The benefits of patronage: How the political appointment of bureaucrats can enhance their accountability and effectiveness*

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Abstract

The political appointment of bureaucrats, a ubiquitous phenomenon around the world, 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. Patronage therefore works as a governance technology. In certain conditions, especially in developing contexts where politicians value the delivery of public services but cannot access other tools to motivate bureaucrats to perform, the benefits of political appointments may outweigh the costs. I test this theory with data on municipal governments in Brazil, leveraging two quasi-experiments with administrative data for schools in the whole country (a difference-in-discontinuities and a regression discontinuity); two original surveys including conjoint experiments in one state (a face-to-face survey of 926 street-level managers and an online survey of 755 politicians); and 121 in-depth interviews with bureaucrats, politicians and anti-corruption agents. The findings challenge the traditional view of patronage as universally detrimental for development, and draw attention to how bureaucrats and politicians can leverage political appointments and connections for public service delivery.

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1 Introduction

The political appointment of bureaucrats –or patronage, in short–[1] is a ubiquitous phenomenon, both in the developed and the developing world (Kopecky et al., 2012; Grindle, 2012; Dahlström et al., 2015). 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 a governance technology that, thanks to the social and political connections between bureaucrats and politicians, facilitates bureaucratic accountability and effectiveness. In particular, I argue that patronage gives 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. In certain contexts, patronage can improve public service delivery and citizen welfare by making bureaucrats more accountable and effective.[3]

My argument is not that patronage is universally good, or that it comes with no costs. The governance technology provided by patronage can be mobilized for rent extraction, public service delivery, or both. The costs of patronage have long been recognized (Pollock, 1937), and we have good quasi-experimental evidence of how patronage can distort the allocation of public jobs and disincentivize bureaucratic effort and performance (Xu, 2018; Colonnelli et al., 2019; Barbosa and Ferreira, 2019). Beyond these long-noted costs of patronage for bureaucrat selection and effort, the theory and evidence I offer in this paper suggest other channels through which patronage can enhance rent extraction, related less to who is inducted into the bureaucracy and how much they work, and more to *how* they work. This governance technology, however, can be used not only

[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. On the narrower end of the spectrum, Stokes et al. (2013) use it to refer to the allocation of government jobs to party members. On the broader end, Scott (1972) uses it to refer to social exchanges between parties of unequal standing.

[2] Throughout the paper, 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.

[3] My argument differs from that of Voth and Xu (2019), who argue that patronage can improve selection when appointments are based on merit. My focus is on appointments based on political criteria.

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 substitutes for this governance technology. This is often the case in developing contexts, and in particular in poor and small localities, with 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[4] 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, especially in transaction-intensive services like healthcare and education, depends to a large extent 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 technology of patronage to be beneficial is that politicians value at least partly the delivery of public services, something that may hold in contexts with electoral accountability and strong oversight institutions.

The argument that patronage is beneficial for service delivery is not completely new, and builds on insights from political science, economics, and public administration. Previous research in political science has acknowledged the ambivalence of patronage and recognized its beneficial uses for party building (Sorauf, 1960; Huntington, 1968), integration of isolated communities into the nation (Weingrod, 1968), interest aggregation (Scott, 1969), political stability (Arriola, 2009), and state building (Grindle, 2012). In public administration there is a long tradition of research on the politicization of the bureaucracy in high-income countries, where politicization is often seen as a resource politicians use to improve their control over policy and implementation (Peters and Pierre, 2004; Kopecký et al., 2016; Bach and Veit, 2017) and to build party networks (Kopecky et al., 2012). The use of political appointments to increase policy control of agencies has been most thoroughly studied in the case of US Presidential appointments (Aberbach and Rockman, 2009; Lewis, 2011). In this tradition, a trade-off is commonly theorized between policy control and bureaucratic performance (Moe, 1985; Lewis, 2007, 2008; Hollibaugh, 2014). Finally, in economics there is some consideration of the theoretical possibility that patronage may improve politicians' ability to

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

deal with selection and agency problems, although no empirical evidence has been uncovered to support this idea (Xu, 2018; Colonnelli et al., 2019). 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. My argument contrasts with the line of thought in American politics and economics, in that patronage may help not only increase control or decrease agency losses, but also enhance bureaucrats' ability to do their job. Empirically, the paper contributes with causal evidence from a developing context. This is, to my knowledge, the first paper to provide causally identified evidence of the benefits of patronage.

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. First, I present results from 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 its quality (measured through students' academic performance), when compared to schools with un-appointed directors. This demonstrates that the connections that patronage facilitates increase bureaucratic effectiveness. A separate regression discontinuity design examines whether the performance of politically appointed bureaucrats affects their job security. If my theory is correct, we would expect to see politicians holding school directors accountable for their performance. This is exactly what the design uncovers: politically appointed school directors who meet their target in a highly visible school quality metric are less likely to be replaced, but meeting the target has no effect on the turnover of un-appointed school directors. This shows that patronage can enhance bureaucratic accountability.

I use two original surveys in one Brazilian state to document empirically the mechanisms through which patronage can enhance bureaucratic effectiveness and accountability. 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, higher levels of trust, and better alignment with politicians than un-appointed 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 a separate, online online survey of 755 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. 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 research in political science, economics, and public administration. First, the paper contributes to classical and emerging debates on bureaucratic politics and the so-called personnel economics of the state (Finan et al., 2016), and opens up new ways of understanding and connecting some of its recent empirical findings. 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 technology it provides. This opens new avenues of research on the conditions under which the costs or the benefits are likely to dominate, and connects the literature on patronage to the literature on the benefits of connections in both public and private organizations (Schneider, 1991; Khwaja and Mian, 2005; Bandiera et al., 2009; Brollo and Nannicini, 2012; Baldwin, 2013; Boas et al., 2014; Tsai and Xu, 2018). 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.

The paper also suggests some implications for policy makers working on public sector reform. The results presented here on the overlooked benefits of patronage imply 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 preceded 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 (Ujhelyi, 2014; Lichand et al., 2017; Gerardino et al., 2017; Weaver, 2018; Wang, 2019). Rather than reducing politician discre-

tion in the appointment of bureaucrats, the findings suggest three alternative and complementary avenues for improving service delivery. First, establishing formal and informal incentives for politicians to use their discretion, local knowledge, and local governance structures for the improvement of service delivery. Second, providing regular, credible, and visible measures of bureaucratic performance in order to ease the information constraints of politicians, bureaucrats, and citizens. Last, investing in the management skills of street-level managers, regardless of their appointment mode.

The rest of the paper proceeds as follows. Section 2 presents my theory of patronage as governance technology, and contrasts it with classical and recent research on bureaucratic politics. Section 3 discusses the formal and informal institutions of local governance, public service delivery, and bureaucrat appointments in Brazil. Section 4 presents evidence from two quasi-experiments and two surveys in support of the theory. Section 5 concludes by summarizing the findings and discussing their implications.

2 Theory

2.1 The standard view of patronage as rent-seeking

The political appointment of bureaucrats is usually seen as a clientelistic, rent-seeking strategy. Scholars of clientelism have long studied the critical role that jobs play in clientelistic equilibria (Wilson, 1961; Chubb, 1982; Auyero, 2001; Golden, 2003; Calvo and Murillo, 2004; Folke et al., 2011; Stokes et al., 2013). In fact, jobs may play a unique role in clientelistic arrangements since they constitute a targetable, credible, and reversible method for redistribution (Robinson and Verdier, 2013). Under this light, patronage is seen as hurting development, through mechanisms like the misallocation of public jobs (Xu, 2018) or reductions in bureaucratic effort (Callen et al., 2018).

Both Weberian and principal-agent models of public bureaucracies tend to see political connections between bureaucrats and politicians as detrimental for development, either because they hinder bureaucratic autonomy or because they limit politicians' ability to hold them accountable for their performance on the job. In the Weberian paradigm, it is precisely bureaucracies' isolation from political intervention, together with bureaucrats' vocation and professional norms, that ensures bureaucratic effectiveness (Weber, 1922; Johnson, 1982; Evans and Rauch, 1999; Carpenter, 2001; Cingolani et al., 2015; Dahlström and Lapuente, 2017). Implicit in this paradigm is the assumption

that politicians' interventions in the bureaucracy are oriented towards short-term political goals that are detrimental to development, if not towards mere rent extraction, as opposed to the long-term, development-enhancing actions of technical and capable bureaucrats.

The principal-agent paradigm, on the other hand, understands the relationship between bureaucrats and politicians through the lens of microeconomic theory. In the classic principal-agent problem, politicians and bureaucrats are seen as parties to a contract where politicians delegate tasks to bureaucrats (Tullock, 1967; Niskanen, 1971; Banerjee, 1997; Gailmard and Patty, 2012; Khemani et al., 2016). The principal-agent paradigm thus assumes a strict separation of roles between the two parties, with politicians seen as able and willing to foster better outcomes through interventions in the bureaucracy. In contrast to the Weberian paradigm, here it is bureaucrats who are seen through a pessimistic lens.[5] While founded on different theoretical assumptions, both models prescribe a separation between bureaucrats and politicians. In contrast, I argue that, at least in some contexts, political connections between them can be beneficial for development.

2.2 Patronage as governance technology

In contrast to these classical models of bureaucrat-politician relationships, while building on some of their insights, I advance a theory of when and how patronage can be beneficial for development. I start by proposing the concept of *upward embeddedness*, namely bureaucrats' political and social connections to politicians. The concepts of embeddedness (Granovetter, 1985) and embedded autonomy (Evans, 1995) are often used 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 a range of social and political connections within bureaucracies, including those based on partisanship (Grindle, 2012), family (Fafchamps and Labonne, 2017), ethnicity (Vanden Eynde et al., 2018) and membership in elite groups (Xu, 2018).

In essence, I argue that political appointments and/or connections make bureaucrats upwardly embedded, which provides a governance technology that can be beneficial for both bureaucrats

[5]This is well illustrated by how a recent article that uses the principal-agent framework models bureaucratic governance: "bureaucrats are agents who dislike exerting effort towards their jobs, but are motivated to work by politicians who may take punitive action against them" (Gulzar and Pasquale, 2017, 164).

and the politicians who oversee them. Depending on how this technology is used, patronage can enhance rent seeking and/or public service delivery. 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 effects below, discussing their relevance for the governance of bureaucracies.

First, bureaucrats with upward embeddedness have enhanced access to political leaders and can more easily obtain *material resources* for public service delivery, as well as *immaterial resources* like legitimacy and authority, which help mobilize and coordinate other bureaucrats. This facilitates their effectiveness at their job. 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 be a key ingredient for government effectiveness (Gulzar and Pasquale, 2017; Raffler, 2019).[6] Upward embeddedness also enhances bureaucrats' accountability to politicians, by facilitating both *formal and informal sanctions and rewards*. 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 much 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 the effectiveness of bureaucrats at delivering public services (Ashraf et al., 2014, 2018; Bertrand et al., 2019). Informal sanctions and rewards are also enhanced by upward embeddedness, thanks to shared social and political networks.

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; Zacka, 2017; Dasgupta and Kapur, 2019). In such challenging environments, alignment of bureaucrats' and politicians' priorities may facilitate implementation and improve service delivery. In fact, alignment has long been recognized as a driver of organizations' performance in the management literature (Biggs et al., 2014), and the importance of collective choice for policy implementation has recently been highlighted in political science (Williams, 2017; Gottlieb and Kosec, 2019). Upward embeddedness also works by *aligning the the incentives* of bureaucrats and politicians, given their shared fate. Unlike

[6] Brierley (2019b) on the other hand finds that when politicians are focused on extracting rents monitoring may fuel corruption. This contrast illustrates the logic of my theory of patronage as technology.

civil service bureaucrats, political appointees are usually fired after a government change, which aligns their incentives to the incumbent's. Recent research has shown that Indian bureaucrats respond to the dynamic incentives used by politicians when their re-election prospects are more certain (Nath, 2016), and Argentinean patronage employees internalize the incumbent's incentives for re-election (Oliveros, 2019).[7] Finally, and largely due to the shared political and social networks and the alignment of priorities and incentives, upward embeddedness fosters *mutual trust* between bureaucrats and politicians. Abudant evidence from psychology shows that trust has beneficial impacts for organizations, which work through multiple mechanisms like lower transaction costs and improved compliance (Kramer, 1999; Dirks and Ferrin, 2002). 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 the governance technology that patronage provides, since it can be mobilized for improving public service delivery, for extracting rents, or for both. 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, the theory implies that they can also make governments more effective at delivering public services. I do not claim that patronage does not have costs. Rather, my argument is that we have overlooked its benefits, and that in certain contexts these benefits may outweigh the costs. Under what conditions is this more likely to be the case?

2.3 Scope conditions

The benefits of patronage will be larger in contexts where potential substitutes for the governance technology it provides are not available. This is true in developing contexts with stricter financial constraints and less human capital, which means the government has drastic constraints to attract and motivate bureaucrats to perform. The benefits are more likely to be worth the potential costs[8] for the appointment of street-level managers who work in the delivery of complex public

[7]The importance of alignment between bureaucrats and politicians is also highlighted in the formal literature on delegation through the so-called "ally principle", by which politicians grant more discretion to bureaucrats as their policy preferences converge (Huber and Shipan, 2006; Fiva et al., 2019).

[8]Costs may include, for example, selecting less educated or less experienced bureaucrats, politicizing public administration, or strengthening the link between political and bureaucratic turnover.

services, because their activities can benefit more from this governance technology. The costs of patronage will be smaller in contexts where politicians value (at least partially) the delivery of public services. Where some or all of these conditions are not present, the net benefits of political connections are less likely to be positive. My theory of patronage therefore implies that its effects depend on the logic of appointments. In a broad sense, a variety of patronage strategies with divergent implications for development can be identified: whereas short-term, electorally-focused appointments are detrimental for service delivery (Toral, 2019b), the political appointment of street-level managers at the beginning of a mandate can enhance bureaucratic accountability and effectiveness.[9]

Governments are constrained in their capacity to use monetary incentives and market mechanisms to foster performance, because they face legal and political barriers and they often act precisely where markets fail (Wilson, 1989; Banerjee, 1997). Local governments in developing contexts have it more difficult, since they face severe financial constraints, and –at least outside large metropolitan areas– hire from a particularly limited pool of candidates with low levels of human capital. While higher wages have been shown to help attract more able people to bureaucratic positions, and to overcome some of the undesirability of remote locations (Dal Bó et al., 2013), local governments in developing countries often face dire financial constraints that prevent them from implementing these or other performance-enhancing policies like performance pay (Hasnain et al., 2014). In these difficult environments, the counterfactual to a political appointee is not necessarily the highly capable, autonomous and driven bureaucrat that the Weberian model envisions. Instead, without adequate human capital and incentives, bureaucrats may lack the capacity and/or motivation to overcome the challenges of the job.[10] In those settings, the governance technology provided by patronage can be particularly useful, to a point where benefits may outweigh the costs.

Street-level managers (e.g., school directors or clinic managers) working to deliver public services in challenging environments are particularly likely to benefit from the governance technology that patronage provides. The success of these managers, who can have large impacts on the quality of public services(Bloom et al., 2014, 2015; Dhuey and Smith, 2014; Tavares, 2015), depends largely on their ability to coordinate efforts and align a complex set of tasks to objectives that are multidimensional and hard to asess. In developing contexts, with less human capital, more

[9]Brierley (2019a) presents a similar argument of differentiating patronage strategies. Her logic is however opposite to mine: she finds that in Ghana politicians hire partisans for menial positions but select professionals on the basis of merit.

[10]Consider for example the evidence presented by Chaudhury et al. (2006), showing two-digit rates of absenteeism among bureaucrats in six countries in Asia, Africa, and Latin America.

rudimentary information systems, and dire financial constraints, the benefits of patronage can be particularly useful for bureaucrats in these managerial positions, especially in the provision of complex services like healthcare or education. These services depend heavily on discretionary and transaction-intensive work, thus making principal-agent problems more severe (Pritchett and Woolcock, 2004). To complicate things further for street-level managers in these policy areas, most of their subordinates (like teachers or nurses) work with very high levels of autonomy and discretion (Lipsky, 1980), often behind closed doors. 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.[11]

For the benefits of patronage to outweigh the costs politicians must be concerned at least partly with public service delivery. Without that, the governance technology of patronage is likely to be leveraged for rent extraction, for instance by using the bureaucracy to campaign or to target public services to core or swing constituents. A variety of reasons may make politicians value the delivery of public services, including intrinsic beliefs and norms (Habyarimana et al., 2018), electoral competition (Rosenzweig, 2015), fear of retrospective voting (Healy and Malhotra, 2013), or constraints imposed by horizontal accountability institutions like anti-corruption agencies (O'Donnell, 1998). In contexts where norms, competition, electoral accountability and/or external control increase politicians' valuing of service delivery, upward embeddedness is more likely to have net beneficial effects. The availability of regular and credible measures of bureaucratic performance can strengthen each and all of these sources of politicians' valuing of service delivery.

2.4 Observable implications

My theory of upward embeddedness and governance has a number of observable implications that I am able to test with the data I present in this paper. First, if the effectiveness of politically appointed bureaucrats relies partly on their connections to politicians, an electoral defeat of the government should hurt their effectiveness more than that of non-appointed bureaucrats. I test this through the difference-in-discontinuities design in Section 4.1. Second, if political appointments respond to a concern with public service delivery and not to mere rent extraction, and if upward embeddedness facilitates accountability, appointed bureaucrats should be more likely to be replaced when they underperform in service provision. I test this with the regression discontinuity design in

[11] In contrast, the effectiveness of street-level bureaucrats like teachers or doctors does not depend so much on their ability to coordinate efforts with other bureaucrats or with higher-ups. In general, these are highly autonomous workers that provide services directly to citizens, very often on their own.

Section 4.2. Third, politically appointed street-level managers should have closer connections to politicians than those who are not politically appointed. I empirically address this in Section 4.3 with observational data from the survey of bureaucrats. Last, if my theory is right, local actors should perceive political appointments and connections as enhancing bureaucratic accountability and effectiveness. I test this with conjoint experiments in the surveys of bureaucrats and politicians in Section 4.3. Table 1 synthesizes the links between the paper's theory and empirical tests.

Table 1: Mapping of theory to empirics

Theoretical elements	Test	Data	Section
Core arguments: Upward embeddedness facilitates...			
... bureaucratic effectiveness in service delivery	Diff-in-disc	Admin. school data	4.1
... bureaucratic accountability	RDD	Admin. school data	4.2
Mechanisms: Bureaucrats with upward embeddedness...			
... have higher levels of trust in, alignment with, and access to politicians	Correlations	Bureaucrat survey	4.3
... communicate better with and are more responsive to the government	Conjoint	Bureaucrat & politician surveys	4.3
... have more access to material resources	Conjoint	Bureaucrat survey	4.3
... exert more effort	Conjoint	Politician survey	4.3

3 Institutional context

3.1 Formal institutions in Brazilian local governments

Brazilian local governments are a particularly useful setting in which to study how political appointments and connections impact bureaucratic accountability and effectiveness. Brazil is a large country with wide variation in bureaucrat appointment systems and development outcomes. Conveniently, the federal government facilitates access to multiple administrative datasets about local bureaucracies and their performance. This makes it possible to design quasi-experimental studies that leverage large amounts of reliable data.

Brazil is a federal country where 5,570 municipalities are responsible for providing primary education, healthcare, and social assistance to over 200 million people. Municipalities spend over 57% of their revenue in education, healthcare, and social assistance (OCED, 2016).[12] Municipal governments, however, depend heavily on inter-governmental transfers and raise only a small frac-

[12]At a minimum, municipalities are constitutionally mandated to spend 40% of their revenue in education and healthcare.

tion of the revenue they spend (Arretche, 2004). The federal government uses rules, oversight and performance metrics to encourage a good use of its funds and progress towards national policy goals like improvements in student learning or basic healthcare. Financial constraints are usually dire. Employees' salaries are low[13] and sometimes they are paid with substantial delays (CNM, 2018). Because of municipalities' prominent role in service provision and because of the lack of opportunities in the private sector in most municipalities, 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.[14] From the point of view of the employer however, these are small labor markets with pretty limited human capital.[15] 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 even when the local government offers to hire with the strong labor protections of a civil service regime. Still, the challenges for human development are vast. For example, the average municipality in Brazil had as of 2010 an infant mortality rate of 19 deaths per 1,000 live births (compared to 4 in the average European Union country), a quarter of its population living below the poverty line, and over 59% of its 19-to-21-year-olds without a high school diploma.[16]

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 (Ferraz and Finan, 2008; Litschig and Zamboni, 2015). Federal and state governments also oversee municipal governments, especially on their use of transfers and on their performance in delivering public services that said

[13]As per my own calculations using administrative data on the universe of municipal employees, the median salary of a municipal employee was 1,763 Brazilian reais in 2016. This is equivalent to about two minimum salaries or roughly 445 US dollars with the January 2016 exchange rate. In general, street-level managers are paid only a little bit more than the street-level bureaucrats they coordinate, and interviewees complain that it is far from enough for the additional hours and responsibilities they take on.

[14]Calculated with administrative data of the universe of formal sector contracts in 2016. Details included in Appendix A.1.

[15]For example, management skills of public schools and hospitals are significantly lower than those in the private sector, and much lower than those in the United States, as shown in Appendix A.2 using data from Bloom et al. (2013, 2015).

[16]Data for Brazilian municipalities calculated from UNDP's Municipal Human Development Atlas (Pinto et al., 2013). Data for infant mortality rate in the EU in 2010 comes from the World Bank's World Development Indicators.

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 improvements – a phenomenon that has been called "performance federalism" in the US context (Kogan et al., 2015).

In order to deliver public services to the local community, municipal governments maintain a network of schools, health clinics, and social assistance centers. While there are strict legal provisions constraining the hiring of street-level bureaucrats,[17] the appointment of managers for schools, clinics, and social assistance centers is legally under the discretion of politicians. Street-level managers have traditionally been political appointees, although it is increasingly common for them to be deployed under alternative systems, including election by the community (where bureaucrats run for the managerial position), civil service (where bureaucrats are tenured for life after a competitive examination), or more or less meritocratic selection procedures (where there usually is a less rigorous selection followed by discretionary appointments without tenure). While civil service bureaucrats can generally not be fired, they can be relocated to a different unit. It is not rare for multiple appointment systems to coexist within the same municipality. Variation in appointment systems is largest in the education sector, where the quasi-experimental studies presented in this paper focus.

Brazilian basic education is structured in two cycles: primary school (grades 1 through 5) and middle school (grades 6 through 9). At both levels there are public schools managed by municipalities, states, and the federal government, as well as by private entities. Public schooling is much more common than private schooling, and within the public sector, municipal governments are mostly responsible for primary schools, while states are mostly responsible for middle schools and high schools. As per the 2018 school census, 81% of primary school students are enrolled in public schools, 83% of whom are in municipal schools.

Municipal school directors are most frequently political appointees, but other appointment modes exist, including election by the school community[18], civil service, some sort of merit-based selection. As of 2017, about 65% of municipal directors were appointed by politicians, 24% elected

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

[18]Transitions to director election systems appear fostered by a participatory norm, by federal and state government action, and by pressure from teacher unions. Previous research suggests government decisions to move away from patronage systems may be driven by electoral competition, party organization, political institutions, financial constraints, or societal demands (Schuster, 2016).

by the community, 5% tenured in a civil service regime, 3% selected through some meritocratic criteria, and the rest appointed through other means. Appendix A.3 shows the variables that are significantly associated to whether a school has an appointed director. Other things being equal, schools are more likely to have an appointed director in municipalities that are smaller, poorer, more electorally fragmented, with a larger share of the population employed by the municipality, and with a larger share of children enrolled in municipal schools. At the school level, it is schools with less organizational complexity that are more likely to have an appointed director. Appointed directors appear to have less experience. This result gives quantitative support to the idea that discretionary appointments in this context are usually based on political criteria and not merit.[19] Yet, after controlling for schools' socioeconomic context, director appointment modes are not correlated with school performance, as shown in Appendix A.3.

Municipal politicians in general value public service delivery, as evidenced by interviews, the regression discontinuity design presented in Section 4.2, and some descriptive statistics from my surveys of bureaucrats and politicians presented in Section 4.3. For example, a large majority of mayors 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., 2019).

3.2 Informal institutions in bureaucratic appointments, as gauged through in-depth interviews

Over 18 months of fieldwork done between January of 2016 and June of 2019, I conduced 121 in-depth, semi-structured interviews with municipal bureaucrats and politicians, and with state-level horizontal accountability actors (like auditors and prosecutors).[20] I conducted these interviews in Portuguese, face-to-face, at the office of the interviewee, and with no audio recording device. The choice to not record interviews responded to the fact that some of the topics discussed were highly sensitive, including corrupt and illegal uses of public employment. While recording interviews would

[19]Further support comes from my survey of street-level managers in education, healthcare, and social assistance one state, detailed in Section 4.3.1. Results shown in Appendix E.6 suggest that politically appointed managers are less likely to have a post-graduate degree, to be a union member, and to live in the municipality where they work, with the opposite applying to civil service managers.

[20]In-depth interviews were approved by MIT's Committee on the Use of Humans as Experimental Subjects under protocols 170593389 and 1806407144.

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 post, the state, and the month of the interview in order to safeguard their identity.

Interviews were done in 45 municipalities in 7 states across 3 different regions of Brazil.[21] Locations were chosen to ensure variation in the political and socioeconomic contexts of fieldwork. Municipalities, which are listed in Appendix B.1, vary widely on both socioeconomic and political characteristics, as shown in Appendix B.2. Details on subject recruitment and how interviews were conducted are reported in Appendix B.3. In total, I interviewed 51 municipal politicians, 54 municipal bureaucrats, and 16 horizontal accountability actors.[22] Three quarters of the interviews were done with bureaucrats and politicians in the social sectors, including 56 education officials, 25 healthcare officials, and 9 social assistance officials.

Interviews were essential to develop and probe hypotheses, as well as to understand the mechanisms behind some of my quantitative findings. They played a particularly important role in understanding the informal institutions that govern patronage in Brazilian municipal governments, and in particular the political dynamics of different appointment systems. Principal-agent theories envision a linear hierarchy of nested relationships between principals and agents. For municipal education in Brazil, this model would envision a neat line where the mayor appoints the secretary of education, who appoints school directors, who appoint teachers. Even though Weberian theories would envision bureaucrats as holding their jobs not on the basis of appointment but of civil service statutes, they would still assume the same hierarchical structure. The reality of street-level manager appointment systems, interviews showed, heavily deviates from both these models. Understanding how appointments work in practice, and the actual flows of accountability that ensue from them, provides a critical foundation for analyzing the relationship between appointment systems and outcomes, as well as for reforms aimed at changing local governance structures.

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

[21] Interviews were done in the states of Ceará (43 interviews), Rio Grande do Norte (21), and Paraíba (15) in the northeast; Rio de Janeiro (19), Minas Geráis (10) and São Paulo (1) in the southeast; and Goiás (12) in the center-west. These states concentrate over half of Brazil's population.

[22] 41 of of the 51 politicians were secretaries, 46 of the 54 bureaucrats were street-level managers, and of the 16 horizontal accountability actors 3 were state audit court councilors or auditors, 8 state prosecutors or prosecutorial staff, and 5 state judges or judicial staff.

of the area. In many cases there is thus no direct link between the secretary of the area and street-level managers. Secretaries are however in charge of the selection of temporary street-level bureaucrats (like teachers or nurses), who generally hold one-year contracts. Civil service street-level bureaucrats, on the other hand, hold their jobs on the basis of a competitive examination and are in practice extremely hard to fire once they pass a short probationary period.[23] Street-level managers generally do not control the hiring of street-level bureaucrats nor their assignment to specific units.

Street-level manager positions are particularly important for politicians, given their strategic position in the center of many social networks and visibility for the community, as well as their wide territorial reach in both urban and rural areas. From the point of view of street-level managers, political appointments and connections may be useful to advance their bureaucratic and/or political career, to increase their material and immaterial resources they can tap on as managers, and to boost their power in the community. In very clientelistic settings street-level managers positions are sometimes used 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, and attendance was recorded on a list."[24] 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.

Nonetheless, more commonly stories emerged of political connections being leveraged for the improvement of public services. The importance of alignment for bureaucratic effectiveness is clearer under the light of the stark contrast between the way manager appointments work in practice and the accountability relationships envisioned by principal-agent and Weberian models. Interviews with 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 demans of the local government, which are usually oriented towards service delivery. 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

[23]See Appendix A.4 for diagrams of accountability relationships in the education sector under the ideal principal-agent model and under the actually existing models of political appointment and election.

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

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."[25] The importance of alignment also came up in the report of a school director who had previously been secretary of education. When talking about how a previous government wanted to reform the director election system to reinstate political appointments, she said that the government argued they could not "govern with enemies."[26]

Another dimension that often came up when asking interviewees about appointment systems was street-level managers' responsiveness to politicians. 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."[27] 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 it was the people who gave them the post and thus they owe nothing to the secretariat."[28]

Some appointed managers also talked about the material and immaterial resources that they gained as a result of their connections. 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."[29]

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 bu-

[25]Secretary of education interviewed in the state of Paraíba in August 2018.
[26]School director interviewed in the state of Rio de Janeiro in February 2017.
[27]School director interviewed in the state of Goiás in March 2017.
[28]Secretary of education interviewed in the state of Paraíba in August 2018.
[29]Social assistance center coordinator interviewed in the state of Rio Grande do Norte in December 2018.

reaucrats with less education),[30] 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 un-appointed 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 (which is an important scope condition of the argument). Third and last, I leverage original surveys of bureaucrats and politicians, including conjoint experiments, to show that bureaucrats with upward embeddedness are perceived as communicating better with and being more responsive to the government, exerting more effort, and raising more resources. Together, these three sets of causally identified evidence and the qualitative data from 121 interviews demonstrate that bureaucrats' upward embeddedness can be beneficial for development.

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

An observable implication of my theory is that political turnover should differentially affect appointed and un-appointed bureaucrats. For appointed bureaucrats, mayoral turnover means a negative shock to upward embeddedness, and therefore to governance resources that help them in public service delivery. For un-appointed bureaucrats, however, mayoral turnover should 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 potential

[30] My survey of street-level managers (detailed in Section 4.3.1) indeed suggests that political appointees have lower education levels, as shown in Appendix E.6.

benefits of a renewed leadership, policy switches, and other shocks to the bureaucracy (Akhtari et al., 2018; Colonnelli et al., 2019; Dahlström and Holmgren, 2019; Toral, 2019c). If my theory is right, the performance of appointed bureaucrats should worsen as a result of political turnover, when compared to that of un-appointed bureaucrats. This is precisely what I find.

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 un-appointed 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). 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 un-appointed 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 negative shock in upward embeddedness after the election (U_{sm}), which in turn is a function of whether the mayor loses the election and the director had been appointed by them (A_{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:

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

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

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 un-appointed directors (whose upward embeddedness is expected to remain unchanged under political turnover). Potential outcomes are therefore a function of both $P_{sm} = p \in \{0, 1\}$ and $U_{sm} = u \in \{0, 1\}$, so we can define them as $Y_{sm}(p, u)$. With that notation, the estimand of interest is:

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

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:

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

Three assumptions are needed for this design to give us an internally valid estimate (Grembi et al., 2016). First, potential outcomes $Y_{sm}(p, u)$ should be continuous in the forcing variable around the threshold. To examine the observable implications of this continuity assumption, I verify in Appendix C.2 that pre-treatment covariates are generally continuous around the cutoff. Second, we need to assume that the effect of political appointment when there is no change to upward embeddedness is constant over time, such that schools with appointed and un-appointed directors would follow parallel trends. To indirectly test for this assumption, I verify in Appendix C.3 that schools with appointed and with un-appointed directors, as well as schools in municipalities with and without political turnover, had parallel trends in performance before the election. With these two assumptions, the diff-in-disc estimator estimates the local causal effect of a negative shock in upward embeddedness, close to the threshold, and for appointed directors. If we make a third homogeneity assumption that the effects of the negative shock in upward embeddedness and of political turnover do not interact, then we can recover a more externally valid quantity, i.e. the local average treatment effect of a drop 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. Since for schools with a new director performance cannot be associated to the change or stability of the director's upward embeddedness, schools with director turnover are excluded from the sample. 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, show 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 of the relevance of student evaluations (among both teachers and students), implementing special remedial classes, doing test simulations to familiarize students with the specifics of federal tests, and even logistics planning to ensure an adequate testing environment. All these actions depend critically on management efforts of the school director, and on their ability to boost the motivation and coordination of school personnel.[31] The successful implementation of municipality-wide initiatives also depends on the adequate communication and coordination with school directors.

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)[32] within the optimal bandwidth of the Calonico et al. (2014) algorithm, and apply it to the following estimating equation:

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

[31] The existence of materials produced by education stakeholders (including NGO's and governments) to help directors prepare the school for the tests attests to the impact of actions they can take in the short term on test results. Sample materials can be found here.

[32] I do not apply kernel weighting, and 'localize' the regression function using the bandwidth alone, as recommended by Lee and Card (2008, 319).

Where Y_{smj} is the change in the quality score of school s in municipality m and election cycle j,$\sum_{k=1}^{K} \eta_k X_{smj}^k$ is a set of state fixed effects and director-, school-, and municipality pre-treatment covariates that significantly predict directors' appointment mode,[33] 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 of continuity in potential outcomes and local parallel trends hold, γ_2 identifies τ_{ddisc}, namely the effect of a negative shock of upward embeddedness on school performance in municipalities with political turnover, around the threshold. If the separability assumption holds, γ_2 more generally identifies the local average treatment around the threshold of a negative shock in upward embeddedness. My hypothesis is that $\gamma_2 < 0$.

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*), also called *Prova Brasil*. This system tests students in 5^{th} and 9^{th} grades (i.e., at the end of primary and middle school) in public schools across the country, every odd year. 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 a score for each school 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. All in all, and in words of a group of World Bank economists, Brazil has "one of the world's most impressive systems for measuring education results [...], superior to current practice in the United States and in many other OECD countries in the quantity, relevance, and quality of the student and school performance information it provides" (Bruns et al., 2012, 7).

I use Ministry of Education data for all municipal primary schools, in the years immediately before and after the elections of 2012 and 2016.[34] I use the Ministry's survey of directors to

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

[34] 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 (17% in 2009 vs 3.5% in 2011 and 1% in 2013).

identify schools where the director had been deployed (through political appointment or through other means) 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 (TSE).

4.1.4 Results

Results of the diff-in-disc are shown in Table 2. The negative shock on upward embeddedness (identified by the differential effect of political turnover among appointed directors) has a negative effect on school performance. In particular, the decrease in upward embeddedness 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 and to alternative bandwidths, as shown in Appendix C.4.[35]

Table 2: 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.

	Model 1	Model 2	Model 3
Political turnover	0.173	0.140	0.051
	(0.108)	(0.111)	(0.128)
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

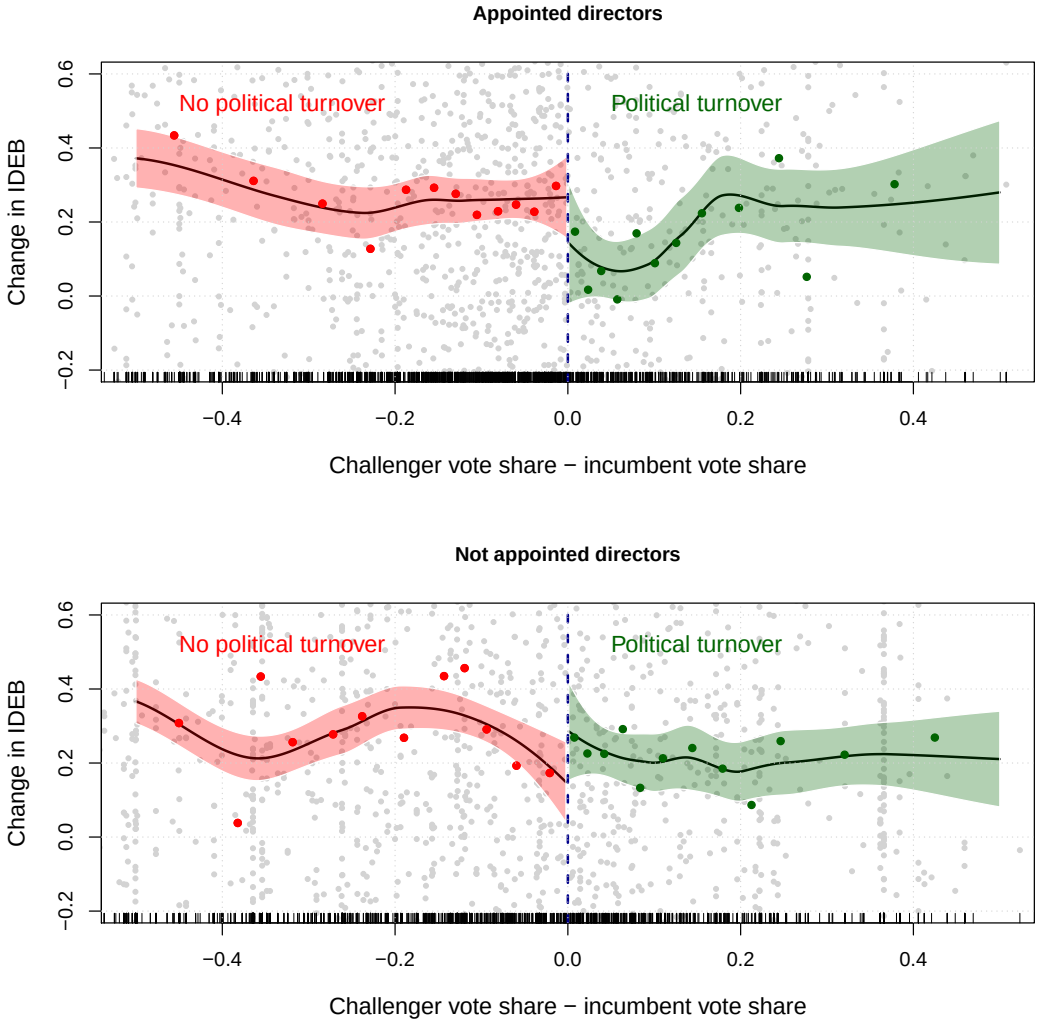
Predictors of whether the director is appointed come from a regression of an indicator for appointed director on a vector of municipality, school, and director variables, as shown in Appendix A.3. Municipality-clustered standard errors in brackets.
*p<0.1; **p<0.05; ***p<0.01.

This designed may suffer from sample selection bias (Heckman, 1979) 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.[36]

[35]If treatment effects are sometimes insignificant with bandwidths smaller than the optimal (likely due to reduced power from smaller samples), their size remains stable.

[36]On average, 71% of directors stay in their post after the election if the mayor wins the election, compared to 33.8% if the mayor loses. In fact, an electoral defeat of the mayor leads to a significant

Figure 1: Effect of political turnover on change in school quality scores, by director appointment mode



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.

This generates groups of schools (under mayor re-election and mayor turnover) that are not necessarily comparable.[37] I address this issue of sample selection bias through three complementary

increase in a director's probability of turnover, as shown in Appendix C.5 using a close-races regression discontinuity. The link between political and bureaucratic turnover in Brazilian schools is also studied by Akhtari et al. (2018).

[37]If we think of mayor turnover as an encouragement instead of a treatment, we can apply the language of instrumental variables (Angrist et al., 1996) to define four types of units in this setting: compliers (schools that would have director turnover only if the mayor lost the election), never-takers (schools where

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 (like years of experience in the post) are significantly more likely to stay in their post. 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 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."[38] This implies that including in the analysis schools without mayor turnover that would have seen their director charge under mayor turnover is actually biasing the results towards zero.[39] Second, I show in Appendix C.7 that diff-in-disc estimates are still significant (and larger) when pre-processing the data with exact matching, such that schools without mayor turnover that do not have an exact match among the schools with political turnover (on the covariates that significantly predict director turnover under political turnover) are excluded from the sample.[40] Third and last, I show in Appendix C.8 that adapting the trimming procedure in Lee (2009) for creating sharp bounds for treatment effects in the presence of sample selection bias generates bounds that are completely 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.[41]

the director would not leave, regardless of the election results), always-takers (schools that would have director turnout regardless of the election), and defiers (where the director would leave if the mayor won the election, but stay if the mayor lost the election). If we make a monotonicity assumption, which is likely safe in this setting, we can rule out defiers. The bias emerges because on one side of the discontinuity (under mayor turnover), schools are of the never-taker type, whereas on the other side of the discontinuity (without mayor turnover) schools can be never-takers or compliers. That is to say, there is a subset of the schools where the director would have changed had the mayor lost the election – in which case they would have left the sample. Because schools with and without mayor turnover have different combinations of principal strata, comparisons of these two groups will not identify the causal effect of treatment (Zhang and Rubin, 2003).

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

[39] In terms of principal strata, this analysis suggests the complier-type schools perform worse than never-takers, and therefore their inclusion in the data (and in particular in the group with no mayor turnover) biases the diff-in-disc estimates towards zero.

[40] This procedure removes some schools in the group without mayor turnover that are predicted to be of the complier type, and thus removes part of the sample selection bias. The resulting diff-in-disc estimate is larger: -0.469 points or 0.44 standard deviations ($p < 0.05$). While other matching procedures could be used (including matching on a score for a school's propensity to have director turnover under political turnover), the exact matching procedure detailed in Appendix C.7 is appealing for its simplicity.

[41] Relying on the assumption of monotonicity (i.e., the non-existence of directors who would remain in the school if the mayor lost but leave if the mayor won), these bounds essentially give us best- and

4.1.5 Alternative mechanism tests and placebo test

The diff-in-disc shows a significant deterioration of the performance of schools with appointed directors in municipalities where the mayor changes. My theory attributes this change to the negative shock to appointed directors' upward embeddedness, but other mechanisms could explain the same result, including changes in the supply and effort of teachers. To test for these alternative mechanisms, I estimate Equation 5 using as the dependent variable changes in the director's answers to survey questions about the prevalence of problems of insufficient teachers, of teacher turnover, and of teacher absenteeism. Results, presented in Appendix C.9, show that there is no effect on these alterantive mechanisms, which are therefore unlikely to explain the results in Table 2.Another alternative mechanisms may be that appointed directors who survive mayor turnover simply anticipate being eventually fired. Data however suggest that most appointed directors who are replaced under a new government lose their post at the beginning of the administration.[42]

I further test the validity of the design through a placebo test. I replicate the design with data for state schools, which should be unaffected by whether the mayor changes or not because they are managed by state governments, and state elections are held two years before and after municipal elections. As shown in Appendix C.10, I find no treatment effect among state schools. This placebo test lends additional support to the design.

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 directors that had been deployed in the same period through other methods. This is consistent with a negative shock to upward embeddedness hurting bureaucratic effectiveness. The results are unlikely to be explained by a form of post-treatment or compositional bias (introduced by the fact that schools whose directors change after the election are excluded from the sample), or by alternative mechanisms like differential changes in director effort, teacher supply, teacher turnover, or teacher absenteeism.

worst-case extremes of the potential impact of sample selection, given the data. Using this procedure I obtain bounds of [-0.907, -0.074]. I then use the bootstrap and the confidence intervals of Imbens and Manski (2004) to estimate a 95% confidence interval of [-0.978, -0.024]. These bounds show that even in the worst-case scenario of sample selection bias, the data is not compatible with the negative shock to appointees' upward embeddedness having a non-negative effect on performance. Details of the bounding exercise and inference by bootstrap are reported in Appendix C.8.

[42]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. Among schools where the director changes, 77% have the same director in late 2015.

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

My theory posits that the political appointment of bureaucrats enhances accountability, and rests on the assumption that politicians care (at least partly) about the delivery of public services. Brazilian school directors offer another opportunity for a quasi-experiment that allows us to test these two ideas. The federal government regularly publishes school quality scores called IDEB, as explained in Section 4.1, and these are usually compared to targets that were defined over a decade ago. We can therefore examine the extent to which directors of schools that miss their targets have higher turnover rates, and how this varies across director appointment types. My theory predicts that appointed directors that miss their targets have higher turnover rates, because they are more likely to be let go by politicians.

To test this, 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. Results support the hypothesis that political appointment enhances accountability, and provide evidence for the assumption that politicians care about public service delivery. 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. This suggests that politicians replace school directors who under-perform.

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 (which are lower for units with a lower baseline performance) were defined following an algorithm that projects progress of schools along logistic trajectories with the goal of getting the country to a score of 6 by 2021 (Fernandes, 2007).[43] Targets were released at the beginning of the period and have not been revised. As a result, 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

[43] A score of 6 was estimated to be equivalent to the average performance levels in OECD countries.

school experiencing director turnover between 2014 and 2015, and to explore heterogeneity by the appointment type of the school's director in 2013. 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., 2019). While actors in the local government and education sector have other sources of information about the quality of schools, IDEB reveals quantitative, reliable information and facilitates common knowledge.[44] I focus on the 2013-2015 IDEB cycle to avoid years with municipal elections, which significantly increase director turnover as shown in Appendix C.5.[45]

More formally, treatment for school s (meeting the school quality target), T_s, is assigned by the difference between its quality score and target ($D_s = score_s - target_s$):[46]

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

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 (Imbens and Lemieux, 2008). While this assumption is empirically untestable, we can examine some of its observable implications, including that there is no evidence of sorting around the threshold (as shown in Appendix D.1) and that pre-treatment covariates are continuous around the threshold (Appendix D.2).

[44] 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).

[45] I do not use data from the 2009-2011 IDEB cycle because, as noted above, before 2011 the question on appointment mode had significantly higher levels of non-response.

[46] 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.

4.2.2 Estimation and inference

I use local linear regression (Gelman and Imbens, 2018),[47] and apply it to the following estimating equation, within the bandwidth selected by the Calonico et al. (2014) 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 for 2013 $\geq$ quality target for 2013). 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:

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

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. Existing ways to relax this assumption are to include region fixed effects (Becker et al., 2013) or to use propensity score weighting (Gerardino et al., 2017). 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.[48]

[47] I do not apply kernel weighting, and 'localize' the regression function using the bandwidth alone, as recommended by Lee and Card (2008, 319).

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

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 a year or less.[49] I use data for school performance in primary education, since this is the most important responsibility of municipal education systems and some but not all schools receive scores for middle education.[50]

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 that had 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 ($p < 0.01$). Figure 2 visualizes this effect.[51] This HLATE could however be biased by confounding in the appointment mode. To explore this possibility, models 3-5 include state fixed effects, municipality fixed effects, and a long set of pre-treatment covariates that significantly predict school directors being appointed. Results are robust to the inclusion of these covariates. It could still be the case that there is unobserved confounding biasing the HLATE estimate, but its stability across specifications including significant predictors of appointment mode gives some confidence in the results.[52]

Among elected or tenured directors, however, meeting the target does not cause any significant change in the probability of turnover, as shown in Appendices D.4 and D.5.[53] The case of elected directors may seem particularly surprising at first. We tend to think of elections as political institutions to discipline agents and make them more accountable. Elected directors however are not being held accountable for their performance in this highly visible metric of school quality. In Appendix D.6 I present some qualitative and quantitative evidence of how low competition, capture,

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

[50] Some municipal schools offer middle education (instead of or in addition to primary education) and get quality scores and targets for that level. Since not all schools have two signals a two-dimensional RDD is not possible.

[51] RD plots for schools with elected or with tenured directors, are shown in Appendix D.3.

[52] Results are similar when splitting the sample by appointment mode, as shown in Appendix D.4.

[53] While tenured directors generally cannot be fired, they can be transferred to a different school. Research from India shows that bureaucratic transfers are frequently used to discipline bureaucrats (Iyer and Mani, 2012), and my interviews suggest this is the case in Brazil as well.

Table 3: Effect of reaching the primary school quality target in 2013 on school director turnover between 2014 and 2015, by whether the director in 2013 was appointed.

	Model 1	Model 2	Model 3	Model 4	Model 5
Quality target met	-0.024	0.011	0.008	0.013	-0.003
	(0.018)	(0.027)	(0.026)	(0.032)	(0.027)
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					✓
Quality target met + interaction		-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 heteroskedasticity consistent standard errors in brackets.
*p<0.1; **p<0.05; ***p<0.01.

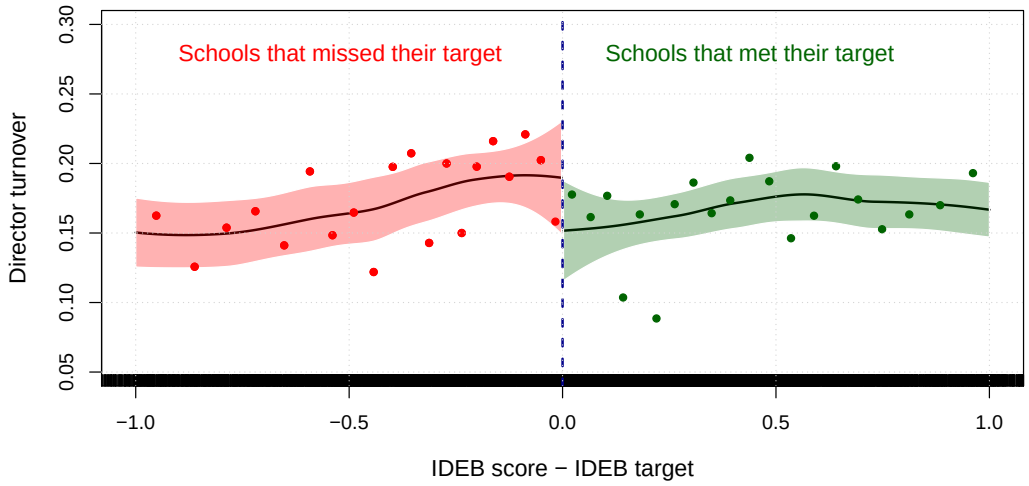
and low participation make director elections unlikely to boost bureaucratic accountability.[54]

Additional robustness checks lend further support to these results. Appendix D.7 shows that the effect of treatment for appointed directors is robust to alternative bandwidths.[55] Placebo tests moving the RD threshold to 20 alternative values of the forcing variable show insignificant results except in 1 case (which is what we would expect with an α of 0.05), as shown in Appendix D.8. Replicating the design with data for municipalities that had a mayor belonging to one of the (back then) two large programmatic parties in Brazil (PT or PSDB) shows larger HLATE estimates, as we would expect if this is a case of governments holding their bureaucrats accountable for their

[54]This finding of elections failing to enhance performance-based accountability of school directors contributes to a literature comparing the effectiveness of appointed and elected bureaucrats in other settings, such as US judges (Maskin and Tirole, 2004; Iaryczower et al., 2013; Lim, 2013; Canes-Wrone et al., 2014), city mangers in Germany or California (Garmann, 2015; Whalley, 2013), heads of regulatory agencies in the US (Besley and Coate, 2003), or school superintendents and school boards in Alabama (Hoover, 2008). These studies suggest that taking bureaucratic appointments away from politicians by establishing bureaucrat elections can make bureaucrats more responsive to constituents and at the same time lead to undesirable consequences – for example in the application of penal law or in fiscal management. They also provide evidence of how the details of bureaucrat election systems matter. The issue has been less studied in mass public service delivery bureaucracies like education or healthcare, especially in developing countries.

[55]If treatment effects are sometimes insignificant with bandwidths smaller than the optimal (likely due to reduced power from smaller samples), their size increases closer to the threshold.

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



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.

performance in service delivery.[56] Results are shown in Appendix D.9.

To sum up, these results suggest that appointed directors are held accountable for school quality, while elected or tenured ones are not. The robustness of the HLATE estimate to the inclusion of relevant pre-treatment covariates and to alternative bandwidths, and the passing of placebo tests all lend support to the interpretation of these findings as causal. The effect is also consistent with qualitative evidence. Secretaries of education often say in interviews that school performance is among the criteria considered for assessing directors and deciding on appointments. For instance, an education secretary replied to a question about how they decide whether to keep or replace a director by saying: "We use the school's performance (for example on IDEB) as well as the relationship to families as the main criteria to decide whether we keep a director or not."[57] Another one said "IDEB is useful to rank schools, which helps management... And it is also useful when it comes to assessing the director. The school's IDEB is a factor to decide if the director continues or not."[58]

[56]Programmatic parties are those having identifiable platforms, and are generally thought as less likely to rely on clientelism (Cruz and Keefer, 2015).

[57]Secretary of education interviewed in the state of Ceará in August 2017.

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

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

How does upward embeddedness foster bureaucratic effectiveness? Here I present results from conjoint experiments embedded in two original surveys that I did in the Brazilian state of Rio Grande do Norte: a face-to-face, representative survey of 926 street-level managers, and an online survey of 755 local politicians.[59] 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.[60]

The survey took place in Rio Grande do Norte (RN), a state at the heart of Brazil's Northeastern region, which has historically been characterized by inferior development outcomes, corruption, and clientelism (Leal, 1948). I chose this particular state due to the state audit court's willingness to partner for a field experiment (Toral, 2019a). Its municipalities, mostly underdeveloped and distant from large metropolitan areas, are typical of the Northeast and fit well the scope conditions described in Section 2.3. The survey focused on the urban areas of 150 small and medium municipalities – the largest 17 municipalities of the state were excluded due to budget and security constraints.[61] 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

[59]Surveys were approved by MIT's Committee on the Use of Humans as Experimental Subjects under protocol 1810539206. Previous online and in-person pilots of the survey of bureaucrats were approved under protocols 170593389, 1803276033 and 1806407132.

[60]A link to the survey instrument can be found in Appendix E.2.

[61]See Appendix E.3 for details on the socioeconomic characteristics of municipalities in Rio Grande do Norte, and Appendix E.4 for details on respondent recruitment and non-response.

5 surveys done per municipality.[62]

First I present results from the observational module of the survey. I collected data on the number of meetings street-level managers held, over the previous three months, with a number of local stakeholders such as the mayor, the secretary in their area, or city councilors. I also asked them how much they agreed (on a 4-point scale) with statements about the mayor and their secretary.[63] 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 $\sum_{k=1}^{K} \gamma^k X_i^k$ is a set of all the demographic and political covariates I collected[64] as well as municipality and social sector (education / healthcare / social assistance) 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.[65] For inference I use HC1 heteroskedasticity-consistent standard errors.

The results of these descriptive analyses are presented in Figure 3. While not causal, and based on self-reported attitudes and behaviors, these results lend support to the theory's predictions. 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 professionals in the education/healthcare/social assistance sector.[66] For item about the mayor, the appointed managers

[62] Descriptive statistics are reported in Appendix E.5.

[63] Response options are "not at all", "a little", "quite" and "a lot".

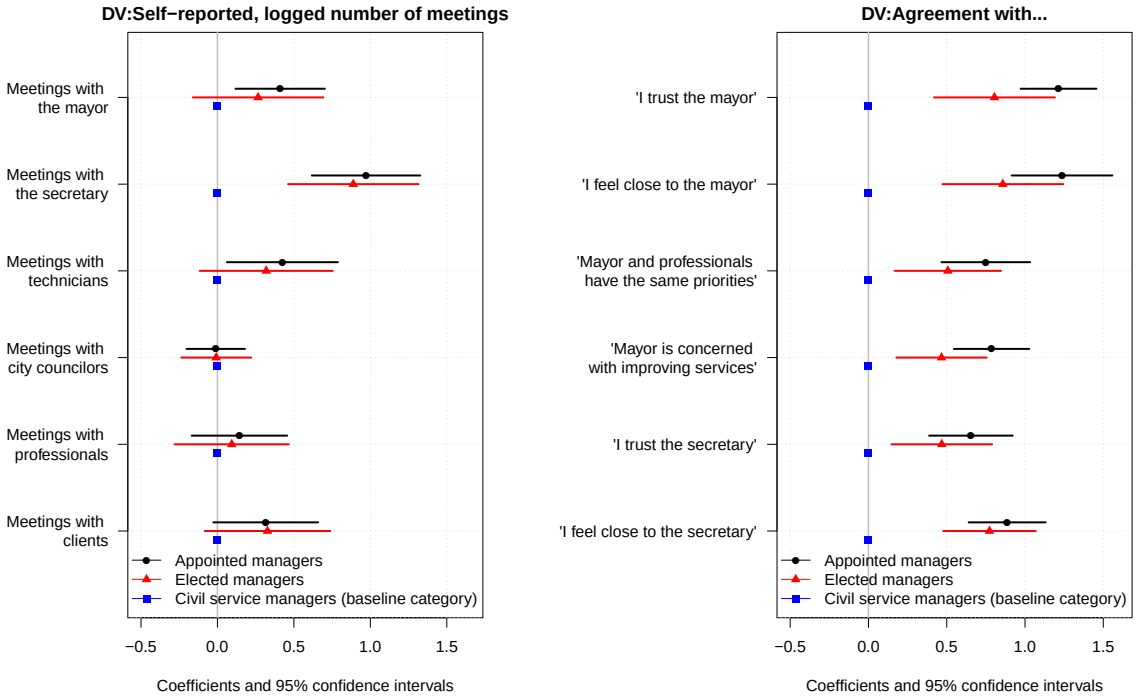
[64] Controls include respondents' sector, age, gender, years of experience as professional in the sector, years of experience as manager, party membership, union membership, whether they have less or more education than a college degree, whether they have other jobs, and whether they live in the municipality where the unit (school/clinic/social assistance center) is located.

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

[66] While these dependent variables are highly non-normal, results are similar when using a binary version.

have significantly higher levels of agreement than elected managers as well.

Figure 3: Observational results from the face-to-face survey of street-level managers: Relationship between appointment type and meetings with and attitudes about politicians.



Points are the regression coefficient corresponding to each appointment mode, and bars are their 95% confidence intervals. Regressions include individual-level controls and municipality and sector fixed effects, as per Equation 10. Results are detailed in Appendix E.7.

While not causal,[67] these associations are strong and aligned with both qualitative evidence and 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), and have already been successfully used to measure perceptions of clientelism among bureaucrats (Oliveros and Schuster, 2016).

In the conjoint, respondents were offered four sets of two hypothetical profiles of managers,

[67] One may be concerned that other factors may explain these correlations. For example, civil service bureaucrats may be less subject to demand effects with questions about trust and alignment. There may also be omitted variable bias.

with randomly assigned attributes in six dimensions (appointment mode, political connections, education, experience, relationship to professionals, and unit performance in federal indicators).[68] 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.

With randomly assigned attributes, assuming that potential outcomes take on the same value when the hypothetical profiles for the same choice task have the same attributes and that the ordering of profiles has no effect, we can estimate the average marginal component effect (AMCE) for each attribute's value using linear regression (Hainmueller et al., 2014):

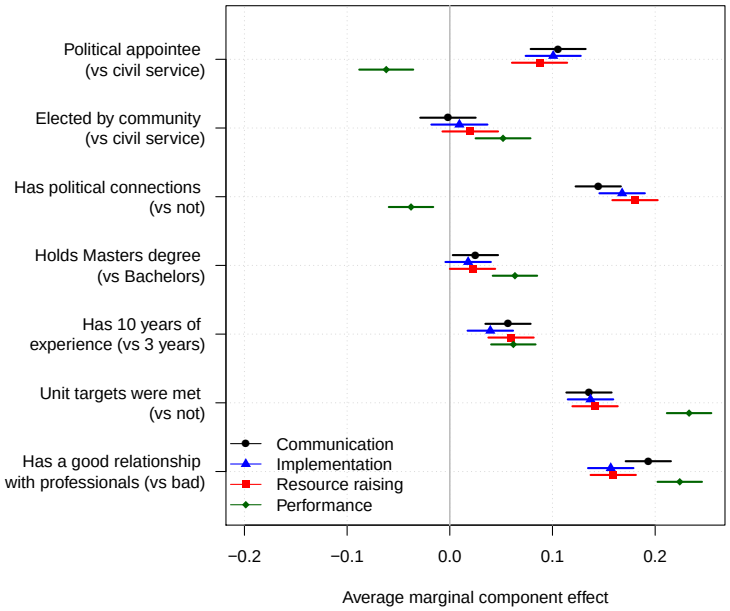
$$Y_{ijk} = \alpha + \boldsymbol{\beta} \boldsymbol{W}_{ijkl} + \varepsilon_{ijk} \tag{11}$$

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, 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 fund raising. 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. One potential concern is that these results are merely driven by appointed and connected bureaucrats trying to portray a good picture of themselves. Results however are similar when subsetting to un-appointed managers, as shown in Appendix E.10.

[68] Details of the attribute values for conjoint profiles are included in Appendix E.8.

Figure 4: Results from the face-to-face conjoint experiment with municipal street-level managers.



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. Results are detailed in Appendix E.9.

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, and goes counter to the quasi-experimental results of the difference-in-discontinuities. 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.[69] 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.11, including only respondents who agree with statements about the mayor and the secretary having programmatic concerns leads to coefficients for the performance question that are much smaller and statistically insignificant,

[69]Descriptive evidence from the survey of managers showing that political appointees are less likely to have graduate degrees, shown in Appendix E.6, lends some support to this idea.

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 primary purpose of the survey was to measure intermediate outcomes of a field experiment (Toral, 2019a), but I included a conjoint experiment to measure their perceptions of how political connections impact bureaucratic responsiveness, effort, and performance.[70] The descriptive module of the survey also offers some evidence in support of the assumption that politicians care about public service delivery.[71] The survey was sent through the state audit court's online system to all mayors and city councilors of the 167 municipalities in the state, plus all municipal secretaries of five key areas.[72] 755 politicians completed the survey, for a response rate of 27% and a median number of 5 responses per municipality.[73] This response rate is relatively high for a survey of elites: recent surveys of state legislators in the US typically have one-digit response rates.[74]

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).[75] 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., 2019; Barbosa and Ferreira, 2019; Toral, 2019b). In fact, the majority of the street-level managers I surveyed believe that political connections influence a lot the appointment of temporary bureaucrats.[76] For each pair,

[70] A link to the survey instrument can be found in Appendix F.1.

[71] 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.

[72] Secretaries of education, healthcare, social assistance, finance, and administration received the survey.

[73] Response rates were higher among secretaries (56%) and mayors (33%) than among city councilors (13%). Details on respondent recruitment and non-response are reported in Appendix F.2. Descriptive statistics are reported in Appendix F.3.

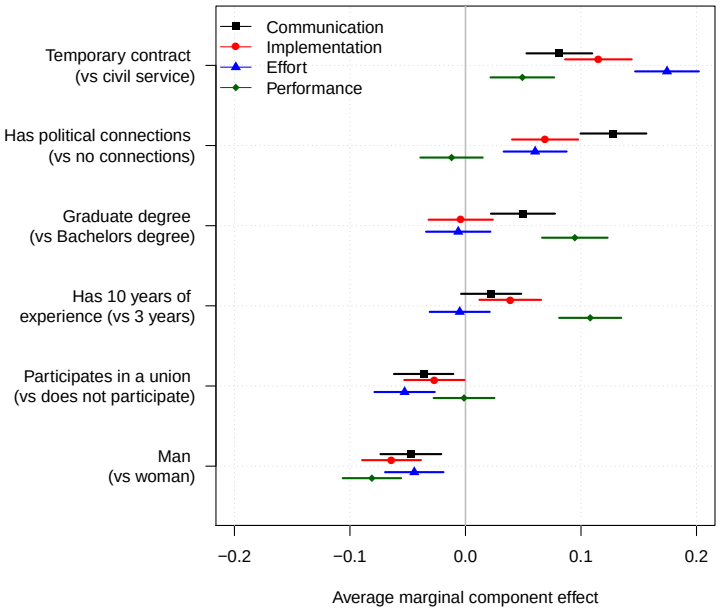
[74] For example, Anderson et al. (2016), Cluverius (2017), Nicholson-Crotty and Carley (2018), and Anderson et al. (2019) report response rates of 5%, 7%, 8%, and 3%, respectively.

[75] Details of the attribute values for conjoint profiles are included in Appendix F.4.

[76] 58% of respondents said political appointments influence "a lot" the hiring of street-level bureaucrats, and only 16% responded "nothing" or "a little."

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 face-to-face conjoint experiment with municipal street-level managers.



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. Results are detailed in Appendix F.5.

Conjoint results, shown in Figure 5, suggest that politicians see bureaucrats with upward 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.[77] While political appointments of street-level managers and contract types of street-level bureaucrats are distinct sources of variation with their own political logics, they are both a source of upward embeddedness and the rationale for their effects on accountability and effectiveness are similar. 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, it's just 'I go, teach my class, and that's it.' It's not absenteeism, they know if they do not show up that will be trouble. But there's not enough commitment. Temporary hires dedicate themselves

[77]Results are similar when looking only at responses given by mayors, or responses given by secretaries, as shown in Appendix F.6.

more. Things flow because managers count on contract workers."[78]

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. Nonetheless, the inferential leverage of conjoint experiments is limited, since they rely on perceptions. Future research should further examine the mechanisms of upward embeddedness using other designs.

5 Conclusion

Patronage, or the political appointment of bureaucrats, is a governance phenomenon present in some degree all around the world (Dahlström et al., 2015). Patronage has traditionally been seen as a clientelistic exchange that hurts development, both from Weberian and principal-agent paradigms (Golden, 2003; Robinson and Verdier, 2013; Dahlström and Lapuente, 2017; Xu, 2018). Previous research in political science, public administration, and economics suggests, however, a brighter side of patronage. In comparative politics, some scholars have highlighted the beneficial uses of patronage for party building (Sorauf, 1960; Huntington, 1968), integrating isolated communities into the nation (Weingrod, 1968), aggregating interests (Scott, 1969), reducing the risk of coups (Arriola, 2009), or supporting a variety of state building projects (Grindle, 2012). Recent research in economics recognizes that, theoretically, patronage may enhance organizational effectiveness in public bureaucracies (Brollo et al., 2017; Xu, 2018; Colonnelli et al., 2019). In public administration and in American politics, researchers have long seen political appointments as an instrument for politicians to gain control over policy and implementation (Peters and Pierre, 2004; Lewis, 2008; Kopecký et al., 2016). Yet, existing theories of the benefits of patronage either do not directly address the impact on public service delivery, fail to spell out mechanisms and scope conditions, and/or do not provide causally identified evidence. This paper contributes to fill this gap.

The core of my argument is that political appointments and connections provide bureaucrats with *upward embeddedness* (political, social, and professional ties to politicians) which can make them more effective and accountable in the delivery of public services. Upward embeddedness

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

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 resources, which together may be seen as a *governance technology*, can be leveraged for extracting rents, for delivering public services, or for both. In other papers I have shown how patronage in the allocation of jobs around elections can hurt public service delivery, either because it is geared towards electoral mobilization (Toral, 2019b) or because it seeks to stack the deck against the opponent before leaving office (Toral, 2019c). Others have also studied how patronage can be a rent-seeking strategy targeted at rewarding supporters (Colonnelli et al., 2019; Barbosa and Ferreira, 2019). This paper focuses on the often-overlooked benefits of patronage. I argue these benefits are more likely to be net positive in the appointment of street-level managers (like school directors) 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 political appointments for bureaucrats' effectiveness and accountability. It does so by leveraging administrative and survey data of municipal bureaucracies in Brazil, a setting where multiple appointment systems coexist. Using a difference-in-discontinuities, I show that the quality of schools with appointed directors decreases (relative to that of schools with un-appointed 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 un-appointed 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, large surveys of street-level managers and politicians, as well as through conjoint experiments embedded in them. Correlational 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. Interviews provide rich accounts of how upward embeddedness can be leveraged to enhance the effectiveness and accountability of street-level managers.

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, misallocations, and other "government failures" (Devarajan and Khemani, 2018), as is often assumed, but also of governance resources that can help overcome development challenges. While the costs of patronage have long been studied (Pollock, 1937), the advantages that upward embeddedness can provide 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. Therefore, the common prescription that bureaucracies be highly depersonalized and insulated[79] can actually be detrimental for development, at least when such a reform is not preceded by significant increases in human capital, which usually take decades to accrue. The fact that we observe robust cross-country correlations between governance outcomes and the separation of bureaucrats and politicians (Dahlström and Lapuente, 2017) does not necessarily imply that imposing such separations where they do not exist will be beneficial. The evidence presented here does not mean that patronage is necessarily preferable to civil service systems – patronage may be however a second best for some developing contexts. By drawing attention to the governance technology that patronage provides, the paper helps us better understand the connection between patronage and 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 developmental projects, and may be one of the reasons why it is such a prevalent phenomenon around the world, even in high-income democracies.

In the short term, a more productive policy approach may be to establish formal and informal mechanisms that increase politicians' concerns with public service delivery. The easiest of these may be establishing strong information systems that provide regular, detailed, and credible measurement of the performance of local bureaucracies, and the widespread dissemination of easy-to-digest results. Measuring performance and communicating the results to local actors has been shown to improve bureaucratic effort (Muralidharan and Sundararaman, 2010) and effectiveness (De Hoyos et al., 2017). Widespread and reliable performance metrics can also contribute to make politicians value service delivery, especially when accompanied by incentive schemes, communications campaigns, and oversight mechanisms. While transparency initiatives geared towards citizens often fail to foster accountability (Lieberman et al., 2014; Kosack and Fung, 2014), politicians, bureaucrats,

[79]Proposals to isolate bureaucrats from political influence are recurrent in academic and policy circles. In Brazil, bills in both the federal House of Representatives and the Senate have proposed to constrain the political appointment of school directors (PL 1672/2019 and PLS 321/2014, respectively).

and oversight agents like auditors and prosecutors are likely to understand, process, and act on the information because they have direct responsibilities to do so. While ending politician discretion in appointments (through civil service systems or bureaucratic elections, for example) usually requires politically costly reforms that may be hard to sustain when imposed from above, collecting and delivering data on the performance of local bureaucracies can be a relatively inexpensive way to encourage the use of that political discretion for the improvement of service delivery. A complementary strategy suggested by the paper would be to boost the management skills of street-level managers through training programs. More management skills would increase the quality of service delivery, especially when complemented with public data about bureaucratic performance.

The paper also has novel implications for our understanding of how political turnover impacts the bureaucracy. Other research focused on Brazilian local governments has shown that political turnover can disrupt the bureaucracy through the hiring of campaign supporters and co-partisans (Colonnelli et al., 2019; Barbosa and Ferreira, 2019) or the replacement of school personnel, including directors (Akhtari et al., 2018). Results presented in this paper suggest a different, complementary mechanism through which political turnover can disrupt public bureaucracies, even in the absence of bureaucratic turnover: the undermining of connections between bureaucrats and public officials. Disruption therefore does not depend on bureaucratic turnover. Neither does the connection between political and bureaucratic turnover depend on a system of political appointments. In Brazil, political turnover increases the turnover not only of politically appointed directors but also elected or tenured ones. Even in Sweden, political turnover significantly increases the turnover of agency heads, despite very strong legal protections of their meritocratic recruitment, autonomy, and fixed terms (Dahlström and Holmgren, 2019). The frequent policy prescription of using civil service regimes to shield the bureaucracy from political influence puts too much faith on the power of formal institutions, and neglects the multiple channels through which political turnover can influence bureaucrats, with or without affecting their turnover. While more research is needed on the different mechanisms through which political turnover affects bureaucrats and public service delivery, ultimately there is a fundamental tension between the democratic commitment to electoral accountability and the desire for a completely impersonal and politically shielded administration of the state.

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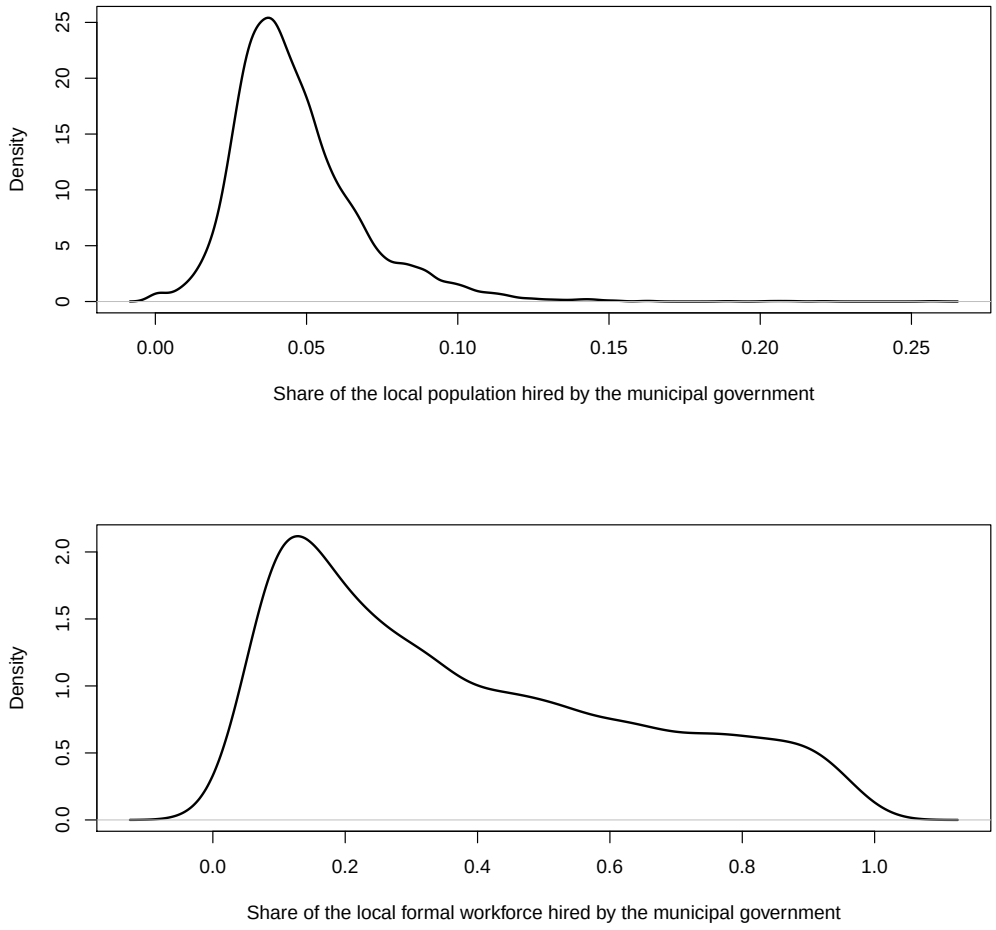
Appendices

A Additional details of the institutional context

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

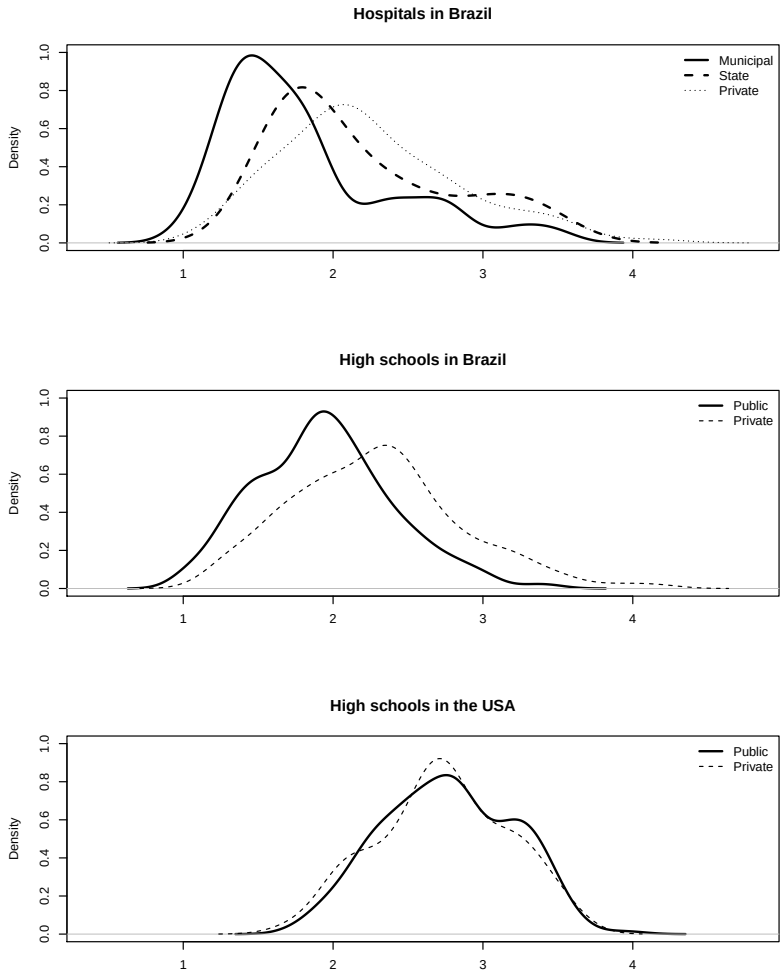
Figure 6: Size of the municipal government workforce as a proportion of the total local population (above) and the total formal labor market workforce (below) in 2016



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. (2013, 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

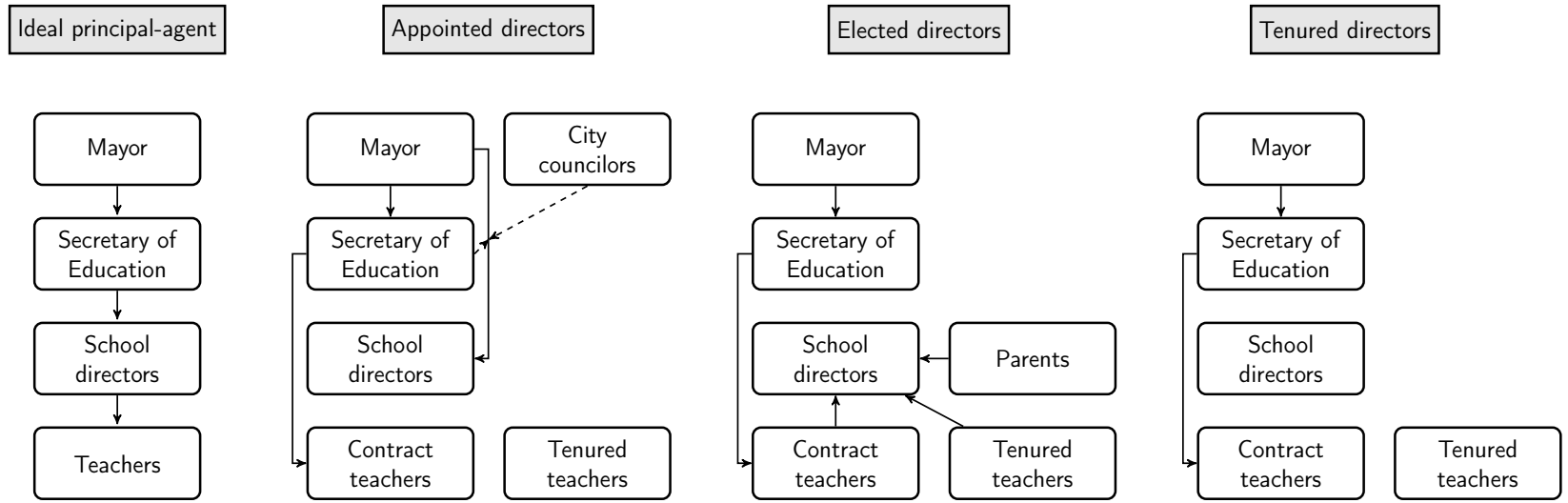
Table 4: Observational predictors of school director appointment modes and school quality test score (IDEB), from cross-section data on municipalities, schools, and directors (2013)

	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

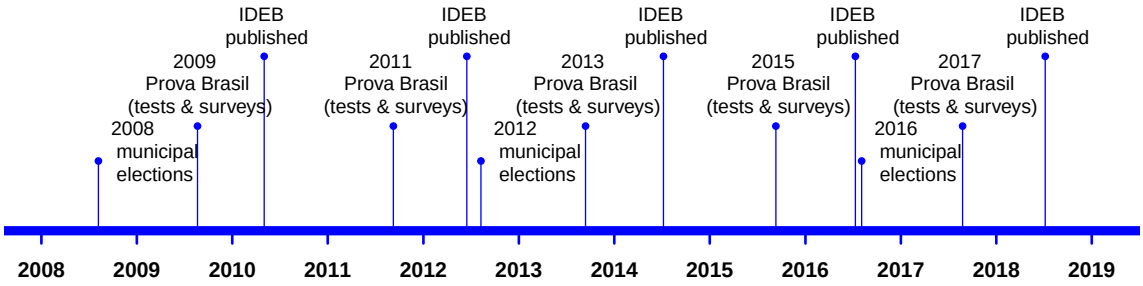
Figure 8: Four models of appointments in Brazil's municipal basic education sector: The ideal principal-agent model (left), and the actually existing models under political appointment, under election , and under civil service



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

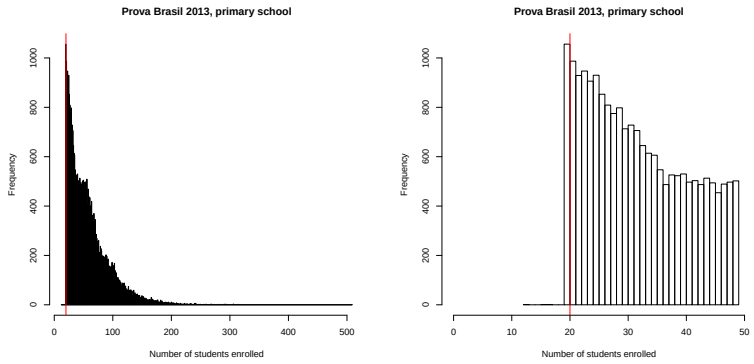
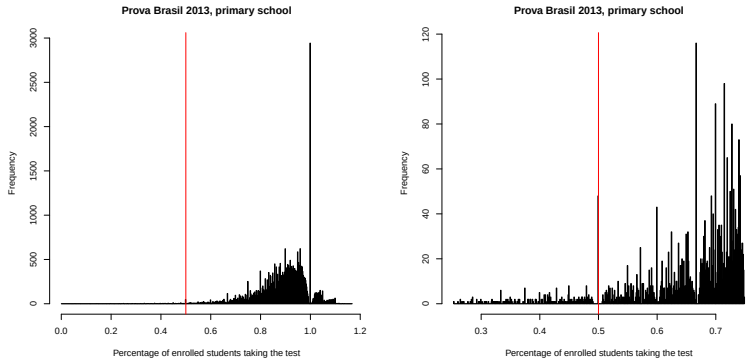


Figure 11: Histogram of the share of enrolled students who take the test, by school



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

B Additional details of in-depth interviews

B.1 Interview locations

Interviews were conducted in the following states and municipalities:

- Ceará (Northeast): Fortaleza, Canindé, Caridade, Madalena, Boa Viagem, Pedra Branca, Tamboril, Sobral, Massapê, Granja, Jijoca de Jericoacoara.
- Rio Grande do Norte (Northeast): Natal, Goianinha, Santa Cruz, Caicó, Cerro Corá, Bento Fernandes, Extremoz, Maxaranguape, Rio do Fogo, Sitio Novo.
- Paraíba (Northeast): Joao Pessoa, Sapé, Sobrado, Riachão do Poço, Cuité de Mamanguapa, Capim.
- Rio de Janeiro (Southeast): Rio de Janeiro, Maricá, Saquarema, Engenheiro Paulo de Frontin, Itaboraí, Mendes, Paracambi, Piraí.
- Minas Gerais (Southeast): Unaí, Paracatú, Itapeva, Camanducaia.
- State of São Paulo (Southeast): São Paulo.
- Goiás (Center-west): Valparaíso de Goiás, Luziânia, Cabeceiras, Formosa, Planaltina, Cristalina.

Figure 12: States where interviews were conducted

B.2 Socioeconomic and political characteristics of interview locations

Figure 13: Distribution of municipality-level socioeconomic characteristics: in black, the distribution of all municipalities in Brazil; in red, municipalities where interviews were conducted.

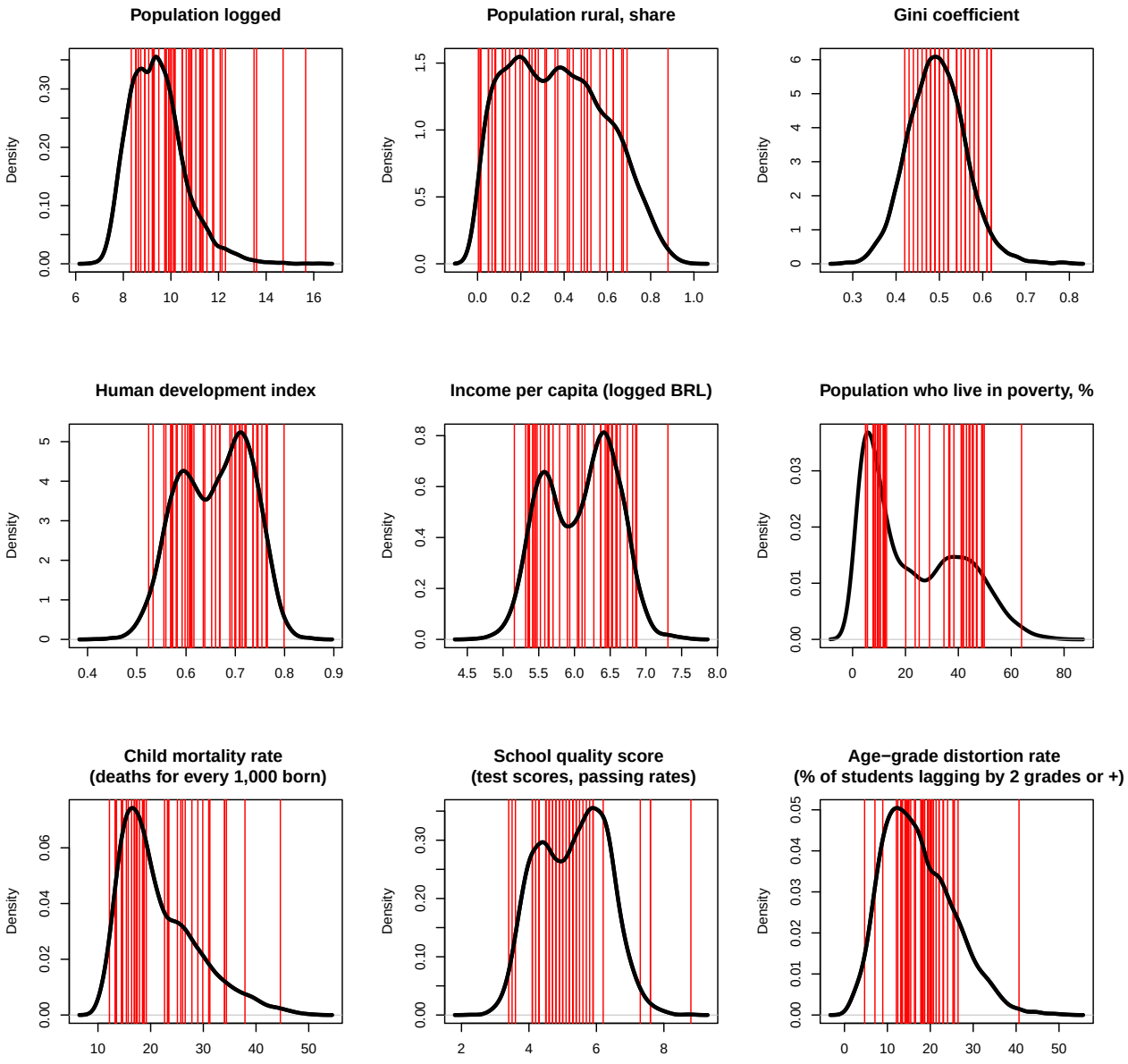
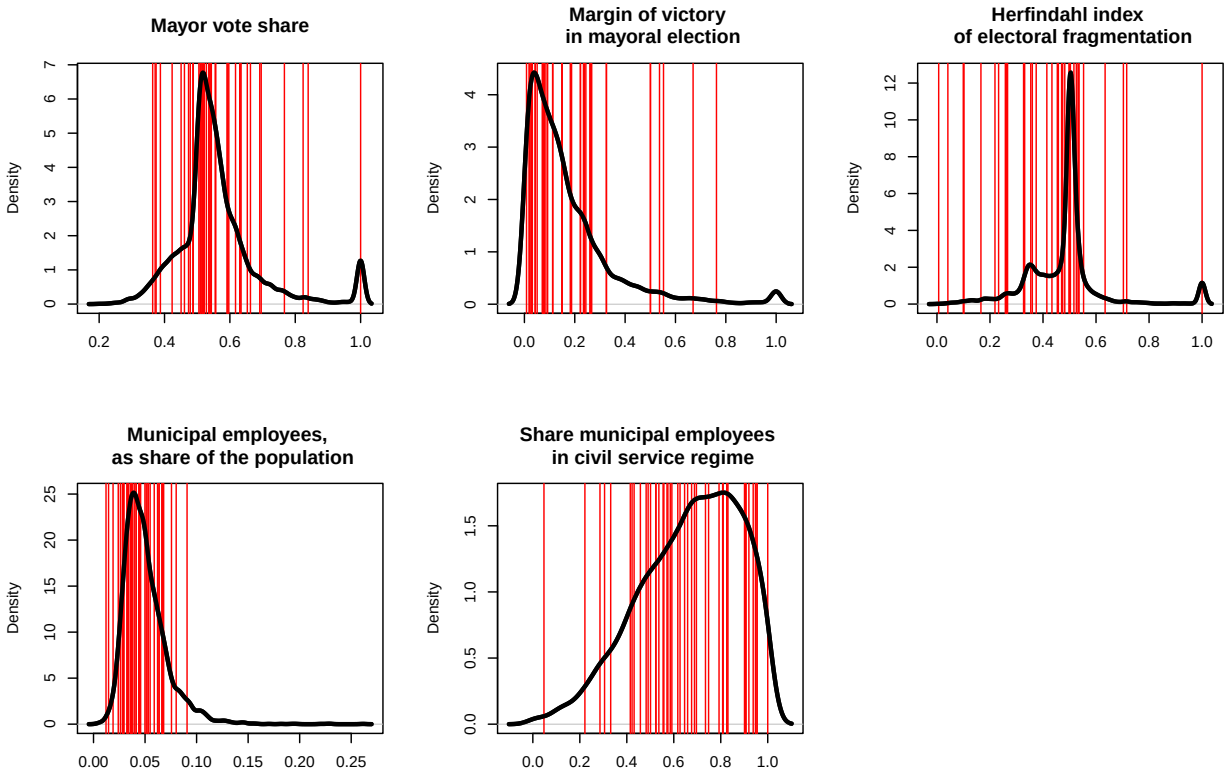


Figure 14: Distribution of municipality-level political characteristics: in black, the distribution of all municipalities in Brazil; in red, municipalities where interviews were conducted.



B.3 Subject recruitment and interview strategies

Within each municipality, fieldwork focused on the center – only a few bureaucrats were interviewed in rural areas. Fieldwork initially focused on bureaucrats and politicians in the education sector and then expanded to cover healthcare and, eventually, social assistance. Horizontal accountability actors and municipal secretaries of finance and of human resources were also added to the target population as saturation in the education and healthcare sectors was reached, and new research questions emerged.

I approached potential interviewees at their offices and requested an interview after introducing myself and the project. No compensation of any sort was offered to participants. Most subjects that I managed to speak to directly agreed to participate, but some refused, mostly arguing they did not have time. Two subjects refused to participate because of the research topic. Many subjects agreed to participate only on the condition of anonymity and/or confidentiality.

Interviews were semi-structured, and usually started as an open conversation about their 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 bureaucrat-politician relationships), local political conflicts (including elections), variation in bureaucratic performance, and the influence of horizontal accountability institutions like the state audit court or the prosecutor's office. I took handwritten notes during and after the interviews. The median duration of the interviews was one hour.

C Additional details of the difference-in-discontinuities

C.1 Continuity of the forcing variable

Figure 15: Histogram of the forcing variable: IDEB target - IDEB score

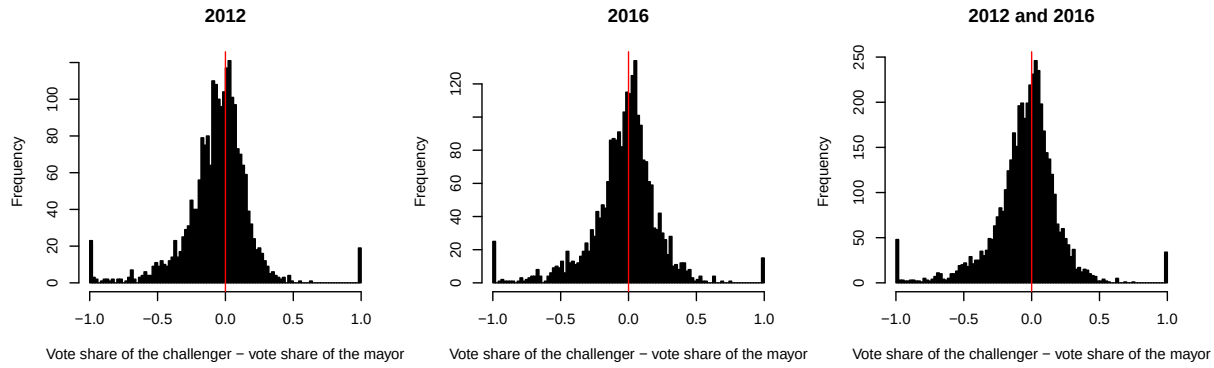
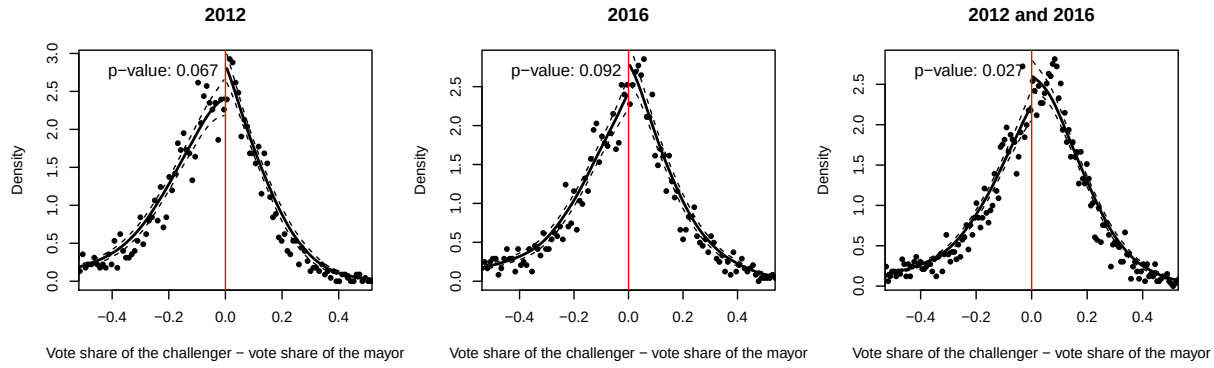


Figure 16: McCrary density test for discontinuity in the forcing variable



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 a phenomenon of incumbent disadvantage, which has been identified before for Brazilian mayors (De Magalhaes, 2015). 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 17: Pre-treatment trends between appointed and not appointed directors, within the RD bandwidth.

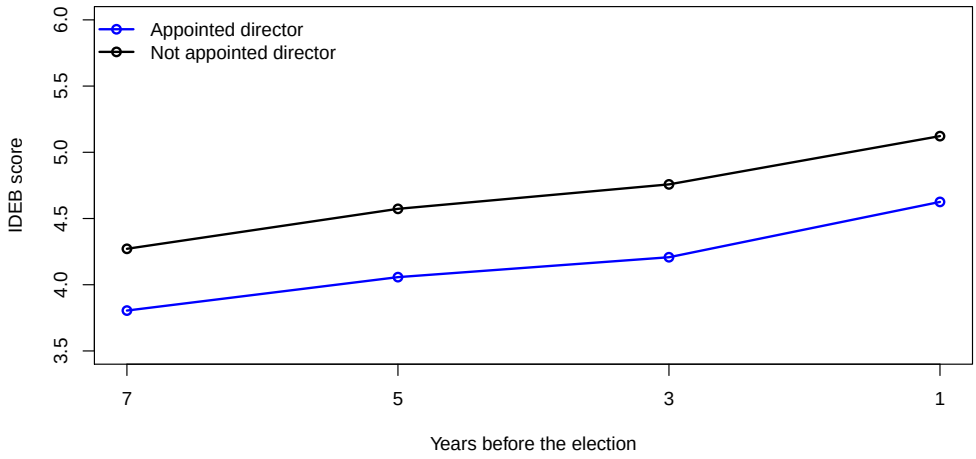
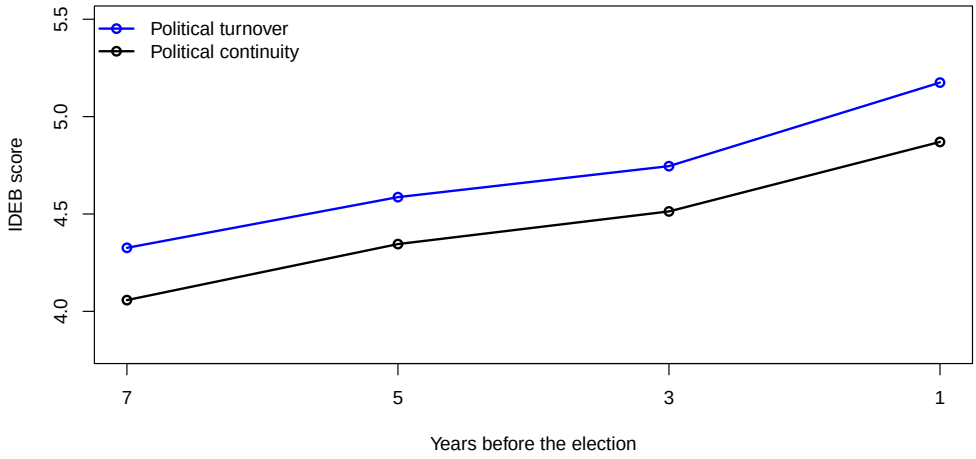
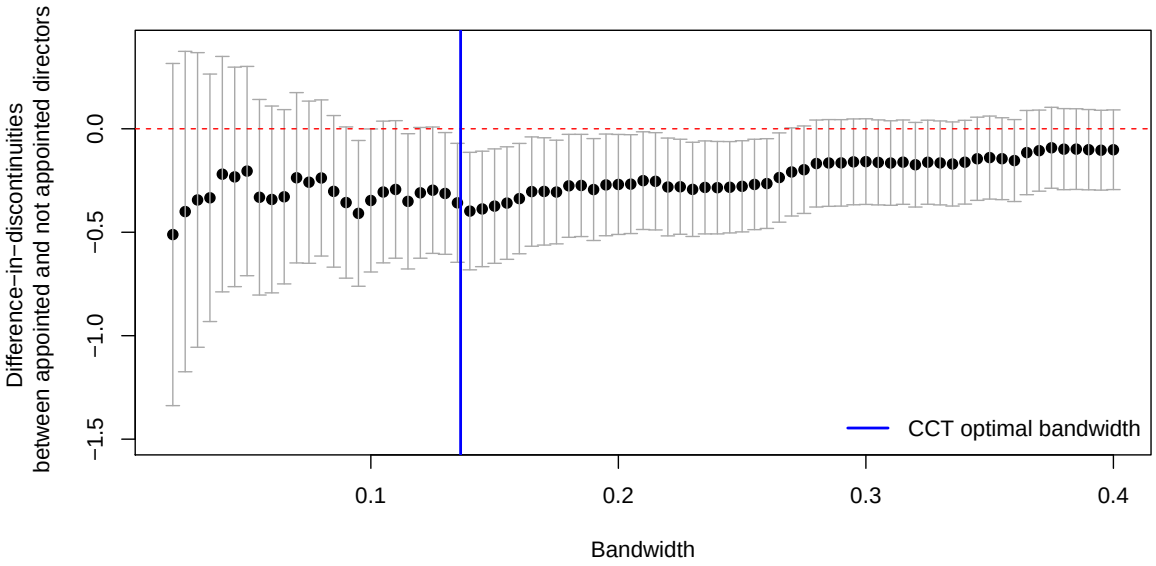


Figure 18: Pre-treatment trends between schools with and without political turnover, within the RD bandwidth.



C.4 Alternative bandwidths

Figure 19: 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 20: Effect of political turnover on bureaucratic turnover, regardless of appointment mode

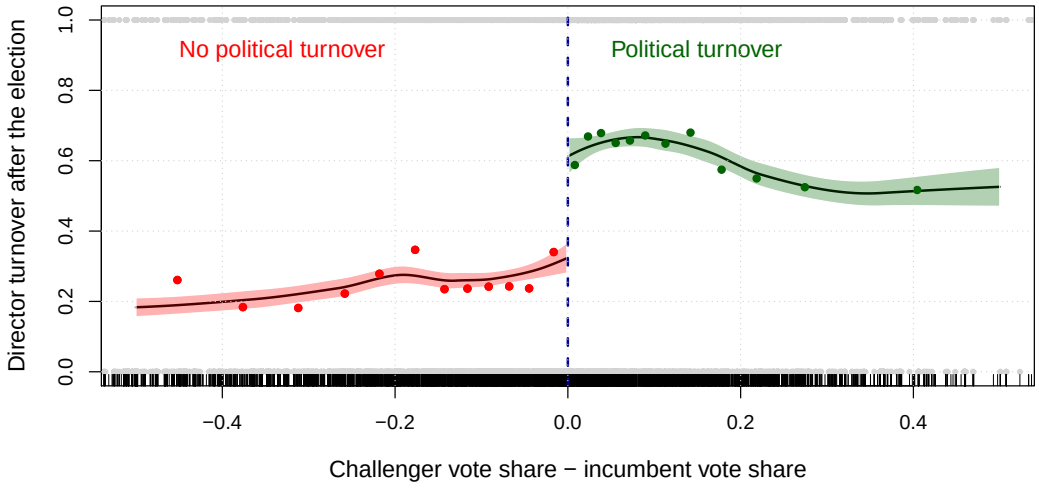
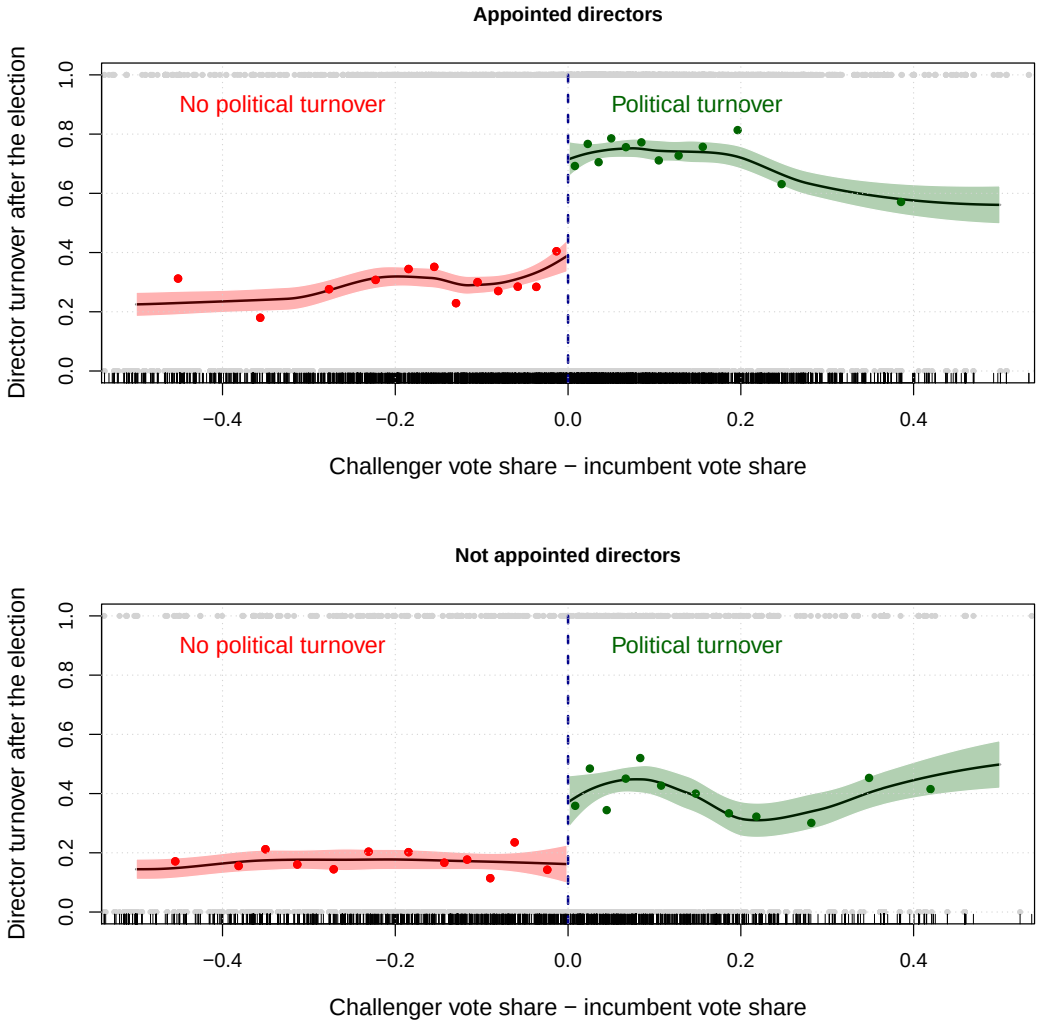


Figure 21: Effect of political turnover on bureaucratic turnover, by whether schools had appointed or not appointed director



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 (Sekhon et al., 2017). 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 un-appointed 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 *un-appointed directors*, estimating $p_0^{\neg a}$ and creating a trimmed sample of un-appointed 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 un-appointed 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 22: Histogram of the forcing variable: IDEB target - IDEB score

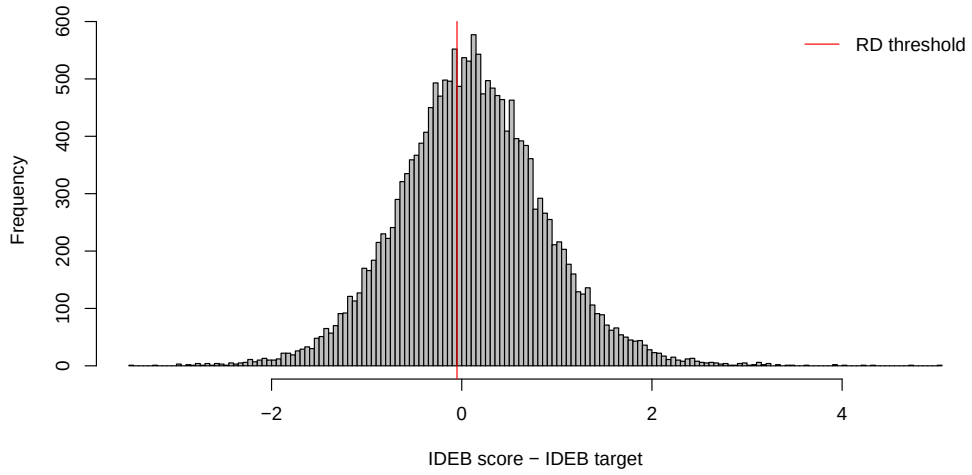
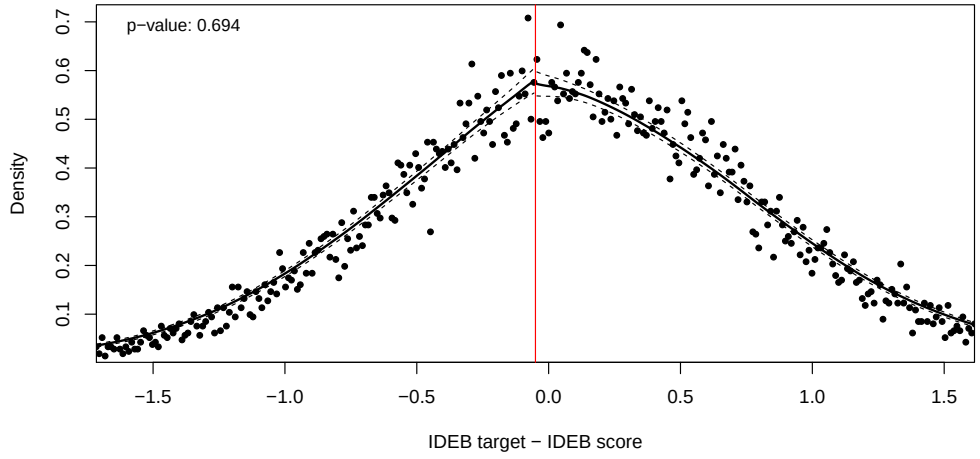


Figure 23: McCrary density test for discontinuity in the forcing variable



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

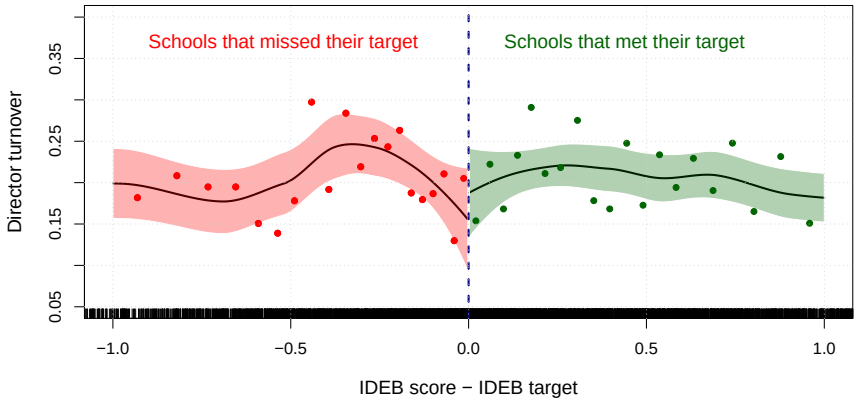


Figure 25: Effect of meeting the IDEB target on director turnover: tenured directors

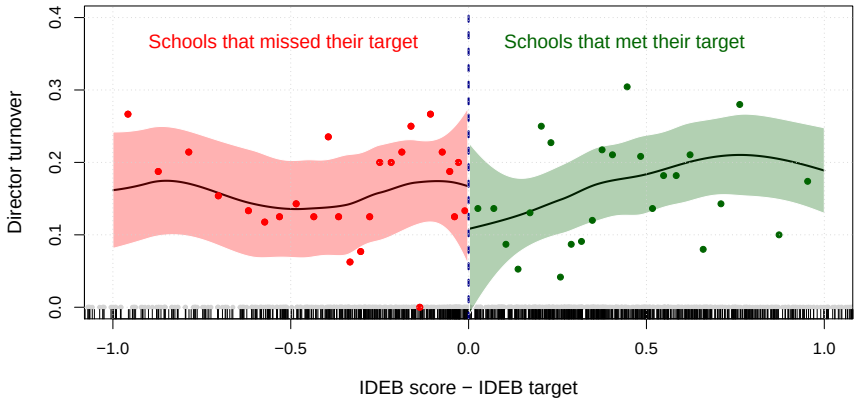
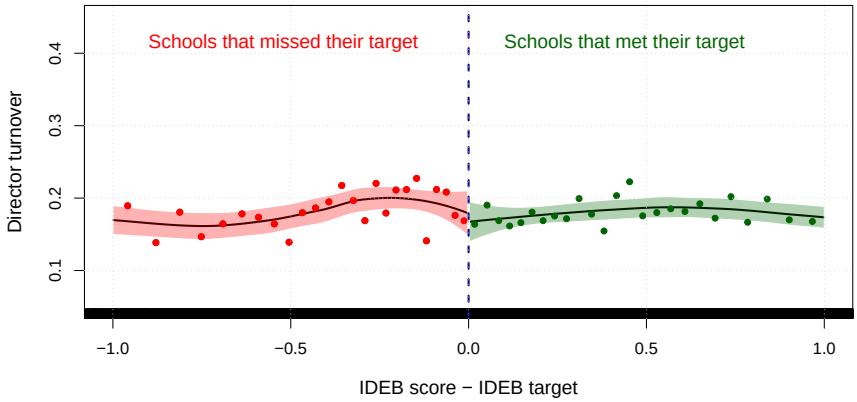


Figure 26: Effect of meeting the IDEB target on director turnover: all appointment modes



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."[80] In practice, the results of the election are usually determined by teachers, especially tenured ones. These dynamics of capture are

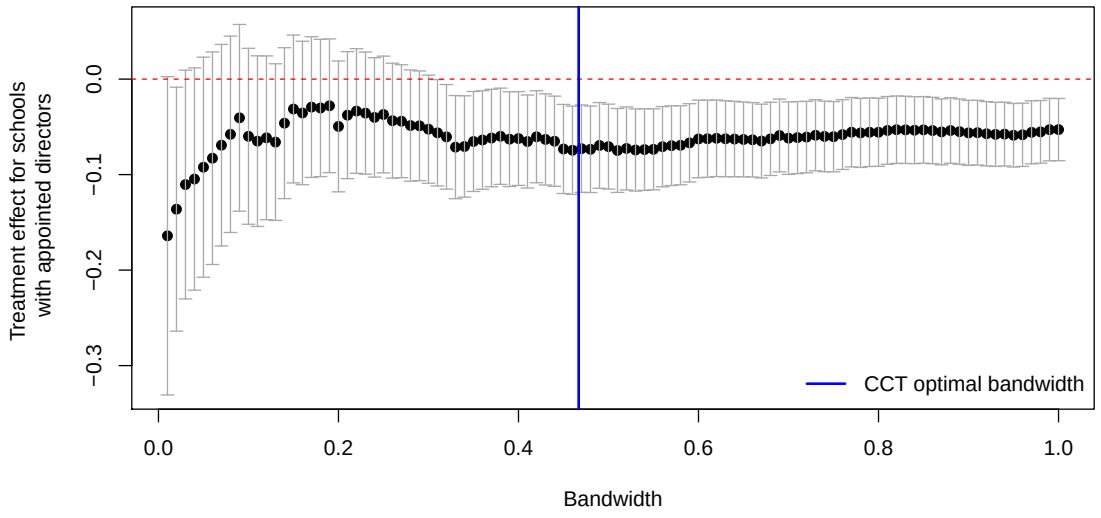
[80]School director interviewed in the state of Rio de Janeiro in February 2017.

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."[81]

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

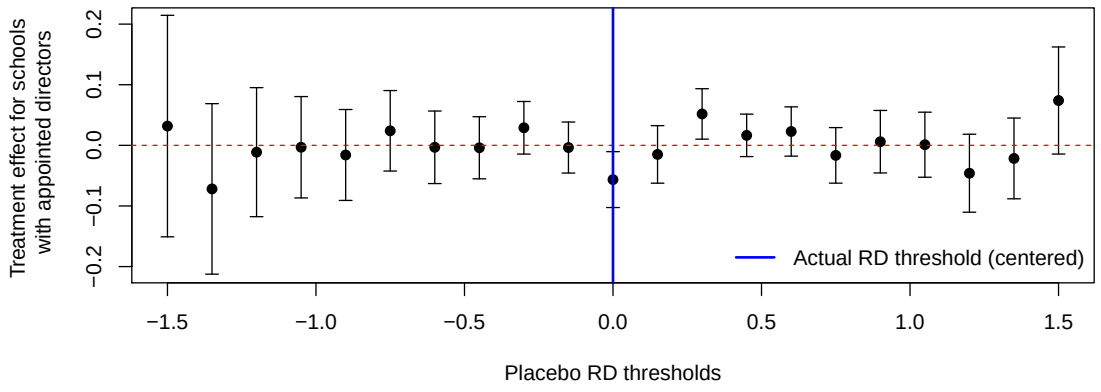
D.7 Alternative bandwidths

Figure 27: Sensitivity of model 3 in Table 3 to alternative bandwidths



D.8 Placebo tests varying the RD threshold

Figure 28: 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

E.1 Research assistants

The following 23 people provided excellent research assistance for the implementation of the field survey in Rio Grande do Norte: Jenair Alves, Marcos Aurélio Freire da Silva Júnior, Francymonni Yasmim Marques de Melo, Karoline de Oliveira, Raiany Juliete da Sila, Aline Juliete de Abreu Feliciano, Pedro Henrique Correia do Nascimento Oliveira, Ana Vitória Araújo Fernandes, Jaedson Gomes dos Santos, Ana Beatriz Germano Barroca, Renata Lima de Morais, Myleyde Dayane Pereira da Silva, Marina Rotenberg, Filipe Ramos Pinheiro, Daniele Vitória Lima da Silva, Elvira Gomes Santos, Matheus Oliveira de Santana, Magda Emanuele Lima da Silva, Ayanne Marília Sousa da Silva, Júlio César Nascimento, Lidiane Freire de Jesús, André Silva, and Pâmela Kaissa Fernandes Lopes.

E.2 Questionnaires

The survey instruments can be found online:

- Survey of school directors in Portuguese and in English
- Survey of clinic managers in Portuguese and in English
- Survey of social assistance center coordinators in Portuguese and in English

E.3 Location of the survey

Figure 29: Location of the face-to-face survey of street-level managers: Rio Grande do Norte, in blue; the Northeast region of Brazil, in grey

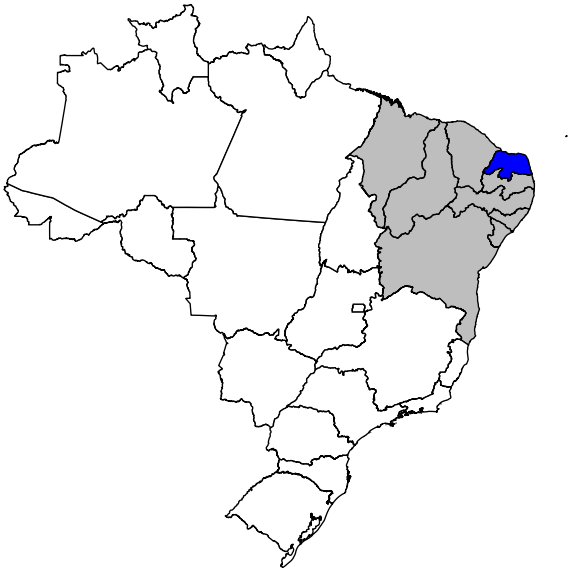


Figure 30: Municipalities in Rio Grande do Norte (colors correspond to the number of surveys done; white corresponds to municipalities excluded from the survey)

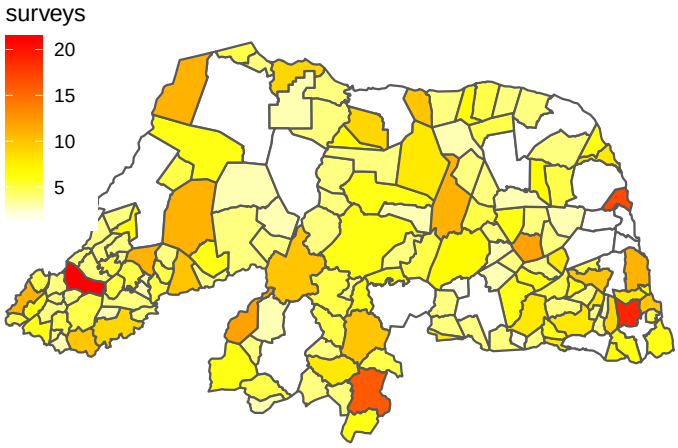
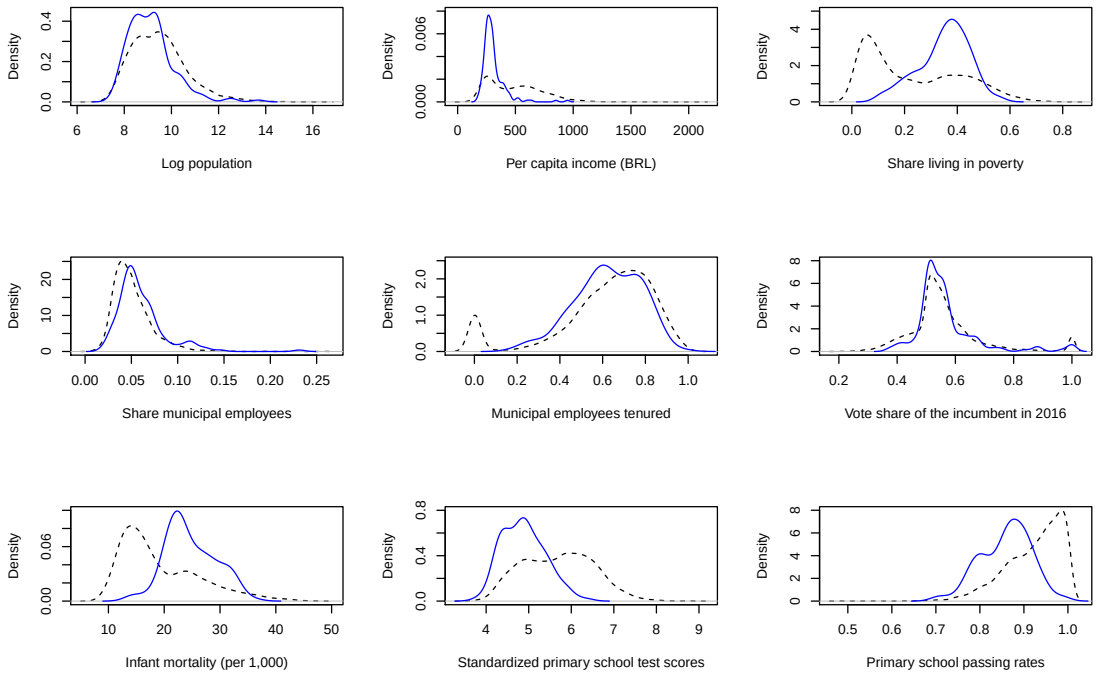


Figure 31: Statistics for municipalities in Rio Grande do Norte (continues, blue line), compared to all municipalities in Brazil (dashed, black line)



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

E.4 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.5 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.6 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.7 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.8 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 32: 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.9 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]?

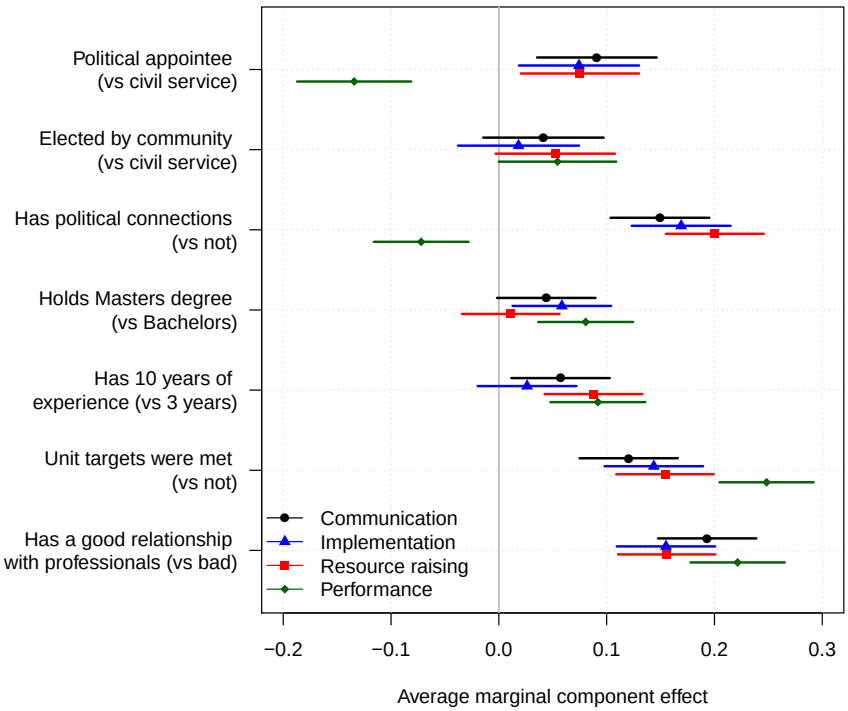
Table 29: Results of the conjoint experiment with street-level managers

	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.10 Conjoint results among un-appointed bureaucrats

Figure 33: Results from the face-to-face conjoint experiment with municipal street-level managers, excluding respondents who are political appointees



E.11 Conjoint results among bureaucrats who perceive politicians as more programmatic

Figure 34: Results from the face-to-face conjoint experiment with municipal street-level managers, excluding respondents who have lower perceptions of how programmatic politicians are

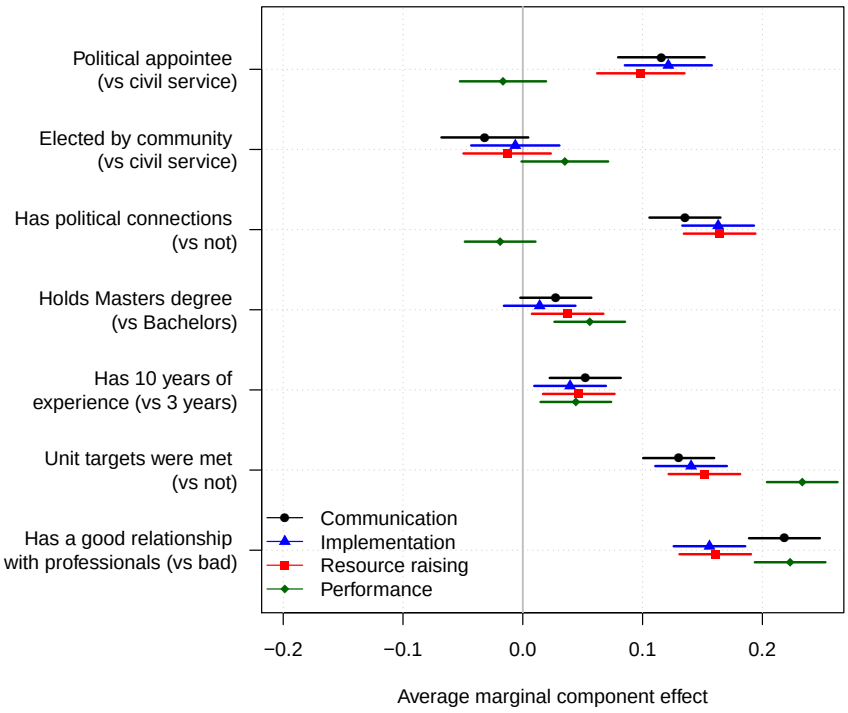


Figure 34 includes only responses from the 504 bureaucrats (54.4%) who expressed the highest level of agreement with the following statements:

- "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

F.1 Questionnaire

The survey instrument can be found online:

- Survey of politicians in Portuguese and in English

F.2 Respondent recruitment and non-response

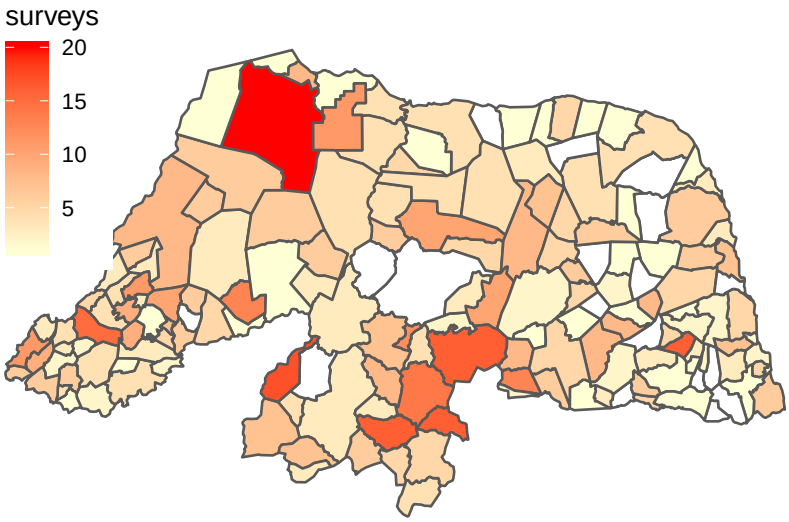
The state audit court of Rio Grande do Norte sent the survey to all mayors, city councilors, and secretaries of five key areas (education, healthcare, social assistance, finance, and human resources) in the 167 municipalities of the state. The survey was sent through the state audit court's online platforms. 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 35: Politician survey responses by municipality



F.3 Descriptive statistics

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

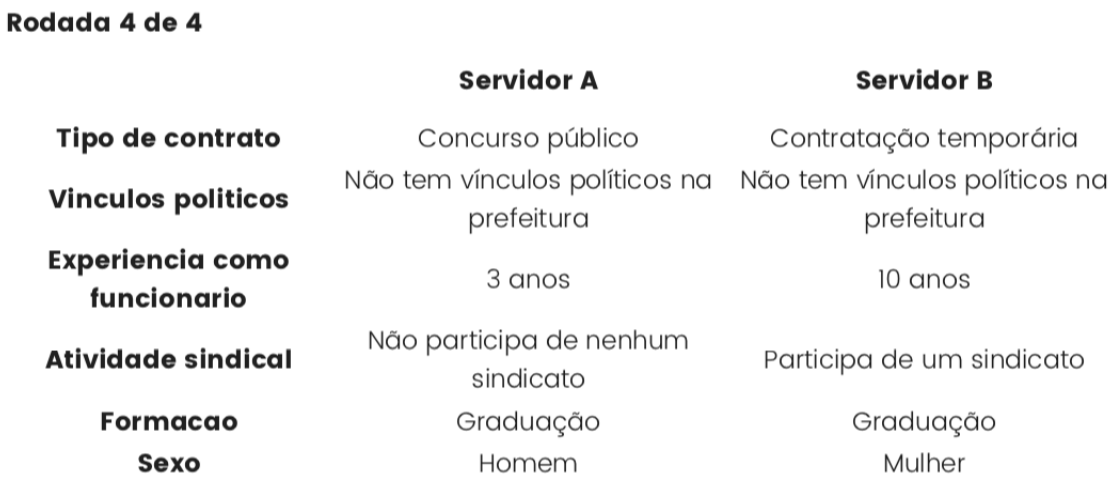
	All N=755		Mayors N=56		Secretaries N=468		City councilors N=231	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	43.37	10.92	49.88	11.86	41.69	10.42	45.22	10.82
Female	0.45	0.50	0.20	0.40	0.60	0.49	0.18	0.39
High school degree or less	0.22	0.41	0.38	0.49	0.07	0.26	0.48	0.50
College degree or more	0.65	0.48	0.52	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.4 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 36: Sample conjoint screen seen by politicians



F.5 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	0.081***	0.115***	0.174***	0.049***
	0.015	0.015	0.014	0.014
Political connections: No (baseline)				
Political connections: Yes	0.128***	0.069***	0.060***	-0.012
	0.015	0.015	0.014	0.014
Education: Bachelors (baseline)				
Education: Masters	0.022	0.039**	-0.005	0.108***
	0.013	0.014	0.013	0.014
Experience: 3 years (baseline)				
Experience: 10 years	0.050***	-0.004	-0.006	0.095***
	0.014	0.014	0.014	0.014
Union membership: No (baseline)				
Union membership: Yes	-0.036**	-0.027*	-0.053***	-0.001
	0.013	0.013	0.013	0.013
Gender: Female (baseline)				
Gender: Male	-0.047***	-0.064***	-0.044***	-0.081***
	0.013	0.013	0.013	0.013
Number of respondents	754	754	754	754
Number of valid profiles	6032	6032	6032	6032

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.

F.6 Conjoint results among mayors and among secretaries

Figure 37: Results from the face-to-face conjoint experiment with politicians, excluding all respondents except mayors

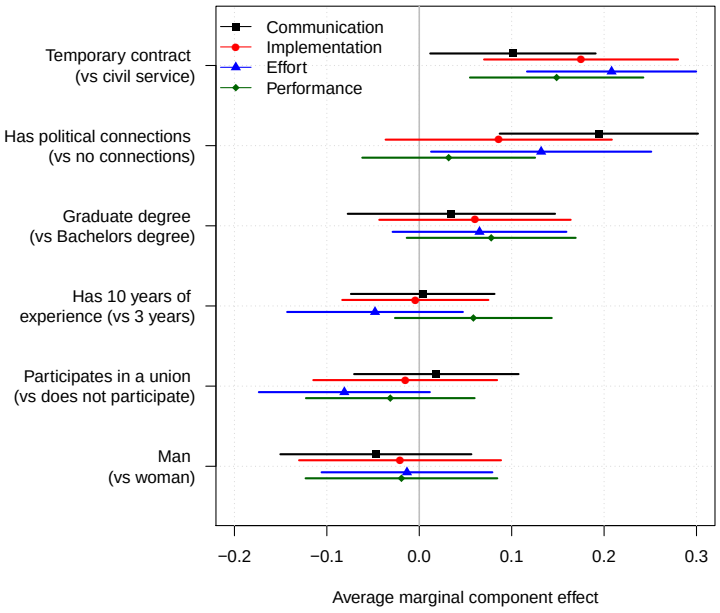


Figure 38: Results from the face-to-face conjoint experiment with politicians, excluding all respondents except secretaries