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

Guillermo Toral†

June 11, 2021

Latest version here

Abstract

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

*I am indebted to Ben Ross Schneider, Lily Tsai and Daniel Hidalgo for invaluable advice and guidance throughout the project. For useful comments I also thank Julián Aramburu, Felipe Barrera-Osorio, Héctor Blanco, Sarah Brierley, Josh Clinton, Julia Smith Coyoli, Aditya Dasgupta, Emmerich Davies, Belén Fernández Milmanda, Charles Gale, Sandra Gomes, Merilee Grindle, Frances Hagopian, Kosuke Imai, José Incio, Junyan Jiang, Paul Lagunes, Horacio Larreguy, Nina McMurry, Virginia Oliveros, Agustina Paglayan, Ignacio Puente, Jan Pierskalla, Pia Raffler, Blair Read, Tesalia Rizzo, Fritz Sager, Christian Schuster, Alberto Simpser, Guadalupe Tuñón, Julie Anne Weaver, and Liz Zechmeister; seminar participants at MIT, Harvard, Universidade Federal do Rio Grande do Norte, Vanderbilt, Yale-NUS, Tecnológico de Monterrey, Centro de Investigación y Docencia Económicas, Rutgers, NYU Abu Dhabi, Instituto Tecnológico Autónomo de México, Northwestern, and the Quality of Government Institute at the University of Gothenburg; and conference participants at APSA, MPSA, NEWEPS, and PolMeth. Special thanks to the following 23 people, who provided excellent research assistance on the survey of bureaucrats: Jenair Alves, Marcos Aurélio Freire da Silva Júnior, Francymonni Yasmim Marques de Melo, Karoline de Oliveira, Raiany Juliete da Silva, 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. I also thank the State Audit Court of Rio Grande do Norte, and in particular Gilberto Jales, Anne Carvalho, Victor Fernandes, Marcelo Araújo and Ilueny Santos, for making the survey of politicians possible. The Lemann Foundation, MIT GOV/LAB, MISTI and CIS provided financial support for fieldwork and surveys. Surveys and interviews were approved by MIT's Committee on the Use of Humans as Experimental Subjects (protocols 170593389, 1806407144 and 1810539206). Any errors are my own.

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

1 Introduction

The political appointment of bureaucrats –or patronage, in short[1]– is ubiquitous throughout the developing world (Grindle, 2012). Patronage is typically understood as a rent-seeking strategy in which 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 patronage can enhance bureaucrats' accountability and effectiveness.[2] Political appointments and connections provide bureaucrats with "upward embeddedness", i.e., social and political connections with politicians, which facilitate bureaucratic accountability and effectiveness. In particular, I argue that patronage gives bureaucrats access to material and non-material resources, facilitates monitoring by politicians, enables the application of sanctions and rewards, aligns priorities and incentives, and increases mutual trust. In certain contexts, patronage can thus make bureaucrats more accountable and effective at service delivery.

My argument is not that patronage is universally good, nor that it comes without costs. Indeed, the costs of patronage have long been recognized (Pollock, 1937), and recent studies have demonstrated how it can distort the allocation of public jobs and disincentivize bureaucratic performance (Xu, 2018; Colonnelli et al., 2020). In contrast to this traditional view of patronage as a system that changes who enters the bureaucracy or how much they work, my theory emphasizes its influence on *how* they work. By changing how bureaucrats work, patronage can be leveraged to extract rents, but also to deliver public services. In this paper I focus on the often overlooked advantages of patronage, and offer a theory of the conditions under which they can benefit public service delivery.

I outline three scope conditions to identify the contexts in which the benefits of patronage are more likely to enhance governance. First, in contexts where there are no easy substitutes for the advantages upward embeddedness provides. This is often the case in developing settings, and particularly in poor and small localities, which typically have dire financial constraints, small labor markets, and limited human capital. In these settings, a competitive, merit-based recruitment

[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 non-merit-based factors. The concept of patronage is contested, and the literature contains a variety of definitions that are both narrower and broader than mine.

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

process is less likely to be sufficient for selecting and motivating effective bureaucrats. Second, patronage may be particularly beneficial for service delivery when appointing "street-level managers" (Gassner and Gofen, 2018), namely bureaucrats such as school directors[3] or health clinic mangers who lead public service delivery units. The effectiveness of these managers, who occupy a critical position in between senior officials and front-line providers, depends on their ability to motivate and coordinate street-level employees, and to align their work with both senior officials' and citizens' demands. Political appointments and connections help street-level managers achieve these two tasks. Third, politicians must at least partly value the effective delivery of public services. This is more likely to be the case in contexts with electoral accountability and strong oversight institutions.

The argument that patronage can improve service delivery builds on insights from political science, public administration, and economics. First, previous comparative politics research has highlighted that patronage can facilitate party building (Sorauf, 1960; Huntington, 1968), nation building (Weingrod, 1968), interest aggregation (Scott, 1969), and state building (Grindle, 2012). Second, the public administration literature often treats the politicization of the bureaucracy in high-income countries as a resource that politicians use to tighten their control over policy and implementation (Peters and Pierre, 2004; Bach and Veit, 2017) and to build party networks (Kopecky et al., 2012). Third, American politics scholars often view political appointments as helping presidents increase policy control over federal agencies (Lewis, 2008; Aberbach and Rockman, 2009). Such studies commonly theorize a trade-off between policy control and bureaucratic performance (Moe, 1985). Finally, development economists have recently explored the theoretical possibility that patronage may help politicians address selection and agency problems, although no empirical evidence has been uncovered to support this idea (Xu, 2018; Colonnelli et al., 2020).

I build on these contributions to develop a theory that links patronage to public service delivery and specifies testable mechanisms and scope conditions. Theoretically, this paper departs from economic theories of the bureaucracy that assume a loyalty-competence trade-off. I instead argue that, in low-capacity settings, connections between bureaucrats and politicians may help not only decrease agency losses but also enhance bureaucrats' ability to do their jobs. Empirically, the paper contributes causal evidence of the benefits of patronage for service delivery in a developing context. Previous research has identified various governance advantages generated by connections among politicians at different levels of government (Jiang, 2018), among bureaucrats (Schneider, 1991), between politicians and party brokers (Brierley and Nathan, 2021), and between politicians and

[3]I use the term *school director* to refer to school leaders, also called principals, headmasters, or head-teachers.

citizens (Tsai and Xu, 2018). I contribute to this literature on the advantages of connections for governance by highlighting the value of ties between bureaucrats and politicians.

Empirically, I study patronage and its effects on bureaucratic effectiveness and accountability using three methods: quasi-experiments, surveys, and interviews. I focus on municipal governments in Brazil, a context where scholars have long noted the pervasiveness of patronage (Hagopian, 1996; Ames, 2001). A critical advantage of this environment is that political appointments coexist with other modes of bureaucratic selection, which enables comparisons within a single institutional environment and thus facilitates inference (Giraudy et al., 2019).

First, to test the core claims of the theory, I use two quasi-experimental studies that leverage administrative data on municipal schools throughout the country. The results of a difference-in-discontinuities study (hereafter diff-in-disc, a design that combines a difference-in-differences with a regression discontinuity) demonstrate that when politically appointed school directors lose their connections to the local government (after the mayor who appointed them is voted out) their schools experience a drop in quality, compared to schools with unappointed directors. This finding demonstrates that political connections can increase bureaucratic effectiveness. A separate regression discontinuity design (RDD) shows that politically appointed school directors who meet their target in a highly visible school quality indicator are less likely to be replaced, whereas meeting the target has no effect on the turnover of unappointed school directors. This result shows that patronage can enhance bureaucratic accountability.

Second, to test the mechanisms through which patronage can enhance bureaucratic effectiveness and accountability, I administered two original surveys in the Brazilian state of Rio Grande do Norte. The first was a face-to-face survey of 926 street-level managers (school directors, clinic managers, and social assistance center coordinators), representative of urban areas in 150 municipalities. Observational regressions indicate that appointed bureaucrats have more frequent contact with, higher levels of trust in, and better alignment with politicians than unappointed bureaucrats do. The results of a conjoint experiment embedded in the survey also show that managers expect bureaucrats 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. The findings of a separate online survey of 455 local politicians corroborate these results. Politicians' responses in another conjoint experiment reveal that they perceive bureaucrats with political connections as being more responsive, better at communicating with them, and more likely to exert greater effort.

Third, I used in-depth interviews to investigate the informal institutions of bureaucratic politics in Brazilian local governments, develop hypotheses, and probe mechanisms. During 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 seven states across three different regions of Brazil.[4] Specific accounts from local actors in widely diverging contexts help illustrate how appointments work in practice, as well as the costs and benefits of patronage.

The finding that political appointments and connections can improve bureaucratic accountability and effectiveness has at least three important implications for both researchers and policymakers. First, the paper suggests that, in some contexts, reforms designed to insulate local bureaucrats from politicians can have detrimental effects on service delivery, at least in the short term and when not complemented by significant increases in human capital that would strengthen the capacity of more autonomous bureaucrats. The paper thus contributes to an emerging literature on the costs of anti-corruption strategies (Rasul and Rogger, 2018; Jiang et al., 2020; Wang, 2021) and on how bureaucratic arrangements that deviate from the Weberian ideal can aid development (Ang, 2016; McDonnell, 2020). Second, the paper helps reconcile the standard view of patronage as a form of corruption with other views linking it to political development. Patronage can facilitate both rent-seeking and public service delivery precisely due to the governance advantages that upward embeddedness provides. This helps explain the remarkable resilience of patronage around the world (Grindle, 2012). Finally, the paper advances our understanding of the mechanisms through which political appointments facilitate policy control and implementation, and thus helps bridge the gap between comparative politics research on patronage in developing contexts, on the one hand, and public administration and American politics research on political appointments in high-income countries, on the other.

2 Theory

2.1 How patronage delivers

Patronage is usually understood as a rent-seeking strategy. Scholars have long studied the critical role of government jobs in clientelistic equilibria (Wilson, 1961; Robinson and Verdier, 2013). From this perspective, patronage is thought to impede development through the misallocation of public jobs and the depression of bureaucratic effort (Xu, 2018; Colonnelli et al., 2020).

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

I advance an alternative view of patronage that emphasizes its potential benefits for bureaucratic accountability and effectiveness. I start by proposing the concept of "upward embeddedness" to refer to bureaucrats' political and social connections to politicians. Scholars of bureaucratic governance often use the idea of embeddedness (Granovetter, 1985) to describe bureaucrats' (downward) 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 richer view of connections between bureaucrats and politicians than Weberian theories allow. From a Weberian perspective, it is the insulation of bureaucrats from politicians that increases bureaucratic effectiveness (Dahlström and Lapuente, 2017; Oliveros and Schuster, 2018). The concept of upward embeddedness also helps integrate different types of connections within bureaucracies into a single framework, including connections based on partisanship (O'Dwyer, 2006), family (Fafchamps and Labonne, 2017), and ethnicity (Hassan, 2020).

I argue that political appointments and connections upwardly embed bureaucrats, which provides a set of governance resources that can be beneficial for both bureaucrats and politicians. Depending on how these resources are used, patronage can enhance either rent seeking or public service delivery. In any case, upward embeddedness operates through five mechanisms: increasing bureaucrats' access to material and non-material resources, helping politicians monitor bureaucrats, facilitating the application of sanctions and rewards, aligning priorities and incentives, and increasing mutual trust. In the remainder of this section I investigate each of these mechanisms and their relevance for bureaucratic governance in turn.

First, upward embeddedness increases bureaucrats' access to political leaders and, through them, to material resources for public service delivery (e.g., funds), as well as non-material resources (e.g., legitimacy and authority). Both help mobilize and coordinate other bureaucrats. Prior studies have demonstrated that resources and legitimacy are important drivers of bureaucratic effectiveness (Dasgupta and Kapur, 2020; Carpenter, 2001).

Second, upward embeddedness facilitates politicians' monitoring of bureaucrats and reduces information asymmetries through shared political and social networks. Although in high-corruption contexts enhanced monitoring may fuel rent extraction (Brierley, 2020), in other development contexts bureaucratic oversight has been shown to improve government effectiveness (Gulzar and Pasquale, 2017; Raffler, 2020).

Third, upward embeddedness enhances bureaucrats' accountability to politicians by facilitating the application of 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 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 rewards can improve bureaucratic effectiveness (Ashraf et al., 2014; Bertrand et al., 2020). Shared social and political networks also enhance the application of informal sanctions and rewards.

Fourth, upward embeddedness fosters the alignment of priorities and values between bureaucrats and politicians by virtue of their common political background and shared networks. Bureaucrats often operate in highly complex environments that require them to multi-task and to negotiate contradictory priorities from different societal actors (Lipsky, 1980; Dasgupta and Kapur, 2020). In such challenging environments, the alignment of bureaucrats' and politicians' priorities can improve service delivery. Management scholars have long recognized the importance of alignment for organizations' performance (Biggs et al., 2014), and political scientists have shown political alignment can improve policy implementation (Williams, 2017; Niedzwiecki, 2018). Upward embeddedness also synchronizes the incentives of bureaucrats and politicians, given their shared fate. Unlike civil service bureaucrats, political appointees are usually fired after a change in government, which ties their incentives to those of the incumbent (Oliveros, 2021). As the formal literature on delegation has long recognized through the ally principle (Bendor et al., 2001), alignment between bureaucrats and politicians can alleviate principal-agent problems.

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

In summary, political appointments and connections foster bureaucrats' upward embeddedness, which provides a number of governance resources, namely access to material and non-material resources, monitoring technology, better ability to apply sanctions and rewards, alignment of priorities and incentives, and increased trust. There is, however, an inherent ambivalence in patronage. On the one hand, political appointments and connections can make political machines more effective at extracting rents. On the other hand, upward embeddedness can help governments deliver public

services more effectively, because service delivery also depends on the coordination and accountability of bureaucrats. I do not claim that patronage does not have costs: it may, for example, lead to the selection of bureaucrats with less education and experience. Rather, I argue that patronage has under-appreciated benefits for bureaucratic accountability and effectiveness that, in certain contexts, can be leveraged for service delivery.

2.2 Scope conditions

The benefits of patronage will be greater in contexts in which there are no easy substitutes for the governance advantages it provides. This is true in developing contexts, and particularly for local governments outside large metropolitan areas, where financial constraints are more dire and human capital is scarcer. This reduces governments' ability to use higher wages (Dal Bó et al., 2013) or performance pay (Hasnain et al., 2014) to improve bureaucratic selection and performance. In these challenging environments, the counterfactual to a political appointee is not necessarily the highly capable, autonomous and driven bureaucrat that Weberian theories presume. Without adequate human capital and incentives, civil servants may simply lack the capacity and motivation to deliver services. Patronage can thus alleviate developmental constraints on bureaucratic governance.

The effectiveness of street-level managers (e.g., school directors or clinic managers) is particularly likely to benefit from the advantages of upward embeddedness. These managers can significantly improve the quality of public services (Bloom et al., 2015; Tavares, 2015), but their effectiveness depends on their ability to coordinate efforts and align a complex set of tasks to objectives that are often multidimensional and hard to assess (Lipsky, 1980), especially in transaction-intensive services like healthcare or education (Pritchett and Woolcock, 2004). Upward embeddedness can help street-level managers leverage the necessary trust, legitimacy, and the ability to coordinate efforts and align teams (Gassner and Gofen, 2018).[5]

For the benefits of patronage to outweigh the costs, politicians must value public service delivery, be it due to intrinsic beliefs and norms (Habyarimana et al., 2018), political competition (Gottlieb and Kosec, 2019), electoral accountability (Healy and Malhotra, 2013), or anti-corruption institutions (O'Donnell, 1998). The availability of regular and credible measures of bureaucratic

[5]In contrast, the effectiveness of frontline providers like teachers or doctors is less dependent on their ability to coordinate efforts with other bureaucrats or with senior officials. The effectiveness of low-skill employees like clerks and cleaners is the least likely to benefit from the advantages of patronage because their tasks are simpler and easier to monitor.

performance can strengthen politicians' concerns about service delivery through all these mechanisms. Where politicians do not value service delivery, the governance advantages of upward embeddedness are more likely to be mobilized for extracting rents.

3 Institutional context

3.1 Formal institutions in Brazilian municipalities

Brazilian local governments are a particularly useful setting in which to study the impact of political appointments and connections on bureaucratic accountability and effectiveness, for three main reasons. First, local governments use a variety of systems to appoint bureaucrats. This enables comparisons within a single institutional environment, which makes inference easier (Giraudy et al., 2019). Second, the federal government publishes rich datasets about local bureaucracies. This facilitates measuring variation in local governance and service delivery. Third, Brazilian municipalities vary widely in their economic and political development. This variation makes them a relevant case for other contexts beyond Brazil.

Brazil is a federal country with 5,570 municipalities, which together are responsible for providing primary education, healthcare, and social assistance to over 200 million people (Arretche et al., 2020). Local governments spend in average around 60% of their revenue (which mostly comes from inter-governmental transfers) in these three sectors.[6] Financial constraints are usually dire, and employees' salaries are low.[7] Due to municipalities' prominent role in service provision and to the scarcity opportunities in the private sector, local governments are typically a very important employer, hiring on average 4.7% of the local population and 38.2% of those who have jobs in the formal sector (Appendix A.1). From the perspective of the local government, however, these are small labor markets with low levels of human capital.[8] Most municipalities are small (with a median population of fewer than 12,000 people) and far from state capitals, which makes it hard to attract talent.

Municipal elections are held every four years to elect a mayor (through a majoritarian system) and city councilors (though a proportional, open-list system).[9] Mayors, who can run for re-election

[6] My own calculations using data from the National Treasury.

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

[8] For example, management skills in public schools and hospitals are low (Appendix A.2).

[9] State and federal elections are held two years before/after municipal elections.

only once, appoint secretaries to run specific policy areas. Mayors, city councilors, and secretaries are overseen by a network of horizontal accountability institutions (including audit courts, prosecutors' offices, and standard courts) that have been shown to reduce rent extraction (Avis et al., 2018; Litschig and Zamboni, 2019). Federal and state governments also oversee municipal governments, especially on their use of transfers and on their performance in delivering public services paid for by these transfers (Gomes, 2009). The federal government regularly measures and publicizes the performance of municipal bureaucracies in several areas.

Municipal governments maintain a network of schools, health clinics, and social assistance centers to serve the local population. While there are strict legal constraints on how frontline providers (e.g., teachers) are hired,[10] laws give politicians considerable discretion for the selection of street-level managers (e.g., school directors). Managers are typically political appointees, but they can also be elected (by community members or professionals) or deployed through the civil service (tenured for life after passing a competitive examination).[11] Among the managers I surveyed, political appointees have significantly lower levels of education than those chosen via other methods, are more likely to live in the municipality where they work, and are more likely to have worked for a local electoral campaign (Appendix E.3). This demonstrates that appointments are based on political criteria rather than merit. Multiple appointment systems often coexist within the same municipality, particularly in the education sector.[12] Government data on education show that political appointments are more common in smaller and poorer municipalities, and in schools serving students of a lower socioeconomic status (Appendix A.3).

Municipal politicians generally value public service delivery, at least partly. Recent experimental evidence 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., 2021). My survey of politicians provides some additional evidence. A large majority of the mayors (76%) declared that they have the highest level of responsibility for improving the quality of public services, out of a list of seven actors. Secretaries of education, healthcare and social assistance report, on average, about one weekly meeting with street-level managers in their area.

[10] For example, the Brazilian constitution stipulates that bureaucrats are by default to be hired under a civil service regime. In practice, many of them are employed under temporary contracts.

[11] Civil service managers cannot be fired, but they can be transferred to a different unit.

[12] As of 2017, about 65% of municipal directors were appointed by politicians, 24% elected by the community, 5% tenured in a civil service regime, and the rest were appointed through other means.

3.2 Informal institutions in bureaucratic appointments

Interviews with bureaucrats and politicians provided valuable insights into the informal institutions that govern bureaucratic appointments in Brazilian municipalities. In practice, mayors usually appoint street-level managers, sometimes in consultation with city councilors in their coalition or with the corresponding secretary. Secretaries are in charge of selecting temporary street-level bureaucrats (like teachers or nurses), who generally hold one-year contracts. It is generally secretaries, and not street-level managers, who control the hiring, firing, or transferring of street-level bureaucrats.[13]

Street-level manager posts are important for politicians, given their strategic position in social networks, their visibility within the community, and their wide territorial reach in both urban and rural areas. In clientelistic settings, street-level managers can be useful for political mobilization. An elected director reported that "at the time of elections, [a previous, appointed director] asked school staff to wear the party's t-shirt, intimidating temporary teachers with the possibility of them losing their contract, and intimidating tenured teachers with them being transferred to another school. [...] People were expected to go to the city councilor's rally."[14] During my interviews, bureaucrats under different appointment systems and politicians conveyed multiple accounts like this, stressing how the resources, monitoring, and accountability of upward embeddedness were used for extracting rents.

More frequently, interviewees reported that political connections were leveraged to improve public services. Both street-level managers and secretaries suggested that the political appointment system pressures bureaucrats to work more and to be more responsive to community needs and local government demands. For example, one secretary said: "our directors are political appointees, but we do it with some criteria, including that they have a university degree, that they live in the community, that they communicate well [...]. But it has to be someone we trust, that's why political appointments matter." When I asked them what trust was 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, they manage many people."[15]

Interviewees also mentioned the advantages of political connections for bureaucratic account-

[13]See Appendix A.4 for diagrams of accountability relationships under different appointment modes.
[14]School director interviewed in the state of Rio de Janeiro in February of 2017.
[15]Secretary of education interviewed in the state of Paraíba in August 2018.

ability. For example, one 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."[16] When I asked a secretary whether they felt any difference in the relationship to elected versus appointed directors, they said: "yes, absolutely. One would expect elected directors to be better, that we would see them 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."[17]

Finally, street-level managers recounted how political connections helped increase the material and non-material resources they can leverage on the job. For example, when I asked a bureaucrat what connections were valuable for, they said: "Things are really hard with connections already, I do not know what I would do without them. [...] For example, we do not have running water in the center, and it is thanks to political connections that I manage to get a water truck to come and fill our tank. That requires an articulation with the secretary of transportation and other actors – I only manage that thanks to my connections to the mayor."[18]

Taken together, my interviews with municipal street-level managers and secretaries suggest that the political appointment of bureaucrats can respond to concerns about both rent-seeking and service provision. Political appointments may come with some costs (like the deployment of bureaucrats with less education or experience), but politicians often appoint street-level managers based on their professional abilities –not just their political ones– and leverage political connections to improve public services.

While the interviews helped me develop and probe hypotheses, as well as design quasi-experiments and interpret their results, it is hard to quantify relationships using qualitative data. The next section turns to quantitative evidence.

4 Empirical evidence

I leverage three types of data and methods to test whether upward embeddedness enhances the accountability and effectiveness of street-level managers. First, I use a diff-in-disc to show that an electoral defeat of the mayor causes a drop in the quality of schools with appointed directors, relative

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

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

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

to schools with unappointed directors. This shows that upward embeddedness helps bureaucrats deliver public services. Second, I use an RDD to show that appointed directors (but not elected or civil service ones) are less likely to be replaced if they meet their school quality target. This is consistent with upward embeddedness enhancing bureaucratic accountability, and with politicians caring about public service delivery. Third, I leverage original surveys of bureaucrats and politicians, including conjoint experiments, to show that upwardly embedded bureaucrats have more access to, trust in, and alignment with politicians. They are also 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, together with the interviews, demonstrate that bureaucrats' upward embeddedness can enhance their accountability and effectiveness. Table 1 synthesizes the links between the theory and the empirical tests presented below.

Table 1: Mapping of theory to empirics

Theoretical claims	Test	Data	Evidence
Core arguments: Upward embeddedness facilitates...			
... bureaucratic effectiveness in service delivery	Diff-in-disc	Administrative data	Figure 2
... bureaucratic accountability	RDD	Administrative data	Figure 3
Mechanisms: Upwardly embedded bureaucrats...			
... have higher levels of trust in, alignment with, and access to politicians	Correlations	Bureaucrat survey	Figure 4
... communicate better with and are more responsive to the government	Conjoint	Bureaucrat and politician surveys	Figures 5 and 6
... have more access to material resources	Conjoint	Bureaucrat survey	Figure 5
... exert more effort	Conjoint	Politician survey	Figure 6

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

A central idea to the theory in section 2 is that upward embeddedness helps politically appointed bureaucrats deliver public services. If that is the case, political turnover should differentially impact the effectiveness of appointed and unappointed bureaucrats. For appointed bureaucrats, mayoral turnover means a decrease in upward embeddedness, and therefore a reduction in the governance resources that help them deliver public services. However, mayoral turnover does not change the upward embeddedness of unappointed bureaucrats. Both types of bureaucrats are exposed to the general effects of political turnover, including the organizational costs of transition, the benefits

of a renewed leadership, and other shocks to the bureaucracy (Akhtari et al., 2020; Toral, 2021). If my theory is correct, appointed bureaucrats' performance should worsen as a result of political turnover compared to their unappointed counterparts.

To exploit the differential impact of political turnover on upward embeddedness, I use a diff-in-disc (Grembi et al., 2016). In essence, this design combines a difference-in-differences (comparing the performance of appointed and unappointed bureaucrats, before and after the election) with a close-races regression discontinuity (comparing the performance of bureaucrats in municipalities where the mayor lost their bid for re-election to those in municipalities where the mayor was re-elected).[19] I use data for municipal school directors, for whom the federal government releases a performance metric (based on student test scores and passing rates) every two years as well as an administrative survey that includes data on their mode of selection. The results show that an electoral defeat of the mayor causes a 0.25-standard-deviation drop in the quality score of schools with appointed directors compared to those with unappointed directors ($p < 0.05$).

4.1.1 Design

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

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

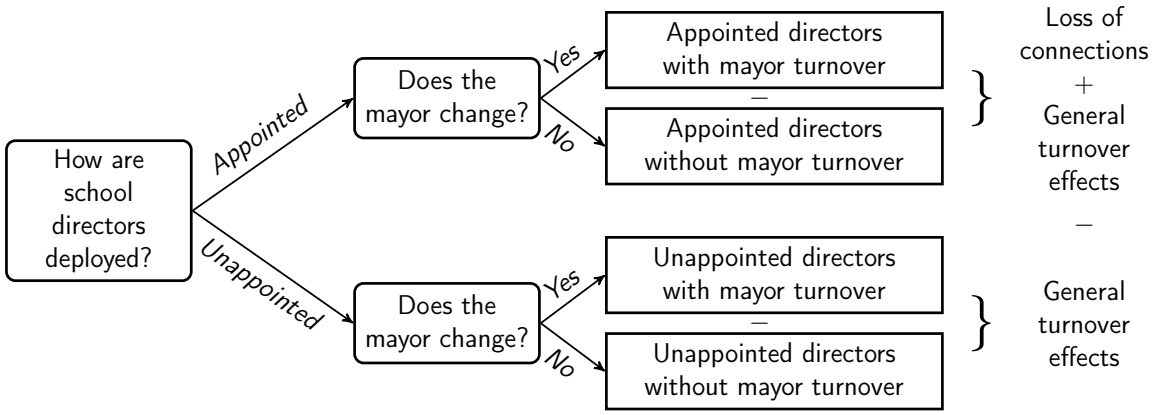
$$U_{sm} = \begin{cases} 1 & \text{if } D_m > 0 \text{ and } A_{sm} = 1 \quad \text{(mayor loses re-election, director was appointed)} \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

To separate the effect of a decrease in upward embeddedness from that of political turnover, I

[19] I focus on the electoral performance of the mayor rather than their political party because this setting is characterized by weak partisan attachments and pervasive party switching by politicians (Boas et al., 2019; Klašnja and Titiunik, 2017). Incumbents often switch parties before seeking re-election (e.g., 26% mayors did so in 2016), so examining the electoral performance of the incumbent party would be misleading.

exploit the difference between appointed directors (who lose upward embeddedness when the mayor fails to win the re-election) and unappointed directors (whose upward embeddedness is not affected by political turnover). Figure 1 illustrates the logic of the design.

Figure 1: Diagram of the difference-in-discontinuities design



The design exploits three sources of variation in performance: within-bureaucrat changes from before to after the election, between appointed and unappointed bureaucrats, and between localities with and without political turnover.

Potential outcomes are 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:

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

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

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

This design relies on three assumptions (Grembi et al., 2016). First, potential outcomes should be continuous in the forcing variable around the threshold. Appendix C.1 shows that pre-treatment covariates are continuous around the cutoff. Second, the effect of political appointment when there is no change to upward embeddedness should be constant over time, such that schools with appointed and unappointed directors follow parallel trends. Appendix C.2 shows that schools with

appointed and unappointed directors, as well as schools in municipalities with and without political turnover, follow parallel trends in performance before the election. With these two assumptions, the diff-in-disc estimates the causal effect of a decrease in upward embeddedness, close to the threshold, and for appointed directors. If we make a third homogeneity assumption that the effects of the decrease in upward embeddedness and of political turnover do not interact, then we can recover the LATE of a decrease in upward embeddedness for schools in municipalities around the threshold.

The design focuses on within-director changes in performance, from before to after the election. I conduct this analysis on the subset of schools for which the director was assigned in the three years before the election, and was still in their post one year after the election. Schools that experience director turnover in this period are excluded from the sample because their changes in performance cannot be associated to changes in the director's upward embeddedness. However, since director turnover increases after the election, this may introduce sample selection bias (Heckman, 1979). In Section 4.1.4 I discuss this issue more fully, and 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 remain fully below zero.

The design focuses on the relatively short-term effects of a decrease in upward embeddedness. Elections took place in October of 2016, winners took office on January 1st of 2017, and the next student tests were done in October and November of that of that same year. Improving student learning is a complex task that requires long-term efforts. Still, actions taken in the months leading up to the tests can have a significant impact on the results, including offering special remedial classes, doing practice tests, and raising awareness among teachers and students of the importance of student evaluations. All of these actions depend critically on directors' management efforts and their coordination with both school staff and the local government.[20]

4.1.2 Estimation and inference

I use local linear regression with a triangular kernel, as recommended by Cattaneo et al. (2019), within the bandwidth selected by the Calonico et al. (2020) algorithm,[21] and apply it to the following estimating equation:

[20] Consistent with short-term director actions having the potential to improve school performance, civil society organizations and governments regularly produce materials to help directors prepare schools for the tests in the months immediately before them. Appendix A.6 includes a sample of such materials.

[21] Results of both the diff-in-disc and the RDD are similar when using a uniform kernel.

$$Y_{sm} = \alpha + \beta_1 P_m + \beta_2 D_m + \beta_3 P_m D_m + A_{sm}(\gamma_1 + \gamma_2 P_m + \gamma_3 D_m + \gamma_4 P_m D_m) + \sum_{k=1}^{K} \eta_k X_{sm}^k + \varepsilon_{sm} \quad (5)$$

Y_{sm} is the change in the quality score of school s in municipality m, from 2015 to 2017. $\sum_{k=1}^{K} \eta_k X_{sm}^k$ is a set of state fixed effects and director-, school-, and municipality pre-treatment covariates that significantly predict whether the director is politically appointed,[22] which I include in some specifications to partially address the endogeneity of appointment modes. ε_{sm} is the error term. Standard errors are clustered at the municipality level, where political turnover is determined. If the diff-in-disc assumptions hold, γ_2 identifies the LATE, around the threshold, of a decrease in upward embeddedness.

4.1.3 Data

I leverage regular, valid, and well-established measurements of school performance conducted by the federal government every two years through the National Assessment of School Performance (ANRESC, *Avaliação Nacional do Rendimento Escolar*).[23] This system tests students at the end of primary and middle school in public schools across the country. Exams are based on item response theory, which ensures that its measures of learning outcomes are valid and comparable over time. ANRESC also surveys school directors and teachers, collecting data about their appointment, experience, demographics, and perceptions of the school. The federal government uses the test results and administrative data on student passing rates to calculate for each school a score in the Basic Education Development Index (IDEB, *Índice de Desenvolvimento da Educação Básica*). IDEB scores are normalized to range from 0 to 10. The system is managed by INEP, an autonomous, high-capacity federal agency (Bersch et al., 2017). All in all, and in words of a group of World Bank economists, ANRESC is "one of the world's most impressive systems for measuring education results" (Bruns et al., 2012, 7).

I use IDEB data for all municipal primary schools, in the years immediately before and after the 2016 elections.[24] I use the government's survey of directors to identify schools where the director was deployed in the years leading to the election and remained their post one year after, and to identify their mode of selection. I merge the school-level data with data on municipal election

[22] Appendix A.3 reports the results of the correlational regressions of appointment modes on covariates.
[23] Appendix A.5 details the timeline of ANRESC tests and the publication of results.
[24] I focus on the 2016 election because before 2013 the questions on director turnover and on director appointment mode have different response options and higher levels of non-response.

candidates and their performance, obtained from Brazil's Supreme Electoral Court.

4.1.4 Results

Table 2 reports the diff-in-disc results. A decrease in upward embeddedness (identified by the differential effect of political turnover on appointed versus unappointed directors) damages school performance. In particular, as shown in model 2, it reduces the school quality score by 0.28 points or 0.25 standard deviations ($p < 0.05$). Figure 2 illustrates the two discontinuities on which the design is based. The results are robust to the inclusion of state fixed effects and covariates predictive of appointment (models 3-4) and to alternative bandwidths (Appendix C.3).

Table 2: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors

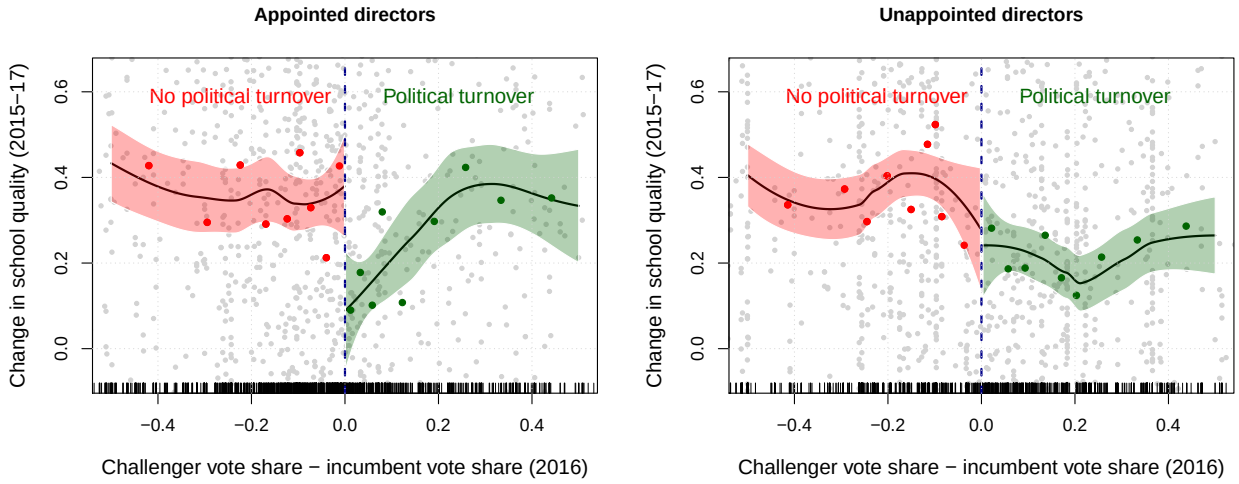
	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.161*	-0.000	-0.007	0.003
	(0.069)	(0.097)	(0.095)	(0.094)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.283*	-0.316*	-0.377**
		(0.128)	(0.124)	(0.125)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.204	0.206	0.206	0.206
N	1628	1623	1623	1569

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

A potential concern with this design is that it may suffer from sample selection bias because, in order to examine within-director changes in performance, schools for which the director changed after the election are excluded from the sample. Director turnover, however, is directly affected by mayor turnover (Appendix C.4). This generates groups of schools (under mayor re-election and mayor turnover) that are not necessarily comparable. I address this issue through three complementary strategies. First, I show in Appendix C.5 that when the mayor is not re-elected, directors with better performance at baseline or who have a number of characteristics associated with performance are significantly more likely to stay in their post.[25] This implies that including in the

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

Figure 2: Effect of political turnover on school quality, by director appointment mode



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

analysis schools without mayor turnover that would have changed directors if there had been mayor turnover biases the results towards zero. Second, I show in Appendix C.6 that diff-in-disc estimates are larger and significant when pre-processing the data with matching on the covariates that predict directors to be replaced after mayor turnover. Third, I show in Appendix C.7 that sharp bounds adapted from Lee (2009) to this diff-in-disc setting remain fully below zero. This suggests that, even in the worst-case scenario of sample selection bias, the diff-in-disc estimate would be negative.

To explore potential mechanisms behind the results in Table 2, I use the ANRESC director survey, and in particular survey items about obstacles directors find while managing the school. Consistent with the theory of upward embeddedness, political turnover differentially depresses appointed directors' responses to a binary question about whether their work is supported by higher instances (Table 3).[26] On the other hand, I find no evidence in favor of alternative mechanisms that would not support the theory: there are no significant, differential changes in directors' reports of problems with financial resources, the supply of teachers, or teacher turnover (Appendix C.8). Another alternative mechanism might be that appointed directors who remain in their post after mayor turnover anticipate being replaced later. Yet, most directors who are not replaced in the first year of a new administration continue in their post three years after the election.[27]

[26] This result is also robust to alternative bandwidths (Appendix C.3).

[27] 63.3% of the directors that enter the diff-in-disc sample with mayor turnover remained in their post in late 2019, versus 69.7% in the re-election group. Others could have been relocated to another school.

Table 3: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting their work is supported by higher instances, for appointed versus unappointed directors

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Political turnover	-0.059	0.089	0.070	0.069
	(0.049)	(0.083)	(0.080)	(0.081)
$\hat{\gamma}_2$: Political turnover × Appointed		-0.239*	-0.229*	-0.267*
		(0.110)	(0.110)	(0.104)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.178	0.177	0.177	0.177
N	1587	1583	1583	1528

See notes under Table 2.

In sum, I find that a mayor's lost bid for re-election hurts the quality of schools with directors that had been appointed by them, when compared to schools with unappointed directors. This demonstrates that a drop in upward embeddedness jeopardizes bureaucratic effectiveness. The results therefore suggest that patronage can be leveraged to improve public service delivery, and that upward embeddedness can substitute for at least some of the potential disadvantages of political appointees.

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

To test whether the political appointment of bureaucrats enhances accountability, I leverage an RDD in which I study the effect of schools meeting their quality target on the probability that the director is replaced. Among schools with appointed directors, meeting the quality target reduces director turnover by 0.19 standard deviations ($p < 0.01$). For schools with elected or civil service directors, the rate of director turnover is not affected by whether they meet their target. These results support the hypothesis that political appointments enhance accountability.

4.2.1 Design

Along with establishing IDEB as a system for measuring the quality of public schools, the federal government defined targets for the period 2007 to 2021 using an algorithm that projected schools' progress along logistic trajectories (Fernandes, 2007). Thus, every two years schools receive 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. If the difference is negative, it missed its target. I exploit this discontinuity to measure the causal effect of a school meeting its target in the 2013 test (the results of which were published in 2014) on its director being replaced between 2014 and 2015, and to explore heterogeneity by the director's appointment type.[28] IDEB scores are widely disseminated, and the emphasis is usually on whether targets were met (Boas et al., 2020). While local actors have other sources of information about the quality of schools, IDEB scores serve as simple, reliable metrics and thus facilitate common knowledge and accountability.[29]

More formally, treatment for school s (meeting the school quality target), T_s, is assigned by the difference between its quality score and target ($D_s = score_s - target_s$):[30]

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

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 LATE around the cutoff by taking the difference in means from above and below the threshold:

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

The key assumption of this design is that potential outcomes are continuous around the threshold. Reassuringly, the forcing variable and pre-treatment covariates are continuous around the cutoff (Appendix D.1).

[28] I focus on 2013-2015 to avoid years with municipal elections.

[29] 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).

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

4.2.2 Estimation and inference

I use local linear regression with a triangular kernel (Cattaneo et al., 2019) within the optimal bandwidth (Calonico et al., 2020), applied to the following estimating equation:

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

Y_s indicates whether school s experienced director turnover between 2014 and 2015. ε_s is the 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, I examine the heterogeneous local average treatment effect or HLATE (Becker et al., 2013). To estimate it, I allow for separate slopes for appointed and unappointed directors:

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

A_s indicates whether the school's director in 2013 was appointed. $\beta_1 + \gamma_2$ identifies the HLATE for appointed directors, under two additional assumptions. First, the subgroup indicator A_s must be continuous around the threshold, as shown in Appendix D.1. Second, the subgroup indicator must be conditionally ignorable, such that around the threshold and conditional on their distance to it, schools with appointed and unappointed directors do not differ systematically in a way that affects their turnover. To relax this assumption, I include $\sum_{k=1}^{K} \eta_k X_s^k$: state fixed effects and a vector of director-, school-, and municipality-level pre-treatment covariates that predict whether the school has an appointed director (Appendix A.3).

4.2.3 Data

I use ANRESC data on primary education quality scores and on directors' mode of selection and turnover. I code a school as having director turnover when the director in 2015 reports they have been in their post for one year or less.[31]

4.2.4 Results

Table 4 presents the RDD results. Model 2 demonstrates that, among schools with an appointed director, 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 ($p < 0.01$).

[31] School directors are not identified, so I cannot track the destination of those who are replaced.

Figure 3 visualizes this effect. Among elected or civil service directors, however, meeting the target does not significantly change in the probability of turnover.[32] The results are robust to the inclusion of state fixed effects and covariates that predict whether directors were appointed (models 3-4).

Table 4: Regression discontinuity estimates of the effect of reaching the performance target on director turnover, by whether the director was politically appointed

	(1)	(2)	(3)	(4)
$\hat{\beta}_1$: Quality target met	-0.033	0.016	0.015	0.005
	(0.018)	(0.027)	(0.027)	(0.028)
$\hat{\gamma}_2$: Quality target met × Appointed		-0.089*	-0.088*	-0.082*
		(0.036)	(0.036)	(0.037)
$\hat{\beta}_1 + \hat{\gamma}_2$		-0.073**	-0.073**	-0.077**
		(0.023)	(0.023)	(0.024)
State fixed effects			✓	✓
Predictors of Appointed				✓
Bandwidth	0.518	0.516	0.516	0.516
N	8458	8387	8387	7734

Predictors of whether the director is appointed come from a regression detailed in Appendix A.3. HC1 standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

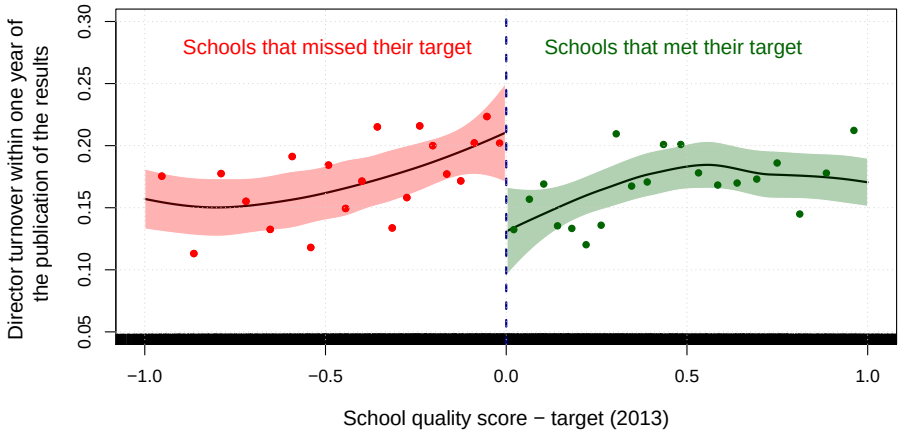
Additional robustness checks lend further support to these results. Alternative bandwidths lead to similar estimates (Appendix D.4). Placebo tests changing the RD threshold generally return insignificant results (Appendix D.5). The results are larger in municipalities with a large, programmatic party in office (Appendix D.6), which is consistent with directors being held accountable for their performance in service delivery.

In summary, these results demonstrate that appointed directors are held accountable for school quality, while elected and civil service ones are not. This suggests that upward embeddedness facilitates bureaucratic accountability, and that politicians consider public service deliver when making political appointments. Multiple interviewees made statements consistent with this. For example, a secretary of education said "the school's IDEB is a factor to decide if the director continues or not."[33]

[32] Appendix D.2 reports the null results among elected and among civil service directors. Appendix D.3 discusses how low competition, capture, and low participation limit the ability of director elections to promote accountability. While tenured directors generally cannot be fired, they can be transferred to a different school.

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

Figure 3: Effect of meeting the performance target on director turnover, for schools whose director was politically appointed



See notes under Figure 2.

4.3 Local actors perceive political appointments and connections as making bureaucrats more responsive: Survey evidence

To explore how upward embeddedness fosters bureaucratic effectiveness and accountability, I leverage a face-to-face, representative survey of 926 street-level managers and an online survey of 455 local politicians. The results from conjoint experiments in these surveys suggest that upwardly embedded bureaucrats 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 (with 23 research assistants that I hired, trained, and coordinated) a face-to-face, representative survey of municipal street-level managers (school directors, health clinic managers, and social assistance center coordinators) in late 2018. This is, to my knowledge, the first representative survey of street-level managers to collect data about their political connections and attitudes.

The survey was administered in Rio Grande do Norte, a state in the heart of Brazil's Northeastern region. The Brazilian Northeast has historically been characterized by inferior development outcomes, corruption, and clientelism, especially in the municipalities of the interior (Leal, 1948;

Nichter, 2018; Frey, 2020).[34] The survey focused on the urban areas of 150 small and medium-sized municipalities – all but the largest 17 municipalities in the state, which were excluded for security concerns.[35] We traveled more than 25,000 kilometers over four weeks to locate every municipal school, health clinic, and social assistance center in the urban areas of those municipalities. The managers of 926 out of 1,027 units (over 90%) were surveyed, with a median number of five respondents per municipality.[36]

First, I present observational data suggesting that appointed managers have more access to, trust in, and alignment with politicians. I leverage survey questions on the number of meetings street-level managers held, over the previous three months, with the mayor and the secretary in their area, among other stakeholders; and questions on their level of agreement (on a 4-point scale) with statements about the mayor and the secretary. To determine if there are robust correlations between these outcomes and political appointments, I regress respondents' answers on indicators for appointment modes and controls:

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

Y_i is the response given by manager i (namely, the logged number of reported meetings with a given stakeholder +1, or the level of agreement with a given statement). A_i and E_i indicate whether that manager is appointed or elected (leaving civil service as the baseline). $\sum_{k=1}^{K} \gamma^k X_i^k$ are all the demographic, professional, and political covariates I collected.[37] To facilitate comparisons between appointment modes, I exclude from these regressions the 11% of respondents who reported having been appointed through more than one method. For inference, I use HC1 heteroskedasticity-consistent standard errors.

Figure 4 presents the results.[38] Compared to civil service managers, political appointees report, on average, more meetings with the mayor, the secretary, technical staff in the secretariat ("technicians"), and social service users ("clients"). They also report higher levels of trust in the

[34] Rio Grande do Norte is no exception to this regional pattern (Lacerda and de Oliveira, 2004; Carvalho, 2018).

[35] The state is consistently ranked as one of the most violent in Brazil.

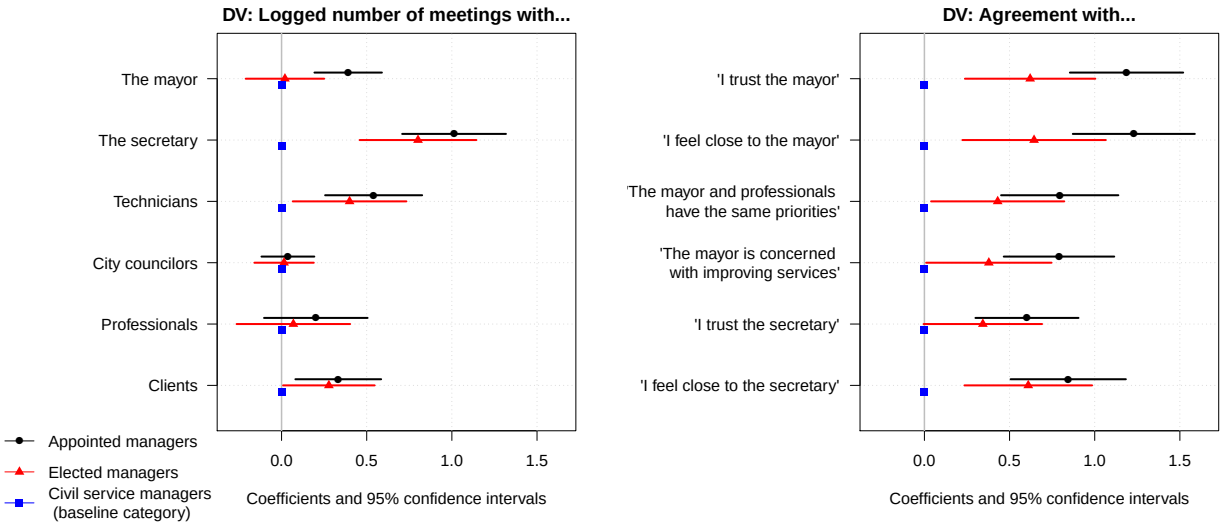
[36] Appendix E.1 reports details on respondent recruitment. Appendix E.2 reports descriptive statistics.

[37] The controls include respondents' sector, age, gender, years of experience as a professional, years of experience as a manager, party membership, union membership, whether they have worked for a local electoral campaign, whether they have more than a college degree, whether they have other jobs, and whether they live in the municipality where they work.

[38] Regression details are in Appendix E.4.

mayor and the secretary, more proximity to them, and stronger beliefs that the mayor cares about improving public services and has the same priorities as bureaucrats. If we restrict the comparison to appointed versus elected managers, appointed ones report more meetings with the mayor and the secretary, and warmer attitudes on all items about them (Appendix E.4). While not causal, these associations are strong and aligned with the theory's predictions.

Figure 4: Association between street-level managers' appointment mode and meetings with and attitudes about politicians.



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

To more directly test the mechanisms of upward embeddedness, I use a conjoint experiment I included 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).

Respondents saw four sets of two hypothetical profiles of managers, with randomly assigned attributes in six dimensions (appointment mode, political connections, education, experience, relationship to professionals in their unit, and whether the unit had met performance targets).[39] To avoid primacy and recency effects, the order of the attributes was randomized across respondents. For each pair, respondents were asked to choose which manager 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 to reform the school /

[39]Details of the attribute values for the conjoint profiles are included in Appendix E.5.

clinic / social assistance center; and (iv) increase the unit's performance in indicators of learning / healthcare / social assistance. These four choice tasks measure the relative impact of different characteristics on perceptions of bureaucrats' ability to perform in key areas of management that my theory predicts upward embeddedness should facilitate.

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

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

Y_{ijk} is the choice expressed by respondent i for profile j in the choice task k (i.e., whether that 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). ε_{ijk} is the error term. I cluster standard errors at the respondent level to account for the dependencies between their choices. $\boldsymbol{\beta}$ 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.

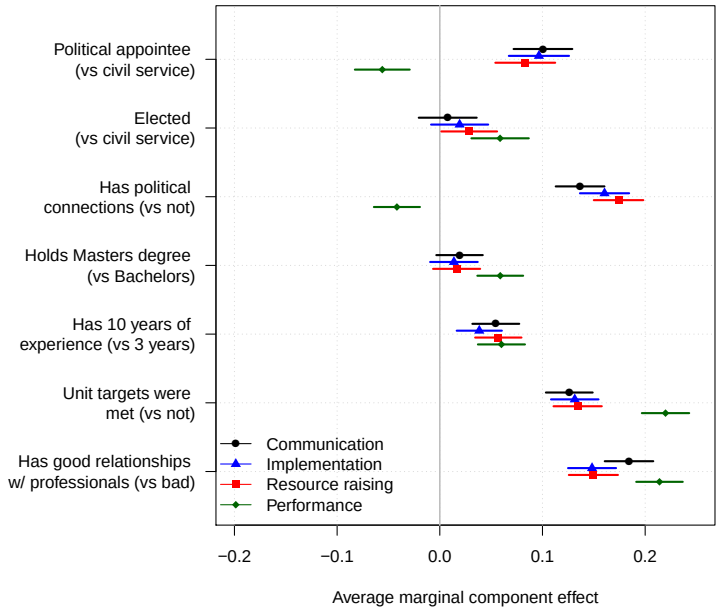
The results of the conjoint experiment, shown in Figure 5,[40] demonstrate that street-level managers see upward embeddedness as facilitating bureaucrats' communication with and responsiveness to the local government, as well as access to resources. Profiles of managers with political connections, or who are political appointees, are seen as significantly more likely than civil service managers to have better communication with the secretariat of their area, to implement changes requested by the local government, or to raise resources for reforming their unit.[41]

However, managers who are politically appointed or have political connections are seen as less likely to improve the performance of the unit. This suggests that patronage may hinder public service delivery. Three factors may explain this result. First, managers may be simply expressing that politically appointed bureaucrats are worse types (e.g., less educated, as shown in Appendix E.3). Second, respondents may be underestimating the indirect channels through which upward embeddedness can aid public service delivery, helping political appointees perform better. Third, the result may be driven by street-level managers who work in the highly clientelistic environments typical of the Northeast, where political appointments are more likely to be leveraged to extract rents. In line with this interpretation, the negative result for performance disappears (without substantively altering other results) when examining the responses of managers who perceive politicians as more

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

[41] The results are similar when examining only responses given by unappointed managers (Appendix E.6).

Figure 5: Results from the conjoint experiment with bureaucrats



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

programmatic (Appendix E.6). In any case, these results draw attention to the potential costs of upward embeddedness, illustrating empirically a trade-off between responsiveness and performance in localities with more clientelistic politicians.

4.3.2 Online survey of politicians

Local politicians also perceive upwardly embedded bureaucrats as more accountable. In partnership with the state audit court of Rio Grande do Norte, I implemented an online survey of local politicians. The survey was sent by the court to the mayor and the secretaries of education, healthcare, social assistance, finance, and administration of all 167 municipalities in the state. A total of 455 politicians completed the survey, for a response rate of 45% (a high value for a survey of elites) and a median number of three responses per municipality.[42]

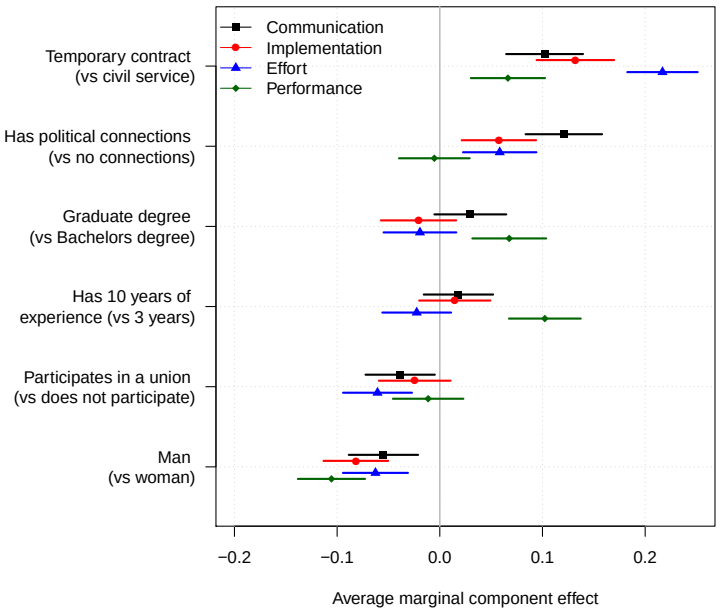
In this conjoint experiment, respondents saw four pairs of hypothetical bureaucrats (without specifying their rank or area of work), with randomly assigned attributes in six dimensions (contract type, political connections, education, experience, union membership, and gender).[43] Contract type

[42] Appendix F.1 has details on recruitment and non-response. Appendix F.2 reports descriptive statistics.
[43] Details of the attribute values for conjoint profiles are included in Appendix F.3.

(temporary versus civil service) was used instead of appointment mode because political appointment and election are not legal selection methods for most bureaucrats. Like political appointments, temporary hires are at will and often based on political connections (Colonnelli et al., 2020).[44] For each pair, respondents were asked to choose which one they believed would be more likely to: (i) maintain better communication with the local government; (ii) implement changes requested by the local government; (iii) work extra hours when necessary; and (iv) achieve better performance.

The results, shown in Figure 6,[45] suggest that politicians view upwardly embedded bureaucrats (i.e., those who were politically appointed or hired under a temporary contract) as more responsive and exerting more effort. They also perceive bureaucrats working under temporary contracts as likely to perform better than those hired under the civil service regime. As a secretary of education explained, "almost all civil service bureaucrats are from other towns. They don't work with the true grit we need. [...] Temporary hires dedicate themselves more."[46]

Figure 6: Results from the conjoint experiment with politicians



See notes under Figure 5.

To summarize, the results of the conjoint experiments with bureaucrats and politicians generally support the key mechanisms of the theory. Both managers and politicians perceive upwardly

[44] 58% of the street-level managers surveyed said political appointments influence "a lot" the hiring of street-level bureaucrats, and only 16% responded "not at all" or "a little."

[45] Regression details are in Appendix F.3.

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

embedded bureaucrats as more likely to communicate well with the local government and to respond to its demands. Managers also perceive upwardly embedded bureaucrats as more likely to raise funds from the government, and politicians believe they are more likely to work extra hours when needed. Together, these results show that actors in the field perceive upward embeddedness as benefiting bureaucratic accountability. Perceptions of the impact of upward embeddedness on effectiveness are more mixed, in line with the high prevalence of clientelism in this region of Brazil (Nichter, 2018; Frey, 2020).

5 Conclusion

Patronage, or the political appointment of bureaucrats, is typically seen as a clientelistic exchange that jeopardizes development by selecting worse types into the bureaucracy and by depressing bureaucratic effort (Pollock, 1937; Xu, 2018; Colonnelli et al., 2020). This paper offers an alternative view of patronage as a system that works by changing not (only) who enters the bureaucracy or how much they work, but *how* they work. I argue that patronage provides bureaucrats with upward embeddedness (political and social ties to politicians), which can make them more accountable and effective. Upward embeddedness works by giving bureaucrats access to material and non-material resources, providing politicians with monitoring technology, facilitating the application of sanctions and rewards, aligning their priorities and incentives, and increasing mutual trust. These governance resources can be leveraged to extract rents, deliver public services, or both. I argue that patronage is most likely to be beneficial for development in the appointment of street-level managers and in 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 paper's main empirical contribution is to provide causally identified evidence of the benefits of patronage for bureaucrats' effectiveness and accountability. It does so by leveraging administrative and survey data on municipal bureaucracies in Brazil, a setting in which multiple appointment systems coexist. To test the core claims of the theory I use two quasi-experiments. Using a diff-in-disc, I show that the quality of schools with appointed directors decreases (relative to those with unappointed directors) if the mayor loses their bid for re-election. Using an RDD, I show that appointed directors (but not unappointed ones) are less likely to be replaced when they meet a highly visible school quality target. To investigate the mechanisms of upward embeddedness, I use original surveys of bureaucrats and politicians. Observational analyses show that appointed bureaucrats have more meetings with local politicians and express higher levels of trust in and alignment with

them. The results of the conjoint experiments show that both street-level managers and politicians perceive upwardly embedded bureaucrats as being better at communicating with the government and responding to its demands. These quantitative findings are grounded in 121 in-depth interviews with bureaucrats, politicians, and anti-corruption agents, which provide rich accounts of how patronage can foster not only rent extraction but also public service delivery.

The findings demonstrate that political appointments and connections provide useful governance resources, that these resources can strengthen bureaucratic effectiveness, and that politicians hold political appointees accountable for their performance in service delivery. These often-overlooked benefits of patronage suggest that politics in the developing world can be a source not only of corruption and misallocations, as is often assumed, but also of governance resources that can help overcome development challenges. A key implication of the paper is that severing the connections between bureaucrats and politicians can hurt public service delivery, at least in the short term and when other sources of bureaucratic effectiveness (e.g., high levels of human capital and strong bureaucratic norms) are not developed.

However, the theory and findings presented here suggest two important weaknesses related to the use of patronage for service delivery, which complements more established views of the costs of patronage. The first weakness is that the same system can be mobilized to *actively* extract rents (e.g., by rigging procurement processes or targeting public services to core supporters). By changing how bureaucrats work –for example, by making them more aligned and more easily monitored and sanctioned– upward embeddedness makes it easier for corrupt politicians to use the bureaucracy to their advantage. This helps explain why patronage is so central to political machines, and why it has proven to be so resilient throughout history (Grindle, 2012). The second weakness is that, even when patronage is used for public service delivery, its benefits are particularly vulnerable to political turnover. While a change in government can make civil service bureaucrats less effective (Toral, 2021), political appointees are especially likely to see their capacity diminished after such a turnover. If political appointees and civil servants rely on different sources of effectiveness (upward embeddedness and a combination of human capital and strong bureaucratic norms, respectively), unaligned political appointees count on none of them. Patronage without connections may therefore be the worst of both worlds.

References

Aberbach, Joel D and Bert A Rockman (2009). The appointments process and the administrative presidency. *Presidential Studies Quarterly 39*(1), 38–59.

Akhtari, Mitra, Diana Moreira, and Laura Trucco (2020). Political Turnover, Bureaucratic Turnover, and the Quality of Public Services. Working paper, available at https://dianamoreira.com (last accessed 6/1/2021).

Ames, Barry (2001). *The Deadlock of Democracy in Brazil*. University of Michigan Press.

Ang, Yuen Yuen (2016). *How China Escaped the Poverty Trap*. Cornell University Press.

Arretche, Marta, Eduardo Marques, and Carlos Aurelio Pimenta de Faria (2020). *As políticas da política: Desigualdades e inclusão nos governos do PSDB e do PT*. Unesp.

Ashraf, Nava, Oriana Bandiera, and B Kelsey Jack (2014). No margin, no mission? A field experiment on incentives for public service delivery. *Journal of Public Economics 120*, 1–17.

Avis, Eric, Claudio Ferraz, and Frederico Finan (2018). Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians. *Journal of Political Economy 126*(5), 1912–1964.

Bach, Tobias and Sylvia Veit (2017). The determinants of promotion to high public office in Germany: Partisan loyalty, political craft, or managerial competencies? *Journal of Public Administration Research and Theory 28*(2), 254–269.

Becker, Sascha O, Peter H Egger, and Maximilian Von Ehrlich (2013). Absorptive capacity and the growth and investment effects of regional transfers: A regression discontinuity design with heterogeneous treatment effects. *American Economic Journal: Economic Policy 5*(4), 29–77.

Bendor, Jonathan, Amihai Glazer, and Thomas Hammond (2001). Theories of Delegation. *Annual Review of Political Science 4*(1), 235–269.

Bersch, Katherine, Sérgio Praça, and Matthew M Taylor (2017). State capacity, bureaucratic politicization, and corruption in the Brazilian state. *Governance 30*(1), 105–124.

Bertrand, Marianne, Robin Burgess, Arunish Chawla, and Guo Xu (2020). The glittering prizes: Career incentives and bureaucrat performance. *The Review of Economic Studies 87*(2), 626–655.

Bhavnani, Rikhil R and Alexander Lee (2018). Local embeddedness and bureaucratic performance: Evidence from India. *The Journal of Politics 80*(1), 71–87.

Biggs, Amanda, Paula Brough, and Jennifer P Barbour (2014). Strategic alignment with organizational priorities and work engagement: A multi-wave analysis. *Journal of Organizational Behavior 35*(3), 301–317.

Bloom, Nicholas, Renata Lemos, Raffaella Sadun, and John Van Reenen (2015). Does management matter in schools? *The Economic Journal 125*(584), 647–674.

Bloom, Nicholas, Raffaella Sadun, and John Van Reenen (2014). Does management matter in healthcare. *Boston, MA: Center for Economic Performance and Harvard Business School*.

Boas, Taylor C, F Daniel Hidalgo, and Marcus André Melo (2019). Norms versus action: Why voters fail to sanction malfeasance in Brazil. *American Journal of Political Science 63*(2), 385–400.

Boas, Taylor C., F. Daniel Hidalgo, and Guillermo Toral (2020). Competence versus Priorities: Negative Electoral Responses to Education Quality in Brazil. *Forthcoming in Journal of Politics*.

Brierley, Sarah (2020). Unprincipled principals: Co-opted bureaucrats and corruption in Ghana. *American Journal of Political Science 64*(2), 209–222.

Brierley, Sarah and Noah Nathan (2021). The Connections of Party Brokers: Which Brokers do Parties Select? *Journal of Politics 83*(3).

Bruns, Barbara, David Evans, and Javier Luque (2012). *Achieving world-class education in Brazil: The next agenda*. World Bank Publications.

Calonico, Sebastian, Matias D Cattaneo, and Max H Farrell (2020). Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs. *The Econometrics Journal 23*(2), 192–210.

Carpenter, Daniel P (2001). *The forging of bureaucratic autonomy: Reputations, networks, and policy innovation in executive agencies, 1862-1928*. Princeton University Press.

Carvalho, Robson Vasconcelos (2018). *Família e política no RN: Alves, Maia e o suporte do Senado*. Clube de Autores.

Cattaneo, Matias D, Nicolás Idrobo, and Rocío Titiunik (2018). A practical introduction to regression discontinuity designs: Extensions. Draft manuscript, available at https://scholar.princeton.edu/titiunik (last accessed 6/1/2021).

Cattaneo, Matias D, Nicolás Idrobo, and Rocío Titiunik (2019). *A Practical Introduction to Regression Discontinuity Designs: Foundations*. Cambridge University Press.

Colonnelli, Emanuele, Mounu Prem, and Edoardo Teso (2020). Patronage and selection in public sector organizations. *American Economic Review 110*(10), 3071–99.

Cruz, Cesi and Philip Keefer (2015). Political parties, clientelism, and bureaucratic reform. *Comparative Political Studies 48*(14), 1942–1973.

Dahlström, Carl and Victor Lapuente (2017). *Organizing Leviathan: Politicians, bureaucrats, and the making of good government*. Cambridge University Press.

Dal Bó, Ernesto, Frederico Finan, and Martín A Rossi (2013). Strengthening state capabilities: The role of financial incentives in the call to public service. *The Quarterly Journal of Economics 128*(3), 1169–1218.

Dasgupta, Aditya and Devesh Kapur (2020). The political economy of bureaucratic overload: Evidence from rural development officials in India. *American Political Science Review 114*(4), 1316–1334.

Depew, Briggs and Ozkan Eren (2016). Born on the wrong day? School entry age and juvenile crime. *Journal of Urban Economics 96*, 73–90.

Evans, Peter B (1995). *Embedded autonomy: States and industrial transformation*. Cambridge University Press.

Fafchamps, Marcel and Julien Labonne (2017). Do politicians' relatives get better jobs? Evidence from municipal elections. *The Journal of Law, Economics, and Organization 33*(2), 268–300.

Fernandes, Reynaldo (2007). Índice de Desenvolvimento da Educação Básica (IDEB): Metas intermediárias para a sua trajetória no Brasil, estados, municípios e escolas. INEP, available at https://download.inep.gov.br/educacao_basica/portal_ideb/o_que_sao_as_metas/Artigo_projecoes.pdf (last accessed 6/1/2021).

Frey, Anderson (2020). Strategic allocation of irrevocable and durable benefits. *American Journal of Political Science*.

Gassner, Drorit and Anat Gofen (2018). Street-level management: A clientele-agent perspective on implementation. *Journal of Public Administration Research and Theory 28*(4), 551–568.

Giraudy, Agustina, Eduardo Moncada, and Richard Snyder (2019). *Inside Countries: Subnational Research in Comparative Politics*. Cambridge University Press.

Gomes, Sandra (2009). Políticas nacionais e implementação subnacional: Uma revisão da descentralização pós-Fundef. *Dados 52*(3), 659–690.

Gottlieb, Jessica and Katrina Kosec (2019). The countervailing effects of competition on public goods provision: When bargaining inefficiencies lead to bad outcomes. *American Political Science Review 113*(1), 88–107.

Granovetter, Mark (1985). Economic action and social structure: The problem of embeddedness. *American Journal of Sociology 91*(3), 481–510.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano (2016). Do fiscal rules matter? *American Economic Journal: Applied Economics*, 1–30.

Grindle, Merilee S (2012). *Jobs for the Boys: Patronage and the State in Comparative Perspective*. Harvard University Press.

Gulzar, Saad and Benjamin J Pasquale (2017). Politicians, bureaucrats, and development: Evidence from India. *American Political Science Review 111*(1), 162–183.

Habyarimana, James, Stuti Khemani, and Thiago Scot (2018). Political selection and bureaucratic productivity. World Bank Working Paper, available at https://openknowledge.worldbank.org/handle/10986/31074 (last accessed 6/1/2021).

Hagopian, Frances (1996). *Traditional Politics and Regime Change in Brazil*. Cambridge University Press.

Hainmueller, Jens, Daniel J Hopkins, and Teppei Yamamoto (2014). Causal inference in conjoint analysis: Understanding multidimensional choices via stated preference experiments. *Political Analysis 22*(1), 1–30.

Hasnain, Zahid, Nick Manning, and Jan Henryk Pierskalla (2014). The promise of performance pay? reasons for caution in policy prescriptions in the core civil service. *The World Bank Research Observer*, lku001.

Hassan, Mai (2020). *Regime Threats and State Solutions: Bureaucratic Loyalty and Embeddedness in Kenya*. Cambridge University Press.

Healy, Andrew and Neil Malhotra (2013). Retrospective voting reconsidered. *Annual Review of Political Science 16*, 285–306.

Heckman, James J (1979). Sample selection bias as a specification error. *Econometrica: Journal of the econometric society*, 153–161.

Hjort, Jonas, Diana Moreira, Gautam Rao, and Juan Francisco Santini (2021). How Research Affects Policy: Experimental Evidence from 2,150 Brazilian Municipalities. *American Economic Review 11*(5), 1442–80.

Huntington, Samuel P (1968). *Political order in changing societies*. Yale University Press.

Imbens, Guido W and Charles F Manski (2004). Confidence intervals for partially identified parameters. *Econometrica 72*(6), 1845–1857.

Ismailescu, Iuliana and Hossein Kazemi (2010). The reaction of emerging market credit default swap spreads to sovereign credit rating changes. *Journal of Banking & Finance 34*(12), 2861–2873.

Iyer, Lakshmi and Anandi Mani (2012). Traveling agents: Political change and bureaucratic turnover in India. *Review of Economics and Statistics 94*(3), 723–739.

Jiang, Junyan (2018). Making bureaucracy work: Patronage networks, performance incentives, and economic development in China. *American Journal of Political Science 62*(4), 982–999.

Jiang, Junyan, Zijie Shao, and Zhiyuan Zhang (2020). The Price of Probity: Anticorruption and Adverse Selection in the Chinese Bureaucracy. *British Journal of Political Science*, 1–24.

Khan, Adnan Q, Asim Ijaz Khwaja, and Benjamin A Olken (2019). Making moves matter: Experimental evidence on incentivizing bureaucrats through performance-based postings. *American Economic Review 109*(1), 237–70.

Klašnja, Marko and Rocio Titiunik (2017). The incumbency curse: Weak parties, term limits, and unfulfilled accountability. *American Political Science Review 111*(1), 129–148.

Kopecky, Petr, Peter Mair, and Maria Spirova (2012). *Party patronage and party government in European democracies*. Oxford University Press.

Kramer, Roderick M (1999). Trust and distrust in organizations: Emerging perspectives, enduring questions. *Annual Review of Psychology 50*(1), 569–598.

Lacerda, Alan and Bruno Cesário de Oliveira (2004). Patronagem e formação de coalizões: o caso da unidade popular no rn. *Cronos 5/6*(1/2), 273–287.

Leal, Victor Nunes (1948). *Coronelismo, enxada e voto: O município e o regime representativo no Brasil*. Companhia das Letras.

Lee, David S (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies 76*(3), 1071–1102.

Lee, David S and David Card (2008). Regression discontinuity inference with specification error. *Journal of Econometrics 142*(2), 655–674.

Lee, David S and Thomas Lemieux (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature 48*, 281–355.

Lewis, David E (2008). *The Politics of Presidential Appointments: Political Control and Bureaucratic Performance*. Princeton University Press.

Lipsky, Michael (1980). *Street-Level Bureaucracy: Dilemmas of the Individual in Public Services*. Russel Sage Foundation.

Litschig, Stephan and Yves Zamboni (2019). Judicial presence and rent extraction. Working paper, available at https://ideas.repec.org/p/ngi/dpaper/19-20.html (last accessed 6/1/2021).

McCrary, Justin (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics 142*(2), 698–714.

McDonnell, Erin Metz (2020). *Patchwork Leviathan: Pockets of Bureaucratic Effectiveness in Developing States*. Princeton University Press.

Moe, Terry M (1985). The politicized presidency. In John E Chubb and Paul E Peterson (Eds.), *The new direction in American politics*, pp. 235–271. Washington DC: Brookings Institution.

Nichter, Simeon (2018). *Votes for Survival: Relational Clientelism in Latin America*. Cambridge University Press.

Niedzwiecki, Sara (2018). *Uneven Social Policies: The Politics of Subnational Variation in Latin America*. Cambridge University Press.

O'Donnell, Guillermo A (1998). Horizontal accountability in new democracies. *Journal of democracy 9*(3), 112–126.

O'Dwyer, Conor (2006). *Runaway State-Building: Patronage Politics and Democratic Development*. Johns Hopkins University Press.

Oliveros, Virginia (2021). Working for the Machine: Patronage jobs and political services in Argentina. *Comparative Politics 53*(3), 381–402.

Oliveros, Virginia and Christian Schuster (2018). Merit, tenure, and bureaucratic behavior: Evidence from a conjoint experiment in the dominican republic. *Comparative Political Studies 51*(6), 759–792.

Peters, B Guy and Jon Pierre (2004). *The Politicization of the Civil Service in Comparative Perspective: A Quest for Control*. Routledge.

Pollock, James Kerr (1937). The cost of the patronage system. *The Annals of the American Academy of Political and Social Science 189*(1), 29–34.

Pritchett, Lant and Michael Woolcock (2004). Solutions when the solution is the problem: Arraying the disarray in development. *World Development 32*(2), 191–212.

Raffler, Pia (2020). Does Political Oversight of the Bureaucracy Increase Accountability? Field Experimental Evidence from an Electoral Autocracy. Working paper, available at http://piaraffler.com (last accessed 6/1/2021).

Rasul, Imran and Daniel Rogger (2018). Management of bureaucrats and public service delivery: Evidence from the Nigerian civil service. *The Economic Journal 128*(608), 413–446.

Robinson, James A and Thierry Verdier (2013). The political economy of clientelism. *The Scandinavian Journal of Economics 115*(2), 260–291.

Samuels, David J and Cesar Zucco (2018). *Partisans, Antipartisans, and Nonpartisans: Voting Behavior in Brazil*. Cambridge University Press.

Schneider, Ben Ross (1991). *Politics within the State: Elite Bureaucrats and Industrial Policy in Authoritarian Brazil*. University of Pittsburgh Press.

Scott, James C (1969). Corruption, machine politics, and political change. *American Political Science Review 63*(4), 1142–1158.

Sorauf, Frank J (1960). The silent revolution in patronage. *Public Administration Review*, 28–34.

Tavares, Priscilla Albuquerque (2015). The impact of school management practices on educational performance: Evidence from public schools in São Paulo. *Economics of Education Review 48*, 1–15.

Toral, Guillermo (2021). Turnover: How electoral accountability disrupts the bureaucracy and service delivery. Working paper, available at https://www.guillermotoral.com (last accessed 6/1/2021).

Tsai, Lily L (2007). *Accountability without democracy: Solidary Groups and Public Goods Provision in Rural China*. Cambridge University Press.

Tsai, Lily L and Yiqing Xu (2018). Outspoken insiders: Political connections and citizen participation in authoritarian China. *Political Behavior 40*(3), 629–657.

Wang, Erik (2021). Frightened Mandarins: The Adverse Effects of Fighting Corruption on Local Bureaucracy. Working Paper, available at https://erikhw.github.io/ (last accessed 6/1/2021).

Weingrod, Alex (1968). Patrons, patronage, and political parties. *Comparative Studies in Society and History 10*(4), 377–400.

Williams, Martin J (2017). The political economy of unfinished development projects: Corruption, clientelism, or collective choice? *American Political Science Review 111*(4), 705–723.

Wilson, James Q (1961). The economy of patronage. *Journal of Political Economy 69*(4), 369–380.

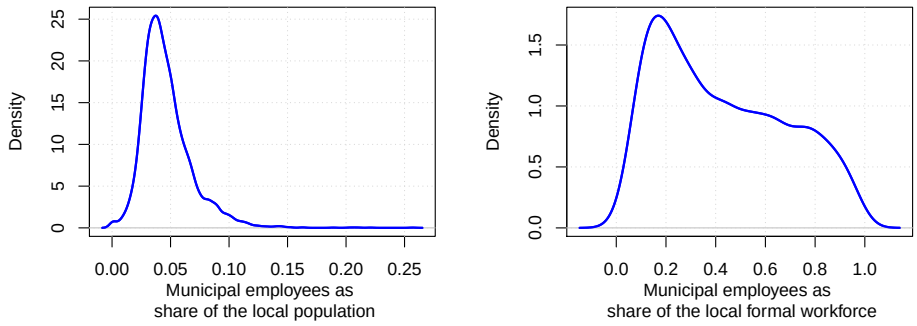
Xu, Guo (2018). The Costs of Patronage: Evidence from the British Empire. *American Economic Review 108*(11), 3170–98.

Appendices

A Additional details of the institutional context

A.1 Size of the municipal government workforce

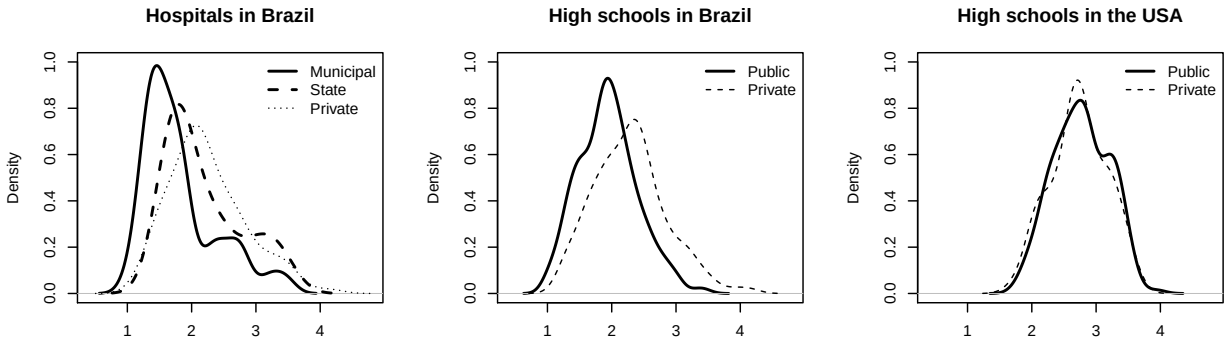
Figure A.1: Size of the municipal government workforce as a proportion of the total local population and the total formal labor market workforce



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

A.2 Management practices in Brazilian schools and hospitals

Figure A.2: Scores of the World Management Survey for hospitals and high schools in Brazil, and for high schools in the USA



Data are from Bloom et al. (2014, 2015) and correspond to 289 hospitals and 513 high schools that were randomly selected in Brazil, as well as 270 high schools in the USA for comparison. 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

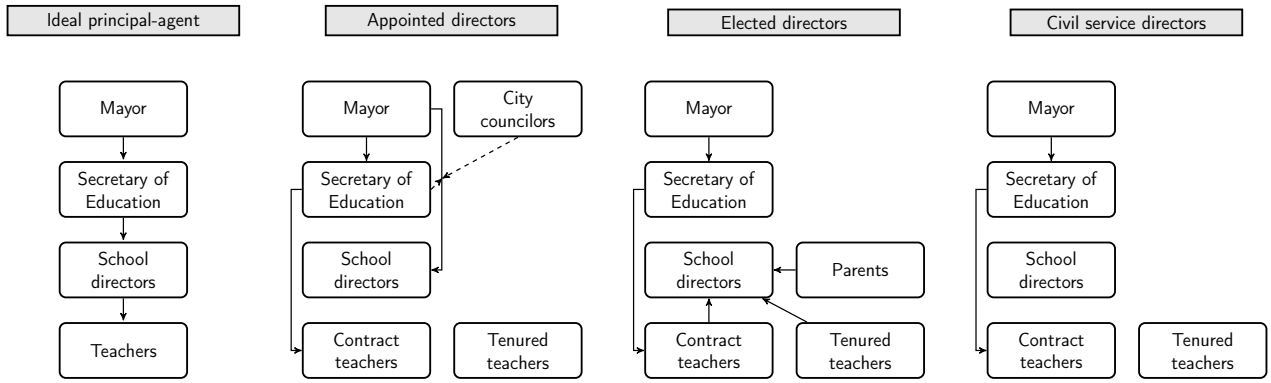
Table A.1: Observational predictors of municipal schools' director appointment mode and quality score (IDEB), from cross-section data on municipalities, schools, and directors (2013)

	Appointed			Elected	Civil service	IDEB score				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intercept	0.965***	2.587***	2.418***	−1.075***	−0.278***	3.735***	−1.991***	−2.141***	−1.451***	4.739***
	(0.018)	(0.038)	(0.043)	(0.039)	(0.022)	(0.068)	(0.138)	(0.121)	(0.114)	(0.009)
Director covariates										
Female	0.004	0.022***	0.033***	−0.021**	−0.015***	0.279***	0.169***	0.015	0.006	
	(0.007)	(0.007)	(0.007)	(0.006)	(0.004)	(0.026)	(0.023)	(0.020)	(0.018)	
Age 40-49 (vs <40)	−0.066***	−0.021***	−0.018**	0.020***	−0.007*	0.073***	0.021	−0.021	−0.012	
	(0.007)	(0.006)	(0.006)	(0.006)	(0.003)	(0.020)	(0.018)	(0.015)	(0.014)	
Age 50+ (vs <40)	−0.097***	−0.016*	−0.012	0.010	−0.005	0.073**	0.006	−0.057**	−0.052**	
	(0.008)	(0.008)	(0.008)	(0.008)	(0.004)	(0.026)	(0.024)	(0.020)	(0.018)	
White (vs other race)	−0.032*	0.019	0.038**	−0.054***	0.023***	0.700***	0.397***	0.113**	0.002	
	(0.014)	(0.013)	(0.014)	(0.013)	(0.007)	(0.047)	(0.042)	(0.035)	(0.032)	
Black/brown (vs other race)	0.070***	0.059***	0.055***	−0.023	−0.019**	−0.096*	−0.049	−0.030	−0.050	
	(0.014)	(0.013)	(0.013)	(0.013)	(0.006)	(0.046)	(0.042)	(0.035)	(0.032)	
Tertiary degree (vs < tertiary)	−0.090***	−0.012	−0.006	0.026**	−0.013**	0.448***	0.291***	0.165***	0.102***	
	(0.011)	(0.010)	(0.011)	(0.009)	(0.005)	(0.036)	(0.033)	(0.030)	(0.028)	
Postgraduate degree (vs < tertiary)	−0.086***	−0.061***	−0.054***	0.038***	0.005	0.299***	0.212***	0.131***	0.118***	
	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.020)	(0.018)	(0.016)	(0.014)	
No other jobs	−0.038***	−0.027***	−0.023***	0.033***	−0.015***	0.213***	0.167***	0.139***	0.097***	
	(0.006)	(0.005)	(0.006)	(0.005)	(0.003)	(0.019)	(0.017)	(0.014)	(0.013)	
6-15 years of teaching exp. (vs <6)	−0.050***	−0.027***	−0.024***	0.026***	0.011**	0.090***	0.040	−0.004	−0.024	
	(0.007)	(0.007)	(0.007)	(0.006)	(0.004)	(0.023)	(0.021)	(0.018)	(0.017)	
>15 years of teaching exp. (vs <6)	−0.032***	−0.015	−0.010	0.030***	−0.005	0.186***	0.080***	0.004	−0.024	
	(0.009)	(0.008)	(0.008)	(0.007)	(0.004)	(0.027)	(0.024)	(0.021)	(0.019)	
3-10 years of director exp. (vs <3)	−0.019*	0.033***	0.033***	−0.082***	0.031***	0.092***	0.072**	0.021	0.014	
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.027)	(0.024)	(0.020)	(0.019)	
>10 years of director exp. (vs <3)	−0.007	0.078***	0.080***	−0.197***	0.095***	0.194***	0.124***	0.022	0.005	
	(0.012)	(0.012)	(0.012)	(0.010)	(0.008)	(0.037)	(0.034)	(0.028)	(0.026)	
3-10 years as director of school (vs <3)	−0.182***	−0.180***	−0.180***	0.222***	−0.010	−0.023	0.003	0.062**	0.053**	
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.026)	(0.024)	(0.020)	(0.019)	
>10 years as director of school (vs <3)	−0.265***	−0.248***	−0.245***	0.275***	0.002	0.029	0.057	0.151***	0.115***	
	(0.015)	(0.014)	(0.014)	(0.013)	(0.011)	(0.043)	(0.040)	(0.034)	(0.030)	
Municipality covariates										
GDP per capita (logged)		−0.088***	−0.043***	0.020***	0.020***		0.665***	0.089***	0.038***	
		(0.004)	(0.005)	(0.004)	(0.002)		(0.014)	(0.013)	(0.011)	
Population (logged)		−0.082***	−0.087***	0.068***	0.006***		−0.104***	−0.097***	−0.073***	
		(0.002)	(0.002)	(0.002)	(0.001)		(0.005)	(0.005)	(0.004)	
Number of deaths per 1,000		−0.017***	−0.009***	0.012***	−0.002**		0.108***	0.020***	0.007	
		(0.002)	(0.002)	(0.002)	(0.001)		(0.005)	(0.005)	(0.004)	
Mayor is in first term		−0.000	0.006	−0.022***	0.004		0.071***	0.034*	0.004	
		(0.006)	(0.006)	(0.006)	(0.003)		(0.017)	(0.015)	(0.014)	
Electoral concentration		0.062**	0.044*	0.017	−0.071***		0.282***	0.452***	0.275***	
		(0.021)	(0.021)	(0.019)	(0.011)		(0.060)	(0.052)	(0.049)	
School covariates										
Rural			0.028***	−0.025***	0.003			−0.014	−0.041*	
			(0.007)	(0.006)	(0.003)			(0.021)	(0.019)	
Number of staff (logged)			0.005	0.015**	−0.016***			0.009	0.005	
			(0.005)	(0.005)	(0.003)			(0.014)	(0.012)	
Students per classroom (average)			0.006***	−0.006***	0.001***			−0.009***	−0.008***	
			(0.001)	(0.001)	(0.000)			(0.002)	(0.002)	
School socioeconomic index			−0.009***	0.007***	0.003***			0.141***	0.078***	
			(0.001)	(0.001)	(0.000)			(0.002)	(0.002)	
Director is appointed (vs civil service)						−0.631***	−0.377***	−0.174***	0.000	
						(0.030)	(0.028)	(0.023)	(0.022)	
Director is elected (vs civil service)						−0.333***	−0.234***	−0.155***	0.011	
						(0.031)	(0.028)	(0.024)	(0.022)	
IDEB target									0.585***	
									(0.010)	
Municipality fixed effects	No	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.099	0.252	0.259	0.186	0.071	0.243	0.382	0.570	0.659	0.000
Num. obs.	30748	30038	29273	29273	29273	17404	17402	16814	15622	19118

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

A.4 Accountability relationships by director appointment mode

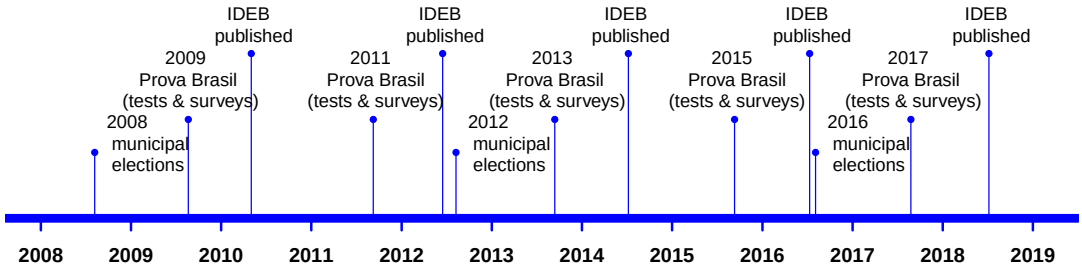
Figure A.3: Four models of appointments in Brazil's municipal basic education sector



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

A.5 Timeline of student testing and information release

Figure A.4: Timeline of IDEB tests and information release



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

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

- *7 ações para aproveitar bem a Prova Brasil* (7 actions to take advantage of Prova Brasil), produced by Gestão Escolar (School Management), the director-geared section of Nova Escola, which is a leading education magazine in Brazil. Published in September, 2 months before the implementation of the tests. Available here.
- *Como preparar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), also produced by Gestão Escolar. Published on August, 3 months before the implementation of the test. Available here.

- *Dicas para preparar sua escola para a Prova Brasil* (Tips to prepare your school for Prova Brasil), produced by Educador360, another education site, in a section called Pedagogic management. Published in early November, at the beginning of the period when the test was implemented. Available here.
- *Escolas da SEMED reforçam atividades de preparação para Prova Brasil* (Municipal Education Secretariat schools reinforce preparation activities for Prova Brasil), produced by the municipality of Manaus (in the Amazon), describing a number of overlapping strategies the municipality implemented to prepare for Prova Brasil. Published in late September, weeks before the test. Available here.
- *Como preprarar a escola para a Prova Brasil* (How to prepare the school for Prova Brasil), slides for how to prepare for the test, dated in October just one month before the test. Published on the site of the secretariat of education of the state of Goiás. Available here.

B Additional details of in-depth interviews

In-depth interviews with local actors gave origin to the hypotheses tested in this article, but were part of a larger empirical study of patronage in Brazil. Over 18 months of fieldwork in the period 2016-2019 I conduced 121 in-depth, semi-structured interviews with municipal bureaucrats and politicians, and with state-level horizontal accountability actors (e.g., prosecutors). I recruited interviewees at their offices, and collected their oral consent after providing information about the research project and their rights as participants. I conducted interviews in Portuguese, face-to-face, and at the interviewee's office. I chose not to record interviews because some of the topics discussed were highly sensitive, including corrupt and illegal uses of public employment. While recording interviews would have allowed for more complete transcripts, it would have seriously hindered the reliability of the data and subjects' willingness to participate. Some subjects agreed to participate on the condition of anonymity or confidentiality. When quoting interviewees, I specify only their position, the state, and the month of the interview in order to safeguard their identity. In total, I interviewed 51 municipal politicians, 54 municipal bureaucrats, and 16 horizontal accountability actors.[47] Interviews were done in 45 municipalities in 7 states across 3 different regions of Brazil.[48] Locations were chosen to ensure diversity in political and socioeconomic variables.

Within each municipality, fieldwork focused on the center, where government offices are. I approached potential interviewees at their offices and requested an interview after introducing myself and the research project. No compensation of any sort was offered or given to participants. Most subjects that I managed to speak to directly agreed to participate.[49] Interviews were semi-

[47] 41 of of the 51 politicians were secretaries. 46 of the 54 bureaucrats were school directors, clinic managers, and social assistance center coordinators. Of the 16 horizontal accountability actors, 8 were state prosecutors or prosecutorial staff.

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

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

structured, and usually started as an open conversation about the interviewee's background, the challenges they faced in their position, and their perception of public services in the municipality. As the conversation advanced, I followed up with questions about the local dynamics of public employment, including in some cases specific questions about the connection between political turnover, bureaucratic turnover, and public service delivery. I took handwritten notes during and after the interviews. The median duration of interviews was one hour.

Interview locations are quite diverse in their social, economic, and political characteristics. Descriptive statistics for interview locations are available from the author.

C Additional details of the difference-in-discontinuities

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

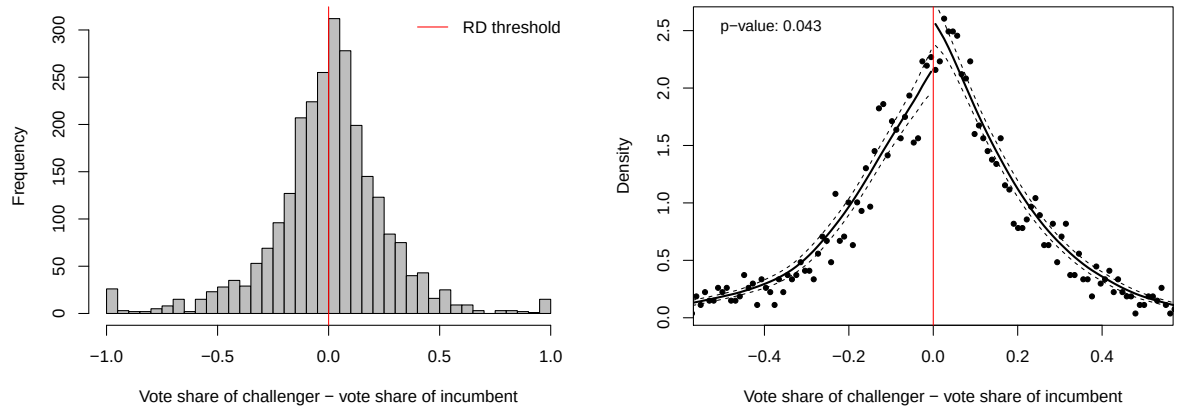
Table A.2: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 5 with pre-treatment covariates as the dependent variable

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

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

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

Figure A.5: Continuity of the forcing variable: Histogram and McCrary (2008) density test

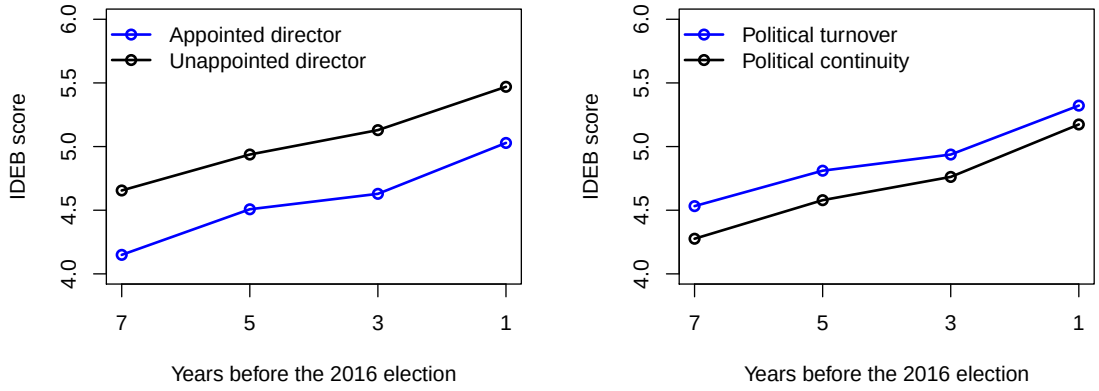


identified by the density test in Figure A.5. This may be due to Brazilian mayors' incumbency disadvantage (Klašnja and Titiunik, 2017). In any case, the key is that actors (in this case, mayors and their challengers) do not have precise manipulation of the forcing variable (Lee and Lemieux, 2010). An additional observable implication of the lack of precise manipulation assumption is that there should be no discontinuous jumps in covariates around the threshold, as shown in Table A.2.

C.2 Pre-election trends

Figure A.6 shows how the average IDEB score of schools that enter the diff-in-disc evolve from 2009 to 2015, by whether their director is appointed and whether the mayor loses the 2016 election.

Figure A.6: Pre-treatment trends in school quality scores, by director appointment mode and by political turnover



C.3 Alternative bandwidths

This Appendix shows how the main result of the diff-in-disc ($\hat{\gamma}_2$ in Equation 5) changes when varying the RD bandwidth, for the results in Tables 2 and 3. If treatment effects are sometimes insignificant with bandwidths smaller than the optimal (likely due to reduced power from smaller samples), coefficients remain large and negative.

Figure A.7: Robustness of results in model 4 in Table 2 to alternative bandwidths

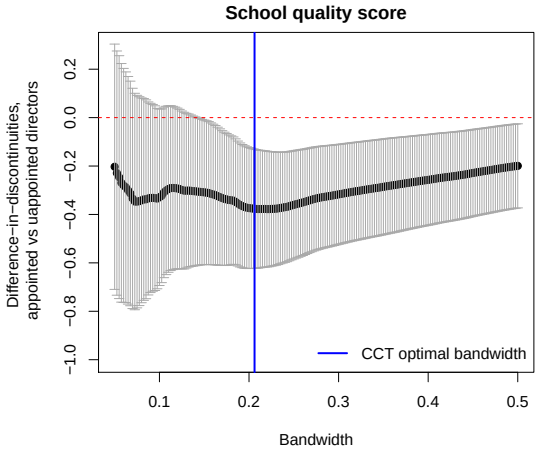
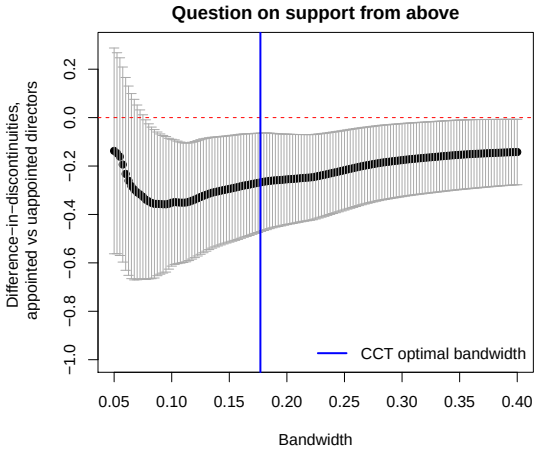


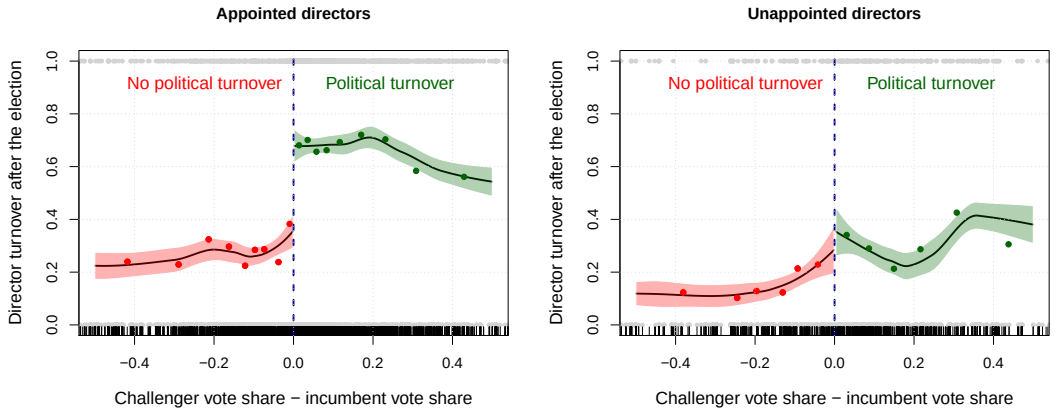
Figure A.8: Robustness of results in model 4 in Table 3 to alternative bandwidths



C.4 RD estimates of the effect of political turnover on director turnover

This appendix shows that an electoral defeat of the incumbent has a significant effect on the replacement of school directors within one year after the election (model 1 in Table A.3). This effect is mostly driven by the replacement of appointed directors (models 2-4).

Figure A.9: Effect of political turnover on director turnover, by director appointment mode



See notes under Figure 2.

Table A.3: Regression discontinuity estimates of the effect of political turnover on director turnover, by director appointment mode

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

See notes under Table 2.

C.5 Predictors of director turnover after mayor turnover

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

Table A.4: Observational predictors of school directors being replaced after political turnover.

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

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

C.6 Alternative estimation: Matching similar schools with and without political turnover

To partially address the sample selection bias, here I match schools in the group without mayor turnover to similar schools in the group with mayor turnover. I do this with exact matching on the covariates that significantly predict director turnover after mayor turnover, as per Appendix C.5. The relevant coefficient is larger than in the main specification in Table 2 and, despite the significantly smaller sample size, remains statistically significant.

Table A.5: Difference-in-discontinuity estimates of the differential impact of political turnover on school quality for appointed versus unappointed directors, after matching

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

See notes under Table 2.

C.7 Bounds to account for sample selection bias

C.7.1 The Lee (2009) bounds for sample selection bias

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

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

C.7.2 Adaptation of the Lee (2009) bounding procedure to the diff-in-disc setting

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

- I first simplify the design to a localized experiment, immediately around the threshold, and therefore based on an assumption of local randomization instead of one of continuity (Cattaneo et al., 2018).[50] I focus exclusively on schools immediately around the cutoff – I use the 0.015 bandwidth but results are similar using 0.01 or 0.02. Within this small bandwidth the density of the the forcing variable is flat, as required when invoking the local randomization assumption.[51]
- Then I build four trimmed datasets: two trimmed datasets for upper and lower bound for appointed directors, and two trimmed datasets for unappointed directors. This is because

[50] Depew and Eren (2016) and others have also used this strategy for adapting Lee (2009) bounds to a regression discontinuity setting.

[51] Replicating the diff-in-disc as a localized diff-in-diff within this narrow bandwidth renders similar results to those presented in Section 4.1. Results are available from the author.

the rates of director turnover (S) are very different for both types of directors, as shown in Appendix C.4.

- To estimate the lower bound, I join the data for the group with no mayor turnover to the two trimmed datasets for lower bounds (one for appointed directors and one for not appointed directors). Then I regress the change in IDEB scores on an indicator for mayor turnover and its interaction with an indicator of the director being politically appointed.

$$Y_{sm} = \alpha + \beta_1 P_m + A_{sm}(\gamma_1 + \gamma_2 P_m) + \varepsilon_{sm} \tag{13}$$

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

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

C.7.3 Inference

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

- I first draw, with replacement, a sample of appointed directors (with or without attrition) within the narrow bandwidth.[52] With that data, I calculate p_0^a.
- I then draw a sample with replacement from the set of schools that did not experience director turnover, within the narrow bandwidth, and that had appointed directors. I trim the set of schools without mayor turnover according to the $\hat{p}_0^a$ estimated before, applying the $\hat{p}_0^a$ and $1 - \hat{p}_0^a$ quantiles to the distribution of $Y|D = 0, S = 1$ within this sample. With that data, I build a trimmed sample of appointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
- I replicate steps 1-3 for unappointed directors, estimating $p_0^{\neg a}$ and creating a trimmed sample of unappointed directors for a lower bound, and a trimmed sample of appointed directors for an upper bound.
- I merge the datasets for appointed and unappointed directors, creating datasets for a lower and an upper bound.

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

- I estimate Equation 13 with each of the two datasets to estimate the difference in the treatment effect for appointed and unappointed directors. I store the two values of $\hat{\gamma_2}$ from each of the two regressions into corresponding vectors.

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

$$\left[\hat{\Delta}^{LB} - \bar{C}_n \times \frac{\hat{\sigma}_{LB}}{\sqrt{n}}, \hat{\Delta}^{UB} + \bar{C}_n \times \frac{\hat{\sigma}_{UB}}{\sqrt{n}}\right] \tag{14}$$

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

$$\Phi\left(\bar{C}_n + \sqrt{n}\frac{\hat{\Delta}^{LB} - \hat{\Delta}^{UB}}{max(\hat{\sigma}_{UB}, \hat{\sigma}_{LB})}\right) - \Phi(-\bar{C}_n) = 1 - \alpha \tag{15}$$

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

C.8 Mechanisms, as measured through the government's survey of school directors

To explore mechanisms I replicate the diff-in-disc using, as the dependent variable for Equation 5, changes in director responses in the ANRESC official survey. In particular, I leverage items in a module called *Views about school problems and obstacles to management.* First, I leverage their answers on a question asking them whether their work is supported by higher instances. That's a question directly aligned with my theory of upward embeddedness, and one where I find significant effects (Table 3). Next, I leverage director answers on questions asking them about the extent to which the school's functioning was hindered by a number of problems, including insufficient financial resources, insufficient teachers, or teacher turnover. These questions, which address potential alternative mechanisms through which political turnover could hurt the effectiveness of appointed directors, are measured on a 4-point scale (from "no" to "yes, a lot"). The diff-in-disc returns statistically insignificant results for all of them, as shown below.

Table A.6: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with teacher turnover

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

See notes under Table 2.

Table A.7: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient teachers

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

See notes under Table 2.

Table A.8: Difference-in-discontinuity estimates of the differential impact of political turnover on directors reporting problems with insufficient financial resources

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

See notes under Table 2.

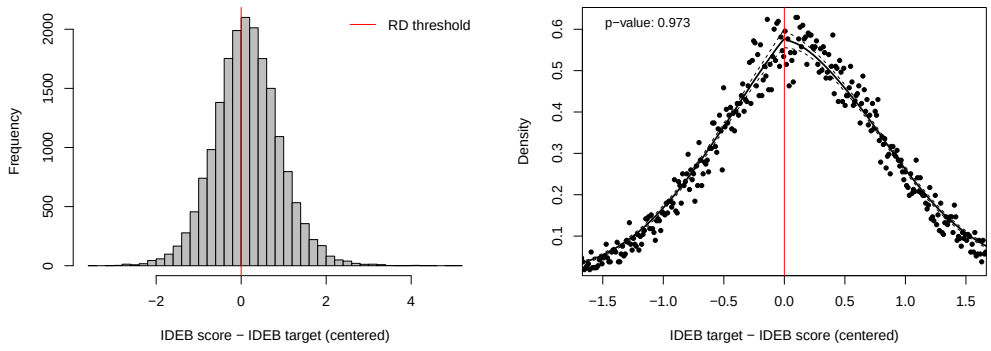
D Additional details of the regression discontinuity

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

Table A.9: Continuity in pre-treatment covariates at the director, municipality, and school level, estimated by applying Equation 8 with pre-treatment covariates as the dependent variable

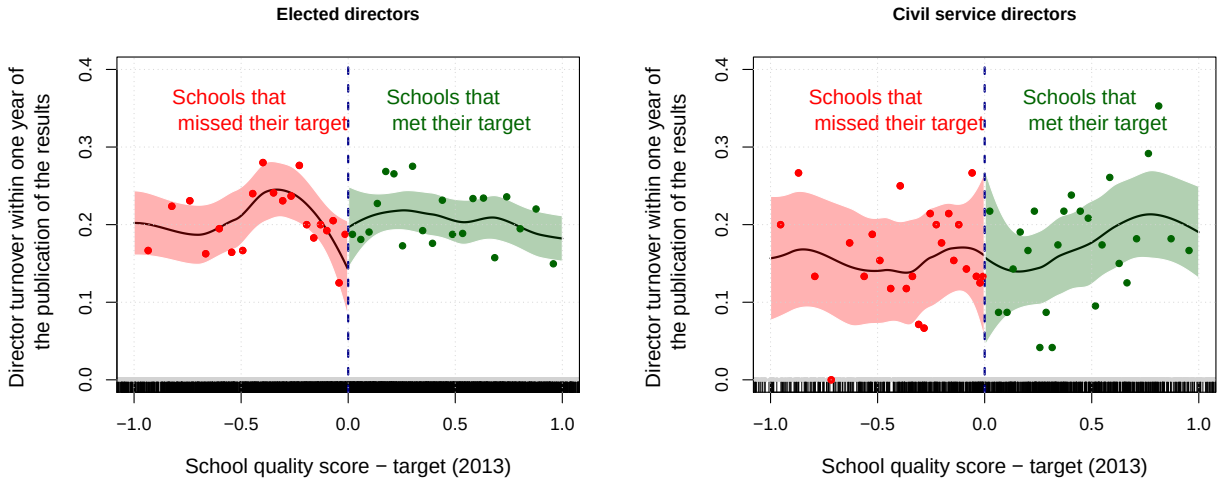
	RD estimate	Standard error	p value
Director is appointed	0.019	0.021	0.369
Director is elected	-0.001	0.019	0.956
Director is civil service	-0.027	0.012	0.023
Director is female	0.008	0.013	0.556
Director is aged <40	-0.002	0.018	0.913
Director is aged 40-49	0.029	0.019	0.130
Director is aged 50+	-0.026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

Figure A.10: Continuity of the forcing variable



D.2 Treatment heterogeneity among unappointed directors

Figure A.11: Effect of meeting the performance target on director performance, for schools whose director was unappointed



See notes under Figure 2.

Table A.10: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was elected

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

See notes under Table 4.

Table A.11: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was civil service

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

See notes under Table 4.

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

The results of the RDD presented in 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.[53]

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

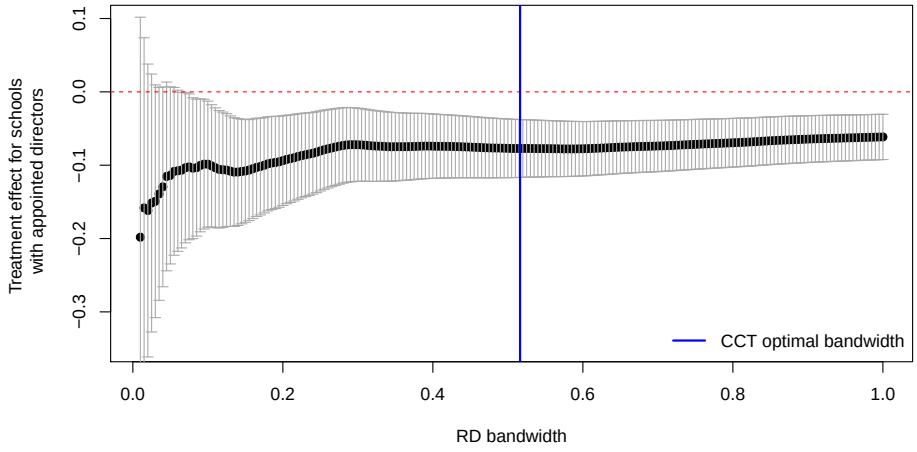
[53]The uncompetitive nature of director elections is not unique to this state. In the municipal school director elections held in 2015 in Vitória da Conquista (the third largest city in the state of Bahia), the average vote share of the winner for schools were valid elections were held was 95.96%. Over 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%.

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."[54] In practice, the results of the election are usually determined by teachers, especially tenured ones. These dynamics of capture are strengthened by the erosion of the democratic norm once elections are established – interviewees often reported a significant drop in community interest and participation in director elections after the first wave. In the words of a secretary, "first there was a democratic response – the first election was genuine, with interest, but the second one had just the very same candidates, and after that it just became a mere [formal] commitment, with the same people. After four years when candidates reached their re-election limit no one ran and the mayor had to appoint somebody."[55]

D.4 Alternative bandwidths

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

Figure A.12: Sensitivity of model 4 in Table 4 to alternative bandwidths

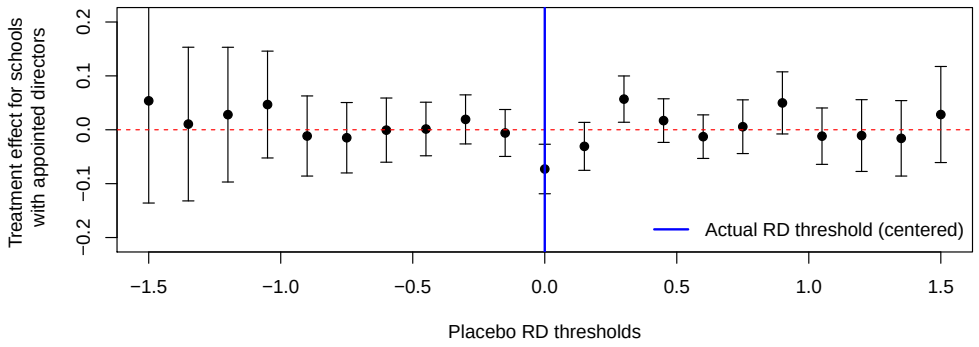


D.5 Placebo tests varying the RD threshold

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

[54] School director interviewed in the state of Rio de Janeiro in February 2017.
[55] Secretary of education interviewed in the state of Rio de Janeiro in February 2017.

Figure A.13: Placebo tests for model 2 in Table 4, moving the RD threshold



D.6 Alternative sample: Municipalities with mayors from programmatic parties

This appendix replicates the diff-in-disc subsetting to municipalities where the mayor belongs to a large, programmatic party (PT or PSDB). Programmatic parties are those having identifiable platforms, and are generally thought as less likely to rely on clientelism and more likely to strengthen bureaucratic accountability (Cruz and Keefer, 2015). Until recently Brazil had two large programmatic parties, PT and PSDB, although their programmatic profile has eroded over time (Samuels and Zucco, 2018).

Table A.12: Regression discontinuity estimates of the effect of reaching the school quality target on director turnover, by whether the director was appointed, among municipalities with a programmatic party in office

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

See notes under Table 4.

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

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

E.1 Details on sampling and non-response

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

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

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

E.2 Descriptive statistics

Table A.13: Descriptive statistics of the survey of street-level managers, by sector

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

E.3 Results of observational regressions of appointment modes

Table A.14: Correlates of street-level managers' appointment mode

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

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

E.4 Results of observational regressions of meetings and attitudes

Table A.15: 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
height	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.391***	1.013***	0.541***	0.037	0.201	0.333**
	(0.101)	(0.155)	(0.145)	(0.079)	(0.155)	(0.128)
Elected	0.020	0.801***	0.399*	0.015	0.069	0.278*
	(0.117)	(0.175)	(0.170)	(0.089)	(0.170)	(0.137)
Observations	786	775	786	786	786	786
R-squared	0.096	0.115	0.090	0.025	0.063	0.167

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

Table A.16: Observational regressions of self-reported number of meetings with stakeholders on street-level managers' appointment mode (versus elected), as per Equation 10

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

See notes under Table A.15.

Table A.17: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

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

See notes under Table A.15.

Table A.18: Observational regressions of attitudes about the mayor and the secretary on street-level managers' appointment mode (baseline category is civil service), as per Equation 10

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

See notes under Table A.15.

E.5 Details of the conjoint experiment with bureaucrats

Table A.19: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment

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

The next table details the regression results visualized in Figure 5. These correspond to the following choice tasks of the conjoint experiment: (i) *Communication:* Which of these [directors/managers/coordinators] do you think would have a better communication with the Secretariat of [education/healthcare/social assistance]?; (ii) *Implementation:* Which of these [directors/managers/coordinators] do you think would have more chances of implementing changes requested by the mayor's office?; (iii) *Resources:* Which of these [directors/managers/coordinators] do you think would obtain a reform for the [school/clinic/social assistance center]?; (iv) *Results:* Which of these [directors/managers/coordinators] do you think would achieve better scores in [student learning/community healthcare/social assistance center indicators]?

E.6 Conjoint results among subsets of respondents

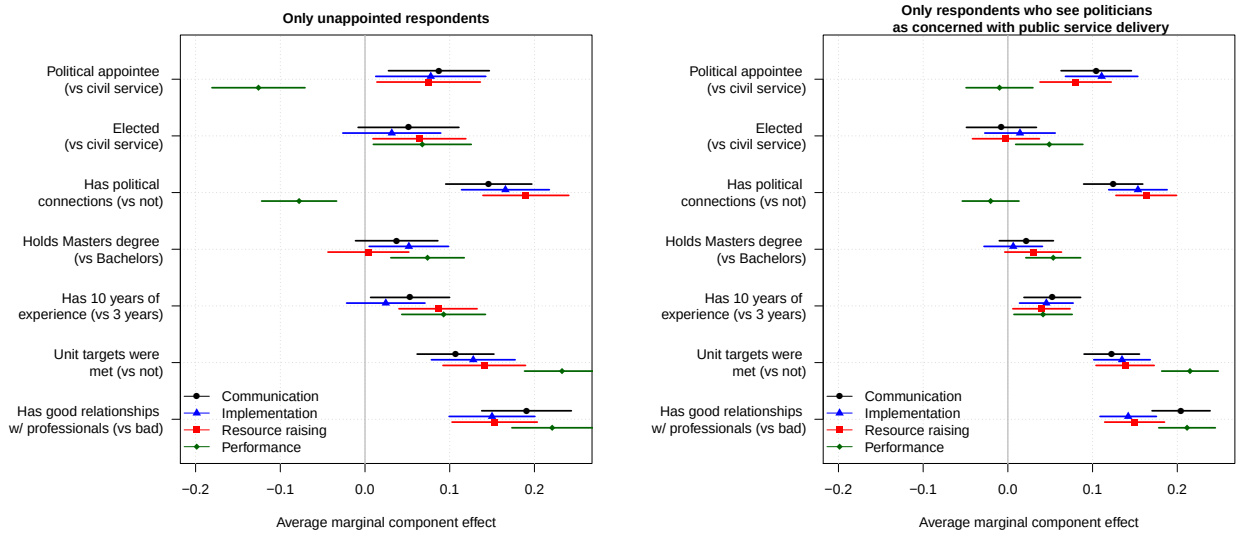
The plot on the left includes only responses from bureaucrats who are not political appointees. The plot on the right includes only responses from bureaucrats who expressed the highest level of agreement with the following statements: "The mayor and [education/healthcare/social assistance] professionals have the same priorities for [units, i.e., schools/clinics/social assistance centers]"; "The mayor is concerned with improving the quality of [units]"; "The secretariat of [education/healthcare/social assistance] helps us improve the performance of the [unit]"; and "The secretariat of [area] holds this [unit] accountable for its results."

Table A.20: Results of the conjoint experiment with street-level managers

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

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

Figure A.14: Conjoint results among subsets of respondents



See notes under Figure 5.

F Additional details of the online survey of politicians

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

F.1 Respondent recruitment and non-response

The state audit court of Rio Grande do Norte sent the survey to all mayors and to secretaries of five key areas (education, healthcare, social assistance, finance, and human resources) in the 167 municipalities of the state through its online platform.[56] Participation was voluntary. A total of 455 politicians participated and finished the survey, of which 50 were mayors and 405 secretaries.

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

Table A.21: Correlates of the number of responses per municipality

	Respondents (log)	No respondents (dummy)	Respondents (log) w/o zeroes
Population (logged)	0.024 (0.062)	0.004 (0.037)	0.036 (0.048)
GDP per capita (logged)	−0.216 (0.139)	0.114 (0.108)	−0.115 (0.136)
Deaths per thousand	0.033 (0.041)	−0.018 (0.023)	0.007 (0.035)
Mayor was reelected in 2016	0.373 (0.149)*	−0.218 (0.085)*	0.101 (0.122)
Constant	2.502 (1.423)	−0.702 (1.051)	1.956 (1.232)
R-squared	0.125	0.138	0.025
Observations	75	75	64

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

These respondents come from 142 municipalities. Municipalities where mayors were in their second term were more likely to participate, but conditional on some politicians responding there are no significant correlations between the number of respondents and basic political and socioeconomic characteristics of the municipality, as shown in Table A.21.

F.2 Descriptive statistics

Table A.22: Descriptive statistics for the survey of politicians, by position

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

F.3 Details of the conjoint experiment with politicians

Table A.23: Attribute and attribute values for bureaucrat profiles used in the conjoint experiment with politicians

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

Table A.24 details the regression results visualized in Figure 6. These correspond to the following choice tasks of the conjoint experiment: (i) Which of these bureaucrats do you think would have a better communication with the local government?; (ii) Which of these bureaucrats do you think would have more chances of implementing changes requested by the local government?;

(iii) Which of these bureaucrats do you think would work extra hours if necessary?; and (iv) Which of these bureaucrats do you think would achieve better performance?

Table A.24: Results of the conjoint experiment with politicians

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

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