How patronage delivers: Political appointments, bureaucratic accountability, and service delivery in Brazil

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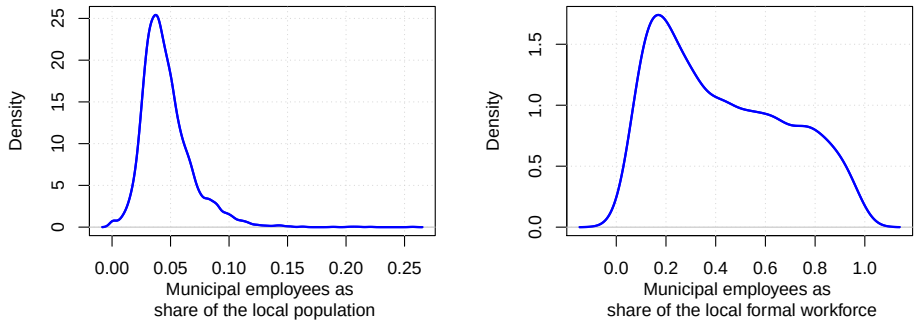
Online Appendices

Appendices

A Additional details of the institutional context

A.1 Size of the municipal government workforce

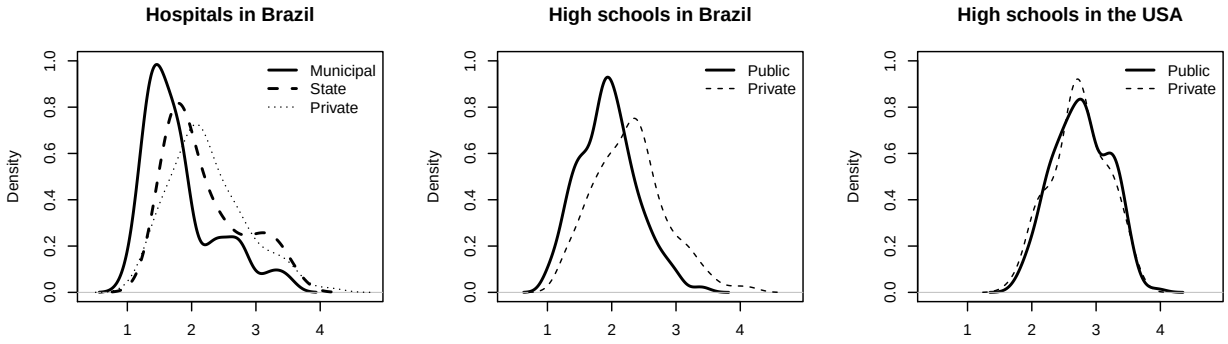
Figure A-9: Size of the municipal government workforce as a proportion of the total local population and the total formal labor market workforce



Calculated using administrative data of the universe of formal labor market contracts in 2016, and official population statistics for 2016.

A.2 Management practices in Brazilian schools and hospitals

Figure A-10: Scores of the World Management Survey for hospitals and high schools in Brazil, and for high schools in the USA



Data correspond to 289 hospitals and 513 high schools that were randomly selected in Brazil, as well as 270 high schools in the USA for comparison. Most public high schools in Brazil are managed by state governments. I only code as municipal or state hospitals those that have those words in their name. Data are from Bloom et al. (2014, 2015).

A.3 Predictors of school directors' appointment mode and school quality

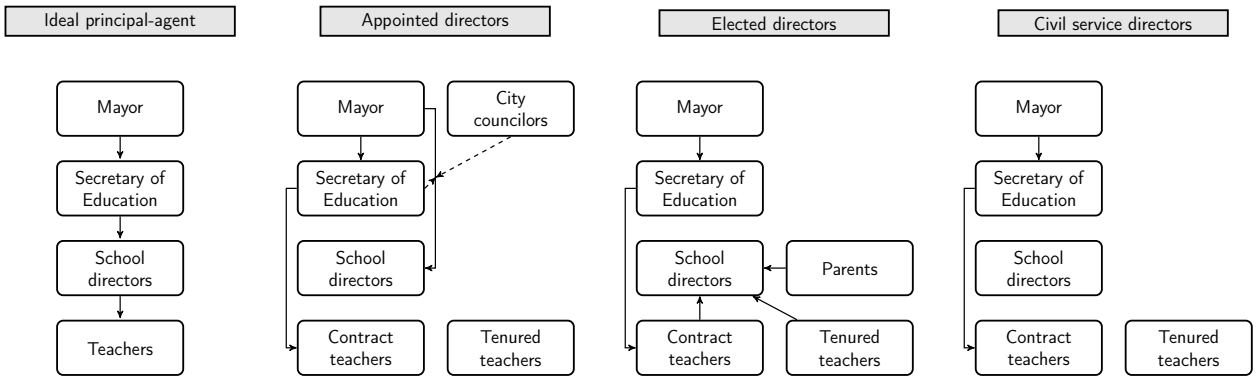
Table A-5: Observational predictors of municipal schools' director appointment mode and quality score (IDEB), from cross-section data on municipalities, schools, and directors (2013)

	Appointed			Elected	Civil service	IDEB score				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intercept	0.965***	2.587***	2.418***	−1.075***	−0.278***	3.735***	−1.991***	−2.141***	−1.451***	
	(0.018)	(0.038)	(0.043)	(0.039)	(0.022)	(0.068)	(0.138)	(0.121)	(0.114)	
Director covariates										
Female	0.004	0.022***	0.033***	−0.021**	−0.015***	0.279***	0.169***	0.015	0.006	0.026
	(0.007)	(0.007)	(0.007)	(0.006)	(0.004)	(0.026)	(0.023)	(0.020)	(0.018)	(0.014)
Age 40-49 (vs <40)	−0.066***	−0.021***	−0.018**	0.020***	−0.007*	0.073***	0.021	−0.021	−0.012	−0.004
	(0.007)	(0.006)	(0.006)	(0.006)	(0.003)	(0.020)	(0.018)	(0.015)	(0.014)	(0.011)
Age 50+ (vs <40)	−0.097***	−0.016*	−0.012	0.010	−0.005	0.073**	0.006	−0.057**	−0.052**	−0.029*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.004)	(0.026)	(0.024)	(0.020)	(0.018)	(0.014)
White (vs other race)	−0.032*	0.019	0.038**	−0.054***	0.023***	0.700***	0.397***	0.113**	0.002	−0.017
	(0.014)	(0.013)	(0.014)	(0.013)	(0.007)	(0.047)	(0.042)	(0.035)	(0.032)	(0.025)
Black/brown (vs other race)	0.070***	0.059***	0.055***	−0.023	−0.019**	−0.096*	−0.049	−0.030	−0.050	−0.030
	(0.014)	(0.013)	(0.013)	(0.013)	(0.006)	(0.046)	(0.042)	(0.035)	(0.032)	(0.024)
Tertiary degree (vs < tertiary)	−0.090***	−0.012	−0.006	0.026**	−0.013**	0.448***	0.291***	0.165***	0.102***	−0.022
	(0.011)	(0.010)	(0.011)	(0.009)	(0.005)	(0.036)	(0.033)	(0.030)	(0.028)	(0.022)
Postgraduate degree (vs < tertiary)	−0.086***	−0.061***	−0.054***	0.038***	0.005	0.299***	0.212***	0.131***	0.118***	0.033**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.020)	(0.018)	(0.016)	(0.014)	(0.012)
No other jobs	−0.038***	−0.027***	−0.023***	0.033***	−0.015***	0.213***	0.167***	0.139***	0.097***	0.001
	(0.006)	(0.005)	(0.006)	(0.005)	(0.003)	(0.019)	(0.017)	(0.014)	(0.013)	(0.011)
6-15 years of teaching exp. (vs <6)	−0.050***	−0.027***	−0.024***	0.026***	0.011**	0.090***	0.040	−0.004	−0.024	−0.028*
	(0.007)	(0.007)	(0.007)	(0.006)	(0.004)	(0.023)	(0.021)	(0.018)	(0.017)	(0.014)
>15 years of teaching exp. (vs <6)	−0.032***	−0.015	−0.010	0.030***	−0.005	0.186***	0.080***	0.004	−0.024	−0.032*
	(0.009)	(0.008)	(0.008)	(0.007)	(0.004)	(0.027)	(0.024)	(0.021)	(0.019)	(0.015)
3-10 years of director exp. (vs <3)	−0.019*	0.033***	0.033***	−0.082***	0.031***	0.092***	0.072**	0.021	0.014	0.009
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.027)	(0.024)	(0.020)	(0.019)	(0.015)
>10 years of director exp. (vs <3)	−0.007	0.078***	0.080***	−0.197***	0.095***	0.194***	0.124***	0.022	0.005	0.038
	(0.012)	(0.012)	(0.012)	(0.010)	(0.008)	(0.037)	(0.034)	(0.028)	(0.026)	(0.021)
3-10 years as director of school (vs <3)	−0.182***	−0.180***	−0.180***	0.222***	−0.010	−0.023	0.003	0.062**	0.053**	0.064***
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.026)	(0.024)	(0.020)	(0.019)	(0.015)
>10 years as director of school (vs <3)	−0.265***	−0.248***	−0.245***	0.275***	0.002	0.029	0.057	0.151***	0.115***	0.101***
	(0.015)	(0.014)	(0.014)	(0.013)	(0.011)	(0.043)	(0.040)	(0.034)	(0.030)	(0.025)
Municipality covariates										
GDP per capita (logged)		−0.088***	−0.043***	0.020***	0.020***		0.665***	0.089***	0.038***	
		(0.004)	(0.005)	(0.004)	(0.002)		(0.014)	(0.013)	(0.011)	
Population (logged)		−0.082***	−0.087***	0.068***	0.006***		−0.104***	−0.097***	−0.073***	
		(0.002)	(0.002)	(0.002)	(0.001)		(0.005)	(0.005)	(0.004)	
Number of deaths per 1,000		−0.017***	−0.009***	0.012***	−0.002**		0.108***	0.020***	0.007	
		(0.002)	(0.002)	(0.002)	(0.001)		(0.005)	(0.005)	(0.004)	
Mayor is in first term		−0.000	0.006	−0.022***	0.004		0.071***	0.034*	0.004	
		(0.006)	(0.006)	(0.006)	(0.003)		(0.017)	(0.015)	(0.014)	
Electoral concentration		0.062**	0.044*	0.017	−0.071***		0.282***	0.452***	0.275***	
		(0.021)	(0.021)	(0.019)	(0.011)		(0.060)	(0.052)	(0.049)	
School covariates										
Rural			0.028***	−0.025***	0.003			−0.014	−0.041*	−0.021
			(0.007)	(0.006)	(0.003)			(0.021)	(0.019)	(0.016)
Number of staff (logged)			0.005	0.015**	−0.016***			0.009	0.005	−0.113***
			(0.005)	(0.005)	(0.003)			(0.014)	(0.012)	(0.013)
Students per classroom (average)			0.006***	−0.006***	0.001***			−0.009***	−0.008***	0.006***
			(0.001)	(0.001)	(0.000)			(0.002)	(0.002)	(0.002)
School socioeconomic index			−0.009***	0.007***	0.003***			0.141***	0.078***	0.093***
			(0.001)	(0.001)	(0.000)			(0.002)	(0.002)	(0.002)
Director is appointed (vs civil service)						−0.631***	−0.377***	−0.174***	0.000	0.032
						(0.030)	(0.028)	(0.023)	(0.022)	(0.028)
Director is elected (vs civil service)						−0.333***	−0.234***	−0.155***	0.011	0.042
						(0.031)	(0.028)	(0.024)	(0.022)	(0.031)
IDEB target									0.585***	0.269***
									(0.010)	(0.010)
Municipality fixed effects	No	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.099	0.252	0.259	0.186	0.071	0.243	0.382	0.570	0.659	
Num. obs.	30748	30038	29273	29273	29273	17404	17402	16814	15622	15622
Adj. R^2 (full model)										0.816
Adj. R^2 (proj model)										0.085

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. HC1 standard errors in brackets

A.4 Accountability relationships by director appointment mode

Figure A-11: Four models of appointments in Brazil's municipal basic education sector



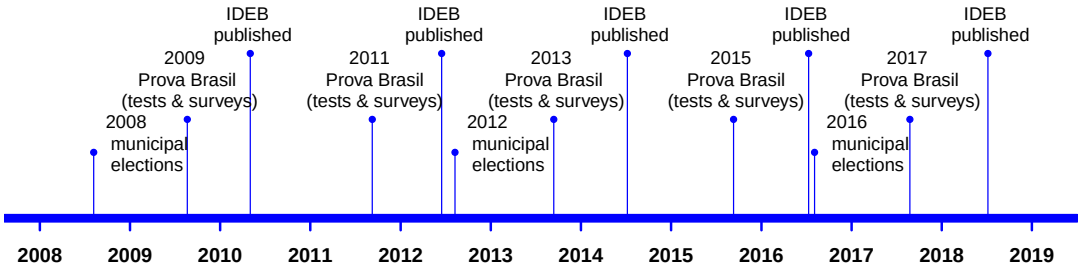
The dashed lines represent occasional participation of city councilors and secretaries in political appointments.

A.5 Additional details on ANRESC

This article leverages data from the National Assessment of School Performance (ANRESC) to measure school quality (through IDEB) and school director turnover (through the director survey). ANRESC is done every two years (Figure A-12). It is designed and implemented by INEP, a high-autonomy, high-capacity agency of the federal government (Bersch et al., 2017). Brazil's system for measuring school performance is regularly praised by international organizations like the World Bank and the OECD. In words of a group of World Bank economists, ANRESC is "one of the world's most impressive systems for measuring education results, superior to current practice in the United States and in many other OECD countries in the quantity, relevance, and quality of the student and school performance information it provides" (Bruns et al., 2011). The OECD recently said the system has "a similar level of sustainability, reliability and validity as national assessment systems found in many OECD countries" (OECD, 2021).

IDEB scores are 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. 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. On the other hand, IDEB targets are impossible to manipulate. They were defined a priori following technical criteria and published at the beginning of the period (Fernandes, 2007) .

Figure A-12: Timeline of IDEB tests and information release



A.6 Illustrative materials for schools to prepare for student tests

The following resources were produced by governments and NGOs to help schools and school directors prepare in the short term for the ANRESC student tests.

- *7 ações para aproveitar bem a Prova Brasil* (7 actions to take advantage of Prova Brasil), produced by Gestão Escolar (School Management), the director-geared section of Nova Escola, which is a leading education magazine in Brazil. Published on September, 2 months before the implementation of the tests. Available here.
- *Como preparar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), also produced by Gestão Escolar. Published in August, 3 months before the implementation of the test. Available here.
- *Dicas para preparar sua escola para a Prova Brasil* (Tips to prepare your school for Prova Brasil), produced by Educador360, another education site, in a section called Pedagogic management. Published in early November, at the beginning of the period when the test was implemented. Available here.
- *Escolas da SEMED reforçam atividades de preparação para Prova Brasil* (Municipal Education Secretariat schools reinforce preparation activities for Prova Brasil), produced by the municipality of Manaus (in the Amazon), describing a number of overlapping strategies the municipality implemented to prepare for Prova Brasil. Published in late September, weeks before the test. Available here.
- *Como preprarar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), slides for how to prepare for the test, dated in October just one month before the test. Published on the site of the secretariat of education of the state of Goiás. Available here.

B Additional details of in-depth interviews

In-depth interviews with local actors gave origin to the hypotheses tested in this article, but were part of a larger empirical study of patronage in Brazil. Over 18 months of fieldwork in the period 2016-2019 I conducted 121 in-depth, semi-structured interviews with municipal bureaucrats and politicians, and with state-level horizontal accountability actors (e.g., prosecutors). I recruited interviewees at their offices, and collected their oral consent after providing information about the research project and their rights as participants. I conducted interviews in Portuguese, face-to-face, and at the interviewee's office. I chose not to record interviews because some of the topics discussed were highly sensitive, including corrupt and illegal uses of public employment. While recording interviews would have allowed for more complete transcripts, it would have seriously hindered the reliability of the data and subjects' willingness to participate. Some subjects agreed to participate on the condition of anonymity or confidentiality. When quoting interviewees, I specify only their position, the state, and the month of the interview in order to safeguard their identity. In total, I interviewed 51 municipal politicians, 54 municipal bureaucrats, and 16 horizontal accountability actors.[1] Interviews were done in 45 municipalities in 7 states across 3 different regions of Brazil.[2] Locations were chosen to ensure diversity in political and socioeconomic variables.

Within each municipality, fieldwork focused on the center, where government offices are. I approached potential interviewees at their offices and requested an interview after introducing myself and the research project. No compensation of any sort was offered or given to participants. Most subjects that I managed to speak to directly agreed to participate.[3] Interviews were semi-structured, and usually started as an open conversation about the interviewee's background, the challenges they faced in their position, and their perception of public services in the municipality. As the conversation advanced, I followed up with questions about the local dynamics of public employment, including in some cases specific questions about the connection between political turnover, bureaucratic turnover, and public service delivery. I took handwritten notes during and after the interviews. The median duration of interviews was one hour.

Interview locations are quite diverse in their social, economic, and political characteristics. Descriptive statistics for interview locations are shown below.

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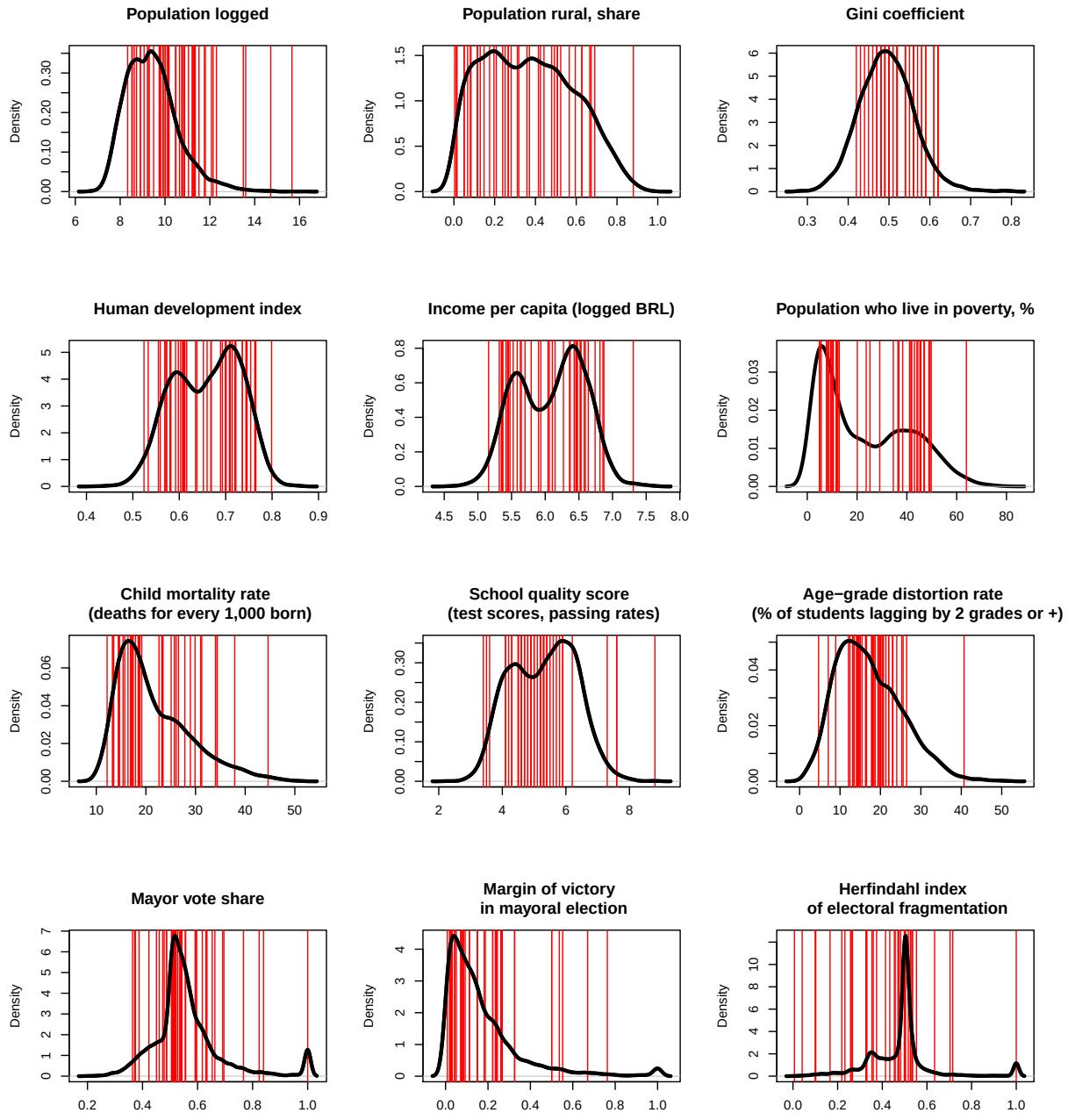
[1] 41 of of the 51 politicians were secretaries. 46 of the 54 bureaucrats were school directors, clinic managers, and social assistance center coordinators. Of the 16 horizontal accountability actors, 8 were state prosecutors or prosecutorial staff.

[2] Interviews were done in the states of Ceará (43 interviews), Rio Grande do Norte (21), Paraíba (15), Rio de Janeiro (19), Minas Geráis (10) São Paulo (1), and Goiás (12).

[3] Some refused, mostly arguing they did not have time. Two refused due to the research topic.

Figure A-13: Characteristics of fieldwork locations



The distribution in black corresponds to all municipalities in Brazil. The vertical, red lines correspond to municipalities where I conducted in-depth interviews.

C Additional details of the difference-in-discontinuities

C.1 Continuity of pre-treatment covariates and the forcing variable

Table A-6 shows the immense majority of pre-treatment covariates do not show a discontinuous jump around the threshold (all but one have p-values above 0.05).

Table A-6: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 5 with pre-treatment covariates as the dependent variable

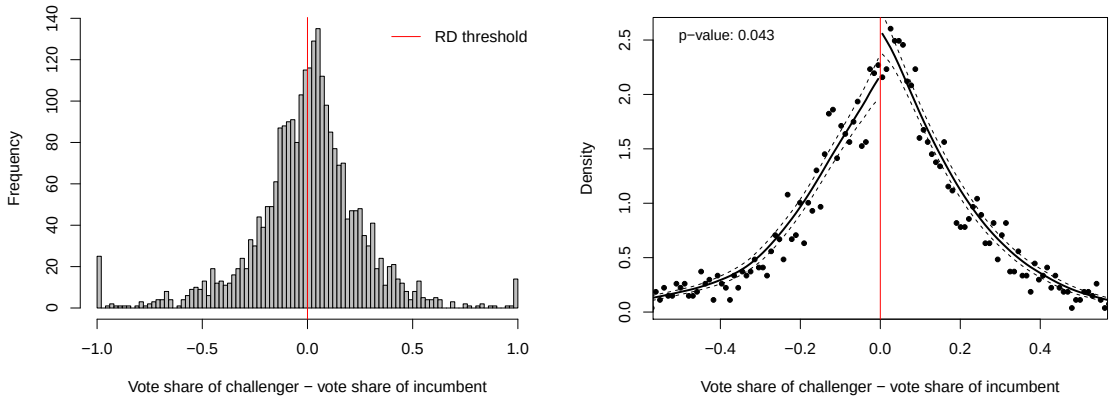
	RD estimate	Standard error	p value
Director is elected	0.141	0.256	0.581
Director is civil service	-0.017	0.099	0.867
Director is female	0.006	0.070	0.928
Director is aged <40	0.057	0.088	0.519
Director is aged 40-49	-0.179	0.099	0.070
Director is aged 50+	0.166	0.091	0.067
Director is white	-0.221	0.116	0.058
Director is black/brown	0.199	0.119	0.093
Director has other race	0.028	0.016	0.093
Director has < tertiary education	-0.023	0.037	0.527
Director has a tertiary degree	0.023	0.037	0.527
Director has a postgraduate degree	0.012	0.057	0.833
Director has no other job	-0.006	0.071	0.929
Director has <6 years of teaching experience	0.096	0.044	0.028
Director has 6-15 years of teaching experience	-0.129	0.084	0.124
Director has >15 years of teaching experience	0.021	0.086	0.810
Director has <3 years of director experience	-0.075	0.081	0.351
Director has 3-10 years of director experience	0.091	0.069	0.189
Director has >10 years of director experience	-0.020	0.041	0.624
Director has held position for <3 years	0.000	0.000	1.000
Municipality GDP per capita (log)	-0.261	0.219	0.234
Municipality population (log)	0.106	0.554	0.849
Municipality deaths per 1,000	-0.225	0.430	0.601
Municipal electoral concentration	0.050	0.033	0.128
School is rural	-0.029	0.067	0.663
Number of staff in the school	2.192	6.770	0.746
School is in a settlement	0.003	0.018	0.887
School is in indigenous land	0.000	0.000	1.000
School is in quilombola land	0.004	0.007	0.543
Students per classroom (average)	2.203	1.098	0.045
School socioeconomic index	-0.528	1.571	0.737
School IDEB score in 2015	-0.239	0.302	0.429
School IDEB target for 2017	-0.287	0.235	0.222

RD estimate corresponds to $\hat{\beta}_1$ in Equation 5.

When it comes to the forcing variable, the histogram shows the number of observations is similar immediately around the cutoff along the forcing variable. Still, the McCrary density test, with a p-value of 0.043, suggests there is a discontinuous jump. While "a running variable with a continuous density is neither necessary nor sufficient for identification" (McCrary, 2008, 701) it is important to consider reasons that may drive the discontinuity identified by the density test. This may be due to Brazilian mayors' incumbency disadvantage (Klašnja and Titiunik, 2017). In any case, the key for identification is "agents' inability to precisely control the assignment variable near the known cutoff" (Lee and Lemieux, 2010). In this case, neither mayors nor their challengers have the ability to precisely control their relative electoral performance. Elections and electoral data are managed by an autonomous, federal court, the Supreme Electoral Court.

An additional observable implication of the lack of precise manipulation assumption is that

Figure A-14: Continuity of the forcing variable: Histogram and McCrary density test



there should be no discontinuous jumps in covariates around the threshold, as shown in Table A-6.

To further assuage concerns about the failure of the McCrary density test, I check the robustness of the results in Table 2 to a "donut hole" approach (Cattaneo et al., 2020, 92). In essence, this strategy assesses the sensitivity of the results to the exclusion of observations immediately around the threshold. Table A-7 shows that results are comparable to those in Table 2 when excluding observations within 0.01 points of the discontinuity.

Table A-7: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors
"Donut hole" approach

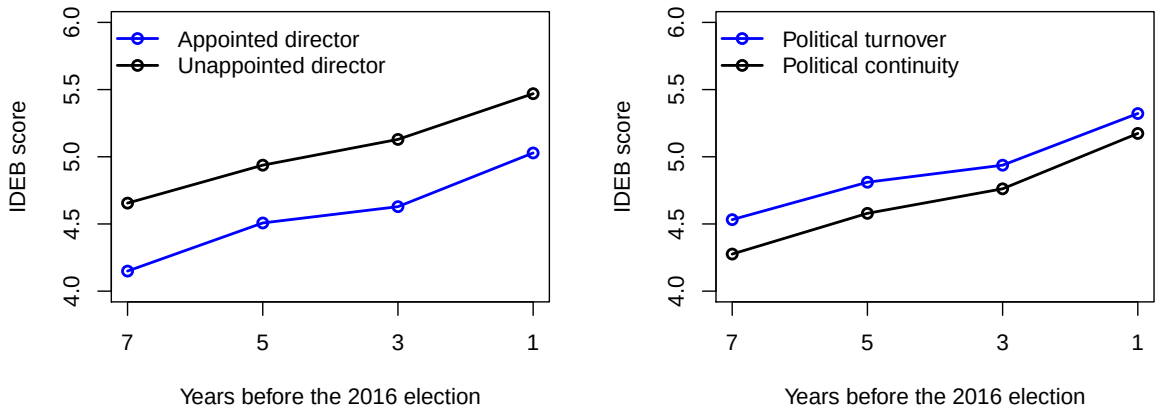
	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.051	0.159	0.087	0.092
	(0.098)	(0.111)	(0.125)	(0.119)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.352*	-0.296	-0.357*
		(0.159)	(0.161)	(0.160)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.161	0.158	0.158	0.158
N	1212	1209	1209	1167

Predictors of whether the director is appointed come from a regression detailed in Appendix A.3. Municipality-clustered standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

C.2 Pre-election trends

Figure A-15 shows how the average IDEB score of schools that enter the diff-in-disc evolve from 2009 to 2015, by whether their director is appointed and whether the mayor loses the 2016 election.

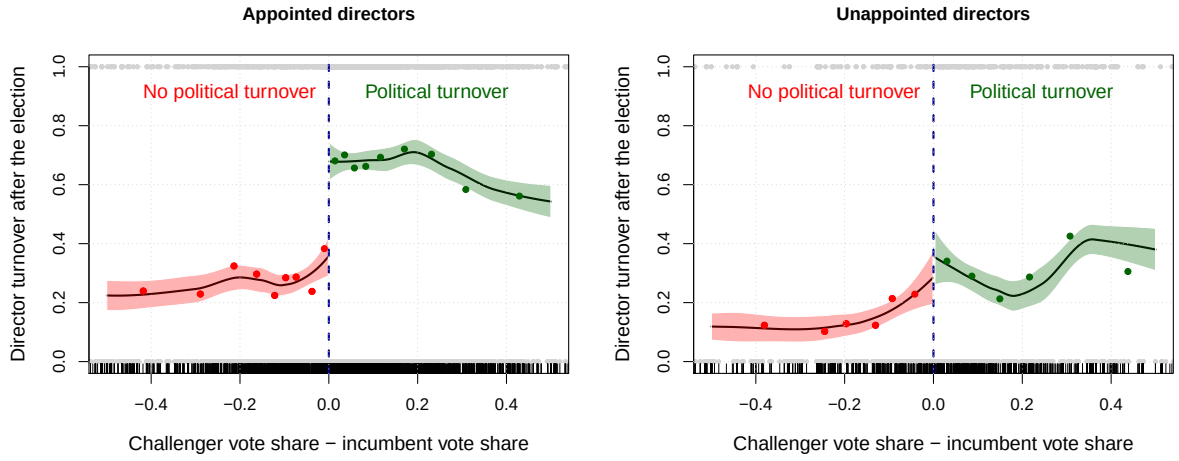
Figure A-15: Pre-treatment trends in school quality scores, by director appointment mode and by political turnover



C.3 RD estimates of the effect of political turnover on director turnover

This appendix shows that an electoral defeat of the incumbent has a significant effect on the replacement of school directors within one year after the election (model 1 in Table A-8). This effect is mostly driven by the replacement of appointed directors (models 2-4).

Figure A-16: Effect of political turnover on director turnover, by director appointment mode



See notes under Figure 2.

Table A-8: Regression discontinuity estimates of the effect of political turnover on director turnover, by director appointment mode

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.255***	0.039	0.046	0.019
	(0.059)	(0.103)	(0.095)	(0.100)
$\hat{\gamma}_2$: Political turnover × Appointed		0.287**	0.298**	0.335**
		(0.110)	(0.103)	(0.105)
$\hat{\beta}_1 + \hat{\gamma}_2$		0.326	0.344**	0.355***
		(0.216)	(0.118)	(0.096)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.116	0.134	0.134	0.134
N	2636	2627	2627	2546

See notes under Table 2.

C.4 Predictors of director turnover after mayor turnover

This appendix examines correlates of directors being replaced after political turnover, using data for municipalities where the mayor loses the election.

Table A-9: Observational predictors of school directors being replaced after political turnover.

	(1)
Intercept	$0.680\ (0.055)^{***}$
Director was appointed	$0.334\ (0.012)^{***}$
IDEB score before the election	$-0.036\ (0.006)^{***}$
Female	$-0.007\ (0.016)$
Age 40-49 (vs <40)	$-0.025\ (0.015)^{*}$
Age 50+ (vs <40)	$-0.038\ (0.018)^{**}$
White (vs other race)	$-0.076\ (0.034)^{**}$
Black/brown (vs other race)	$-0.031\ (0.034)$
Tertiary degree (vs < tertiary)	$-0.074\ (0.030)^{**}$
Postgraduate degree (vs < tertiary)	$-0.031\ (0.016)^{**}$
No other jobs	$-0.023\ (0.013)^{*}$
6-15 years of teaching exp. (vs <6)	$-0.009\ (0.017)$
>15 years of teaching exp. (vs <6)	$0.033\ (0.019)^{*}$
3-10 years of director exp. (vs <3)	$0.006\ (0.015)$
>10 years of director exp. (vs <3)	$-0.046\ (0.021)^{**}$
1-2 years as director of school (vs 3)	$0.000\ (0.015)$
Num. obs.	6558
Adj. R^2 (full model)	0.151
Adj. R^2 (proj model)	0.151

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. HC1 standard errors in brackets

C.5 Alternative estimation: Matching similar schools with and without political turnover

To partially address the sample selection bias, here I match schools in the group without mayor turnover to similar schools in the group with mayor turnover. I do this with exact matching on the covariates that significantly predict director turnover after mayor turnover, as per Appendix C.4. The relevant coefficient is larger than in the main specification in Table 2 and, despite the significantly smaller sample size, remains statistically significant.

Table A-10: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors, after matching

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.237*	0.075	0.031	0.063
	(0.116)	(0.112)	(0.130)	(0.129)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.513**	-0.469*	-0.486*
		(0.196)	(0.206)	(0.204)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.180	0.204	0.204	0.204
N	489	488	488	486

See notes under Table 2.

C.6 Bounds to account for sample selection bias

C.6.1 The Lee (2009) bounds for sample selection bias

To deal with issues of sample selection bias, Lee (2009) proposes a simple procedure to generate bounds for experimental treatment effects. In his framework, each unit has two latent potential outcomes (Y_1^*, Y_0^*) as well as a potential sample selection indicators (S_1, S_0) under treatment $(D = 1)$ and under control $(D = 0)$. For each unit we only observe S_1 or S_0, and one potential outcome Y_1^* or Y_0^* and only if they select into the sample $(S = 1)$. To construct the bounds we need to make two assumptions: independence $(\{Y_1^*, Y_0^*, S_1, S_0\} \perp\!\!\!\perp D)$ and monotonicity (either $S_1 \geq S_0$ or $S_0 \geq S_1$). I use the case where $S_0 \geq S_1$ (i.e., more units are selected into the sample under control than under treatment), for symmetry with my setting. Lee's procedure consists of the following steps:

- Estimate p_0, the proportion of units in the control group that are induced to have an outcome

data ($S = 1$) because of their assignment to control:

$$p_0 = \frac{Pr(S = 1|D = 0) - Pr(S = 1|D = 1)}{Pr(S = 1|D = 0)} \tag{1}$$

- Estimate the p_0^{th} and $(1 - p_0)^{th}$ quantiles of the distribution of $Y|D = 0, S = 1$, which we will call y_{p_0} and y_{1-p_0}, respectively.
- Estimate the lower bound of the treatment effect by taking the difference in means between the treated and between a trimmed control group where all observations above y_{1-p_0} are excluded: $\Delta_0^{LB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \geq y_{1-p_0}]$.
- Estimate the upper bound of the treatment effect by taking the difference in means between the treated and between a trimmed control group where all observations below y_{p_0} are excluded: $\Delta_0^{UB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \leq y_{p_0}]$.
- Using the sample analogues of $p_0, \Delta_0^{LB}, \Delta_0^{UB}$, one can construct sharp bounds for the average treatment effect for units with $S_1 = 0, S_0 = 1$ (i.e., those that will be selected irrespective of treatment assignment): $[\Delta_0^{LB}, \Delta_0^{UB}]$.

C.6.2 Adaptation of the Lee (2009) bounding procedure to the diff-in-disc setting

Lee makes it clear that his procedure can be applied to non-experimental settings (Lee, 2009, 1073). In this case, the quantity of interest is not a difference in means but a difference in discontinuities, where treatment is determined at a discontinuity, and I am comparing how treatment affects one group relative to another. To account for these complications, I adapt the Lee bounding procedure as follows to produce bounds for $\hat{\tau}_{ddisc}$

- I first simplify the design to a localized experiment, immediately around the threshold, and therefore based on an assumption of local randomization instead of one of continuity.[4] I focus exclusively on schools immediately around the cutoff – I use the 0.015 bandwidth but results are similar using 0.01 or 0.02. Within this small bandwidth the density of the the forcing variable is flat, as required when invoking the local randomization assumption.[5]

[4]Others have also used this strategy for adapting Lee (2009) bounds to a regression discontinuity setting, e.g. Depew and Eren (2016). On the local-randomization-based RDD, see Cattaneo et al. (2020).

[5]Replicating the diff-in-disc as a localized diff-in-diff within this narrow bandwidth renders similar results to those presented in Table 2. Results are available from the author.

- Then I build four trimmed datasets: two trimmed datasets for upper and lower bound for appointed directors, and two trimmed datasets for unappointed directors. This is because the rates of director turnover (S) are very different for both types of directors, as shown in Appendix C.3.
- To estimate the lower bound, I join the data for the group with no mayor turnover to the two trimmed datasets for lower bounds (one for appointed directors and one for not appointed directors). Then I regress the change in IDEB scores on an indicator for mayor turnover and its interaction with an indicator of the director being politically appointed.

$$Y_{sm} = \alpha + \beta_1 P_m + A_{sm}(\gamma_1 + \gamma_2 P_m) + \varepsilon_{sm} \tag{2}$$

- I do the same with the trimmed datasets for the upper bound.
- The $\hat{\gamma_2}$ of each of the two regressions gives me the bounds for $\hat{\tau}_{ddisc}$.

Using this procedure, I obtain the bounds [-0.473, -0.211].

C.6.3 Inference

To make inference about the bounds, I use the bootstrap. For each of 50,000 replications:

- I first draw, with replacement, a sample of appointed directors (with or without attrition) within the narrow bandwidth.[6] With that data, I calculate p_0^a.
- I then draw a sample with replacement from the set of schools that did not experience director turnover, within the narrow bandwidth, and that had appointed directors. I trim the set of schools without mayor turnover according to the $\hat{p}_0^a$ estimated before, applying the $\hat{p}_0^a$ and $1 - \hat{p}_0^a$ quantiles to the distribution of $Y|D = 0, S = 1$ within this sample. With that data, I build a trimmed sample of appointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
- I replicate steps 1-3 for unappointed directors, estimating $p_0^{\neg a}$ and creating a trimmed sample of unappointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.

---

[6]The following steps take into account whether this sample has more director turnover in the treatment or in the control group, adjusting accordingly as explained in Lee (2009). For brevity below I describe the steps I take when the bootstrapped sample has more attrition in the mayor-turnover group, which is by far the most common scenario.

- I merge the datasets for appointed and unappointed directors, creating datasets for a lower and an upper bound.
- I estimate Equation 2 with each of the two datasets to estimate the difference in the treatment effect for appointed and unappointed directors. I store the two values of $\hat{\gamma_2}$ from each of the two regressions into corresponding vectors.

As a result of this bootstrapping exercise, I obtain two distributions, one of lower bounds and one of upper bounds. I then estimate the standard deviation of those distributions, and use it to build a confidence interval for the bounds following Imbens and Manski (2004) as suggested by Lee (2009):

$$\left[\hat{\Delta}^{LB} - \bar{C}_n \times \frac{\hat{\sigma}_{LB}}{\sqrt{n}}, \hat{\Delta}^{UB} + \bar{C}_n \times \frac{\hat{\sigma}_{UB}}{\sqrt{n}}\right] \tag{3}$$

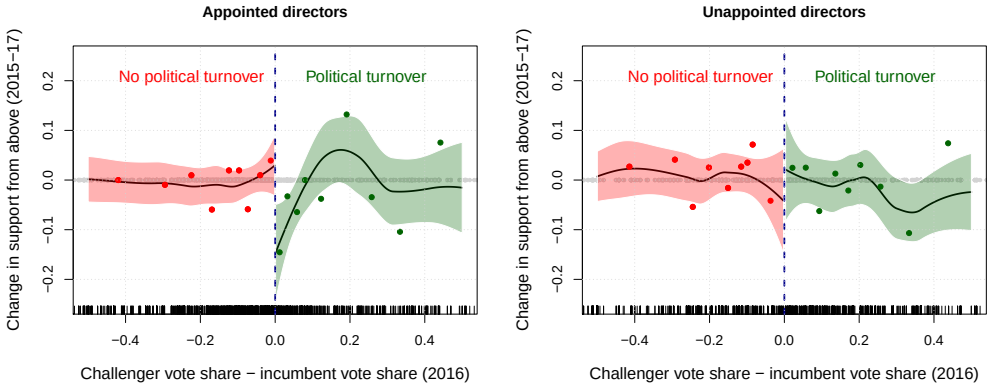
The value of $\bar{C}_n$ is chosen such that it satisfies:

$$\Phi\left(\bar{C}_n + \sqrt{n}\frac{\hat{\Delta}^{LB} - \hat{\Delta}^{UB}}{max(\hat{\sigma}_{UB}, \hat{\sigma}_{LB})}\right) - \Phi(-\bar{C}_n) = 1 - \alpha \tag{4}$$

Following this procedure, I obtain a 95% confidence interval of [-0.51, -0.15].

C.7 Mechanisms, as measured through the government's survey of school directors

Figure A-17: Effect of political turnover on school directors' response to the government survey question on whether their work is supported by higher instances, by director appointment mode



Colored dots are local averages for equally-sized bins. Lines are loess regression lines estimated at both sides of the threshold with no controls. Shaded regions denote 95% confidence intervals.

To explore mechanisms I replicate the diff-in-disc using, as the dependent variable for Equation 5, changes in director responses in the ANRESC official survey. In particular, I leverage items in a

module called *Views about school problems and obstacles to management.* First, I leverage their answers on a question asking them whether their work is supported by higher instances. That's a question directly aligned with my theory of upward embeddedness, and one where I find significant effects (Table 3 and Figure A-17).

Next, I leverage director answers on questions asking them about the extent to which the school's functioning was hindered by a number of problems, including insufficient financial resources, insufficient teachers, or teacher turnover. These questions, which address potential alternative mechanisms through which political turnover could hurt the effectiveness of appointed directors, are measured on a 4-point scale (from "no" to "yes, a lot"). The diff-in-disc returns statistically insignificant results for all of them, as shown below.

Table A-11: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with teacher turnover

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.102	0.163	0.199	0.194
	(0.101)	(0.155)	(0.141)	(0.140)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.113	-0.103	-0.083
		(0.217)	(0.212)	(0.217)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.211	0.212	0.212	0.212
N	1812	1807	1807	1749

See notes under Table 2.

Table A-12: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient teachers

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.128	-0.059	0.023	0.046
	(0.137)	(0.231)	(0.217)	(0.228)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.104	-0.152	-0.312
		(0.250)	(0.273)	(0.289)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.114	0.130	0.130	0.130
N	1325	1321	1321	1274

See notes under Table 2.

Table A-13: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient financial resources

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	0.158	-0.065	-0.163	-0.100
	(0.150)	(0.252)	(0.229)	(0.233)
$\hat{\gamma}_2$: Political turnover × Appointed		0.433	0.484	0.424
		(0.301)	(0.271)	(0.290)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.169	0.221	0.221	0.221
N	1847	1842	1842	1783

See notes under Table 2.

C.8 Results when interacting covariates with treatment

Table A-14: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.161*	-0.000	-0.007	-0.134
	(0.069)	(0.097)	(0.095)	(0.766)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.283*	-0.316*	-0.346**
		(0.128)	(0.124)	(0.129)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.204	0.206	0.206	0.206
N	1628	1623	1623	1569

See notes under Table 2.

Table A-15: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting their work is supported by higher instances, for appointed versus unappointed directors

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.059	0.089	0.070	-0.665
	(0.049)	(0.083)	(0.080)	(0.478)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.239*	-0.229*	-0.259*
		(0.110)	(0.110)	(0.107)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.178	0.177	0.177	0.177
N	1587	1583	1583	1528

See notes under Table 2.

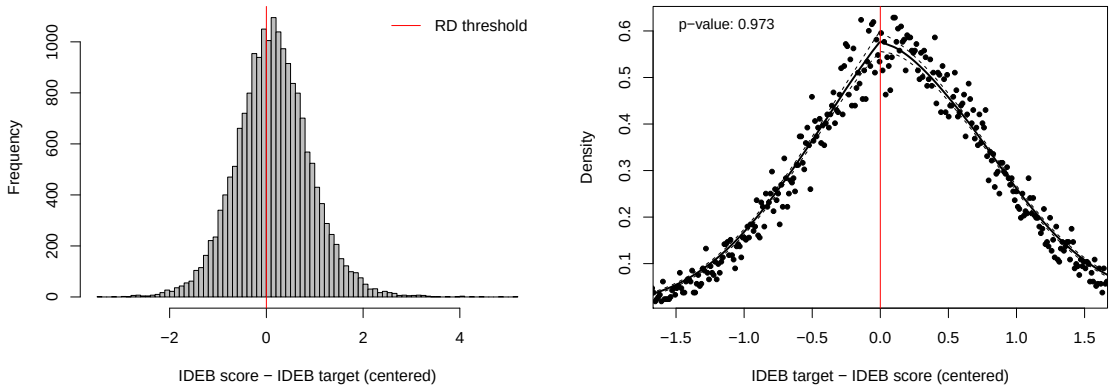
D Additional details of the regression discontinuity

D.1 Continuity of pre-treatment covariates and the forcing variable

Table A-16: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 8 with pre-treatment covariates as the dependent variable

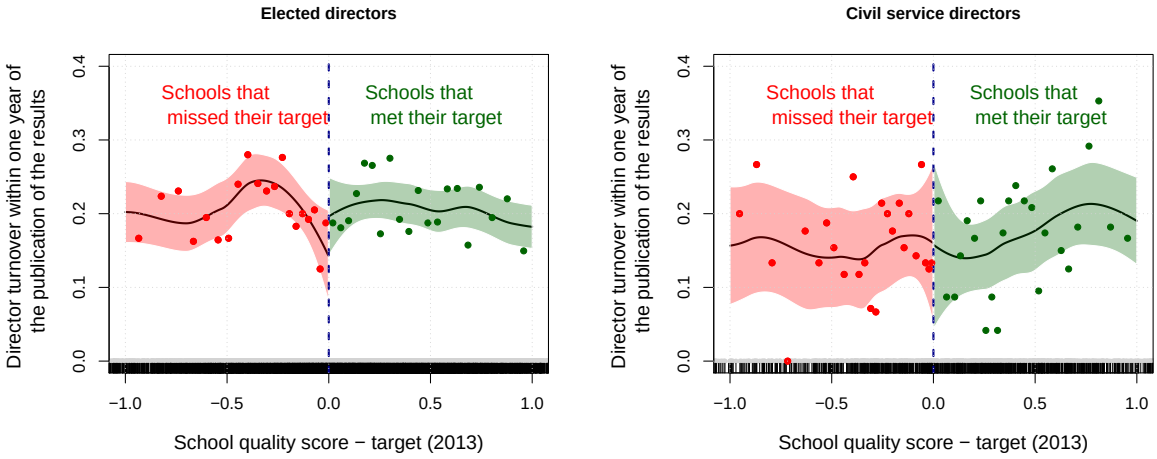
	RD estimate	Standard error	p value
Director is appointed	0.019	0.021	0.369
Director is elected	-0.001	0.019	0.956
Director is civil service	-0.027	0.012	0.023
Director is female	0.008	0.013	0.556
Director is aged <40	-0.002	0.018	0.913
Director is aged 40-49	0.029	0.019	0.130
Director is aged 50+	-0.026	0.019	0.157
Director is white	-0.012	0.022	0.586
Director is black/brown	-0.003	0.022	0.875
Director has other race	0.014	0.007	0.057
Director has < tertiary education	-0.001	0.009	0.912
Director has a tertiary degree	0.001	0.009	0.912
Director has a postgraduate degree	0.008	0.019	0.647
Director has no other job	-0.002	0.019	0.909
Director has <6 years of teaching experience	-0.020	0.015	0.189
Director has 6-15 years of teaching experience	-0.004	0.019	0.853
Director has >15 years of teaching experience	0.028	0.020	0.159
Director has <3 years of director experience	0.005	0.019	0.798
Director has 3-10 years of director experience	0.044	0.021	0.038
Director has >10 years of director experience	-0.048	0.015	0.002
Director has held position for <3 years	0.019	0.018	0.298
Director has held position for 3-10 years	0.008	0.019	0.676
Director has held position for >10 years	-0.034	0.011	0.002
Municipality GDP per capita (log)	-0.009	0.031	0.766
Municipality population (log)	-0.090	0.072	0.209
Municipality deaths per 1,000	0.029	0.060	0.626
Mayor is in their first term	0.019	0.017	0.264
Municipal electoral concentration	0.001	0.005	0.863
Mayor belongs to a large, programmatic party	-0.003	0.018	0.881
School is rural	0.017	0.015	0.276
Number of staff in the school	-1.247	0.844	0.140
School is in a settlement	0.006	0.004	0.126
School is in indigenous land	0.002	0.002	0.315
School is in quilombola land	0.002	0.003	0.503
Students per classroom (average)	-0.252	0.175	0.148
School socioeconomic index	-0.307	0.239	0.199
School IDEB target for 2013	-0.001	0.041	0.977
School IDEB score in 2011	-0.017	0.045	0.711
School ANRESC test scores 2011	-0.018	0.036	0.616
School student passing rate 2011	0.002	0.004	0.643

Figure A-18: Continuity of the forcing variable: Histogram and McCrary density test



D.2 Treatment heterogeneity among unappointed directors

Figure A-19: Effect of meeting the performance target on director performance, for schools whose director was unappointed



See notes under Figure 2.

Table A-17: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was elected

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033	-0.066**	-0.068**	-0.074***
	(0.018)	(0.021)	(0.021)	(0.022)
$\hat{\gamma}_2$: Quality target met × Elected		0.104**	0.105**	0.103**
		(0.039)	(0.038)	(0.040)
$\hat{\beta}_1 + \hat{\gamma}_2$		0.038	0.038	0.029
		(0.032)	(0.032)	(0.033)
State fixed effects			✓	✓
Predictors of Elected				✓
Bandwidth	0.518	0.520	0.520	0.520
N	8503	8432	8432	7603

See notes under Table 4.

Table A-18: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was civil service

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033*	-0.035*	-0.036*	-0.041**
	(0.014)	(0.015)	(0.015)	(0.016)
$\hat{\gamma}_2$: Quality target met × Civil service		0.006	0.008	0.005
		(0.055)	(0.055)	(0.059)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.029	-0.028	-0.036
		(0.063)	(0.063)	(0.069)
State fixed effects			✓	✓
Predictors of Civil service				✓
Bandwidth	0.518	0.517	0.517	0.517
N	8473	8402	8402	7574

See notes under Table 4.

D.3 Details on director elections that help explain the null result for elected directors

The results of the RDD presented in Table 4 show that while appointed directors are held by politicians for their performance in IDEB, but that elected (and civil service) ones are not. The fact that voters (teachers and parents, mostly) are not holding directors accountable is remarkable, given their stakes in the quality of the school, their relatively high levels of information, and their ability to take action through voting and coordination among relatively small groups.

My interviews in the education sector provide some insights as to why this may be the case. The election of school directors – which is in practice the most common alternative in this setting – establishes even more complex accountability relationships. Director elections are regulated by municipal laws, but generally they provide for the electoral participation of teachers, other school staff, and parents (or students, in high schools), sometimes with larger weights for teachers' votes. Interviews provided evidence of why director elections fail to boost accountability and performance. Elections for school director are often uncompetitive – several school directors reported having been elected with vote shares above 95%. My survey of school directors provides some quantitative data on school director elections, representative for the urban areas of all but the largest municipalities in Rio Grande do Norte. In this setting, elected directors reported a median level of support of 90% of the votes in the last election. More than 70% of directors report having run unopposed.[7]

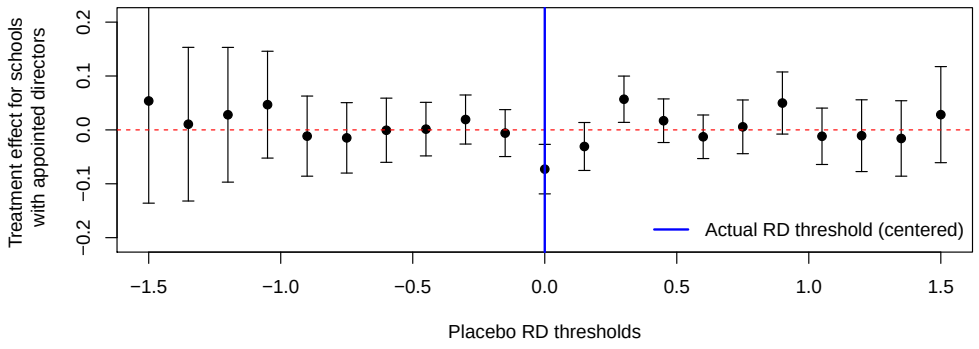
[7]The uncompetitive nature of director elections is not unique to this state. In the municipal school director elections held in 2015 in Vitória da Conquista (the third largest city in Bahia), the average vote share of the winner for schools were valid elections were held was 95.96%. Over a third of the schools had no candidates. The results for the urban, municipal school director elections held in 2013 in Santarém, the third largest city in Pará, had winners with an average vote share of 81.95%.

Oftentimes schools have no candidates, and in those cases the director is normally directly appointed by the mayor. When the election does happen, it is easily prone to capture. A director said that "in community consultations [elections] it is very easy to get the support of the community – your supporters show up to vote, the rest does not show up."[8] In practice, the results of the election are usually determined by teachers, especially tenured ones. These dynamics of capture are strengthened by the erosion of the democratic norm once elections are established – interviewees often reported a significant drop in community interest and participation in director elections after the first wave. In the words of a secretary, "first there was a democratic response – the first election was genuine, with interest, but the second one had just the very same candidates, and after that it just became a mere [formal] commitment, with the same people. After four years when candidates reached their re-election limit no one ran and the mayor had to appoint somebody."[9]

D.4 Placebo tests varying the RD threshold

Only one of the 20 placebo tests returns a statistically significant result, which is roughly what we would expect with $\alpha = 0.05$.

Figure A-20: Placebo tests for model 2 in Table 4, moving the RD threshold



D.5 Alternative sample: Municipalities with mayors from programmatic parties

This appendix replicates the diff-in-disc subsetting to municipalities where the mayor belongs to a large, programmatic party (PT or PSDB). Programmatic parties are those having identifiable platforms, and are generally thought as less likely to rely on clientelism and more likely to strengthen bureaucratic accountability (Cruz and Keefer, 2015). Until recently Brazil had two large programmatic

[8] School director interviewed in Rio de Janeiro in February 2017.

[9] Secretary of education interviewed in Rio de Janeiro in February 2017.

parties, PT and PSDB, although their programmatic profile has eroded over time (Samuels and Zucco, 2018).

Table A-19: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was appointed, among municipalities with a programmatic party in office

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.046	0.027	0.031	0.031
	(0.029)	(0.043)	(0.043)	(0.045)
$\hat{\gamma}_2$: Quality target met × Appointed		-0.134*	-0.152**	-0.137*
		(0.058)	(0.058)	(0.060)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.107**	-0.121**	-0.106**
		(0.038)	(0.038)	(0.039)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.652	0.666	0.666	0.666
N	2935	2911	2911	2656

See notes under Table 4.

E Additional details of the face-to-face survey of bureaucrats

The survey instrument (in English and Portuguese) is available from the author, as are descriptive statistics about municipalities in Rio Grande do Norte.

E.1 Details on sampling and non-response

I excluded the largest 17 municipalities in the state (which had as of the 2010 census more than 30,000 inhabitants) for budget and security reasons. Surveying street-level managers in these large municipalities would significantly increase the cost oft he survey, and more importantly it would have exposed enumerators to the serious security challenges typical of large urban areas of the Northeast. Rio Grande do Norte is consistently ranked among the most dangerous states in Brazil.

Rural areas in all municipalities were excluded from the study's population, for three main reasons. First, rural schools, clinics, and social assistance centers in Brazil are often staffed for a limited number of days and hours per week. Second, the managers of these units often work at the municipality's urban center, and tend to direct several units at once. Third, rural areas in the Northeast are logistically hard to reach – they are often accessible only through dirt roads with limited or no GPS service, unmapped on GPS services – and pose additional security challenges. Therefore, including rural areas in the sample would have heavily increased the time and budget

required for the survey, and would have risen security issues for enumerators. While there are many schools and clinics in rural areas, most of the population lives in urban areas.

Before the survey, and using the most up-to-date administrative data, I identified 1,027 schools, clinics, and social assistance centers in the urban areas of the target 150 municipalities. Throughout four weeks of fieldwork, we managed to interview 926 street-level managers. The gap between the two numbers is due to rejections (17 managers refused to participate), overlaps (15 units had as manager somebody who had already been surveyed), misclassification (25 units were mis-identified as urban, when in fact they were in rural areas), and failures to locate some managers (we tried at least twice with each of them). On the other hand, we located and did surveys at 38 urban units that, mostly because they were of recent establishment, were not in the administrative data.

Survey participants were recruited in their offices, and consent was collected (and recorded on the tablets we used) after providing information about the research project and their rights as participants. Participants were not compensated in any form.

E.2 Descriptive statistics

Table A-20: Descriptive statistics of the survey of street-level managers, by sector

	All sectors N=926		Education N=481		Healthcare N=292		Social assistance N=153	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	40.71	9.76	45.20	8.34	35.27	8.37	36.99	9.57
Female	0.86	0.34	0.85	0.36	0.86	0.35	0.92	0.27
High school degree or less	0.08	0.27	0.01	0.11	0.22	0.41	0.03	0.16
College degree	0.31	0.46	0.25	0.43	0.29	0.46	0.51	0.50
Politically appointed	0.77	0.42	0.79	0.41	0.67	0.47	0.87	0.34
Elected	0.09	0.28	0.17	0.38	0.00	0.00	0.00	0.00
Civil service	0.04	0.19	0.01	0.09	0.09	0.28	0.03	0.16
Experience as a manger	4.66	4.33	5.58	4.79	3.98	3.71	3.05	2.97
Experience as a professional	15.15	10.61	20.89	9.00	8.77	7.17	8.61	10.23
Exclusive dedication	0.57	0.50	0.80	0.40	0.00	0.00	0.92	0.28
Union member	0.35	0.48	0.54	0.50	0.17	0.38	0.10	0.31
Party member	0.16	0.37	0.16	0.37	0.15	0.36	0.18	0.38
Worked for a campaign	0.40	0.49	0.42	0.49	0.34	0.47	0.47	0.50

E.3 Results of observational regressions of appointment modes

Table A-21: Correlates of street-level managers' appointment mode

	Appointed	Elected	Civil service
Party member	−0.008 (0.035)	0.007 (0.026)	0.000 (0.013)
Has worked for an electoral campaign	0.117 (0.027)***	−0.054 (0.019)**	−0.033 (0.010)***
Union member	−0.174 (0.033)***	0.080 (0.024)**	0.070 (0.020)***
Experience as manager	−0.006 (0.004)	−0.005 (0.003)	0.007 (0.002)***
Experience as professional	−0.000 (0.002)	0.001 (0.001)	−0.000 (0.000)
Lives in the municipality	0.156 (0.047)***	−0.018 (0.023)	−0.091 (0.031)**
Has no other jobs	−0.046 (0.045)	−0.027 (0.041)	0.026 (0.013)*
Female	−0.007 (0.040)	−0.009 (0.029)	0.014 (0.014)
Age	0.005 (0.002)*	0.001 (0.001)	−0.003 (0.001)***
Has more than a college degree	−0.139 (0.029)***	0.059 (0.017)***	0.014 (0.013)
Has less than a college degree	0.096 (0.044)*	0.045 (0.022)*	−0.003 (0.021)
Healthcare sector (vs education)	−0.203 (0.054)***	−0.157 (0.042)***	0.087 (0.022)***
Social assistance (vs education)	0.001 (0.037)	−0.112 (0.020)***	0.035 (0.017)*
Constant	0.653 (0.093)***	0.130 (0.066)*	0.116 (0.045)*
Observations	883	883	883
R-squared	0.163	0.135	0.126

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. HC1 standard errors in brackets.

E.4 Results of observational regressions of meetings and attitudes

Table A-22: Observational regressions of self-reported number of meetings with stakeholders on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

	Dependent variable: Self-reported, logged number of meetings with					
	Mayor	Secretary	Technicians	City councilors	Professionals	Clients
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.391***	1.013***	0.541***	0.037	0.201	0.333**
	(0.101)	(0.155)	(0.145)	(0.079)	(0.155)	(0.128)
Elected	0.020	0.801***	0.399*	0.015	0.069	0.278*
	(0.117)	(0.175)	(0.170)	(0.089)	(0.170)	(0.137)
Observations	786	775	786	786	786	786
R-squared	0.096	0.115	0.090	0.025	0.063	0.167

HC1 standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001. All regressions control for respondents' sector, age, gender, party membership, union membership, whether they have worked for a local electoral campaign, years of experience as manager, experience as professional, whether they live in the municipality, whether they have other jobs, and whether they have more than a college degree.

Table A-23: Observational regressions of self-reported number of meetings with stakeholders on street-level managers' appointment mode (versus elected), as per Equation 10

	Dependent variable: Self-reported, logged number of meetings with					
	Mayor	Secretary	Technicians	City councilors	Professionals	Clients
height	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.374***	0.216*	0.142	0.023	0.129	0.058
	(0.071)	(0.097)	(0.104)	(0.045)	(0.072)	(0.062)
Observations	754	743	754	754	754	754
R-squared	0.082	0.080	0.077	0.025	0.062	0.167

See notes under Table A-22.

Table A-24: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

	Dependent variable: Agreement with (1-4 scale)					
	Trust mayor	Feel close to mayor	Mayor & professionals aligned	Mayor is concerned w/ quality	Trust secretary	Feel close to secretary
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	1.187***	1.230***	0.794***	0.790***	0.602***	0.844***
	(0.169)	(0.182)	(0.175)	(0.165)	(0.154)	(0.172)
Elected	0.621**	0.644**	0.430*	0.378*	0.343	0.610**
	(0.195)	(0.215)	(0.199)	(0.188)	(0.177)	(0.191)
Observations	785	781	786	785	774	774
R-squared	0.247	0.193	0.145	0.172	0.114	0.138

See notes under Table A-22.

Table A-25: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

	Dependent variable: Agreement with (1-4 scale)					
	Trust mayor	Feel close to mayor	Mayor & professionals aligned	Mayor is concerned w/ quality	Trust secretary	Feel close to secretary
height	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.571***	0.591***	0.372***	0.419***	0.259**	0.237**
	(0.102)	(0.120)	(0.101)	(0.095)	(0.094)	(0.090)
Observations	753	749	754	753	742	742
R-squared	0.143	0.100	0.086	0.109	0.070	0.059

See notes under Table A-22.

E.5 Details of the conjoint experiment with bureaucrats

Table A-26: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment

Attribute	Values
Education	Bachelors degree / Masters degree
Experience	3 years / 10 years
Political connections	Has / lacks connections with the municipal government
Relationship to professionals	Good / bad relationship to professionals
Unit performance	Targets were met / not met
Selection mode	Civil service exam / election by the community / political appointment

The next table details the regression results visualized in Figure 7. These correspond to the following choice tasks of the conjoint experiment: (i) *Communication:* Which of these [directors / managers / coordinators] do you think would have a better communication with the Secretariat of [education / healthcare / social assistance]?; (ii) *Implementation:* Which of these [directors / managers / coordinators] do you think would have more chances of implementing changes requested by the mayor's office?; (iii) *Resources:* Which of these [directors/managers / coordinators] do you think would obtain a reform for the [school / clinic / social assistance center]?; (iv) *Results:* Which of these [directors/managers/coordinators] do you think would achieve better scores in [student learning/community healthcare/social assistance center indicators]?

Table A-27: Results of the conjoint experiment with street-level managers

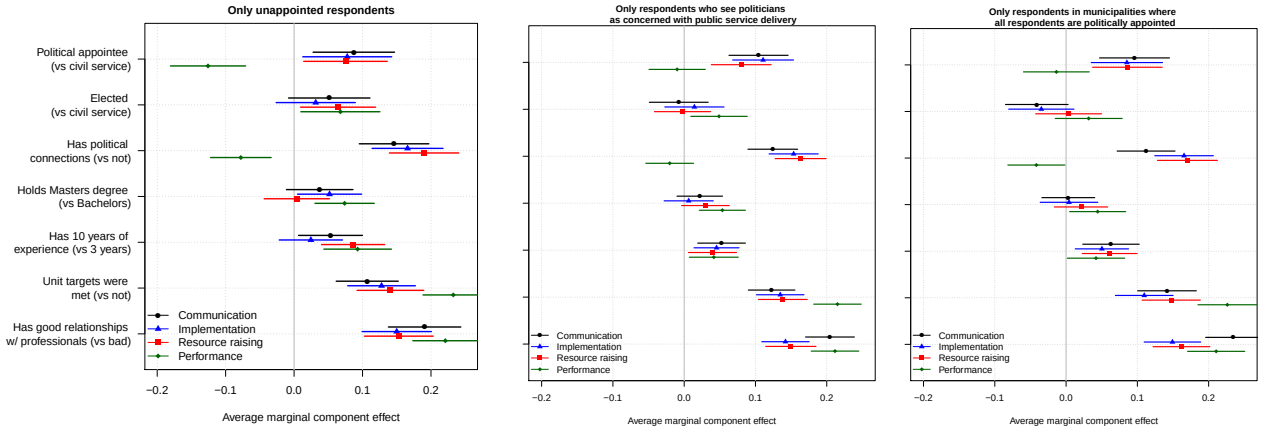
	Communication	Implementation	Resources	Performance
Appointment: Political (vs civil service)	0.1*** (0.015)	0.097*** (0.015)	0.083*** (0.015)	-0.056*** (0.014)
Appointment: Election (vs civil service)	0.008 (0.014)	0.019 (0.014)	0.029* (0.014)	0.059*** (0.014)
Political connections: Yes (vs no)	0.137*** (0.012)	0.16*** (0.012)	0.174*** (0.012)	-0.042*** (0.011)
Education: Masters (vs Bachelors)	0.019 (0.012)	0.014 (0.012)	0.016 (0.012)	0.059*** (0.011)
Experience: 10 years (vs 3 years)	0.055*** (0.012)	0.038*** (0.011)	0.057*** (0.012)	0.06*** (0.012)
Unit performance: Targets were met (vs not met)	0.126*** (0.012)	0.131*** (0.012)	0.134*** (0.012)	0.22*** (0.012)
Relationship to professionals: Good (vs bad)	0.184*** (0.012)	0.148*** (0.012)	0.15*** (0.012)	0.214*** (0.012)
Number of respondents	917	917	916	917
Number of valid profiles	7224	7224	7222	7224

*p<0.05; **p<0.01; ***p<0.001. Standard errors clustered at the respondent level.

E.6 Conjoint results among subsets of respondents

The plot on the left includes only responses from bureaucrats who are not political appointees. The plot on the center includes only responses from bureaucrats who expressed the highest level of agreement with the following statements: "The mayor and [education / healthcare / social assistance] professionals have the same priorities for [units, i.e., schools / clinics / social assistance centers]"; "The mayor is concerned with improving the quality of [units]"; "The secretariat of [education / healthcare / social assistance] helps us improve the performance of the [unit]"; and "The secretariat of [area] holds this [unit] accountable for its results." The plot on the center includes only responses from municipalities where all respondents are politically appointed (∼42% of the localities where we did surveys).

Figure A-21: Conjoint results among subsets of respondents



See notes under Figure 7.

F Additional details of the online survey of politicians

The survey instrument (in English and Portuguese) is available from the author.

F.1 Respondent recruitment and non-response

Table A-28: Correlates of the number of responses per municipality

	Respondents (log)	No respondents (dummy)	Respondents (log) w/o zeroes
Population (logged)	0.032 (0.052)	−0.008 (0.032)	0.027 (0.037)
GDP per capita (logged)	−0.181 (0.120)	0.118 (0.085)	−0.039 (0.104)
Deaths per thousand	0.040 (0.032)	−0.027 (0.019)	0.004 (0.026)
Mayor was reelected in 2016	0.214 (0.102)*	−0.137 (0.046)**	0.033 (0.087)
Constant	2.213 (1.123)	−0.652 (0.762)	1.416 (0.904)
R-squared	0.046	0.063	0.004
Observations	167	167	142

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$. HC1 standard errors in brackets.

The state audit court of Rio Grande do Norte sent the survey to all mayors and to secretaries of five key areas (education, healthcare, social assistance, finance, and human resources) in the 167 municipalities of the state through its online platform.[10] Participation was voluntary. A total of 455 politicians participated and finished the survey, of which 50 were mayors and 405 secretaries. These respondents come from 142 municipalities. Municipalities where mayors were in their second

[10]The survey was also sent to city councilors, but their responses are excluded here because the theory in this paper focuses on executive politicians. Including city councilors' responses, however, does not alter the results.

term were more likely to participate, but conditional on some politicians responding there are no significant correlations between the number of respondents and basic political and socioeconomic characteristics of the municipality, as shown in Table A-28. Participants were recruited through the court's online platform, where they received information about the research project and their rights as participants. Participants were not compensated in any form.

F.2 Descriptive statistics

Table A-29: Descriptive statistics for the survey of politicians, by position

	All (N=455)		Mayors (N=50)		Secretaries (N=405)	
	Mean	SD	Mean	SD	Mean	SD
Age	42.62	10.61	48.68	11.09	41.87	10.32
Female	0.57	0.50	0.22	0.42	0.61	0.49
High school degree or less	0.10	0.30	0.32	0.47	0.07	0.26
College degree or more	0.79	0.41	0.58	0.50	0.81	0.39
Party member	0.52	0.50	0.98	0.14	0.46	0.50
Experience as bureaucrat (years)	0.72	0.45	0.38	0.49	0.76	0.43
Experience as politician (years)	4.61	4.88	7.26	6.90	4.28	4.47

F.3 Details of the conjoint experiment with politicians

Table A-30: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment with politicians

Attribute	Values
Education	Bachelors degree / Masters degree
Experience	3 years / 10 years
Political connections	Has / lacks connections to the municipal government
Union membership	Participates / does not participate in a union
Gender	Woman / Man
Contract type	Civil service contract / Temporary contract

Table A-31 details the regression results visualized in Figure 8. These correspond to the following choice tasks of the conjoint experiment: (i) Which of these bureaucrats do you think would have a better communication with the local government?; (ii) Which of these bureaucrats do you think would have more chances of implementing changes requested by the local government?; (iii) Which of these bureaucrats do you think would work extra hours if necessary?; and (iv) Which of these bureaucrats do you think would achieve better performance?

Table A-31: Results of the conjoint experiment with politicians

	Communication	Implementation	Effort	Performance
Contract: Temporary (vs civil service)	0.102*** (0.019)	0.132*** (0.019)	0.217*** (0.017)	0.066*** (0.018)
Political connections: Yes (vs no)	0.121*** (0.019)	0.058** (0.018)	0.058** (0.018)	-0.005 (0.018)
Education: Masters (vs Bachelors)	0.018 (0.017)	0.015 (0.018)	-0.022 (0.017)	0.102*** (0.018)
Experience: 10 years (vs 3 years)	0.03 (0.018)	-0.021 (0.019)	-0.019 (0.018)	0.068*** (0.018)
Union membership: Yes (vs no)	-0.039* (0.017)	-0.024 (0.018)	-0.061*** (0.017)	-0.011 (0.018)
Gender: Male (vs female)	-0.055** (0.017)	-0.082*** (0.016)	-0.063*** (0.016)	-0.106*** (0.017)
Number of respondents	455	455	455	455
Number of valid profiles	3640	3640	3640	3640

*p<0.05; **p<0.01; ***p<0.001. Standard errors clustered at the respondent level.

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