The Costs of Running a Minority Government

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Abstract

Executive branch representatives must garner support from elected legislative officials to govern. Building a legislative majority is an important step in most executive mandates, but the process may impose significant costs on society. Using a regression discontinuity design, I show that Brazilian municipalities where mayors hold relatively few seats in the municipal chamber hire substantially more non-tenured civil servants. RD estimates indicate that minority-mayors hire (53.7 percent), spend (70.7 percent), and rely more on non-tenured civil servants as a labor supply source (40.9 percent). This increase in hiring is not limited to top government posts but extends to positions filled by both skilled and unskilled workers. Teacher and school principal surveys show that these additional hires are often inexperienced and perform worse on indicators of job quality. Municipalities led by mayors with limited legislative support also see declines in standardized test scores. Heterogeneous causal estimates further show that the effects on bureaucratic turnover decline monotonically with the number of seats held by the winning mayor in the municipal chamber.

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1 Introduction

The principle of separation of powers has profoundly shaped the modern nation-state. Democracies have sought to institutionalize checks and balances by dividing authority among executive, legislative, and judicial branches (De Montesquieu (1989)). Effective governance requires cooperation across these branches. In non-parliamentary systems, however, alignment between the executive and legislative is not guaranteed. It is common for the executive leader and the legislative majority to belong to different political coalitions.

This paper investigates how misalignment between the executive and legislative branches affects the bureaucracy and the provision of public services. The consequences of such misalignment are theoretically ambiguous. On the one hand, divided control may intensify political competition Besley et al. (2010) and strengthen accountability Jones (2013)). On the other hand, it may interfere with cohesion or induce executive leaders to trade government resources for legislative support, with potential implications for bureaucratic functioning and service delivery.

To examine the effects of executive–legislative misalignment, I employ a close-election regression discontinuity design. The empirical strategy proceeds in three steps. First, I identify municipalities in which a coalition secures a majority of seats in the municipal chamber. Second, within each of these municipalities, I determine the mayoral candidate aligned with that majority coalition. Third, I compare outcomes across municipalities where the majority coalition's candidate narrowly lost to a minority coalition candidate (treatment) and where the candidate narrowly won (control).

I draw on three primary data sources to implement this research design. First, Brazil's electoral court (*Tribunal Superior Eleitoral*) provides complete records of municipal elections for the 2004, 2008, 2012, and 2016 electoral cycles. Second, administrative data from the Ministry of Labor (*RAIS*) contain annual information on the universe of formal employment in Brazil. From these records, I extract detailed information on all hirings and firings of workers employed by municipal executive branches between 2003 and 2020. Third, data from the Ministry of Education provide standardized test scores and biennial principal—teacher surveys covering the years 2007 to 2019. Merging these sources at the municipal level yields a dataset of 8,544 municipal elections in which at least one mayoral candidate represented a coalition holding a majority of seats in the municipal chamber.

To address concerns about potential manipulation of the running variable, I implement the density test proposed by Cattaneo et al. (2018). In addition, I examine a broad set of pre-treatment and baseline covariates and show that they evolve smoothly across the RD cutoff. With the validity of the design thus supported, I proceed to establish three main facts.

First, I show that electing a mayor from a minority coalition leads to a sharp increase in workers hired under a non-tenured civil servant regime. The hiring of high-ranking officials—such as municipal secretaries and public service directors—rises by 28 percent under minority mayors. Among all non-tenured public servants, hirings increase by 40.9 percent. This increase in hirings is widespread across the public sector, affecting a broad set of occupations. I estimate large increases in the hiring of public sector heads, healthcare directors, lawyers, nurses, public health officials, social workers, among others.

Second, I show that electing a mayor from a minority coalition results in the hiring of inexperienced workers and a marked decline in indicators of bureaucratic performance. Drawing on principal and teacher surveys, I find that municipalities governed by minority mayors increase the shares of first-time principals and first-time civil servant teachers by 0.105 and 0.172, respectively. At the same time, measures of worker performance deteriorate. Teachers report that principals are less likely to "care about administration," while they themselves are more likely to feel overburdened with work and to perceive that students have fallen behind. Reports of teachers holding meetings to evaluate student performance also decline.

Third, I show that minority coalition mayors considerably reduce student learning outcomes. Over a four-year mandate, students in municipalities governed by minority mayors score 0.087 standard deviations lower on 5th-grade mathematics exams compared to those under majority mayors. The estimated effect is slightly smaller for 9th-grade students, who score 0.073 standard deviations lower.

Finally, I examine heterogeneous treatment effects to test the hypothesis that governments use public sector jobs as bargaining chips to secure legislative support. The intuition is that if the executive branch acquires a legislative majority by trading public sector jobs, then non-tenured civil servant hirings should decline as the mayor's coalition controls a larger share of seats in the municipal chamber.

To assess this hypothesis, I broaden the sample to include not only municipalities where a

coalition secured a majority of legislative seats but also those in which one coalition held a plurality of seats. I refer to coalitions with a plurality as *major coalitions*, and their competitors as *minor coalitions*. I then estimate the treatment effects of electing a minor coalition mayor at varying levels of counterfactual legislative support.

If the "majority acquiring" hypothesis holds, treatment effects on public sector hirings should be larger when the elected coalition controls a greater share of seats in the municipal legislature. To estimate these heterogeneous effects, I develop an extension of the standard one-dimensional regression discontinuity design.

The empirical strategy generalizes the conventional RDD framework to a multi-dimensional setting. For each share s of total municipal legislative seats, I use local linear regression to non-parametrically estimate the effect of electing a candidate from a minor coalition as opposed to a candidate from a major coalition holding a share $s \in [0.35, 0.75]$ of seats in the municipal chamber. Section 5 provides details on the implementation of this multi-dimensional regression discontinuity design.

The results of this design are consistent with the "majority acquiring" hypothesis. Specifically, electing a mayor whose coalition controls only a limited share of seats has small and often statistically insignificant effects on hirings. In line with the hypothesis, however, these effects become progressively larger as the share of seats held by the mayor's coalition increases. For example, electing a mayor whose coalition commands a qualified majority reduces the hiring and firing of municipal secretaries and public sector directors by more than 67% and 75%, respectively. Electing a mayor whose coalition does not hold a majority, however, has no statistically significant impact on hirings. A similar pattern emerges across broader measures, including the hiring and firing of all public sector employees, the hiring of school principals and teachers, and the recruitment of inexperienced teachers and principals.

This progressivity, however, is less evident for lower-level public service workers. In particular, no clear pattern arises for hirings among total non-tenured civil servants or for standardized test scores. The absence of consistent trends in these outcomes suggests that additional mechanisms may be at play.

This paper most closely relates to the literature on bureaucracy and public service delivery. Recent scholarship has highlighted state capacity as a central determinant of development (Acemoglu

and Robinson (2012), Besley and Persson (2014), Besley et al. (2022)), placing bureaucracy at the center of research in political science and development economics. It is well established that bureaucratic jobs are often used by politicians to reward supporters (Grindle (2012)). Geddes (2023) documents that, anecdotally, such practices trace back to at least the 1930s in Latin America. A growing body of work has since examined how bureaucratic appointments function as instruments of political reward. Among the most relevant, Barbosa and Ferreira (2023), Brollo et al. (2017), and Colonnelli et al. (2020) show that patronage is widespread in Brazilian public employment, rewarding party members, donors, and unelected politicians. Akhtari et al. (2022) demonstrates that political turnover induces bureaucratic turnover and potentially worsens educational outcomes. Beyond Brazil, Xu (2018) uses historical evidence from the British Empire to show how patronage distorted bureaucratic allocation and incentives, while Iyer and Mani (2012) and Brassiolo et al. (2020) show analogous dynamics in India and Ecuador, respectively. Together, this literature highlights the prevalence of patronage as a mechanism through which politicians influence bureaucratic structures across contexts.

The mechanism I propose in this paper, however, is distinct from patronage. While both involve the allocation of coveted bureaucratic positions, majority buying and patronage are conceptually different. Patronage implies a hierarchical relationship, in which a patron rewards a subordinate supporter. By contrast, majority buying is better described as a horizontal quid-pro-quo exchange across political coalitions: one coalition provides legislative support, while the other gains access to bureaucratic appointments. This paper therefore departs from the existing patronage literature by identifying a novel mechanism through which political bargaining shapes bureaucratic allocations.

A related strand of research shows that political affiliation influences bureaucratic behavior more broadly. Brollo et al. (2020) finds that politically appointed headmasters alter the distribution of conditional cash transfers around election periods. Spenkuch et al. (2023) shows that ideological misalignment between bureaucrats and politicians in the United States leads to higher costs, overruns, and delays in procurement. Toral (2024) demonstrates that bureaucratic replacement intensifies as politicians approach the end of their mandates. Complementing this empirical work, a large theoretical literature analyzes how bureaucratic structures affect performance, often emphasizing the accountability—autonomy tradeoff in designing institutions (Ashraf and Bandiera (2018), Besley and Ghatak (2005), Bostashvili and Ujhelyi (2019), Dahlström and Lapuente (2022),

Finan et al. (2015), Pepinsky et al. (2017), Rauch and Evans (2000)).

Finally, I connect this paper to research on the incentives faced by legislators. Fournaies and Hall (2022) show that legislators who cannot seek reelection exert less effort. Kroeger et al. (2017) and Bertrand et al. (2021) find that interest groups influence legislative voting patterns in the United States, while Baumgartner et al. (2009), Bertrand et al. (2014), and Drutman (2015) examine the mechanisms through which lobbying affects legislators' behavior.

2 Institutional Background

Brazil's political system shares many features with other Latin American democracies. At the municipal level, the legislature is unicameral. All municipal legislative officials (*vereadores*) are elected to four-year terms, with elections held simultaneously across the country. The number of representatives in each municipality is proportional to its population, ranging from a minimum of 9 to a maximum of 55 seats. Although elections for legislative officials and mayors are held on the same day, they are institutionally independent: voters cast separate ballots for mayoral and legislative candidates.

Mayoral and municipal legislative terms coincide. In municipalities with up to 200,000 registered voters (roughly 400,000 residents), mayoral elections are decided by plurality rule, with the candidate receiving the most votes declared the winner. In larger municipalities, a two-round system is employed: if no candidate secures an outright majority in the first round, the two leading candidates advance to a runoff.

Municipal parties may register independently or form coalitions to contest the mayoral race. When parties form a coalition, they must jointly nominate a single candidate to represent them in the mayoral election. In contrast, parties always compete independently for legislative seats. Each party fields its own list of candidates for the municipal chamber, and seats are distributed under a system of proportional representation. The number of seats a party receives is proportional to its total vote share, and within each party, seats are allocated to the candidates with the highest individual vote totals.

3 Datasets

My dataset combines five administrative sources collected by the Brazilian federal government. Most of these data are originally reported at the submunicipal level. Because treatment status is defined at the municipal level, I aggregate all variables to the municipal level for the main analysis. Additional details on aggregation and merging procedures are provided in the Data Appendix.

TSE

The Brazilian electoral court (*Tribunal Superior Eleitoral*, TSE) organizes municipal elections every four years. Since 1998, it has published detailed data on votes, coalitions, and victorious candidates for each election year. These records allow me to identify both mayoral winners and losers in all municipal elections, as well as to calculate the share of legislative seats held by each coalition competing in the mayoral race. The data are reported at the submunicipal (electoral-zone) level.

To match the availability of the RAIS and SAEB datasets, I focus on municipal elections held between 2004 and 2016.

RAIS

The Brazilian Ministry of Labor compiles the *Relação Anual de Informações Sociais* (RAIS), an annual census of formal employment. At the end of each year, all private and public organizations are legally required to submit reports on every employment contract active at any point during that year. These records include information on whether workers were hired or dismissed during the year, total hours and months worked, civil servant status, tenure status, and occupation. Each contract is linked to an occupation code¹, which enables the construction of variables at the municipal—occupation level.

For this study, I restrict attention to organizations affiliated with the municipal executive branch². Because RAIS is reported at the worker–contract level, I aggregate contracts to construct municipality-level counts of hirings, separations, real wage expenditure, and 40-hour–equivalent workers for organizations located in each municipality. To ensure comparability across municipalities and over time, I normalize effective labor supply so that one unit corresponds to a full-time

¹Identified through the Classification of Brazilian Occupations (CBO).

²Natureza Jurídica 103-1.

worker (40 hours per week) employed continuously for 12 months.

Although RAIS has been collected since the 1980s, its coverage and format changed substantially in earlier decades. To ensure consistency, I restrict the sample to 2003–2020, during which the structure of the dataset remained stable.

SAEB and Censo Escolar

Between 2007 and 2023, the Ministry of Education administered standardized tests and questionnaires to assess the quality of public education in Brazil. These assessments, collectively referred to as the *SAEB evaluations*, were conducted biennially. In each round, 5th- and 9th-grade students took nationwide standardized tests in Portuguese and Mathematics, while teachers and principals completed questionnaires regarding working conditions and performance. The evaluations covered all schools with at least 20 enrolled students in the relevant grades and included a representative sample of schools with at least 10 students.³

No aggregation is required for standardized test scores, as the Ministry of Education reports municipal-level averages for 5th- and 9th-grade students. Questionnaire responses from principals and teachers are aggregated at the municipal level, based on their school's location.

The school census (*Censo Escolar*) provides complementary information. Conducted annually by the Ministry of Education, it compiles administrative data on the organization and infrastructure of public schools, including the number of teachers, students, and schools, as well as basic self-reported indicators such as access to clean water, sewage, and electricity.

For this study, I harmonize test scores and questionnaire responses from 2007 to 2019. The 2021 round introduced major methodological changes, which prevent consistent harmonization across years.

IPEA

The Institute of Applied Economic Research (*Instituto de Pesquisa Econômica Aplicada*, IPEA) is a federal research agency that collects and publishes economic and demographic data. I rely on IPEA data to obtain municipal-level information on public finances, population, and GDP for the period 2003–2020.

³In 2007, schools located in rural areas were excluded from the census component of SAEB.

Sample Selection

For the empirical analysis, I implement several restrictions to ensure consistency and validity. First, the baseline analysis in Section 4 considers only elections in which a coalition secured a majority in the municipal legislature.⁴ Second, a small number of elections are annulled and repeated due to irregularities such as vote buying or ineligible candidacies. I exclude these elections from the sample. Third, to avoid complications arising from the plurality–runoff electoral system described in Section 2, I restrict attention to municipalities with up to 200,000 registered voters (roughly 400,000 residents).

Fourth, and most importantly, I address concerns of manipulation around the cutoff in very small municipalities. The density test proposed by Cattaneo et al. (2018) suggests discontinuities in the probability of victory near the cutoff, particularly for municipalities with fewer than 7,000 residents, where candidates from the majoritarian coalition appear disproportionately likely to win. To mitigate this concern, I exclude municipalities with fewer than 10,000 inhabitants at the start of the election year. Importantly, results are robust to including all municipalities; the exclusion is driven solely by concerns about manipulation. Section 4 discusses possible sources of this discontinuity.

The final sample in Section 4 consists of 8,544 municipal elections held across four electoral cycles between 2004 and 2016, covering 2,954 of Brazil's 5,569 municipalities. The sample in Section 5 includes 9,520 elections across 3,037 municipalities.

4 First Empirical Strategy

This section estimates the effects of electing a minority government. In every municipality in the final sample, at least one mayoral candidate was supported by a coalition holding a simple majority of seats in the municipal legislative chamber. I exploit this setting to estimate the causal impact of electing a mayor from a minority coalition relative to electing a majority coalition mayor. The empirical strategy is based on a sharp regression discontinuity design.

Many of the outcome variables are municipal-level hiring and firing counts, which are characterized by a large number of zeros and considerable right-skewness. Following the applied econometrics

⁴This restriction is relaxed in Section 5.

literature, I apply the inverse hyperbolic sine (IHS) transformation:

$$\sinh^{-1}(Y) = \ln\left(Y + \sqrt{Y^2 + 1}\right). \tag{1}$$

This transformation preserves zeros, mitigates skewness, and allows point estimates to be interpreted as approximate percentage changes. In addition, it improves the performance of asymptotic approximations in the presence of highly skewed count outcomes.

For municipality m in election year t, I construct two types of outcome variables. The first captures the short-run effect, defined as the outcome measured in the year immediately following the election:

$$Y_{m.t}^{SR} = Y_{m,t+1}. (2)$$

The second captures the medium-run effect, defined as the average outcome over the entire four-year electoral cycle:

$$Y_{m,t}^{MR} = \frac{1}{N} \sum_{k=0}^{3} Y_{m,t+k},\tag{3}$$

where N denotes the number of times an outcome has been measured in a given 4-year election cycle.

Finally, I standardized the municipalities' average student test scores so that point estimates can be compared to other papers in the literature and will be interpreted in standard deviation units.

Regression discontinuity estimation

I rely on a close-election regression discontinuity design and follow the robust bias-correction procedure proposed by Calonico et al. (2014) to non-parametrically estimate the effect of electing a minority-coalition mayor relative to a majority-coalition candidate. The design is implemented at the municipality-election year level (m, t) and is estimated using the following specification:

$$Y_{m,t}^* = \alpha + \beta_1 X_{m,t} + \beta_2 X_{m,t} T_{m,t} + \gamma T_{m,t} + \varepsilon_{m,t}, \tag{4}$$

where $X_{m,t}$ is the vote margin of the best-placed candidate from a minority coalition relative to the candidate of the majority coalition, and $T_{m,t}$ is an indicator equal to one if a minoritycoalition candidate won the mayoral election in municipality m and year t. The dependent variable $Y_{m,t}^*$ denotes an arbitrary outcome after applying the transformations described in the previous subsection. For each outcome, I report the bias-corrected point estimate γ , robust standard errors, and the p-value from the robust confidence interval of γ .

This regression discontinuity design identifies the causal effect of executive—legislative misalignment at the cutoff c = 0. While the design does not guarantee external validity of the estimates away from the cutoff, it provides the closest feasible approximation to an ideal experiment. In such a setting, municipalities would be randomly assigned either a majority-coalition mayor (control) or a minority-coalition mayor (treatment), and outcomes would be compared across these assignments.

Validity Checks

As with any regression discontinuity design, a central concern is potential manipulation of the running variable. In this setting, the main risk is that majority-coalition mayoral candidates might strategically influence the running variable to place themselves disproportionately on the winning side of close elections. Consistent with this concern, the density test proposed by Cattaneo et al. (2018) provides statistically significant evidence of manipulation (p-value = 0.02), indicating that majority-coalition candidates are unusually likely to win narrowly contested races. This discontinuity is concentrated in small municipalities with populations below 7,000.

Although explicit electoral fraud is one possible explanation, it is not the only one. In very small municipalities, popular coalitions may be better positioned to sway close elections through voter influence.⁵

To mitigate this concern, I restrict the sample to municipalities with populations of at least 10,000 at the start of the election year. With this restriction, the p-value under the null hypothesis of no manipulation increases to 0.71. Importantly, the main results remain robust when including municipalities with fewer than 10,000 inhabitants; the restriction is imposed solely to address concerns of manipulation.

⁵For instance, vote buying remains anecdotally common in small municipalities. As of 2024, over 300 cases of vote buying were under investigation by Brazil's federal police.

Finally, I conduct additional validity checks reported in the appendix. Baseline covariates evolve smoothly across the cutoff, and key outcomes—including turnover among high-ranking public sector workers, principals, and non-tenured teachers, as well as average standardized test scores—show no discontinuities in the year prior to the mayoral election.

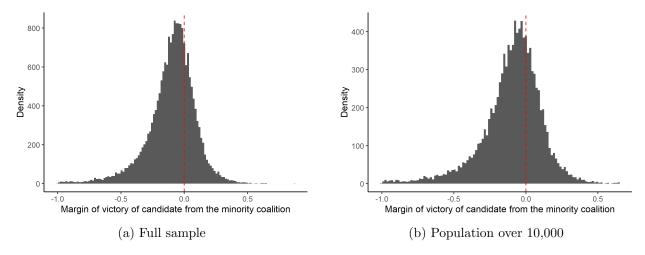


Figure 1: Sample density across specifications

4.1 First Empirical Strategy: Results

I begin by presenting the RD treatment effect estimates (γ) from equation (4) for government positions. Table 1 and Panel 2 summarize how misalignment between the executive and legislative branches affects turnover across different categories of government workers.

The results reveal a consistent pattern: electing a mayor from a minority coalition generates a substantial increase in non tenured hiring. Overall, all hirings rise by approximately 27.3 percent at the cutoff. This increase is largely driven by non-tenured civil servants, for whom hiring rises by 53.7 percent.

Having established that misalignment between the executive and legislative branches leads to increased bureaucratic hirings, I next decompose these effects by occupation. The results are presented in Table 3.

Table 2 presents the estimated effects of electing a minority-coalition mayor on municipal labor supply and wage expenditures, disaggregated by contract type. The results show that minority governments rely heavily on non-tenured civil servants. In the short run (t + 1), municipalities governed by minority mayors increase both the number and the wage bill of non-tenured civil

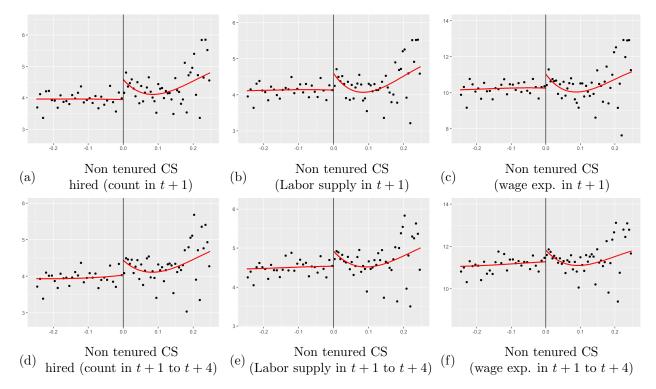


Figure 2: Hiring and firing patterns in the year after the election by contract type (sinh⁻¹(counts)).

servants, with wage expenditures on non-tenured civil servants rising by as much as 70.7 percent. This increase is driven by a substantial expansion in the labor supply of non-tenured employees, rather than higher individual compensation. Hours of non-tenured civil servants increase by roughly 40.9 percent.

Importantly, these effects are not transitory. The medium-run estimates, covering the full four-year electoral cycle, indicate that the elevated reliance on non-tenured staff persists throughout a mayor's mandate. At the same time, there is suggestive evidence that this expansion occurs at the expense of other categories: point estimates for tenured civil servants and temporary workers are generally negative or close to zero, though with relatively higher p-values. Taken together, the findings highlight how minority governments systematically reallocate municipal employment toward more politically pliable, non-tenured positions.

The regression discontinuity estimates also indicate that the increase in hirings induced by electing a minority-coalition mayor is pervasive across occupations. When the mayor lacks a legislative majority, the executive hires substantially more public sector heads⁶ (+28 percent), healthcare

⁶Typically municipal secretaries, service coordinators, and service directors.

directors (+18.9 percent), and lawyers⁷ (+16.9 percent).

Perhaps more surprisingly, Table 3 shows that this increase in hiring extends across a broad range of public sector occupations. Under minority governments, positive and statistically significant effects on hirings are observed for nurses (24.2 percent), social workers (11.9 percent), drivers (30.2 percent), and public health officials (17 percent). These patterns highlight that the surge in hirings is more prevalent in skilled non-tenured civil servant occupations.

Building on the evidence of the heightened reliance on non-tenured civil servants, I next examine the characteristics of new hires by focusing on the education sector. The *SAEB* dataset provides detailed information on public school principals and teachers, allowing me to assess not only the scale of hiring but also the composition of the workforce. Table 4 and Panel 3 document how minority governments increase their reliance on first-time principals and novice teachers, thereby reshaping the profile of the educational bureaucracy.

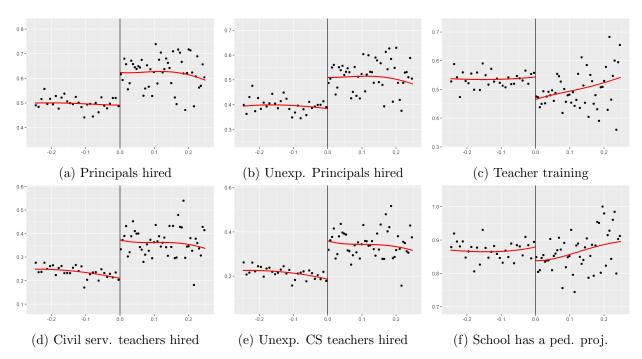


Figure 3: (SAEB) Effects of a minor coalition mayor on public schools in the year after an election (share of total employment)

Similar effects emerge in the education sector. Drawing on the *SAEB* dataset, I show that electing a mayor from a minority coalition induces substantial hirings among both school principals and teachers. Consistent with the RAIS estimates, minority mayors increase hiring in both top-tier

⁷Typically municipal prosecutors, public defenders, consultants, etc.

positions (principals) and downstream positions (civil servant teachers). This increase is primarily driven by the entry of first-time hires: the estimated increases in the shares of recently hired principals (0.11) and teachers (0.16) closely align with the increases in the shares of principals (0.105) and teachers (0.172) who are in their first year in the occupation. The magnitude of these effects is substantial: the RD estimates imply that electing a minority-coalition mayor exposes roughly one in six schools to an inexperienced principal or teacher.

This striking shift in the composition of the public workforce is consistent with the *majority* buying hypothesis. If sought-after public sector jobs are deployed as bargaining chips to secure legislative support, these positions are likely to be allocated to individuals who would otherwise face difficulty obtaining them on their own. In this context, one would expect to observe an increase in the hiring of inexperienced and otherwise less-qualified workers, reflecting the use of bureaucratic appointments as political currency.

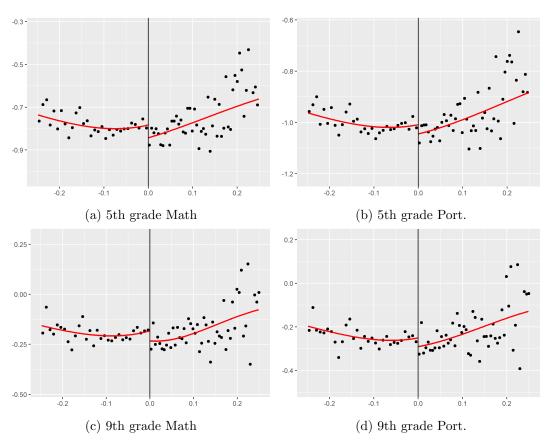


Figure 4: (SAEB) Effects of a minor coalition mayor on public schools' standardized test scores in t + 1 (std. deviations)

Finally, I analyze the impact of electing a minority coalition mayor on worker performance

indicators and students' test scores. Table 4 and panel 4 indicate that students in municipalities ruled by a minority government perform worse, on average. Point estimates indicate that electing a mayor who holds a minority in the legislative chamber decreases 5th-grade students' math scores on a standardized test by 0.087 standard deviations in the year following the election. Similarly, 9th graders experience a 0.073 drop in average test scores. While the point estimates for ninth-grade students and for the medium run (the full election cycle) are noisier, they nonetheless remain consistent with a decline in student performance.

It is worth noting that the SAEB evaluations indicate significant and persistent negative impacts of being ruled by a minority mayor on four indicators of teacher and principal on-the-job performance. The share of principals reporting that they offer training opportunities to teachers decreases 10.4 percentage points under a minority mayor. Principals are also 5.7 percentage points less likely to have a pedagogical project⁸. Teachers, on the other hand, are 5.6 percentage points less likely to report that the "principal cares about students' learning," and 6.9 percentage points more likely to state that "their students fell behind." These effects are not only persistent but also tend to intensify in the medium run.

Interpretation of the shape on the regression discontinuity

The discontinuities in hirings displayed in figure 2 exhibit a consistent pattern: a relatively flat (sometimes mildly sloped) relationship approaching the cutoff from the left, followed by a sharp positive discontinuity at the threshold and a steep downward slope that eventually converges toward a flat function on the right-hand side.

A simple and intuitive explanation accounts for this shape. The voting margin can be interpreted as a proxy for the public's support for the minority-coalition candidate. On the left-hand side of the cutoff, when a majority-coalition candidate is elected, non-tenured civil servant hirings are uncorrelated with support for the minority candidate, which explains the flat relationship. Once the minority candidate wins, however, popular support becomes directly relevant: the stronger the electoral mandate, the lower the level of bureaucratic hiring.

⁸In Brazil, the *projeto pedagógico* (or *projeto político-pedagógico*) is the legally mandated document that guides each school's educational philosophy, curriculum, and teaching practices. Established by the National Education Guidelines and Framework Law (*Lei de Diretrizes e Bases da Educação*, Law 9.394/1996), it serves as both a pedagogical plan and a political commitment to the school community. It is developed as a strategic plan to ensure accountability and improve educational quality.

More specifically, hirings decline at a decreasing rate as minority mayors enjoy broader electoral support, eventually flattening or turning slightly positive. This pattern is consistent with the majority buying hypothesis. According to this hypothesis, minority-coalition mayors secure legislative backing by distributing coveted bureaucratic positions to other parties. A more popular mayor is plausibly in a stronger bargaining position, since legislators value alignment with an executive who commands broad electoral support. Conversely, when a minority-coalition mayor prevails in a razor-thin contest, legislators from the majority coalition are able to demand greater access to bureaucratic appointments as a condition for supporting the new administration.

Alternative mechanisms and discussion

Although the evidence presented so far points toward the use of bureaucratic appointments as a tool for securing legislative support, it is important to examine alternative explanations for the observed results. I consider three such possibilities.

First, the effects could be driven by political turnover rather than a legislative-executive misalignment. As shown by Akhtari et al. (2022), political turnover naturally induces changes in the bureaucracy. Since incumbent mayors may plausibly help elect more municipal legislators, one might argue that my findings simply reflect the consequences of overturning incumbency overturn. Table 6 shows this is not the case. Estimated increases in hirings are sizable even when incumbency is not at stake.

Second, changes in local economic or fiscal conditions could explain the results. Table 8 rejects this possibility. Neither in the short run nor across the full election cycle do I find effects of electing a minority-coalition mayor on population, GDP, intergovernmental transfers, government labor expenditures, or education expenditures.

Finally, one could argue that the increase in hirings increases the public service delivery. Table 7 provides (limited) evidence in favor of this interpretation. Municipalities ruled by minority mayors potentially have more students enrolled and open schools within their jurisdictions. However, it may be the case that this expansion in public services coverage is of lower quality, as, for instance, schools under minority governments are 2 percent less likely to have access to a proper sewage system.

5 Second Empirical strategy: Heterogeneous Treatment Effects

Once the causal results are established in the previous section, I turn to the underlying mechanisms.

The analysis above provided several pieces of evidence consistent with the interpretation that nontenured civil servant appointments are strategically allocated to secure legislative support.

First, non-tenured civil servants are the only category of workers for whom hiring increases under a minority government, as shown in Table 2. These positions are highly coveted, and mayors retain considerable discretion in making such appointments. They are typically located high in the governmental hierarchy and come with substantial benefits⁹, including generous salaries, retirement provisions, and paid leave.

Second, the estimated effects on the composition of school personnel reported in Panel 3 and Table 4 appear to be driven by the hiring of inexperienced workers. This pattern suggests that public sector appointments often select individuals who would otherwise face significant obstacles in securing government employment based solely on their qualifications.

Finally, the recurring pattern of discontinuities across measures of non-tenured civil servant hirings indicates that these effects diminish as the elected mayor commands greater voter support. This finding is consistent with the majority-buying hypothesis, in which a mayor's popularity functions as an additional bargaining asset when negotiating with the local legislature.

Although these three pieces of evidence point toward the majority-buying mechanism, they remain circumstantial. To more directly assess whether mayors strategically deploy public sector appointments to secure legislative backing, I propose one final test.

This final test builds on a straightforward observation: if mayors use bureaucratic appointments to secure legislative support, then effects on hirings should be larger when the counterfactual mayor would have commanded stronger support in the municipal chamber. To illustrate, consider the following two elections:

2004 Election in Nhandeara (SP): In 2004, mayoral candidate Nelson Magalhães narrowly lost the election in Nhandeara, by a razor-thin margin of just 0.77 percent of the vote (roughly 51 ballots). His coalition nevertheless won the largest share of council seats, securing 3 out of 9. This was not enough for a majority, however, and Magalhães would have needed the support of at least

⁹See, for instance, Lei 8.112/1990; Art. 37, V, CF/88; Art. 37, IX, CF/88

two additional council members to govern with a stable majority.

2008 Election in Sooretama (ES): In 2008, Joana Rangel prevailed in Sooretama by an equally narrow margin of 73 votes. Unlike in Nhandeara, her coalition secured not only a majority but full dominance of the legislative chamber, winning all 9 of the 9 available seats.

In both cases, the coalition with the most council seats barely won the mayoralty. According to the majority-buying hypothesis, however, one would expect non-tenured civil servant hirings to decline most sharply in Sooretama. Had Joana Rangel lost in 2008, her opponent Esmael Loureiro would have assumed office with no representation in the municipal chamber, facing a legislature fully dominated by his rival coalition. By contrast, even if Dr. Odilon had won in Nhandeara in 2004, the legislative balance would have remained largely unchanged, since Nelson Magalhães's coalition already held a plurality and Odilon would still have had to negotiate additional support to reach a majority. Thus, the majority-buying hypothesis predicts stronger hiring effects in Sooretama than in Nhandeara.

Based on the thought experiment above, I propose estimating the treatment effects of electing a minority-coalition mayor conditional on the level of legislative support held by the winning coalition. Under the majority-buying hypothesis, the magnitude of the estimated effects should increase with the degree of legislative backing available to the elected mayor.

5.1 Second Empirical strategy: Estimation

There are substantial challenges to estimating treatment effects separately by the level of counterfactual legislative support that a mayor would have enjoyed. In principle, one can still rely on a close-elections design to obtain quasi-experimental evidence. The task at hand, however, is non-trivial. Ideally, one would like to estimate the treatment effect of electing a minority-coalition mayor for every point $p \in [0,1]$, where p denotes the share of counterfactual legislative support. The standard RD design, however, only yields a single local average treatment effect at the cutoff and, by construction, cannot be used to study such heterogeneous effects.

One strategy, common in the economics literature, is to introduce interaction terms. As in any regression framework, one can interact the treatment indicator with another variable to examine how treatment effects vary. The main drawback of this approach is that it imposes a functional form on the heterogeneity. For example, suppose the RD estimate is interacted with a linear term.

Then Equation 4 can be rewritten as:

$$\Delta Y_{m,t}^* = \alpha + \beta_1 X_{m,t} + \beta_2 X_{m,t} T_{m,t} + \gamma_0 T_{m,t} + \gamma_1 (T_{m,t} \times Z_{m,t}) + \varepsilon_{m,t}, \tag{5}$$

where $Z_{m,t}$ captures the share of legislative seats held by the major coalition of municipality m in election year t. This specification constrains the treatment effect to vary linearly with legislative support, even though no theoretical or empirical reason justifies such a functional form.

The linearity assumption can be relaxed by including higher-order polynomial interactions, but these too impose restrictive functional form assumptions. Moreover, as the degree of the polynomial increases, one reduces bias at the expense of higher variance, leading to increasingly imprecise estimates.

I therefore propose an alternative estimation strategy. Rather than estimating a standard RD in a two-dimensional space, I extend the framework to a three-dimensional setting. The first dimension is the usual running variable (X), the second is the outcome variable (Y), and the third dimension (Z) captures heterogeneity in treatment effects across legislative support.

Formally, I define a grid of points $\{p_n\}_{n=1}^N \subset [0,1]$ at which treatment effects will be estimated. For each grid point $(0, p_n)$, I estimate the effect of electing a minority-coalition mayor relative to a mayor whose coalition controls a share p_n of the municipal legislature, by estimating the following equation:

$$\Delta Y_{m,t} * = \alpha + \beta_1 X_{m,t} + \beta_2 X_{m,t} T_{m,t} + \gamma_0 T_{m,t} + \gamma_1 (Z_{m,t} - p_n) + \gamma_2 (Z_{m,t} - p_n) \times T_{m,t} + \varepsilon_{m,t}$$
 (6)

As before, $Z_{m,t}$ represents the share of seats of a municipality's major coalition¹⁰ of municipality m during election year t. $X_{m,t}$ is the voting margin of the best-placed candidate of a minor coalition relative to the mayoral candidate of the major coalition. $T_{m,t}$ is an indicator variable equal to one if a minor coalition candidate wins the majoral election in year t and municipality m.

In order to obtain a non-parametric estimate, the equation above is estimated using local linear regression. That is, only a subset of observations $\{(x_i, y_i, z_i)\}$ is used to estimate Equation 6 at a given point $(0, p_n)$. Observation i is included if and only if its distance to $(0, p_n)$ is smaller than or

¹⁰As stated in the introduction, this refers to the coalition with the most seats in the legislative

equal to a bandwidth b_n .

I define the distance d_i of an observation as the standard Euclidean distance, which in two dimensions collapses to the Pythagorean theorem:

$$d_i = \sqrt{(x_i - 0)^2 + (z_i - p_n)^2}. (7)$$

Finally, to mimic the triangular kernel weights commonly used in regression discontinuity designs, each local regression is estimated with "conical weights." The weight w_i of an observation i is defined as:

$$w_{i} = \begin{cases} 1 - \frac{|d_{i}|}{b_{n}}, & \text{if } d_{i} \leq b_{n}, \\ 0, & \text{if } d_{i} > b_{n}, \end{cases}$$
(8)

where d_i denotes the Euclidean distance from observation i to the evaluation point $(0, p_n)$, and b_n is the bandwidth parameter.

Figure 7 provides an illustration of how information from the observed joint distribution $(X_{m,t}, Z_{m,t})$ is leveraged to non-parametrically estimate treatment effects for four distinct points using illustrative bandwidths. To the best of my knowledge, this is the first paper to propose a multidimensional extension of the regression discontinuity design in order to capture heterogeneous treatment effects¹¹.

Extending the usual RD framework to a two-dimensional setting introduces two main complications. First, there is no established method for optimal bandwidth selection in multidimensional RD designs. While Calonico et al. (2014) has become the gold standard for bandwidth selection in canonical one-dimensional RD applications, their procedure is not designed for higher-dimensional settings. Second, moving from one to two dimensions raises concerns about the curse of dimensionality: as the dimensionality of the problem increases, data points become more dispersed, reducing the precision of local estimates.

In my main specification, I set the bandwidth to 0.15. As shown in Tables 1 to 4, optimal one-

¹¹Shortly after the completion of my first draft Calonico et al. (2025) published an econometric rigorous article formalizing this generalization of a regression discontinuity design. Their work will be incorporated into my future draft revisions

dimensional bandwidths typically fall between 0.09 and 0.12. Choosing 0.15 therefore represents a modest increase intended to offset the loss of precision caused by the additional dimension. Since this choice is admittedly arbitrary, I show in the Appendix that the results are robust to alternative bandwidths of 0.10 and 0.20.

To mimic the triangular kernel weights commonly used in one-dimensional RD designs, I adopt "conical weights," where the weight w_i assigned to observation i is defined as

$$w_i = 1 - \frac{|d_i|}{b},\tag{9}$$

with d_i denoting the Euclidean distance from observation i to the evaluation point and b the chosen bandwidth.

Finally, I restrict estimation to grid points $p_n \in \{0.4, 0.41, 0.42, \dots, 0.725\}$. As shown in Figure 7, most observations lie within this interval. Outside this range, point estimates and confidence intervals become too volatile. The grid increment of 0.01 could be refined, but doing so has negligible impact on the results while substantially increasing computational costs.

5.2 Second Empirical strategy: Results

I use the continuous RD framework described above to estimate the causal effect of electing a minority coalition government relative to a plurality government with legislative support p_n . These effects are identified by the parameter γ_0 in Equation 6, and the associated confidence intervals are constructed using the robust standard errors of this coefficient. The first set of outcome variables examined in this framework pertains to non-tenured civil servant hirings.

Figure 5 illustrates the heterogeneous effects of electing a minority-coalition mayor. The impact on the hiring of teachers and principals increases with the counterfactual level of legislative support that the mayor would have received from the municipal council. The figure also highlights that the nearly monotonic rise in school principal hirings is driven primarily by inexperienced workers. Indeed, the data in Figure 5 suggest that virtually all of the increase in school staff hirings attributable to minority-coalition mayors stems from higher rates of hiring inexperienced personnel.

I interpret these results as strong evidence in support of the majority buying mechanism. The observed hiring patterns are broadly consistent with the interpretation that mayors strategically

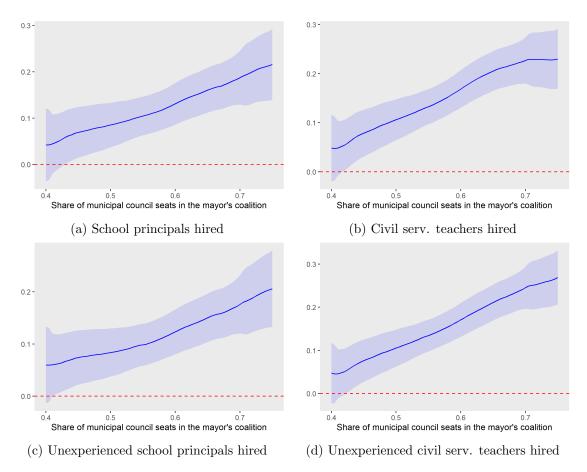


Figure 5: Hetereogeneous effects of electing a minor coalition mayor on school hirings $(sinh^{-1}(counts))$

allocate government positions to secure legislative support.

Panel 9 similarly indicates that bureaucratic performance is closely tied to a mayor's legislative backing. The estimated impacts of electing a mayor with little legislative support tend to be largest when compared to those of a mayor with ample support. As a mayor's counterfactual legislative support rises, so too does the share of teachers who report that they "feel overwhelmed," that "their students fell behind," and that "the school council met this year¹²." Comparable patterns emerge for measures of principal performance, though they are less pronounced than those observed for teachers.

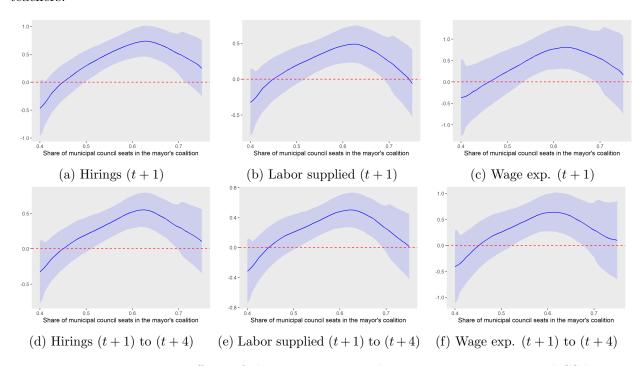


Figure 6: Hetereogeneous effects of electing a minor coalition mayor on non tenured CS hiring

The panel in Figure 6, however, paints a more nuanced picture. The findings for school workers do not generalize to the broader set of workers observed in RAIS. In particular, the estimates suggest that treatment effects follow a U-shape. While electing a minority-coalition mayor increases the hiring of non-tenured civil servants overall, the effects are not monotonic in the counterfactual level of legislative support. Instead, the treatment effect appears to attenuate once the counterfactual support surpasses a qualified majority.

Finally, I extend the analysis to examine the effects of electing a minority-coalition mayor on

 $^{^{12}}$ The school council refers to meetings between teachers (and in some cases parents) to discuss ways to improve student learning

standardized test scores. Panel 10 reports the results. In this case, however, the heterogeneous effects do not clearly align with a single underlying mechanism. Treatment effects on 5th- and 9th-grade test scores are non-monotonic and follow distinct patterns across the two grade levels.

6 Conclusion

The previous two sections establish several empirical facts regarding the causal effect of electing a mayor from a minority coalition. First, I show using a regression discontinuity design (RDD) that electing a minority-coalition mayor leads to an increase in hirings of non-tenured civil servants. Second, I demonstrate that in public schools, such elections result in higher rates of hiring inexperienced workers. Moreover, minority-coalition mayors are associated with worse measures of worker performance and lower student test scores in public schools.

Turning to the underlying mechanism, I find evidence in support of the claim that mayors with little legislative backing use coveted public sector positions as bargaining chips to secure the support of municipal legislators. RDD estimates show that electing a minority-coalition mayor sharply increases hirings of non-tenured civil servants across a wide range of occupations. Detailed school-level data further reveal that these new hires disproportionately consist of inexperienced teachers and principals. At the same time, measures of school worker performance decline markedly under minority administrations.

Next, I demonstrate how the RDD evidence rules out two prominent alternatives to my proposed mechanism. First, there is no evidence that the observed hiring patterns are driven by incumbency losses of the type studied in Akhtari et al. (2022). Second, municipal-level "macro" variables—such as GDP, intergovernmental transfers, and education budgets—are unaffected by the election of a minority-coalition mayor.

Finally, by employing my novel multidimensional RDD strategy, I show that estimates of heterogeneous treatment effects provide further support for the majority buying mechanism. The impact of electing a minority-coalition mayor on overall hirings of school principals, and teachers increases monotonically with the level of counterfactual legislative support. Similar upward-sloping patterns appear in the hiring of inexperienced school workers and in measures of school worker performance.

There are, however, two important nuances to the majority buying mechanism. First, the het-

erogeneity in the hiring of non-tenured civil servants suggests that additional mechanisms may also be at work. Second, the heterogeneous effects of electing a minority-coalition mayor on student test scores indicate that majority buying is not the only relevant channel linking legislative—executive alignment to public service delivery.

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Table 1: (RAIS) Hiring and firing patterns in the year after the election by contract type

	$sinh^{-\frac{1}{2}}$	(number	of recently	$sinh^{-1}$ (number of recently fired)				
	Total Workers	Civil Serv.	Civil Serv. (tenured)	Temp. Workers	Total Workers	Civil Serv.	Civil Serv. (tenured)	Temp. Workers
γ	0.273**	0.537***	-0.159	-0.124	0.205	0.315*	0.056	-0.092
SE	(0.115)	(0.182)	(0.168)	(0.218)	(0.166)	(0.171)	(0.154)	(0.193)
p-value	[0.018]	[0.003]	[0.344]	[0.568]	[0.217]	[0.065]	[0.716]	[0.631]
N	8544	8544	8544	8544	8544	8544	8544	8544
Eff. N	4362	4207	4100	3832	4640	4553	4199	4036
b	0.123	0.117	0.113	0.104	0.133	0.129	0.117	0.111

Table 2: Effects of Minority-Coalition Mayors on labor supply and Wages (Short-run vs. Medium-run)

	Labor supply: Non-tenured CS	Labor supply: Tenured CS	Labor supply: Temporary	Wage exp.: Non-tenured CS	Wages exp.: Tenured CS	Wages exp.: Temporary				
Panel A: Short-run $(t+1)$										
γ	0.409**	-0.191	-0.157	0.707**	-0.449^*	-0.237				
SE	(0.183)	(0.16)	(0.215)	(0.357)	(0.269)	(0.459)				
p-value	[0.026]	[0.23]	[0.464]	[0.047]	[0.095]	[0.605]				
N	8544	8544	8544	8544	8544	8544				
Eff. N	3763	3807	4007	3744	3728	4024				
b	0.101	0.103	0.11	0.101	0.1	0.111				
Panel I	3: Medium-run ((t+1 to t+4)								
γ	0.382**	0.045	-0.102	0.525**	0.06	-0.347				
SE	(0.159)	(0.117)	(0.221)	(0.261)	(0.187)	(0.445)				
p-value	[0.016]	[0.703]	[0.644]	[0.045]	[0.748]	[0.435]				
N	8144	8144	8144	8144	8144	8144				
Eff. N	3755	3687	3646	3458	3655	3810				
b	0.108	0.105	0.104	0.097	0.104	0.11				

Table 3: (RAIS) Hiring and firing patterns in the year after the election by occupation

					$\sinh^{-1}(counts)$)				
	Public Sector Heads	Health Directors	Lawyers	Nurses	Public Health Officials	Social Workers	Teachers	Auditors	Drivers	Guards
Panel	A: Hired									
γ	0.28*	0.189***	0.169***	0.242**	0.17^{*}	0.119**	0.32^{*}	0.048	0.302**	0.146
SE	(0.161)	(0.068)	(0.063)	(0.114)	(0.092)	(0.057)	(0.166)	(0.031)	(0.12)	(0.151)
p-value	[0.082]	[0.006]	[0.007]	[0.034]	[0.064]	[0.038]	[0.054]	[0.114]	[0.012]	[0.331]
N	8260	8260	8260	8260	8260	8260	8260	8260	8260	8260
Eff. N	3818	2773	3713	4213	3808	4389	3936	4386	3512	3483
b	0.108	0.074	0.104	0.122	0.108	0.128	0.112	0.128	0.097	0.096
Panel	B: Fired									
γ	0.121	0.047	0.071^{*}	0.122	0.113*	0.034	0.32**	0.043*	0.231**	0.154
SE	(0.118)	(0.037)	(0.042)	(0.085)	(0.067)	(0.044)	(0.143)	(0.025)	(0.107)	(0.122)
p-value	[0.305]	[0.202]	[0.089]	[0.151]	[0.093]	[0.437]	[0.025]	[0.085]	[0.03]	[0.207]
N	8260	8260	8260	8260	8260	8260	8260	8260	8260	8260
Eff. N	3576	3758	3273	4410	3568	4479	4073	3099	3201	3588
b	0.099	0.106	0.09	0.129	0.099	0.132	0.117	0.084	0.087	0.099

Dependent variables are transformed using the inverse hyperbolic sine: $\sinh^{-1}(\text{counts})$.

Skilled hirings (e.g., lawyers, nurses) refer to non-tenured civil servant contracts. Unskilled occupations (e.g., public health officials, social workers, teachers, drivers, guards) are observed through both civil servant and temporary contracts.

Table 4: (SAEB) School worker hirings patterns and performance $\,$

						Share	of all municipa	l reports				
		Principals'	Survey			Teachers' Survey						
	Principals hired	Unexp. princ. hired	Offers teacher training	Sch. has a pedag. proj.	Civil serv. Teachers hired	Unexp. civil serv. teachers hired	School council met this year	"I feel over- whealmed"	"My students fell behind"	"Principal cares about: students' learning"	"Principal cares about: management"	"Principal cares about: maintenance"
Panel	A: Short-ru	n (t+1)										
γ	0.118***	0.105***	-0.104***	-0.057***	0.16***	0.172***	-0.029	0.041	0.069**	-0.056**	0.002	-0.02
SE	(0.034)	(0.033)	(0.031)	(0.021)	(0.029)	(0.03)	(0.024)	(0.028)	(0.035)	(0.024)	(0.024)	(0.024)
p-value	[0.001]	[0.002]	[0.001]	[0.008]	[0.000]	[0.000]	[0.224]	[0.144]	[0.049]	[0.021]	[0.919]	[0.419]
N	6293	6293	6293	6293	6204	6204	6204	6204	6204	6204	6204	6204
Eff. N	2888	2820	2648	3284	2750	2744	3287	2488	2528	2834	2978	3239
b	0.108	0.105	0.097	0.128	0.104	0.103	0.131	0.092	0.094	0.108	0.115	0.129
Panel	B: Medium-	$\mathbf{run} \ (t+1 \ \mathbf{to} \ t+$	- 4)									
γ	0.056**	0.049^{*}	-0.053**	-0.028*	0.084***	0.094***	-0.03	0.061*	0.095**	-0.071**	-0.022	-0.036
SE	(0.027)	(0.026)	(0.022)	(0.016)	(0.017)	(0.017)	(0.02)	(0.036)	(0.046)	(0.03)	(0.017)	(0.026)
p-value	[0.040]	[0.055]	[0.013]	[0.075]	[0.000]	[0.000]	[0.131]	[0.090]	[0.039]	[0.017]	[0.190]	[0.163]
N	4810	4900	6232	6232	6144	6144	6144	3970	3970	6144	6144	3970
Eff. N	2412	2258	3068	2951	2794	2689	3053	1538	1474	2428	2788	1861
b	0.12	0.107	0.119	0.114	0.107	0.102	0.121	0.085	0.081	0.091	0.107	0.108

Table 5: (SAEB) Effects on student test scores in the 4 years after the election

		5th Grade)	9th Grade							
	Math	Port.	Port. Combined Math Port.								
Panel A: Short-run $(t+1)$											
γ	-0.087**	-0.059**	-0.073**	-0.076**	-0.055	-0.061^*					
SE	(0.038)	(0.03)	(0.033)	(0.038)	(0.036)	(0.034)					
p-value	[0.02]	[0.047]	[0.029]	[0.046]	[0.127]	[0.074]					
N	6427	6427	6427	4833	4833	4833					
Eff. N	2901	3148	2974	2696	2700	2976					
b	0.106	0.118	0.11	0.136	0.136	0.155					
Panel	B: Medium	-run (t + 1	to $t+4$)								
γ	-0.066	-0.047	-0.057	-0.064	-0.053	-0.058					
SE	(0.045)	(0.04)	(0.042)	(0.05)	(0.047)	(0.048)					
p-value	[0.142]	[0.243]	[0.177]	[0.202]	[0.267]	[0.228]					
N	4242	4242	4242	3042	3042	3042					
Eff. N	2024	1917	1958	1556	1562	1554					
b	0.119	0.11	0.114	0.127	0.127	0.126					

Table 6: Effects of electing a minority-coalition mayor on hirings, wages, and education outcomes

	Non-ten. civil serv. hired	Wages: non-ten. civil serv.	Labor sup.: non-ten. civil serv.	New principals	Inexp. principals	New teachers	Inexp. teachers	Offer training	Pedag. project	Teach. lack content	Teach. over- whelmed
Panel A	: All election	ıs									
Estimate	0.537***	0.707**	0.409**	0.118***	0.105***	0.160***	0.172***	-0.104***	-0.057***	0.069**	0.041
SE	(0.182)	(0.357)	(0.183)	(0.034)	(0.033)	(0.029)	(0.030)	(0.031)	(0.021)	(0.035)	(0.028)
p-value	[0.003]	[0.047]	[0.026]	[0.001]	[0.002]	[0.000]	[0.000]	[0.001]	[0.008]	[0.049]	[0.144]
N	8544	8544	8544	6293	6293	6204	6204	6293	6293	6204	6204
Eff. N	4207	3744	3763	2888	2820	2750	2744	2648	3284	2528	2488
b	0.117	0.101	0.101	0.108	0.105	0.104	0.103	0.097	0.128	0.094	0.092
Panel B	: No incumb	ent candida	te								
Estimate	0.492**	0.842*	0.437*	0.145***	0.136***	0.179***	0.192***	-0.154***	-0.068**	0.059	0.062
SE	(0.245)	(0.485)	(0.236)	(0.039)	(0.041)	(0.036)	(0.037)	(0.041)	(0.027)	(0.045)	(0.038)
p-value	[0.045]	[0.082]	[0.064]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.013]	[0.185]	[0.102]
N	4672	4672	4672	3263	3263	3229	3229	3263	3263	3229	3229
Eff. N	2392	2128	2317	1991	1754	1655	1694	1489	1701	1492	1436
b	0.120	0.103	0.115	0.151	0.129	0.121	0.125	0.104	0.124	0.106	0.100
Panel C	: No incumb	ent candida	te or incumb	ent party							
Estimate	0.633**	1.044**	0.560**	0.156***	0.150***	0.201***	0.216***	-0.103**	-0.049	0.101**	0.066
SE	(0.281)	(0.522)	(0.267)	(0.049)	(0.047)	(0.038)	(0.038)	(0.043)	(0.033)	(0.051)	(0.042)
p-value	[0.024]	[0.046]	[0.036]	[0.002]	[0.001]	[0.000]	[0.000]	[0.017]	[0.132]	[0.047]	[0.115]
N	4080	4080	4080	2942	2942	2895	2895	2942	2942	2895	2895
Eff. N	1905	1746	1785	1420	1508	1377	1375	1458	1396	1162	1082
b	0.110	0.099	0.101	0.115	0.124	0.113	0.113	0.118	0.112	0.092	0.085

Table 7: Alternative mechanisms: Effects of electing a minority-coalition mayor on other school outcomes

	Open schools	K-12 students	Teachers on payroll	Schools with water	Schools with energy	Schools with sewage	Schools: "insuff. infra"	Schools: "insuff. funds"	Schools: "insuff. staff"	Schools: "insuff. textbooks"
Panel	A: Short	run $(t+1)$								
γ	0.127	0.105	0.127*	-0.016*	-0.009	-0.019**	0.044	0.016	-0.016	0.031
SE	(0.092)	(0.075)	(0.070)	(0.009)	(0.009)	(0.009)	(0.035)	(0.021)	(0.023)	(0.035)
p-value	[0.168]	[0.165]	[0.070]	[0.069]	[0.332]	[0.034]	[0.209]	[0.441]	[0.473]	[0.380]
N	6615	6615	6615	6615	6615	6615	6204	6293	6293	6204
Eff. N	2771	2875	2616	3197	3099	3466	2279	3418	3712	2903
b	0.096	0.101	0.091	0.116	0.111	0.129	0.082	0.135	0.150	0.112
Panel	B: Mediu	\mathbf{m} run (t +	1 to $t + 4$)							
γ	0.123	0.106	0.130*	-0.012	-0.007	-0.019**	0.072*	0.019	-0.015	0.045
SE	(0.090)	(0.075)	(0.070)	(0.008)	(0.008)	(0.009)	(0.042)	(0.020)	(0.019)	(0.032)
p-value	[0.173]	[0.157]	[0.063]	[0.153]	[0.385]	[0.025]	[0.081]	[0.338]	[0.425]	[0.165]
N	6614	6614	6614	6614	6614	6614	3970	6232	6232	3970
Eff. N	2780	2908	2635	3035	3069	3454	1756	3209	3023	1924
b	0.097	0.103	0.092	0.108	0.110	0.128	0.100	0.126	0.117	0.113

Table 8: Alternative mechanisms: Effects of electing a minority-coalition mayor on municipal budget, GDP, and population (IPEA data)

	Log(munic. labor exp.)	Log(munic.	Log(munic. educ. exp.)	Log(munic.	Log(munic. population)	Log(munic.
Estimate	0.037	0.087	0.108	-0.005	0.018	0.041
SE	(0.104)	(0.081)	(0.107)	(0.088)	(0.057)	(0.059)
p-value	[0.724]	[0.281]	[0.314]	[0.953]	[0.757]	[0.480]
N	8508	8508	8487	8702	8702	8508
Eff. N	4407	4504	4142	4058	3889	3435
b	0.125	0.129	0.116	0.109	0.103	0.092

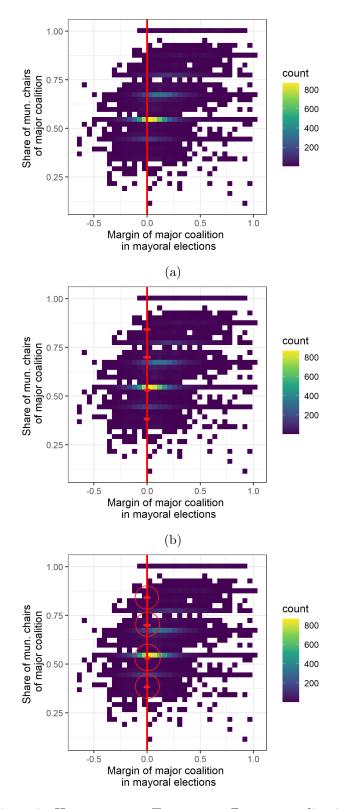


Figure 7: Heterogeneous Treatment effects generalization

Figure 8: (SAEB) Teachers' and Principals' performance in the 4 years after the election $(sinh^{-1}(counts))$

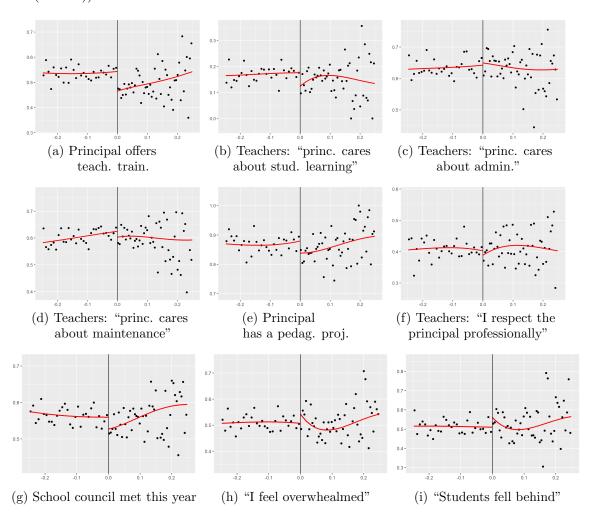


Figure 9: Hetereogeneous effects of electing a minor coalition mayor on bureaucratic performance in the 4 years after the election $(sinh^{-1}(counts))$

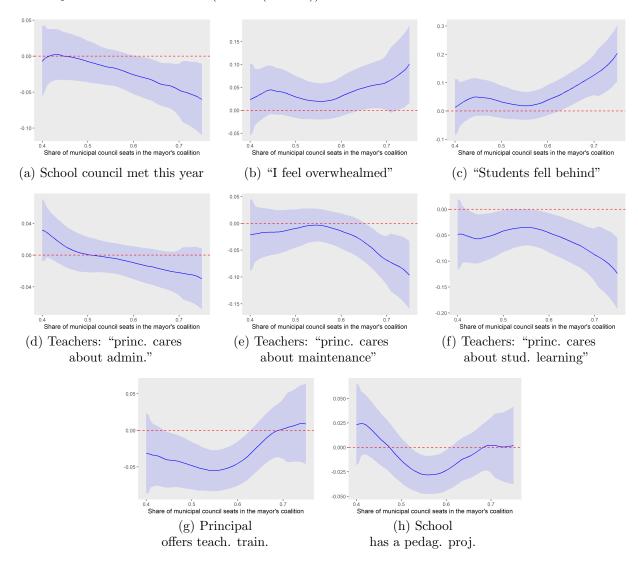


Figure 10: Hetereogeneous effects of electing a minor coalition mayor on standardized test scores (std. dev's)

