The benefits of patronage: How political appointments can enhance bureaucratic accountability and effectiveness*

Guillermo Toral†

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Abstract

The political appointment of bureaucrats is typically seen as a rent-seeking strategy whereby politicians sustain clientelistic networks and manipulate public administration to their advantage. I argue that political appointments can also increase bureaucratic accountability and effectiveness in public service delivery because they provide political and social connections between bureaucrats and politicians. These connections provide access to material and immaterial resources, enhance monitoring, facilitate the application of sanctions and rewards, align priorities and incentives, and increase mutual trust. In certain conditions, political connections can thus enhance bureaucrats' responsiveness and the delivery of services. I test this theory with data on Brazilian municipal governments, leveraging two quasi-experiments, two original surveys of bureaucrats and politicians, and in-depth interviews. The findings challenge the traditional view of patronage as universally detrimental for development, and draw attention to how political appointments and connections can be leveraged for public service delivery.

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†Assistant Professor of Political Science, Vanderbilt University: guillermo.toral@vanderbilt.edu.

1 Introduction

The political appointment of bureaucrats –or patronage, in short–[1] is ubiquitous throughout the developing world (Grindle, 2012). Patronage is typically understood as rent-seeking: a strategy whereby politicians build and maintain clientelistic networks and steer bureaucratic efforts for political and/or private gain, which hurts development. In contrast to this view, I propose a theory of how and when political appointments and connections can enhance bureaucratic accountability and effectiveness.[2] I advance a view of patronage as providing bureaucrats with "upward embeddedness", i.e., social and political connections with politicians, which facilitate bureaucratic accountability and effectiveness. In particular, I argue that patronage gives bureaucrats access to material and immaterial resources, facilitates monitoring by politicians, enables the application of sanctions and rewards, aligns priorities and incentives, and increases mutual trust. In certain contexts, patronage can improve public service delivery and citizen welfare by making bureaucrats more accountable and effective.

My argument is not that patronage is universally good, or that it comes with no costs. The governance advantages of patronage can be mobilized for rent extraction, public service delivery, or both. The costs of patronage have long been recognized (Pollock, 1937), and recent studies have shown how patronage can distort the allocation of public jobs and disincentivize bureaucratic performance (Xu, 2018; Callen et al., 2018; Colonnelli et al., 2020). This paper goes beyond these long-noted costs of patronage for bureaucratic selection and effort by suggesting an additional set of mechanisms through which patronage can enhance rent extraction, related less to *who* is selected into the bureaucracy and *how much* they work, and more to *how* they work. The governance advantages of patronage, however, can be leveraged not only to extract rents, but also to deliver better public services. In this paper I focus on these often overlooked benefits of patronage, and offer a theory of the conditions under which they are most likely to outweigh the costs.

The net benefits of patronage are more likely to be positive in contexts where there are no easy

[1] I use *patronage* to refer to the political appointment of bureaucrats or, more specifically, the discretionary appointment of bureaucrats by politicians based, at least partly, on political criteria or other factors deviating from merit. The concept of patronage is contested, and a variety of definitions exist in the literature, both narrower and broader than the one I use.

[2] I use bureaucratic *accountability* to refer to bureaucrats' responsiveness to the demands of their principals (politicians, senior officials, and upper-level bureaucrats), and their career paths being affected by it. I use bureaucratic *effectiveness* to refer to bureaucrats' success at delivering public services and improving policy outcomes within their area of competency.

substitutes for the governance advantages it provides. This is often the case in developing contexts, and in particular in poor and small localities, which typically have dire financial constraints, small labor markets, and limited human capital. In these settings, the competitive, merit-based recruitment of bureaucrats is less likely to be sufficient for selecting and motivating effective bureaucrats. The benefits of patronage are especially important among "street-level managers" (Gassner and Gofen, 2018), namely bureaucrats like school directors[3] or health clinic mangers who lead public service delivery units throughout the territory, occupying a critical position in between senior officials and front-line providers. The effectiveness of these managers depends on their ability to motivate and coordinate street-level employees, and to align their work with both managerial and citizen demands. These are two tasks that political appointments and connections facilitate. A last but critical scope condition for the governance gains of patronage to be net positive is that politicians value at least partly the delivery of public services. This is more likely to be the case in contexts with electoral accountability and strong oversight institutions.

The argument that patronage is beneficial for service delivery builds on insights from political science, public administration, and economics. Previous research in comparative politics has acknowledged the ambivalence of patronage and recognized its beneficial uses for party building (Sorauf, 1960; Huntington, 1968), nation building (Weingrod, 1968), interest aggregation (Scott, 1969), and state building (Grindle, 2012). In public administration the politicization of the bureaucracy in high-income countries is often seen as a resource politicians use to improve their control over policy and implementation (Peters and Pierre, 2004; Bach and Veit, 2017) and to build party networks (Kopecky et al., 2012). In American politics, political appointments are often seen as a way for Presidents to increase policy control over federal agencies (Aberbach and Rockman, 2009; Lewis, 2008). In this tradition, a trade-off is commonly theorized between policy control and bureaucratic performance (Moe, 1985). Finally, development economists have recently engaged with the theoretical possibility that patronage may improve politicians' ability to deal with selection and agency problems, although no empirical evidence has been uncovered to support this idea (Xu, 2018; Colonnelli et al., 2020).

I build on these contributions to offer a theory that links patronage to public service delivery and to development outcomes, specifying testable mechanisms and scope conditions. Theoretically, this paper departs from economic theories of the bureaucracy that assume a loyalty-competence trade-off by emphasizing that, in low-capacity settings, the connections between bureaucrats and politicians may help not only decrease agency losses but also enhance bureaucrats' ability to do their

[3]By directors I refer to school leaders, also called principals, headmasters, or headteachers.

job. Empirically, the paper contributes with causal evidence on the benefits of patronage for service delivery in a developing context. While previous research has identified benefits in the political connections among politicians in different levels of government (Jiang, 2018; Lei, 2020), between politicians and party brokers (Nathan and Brierley, 2020), and between politicians and citizens (Tsai and Xu, 2018), this is, to my knowledge, the first paper to provide causally identified evidence of the benefits for service delivery of political connections between bureaucrats and politicians.

I combine quasi-experiments, surveys, and interviews to empirically study patronage and its effects on bureaucratic effectiveness and accountability. I focus on municipal governments in Brazil, a data-rich environment where political appointments coexist with other modes for bureaucratic selection. To test the core claims of the theory I use two quasi-experimental studies leveraging administrative data of municipal schools in the whole country. A difference-in-discontinuities (combining a difference-in-differences and a regression discontinuity) shows that when politically appointed school directors lose their connections to the local government (because of an electoral defeat of the mayor who appointed them) the school experiences a drop in quality (measured by students' academic performance), when compared to schools with unappointed directors. This demonstrates that political connections can increase bureaucratic effectiveness. A separate regression discontinuity design shows that politically appointed school directors who meet their target in a highly visible school quality metric are less likely to be replaced, whereas meeting the target has no effect on the turnover of unappointed school directors. This shows that patronage can enhance bureaucratic accountability.

To document the mechanisms through which patronage can enhance bureaucratic effectiveness and accountability I use two original surveys in one Brazilian state. First, I use a face-to-face survey of 926 street-level managers (school directors, clinic managers, and social assistance center coordinators) representative of urban areas in all but the largest municipalities of that state. Observational regressions show that appointed bureaucrats have more frequent contacts with, higher levels of trust in, and better alignment to politicians than unappointed bureaucrats. A conjoint experiment embedded in the survey also shows that managers expect those who are politically appointed or connected to communicate better with the government, to be more responsive to its demands, and to be more effective at raising funds from it. These results are corroborated by similar findings in an online survey of 524 local politicians. Politicians perceive bureaucrats with political connections as more responsive, better at communicating with them, and more likely to exert more effort.

Last but not least, I used in-depth interviews to understand the informal institutions of bureaucratic politics in Brazilian local governments, develop hypotheses, and probe mechanisms. Over 18 months of fieldwork, I conducted 121 in-depth interviews with bureaucrats, politicians, and anti-corruption actors (such as auditors and prosecutors) in 45 municipalities in 7 states across 3 different regions of Brazil.[4] Specific accounts from local actors in widely diverging contexts help understand how appointments work in practice, and what the costs and benefits of patronage are.

The finding that political appointments and connections can be beneficial for bureaucratic accountability and effectiveness has important implications for both researchers and policymakers. First, the paper suggests that reforms aimed at insulating local bureaucrats from politicians can in some contexts have detrimental effects on service delivery, at least in the short term and when not complemented by significant increases in human capital that would foster the performance of more autonomous bureaucrats. In this, the paper contributes to an emerging literature on the costs of anti-corruption strategies (Wang, 2021; Jiang et al., 2020; Rasul and Rogger, 2018). Second, the paper helps reconcile the standard view of patronage as rent-seeking with other views linking patronage to political development. Patronage can serve both rent-seeking and public service delivery projects precisely because of the governance advantages it provides. This helps explain the remarkable resilience of patronage around the world (Grindle, 2012). Finally, the paper advances our understanding of the mechanisms through which political appointments may facilitate policy control and implementation, and thus helps bridge the gap between the comparative politics research on patronage in developing contexts, on one hand, and the public administration and American politics research on political appointments in high-income countries, on the other.

2 Theory

2.1 How patronage delivers

The political appointment of bureaucrats is usually seen as a rent-seeking strategy. Scholars have long studied the critical role of government jobs in clientelistic equilibria (Wilson, 1961; Chubb, 1982). Jobs are a powerful tool for clientelism because it constitutes a targetable, credible, and reversible method for redistribution (Robinson and Verdier, 2013). Under this light, patronage is seen as hurting development through the misallocation of public jobs and the depression of bureaucratic effort (Xu, 2018; Callen et al., 2018; Colonnelli et al., 2020).

[4]See Appendix B for additional details on interviews.

I advance an alternative view of patronage that emphasizes its potential benefits for bureaucratic accountability and effectiveness. I start by proposing the concept of upward embeddedness to refer to bureaucrats' political and social connections to politicians. Scholars of bureaucratic governance often use the idea of embeddedness (Granovetter, 1985) to describe bureaucrats' relations to local communities and how they can foster government effectiveness (Evans, 1995; Tsai, 2007; Bhavnani and Lee, 2018). Applying the concept of embeddedness upward (i.e., in relation to politicians instead of societal actors) enables a more positive view of bureaucrats' political connections than existing models of bureaucratic politics allow. It also helps integrate into a single framework different types of connections within bureaucracies, including those based on partisanship (Grindle, 2012), family (Fafchamps and Labonne, 2017), and ethnicity (Vanden Eynde et al., 2018).

I argue that political appointments and connections make bureaucrats upwardly embedded, which provides a set of governance resources that can be beneficial for both bureaucrats and politicians. Depending on how these resources are used, patronage can enhance rent seeking or public service delivery. Specifically, patronage increases bureaucrats' access to material and immaterial resources, provides monitoring technology to politicians, facilitates the application of sanctions and rewards, aligns priorities and incentives, and increases mutual trust. I develop each of these mechanisms and their relevance for bureaucratic governance below.

First, upward embeddedness increases bureaucrats' access to political leaders, and through them to material resources for public service delivery, as well as immaterial resources like legitimacy and authority. Both help mobilize and coordinate other bureaucrats. Resources and legitimacy are important drivers of bureaucratic effectiveness (Dasgupta and Kapur, 2020; Carpenter, 2001).

Second, upward embeddedness facilitates the monitoring of bureaucrats by politicians and reduces information asymmetries, thanks to shared political and social networks. Upward embeddedness therefore facilitates the oversight of bureaucrats, which has been shown to improve government effectiveness in developing contexts (Gulzar and Pasquale, 2017; Raffler, 2020).

Upward embeddedness also enhances bureaucrats' accountability to politicians, by facilitating the application of sanctions and rewards – both formal and informal. This motivates bureaucrats to exert more effort, and makes them more responsive to politicians' demands. Political appointees are usually hired at will, which makes it easier to sanction bad performers (through firing) and reward good performers (through promotions). Transfers can be used for both sanctions and rewards (Iyer and Mani, 2012; Khan et al., 2019), and career incentives and extrinsic immaterial rewards can

improve bureaucratic effectiveness (Ashraf et al., 2014; Bertrand et al., 2020). Shared social and political networks also enhance the application of informal sanctions and rewards.

By virtue of actors' common political background and shared networks, upward embeddedness fosters the alignment of priorities and values between bureaucrats and politicians. Bureaucrats often operate in highly complex environments that require them to multi-task and to negotiate contradictory priorities from different societal actors (Lipsky, 1980; Dasgupta and Kapur, 2020). In such challenging environments, alignment of bureaucrats' and politicians' priorities can facilitate implementation and improve service delivery. Management scholars have long recognized the importance of alignment for organizations' performance (Biggs et al., 2014). Upward embeddedness also aligns the incentives of bureaucrats and politicians, given their shared fate. Unlike civil service bureaucrats, political appointees are usually fired after a government change, which aligns their incentives to the incumbent's (Oliveros, 2021). As the formal literature on delegation has long recognized, alignment of priorities and incentives between bureaucrats and politicians can alleviate principal-agent problems (Huber and Shipan, 2011).

Finally, due to the shared political and social networks and the alignment of priorities and incentives, upward embeddedness fosters mutual trust between bureaucrats and politicians. Abundant evidence from psychology shows that trust improves organizations' performance by lowering transaction costs and improving compliance (Kramer, 1999). Together with the alignment of priorities and incentives, trust may also decrease the need for monitoring.

To sum up, political appointments and connections foster bureaucrats' upward embeddedness, which provides a number of governance resources, namely access to material and immaterial resources, monitoring technology, better ability to apply sanctions and rewards, alignment of priorities and incentives, and increased trust. There is however an inherent ambivalence in patronage. On one hand, my theory implies that political appointments and connections make political machines more effective at extracting rents, which may help explain their resilience. On the other hand, upward embeddedness can also make governments more effective at delivering public services, because service delivery also depends on the coordination and accountability of bureaucrats. I do not claim that patronage does not have costs – it may, for example, lead to the selection of bureaucrats with less education and experience. Rather, my argument is that we have overlooked the benefits of patronage, which in certain contexts may outweigh the costs.

2.2 Scope conditions

The benefits of patronage will be larger in contexts where potential substitutes for the governance advantages it provides are not available. This is true in developing contexts, and particularly for local governments outside large metropolitan areas, where financial constraints are more dire and human capital is scarcer. This reduces governments' ability to use higher wages (Dal Bó et al., 2013) or performance pay (Hasnain et al., 2014) to improve bureaucratic selection and performance. In these challenging environments, the counterfactual to a political appointee is not necessarily the highly capable, autonomous and driven bureaucrat that Weberian theories presume. Without adequate human capital and incentives, bureaucrats may lack the capacity and motivation to deliver services. Patronage can thus alleviate developmental constraints on bureaucratic governance.

Street-level managers (e.g., school directors or clinic managers) are particularly likely to benefit from the governance advantages of patronage. These managers can have major impacts on the quality of public services (Bloom et al., 2014, 2015; Tavares, 2015), but their effectiveness depends on their ability to coordinate efforts and align a complex set of tasks to objectives that are often multidimensional and hard to assess (Lipsky, 1980), especially in transaction-intensive services like healthcare or education (Pritchett and Woolcock, 2004). To handle these challenges, street-level managers need to leverage trust, legitimacy, and the ability to coordinate efforts and align teams (Gassner and Gofen, 2018). Upward embeddedness helps overcome these challenges.[5]

For the benefits of patronage to outweigh the costs politicians must be concerned at least partly with public service delivery. A variety of reasons may make politicians value the delivery of public services, including intrinsic beliefs and norms (Habyarimana et al., 2018), electoral competition (Gottlieb and Kosec, 2019), fear of retrospective voting (Healy and Malhotra, 2013), or anti-corruption institutions (O'Donnell, 1998). The availability of regular and credible measures of bureaucratic performance can strengthen politicians' concerns with service delivery through all these mechanisms, namely norms, competition, electoral accountability, and external control.

[5]In contrast, the effectiveness of frontline providers like teachers or doctors is less dependent on their ability to coordinate efforts with other bureaucrats or with senior officials.

3 Institutional context

3.1 Formal institutions in Brazilian municipalities

Brazilian local governments are a particularly useful setting in which to study the impact of political appointments and connections on bureaucratic accountability and effectiveness, for two main reasons. First, local governments use a variety of bureaucrat appointment systems. Second, the federal government publishes multiple administrative datasets about local bureaucracies and their performance.

Brazil is a federal country with 5,570 municipalities, which are responsible for providing primary education, healthcare, and social assistance to over 200 million people. Local governments spend over 57% of their revenue (which comes mostly from inter-governmental transfers) in these three sectors (OCED, 2016). Financial constraints are usually dire and employees' salaries are low.[6] Because of municipalities' prominent role in service provision and limited opportunities in the private sector, local governments are typically a very important employer, hiring on average 4.7% of the local population and 38.2% of those who have jobs in the formal sector (Appendix A.1). From the point of view of the employer however, these are small labor markets with scarce human capital.[7] Most municipalities are small (with median population of less than 12,000 people) and far from state capitals, which makes it hard to attract talent.

Municipal elections take place every four years, and consist of simultaneous elections for a mayor (who is elected through a majoritarian system) and for a variable number of city councilors (elected though a proportional, open-list system). Mayors, who can run for re-election only once, appoint a set of non-elected secretaries who are in charge of specific policy areas. Politicians are overseen by a network of horizontal accountability institutions, including audit courts, prosecutors offices, and standard courts that have been shown to reduce rent extraction (Avis et al., 2018; Litschig and Zamboni, 2019). Federal and state governments also oversee municipal governments, especially on their use of transfers and on their performance in delivering public services that said transfers help maintain. The federal government regularly measures and publicizes the performance of municipal bureaucracies in a variety of areas, often leveraging targets and incentives to promote

[6] The median municipal employee earned a salary of 1,763 Brazilian reais in 2016 (roughly two minimum salaries or USD445).

[7] For example, management skills in public schools and hospitals are significantly lower than those in the private sector, and much lower than those in the United States (Appendix A.2).

improvements.

Municipal governments maintain a network of schools, health clinics, and social assistance centers to serve the local population. While there are strict legal provisions constraining the hiring of frontline providers,[8] politicians have legal discretion for the appointment of street-level managers. The default mode for deploying managers is political appointment. Politically appointed managers tend to have lower levels of education and experience (Appendix E.4), which is consistent with appointments being based on political criteria rather than merit. Other common deployment modes include election by the community (where bureaucrats run for the managerial position) and civil service (where bureaucrats are tenured for life after a competitive examination). While civil service bureaucrats can generally not be fired, they can be transferred to a different unit. It is not rare for multiple appointment systems to coexist within the same municipality, and variation in appointment systems is largest in the education sector.[9]

Municipal politicians in general value public service delivery, as evidenced by interviews and the regression discontinuity design presented in Section 4.2. A large majority of the mayors I surveyed declare that they have the most responsibility for improving the quality of public services, from a list of seven actors. Recent experimental work shows that Brazilian mayors value high-quality evidence on policy effectiveness, update their priors in response to such evidence, and use it to improve public programs (Hjort et al., 2020).

3.2 Informal institutions in bureaucratic appointments

Interviews with bureaucrats and politicians provided valuable insights into the informal institutions that govern patronage in Brazilian municipalities, and in particular on the political dynamics of different appointment systems. In practice, decisions on the political appointment of street-level managers are usually taken by the mayor, sometimes in consultation with city councilors in their coalition or with the secretary of the area. Secretaries are however in charge of the selection of temporary street-level bureaucrats (like teachers or nurses), who generally hold one-year contracts. Street-level managers generally do not control the hiring, firing, or transferring of street-level

[8]For example, as per the Brazilian constitution, bureaucrats are by default supposed to be hired under a civil service regime. In practice, many of them are hired under temporary contracts.

[9]As of 2017, about 65% of municipal directors were appointed by politicians, 24% elected by the community, 5% tenured in a civil service regime, 3% selected through some meritocratic criteria, and the rest appointed through other means. Political appointments are more common in smaller and poorer municipalities (Appendix A.3).

bureaucrats.[10]

Street-level manager positions are particularly important for politicians, given their strategic position in social networks, their visibility for the community, and their wide territorial reach in both urban and rural areas. In clientelistic settings street-level managers can thus be useful for political mobilization. An elected director reported that "at the time of elections, [a previous, appointed director] asked school staff to wear the party's t-shirt, intimidating temporary teachers with the possibility of them losing their contract, and intimidating tenured teachers with them being transferred to another school. [...] People were expected to go to the city councilor's rally."[11] During my interviews, bureaucrats under different appointment systems and politicians conveyed multiple accounts like this one where the resources, monitoring, and accountability of upward embeddedness were being mobilized with rent-seeking purposes.

More frequently interviewees provided reports of political connections being leveraged for the improvement of public services. Both street-level managers and secretaries suggested that the system of political appointment puts pressure on bureaucrats to work more and to be more responsive to the demands of the local government and the needs of the community. For example, a secretary said: "our directors are political appointees, but we do it with some criteria, including that they have a university degree, that they live in the community, that they communicate well [...]. But it has to be someone we trust, that's why political appointments matter." When I asked them what was trust important for, they said: "To meet deadlines, to implement programs within the law, to treat families well, and to be a bridge between the government and the families – whether we like it or not, the director is a very political position, they relate to many people, manage many people."[12]

Interviewees also referred to the advantages of political connections for bureaucratic accountability. For example, a director said that "when the director is appointed they want to measure up to the invitation that was made to them. But the person who became director because they passed a test thinks they have that position because of a test and that they owe nothing to anybody."[13] When I asked a secretary whether they felt any difference in the relationship to the elected and appointed directors, they said: "yes, absolutely. One would expect elected directors to be better, that we would see more committed. But it is quite the opposite, it's as if elected directors felt that

[10]See Appendix A.4 for diagrams of accountability relationships in the education sector under different appointment modes.

[11]School director interviewed in the state of Rio de Janeiro in February of 2017.

[12]Secretary of education interviewed in the state of Paraíba in August 2018.

[13]School director interviewed in the state of Goiás in March 2017.

it was the people who gave them the post and thus they owe nothing to the secretariat."[14]

Finally, street-level managers recounted how political connections were useful to increase the material and immaterial resources they can leverage on the job. For example, when I asked a bureaucrat what connections were valuable for, they said: "Things are really hard with connections already, I do not know what I would do without them. [...] For example, we do not have running water in the center, and it is thanks to political connections that I manage to get a water truck to come and fill our tank. That requires an articulation with the secretary of transportation and other actors – I only manage that thanks to my connections to the mayor."[15]

Taken together, interviews with municipal street-level managers and secretaries suggest that the political appointment of bureaucrats can respond to a combined concern for rent-seeking and service provision. Political appointments may come with some costs (like the deployment of bureaucrats with less education or experience), but politicians often appoint street-level managers thinking of their professional abilities –not just their political ones– and leverage political connections for the improvement of public services. While interviews provided a critical role for developing and probing hypotheses, as well as for designing quasi-experiments and interpreting their results, they make it hard to quantify relationships. The next section turns to quantitative evidence.

4 Empirical evidence

I leverage several data and methods to test whether upward embeddedness enhances the accountability and effectiveness of street-level managers. First, I use a difference-in-discontinuities to show that an electoral defeat of the mayor causes a drop in the quality of schools with appointed director, relative to schools with unappointed directors. This is consistent with a negative shock in upward embeddedness hurting bureaucratic effectiveness. Second, I use a regression discontinuity to show that appointed directors (but not elected or tenured ones) experience a decrease in their probability of turnover after meeting their school quality target. This is consistent with upward embeddedness enhancing accountability, and with politicians caring about public service delivery. Third and last, I leverage original surveys of bureaucrats and politicians, including conjoint experiments, to show that bureaucrats with upward embeddedness have more access to, trust in, and alignment with politicians. They are also perceived as communicating better with and being more responsive to

[14]Secretary of education interviewed in the state of Paraíba in August 2018.

[15]Social assistance center coordinator interviewed in the state of Rio Grande do Norte in December 2018.

the government, exerting more effort, and raising more resources. Together, these three sets of causally identified evidence and the qualitative data from interviews demonstrate that bureaucrats' upward embeddedness can enhance their accountability and effectiveness. Table 1 synthesizes the links between the theory and the empirical tests presented below.

Table 1: Mapping of theory to empirics

Theoretical claims	Test	Data	Evidence
Core arguments: Upward embeddedness facilitates...			
... bureaucratic effectiveness in service delivery	Diff-in-disc	Administrative data	Figure 1
... bureaucratic accountability	RDD	Administrative data	Figure 2
Mechanisms: Bureaucrats with upward embeddedness...			
... have higher levels of trust in, alignment with, and access to politicians	Correlations	Bureaucrat survey	Figure 3
... communicate better with and are more responsive to the government	Conjoint	Bureaucrat and politician surveys	Figures 4 and 5
... have more access to material resources	Conjoint	Bureaucrat survey	Figure 4
... exert more effort	Conjoint	Politician survey	Figure 5

4.1 Losing political connections makes appointed bureaucrats less effective: Difference-in-discontinuities evidence

A key observable implication of the theory in Section 2 is that political turnover should differentially affect appointed and unappointed bureaucrats. For appointed bureaucrats, mayoral turnover means a decrease in upward embeddedness, and therefore to governance resources that help them in public service delivery. For unappointed bureaucrats, however, mayoral turnover does not change their upward embeddedness. Both types of bureaucrats are exposed to the general effects that political turnover can have on public administration, including the organizational costs of transition, the benefits of a renewed leadership, and other shocks to the bureaucracy (Akhtari et al., 2020; Toral, 2021). If my theory is right, the performance of appointed bureaucrats should worsen as a result of political turnover, when compared to that of unappointed bureaucrats.

To exploit the differential impact of political turnover on upward embeddedness I use a difference-in-discontinuities design (Grembi et al., 2016). In essence, this design combines a difference-in-differences (comparing the performance of appointed and unappointed bureaucrats, before and after the election) with a close-races regression discontinuity (comparing the performance

of bureaucrats in municipalities where the mayor lost the re-election to bureaucrats in municipalities where the mayor was re-elected).[16] I use data for municipal school directors, for whom the federal government releases every two years a measure of performance (based on student test scores and passing rates) as well as an administrative survey that includes data about their appointment mode. The design shows that an electoral defeat of the mayor causes a drop of about 0.3 standard deviations in the quality score of schools with appointed directors, when compared to those with unappointed directors ($p < 0.01$).

4.1.1 Design

The design exploits two treatments: whether a municipality m experiences political turnover (P_m), and whether a school s experiences a decrease in upward embeddedness after the election (U_{sm}). The political turnover treatment is assigned by the difference between the vote share of the strongest challenger (V_m^c) and that of the incumbent (V_m^i): $D_m = V_m^c - V_m^i$. If this forcing variable is above 0, the municipality experiences political turnover, otherwise the mayor is re-elected and there is no political turnover. The upward embeddedness treatment is assigned by the combination of the municipality experiencing political turnover and the school having a director that had been appointed by the mayor (A_{sm}):

$$P_{sm} = \begin{cases} 1 & \text{if } D_m > 0 \quad \text{(mayor loses re-election)} \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

$$U_{sm} = \begin{cases} 1 & \text{if } D_m > 0 \text{ and } A_{sm} = 1 \quad \text{(mayor loses re-election, director was appointed)} \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

To separate the effect of a negative shock to upward embeddedness from that of political turnover, I exploit the difference between appointed directors (who lose upward embeddedness when their patron loses the election) and unappointed directors (whose upward embeddedness is not affected by political turnover).

Potential outcomes are a function of both $P_{sm} = p \in \{0, 1\}$ and $U_{sm} = u \in \{0, 1\}$, so we

[16] I focus on the electoral performance of the mayor rather than their political party because this setting is characterized by weak partisan attachments (Boas et al., 2019) and pervasive party switching by politicians (Klašnja and Titiunik, 2017). It is quite common for incumbents to run for reelection with a party different from the one they got elected under (e.g., 30.4% mayors did it in 2008), so examining the electoral performance of the incumbent party would be misleading.

can define them as $Y_{sm}(p,u)$. With that notation, the estimand of interest is:

$$\tau_{ddisc} = \mathbb{E}[Y_{sm}(1,1) - Y_{sm}(0,0)|D_m = 0, A_{sm} = 1] - \mathbb{E}[Y_{sm}(1,0) - Y_{sm}(0,0)|D_m = 0, A_{sm} = 0] \tag{3}$$

We can identify the local average treatment effect (LATE) around the threshold by taking the difference in means from below and above the threshold for each type of director, and subtracting them:

$$\begin{aligned}\hat{\tau}_{ddisc} = &\left(\lim_{D_m \downarrow 0} \mathbb{E}[Y_{sm}|D_m = 0, A_{sm} = 1] - \lim_{D_m \uparrow 0} \mathbb{E}[Y_{sm}|D_m = 0, A_{sm} = 1]\right) \\ &- \left(\lim_{D_m \downarrow 0} \mathbb{E}[Y_{sm}|D_m = 0, A_{sm} = 0] - \lim_{D_m \uparrow 0} \mathbb{E}[Y_{sm}|D_m = 0, A_{sm} = 0]\right)\end{aligned} \tag{4}$$

This design relies on three assumptions (Grembi et al., 2016). First, potential outcomes $Y_{sm}(p,u)$ should be continuous in the forcing variable around the threshold. Appendix C.2 shows that pre-treatment covariates are generally continuous around the cutoff. Second, the effect of political appointment when there is no change to upward embeddedness should be constant over time, such that schools with appointed and unappointed directors would follow parallel trends. Appendix C.3 shows that schools with appointed and with unappointed directors, as well as schools in municipalities with and without political turnover, follow parallel trends in performance before the election. With these two assumptions, the diff-in-disc estimator estimates the local causal effect of a decrease in upward embeddedness, close to the threshold, and for appointed directors. If we make a third homogeneity assumption that the effects of the decrease in upward embeddedness and of political turnover do not interact, then we can recover the local average treatment effect of a decrease in upward embeddedness for schools in municipalities close to the threshold.

The design focuses on within-director changes in performance. I include only schools where the director had been assigned to their school in the years before the election, and was still in their post one year after. Schools with director turnover are excluded from the sample because their changes in performance cannot be associated to the change or stability of the director's upward embeddedness. However since director turnover can happen after the election (and is in fact affected by election results), this may introduce sample selection bias (Heckman, 1979). In Section 4.1.4 I discuss this issue more fully, showing that it is likely to bias my results towards zero, that removing part of that bias increases the size of the effect, and that bounds that account for the worst possible case of sample selection bias are fully below zero.

The design focuses on relatively short-term effects of negative shocks to upward embeddedness. Elections take place every four years on the first Sunday of October, the new government is sworn in on January 1st of the following year, and the next student tests are done in early November of the following year. While increasing student learning is a complex task that requires long-term efforts, short-term actions implemented in the months and the weeks leading up to the tests can have a significant impact on the results, including raising awareness (among both teachers and students) about the importance of student evaluations, implementing special remedial classes, and doing test simulations to familiarize students with federal tests. All these actions depend critically on directors' management efforts and their coordination with school staff and with the local government.[17]

4.1.2 Estimation and inference

To estimate the difference-in-discontinuities, I follow the common practice of using local linear regression (Gelman and Imbens, 2018) within the optimal bandwidth of the Calonico et al. (2020) algorithm, and apply it to the following estimating equation:

$$Y_{smj} = \alpha + \beta_1 P_{mj} + \beta_2 D_{mj} + \beta_3 P_{mj} D_{mj} + A_{smj}(\gamma_1 + \gamma_2 P_{mj} + \gamma_3 D_{mj} + \gamma_4 P_{mj} D_{mj}) \quad (5)$$
$$+ \lambda \boldsymbol{I}[j = 2016] + \sum_{k=1}^{K} \eta_k X_{smj}^k + \varepsilon_{smj}$$

Where Y_{smj} is the change in the quality score of school s in municipality m and election cycle j, $\lambda \boldsymbol{I}[j = 2016]$ is an election cycle fixed effect, $\sum_{k=1}^{K} \eta_k X_{smj}^k$ is a set of state fixed effects and director-, school-, and municipality pre-treatment covariates that significantly predict directors' appointment mode,[18] which I include in some specifications to partially address the endogeneity of appointment modes. ε_{smj} is an error term. Standard errors are clustered at the municipality level, where political turnover is determined. If the diff-in-disc assumptions hold, γ_2 identifies τ_{ddisc}, namely the local average treatment around the threshold of a negative shock in upward embeddedness.

[17] Consistent with short-term director actions having the potential to improve school performance, NGOs and governments regularly produce materials to help directors prepare the school for the tests (see Appendix A.7).

[18] Results of the correlational regression of appointment mode on covariates are included in Appendix A.3.

4.1.3 Data

I leverage regular, valid, and well-established measurements of school performance done by the federal government every two years through the National Assessment of School Performance (ANRESC, *Avaliação Nacional do Rendimento Escolar*). This system tests students at the end of primary and middle school in public schools across the country. Exams are based on item response theory, which ensures that its measures of learning outcomes are valid and comparable over time. Together with the tests, the government also implements a survey of the director of the school, with questions about their appointment, experience, demographics, and perceptions of the school. Combining test results and administrative data on student passing rates, the federal government calculates for each school a score in the Basic Education Development Index (IDEB, *Índice de Desenvolvimento da Educação Básica*). IDEB scores (which are separate for primary and middle education) are normalized so that they range from 0 to 10.

I use Ministry of Education data for all municipal primary schools, in the years immediately before and after the elections of 2012 and 2016.[19] I use the Ministry's survey of directors to identify schools where the director had been deployed in the years leading to the election and were still in their post one year after, as well as to identify the director's appointment mode. I merge the school-level data with data on municipal election candidates and their performance obtained from Brazil's Supreme Electoral Court.

4.1.4 Results

Results of the diff-in-disc are shown in Table 2. A decrease in upward embeddedness (identified by the differential effect of political turnover among appointed directors) has a negative effect on school performance. In particular, it reduces the school quality score by 0.39 points or about 0.36 standard deviations ($p < 0.01$). Figure 1 illustrates the two discontinuities on which the design is based. The result is robust to the inclusion of covariates (models 2 and 3 in Table 2) and to alternative bandwidths (Appendix C.4).

A potential concern with this design is that it may suffer from sample selection bias because, in order to examine within-director changes in performance, schools where the director changes after the election are excluded from the sample. Director turnover however is directly affected by mayor turnover (Appendix C.5). This generates groups of schools (under mayor re-election and mayor

[19] I focus on the 2012 and 2016 cycles because before 2011 the question on director turnover has different response options and much higher levels of non-response.

Table 2: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality, by appointment mode

	Model 1	Model 2	Model 3
$\hat{\beta}_1$: Political turnover	0.173	0.140	0.051
	(0.108)	(0.111)	(0.128)
$\hat{\gamma}_2$: Political turnover × Appointed	-0.392***	-0.376***	-0.369**
	(0.145)	(0.144)	(0.164)
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.136	0.136	0.136
N	1531	1531	831

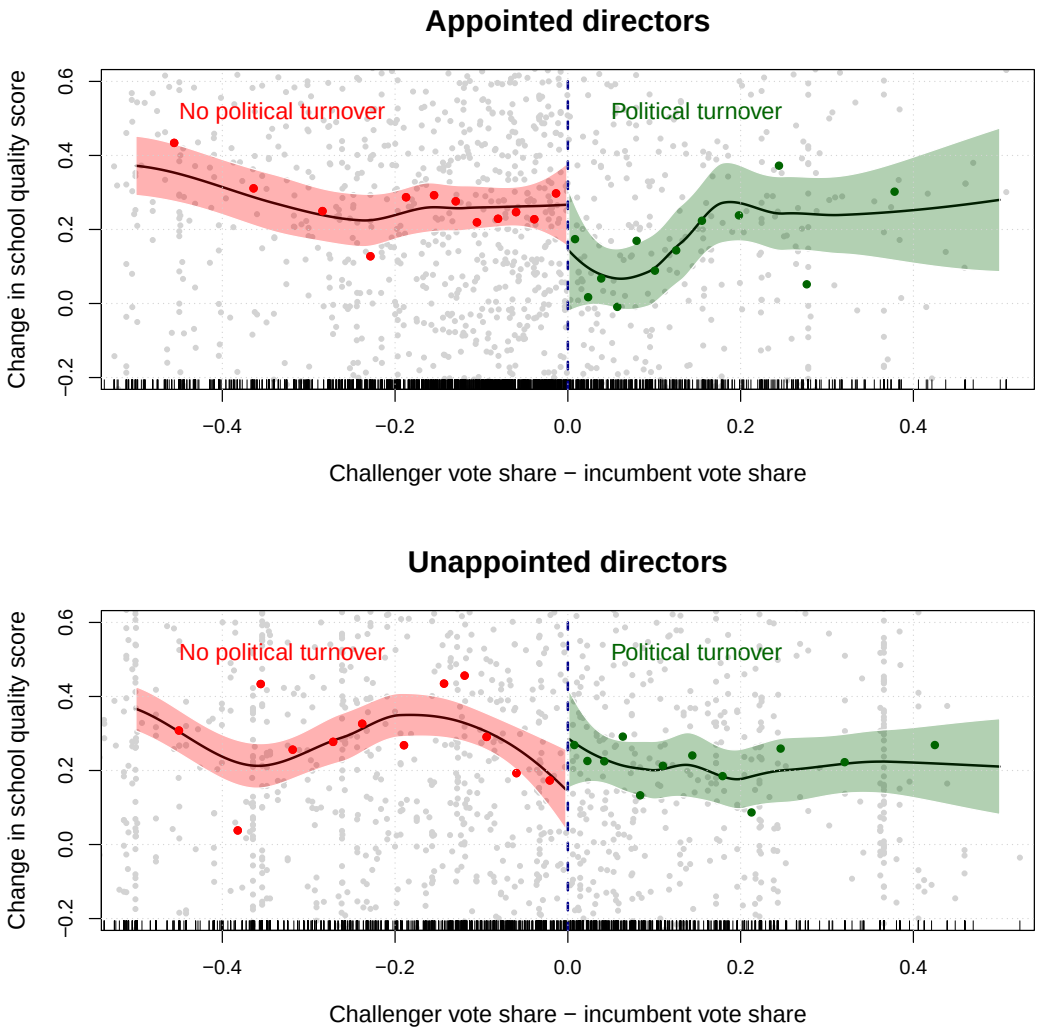
Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a vector of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors in brackets. *p<0.1; **p<0.05; ***p<0.01.

turnover) that are not necessarily comparable. I address this issue of sample selection bias through three complementary strategies. First, I show in Appendix C.6 that when there is mayor turnover, directors with better performance at baseline or with a number of characteristics associated to performance are significantly more likely to stay in their post.[20] This implies that including in the analysis schools without mayor turnover that would have seen their director change under mayor turnover biases the results towards zero. Second, I show in Appendix C.7 that diff-in-disc estimates remain large and significant when pre-processing the data with exact matching. Third and last, I show in Appendix C.8 that sharp bounds adapted from Lee (2009) to this difference-in-discontinuities setting remain fully below zero. This suggests that, even in the worst-case scenario of sample selection bias, the diff-in-disc estimates of the effect of political turnover on the effectiveness of appointed directors would be negative.

As additional robustness checks, I find no evidence that these effects are driven by changes in the supply of teachers, teacher turnover, or teacher absenteeism (Appendix C.9). Another alternative mechanism might be that appointed directors who survive mayor turnover anticipate being fired, but in fact most of the directors who are replaced under a new government lose their

[20]This is consistent with several interviewees' reports that IDEB performance is an important input when a new government decides what directors to keep. For example a school director interviewed in the state of Goiás in March 2017 said "political appointment makes sense, it's a position of trust – but when the government changes, and the director has made a good job (with a good diagnosis, a good IDEB score, has sent paperwork in time...) he gets to stay.".

Figure 1: Effect of political turnover on school quality, 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 are their 95% confidence intervals.

post at the beginning of the new administration.[21] I further test the validity of the design through a placebo test, by replicating the analysis with data for state schools, which should be unaffected by mayoral turnover (Appendix C.10).

In sum, I find that an electoral defeat of the mayor differentially hurts the quality of schools with directors that had been appointed by them, when compared to schools with unappointed directors.

[21]Of those directors appointed under the 2009-2012 administration in municipalities where a new government was sworn in January 2013 an who were not replaced by November, 60% remained in their post in late 2015. Others could have been relocated to another school.

This is consistent with decreases in upward embeddedness hurting bureaucratic effectiveness.

4.2 Appointed bureaucrats are held accountable for their performance in a service delivery indicator: Regression discontinuity evidence

To test whether the political appointment of bureaucrats enhances accountability, I leverage a regression discontinuity design, where I study the effect of schools meeting their school quality target for 2013 on the probability of the director being replaced by 2015. Among schools with appointed directors, meeting the quality target reduces director turnover by 0.2 standard deviations ($p < 0.01$). For schools with elected or tenured directors, the rate of director turnover is not affected by whether they meet their target. Results support the hypothesis that political appointment enhances accountability.

4.2.1 Design

Together with the establishment of IDEB as a system for measuring the quality of public schools, the federal government defined targets for every two-year period from 2007 to 2021. These targets were defined following an algorithm that projects progress of schools along logistic trajectories (Fernandes, 2007) and have not been revised. Thus, every two years schools get a quality score for their performance, which can be compared to their pre-defined target for that year. If the difference between the score and the target is zero (or above), the school met (or surpassed) its target. Conversely, if that difference is negative, the school missed its target. I exploit this discontinuity to measure the causal effect of a school meeting its target in the 2013 test (which was published in 2014) on the school experiencing director turnover between 2014 and 2015, and to explore heterogeneity by the appointment type of the school's director in 2013.[22] Information about IDEB is widely disseminated after its release by the federal government, and emphasis is usually given to whether targets were met (Boas et al., 2020). While local actors have other sources of information about the quality of schools, IDEB reveals simple, reliable metrics and thus facilitates common knowledge.[23]

More formally, treatment for school s (meeting the school quality target), T_s, is assigned by

[22] I focus on the 2013-2015 IDEB cycle to avoid years with municipal elections, which significantly increase director turnover (Appendix C.5).

[23] Note for example that actors in financial markets also respond to binary signals (e.g. credit rating downgrades) despite being in a much thicker information environment (Ismailescu and Kazemi, 2010).

the difference between its quality score and target ($D_s = score_s - target_s$):[24]

$$T_s = \begin{cases} 1 & \text{if } D_s \geq 0 \quad \text{(quality score} \geq \text{quality target)} \\ 0 & \text{if } D_s < 0 \quad \text{(quality score} < \text{quality target)} \end{cases} \tag{6}$$

The estimand of interest is $\tau = \mathbb{E}[Y_s(1) - Y_s(0)]$, where $Y_s(1)$ and $Y_s(0)$ represent the potential outcome of interest (director turnover in school s), under treatment (having met the target) and under control (having missed it). We can identify the local average treatment effect (LATE) around the cutoff by taking the difference in means from above and from below the threshold:

$$\hat{\tau}_{rdd} = \lim_{D_s \downarrow 0} \mathbb{E}[Y_s(1)|D_s = 0] - \lim_{D_s \uparrow 0} \mathbb{E}[Y_s(0)|D_s = 0] \tag{7}$$

The key assumption of this design is that potential outcomes are continuous around the threshold. Reassuringly, there is no evidence of sorting around the threshold (Appendix D.1), and pre-treatment covariates are continuous around the threshold (Appendix D.2).

4.2.2 Estimation and inference

I use local linear regression and apply it to the following estimating equation, within the bandwidth selected by the Calonico et al. (2020) algorithm:

$$Y_s = \alpha + \beta_1 T_s + \beta_2 D_s + \beta_3 T_s D_s + \varepsilon_s \tag{8}$$

Where Y_s is the indicator for whether school s had director turnover between 2014 and 2015. T_s is a treatment indicator for school s: $\mathbb{1}$(quality score $\geq$ quality target). D_s is the distance to the threshold in the forcing variable. ε_s is an error term. If the RDD assumptions hold, β_1 identifies the LATE in Equation 7. For inference I use the HC1 heteroskedasticity consistent estimator. In order to examine whether appointed directors are held accountable for their performance, we need to measure and make inference about the effect of treatment in a subset of the data. Here the estimand is the heterogeneous local average treatment effect or HLATE (Becker et al., 2013). To estimate it, I allow for separate slopes for appointed and not appointed directors:

[24]While the Ministry of Education uses figures with one decimal only, I use a continuous measure to increase statistical power and avoid the issues with discrete forcing variables in RDDs (Lee and Card, 2008). -0.05 in the continuous measure is equivalent to 0 with the rounding applied by the Ministry. I therefore re-center the forcing variable by adding 0.05.

$$Y_s = \alpha + \beta_1 T_s + \beta_2 D_s + \beta_3 T_s D_s + A_s(\gamma_1 + \gamma_2 T_s + \gamma_3 D_s + \gamma_4 T_s D_s) + \sum_{k=1}^{K} \eta_k X_s^k + \varepsilon_s \quad (9)$$

Where A_s is an indicator for whether the school's director in 2013 was appointed. $\beta_1 + \gamma_2$ identify the HLATE, under two additional assumptions. First, the subgroup indicator A_s must be continuous around the threshold. Appendix D.2 shows that there is continuity around the threshold in this and dozens of other pre-treatment covariates. Second, the subgroup indicator A_s must be conditionally ignorable, or as if-randomly assigned, such that around the threshold and conditional on their distance to the RD threshold, schools with appointed and not appointed directors do not differ systematically in a way that affects their turnover. To relax this assumption, I include $\sum_{k=1}^{K} \eta_k X_s^k$: state and municipality fixed effects, and a vector of director-, school-, and municipality-level pre-treatment covariates that predict whether the school has an appointed director.[25]

4.2.3 Data

I use official data on primary education quality scores from the Ministry of Education, and combine them with data from the 2015 director survey to measure director turnover. I code a school as having director turnover when the respondent says they have been in their post for one year or less.[26]

4.2.4 Results

Table 3 presents the results. Model 1 shows that, overall, reaching the quality target does not affect school directors' turnover. Among schools with an appointed director in 2013, however, meeting the target depresses the probability of director turnover in the year following the publication of the results by 7.3 percentage points or about 0.19 standard deviations ($p < 0.01$), as shown in model 2. Figure 2 visualizes this effect.[27] Among elected or tenured directors, however, meeting the target does not cause any significant change in the probability of turnover (Appendices D.4 and D.5).[28] Results are robust to the inclusion of state or municipality fixed effects, and a long set of

[25] Details on the predictors of appointment mode are in Appendix A.3.

[26] Unfortunately school directors are not identified, so I cannot track the destination of directors who are replaced.

[27] Plots for schools with unappointed directors are in Appendix D.3.

[28] While tenured directors generally cannot be fired, they can be transferred to a different school. Interviews suggest transfers are frequently used to discipline bureaucrats. In Appendix D.6 I present some qualitative and quantitative evidence of how low competition, capture, and low participation limit the ability

pre-treatment covariates that significantly predict school directors being appointed (models 3-5). Additional robustness checks lend further support to these results. Alternative bandwidths lead to similar estimates (Appendix D.7). Placebo tests changing the RD threshold generally return insignificant results (Appendix D.8). Results are larger in municipalities with a large, programmatic party in office (Appendix D.9), which is consistent with directors being held accountable for their performance in service delivery.

Table 3: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by appointment mode

	Model 1	Model 2	Model 3	Model 4	Model 5
$\hat{\beta}_1$: Quality target met	-0.024	0.011	0.008	0.013	-0.003
	(0.018)	(0.027)	(0.026)	(0.032)	(0.027)
$\hat{\gamma}_2$: Quality target met × Appointed		-0.084**	-0.082**	-0.099**	-0.067*
		(0.035)	(0.035)	(0.045)	(0.036)
State fixed effects			✓		✓
Municipality fixed effects				✓	
Predictors of Appointed					✓
$\hat{\beta}_1 + \hat{\gamma}_2$:		-0.073***	-0.074***	-0.085***	-0.070***
		(0.023)	(0.023)	(0.029)	(0.024)
Bandwidth	0.437	0.467	0.467	0.467	0.467
N	7362	7466	7434	7434	6942

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. HC1 standard errors in brackets. *p<0.1; **p<0.05; ***p<0.01.

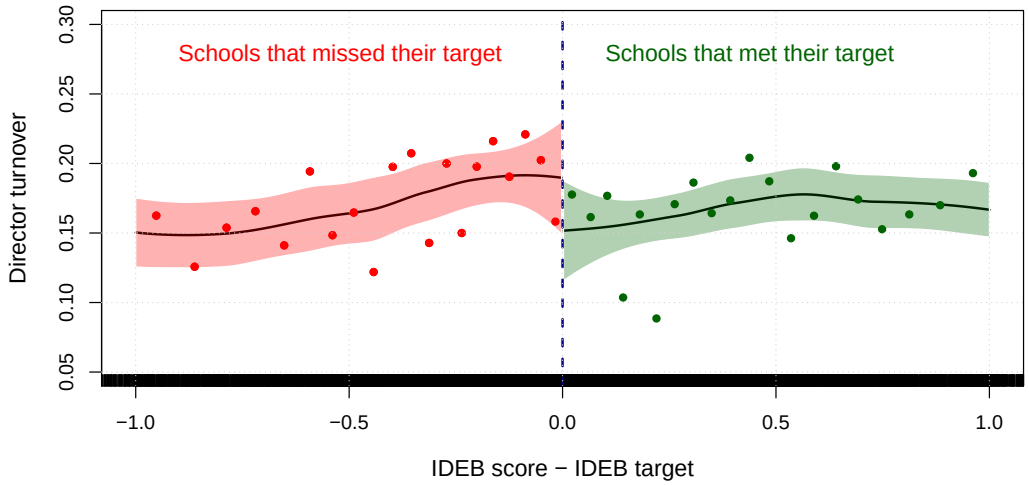
To sum up, these results suggest that appointed directors are held accountable for school quality, while elected or tenured ones are not. This is consistent with upward embeddedness facilitating bureaucratic accountability.

4.3 Bureaucrats and politicians perceive political appointments and connections as making bureaucrats more responsive: Survey evidence

How does upward embeddedness foster bureaucratic effectiveness and accountability? To address this question I leverage a face-to-face, representative survey of 926 street-level managers, and an

of director elections to boost accountability.

Figure 2: Effect of meeting the school quality target on director turnover, for schools that had an appointed director



See notes under Figure 1.

online survey of 524 local politicians. Results from conjoint experiments in these surveys suggest that bureaucrats with upward embeddedness communicate better with and are more responsive to the local government, obtain more resources, and exert more effort.

4.3.1 Face-to-face survey of street-level managers

Based on my in-depth interviews with bureaucrats and politicians, I designed and implemented a large, face-to-face, representative survey of municipal street-level managers (school directors, health clinic managers, and social assistance center coordinators) between November and December of 2018. This is, to my knowledge, the first representative survey of street-level managers collecting data about their political connections and attitudes. The survey took place in Rio Grande do Norte, a state at the heart of Brazil's Northeastern region, which has historically been characterized by inferior development outcomes, corruption, and clientelism. The survey focused on the urban areas of 150 small and medium municipalities (all but the largest 17 municipalities in the state, which were excluded for security concerns). The field team traveled more than 25,000 kilometers over four weeks to locate every municipal school, health clinic, and social assistance center in the urban area of those municipalities. The managers of 926 out of 1,027 units (over 90%) were surveyed, with a median number of 5 surveys done per municipality.[29]

[29]See Appendix E.1 for details on the municipalities in the state, Appendix E.2 for details on respondent recruitment and non-response, and Appendix E.3 for descriptive statistics.

First I present observational data suggesting that appointed managers have more access to, trust in, and alignment with politicians. For that I leverage survey questions on the number of meetings street-level managers held, over the previous three months, with the mayor and the secretary, among other stakeholders; and questions on their level of agreement (on a 4-point scale) with statements about the mayor and their secretary. To find if there are robust correlations between managers' appointment mode and their number of meetings with, or attitudes about, local stakeholders, I regress respondents' answers on indicators for appointment modes (appointed or elected, leaving civil service as the baseline) and controls:

$$Y_i = \alpha + \beta_1 A_i + \beta_2 E_i + \sum_{k=1}^{K} \gamma^k X_i^k + \varepsilon_{im} \tag{10}$$

Where Y_i is the response given by manager i (namely, the log of the number of reported meetings with a given stakeholder +1, or the level of agreement with a given statement); A_i and E_i are indicators for whether that manager is appointed or elected (with civil service being the baseline); and $\sum_{k=1}^{K} \gamma^k X_i^k$ are all the demographic and political covariates I collected[30] as well as municipality and sector fixed effects. To facilitate comparisons between appointment modes I exclude from these regressions the 18% respondents who report having been appointed through a mixture of methods.[31] For inference I use HC1 heteroskedasticity-consistent standard errors.

Figure 3 presents the results.[32] Compared to civil service managers, political appointees report, on average, a higher number of meetings with the mayor, the secretary, and technicians in the area; as well as higher levels of trust in the mayor and the secretary, feelings of proximity to them, and beliefs that the mayor cares about improving public services and has the same priorities as street-level bureaucrats.

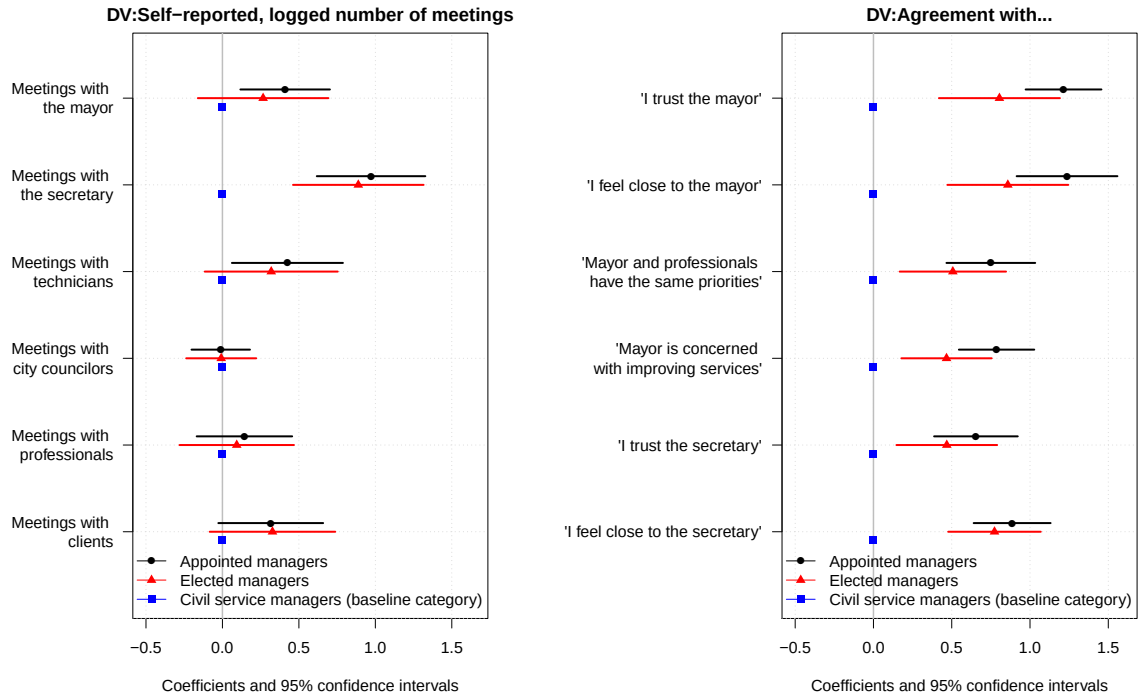
While not causal, these associations are strong and aligned with the predictions of the theory. To more directly test the relationship between upward embeddedness and accountability and performance, I use a conjoint experiment embedded in the survey. Conjoint experiments allow researchers to non-parametrically identify and estimate the causal effect of several variables simultaneously while limiting social desirability bias (Hainmueller et al., 2014).

[30] Controls include respondents' sector, age, gender, years of experience as professional in the sector, years of experience as manager, party membership, union membership, whether they have less or more education than a college degree, whether they have other jobs, and whether they live in the municipality where they work.

[31] Results for appointed bureaucrats are similar when including the whole sample.

[32] Regression details are in Appendix E.5.

Figure 3: Observational relationship between street-level managers' appointment type and meetings with and attitudes about politicians.



Points are the regression coefficient corresponding to each appointment mode (as per Equation 10) and bars are their 95% confidence intervals.

In the conjoint, respondents saw four sets of two hypothetical profiles of managers, with randomly assigned attributes in six dimensions (appointment mode, political connections, education, experience, relationship to professionals, and unit performance in federal indicators).[33] To avoid primacy and recency effects, the order of the attributes was randomized across respondents. For each pair, respondents were asked to choose which one they believed would be more likely to: (i) maintain better communication with the secretariat; (ii) implement school changes requested by the municipal government; (iii) raise more material resources for a reform of the school / clinic / social assistance center; and (iv) increase the unit's performance in indicators of learning / healthcare / social assistance. These four choice tasks aim at measuring the relative impact of different bureaucratic characteristics on their (perceived) ability to perform on key areas of management that my theory predicts upward embeddedness should facilitate.

[33]Details of the attribute values for conjoint profiles are included in Appendix E.6.

I estimate the average marginal component effect (AMCE) for each attribute's value using linear regression (Hainmueller et al., 2014):

$$Y_{ijk} = \alpha + \boldsymbol{\beta} \boldsymbol{W}_{ijkl} + \varepsilon_{ijk} \tag{11}$$

Where Y_{ijk} is the choice expressed by respondent i for profile j in the choice task k (i.e. whether that given manager profile was chosen); W_{ijkl} is the vector of dummy variables for the l levels of each attribute in profile j (omitting a baseline category in each attribute); and ε_{ijk} is an error term. I cluster standard errors at the respondent level to account for the dependencies between the choices each respondent makes. $\boldsymbol{\beta}$ nonparametrically identifies the AMCE for each of the attributes and their values on a hypothetical manager being chosen for a given task in the sample.

The results of the conjoint experiment, shown in Figure 4,[34] demonstrate that street-level managers see upward embeddedness as an important resource facilitating bureaucratic communication with and responsiveness to the local government, as well as access to resources. Profiles of managers with political connections, or who are political appointees, are seen as significantly more likely to have better communication with the secretariat of their area, to implement changes requested by the local government, or to raise resources for reforming the school/clinic/social assistance center, when compared to civil service managers.[35]

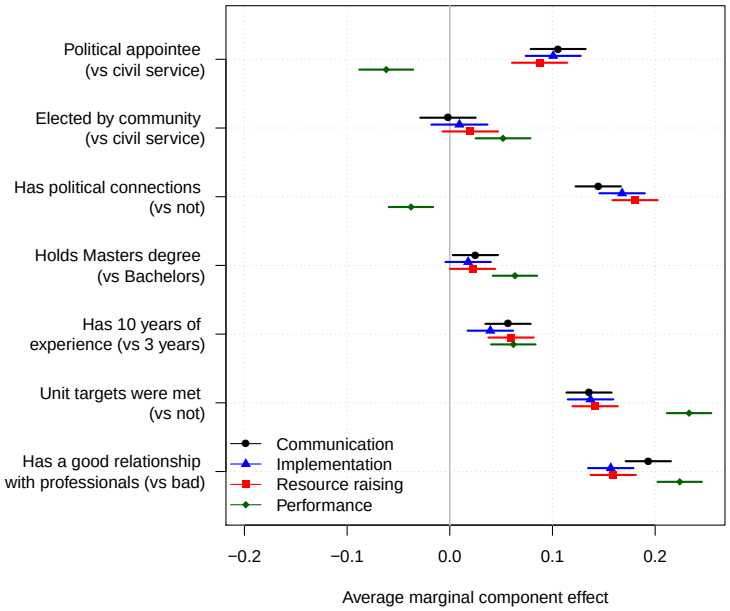
On the other hand, managers who are politically appointed or have political connections are seen as less likely to improve the performance of the school/clinic/social assistance center. This suggests that upward embeddedness may hinder public sector delivery. Several factors may explain this. First, the strong Weberian norm existing in the field, where actors (including politically appointed bureaucrats) often believe that all bureaucrats should in principle be tenured, may lead managers to believe that appointees perform worse. Second, respondents may be underestimating the indirect effects that upward embeddedness has on public service delivery. Third, managers may be expressing here that politically appointed bureaucrats are in fact worse types who would indeed perform worse without the benefits of upward embeddedness.[36] Fourth and last, part of this result may be driven by the inclusion in the respondent pool of street-level managers who work in a variety of settings, including highly clientelistic ones. As shown in Appendix E.9, including only respondents

[34] Regression details are in Appendix E.7.

[35] One potential concern is that these results are driven by appointed bureaucrats trying to portray a good picture of themselves. Results however are similar when subsetting to unappointed managers (Appendix E.8).

[36] Indeed, politically appointed managers are less likely to have graduate degrees (Appendix E.4).

Figure 4: Results from the conjoint experiment with bureaucrats



Points are the average marginal component effect (AMCE), and bars their 95% confidence intervals. AMCEs estimated for each choice task separately, as per Equation 11.

who agree with statements about the mayor and the secretary having programmatic concerns leads to smaller and insignificant coefficients for the performance question, without substantially altering the results for all other questions. In any case, these results draw attention to the potential costs of political appointments and connections discussed in Section 2.

4.3.2 Online survey of politicians

Local politicians also perceive bureaucrats with upward embeddedness as more accountable. In partnership with the state audit court of Rio Grande do Norte I implemented an online survey of local politicians. The descriptive module of the survey also offers some evidence in support of the assumption that politicians care about public service delivery.[37] The survey was sent through the court's online system to the mayor and the municipal secretaries of education, healthcare, social assistance, finance, and administration[38] of all 167 municipalities in the state. 524 politicians com-

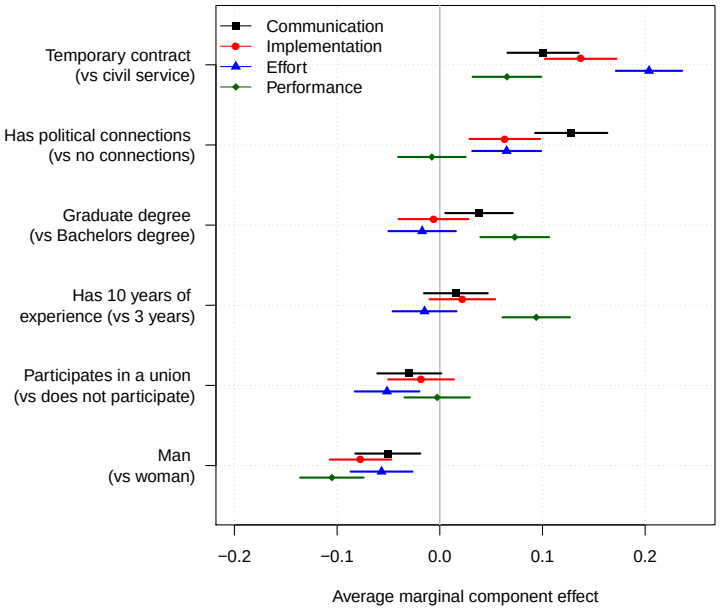
[37] 70% of the mayors believe mayors have the most responsibility for improving the quality of public services like municipal education and healthcare. Secretaries of education and healthcare report, on average, one weekly meeting with street-level managers in their area.

[38] Secretaries of administration are in charge of human resources for the municipal government.

pleted the survey, for a response rate of 52% and a median number of 3 responses per municipality.[39] This response rate is high for a survey of elites.

In this conjoint experiment, respondents saw four pairs of hypothetical bureaucrats (without specifying their rank or area of work), with randomly assigned attributes in six dimensions (contract type, political connections, education, experience, union membership, and gender).[40] Contract type (temporary versus civil service) was used instead of appointment mode because political appointment and election can only be used for managers. Like political appointments, temporary hires are at will and often based on political connections (Colonnelli et al., 2020).[41] For each pair, respondents were asked to choose which one they believed would be more likely to: (i) maintain better communication with the local government; (ii) implement changes requested by the local government; (iii) work extra hours when necessary; and (iv) achieve better performance.

Figure 5: Results from the conjoint experiment with politicians



See notes under Figure 4.

Conjoint results, shown in Figure 5,[42] suggest that politicians see bureaucrats with upward

[39] Details on respondent recruitment and non-response are reported in Appendix F.1. Descriptive statistics are reported in Appendix F.2.

[40] Details of the attribute values for conjoint profiles are included in Appendix F.3.

[41] 58% of street-level managers in my survey said political appointments influence "a lot" the hiring of street-level bureaucrats, and only 16% responded "nothing" or "a little."

[42] Regression details are in Appendix F.4.

embeddedness (i.e., politically appointed or hired under a temporary contract) as more responsive and exerting more effort. They also see bureaucrats with temporary contracts as likely to perform better than those hired under the civil service regime. In words of a secretary of education, "almost all civil service bureaucrats are from other towns. They don't work with the true grit we need. [...] Temporary hires dedicate themselves more. Things flow because managers count on contract workers."[43]

All in all, conjoint experiments with bureaucrats and politicians are generally supportive of the key mechanisms of the theory. Both bureaucrats and politicians perceive bureaucrats with more upward embeddedness as more likely to communicate well with the local government and respond to its demands. Bureaucrats also perceive them as more likely to raise funds from the government, and politicians perceive them as more likely to work extra hours when needed. Together, these results show that actors in the field perceive political appointments and connections as benefiting bureaucratic accountability and – at least in some dimensions – their effectiveness.

5 Conclusion

Patronage, or the political appointment of bureaucrats, is typically seen as a clientelistic exchange that hurts development by selecting into the bureaucracy worse types and/or depressing bureaucratic effort. This paper offers an alternative view of patronage whereby political appointments can enhance bureaucratic effectiveness and accountability. In particular, I argue that patronage provides bureaucrats with *upward embeddedness* (political, social, and professional ties to politicians) which can make them more accountable and effective in public service delivery. Upward embeddedness works by giving bureaucrats access to material and immaterial resources, providing politicians with monitoring technology, facilitating the application of sanctions and rewards, aligning their priorities and incentives, and increasing mutual trust. These governance resources can be leveraged for extracting rents, for delivering public services, or for both. I argue the benefits of patronage are more likely to be net positive in the appointment of street-level managers (like school directors) and in developing contexts where politicians value the delivery of public goods but face human capital and financial constraints on their capacity to attract and motivate bureaucrats to perform.

The main empirical contribution of the paper is to provide causally identified evidence of the benefits of patronage for bureaucrats' effectiveness and accountability. It does so by leveraging administrative and survey data of municipal bureaucracies in Brazil, a setting where multiple ap-

[43]Secretary of education interviewed in the state of Paraíba in August 2018.

pointment systems coexist. Using a difference-in-discontinuities, I show that the quality of schools with appointed directors decreases (relative to that of schools with unappointed directors) when the mayor loses the re-election. This is consistent with political connections facilitating bureaucratic effectiveness. Using a regression discontinuity, I show that appointed directors (but not unappointed ones) are less likely to be replaced when they meet a highly visible school quality target. This is consistent with politicians using new information on director performance to decide which ones to keep and which ones to replace, thus holding directors accountable for the quality of the schools they manage. I explore the mechanisms of political appointments and connections through original surveys of bureaucrats and politicians. Observational analyses show that appointed bureaucrats tend to have more meetings with local politicians, and to express higher levels of trust in and alignment with them. Conjoint experiments show that both street-level managers and politicians perceive bureaucrats with upward embeddedness as better at communicating with the government and responding to its demands. Quantitative findings are grounded on 121 in-depth interviews with bureaucrats, politicians, and anti-corruption agents, which provide rich accounts of how patronage can enhance bureaucratic accountability and effectiveness.

The paper has several implications for the study of bureaucratic politics and governance, and for policies of public sector reform. The findings suggest that politics in the developing world can be a source not only of corruption and misallocations, as is often assumed, but also of governance resources that can help overcome development challenges. While the costs of patronage have long been studied, the advantages that upward embeddedness provides for enhancing bureaucrats' accountability and effectiveness in public service delivery have been largely overlooked. In certain contexts, the paper has shown, these advantages may outweigh the costs. This implies that the common prescription that bureaucracies be highly depersonalized and insulated can actually be detrimental for development, at least in the short term. On the other hand, by drawing attention to the governance advantages of patronage, the paper helps us better understand why it is frequently used for rent extraction. Political appointments and connections influence not only who enters the bureaucracy and how much they work, but also –and perhaps most importantly– *how* they work. This explains why patronage can be useful both to extract rents and for providing public goods and services, and may be one of the reasons why it is such a prevalent phenomenon around the world, even in high-income democracies.

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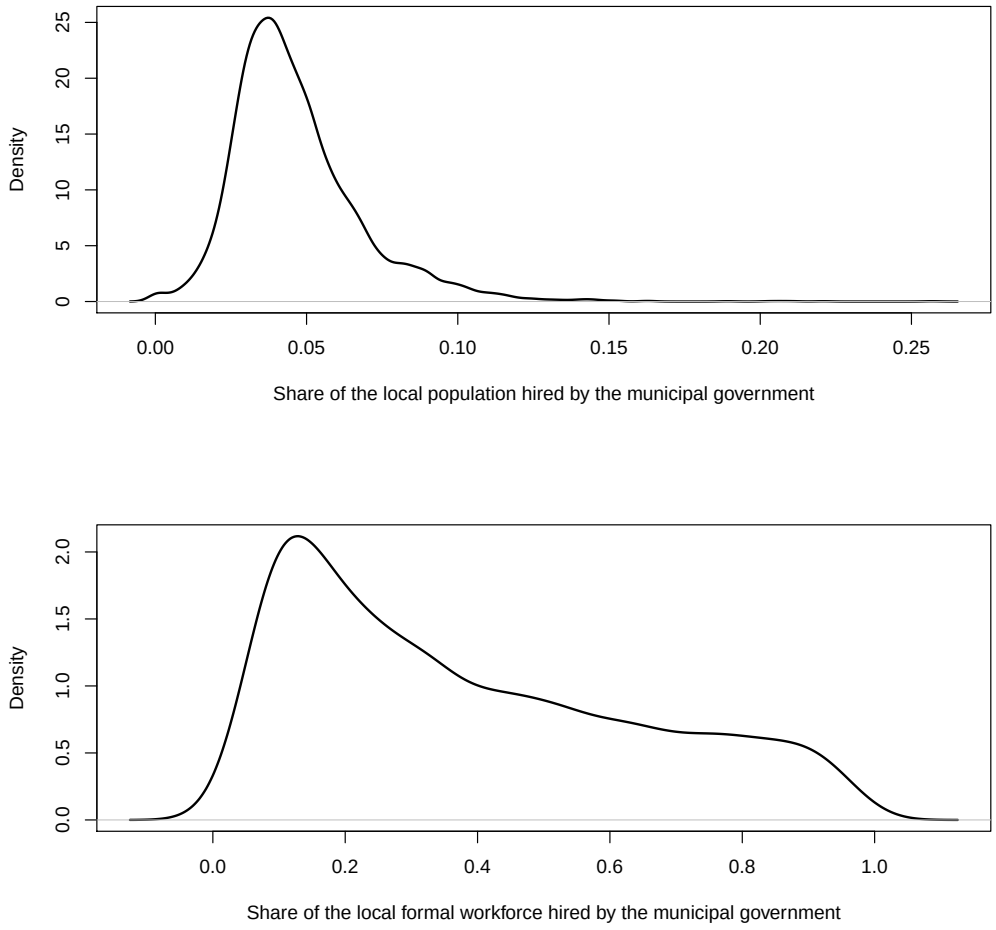
Appendices

A Additional details of the institutional context

A.1 Municipal government workforce as a share of overall population and overall formal workforce

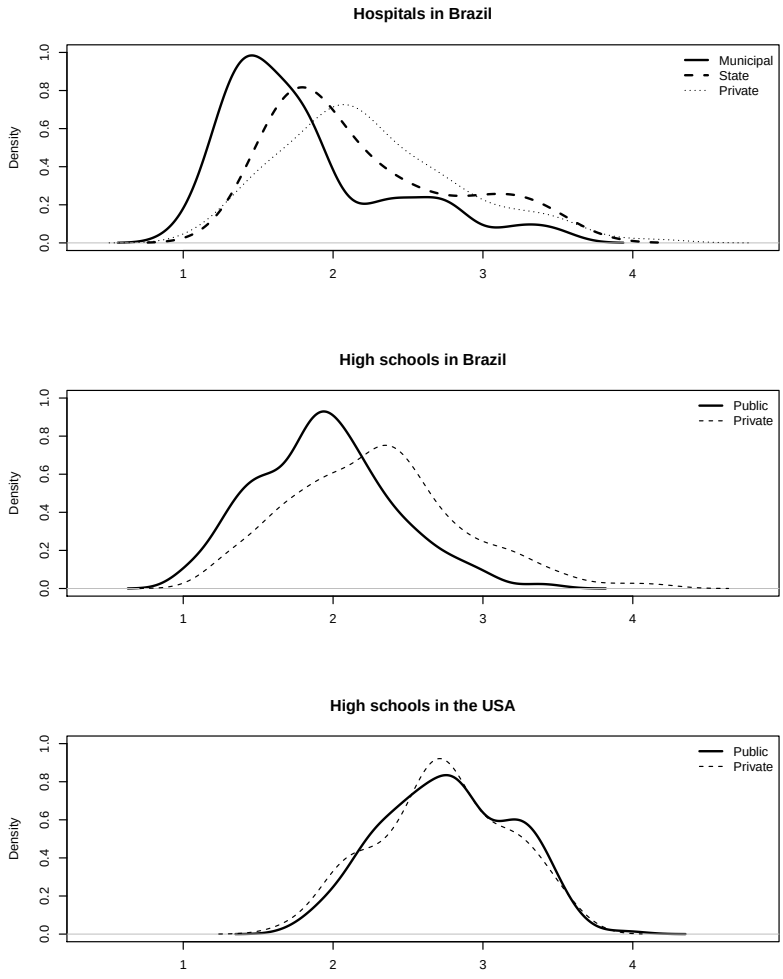
Figure 6: Size of the municipal government workforce as a proportion of the total local population (above) and the total formal labor market workforce (below) in 2016



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 7: Scores of the World Management Survey for hospitals and high schools in Brazil, and for high schools in the USA



Data are from Bloom et al. (2014, 2015) and 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. I gratefully acknowledge the authors' granting me access to the data.

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.

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

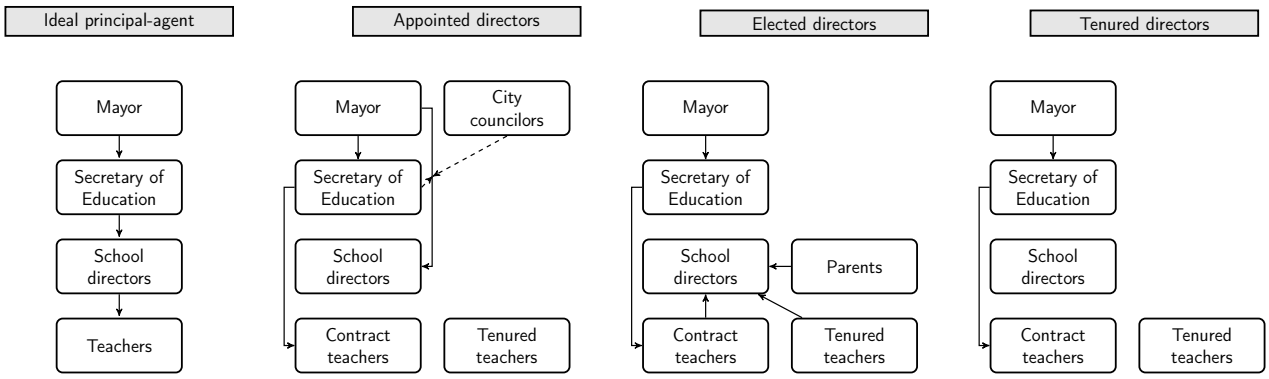
Table 4: Observational predictors of school director appointment modes and school quality test score (IDEB), from cross-section data on municipalities, schools, and directors (2013)

	Dependent variable:				
	Appointed director	Elected director	Tenured director	School quality score	
	(1)	(2)	(3)	(4)	(5)
rural	0.013 (0.010)	−0.008 (0.009)	0.003 (0.005)	−0.044** (0.016)	−0.045** (0.015)
log_workers	0.007 (0.009)	0.014 (0.008)	−0.014** (0.005)	−0.068*** (0.014)	−0.071*** (0.013)
in_assentamento	−0.003 (0.027)	−0.019 (0.024)	0.002 (0.014)	−0.022 (0.044)	−0.031 (0.042)
in_indigenous	−0.174 (0.101)	0.059 (0.089)	0.042 (0.053)	−0.481** (0.176)	−0.465** (0.159)
inse	0.001 (0.001)	−0.001 (0.001)	−0.00002 (0.001)	0.073*** (0.002)	0.072*** (0.002)
complexidade	−0.010*** (0.003)	0.009*** (0.002)	0.001 (0.001)	−0.023*** (0.004)	−0.024*** (0.004)
log(num_alunos)	−0.004 (0.006)	0.004 (0.006)	−0.001 (0.003)	0.052*** (0.010)	0.058*** (0.010)
logpopulation	−0.063*** (0.005)	0.049*** (0.004)	−0.002 (0.002)	−0.039*** (0.007)	−0.038*** (0.007)
bf_fam_to_pop_ratio	−0.088 (0.167)	−0.329* (0.148)	0.348*** (0.088)	1.696*** (0.251)	1.247*** (0.238)
household_monthly_pc_income_2010	−0.0003*** (0.00003)	0.0002*** (0.00002)	0.00001 (0.00001)	−0.00002 (0.00004)	−0.0001 (0.00004)
share_concursados	−0.017 (0.019)	0.045** (0.016)	0.007 (0.010)	0.069* (0.028)	0.053* (0.026)
share_enrolment_mun	0.109*** (0.024)	0.003 (0.021)	−0.060*** (0.013)	−0.0004 (0.036)	0.017 (0.034)
radios_2012	0.006 (0.004)	0.001 (0.003)	−0.001 (0.002)	0.001 (0.006)	0.002 (0.006)
mayor_reelected	−0.005 (0.008)	−0.005 (0.007)	0.017*** (0.004)	0.044*** (0.012)	0.056*** (0.011)
share_funcionarios	0.367 (0.266)	−0.512* (0.235)	0.041 (0.140)	−0.223 (0.400)	−0.186 (0.379)
herfindahl	0.139*** (0.026)	−0.081*** (0.023)	−0.086*** (0.013)	−0.054 (0.038)	−0.079* (0.036)
ideb_mun	0.003 (0.007)	−0.010 (0.006)	0.009** (0.004)	0.535*** (0.011)	0.551*** (0.010)
female	0.031** (0.010)	−0.019* (0.009)	−0.016** (0.005)	0.014 (0.016)	
age_25a29	0.048 (0.046)	−0.022 (0.041)	0.010 (0.024)	−0.006 (0.073)	
age_30a39	0.059 (0.044)	−0.021 (0.039)	0.002 (0.023)	−0.004 (0.070)	
age_40a49	0.065 (0.044)	−0.026 (0.039)	−0.003 (0.023)	−0.015 (0.070)	
age_50a54	0.054 (0.045)	−0.010 (0.040)	0.001 (0.024)	−0.045 (0.071)	
age_above54	0.088 (0.046)	−0.045 (0.040)	−0.005 (0.024)	−0.055 (0.072)	
race_black	−0.002 (0.008)	0.016* (0.007)	−0.010* (0.004)	−0.013 (0.012)	
race_brown	−0.012 (0.013)	0.020 (0.011)	−0.006 (0.007)	−0.041* (0.019)	
race_yellow	−0.049* (0.022)	0.042* (0.020)	0.002 (0.012)	0.108** (0.034)	
race_indigenous	−0.047 (0.041)	0.044 (0.036)	−0.013 (0.021)	−0.073 (0.061)	
race_notinformed	0.008 (0.044)	0.004 (0.039)	−0.042 (0.023)	−0.059 (0.065)	
schooling_lessthanhighschool	0.020 (0.094)	0.074 (0.083)	−0.081 (0.049)	−0.085 (0.144)	
schooling_magisterio	0.019 (0.038)	0.018 (0.033)	−0.047* (0.020)	0.064 (0.058)	
schooling_tertiary_pedagogy	−0.002 (0.035)	0.061 (0.031)	−0.057** (0.018)	0.091 (0.054)	
schooling_tertiary_normal	−0.018 (0.038)	0.085* (0.033)	−0.065** (0.020)	0.096 (0.058)	
schooling_tertiary_licenciatura	0.006 (0.036)	0.062 (0.031)	−0.061*** (0.019)	0.082 (0.055)	
schooling_tertiary_other	−0.012 (0.038)	0.053 (0.033)	−0.054** (0.020)	0.050 (0.058)	
schooling_noposgraduate	0.036 (0.028)	0.072** (0.025)	−0.114*** (0.015)	−0.0003 (0.043)	
schooling_atualizacao	0.038 (0.032)	0.073** (0.028)	−0.109*** (0.017)	0.026 (0.047)	
schooling_especializacao	0.010 (0.028)	0.073** (0.024)	−0.099*** (0.014)	0.053 (0.041)	
schooling_doctorate	−0.059 (0.075)	0.001 (0.066)	0.114** (0.039)	−0.071 (0.116)	
has_other_job_education	−0.006 (0.009)	−0.006 (0.008)	0.015*** (0.004)	−0.023 (0.013)	
has_other_job_noeducation	0.007 (0.013)	−0.015 (0.012)	0.007 (0.007)	−0.019 (0.020)	
works_morethan40h	0.004 (0.007)	0.005 (0.007)	−0.010** (0.004)	−0.006 (0.011)	
teacher_experience_lessthan1yr	0.086* (0.040)	0.020 (0.035)	−0.054** (0.021)	−0.034 (0.060)	
teacher_experience_1to2yr	0.114*** (0.032)	−0.005 (0.028)	−0.075*** (0.017)	−0.011 (0.049)	
teacher_experience_3to5yr	0.077** (0.029)	0.037 (0.025)	−0.078*** (0.015)	−0.007 (0.043)	
teacher_experience_6to10yr	0.031 (0.028)	0.079** (0.025)	−0.064*** (0.015)	−0.052 (0.042)	
teacher_experience_11to15yr	0.040 (0.028)	0.076** (0.025)	−0.065*** (0.015)	−0.074 (0.043)	
teacher_experience_16to20yr	0.053 (0.029)	0.072** (0.025)	−0.081*** (0.015)	−0.060 (0.044)	
teacher_experience_over20yr	0.073* (0.029)	0.061* (0.026)	−0.081*** (0.015)	−0.061 (0.045)	
director_experience_1to2yr	−0.063*** (0.014)	0.085*** (0.012)	−0.022** (0.007)	−0.003 (0.021)	
director_experience_3to5yr	0.026 (0.044)	−0.100* (0.039)	0.042 (0.023)	−0.098 (0.068)	
director_experience_6to10yr	0.023* (0.011)	−0.036*** (0.010)	−0.005 (0.006)	−0.010 (0.017)	
director_experience_11to15yr	0.057 (0.041)	−0.109** (0.037)	0.020 (0.022)	−0.041 (0.063)	
director_experience_16to20yr	−0.095*** (0.019)	0.029 (0.017)	0.065*** (0.010)	0.058* (0.029)	
director_experience_over20yr	0.029 (0.042)	−0.102** (0.037)	0.032 (0.022)	−0.020 (0.064)	
director_here_lessthan1yr	−0.001 (0.009)	0.009 (0.008)	0.004 (0.005)	−0.055*** (0.013)	
director_here_1to2yr	−0.147*** (0.013)	0.130*** (0.012)	0.041*** (0.007)	0.009 (0.020)	
director_here_3to5yr	−0.182*** (0.009)	0.196*** (0.008)	0.012** (0.005)	0.059*** (0.013)	
director_here_6to10yr	−0.254*** (0.013)	0.243*** (0.012)	0.052*** (0.007)	0.093*** (0.020)	
director_here_11to15yr	−0.257*** (0.017)	0.186*** (0.015)	0.100*** (0.009)	0.143*** (0.025)	
director_here_morethan20yr	−0.272*** (0.033)	0.197*** (0.030)	0.086*** (0.018)	0.130** (0.050)	
education_experience_1to2yr	0.009 (0.053)	−0.085 (0.047)	0.034 (0.028)	−0.138 (0.081)	
education_experience_6to10yr	0.056 (0.042)	−0.110** (0.037)	0.024 (0.022)	−0.064 (0.064)	
education_experience_16to20yr	0.059 (0.042)	−0.116** (0.037)	0.027 (0.022)	−0.032 (0.064)	
appointed				−0.016 (0.021)	−0.023 (0.020)
elected				−0.016 (0.023)	−0.005 (0.021)
tenured				−0.017 (0.028)	0.002 (0.027)
meta_2013				0.296*** (0.009)	0.299*** (0.009)
Observations	16,570	16,570	16,570	15,497	17,252
R^2	0.328	0.362	0.283	0.758	0.755

HC1 heteroskedasticity-consistent standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001

A.4 Accountability relationships by director appointment mode

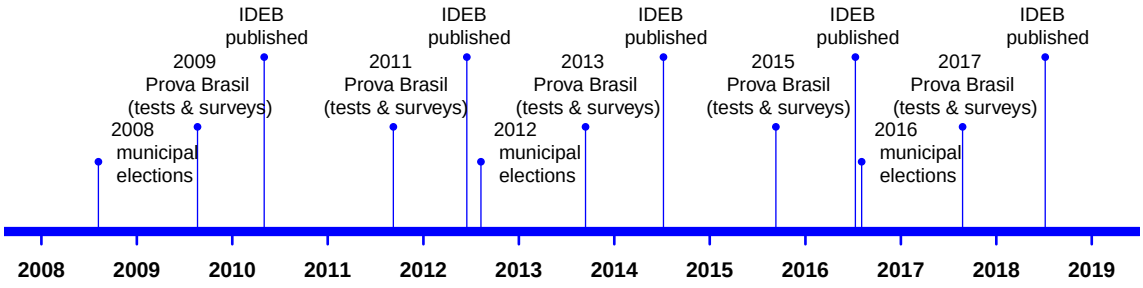
Figure 8: Four models of appointments in Brazil's municipal basic education sector: The ideal principal-agent model (left), and the actually existing models under political appointment, under election , and under civil service



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

A.5 Timeline of federal student testing and information release

Figure 9: Timeline of IDEB tests and information release



A.6 School selection into ANRESC

Schools that have less than 20 students enrolled in the grades to be assessed in a given year did not participate in ANRESC for the years I am examining. Moreover, IDEB results are released at the school level for every school where at least 50% of its enrolled students sit the exam. For schools that do not reach that minimum threshold, the Ministry publishes the information but hides the school identifier. While this may raise concerns that schools strategically select into or out of ANRESC, in practice this is very unlikely to matter for the result. As shown in the Figures below, very few schools do not reach these thresholds, and schools around that (rare) part of the distribution appear to actually select *into* ANRESC and not out of it. Together, this evidence suggests that self-selection into ANRESC is not a significant issue for the validity of the quasi-experimental designs.

Figure 10: Histogram of the number of students enrolled in the last grade of primary school, by school

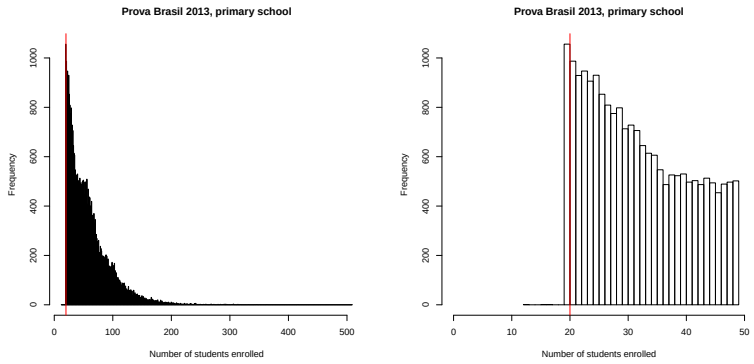
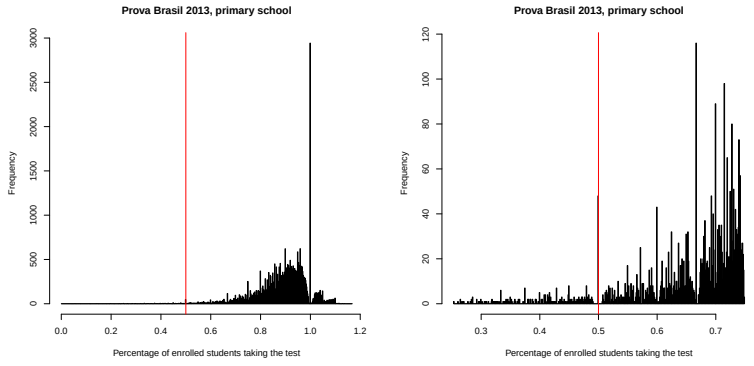


Figure 11: Histogram of the share of enrolled students who take the test, by school



On the left, histogram over the whole range of the variable. On the right, same histogram zoomed into around the discontinuity.

A.7 Illustrative materials for schools to prepare for student tests

The following resources illustrate a series of materials produced by governments and NGOs to help schools – and in particular school directors – prepare in the short term for the Prova Brasil 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 in 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 on 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 conduced 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.[44] Interviews were done in 45 municipalities in 7 states across 3 different regions of Brazil.[45] 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.[46] 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.

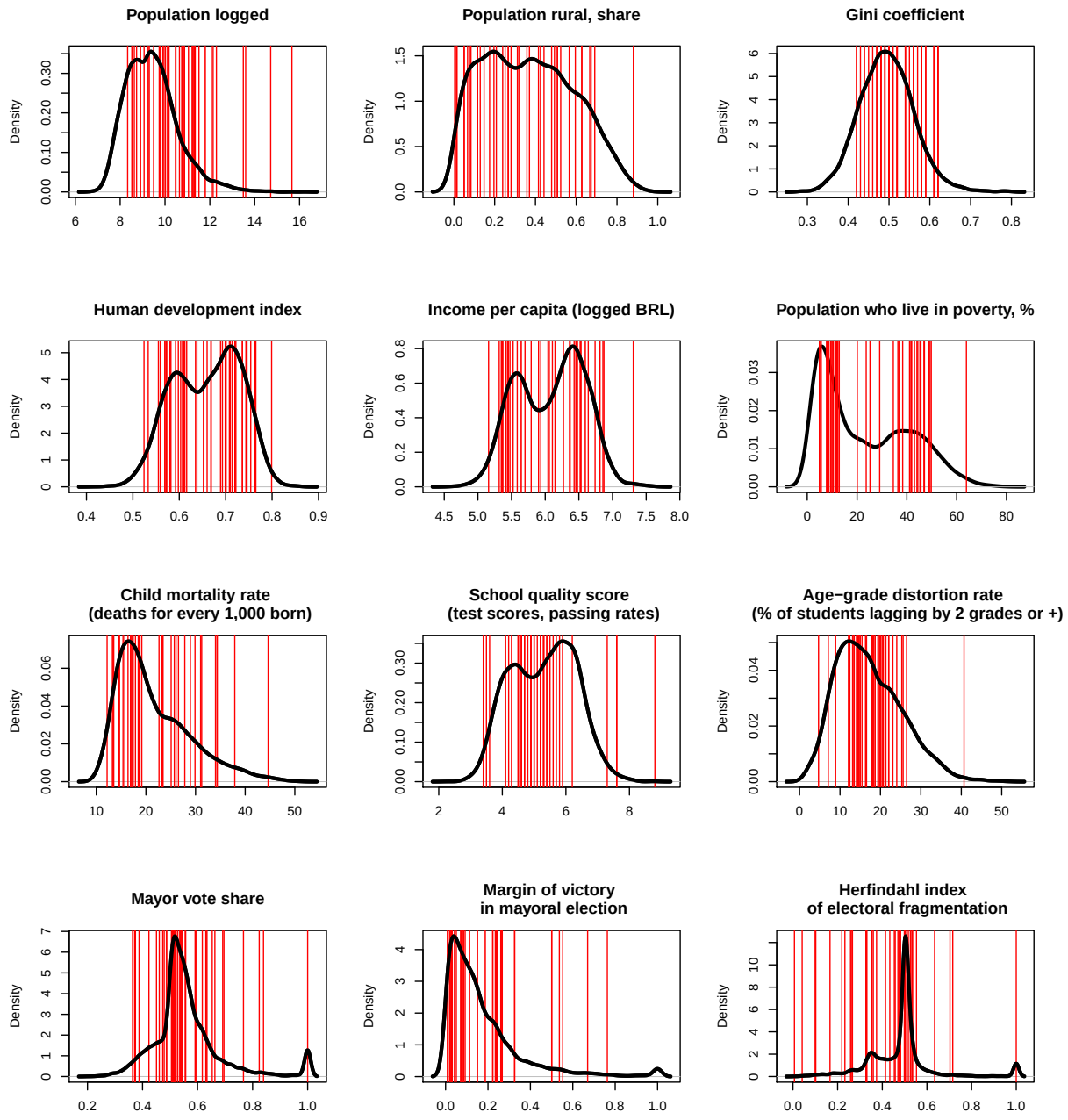
[44] 41 of of the 51 politicians were secretaries. 46 of the 54 bureaucrats were school directors, clinic managers, and social assistance center coordinators. Of the 16 horizontal accountability actors, 8 were state prosecutors or prosecutorial staff.

[45] 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).

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

Interview locations are quite diverse in their social, economic, and political characteristics, as shown in Figure 12.

Figure 12: 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 the forcing variable

Figure 13: Histogram of the forcing variable: IDEB target - IDEB score

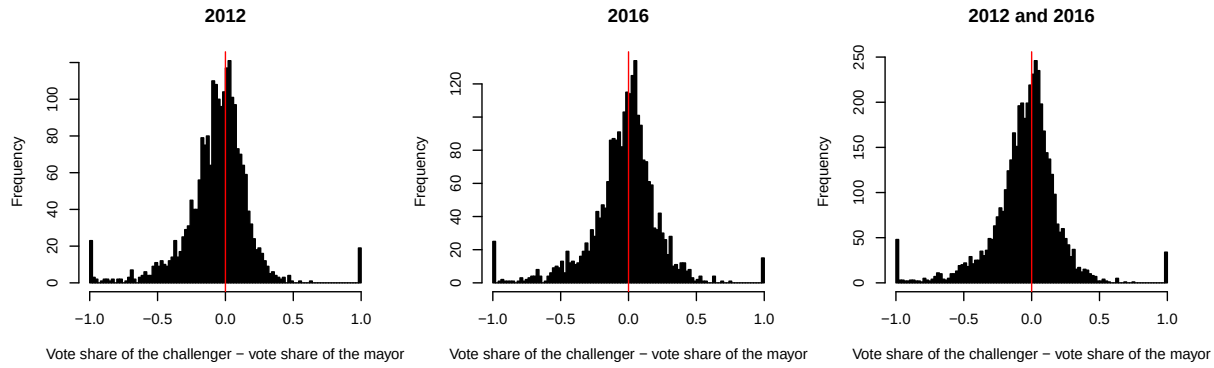
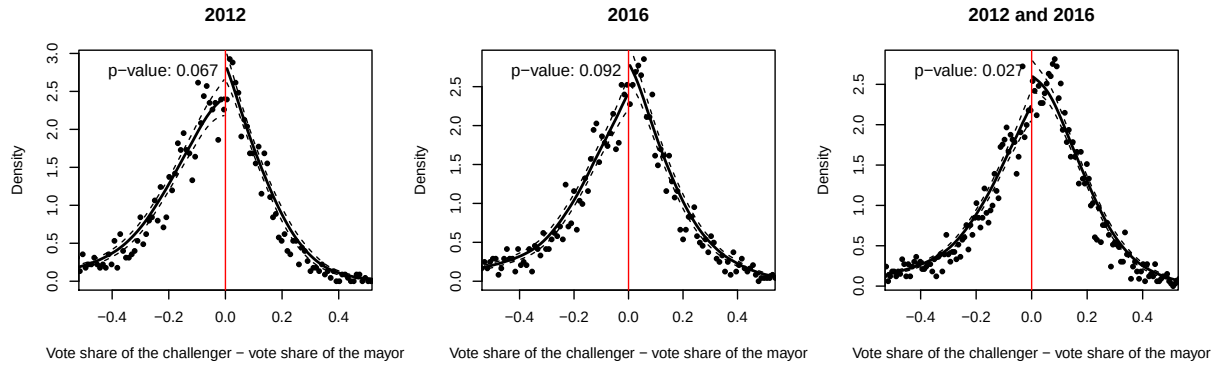


Figure 14: McCrary density test for discontinuity in the forcing variable



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 for data with 2012 and 2016. This may be due to Brazilian mayors' incumbency disadvantage (Klašnja and Titiunik, 2017). In any case, the key is that actors (in this case, mayors and their challengers) do not have precise manipulation of the forcing variable (Lee and Lemieux, 2010). An additional observable implication of the lack of precise manipulation assumption is that there should be no discontinuous jumps in covariates around the threshold, as shown in Appendix C.2.

C.2 Continuity in pre-treatment covariates

I check for balance replicating Equation 5 with pre-treatment data. These un-adjusted balance checks detect discontinuous jumps in 4 of 60 covariates (roughly what we would expect with an α of 0.05), 3 of which related to teacher and director experience.

Table 5: Balance in pre-treatment covariates at the school and municipality level

	RD estimate	Standard error	p value
ideb_pre	0.122	0.151	0.422
logpopulation	0.165	0.217	0.447
bf_families_ratio	-0.028	0.012	0.015
herfindahl_pre	0.019	0.027	0.484
household_monthly_pc_income_2010	59.294	35.604	0.096
share_concursados_pre	0.009	0.034	0.784
share_enrolment_mun_pre	-0.006	0.042	0.877
radios_2012	0.068	0.182	0.706
cod_incumbent_party	2.909	3.540	0.411
ideb_mun_pre	0.043	0.140	0.760
share_funcionarios	0.000	0.002	0.829
elected_pre	0.006	0.048	0.897
appointed_pre	-0.047	0.056	0.404
tenured_pre	0.006	0.015	0.690

Table 6: Balance in pre-treatment covariates at the director level

	RD estimate	Standard error	p value
director_here_lessthan1yr_pre	0.016	0.036	0.647
director_here_1to2yr_pre	-0.016	0.036	0.647
female	-0.025	0.027	0.352
age_below24	-0.005	0.008	0.509
age_25a29	0.017	0.014	0.233
age_30a39	0.002	0.029	0.938
age_40a49	0.017	0.027	0.537
age_50a54	-0.032	0.019	0.096
age_above54	0.003	0.012	0.776
race_white	-0.080	0.042	0.058
race_black	0.040	0.037	0.277
race_brown	0.002	0.017	0.926
race_yellow	0.011	0.009	0.218
race_indigenous	0.006	0.004	0.124
race_notinformed	0.003	0.003	0.284
schooling_lessthanhighschool	0.001	0.002	0.528
schooling_magisterio	-0.004	0.011	0.740
schooling_otherhighschool	-0.001	0.005	0.784
schooling_tertiary_pedagogy	-0.091	0.047	0.054
schooling_tertiary_normal	0.019	0.015	0.203
schooling_tertiary_licenciatura	0.042	0.034	0.219
schooling_tertiary_other	-0.028	0.022	0.199
schooling_noposgraduate	-0.032	0.039	0.412
schooling_atualizacao	-0.012	0.012	0.338
schooling_especializacao	0.054	0.042	0.198
schooling_masters	0.000	0.008	0.993
schooling_doctorate	-0.003	0.002	0.286
has_other_job_education	-0.017	0.029	0.546
has_other_job_noeducation	-0.006	0.014	0.650
works_morethan40h	0.042	0.038	0.269
teacher_experience_lessthan1yr	0.012	0.008	0.148
teacher_experience_1to2yr	0.002	0.015	0.888
teacher_experience_3to5yr	0.007	0.021	0.757
teacher_experience_6to10yr	0.072	0.029	0.013
teacher_experience_11to15yr	0.014	0.028	0.631
teacher_experience_16to20yr	-0.017	0.026	0.514
teacher_experience_over20yr	-0.062	0.030	0.036
director_experience_1to2yr	-0.086	0.043	0.046
director_experience_3to5yr	0.006	0.014	0.704
director_experience_6to10yr	0.026	0.013	0.051
director_experience_11to15yr	0.015	0.019	0.418
director_experience_16to20yr	0.001	0.008	0.906
director_experience_over20yr	-0.055	0.032	0.085
education_experience_1to2yr	0.005	0.006	0.442
education_experience_6to10yr	-0.020	0.023	0.379
education_experience_16to20yr	0.044	0.027	0.109

C.3 Pre-election trends

Figure 15: Pre-treatment trends between appointed and not appointed directors, within the RD bandwidth.

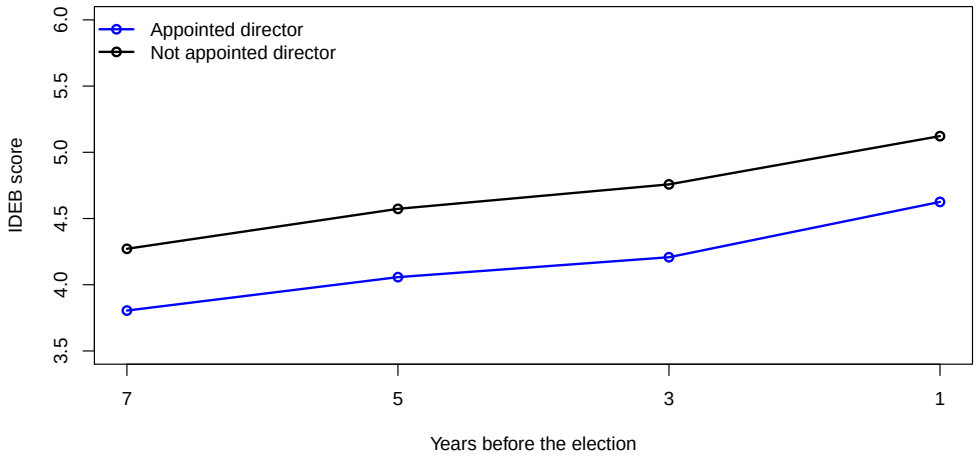
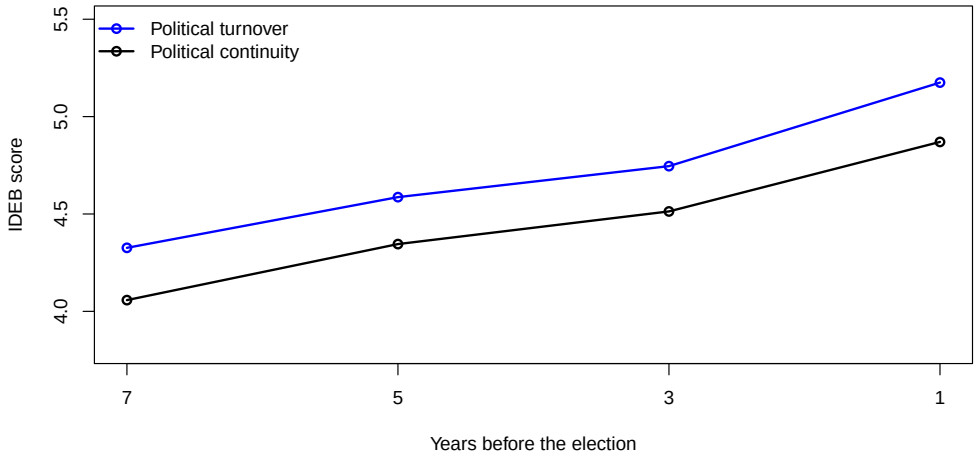
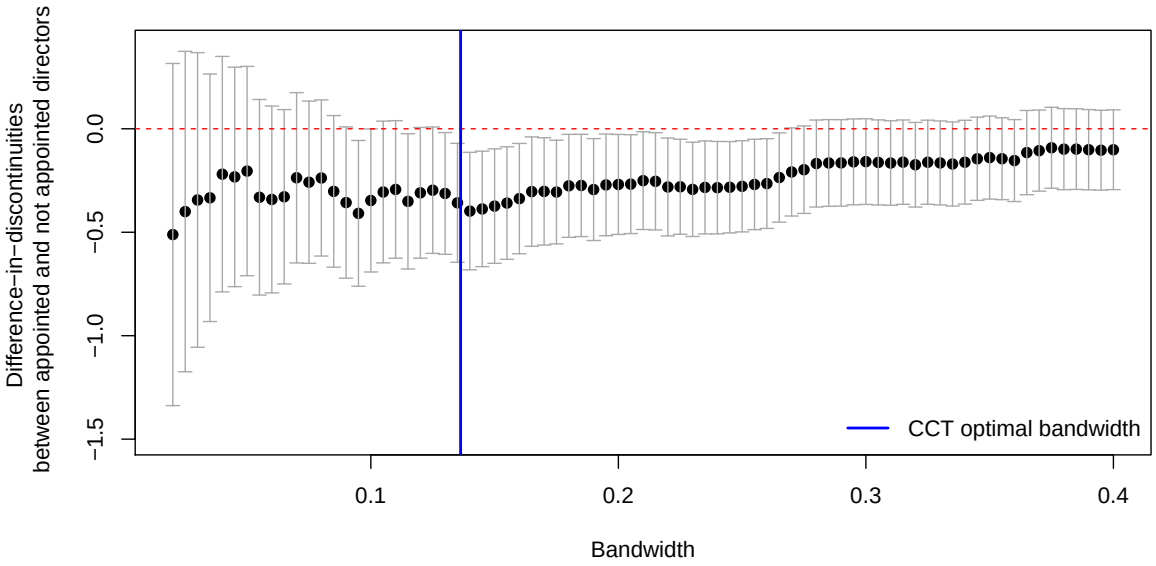


Figure 16: Pre-treatment trends between schools with and without political turnover, within the RD bandwidth.



C.4 Alternative bandwidths

Figure 17: Robustness of results in Model 1 in Table 2 to alternative bandwidths



C.5 Regression discontinuity estimates of the effect of political turnover on director turnover

Table 7: Effect of political turnover on bureaucratic turnover

	Model 1	Model 2	Model 3	Model 4
Political turnover	0.202***	0.175	0.164	0.148
	0.067	0.115	0.112	0.136
Political turnover × Appointed		0.068	0.077	0.075
		0.126	0.121	0.144
Election cycle fixed effects	✓	✓	✓	✓
State fixed effects			✓	✓
Predictors of Appointed				✓
Political turnover + interaction		0.243***	0.241***	0.223***
		0.063	0.064	0.074
Bandwidth	0.057	0.057	0.057	0.057
N	1727	1721	1721	972

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

Figure 18: Effect of political turnover on bureaucratic turnover, regardless of appointment mode

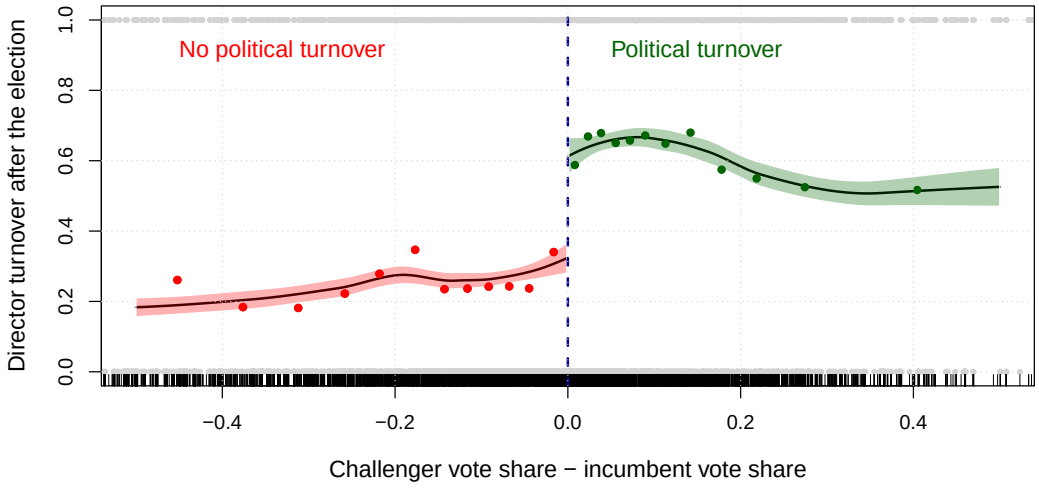
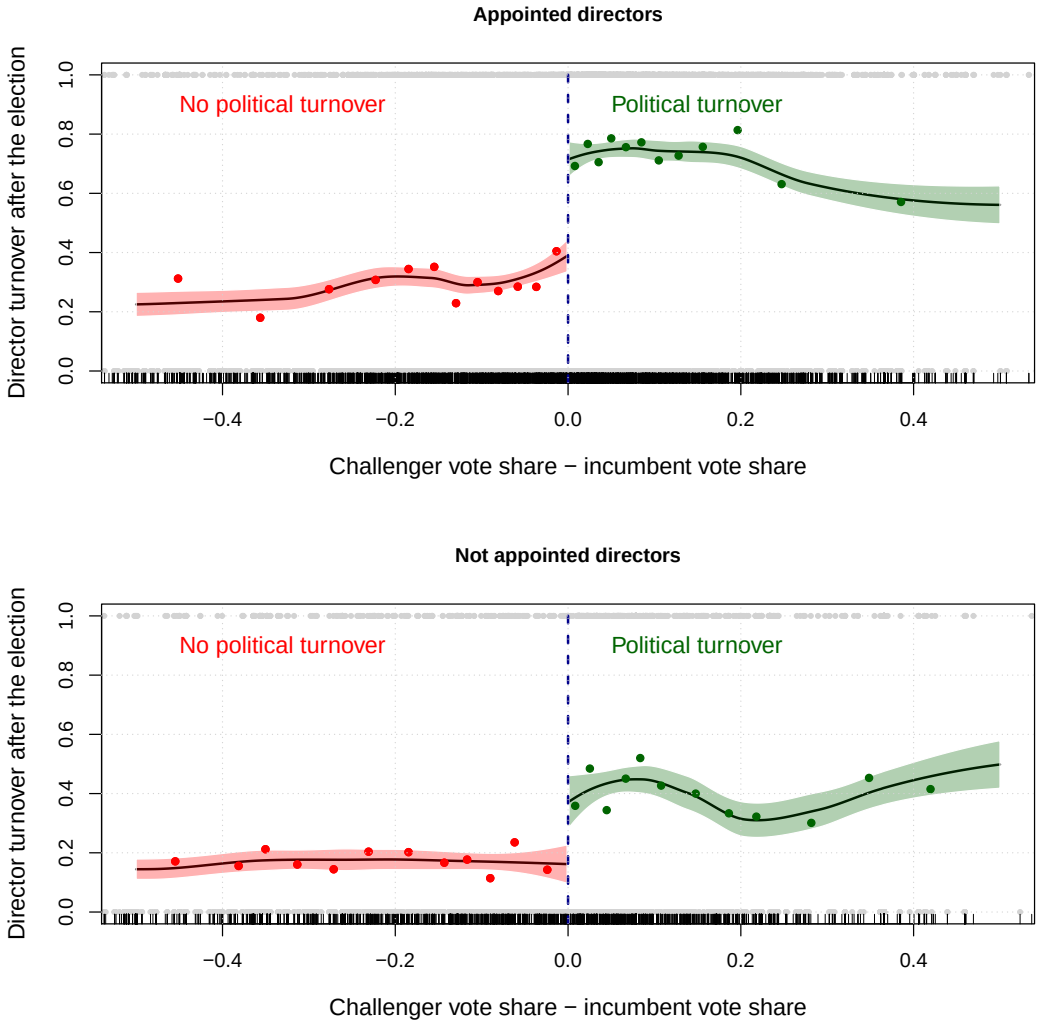


Figure 19: Effect of political turnover on bureaucratic turnover, by whether schools had appointed or not appointed director



Grey dots are school observations. 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 are their 95% confidence intervals.

C.6 Characterization of schools that experience director turnover under political turnover

Table 8: Regression of whether a school experiences director turnover after political turnover. Schools from municipalities without political turnover are excluded.

	Model 1	Model 2
appointed_pre	0.268*** (0.018)	0.269*** (0.018)
ideb_pre	−0.041*** (0.008)	
ideb_3quartile		0.015 (0.024)
ideb_2quartile		0.047* (0.025)
ideb_1quartile		0.122*** (0.026)
female	0.017 (0.022)	0.017 (0.022)
age_below24	0.067 (0.095)	0.073 (0.095)
age_25a29	0.052 (0.073)	0.058 (0.073)
age_30a39	0.038 (0.067)	0.044 (0.067)
age_40a49	0.047 (0.067)	0.051 (0.067)
age_50a54	0.034 (0.071)	0.040 (0.071)
age_above54	0.069 (0.074)	0.074 (0.074)
race_yellow	0.027 (0.095)	0.019 (0.095)
race_white	−0.038 (0.083)	−0.041 (0.083)
race_black	0.017 (0.083)	0.016 (0.083)
race_brown	−0.012 (0.087)	−0.011 (0.087)
race_indigenous	0.089 (0.162)	0.102 (0.162)
schooling_lessthanhighschool	0.211 (0.213)	0.198 (0.213)
schooling_magisterio	0.012 (0.066)	0.012 (0.066)
schooling_otherhighschool	0.050 (0.103)	0.046 (0.103)
schooling_tertiary_pedagogy	−0.094** (0.039)	−0.098** (0.039)
schooling_tertiary_normal	−0.068* (0.040)	−0.068* (0.040)
schooling_tertiary_licenciatura	−0.054 (0.035)	−0.055 (0.035)
schooling_tertiary_other	−0.051 (0.040)	−0.053 (0.040)
schooling_noposgraduate	0.073 (0.097)	0.078 (0.097)
schooling_atualizacao	0.076 (0.104)	0.082 (0.104)
schooling_especializacao	0.031 (0.096)	0.037 (0.096)
schooling_masters	−0.014 (0.112)	−0.009 (0.112)
schooling_doctorate	0.128 (0.217)	0.121 (0.217)
has_other_job_noeducation	0.017 (0.032)	0.016 (0.032)
has_other_job_education	0.010 (0.021)	0.010 (0.021)
works_morethan40h	−0.004 (0.019)	−0.004 (0.019)
director_experience_1to2yr	0.019 (0.024)	0.018 (0.024)
director_experience_3to5yr	0.006 (0.044)	0.010 (0.044)
director_experience_6to10yr	−0.018 (0.035)	−0.018 (0.035)
director_experience_16to20yr	0.036 (0.055)	0.042 (0.055)
director_experience_over20yr	0.009 (0.020)	0.007 (0.020)
director_here_1to2yr_pre	−0.042* (0.022)	−0.042* (0.022)
cycle_2016	0.017 (0.022)	0.015 (0.022)
Constant	0.597*** (0.147)	0.347** (0.145)
Observations	3,185	3,185
R^2	0.120	0.120

HC1 standard errors in brackets. *p<0.1; **p<0.05; ***p<0.01

C.7 Alternative estimation: Matching similar schools with and without political turnover

Table 9: Diff-in-disc estimates of the differential impact of political turnover on changes in school quality scores, by director appointment mode, as per Equation 5. Excludes schools in the no political turnover group without an exact match in the political turnover group.

	Model 1	Model 2	Model 3
Political turnover	0.298**	0.228	0.067
	0.138	0.140	0.152
Political turnover × Appointed	-0.469**	-0.443**	-0.397**
	0.183	0.177	0.189
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.146	0.146	0.146
	941	941	582

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

C.8 Bounds à la Lee (2009) to account for sample selection bias

C.8.1 The Lee (2009) approach to bounding treatment effects when there is sample selection bias

To deal with issues of sample selection bias, Lee (2009) proposes a simple procedure to generate bounds for 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 a outcome data ($S = 1$) because of their assignment to control:

$$p_0 = \frac{Pr(S = 1|D = 0) - Pr(S = 1|D = 1)}{Pr(S = 1|D = 0)} \tag{12}$$

- 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.

$$\Delta_0^{LB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \leq y_{1-p_0}] \tag{13}$$

- 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.

$$\Delta_0^{UB} = \mathbb{E}[Y|D = 1, S = 1] - \mathbb{E}[Y|D = 0, S = 1, Y \geq y_{p_0}] \tag{14}$$

- 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.8.2 Adaptation of the Lee (2009) procedure for the diff-in-disc

Lee makes it clear that his procedure can be applied to non-experimental settings (Lee, 2009, 1073). However my estimand 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 in order to produce sharp bounds on $\hat{\tau}_{ddisc}$

- I first simplify the design to a localized experiment based on local randomization instead of continuity. To do so, I focus exclusively on schools in the 0.01 bandwidth around the discontinuity (vs the optimal RD bandwidth of 0.136). This results in a much smaller dataset of 117 schools. Around this narrow threshold it is more sensible to treat the design as a localized experiment, such that we can simply compare units under mayor turnover and units under mayor continuity.
- Then I build four instead of two 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.5.
  - For appointed directors, I get $\hat{p}_0^a = 0.59$.
  - For unappointed directors, I get $\hat{p}_0^{\neg a} = 0.09$.
- With those probabilities, I trim the data for each subgroup, using the corresponding quantiles on the distribution of the change before and after the election in IDEB scores.
- 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, as well as a fixed

effect for the 2016 election cycle. As before, standard errors are clustered at the municipality level, where election results are defined.

$$Y_{smj} = \alpha + \beta_1 P_{mj} + \beta_2 A_{smj} + \beta_3 P_{mj} A_{smj} + \lambda \boldsymbol{I}[j = 2016] + \varepsilon_{smj} \tag{15}$$

- I do the same with the trimmed datasets for the upper bound.
- The $\hat{\beta}_3$ of each of the two regressions gives me the bounds for $\hat{\tau}_{ddisc}$.

Using this procedure within the 0.01 bandwidth, I get bounds [-0.907, -0.074].

C.8.3 Inference

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

1. I first draw, with replacement, a sample of *appointed* directors (with or without attrition) within the 0.01 bandwidth. The following steps take into account whether this sample has more director turnover in the treatment or in the control group, adjusting accordingly. For brevity below I describe the steps I take when the bootstrapped sample has more attrition in the mayor-turnover group (which is the case in 87% of the cases). With that data, I calculate p_0^a.
2. I then draw a sample with replacement from the set of schools that did not experience director turnover, within the 0.01 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.
3. As a result, I build a trimmed sample of appointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
4. 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.
5. I merge the adequately trimmed datasets for the lower bound on one hand, and for the upper bound on the other hand.

6. I estimate Equation 15 with each of the two datasets to estimate the difference in the treatment effect for appointed and unappointed directors.

7. I store the two values of $\hat{\beta}_3$ 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):

$$\left[\hat{\Delta}^{LB} - \bar{C}_n \times \frac{\hat{\sigma}_{LB}}{\sqrt{n}}, \hat{\Delta}^{UB} + \bar{C}_n \times \frac{\hat{\sigma}_{UB}}{\sqrt{n}}\right] \tag{16}$$

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

$$\Phi\left(\bar{C}_n + \sqrt{n}\frac{\hat{\Delta}^{LB} - \hat{\Delta}^{UB}}{max(\hat{\sigma}_{UB}, \hat{\sigma}_{LB})}\right) - \Phi(-\bar{C}_n) = 1 - \alpha \tag{17}$$

Following this procedure, I obtain a 95% confidence interval for the bounds of [-0.979, -0.024].

C.9 Alternative mechanisms: teacher effort and teacher supply

Table 10: Diff-in-disc estimates of the differential impact of political turnover on changes in director-reported problems of inssuficient teachers, by appointment mode, as per Equation 5.

	Model 1	Model 2	Model 3
Political turnover	0.048	0.092	0.061
	0.183	0.161	0.225
Political turnover × Appointed	-0.061	-0.098	-0.250
	0.223	0.213	0.283
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.160	0.160	0.160
N	1899	1899	1006

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

Table 11: Diff-in-disc estimates of the differential impact of political turnover on changes in director-reported problems of teacher turnover, by director appointment mode, as per Equation 5.

	Model 1	Model 2	Model 3
Political turnover	0.003	-0.021	0.096
	0.165	0.162	0.225
Political turnover × Appointed	0.017	0.036	-0.045
	0.233	0.236	0.320
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.156	0.156	0.156
N	1855	1855	984

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

Table 12: Diff-in-disc estimates of the differential impact of political turnover on changes in director-reported problems of teacher absenteeism, by director appointment mode, as per Equation 5.

	Model 1	Model 2	Model 3
Political turnover	-0.067	-0.070	-0.105
	0.127	0.130	0.172
Political turnover × Appointed	0.019	0.026	0.206
	0.188	0.189	0.222
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.191	0.191	0.191
N	2111	2111	1128

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

C.10 Placebo test with state schools

Table 13: Diff-in-disc estimates of the differential impact of municipal political turnover on changes in state school quality scores, by director appointment mode, as per Equation 5.

	Model 1	Model 2	Model 3
Political turnover	0.229	0.090	0.120
	0.155	0.148	0.212
Political turnover × Appointed	-0.067	-0.038	-0.484
	0.226	0.214	0.473
Election cycle fixed effects	✓	✓	✓
State fixed effects		✓	✓
Predictors of Appointed			✓
Bandwidth	0.092	0.092	0.092
N	618	618	145

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

D Additional details of the regression discontinuity

D.1 Continuity of the forcing variable

Figure 20: Histogram of the forcing variable: IDEB target - IDEB score

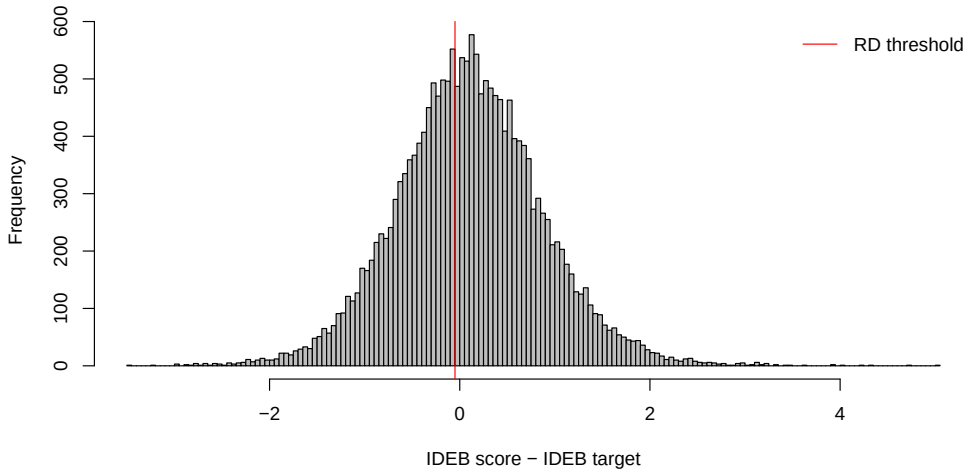
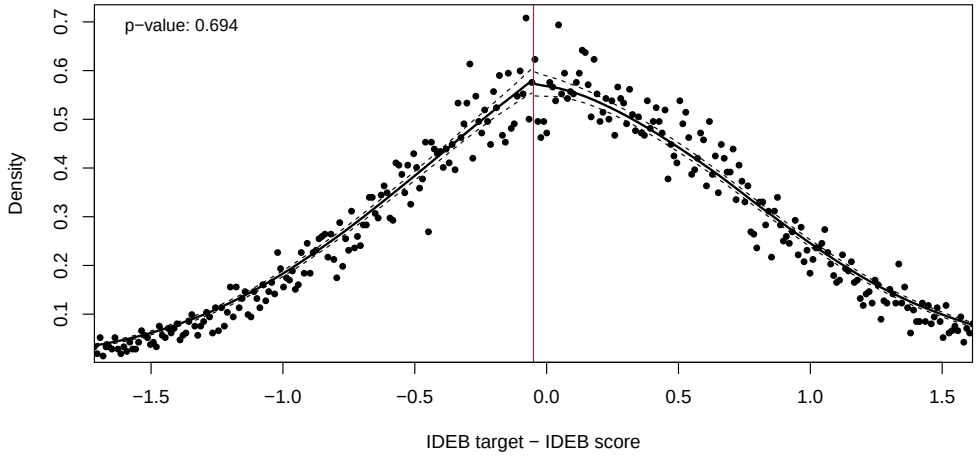


Figure 21: McCrary density test for discontinuity in the forcing variable



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 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). An additional observable implication of the lack of precise manipulation assumption is that there should be no discontinuous jumps in covariates around the threshold – Appendix D.2 presents a balance table examining this balance in pre-treatment covariates.

D.2 Continuity in pre-treatment covariates

Table 14: Continuity in pre-treatment covariates at the school level, estimated by applying Equation 8 with pre-treatment covariates as the dependent variable

	RD estimate	Standard error	p value
rendimento_2005	0.004	0.007	0.555
rendimento_2007	-0.003	0.005	0.554
rendimento_2009	0.000	0.004	0.969
rendimento_2011	0.002	0.003	0.565
nota_2005	0.030	0.034	0.377
nota_2007	-0.012	0.032	0.703
nota_2009	0.026	0.035	0.470
nota_2011	-0.012	0.032	0.701
ideb_2005	0.025	0.051	0.628
ideb_2007	-0.026	0.042	0.533
ideb_2009	0.025	0.046	0.582
ideb_2011	-0.005	0.040	0.902
rural	0.010	0.013	0.457
log_workers	-0.031	0.019	0.096
in_assentamento	0.004	0.005	0.400
in_indigenous	0.002	0.001	0.087
complexidade	-0.062	0.050	0.208
num_alunos	-5.308	3.105	0.087
inse	-0.236	0.217	0.277
distorcao	-0.292	0.498	0.558

Table 15: Continuity in pre-treatment covariates at the municipality level, estimated by applying Equation 8 with pre-treatment covariates as the dependent variable

	RD estimate	Standard error	p value
bf_fam_to_pop_ratio	-0.000	0.002	0.941
logpopulation	-0.073	0.066	0.265
household_monthly_pc_income_2010	-0.562	10.321	0.957
share_concursados	-0.000	0.007	0.971
share_enrolment_mun	0.001	0.008	0.932
radios_2012	-0.008	0.040	0.848
mayor_reelected	-0.018	0.016	0.249
share_funcionarios	0.001	0.001	0.078
ideb_mun	0.044	0.033	0.183
herfindahl	0.012	0.008	0.128

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

	RD estimate	Standard error	p value
female	0.002	0.012	0.858
age_below24	-0.002	0.003	0.469
age_25a29	-0.002	0.006	0.699
age_30a39	0.003	0.019	0.868
age_40a49	0.033	0.019	0.076
age_50a54	0.001	0.015	0.938
age_above54	-0.033	0.012	0.005
race_white	-0.014	0.019	0.457
race_black	-0.016	0.019	0.396
race_brown	0.016	0.010	0.131
race_yellow	0.010	0.006	0.078
race_indigenous	0.006	0.004	0.107
race_notinformed	-0.001	0.003	0.851
schooling_lessthanhighschool	0.001	0.001	0.606
schooling_magisterio	-0.001	0.008	0.910
schooling_otherhighschool	0.002	0.004	0.628
schooling_tertiary_pedagogy	-0.015	0.021	0.472
schooling_tertiary_normal	0.001	0.009	0.875
schooling_tertiary_licenciatura	0.018	0.021	0.387
schooling_tertiary_other	0.005	0.008	0.508
schooling_noposgraduate	-0.003	0.017	0.856
schooling_atualizacao	-0.005	0.008	0.526
schooling_especializacao	0.004	0.018	0.814
schooling_masters	0.002	0.005	0.741
schooling_doctorate	0.001	0.002	0.586
has_other_job_education	-0.005	0.015	0.723
has_other_job_noeducation	-0.004	0.009	0.619
works_morethan40h	0.036	0.018	0.043
teacher_experience_lessthan1yr	-0.000	0.004	0.931
teacher_experience_1to2yr	0.000	0.006	0.966
teacher_experience_3to5yr	-0.016	0.010	0.126
teacher_experience_6to10yr	-0.009	0.016	0.571
teacher_experience_11to15yr	0.019	0.016	0.236
teacher_experience_16to20yr	0.009	0.014	0.529
teacher_experience_over20yr	0.005	0.015	0.741
director_experience_1to2yr	-0.006	0.012	0.642
director_experience_3to5yr	-0.006	0.006	0.327
director_experience_6to10yr	0.018	0.013	0.162
director_experience_11to15yr	0.005	0.015	0.718
director_experience_16to20yr	-0.019	0.007	0.007
director_experience_over20yr	-0.000	0.017	0.987
education_experience_1to2yr	0.003	0.003	0.414
education_experience_6to10yr	0.015	0.013	0.229
education_experience_16to20yr	0.009	0.015	0.550
elected	-0.006	0.017	0.707
appointed	0.021	0.019	0.271
tenured	-0.027	0.012	0.026
selected_and_elected	-0.017	0.010	0.103
selected_and_appointed	0.002	0.008	0.826
other_mode	0.001	0.007	0.865
director_here_lessthan1yr	-0.021	0.014	0.127
director_here_1to2yr	-0.016	0.014	0.232
director_here_3to5yr	0.003	0.016	0.825
director_here_6to10yr	0.010	0.012	0.415
director_here_11to15yr	-0.018	0.008	0.020
director_here_16to20yr	-0.004	0.005	0.448
director_here_morethan20yr	-0.004	0.004	0.370

D.3 Additional RD plots

Figure 22: Effect of meeting the IDEB target on director turnover: elected directors

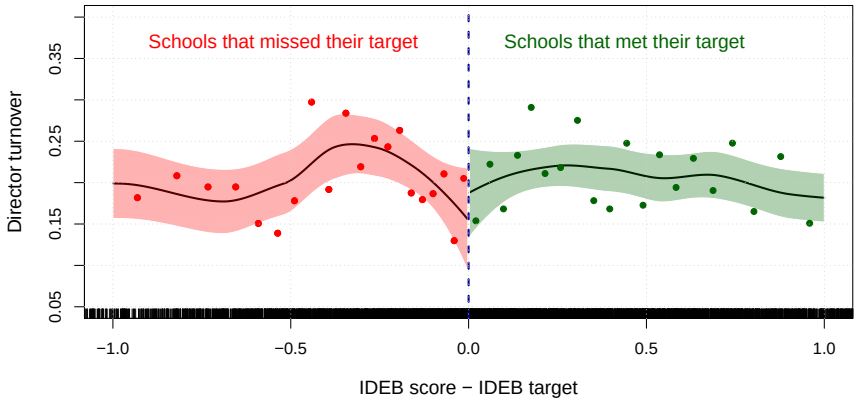


Figure 23: Effect of meeting the IDEB target on director turnover: tenured directors

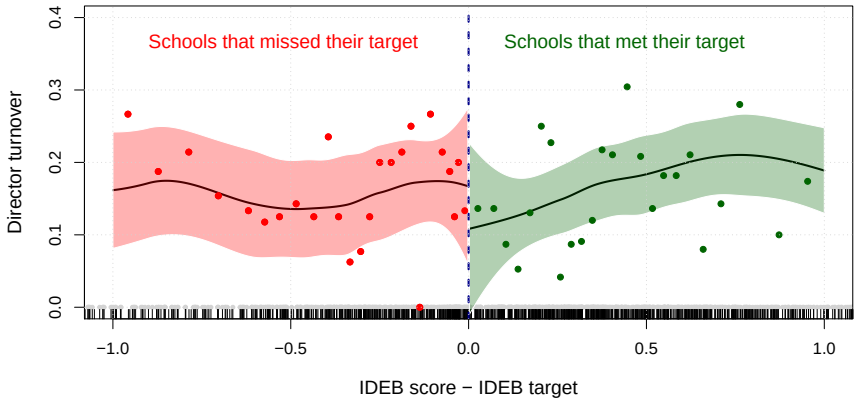
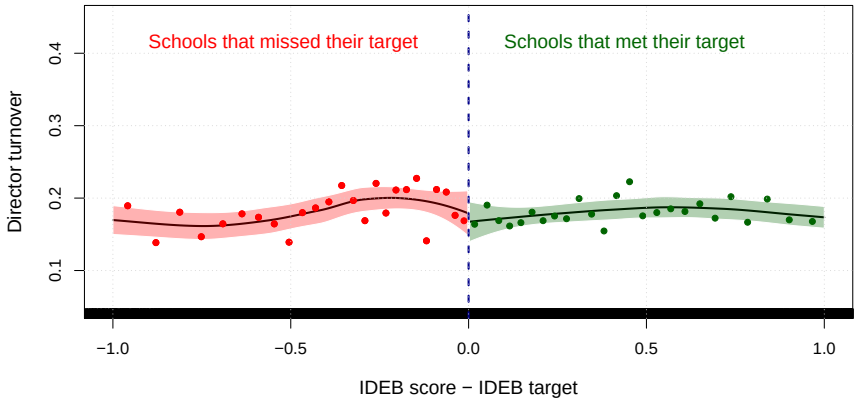


Figure 24: Effect of meeting the IDEB target on director turnover: all appointment modes



D.4 Alternative specification: Split sample

Table 17: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, among the set of schools that had an appointed director

	Model 1	Model 2	Model 3	Model 4
IDEB target met	-0.071***	-0.074***	-0.087***	-0.071***
	0.022	0.022	0.028	0.022
State fixed effects		✓		✓
Municipality fixed effects			✓	
Predictors of Appointed				✓
Bandwidth	0.525	0.525	0.491	0.491
N	4687	4687	4687	4332

Table 18: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, among the set of schools that had an elected director

	Model 1	Model 2	Model 3	Model 4
IDEB target met	0.036	0.040	0.044	0.040
	0.033	0.032	0.033	0.034
State fixed effects		✓		✓
Municipality fixed effects			✓	
Predictors of Elected				✓
Bandwidth	0.491	0.491	0.491	0.491
N	2262	2262	2262	2073

Table 19: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, among the set of schools that had a tenured director

	Model 1	Model 2	Model 3	Model 4
IDEB target met	-0.057	-0.039	-0.086	-0.001
	0.061	0.063	0.079	0.065
State fixed effects		✓		✓
Municipality fixed effects			✓	
Predictors of Tenured				✓
Bandwidth	0.426	0.426	0.426	0.426
N	536	536	536	470

Predictors of whether the director is tenured come from a regression of an indicator for tenured director on a long set of municipality, school, and director variables, as shown in Appendix A.3. HC1 heteroskedasticity consistent standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

D.5 Alternative specification: Treatment heterogeneity among elected and among tenured directors

Table 20: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, by whether the director is elected

	Model 1	Model 2	Model 3	Model 4
IDEB target met	-0.072***	-0.074***	-0.085***	-0.085***
	0.020	0.021	0.028	0.022
IDEB target met × Elected	0.115***	0.116***	0.137***	0.133***
	0.038	0.038	0.047	0.040
State fixed effects		✓		✓
Municipality fixed effects			✓	
Predictors of Elected				✓
IDEB target met + interaction	0.043	0.042	0.052	0.048
	0.032	0.032	0.033	0.033
Bandwidth	0.468	0.468	0.467	0.467
N	7470	7470	7470	6709

Table 21: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, by whether the director is tenured

	Model 1	Model 2	Model 3	Model 4
IDEB target met	-0.033*	-0.035*	-0.031	-0.033*
	0.018	0.018	0.024	0.019
IDEB target met × Tenured	-0.059	-0.057	-0.059	-0.060
	0.063	0.063	0.086	0.068
State fixed effects		✓		✓
Municipality fixed effects			✓	
Predictors of Tenured				✓
IDEB target met + interaction	-0.092	-0.092	-0.090	-0.093
	0.064	0.064	0.071	0.069
Bandwidth	0.470	0.470	0.467	0.467
N	7505	7505	7505	6740

Predictors of whether the director is tenured come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3.
All other terms in Equation 9 are omitted from the table.
HC1 heteroskedasticity consistent standard errors below coefficients. *p<0.1; **p<0.05; ***p<0.01.

D.6 What explains the null finding for elected directors?

The results of the RDD presented in Section 4.2 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.

The uncompetitive nature of director elections is probably not particular to Rio Grande do Norte. While it is surprisingly hard to find electoral data for school director elections, I found data on director election results in two large cities. In the municipal school director elections held in 2015 in Vitória da Conquista (the third largest city in the state of 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 the state of 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."[47] In practice, the results of the election are usually determined by teachers, especially tenured ones. These dynamics of capture are

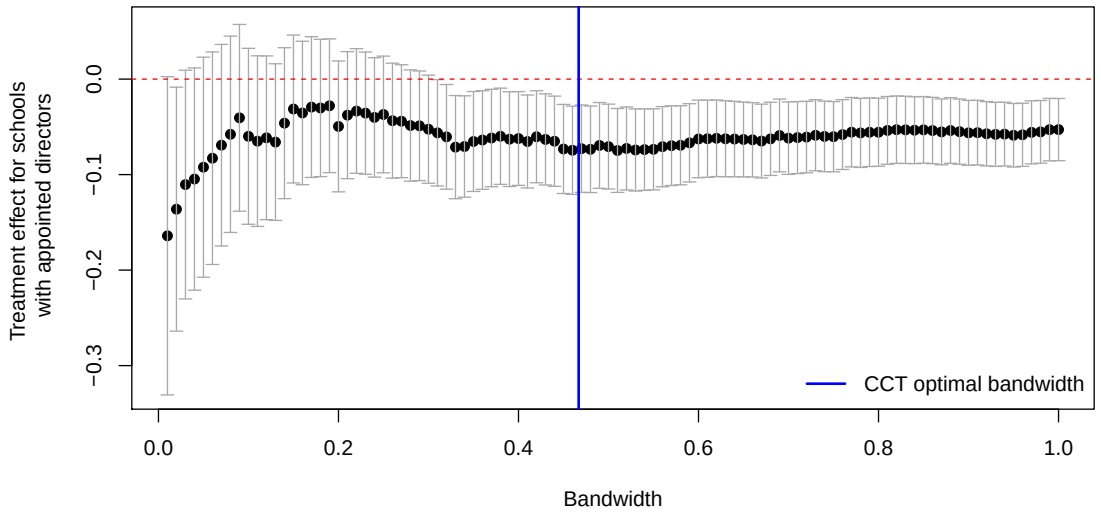
[47]School director interviewed in the state of Rio de Janeiro in February 2017.

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."[48]

[48]Secretary of education interviewed in the state of Rio de Janeiro in February 2017.

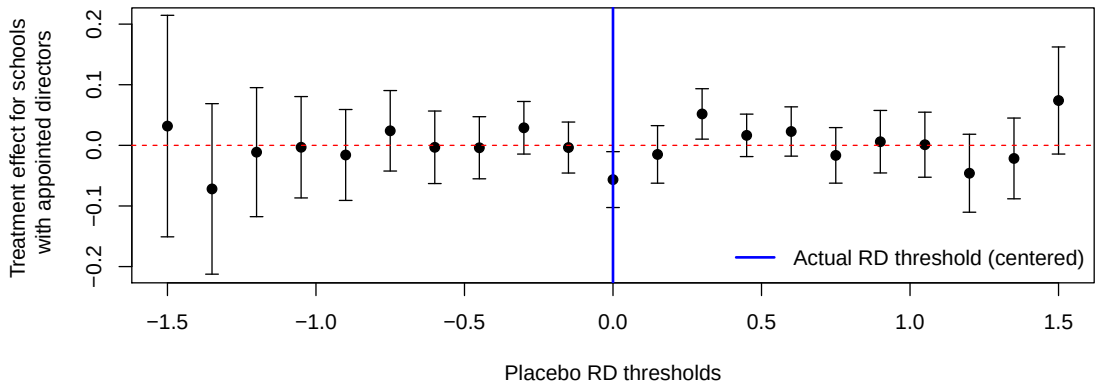
D.7 Alternative bandwidths

Figure 25: Sensitivity of model 3 in Table 3 to alternative bandwidths



D.8 Placebo tests varying the RD threshold

Figure 26: Placebo tests for model 3 in Table 3, moving the RD threshold to alternative values of the forcing variable



D.9 Alternative sample: Municipalities with mayors from programmatic parties

Table 22: Effect of reaching the primary school IDEB target in 2013 on school director turnover between 2014 and 2015, by whether the director in 2013 was appointed, subsetting to municipalities with a mayor from a programmatic party (PT or PSDB)

	Model 1	Model 2	Model 3	Model 4	Model 5
IDEB target met	-0.063**	-0.019	-0.021	-0.017	-0.029
	0.029	0.042	0.041	0.047	0.043
IDEB target met × Appointed		-0.078	-0.082	-0.062	-0.065
		0.059	0.059	0.071	0.061
State fixed effects			✓		✓
Municipality fixed effects				✓	
Predictors of Appointed					✓
IDEB target met + interaction		-0.097**	-0.103**	-0.079	-0.094**
		0.043	0.043	0.050	0.045
Bandwidth	0.524	0.523	0.523	0.523	0.523
N	2618	2507	2507	2507	2351

Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a long set of municipality, school, and director variables, as shown in Appendix A.3. HC1 heteroskedasticity consistent standard errors below coefficients.
*p<0.1; **p<0.05; ***p<0.01.

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

Survey instruments can be found online: school directors in Portuguese and in English, clinic managers in Portuguese and in English, social assistance center coordinators in Portuguese and in English

E.1 Location of the survey

Figure 27: Location of the face-to-face survey of street-level managers: Rio Grande do Norte, in blue; the Northeast region of Brazil, in grey

Figure 28: Municipalities in Rio Grande do Norte (colors correspond to the number of surveys done; white corresponds to municipalities excluded from the survey)

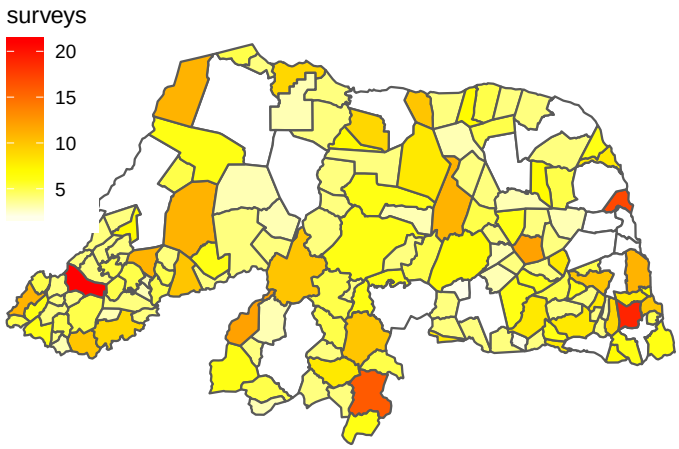
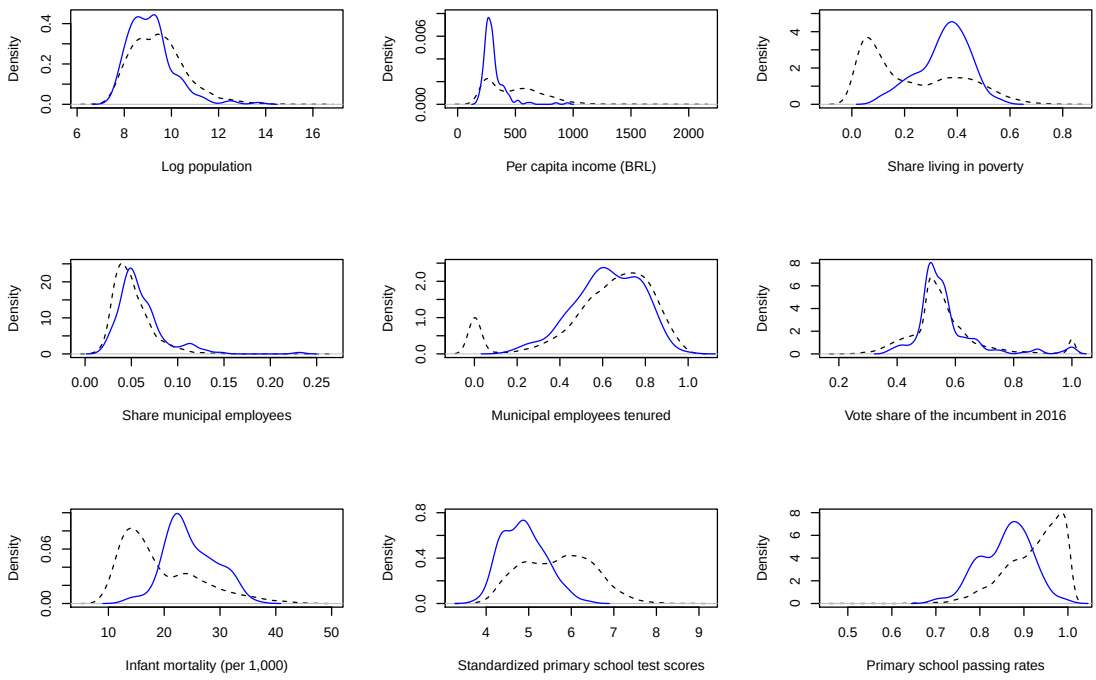


Figure 29: Statistics for municipalities in Rio Grande do Norte (continues, blue line), compared to all municipalities in Brazil (dashed, black line)



Data are from IBGE (Brazil's official statistics institute) and the Ministry of Education

E.2 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.

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 like Waze or Google Maps – and pose additional security challenges. Therefore, including rural areas in the sampling frame would have heavily increased the time and budget required for the survey, and could have risen security issues for enumerators. While there are many schools and clinics in rural areas, most of the population lives in urban areas and is thus served by urban bureaucracies. For example, while over 55% of the 2,415 municipal schools in Rio Grande do Norte are in rural areas, they concentrate less than 27% of municipal student enrollments in basic education.

Before the survey, and using the most up-to-date administrative data, I had 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 federal data. The break-up by sector is 481 school directors, 292 clinic managers, and 153 social assistance coordinators.

E.3 Descriptive statistics

Table 23: 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
Selected	0.04	0.21	0.00	0.00	0.11	0.31	0.06	0.24
Civil service	0.04	0.19	0.01	0.09	0.09	0.28	0.03	0.16
Appointed by mayor	0.46	0.50	0.54	0.50	0.34	0.47	0.46	0.50
Appointed by secretary	0.25	0.43	0.24	0.43	0.22	0.42	0.35	0.48
Appointed by city councilor	0.02	0.13	0.02	0.13	0.02	0.15	0.01	0.11
Experience in post	2.66	2.83	3.06	3.17	2.17	2.35	2.32	2.33
Experience as manager	4.66	4.33	5.58	4.79	3.98	3.71	3.05	2.97
Experience as professional	15.15	10.61	20.89	9.00	8.77	7.17	8.61	10.23
Hours worked per week	39.97	8.08	40.91	9.70	39.24	6.01	38.40	4.94
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

Table 24: Descriptive statistics of the survey of street-level managers, by appointment mode

	All modes N=926		Appointment N=710		Election N=82		Civil service N=41	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	40.71	9.76	41.14	9.61	46.43	8.49	34.67	9.02
Female	0.86	0.34	0.86	0.35	0.85	0.36	0.94	0.24
High school degree or less	0.08	0.27	0.09	0.29	0.02	0.16	0.03	0.17
College degree	0.31	0.46	0.35	0.48	0.10	0.30	0.18	0.39
More than a college degree	0.59	0.49	0.53	0.50	0.88	0.33	0.76	0.44
Experience in post	2.66	2.83	2.53	2.84	3.06	2.99	3.33	2.57
Experience as manager	4.66	4.33	4.58	4.35	4.78	4.52	6.52	5.58
Experience as professional	15.15	10.61	15.34	10.69	22.21	8.81	10.75	8.99
Hours worked per week	39.97	8.08	40.16	8.22	40.54	9.98	36.06	7.37
Exclusive dedication	0.57	0.50	0.60	0.49	0.76	0.43	0.21	0.42
Union member	0.35	0.48	0.31	0.46	0.74	0.44	0.48	0.51
Party member	0.16	0.37	0.17	0.38	0.15	0.36	0.09	0.29

Note that some street-level managers reported having been appointed through a variety of methods.

E.4 Results of observational regressions of appointment modes

Table 25: Observational regressions of street-level managers' appointment mode on political and socioeconomic characteristics

	Dependent variable: Manager is...		
	Politically appointed	Elected by community	Civil service regime
	(1)	(2)	(3)
Party member	0.055	−0.009	−0.007
	(0.039)	(0.026)	(0.020)
Union member	−0.159***	0.049*	0.066***
	(0.035)	(0.023)	(0.017)
Experience as manager	−0.006	−0.004	0.008***
	(0.004)	(0.002)	(0.002)
Experience as professional	0.001	−0.0001	−0.00004
	(0.002)	(0.001)	(0.001)
Lives in the municipality	0.180***	−0.051	−0.111***
	(0.045)	(0.029)	(0.022)
Has no other jobs	−0.075	−0.009	0.016
	(0.045)	(0.029)	(0.022)
Female	0.008	−0.019	0.020
	(0.042)	(0.027)	(0.021)
Age	0.004*	0.001	−0.004***
	(0.002)	(0.001)	(0.001)
Has more than a college degree	−0.122***	0.049*	0.009
	(0.032)	(0.021)	(0.016)
Has less than a college degree	0.098	0.029	−0.022
	(0.053)	(0.034)	(0.026)
Municipality FE	✓	✓	✓
Social sector FE	✓	✓	✓
Observations	838	838	838
R^2	0.423	0.498	0.282

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

E.5 Results of observational regressions of trust and attitudes

Table 26: 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	0.410**	0.972***	0.425**	−0.011	0.144	0.316
	(0.149)	(0.181)	(0.149)	(0.097)	(0.159)	(0.174)
Elected	0.266	0.889***	0.319	−0.008	0.094	0.327
	(0.179)	(0.217)	(0.179)	(0.117)	(0.191)	(0.209)
Controls	✓	✓	✓	✓	✓	✓
Municipality & sector FE	✓	✓	✓	✓	✓	✓
N	754	743	754	754	754	754
R^2	0.368	0.377	0.334	0.255	0.288	0.364

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

Table 27: 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					
	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	1.214***	1.237***	0.750***	0.786***	0.655***	0.886***
	(0.123)	(0.164)	(0.123)	(0.123)	(0.136)	(0.125)
Elected	0.805***	0.858***	0.507***	0.467**	0.468**	0.773***
	(0.148)	(0.197)	(0.148)	(0.147)	(0.164)	(0.151)
Controls	✓	✓	✓	✓	✓	✓
Municipality & sector FE	✓	✓	✓	✓	✓	✓
N	753	749	754	753	742	742
R^2	0.479	0.435	0.387	0.387	0.328	0.372

HC1 standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001
Controls include respondents' sector, age, gender, years of experience as professional in the sector years of experience as manager, party membership, union membership, whether they have less or more education than a college degree, whether they have other jobs, and whether they live in municipality where the unit (school/clinic/social assistance center) is located.

E.6 Details of the conjoint experiment with bureaucrats

Table 28: 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 no connections with the municipal government Has connections with the municipal government
Relationship to professionals	Bad relationship to professionals Good relationship to professionals
Unit performance	Targets were not met Targets were met
Selection mode	Civil service exam Election by the community Political appointment

Figure 30: Sample conjoint screen seen by school directors

Rodada 3 de 4

	Diretor A	**Diretor B**
Selecao	Eleição pela comunidade	Indicação política
Vinculos politicos	Tem vínculos políticos na prefeitura	Não tem vínculos políticos na prefeitura
Experiencia como diretor	3 anos	10 anos
Relacao com os professores	Fraca relação com os professores	Boa relação com os professores
Formacao	Mestrado	Licenciatura
Desempenho no IDEB	Meta da escola foi atingida	Meta da escola foi atingida

E.7 Results of conjoint experiment with bureaucrats

The next table details the regression results visualized in Figure 4. These correspond to the following choice tasks of the conjoint experiment:

- *Communication:* Which of these [directors/managers/coordinators] do you think would have a better communication with the Secretariat of [education/healthcare/social assistance]?
- *Implementation:* Which of these [directors/managers/coordinators] do you think would have more chances of implementing changes requested by the mayor's office?
- *Resources:* Which of these [directors/managers/coordinators] do you think would obtain a reform for the [school/clinic/social assistance center]?
- *Results:* Which of these [directors/managers/coordinators] do you think would achieve better scores in [student learning/community healthcare/social assistance center indicators]?

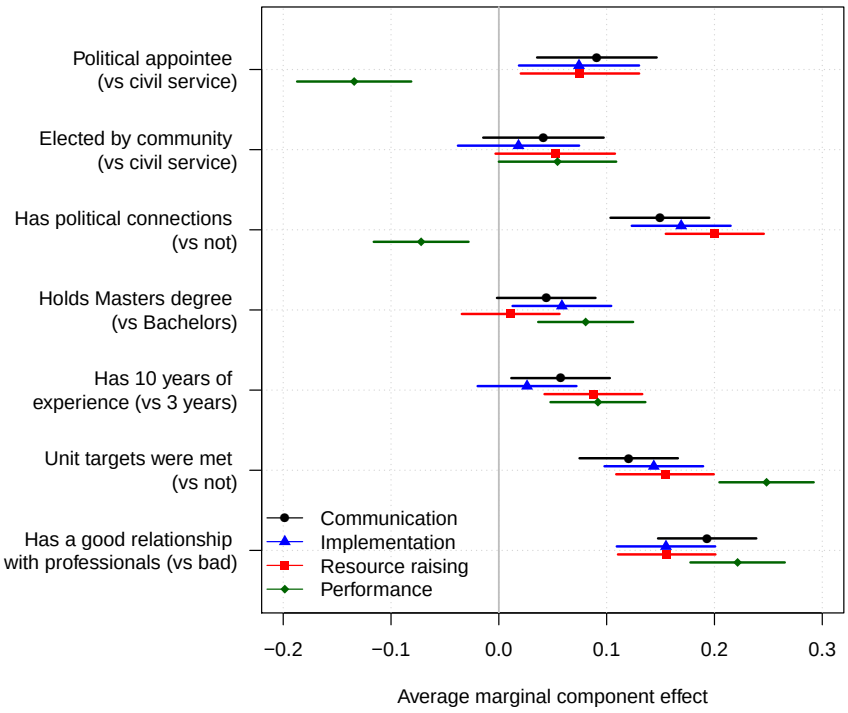
Table 29: Results of the conjoint experiment with street-level managers

	Communication	Implementation	Resources	Performance
Appointment: Civil service (baseline)				
Appointment: Political	0.106***	0.101 ***	0.087***	-0.062***
	0.014	0.014	0.014	0.013
Appointment: Election	-0.002	0.009***	0.020	0.052
	0.014	0.014	0.014	0.014
Political connections: No (baseline)				
Political connections: Yes	0.145***	0.168***	0.180***	-0.038***
	0.011	0.011	0.011	0.011
Education: Bachelors (baseline)				
Education: Masters	0.025*	0.018***	0.022*	0.063
	0.011	0.011	0.011	0.011
Experience: 3 years (baseline)				
Experience: 10 years	0.057***	0.039***	0.060***	0.062***
	0.011	0.011	0.011	0.011
Unit performance: Targets not met (baseline)				
Unit performance: Targets were met	0.136***	0.137***	0.141***	0.233***
	0.011	0.011	0.011	0.011
Relationship to professionals: Bad (baseline)				
Relationship to professionals: Good	0.193***	0.157***	0.159***	0.224***
	0.011	0.011	0.011	0.011
Number of respondents	917	917	917	917
Number of valid profiles	7224	7224	7224	7222

Estimates are average marginal component effects (AMCE). Standard errors clustered at the respondent level below coefficients. *p<0.05; **p<0.01; ***p<0.001.

E.8 Conjoint results among unappointed bureaucrats

Figure 31: Results from the face-to-face conjoint experiment with municipal street-level managers, excluding respondents who are political appointees



E.9 Conjoint results among bureaucrats who perceive politicians as more programmatic

Figure 32: Results from the face-to-face conjoint experiment with municipal street-level managers, excluding respondents who have lower perceptions of how programmatic politicians are

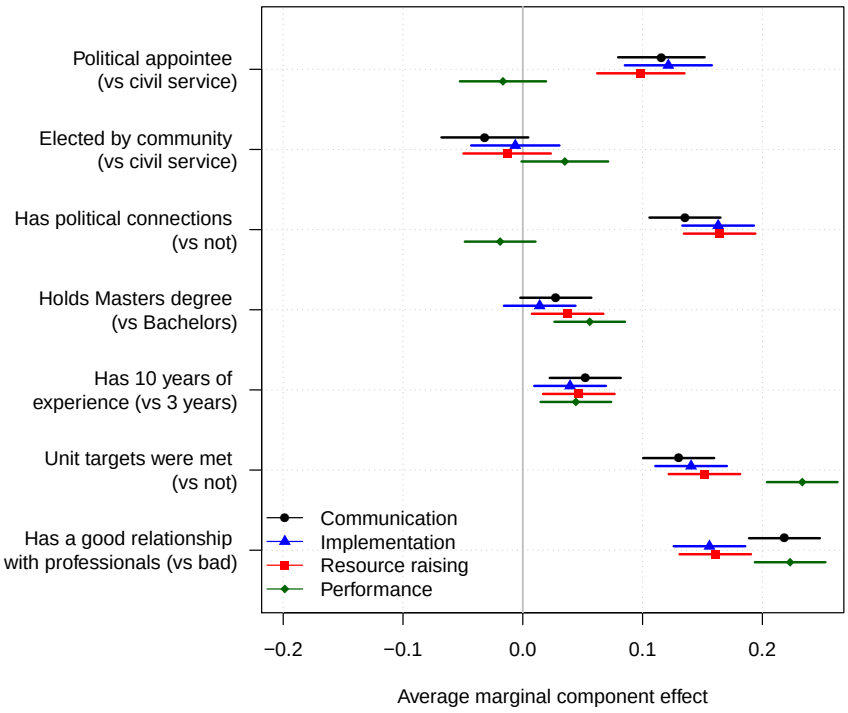


Figure 32 includes only responses from the 504 bureaucrats (54.4%) who expressed the highest level of agreement with the following statements:

- "The mayor and [education/healthcare/social assistance] professionals have the same priorities for [schools/clinics/social assistance centers]."
- "The mayor is concerned with improving the quality of [schools/clinics/social assistance centers]."
- "The secretariat of [education/healthcare/social assistance] holds this [school/clinic/social assistance center] accountable for its results."
- 'The secretariat of [education/healthcare/social assistance] helps us improve the performance of the [school/clinic/social assistance center]."

F Additional details of the online survey of politicians

The survey instrument can be found online in Portuguese and in English.

F.1 Respondent recruitment and non-response

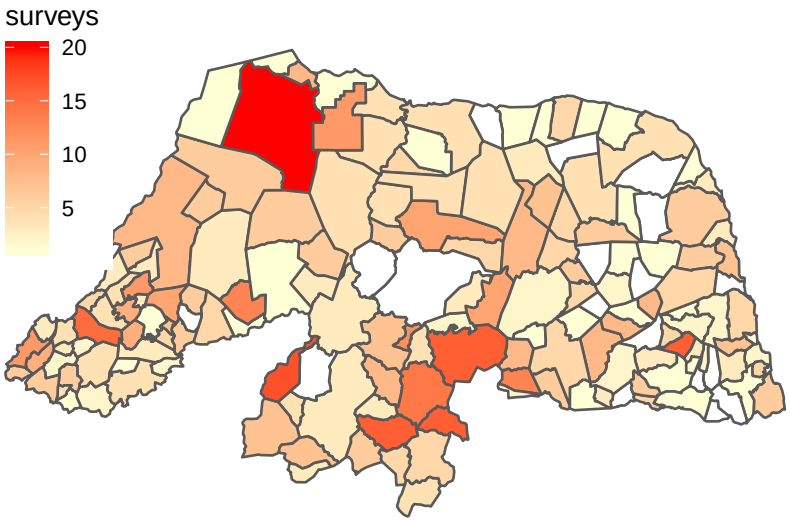
The state audit court of Rio Grande do Norte sent the survey to all mayors, city councilors, and secretaries of five key areas (education, healthcare, social assistance, finance, and human resources) in the 167 municipalities of the state. The survey was sent through the state audit court's online platforms. Because the theory in this paper is unrelated to city councilors, I exclude their responses for these analyses (although their inclusion leads to similar results). Politicians were encouraged to apply and reminded through a variety of means, but participation was voluntary. A total of 755 politicians participated and finished the survey, of which 56 were mayors, 468 secretaries, and 231 city councilors. These respondents come from 142 municipalities, out of the state's 167. There are no correlations between the number of participants and basic political and socioeconomic characteristics of the municipality.

Table 30: Correlates of the number of responses per municipality (excludes responses from city councilors, the number of which varies with the municipality's population)

logpopulation	−0.051 (0.246)
herfindahl	−3.803 (4.066)
mayor_reelected	0.278 (0.475)
mayor_voteshare	4.169 (4.020)
pc_pobres	−0.026 (0.027)
ideb	0.631 (0.380)
Constant	1.867 (3.573)
Observations	134
R^2	0.062

HC1 standard errors. *p<0.05; **p<0.01; ***p<0.001

Figure 33: Politician survey responses by municipality



F.2 Descriptive statistics

Table 31: Descriptive statistics for the survey of politicians, by position

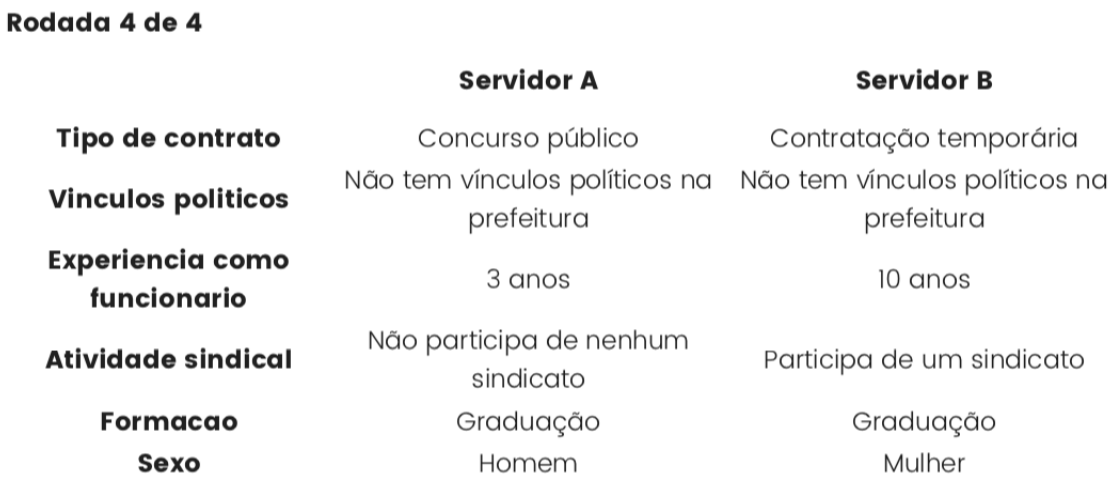
	All N=755		Mayors N=56		Secretaries N=468		City councilors N=231	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	43.37	10.92	49.88	11.86	41.69	10.42	45.22	10.82
Female	0.45	0.50	0.20	0.40	0.60	0.49	0.18	0.39
High school degree or less	0.22	0.41	0.38	0.49	0.07	0.26	0.48	0.50
College degree or more	0.65	0.48	0.52	0.50	0.82	0.39	0.36	0.48
Party member	0.66	0.47	0.98	0.13	0.46	0.50	0.98	0.15
Experience as bureaucrat (years)	0.64	0.48	0.38	0.49	0.76	0.43	0.46	0.50
Experience as politician (years)	5.56	5.65	7.64	7.21	4.34	4.45	7.53	6.65

F.3 Details of the conjoint experiment with politicians

Table 32: 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 no connections with the municipal government Has connections with the municipal government
Union membership	Participates in a union Does not participate in a union
Gender	Woman Man
Contract type	Civil service Temporary contract

Figure 34: Sample conjoint screen seen by politicians



F.4 Results of conjoint experiment with politicians

The next table details the regression results visualized in Figure 5. These correspond to the following choice tasks of the conjoint experiment:

- *Communication:* Which of these bureaucrats do you think would have a better communication with the local government?
- *Implementation:* Which of these bureaucrats do you think would have more chances of implementing changes requested by the local government?
- *Effort:* Which of these bureaucrats do you think would work extra hours if necessary?
- *Results:* Which of these bureaucrats do you think would achieve better performance?

Table 33: Results of the conjoint experiment with politicians

	Communication	Implementation	Effort	Performance
Contract: Civil service (baseline)				
Contract: Temporary	0.101***	0.137***	0.204***	0.065***
	0.018	0.018	0.016	0.017
Political connections: No (baseline)				
Political connections: Yes	0.128***	0.063***	0.065***	-0.008
	0.018	0.018	0.017	0.017
Education: Bachelors (baseline)				
Education: Masters	0.016	0.022	-0.015	0.094***
	0.016	0.016	0.016	0.017
Experience: 3 years (baseline)				
Experience: 10 years	0.038*	-0.006	-0.017	0.073***
	0.017	0.017	0.017	0.017
Union membership: No (baseline)				
Union membership: Yes	-0.030	-0.018	-0.051**	-0.003
	0.016	0.016	0.016	0.016
Gender: Female (baseline)				
Gender: Male	-0.051**	-0.077***	-0.057***	-0.105***
	0.016	0.015	0.015	0.016
Number of respondents	524	524	524	524
Number of valid profiles	4192	4192	4192	4192

Estimates are average marginal component effects (AMCE). Standard errors clustered at the respondent level below coefficients. *p<0.05; **p<0.01; ***p<0.001.