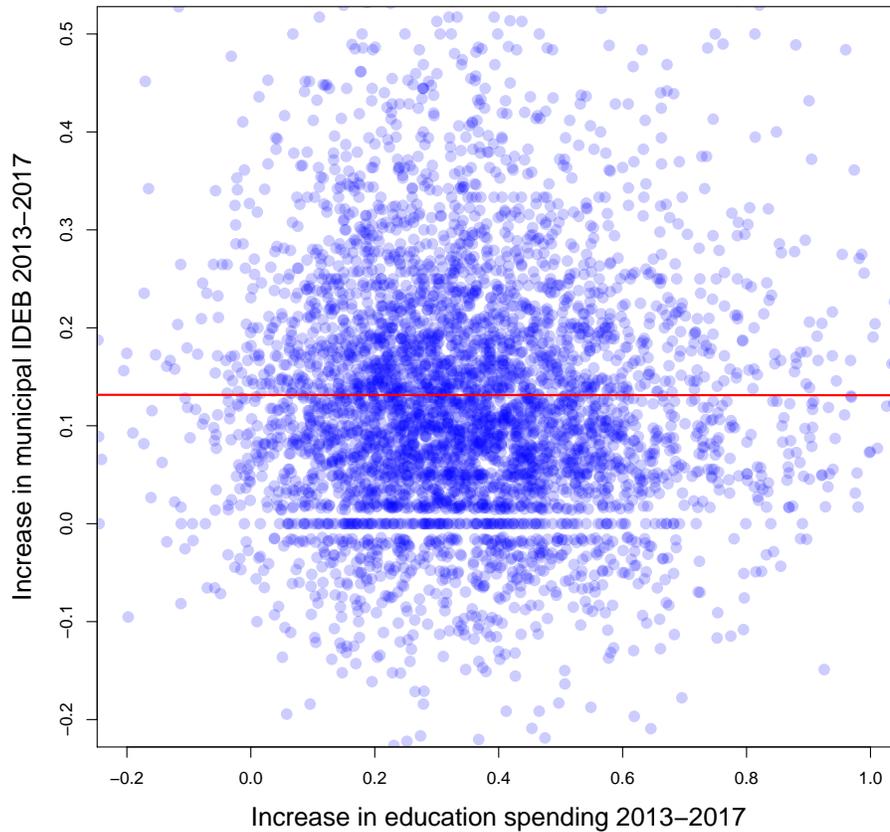

Figure D.14 – Relationship between relative increases in municipal education spending and relative increases in municipal school quality scores. Red line is the regression line from Model 4 in Table D.31.

D.2 Sample Comparisons

	RDD: Reelection	Census (Municipalities)	Panel Survey	Online Survey	Census (Individuals)
Municipality					
Median Population	9,000.5	10,934	27,912	37,164	145,705
Region					
Center-West	10.1	8.4	0	10.3	7.3
Northeast	14.3	32.2	100	30.4	26.6
North	11.9	8.1	0	7.5	7.4
Southeast	39.5	30	0	32.8	43.8
South	24.2	21.3	0	19	14.9
Race					
White	50.3	47.1	28.7	45.9	49.2
Black	6.7	7.1	9.2	9.8	8.2
Brown	41.2	44.1	58.9	40.7	41
Other	1.8	1.6	3.2	3.6	1.5
Education					
Less than Primary	58.9	60.5	56.8	4.1	45.3
Primary	15	14.7	11.3	11.8	16.7
Secondary	20.6	19.8	26.6	54.8	28
Higher	5.6	5.1	5.3	29.3	10
Other					
Median Age	39.7	39.3	38	32	38
Male	50.5	50.3	49.6	37.8	48.2

Table D.32 – Sample Statistics

Figures are percentages, except for median population and median age. RDD figures are municipal-level averages of census data for all unique municipalities within the bandwidth, weighting each municipality equally. Survey and Brazilian census figures weight each individual equally. Education is the highest level completed. Census figures are for residents 18 and older, except for median municipal population. All census data are from 2010.

D.3   Education versus Other Problems

	Average	2019	2017	2014	2012	2010	2008	2007
Security	21.6	17.6	8.0	26.6	18.3	24.4	26.9	29.7
Health	15.8	15.1	8.6	25.2	21.3	14.0	17.5	9.1
Corruption	13.4	16.4	19.0	14.0	12.0	8.5	10.8	13.3
Unemployment	13.3	18.0	17.8	3.7	8.1	12.1	13.4	20.3
Poverty/Inequality	6.2	1.0	0.8	5.9	8.0	9.5	10.2	8.4
Economy	5.4	6.6	19.2	3.3	1.9	3.4	2.1	1.1
Drugs	4.0	0.9	1.0	6.6	7.0	6.7	4.5	1.6
Education	3.6	4.0	1.3	4.1	5.7	3.9	4.1	2.2

Table D.33 – Most Serious Problem Facing the Country: AmericasBarometer

Entries are percentage of respondents spontaneously mentioning each item. Some categories combine similar items labeled separately in the data files.
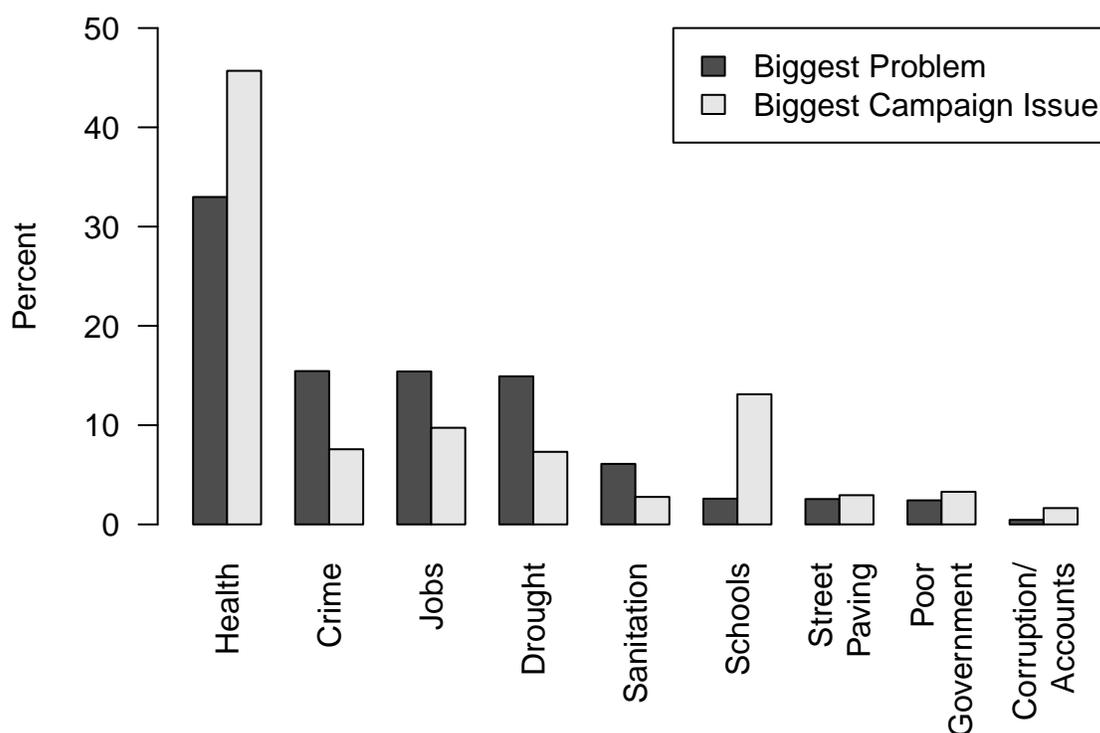

Figure D.15 – Biggest Problem and Biggest Campaign Issue in the Municipality: Pernambuco Survey

D.4 Predictors of Education as a Problem or Priority

	DV: Education as a...		
	Problem (LAPOP)	Problem (Panel)	Priority (Online)
	(1)	(2)	(3)
Education	0.149***	0.056**	0.091***
	(0.016)	(0.028)	(0.018)
Age	−0.006	−0.027***	0.001
	(0.004)	(0.009)	(0.003)
Female	0.130	0.382	0.103
	(0.109)	(0.235)	(0.079)
White	0.356***	0.065	−0.211**
	(0.127)	(0.255)	(0.089)
Black	0.146	−0.011	−0.089
	(0.179)	(0.442)	(0.137)
IDEB Gap	−0.063	0.454	−0.030
	(0.138)	(0.286)	(0.069)
Constant	−4.243***	−3.623***	0.230
	(0.312)	(0.519)	(0.195)
Observations	9,138	3,192	3,092
Log Likelihood	−1,424.485	−356.147	−1,950.340
Akaike Inf. Crit.	2,880.969	726.294	3,922.679

Table D.34 – Predictors of Education as a Problem or Priority

NOTE: Entries are logistic regression coefficients with estimated standard errors in parentheses. Model 1 analyzes the biggest problem facing the country using the 2008, 2010, 2012, 2014, 2017, and 2019 waves of the AmericasBarometer by LAPOP and includes region and year fixed effects. Model 2 analyzes the biggest problem facing the municipality using the Pernambuco panel survey. Model 3 analyzes priorities in the municipality using the online survey and includes region fixed effects. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

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