

The Costs of Running a Minority Government

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Abstract

In non-parliamentary political systems, the executive and legislative branches can be controlled by opposing coalitions. I study how this political misalignment shapes fiscal and bureaucratic behavior. Using a regression discontinuity design for Brazilian municipalities, I find that mayors who govern with a legislative minority hire 53.7 percent more non-tenured civil servants and spend 70.7 percent more on their wages compared to mayors who hold a legislative majority. Survey evidence from teachers and school principals indicates that the rise in hiring is driven by the recruitment of inexperienced bureaucrats. Furthermore, municipalities governed by minority mayors subsequently experience a persistent decline in bureaucratic performance. Results from a three-dimensional regression discontinuity design show that these effects intensify as the share of legislative seats held by the mayor's coalition decreases.

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1 Introduction

The principle of separation of powers has long shaped the organization of modern states. Democracies establish checks and balances by dividing authority among the executive, legislative, and judicial branches. Effective governance requires cooperation across these branches. In non-parliamentary systems, however, alignment between the executive and legislative branches is not guaranteed. It is common for the executive and the legislative majority to belong to different political coalitions.

This paper examines how misalignment between the executive and legislative branches affects bureaucratic appointments and performance. 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 weaken policy cohesion and induce executive leaders to trade bureaucratic appointments for legislative support, resulting in suboptimal hiring and weaker bureaucratic performance.

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 municipality, I determine the mayoral candidate aligned with that majority coalition. Third, I compare outcomes between municipalities where the majority coalition’s candidate narrowly lost to a minority coalition candidate (treatment) and those where the candidate narrowly won (control).

I draw on three primary data sources to implement this research design. First, records from Brazil’s electoral court (*Tribunal Superior Eleitoral*) provide complete information on municipal elections for the 2004, 2008, 2012, and 2016 electoral cycles. Second, administrative data from the Ministry of Labor’s *RAIS* system contain annual information on the universe of formal employment in Brazil. From these records, I extract detailed data on all hirings and separations of workers employed by municipal executive branches between 2003 and 2020. Third, data from the Ministry of Education include standardized test scores and biennial surveys of school principals and teachers covering the years 2007 to 2019. Merging these sources at the municipal level yields a dataset encompassing 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. Finally, I proceed to establish three main facts.

First, I examine changes in the hiring patterns of non-tenured civil servants. These positions are highly sought after due to their generous pay and benefits, and mayors exercise considerable discretion in making such appointments. I show that electing a mayor from a minority coalition leads to a sharp increase in hiring under non-tenured contracts. Specifically, municipalities governed by minority mayors hire 53.7percent more non-tenured civil servants. This expansion is broad-based across the public sector, encompassing positions such as department heads, healthcare directors, lawyers, nurses, public health officials, and social workers. Correspondingly, spending on the wages of non-tenured civil servants rises by roughly 70.7percent.

Second, to examine the characteristics of newly hired workers, I draw on survey data from public school teachers and principals. The estimates indicate that the overall increase in hiring is driven by a proportional rise in the recruitment of inexperienced employees. Electing a minority mayor increases the share of recently hired principals by 0.118 percentage points relative to all principals in the municipality. Simultaneously, electing a minority mayor raises the share of inexperienced teachers by 10.5 percentage points. Similarly, the share of recently hired and inexperienced civil servant teachers in a municipality increases by 0.16 and 0.172 percentage points, respectively.

Third, following the hiring of inexperienced bureaucrats, I show that measures of worker performance deteriorate. Under minority governments, regression discontinuity estimates indicate that teachers are less likely to report that principals “care about students’ learning” and more likely to state that “their students fell behind.” Principals, in turn, are significantly less likely to offer training opportunities to teachers or to maintain a formal plan for improving school performance (*projeto pedagógico*).

Finally, I examine heterogeneous treatment effects to shed light on the mechanisms underlying the three main results. To do so, I broaden the sample to include not only municipalities where a coalition secured a legislative majority but also those where 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 across varying levels of counterfactual legislative support. This empirical strategy generalizes the conventional regression

discontinuity design (RDD) framework to a three-dimensional setting. For each share s of total municipal legislative seats, I use local linear regression to nonparametrically estimate the effect of electing a candidate from a *minor coalition* as opposed to a candidate from a *major coalition* holding a share $s \in [0.40, 0.75]$ of seats in the municipal chamber. Section 5 provides details on the implementation of this three-dimensional RDD.

The results of this exercise reinforce the finding that legislative support is a key determinant of bureaucratic hiring and performance. In general, treatment effects on the hiring of principals and civil servant teachers increase with the share of municipal legislative seats held by the elected mayor. In other words, the effects of electing a candidate from a major coalition are relatively weak when that coalition holds limited power in the legislative branch.

These results show that mayors with limited legislative support tend to increase hiring and to recruit inexperienced bureaucrats who subsequently underperform in their duties. This pattern is consistent with the notion of “majority buying” observed in presidential systems. Media outlets in countries such as Argentina, Colombia, Brazil, and Mexico frequently report that political appointments are exchanged for legislative support. The evidence presented is consistent with such exchanges as the potential underlying mechanism of the results in this paper.

Literature Review

This paper is most closely related 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) document 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 examined in this paper differs from traditional patronage. Although both involve the allocation of desirable bureaucratic positions, they operate through distinct channels. Patronage implies a vertical relationship in which a politician rewards loyal supporters. Majority buying, by contrast, is a horizontal exchange between political coalitions: legislative backing is traded for control over bureaucratic appointments. This paper departs from the patronage literature by providing evidence of this alternative form of political bargaining and its consequences for bureaucratic performance and allocation.

A related strand of research shows that political alignment shapes bureaucratic behavior more broadly. Brollo et al. (2020) finds that politically appointed school principals adjust the distribution of conditional cash transfers around election periods. Spenkuch et al. (2023) shows that ideological misalignment between bureaucrats and elected officials in the United States leads to higher costs, overruns, and delays in procurement. Toral (2024) documents that bureaucratic turnover intensifies as politicians near the end of their terms. Complementing this empirical work, a large theoretical literature examines how bureaucratic organization affects performance, often highlighting the accountability–autonomy trade-off in institutional design (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). I relate to this literature by showing that bureaucratic appointments and performance are heavily shaped by the level of legislative support held by the executive branch. The evidence in this paper suggests that legislators trade legislative support for political appointments in the executive branch.

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 legislators (*vereadores*) are elected to four-year terms, with elections held simultaneously nationwide. The number of representatives in each municipality is proportional to its population, ranging from a minimum of nine to a maximum

of fifty-five seats. Although elections for mayors and legislators occur on the same day, they are institutionally independent: voters cast separate ballots for each office.

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.

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 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 the period 2003–2020, during which the dataset’s structure remained stable. I rely on the data cleaning procedures documented by Dahis et al. (2022) as the source for this analysis.

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

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

²Natureza Jurídica 103-1.

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

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.

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 (usually this number amounts to 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.

⁴This restriction is relaxed in Section 5.

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.

The final sample in Section 4 consists of 8,702 municipal elections held across four electoral cycles between 2004 and 2016, covering 3,073 of Brazil's 5,569 municipalities. The sample in Section 5 includes 10,579 elections across 3,147 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 literature, I apply the inverse hyperbolic sine (IHS) transformation:

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

This transformation avoids dropping municipalities with zero counts, 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:

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

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

$$Y_{m,t}^{MR} = \frac{1}{N} \sum_k Y_{m,t+k}, \quad (3)$$

where N denotes the number of times an outcome has been measured in a given 4-year election cycle, and k denotes the years this outcome has been measured relative to the last election.

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:

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

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 minority-coalition 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.

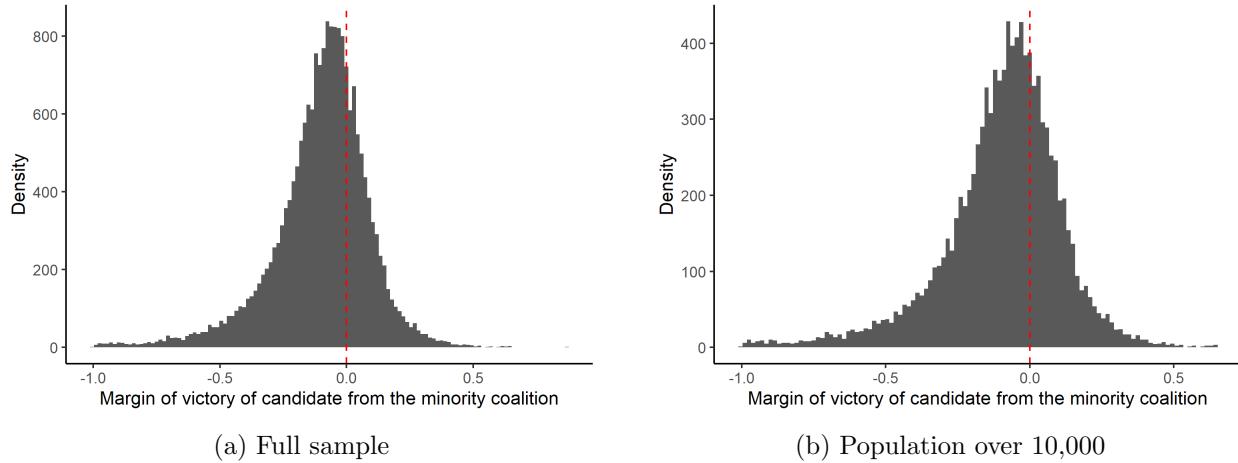


Figure 1: Sample density across specifications

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 of this paper remain robust when including municipalities with fewer than 10,000 inhabitants; the restriction is imposed solely to address concerns of manipulation.

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

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.

workers, principals, and non-tenured teachers, as well as average standardized test scores—show no discontinuities in the year prior to the mayoral election.

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 leads to a substantial increase in non-tenured hiring. Overall, total hiring rises 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. Moreover, municipalities governed by minority mayors spend 70.3 percent more on the wages of non-tenured civil servants and increase their total hours worked—used as a proxy for labor supplied—by 40.9 percent.

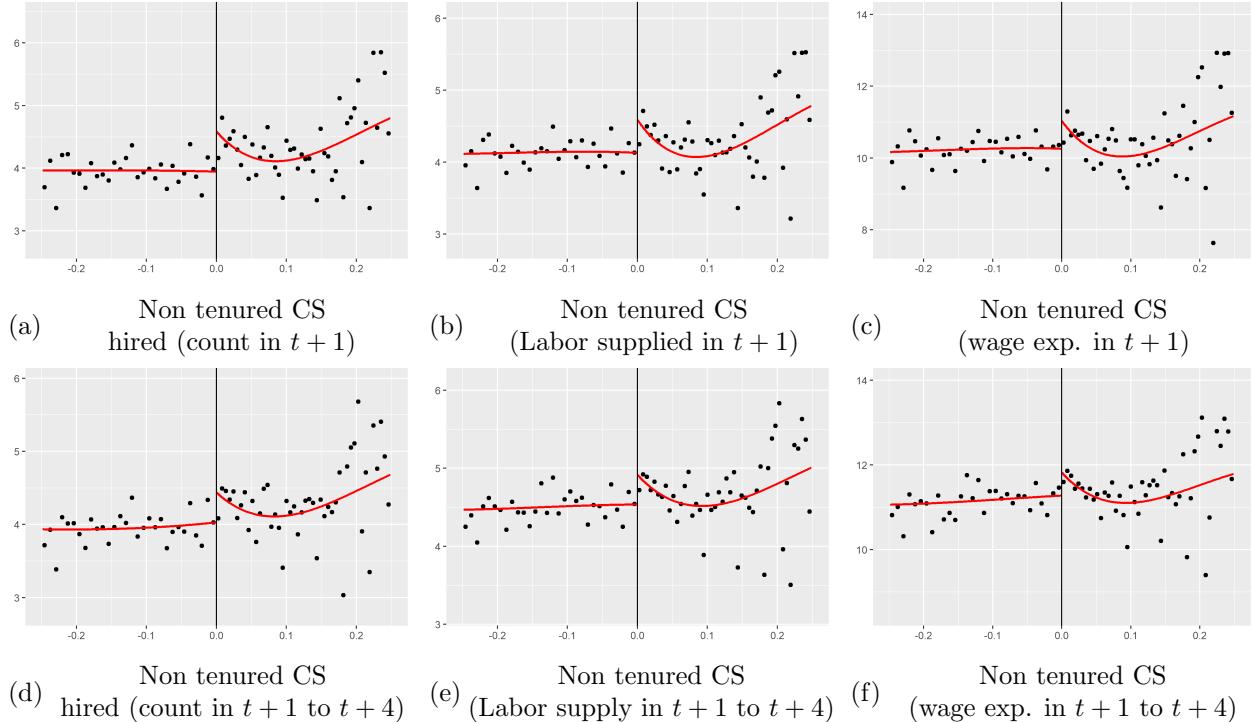


Figure 2: Hiring, wage expenditure, and labor supply patterns in $t + 1$ ($\sinh^{-1}(\text{counts})$).

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 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. Medium-run estimates, covering the full four-year electoral cycle, indicate that the elevated reliance on non-tenured staff persists throughout a mayor's term. At the same time, there is suggestive evidence that this expansion has spillovers effect to other employment categories: point estimates for the hiring of tenured civil servants and temporary workers are generally negative or close to zero, albeit with relatively higher p-values. In addition, I find evidence of increased firings among existing non-tenured civil servants. Taken together, these results suggest that the expansion of non-tenured employment likely occurs at the expense of previously employed non-tenured workers, tenured civil servants, and temporary staff. Having established that misalignment between the executive and legislative branches leads to greater bureaucratic hiring, I next decompose these effects by occupation. The results are presented in Table 3.

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

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 although the surge in hirings is widespread.

Building on the evidence of the increased reliance on non-tenured civil servants, I next examine the characteristics of new hires by focusing on the education sector. The *SAEB* dataset provides

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

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

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 as a share of total education workforce.

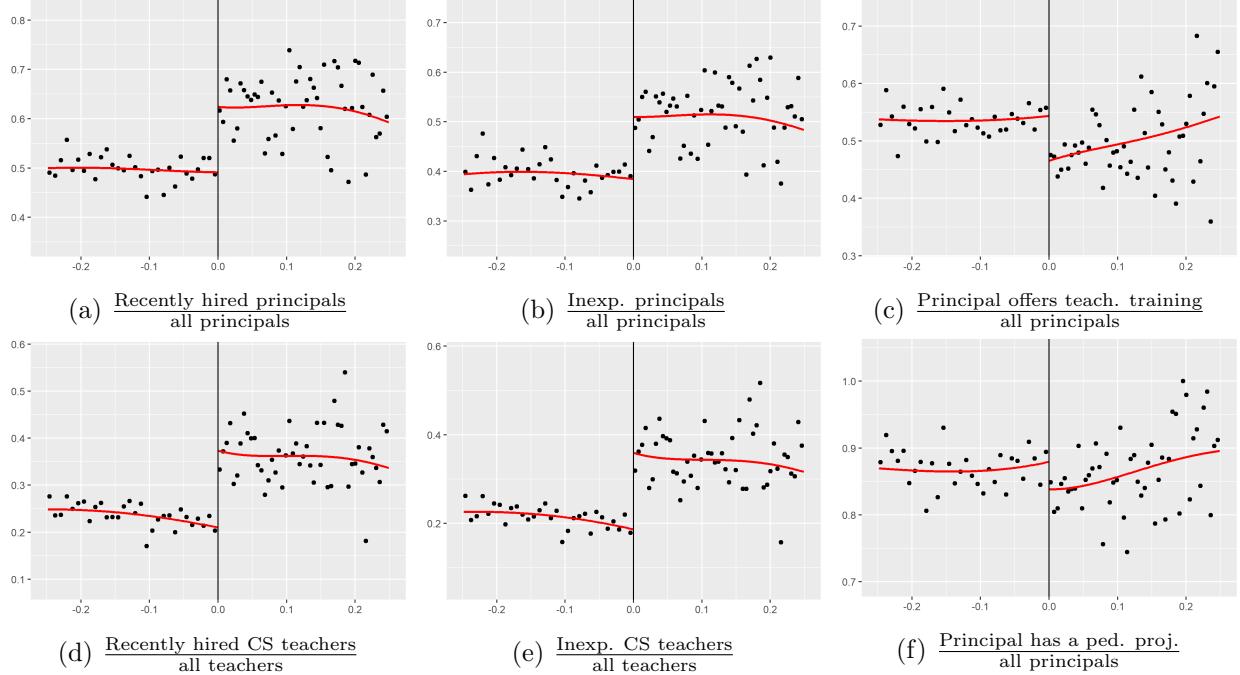


Figure 3: (SAEB) Effects of a minor coalition mayor on public schools in $t + 1$

Consistent with the RAIS estimates, minority mayors increase hiring in both top-tier positions (principals) and downstream positions (civil servant teachers). The estimated increases in the shares of recently hired principals (0.11) and teachers (0.16) are substantial. The regression discontinuity estimates imply that electing a minority-coalition mayor exposes roughly one in six students to an inexperienced teacher and one in nine students to an inexperienced principal.

The observed rise in civil servant hiring is primarily driven by an increase in the recruitment of inexperienced bureaucrats. The estimated increase in the share of new hires is estimated to be almost exactly the same magnitude as the rise in the share of inexperienced workers following the election of a minority mayor. This striking shift in the composition of the public workforce is consistent with the anecdotes of majority buying. 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.

Given this influx of inexperienced labor, one might anticipate potential adverse effects on bureaucratic performance. To assess whether these hiring patterns translate into measurable declines in bureaucratic effectiveness, I examine qualitative evidence from teacher and principal surveys.

Table 4, and panels 3 and 8 present the estimated impact of electing a minority-coalition mayor on bureaucratic performance indicators. The results consistently indicate a persistent decline in bureaucratic performance following the election of a minority mayor and the subsequent hiring of inexperienced bureaucrats. In the year following the election, principals are 10.4 percentage points less likely to offer training to teachers and 5.7 percentage points less likely to have an established plan to manage the school’s educational activities.⁸ 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. These effects are persistent and generally do not attenuate as time passes.

Table 5 and Panel 9 show that standardized test scores decline following the election of a mayor with limited legislative support. In the year immediately after the election, math scores fall by 0.087 and 0.076 standard deviations for fifth- and ninth-grade students, respectively. Although these results are consistent with the deterioration in bureaucratic performance documented in this section, Section 5 demonstrates that the majority-buying mechanism alone cannot account for these patterns, given the heterogeneity in treatment effects.

Interpreting the patterns of the regression discontinuity design

The discontinuities in hiring displayed in Figure 2 follow a consistent pattern: a relatively flat (or mildly sloped) relationship as the cutoff is approached from the left, followed by a sharp positive jump at the threshold and a steep decline that gradually converges to a flat segment on the right-hand side, before sloping upward.

A simple and intuitive explanation accounts for this pattern. The voting margin can be interpreted as a proxy for public support for the minority-coalition candidate. On the left-hand side of the cutoff, where a majority-coalition candidate is elected, non-tenured civil-servant hiring is

⁸In Brazil, the *projeto pedagógico* is a legally mandated document that articulates each school’s educational philosophy, curriculum, and pedagogical practices. It serves as a strategic plan designed to ensure accountability and improve educational quality.

uncorrelated with support for the minority candidate, resulting in a 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.

More specifically, hiring declines at a decreasing rate as minority mayors enjoy broader electoral support, eventually flattening or turning slightly positive. This pattern aligns with the majority-buying mechanism. A more popular mayor is plausibly in a stronger bargaining position, as 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 can demand greater access to bureaucratic appointments as a condition for supporting the new administration. As a mayor's electoral support strengthens, institutional checks and balances may erode. With overwhelming popular mandates, legislative constraints may weaken, enabling mayors to expand the hiring of non-tenured civil servants in the interest of their own political coalition, which would explain the eventual sloping up of the plotted curves.

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. 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 inconclusive evidence in favor of this interpretation. Municipalities ruled by minority

mayors potentially have more students enrolled and open schools within their jurisdictions. It is important to highlight, however, that this increase in public service delivery does not contradict the majority buying mechanism proposed.

5 Second Empirical strategy: Heterogeneous Treatment Effects

Once the causal results are established in the previous section, I turn to analyzing the underlying mechanism. The analysis above provided several pieces of evidence consistent with the interpretation that non-tenured civil servant appointments are strategically allocated to secure legislative support.

First, as shown in Table 2, non-tenured civil servants are the only category of workers for whom hiring increases under a minority government. These positions are highly coveted, and mayors exercise substantial discretion over such appointments. They are typically situated high within the governmental hierarchy and offer considerable benefits,⁹ 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 are circumstantial. To more directly assess whether mayors strategically use 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 most treatment effects on hirings should be larger when the counterfactual mayor would have commanded stronger support in the municipal chamber. To

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

illustrate, consider the following two elections:

2004 Election in Nhandeara (SP): In 2004, mayoral candidate Nelson Magalhães narrowly won the election in Nhandeara by a razor-thin margin of just 0.77 percent of the vote (roughly 51 ballots) over Dr. Odilon. Moreover, his coalition won the largest share of council seats, securing 3 out of 9. This was not enough for a majority, however, as Magalhães would have needed the support of at least 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 largest number of council seats narrowly won the mayoralty. According to the majority-buying mechanism, however, one would expect non-tenured civil-servant hiring to decline more 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 entirely dominated by his rival coalition. In Nhandeara's 2004 election, regardless of whether Magalhães or his opponent, Dr. Odilon, had won, the legislative balance would have remained largely unchanged—any incoming administration would still have needed to negotiate further support to secure a majority. Thus, the majority-buying mechanism predicts stronger treatment 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

Estimating treatment effects separately by the level of counterfactual legislative support that a mayor would have enjoyed poses substantial challenges. In principle, a close-elections design can still provide quasi-experimental variation, but the task 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, identifies only a single local average treatment effect at the cutoff and, by construction, cannot

capture such heterogeneous effects.

A common strategy in the economics literature is to introduce interaction terms. Within a regression framework, the treatment indicator can be interacted with another variable to explore heterogeneity in treatment effects. The main limitation of this approach is that it imposes a specific functional form on the heterogeneity. For instance, if the RD estimate is interacted with a linear term, Equation 4 can be rewritten as:

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

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 strategy¹⁰. 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:

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

As before, $Z_{m,t}$ represents the share of seats of a municipality's major coalition¹¹ of municipality

¹⁰Shortly 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.

¹¹As stated in the introduction, this refers to the coalition with the most seats in the legislative

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 mayoral 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 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:

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

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:

$$w_i = \begin{cases} 1 - \frac{|d_i|}{b_n}, & \text{if } d_i \leq b_n, \\ 0, & \text{if } d_i > b_n, \end{cases} \quad (8)$$

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 use a multidimensional extension of the regression discontinuity design in order to capture heterogeneous treatment effects.

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 dimension-

ality: 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-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

$$w_i = 1 - \frac{|d_i|}{b}, \quad (9)$$

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.

Figure 4 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 monotonic rise in school principal hirings is still driven primarily by the hiring of inexperienced workers.

I interpret these results as strong evidence in support of the majority buying mechanism. The

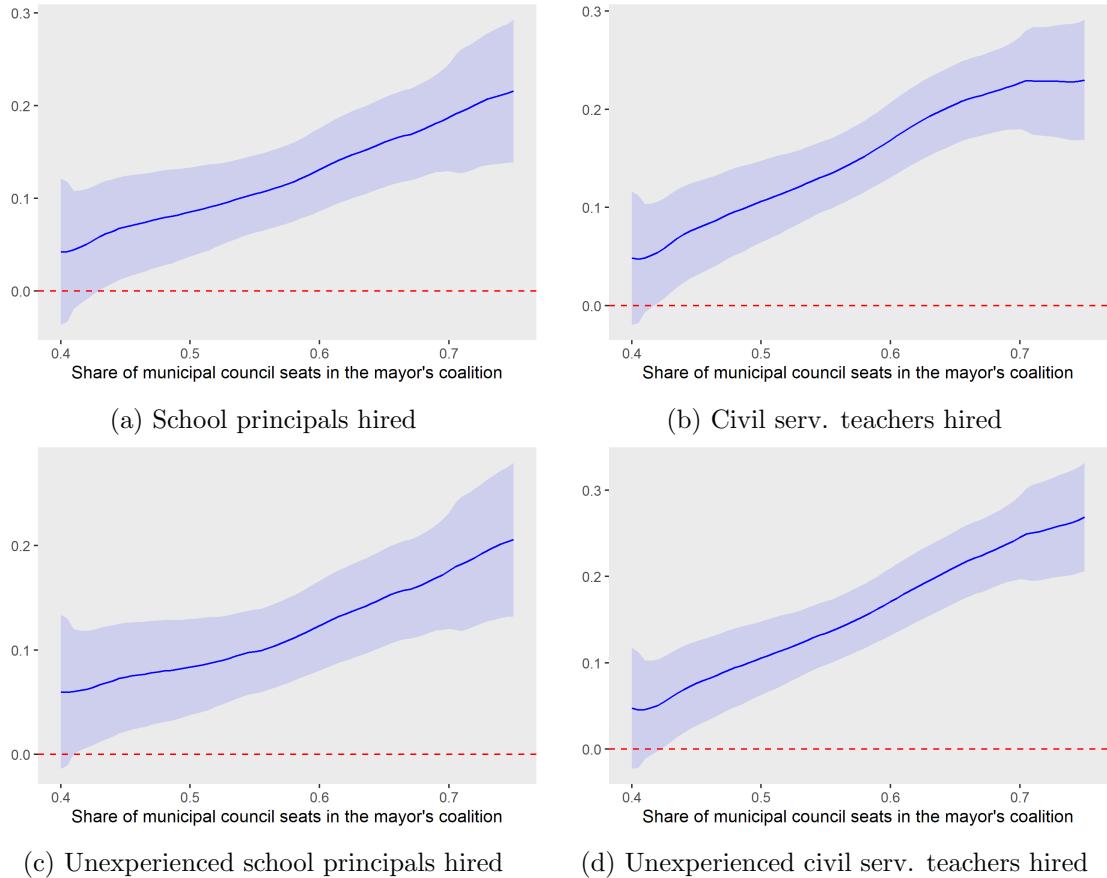
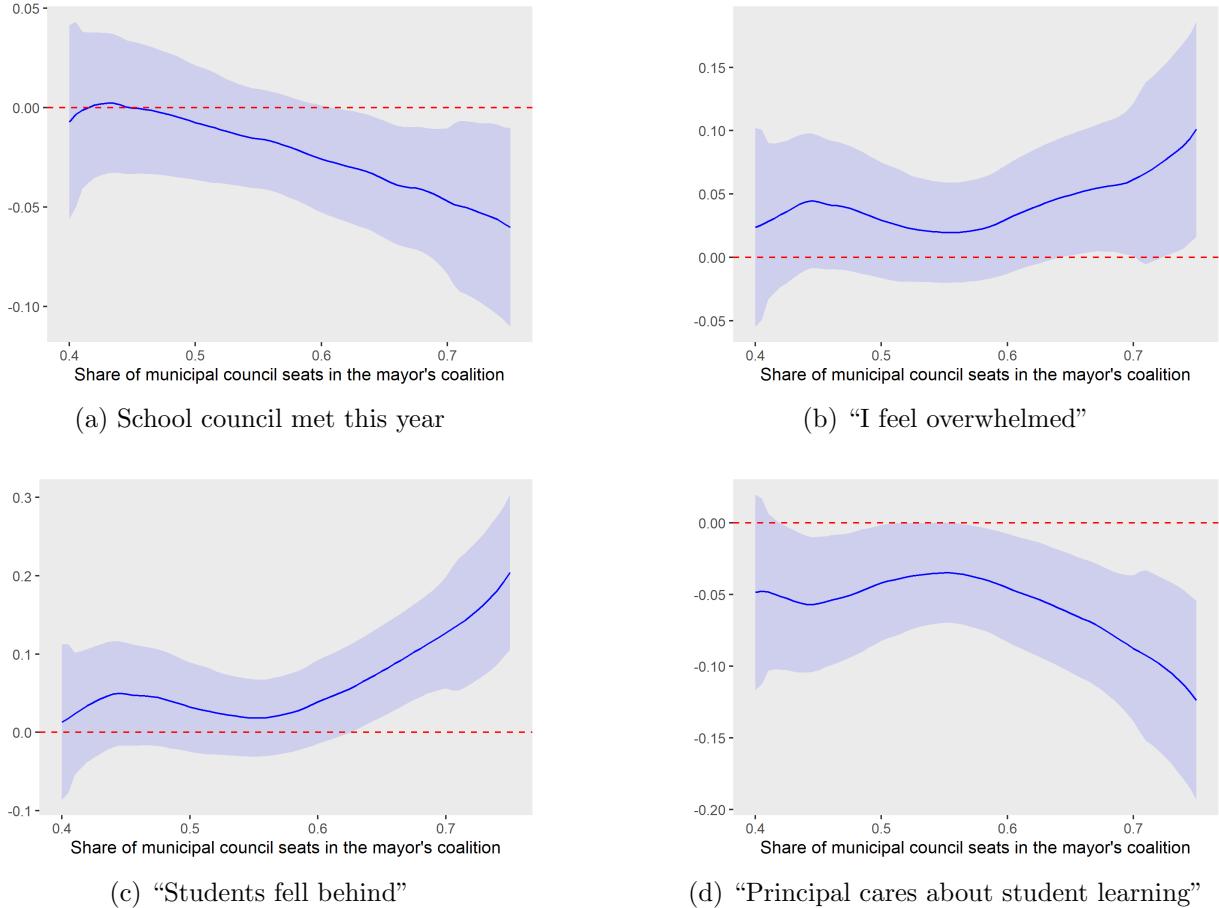


Figure 4: Heterogeneous effects of electing a minor coalition mayor on school hirings ($\sinh^{-1}(\text{counts})$)

observed hiring patterns are broadly consistent with the interpretation that mayors strategically allocate government positions to secure legislative support.

Figure 5: Heterogeneous treatment effects on bureaucratic performance from $t + 1$ to $t + 4$ ($\sinh^{-1}(\text{counts})$)



Panels 5 and 10 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 year"¹²." Comparable patterns emerge for measures of principal performance, though they are less pronounced than those observed for teachers.

The panel in Figure 6, however, paints a more nuanced picture. The findings for school workers

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

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.

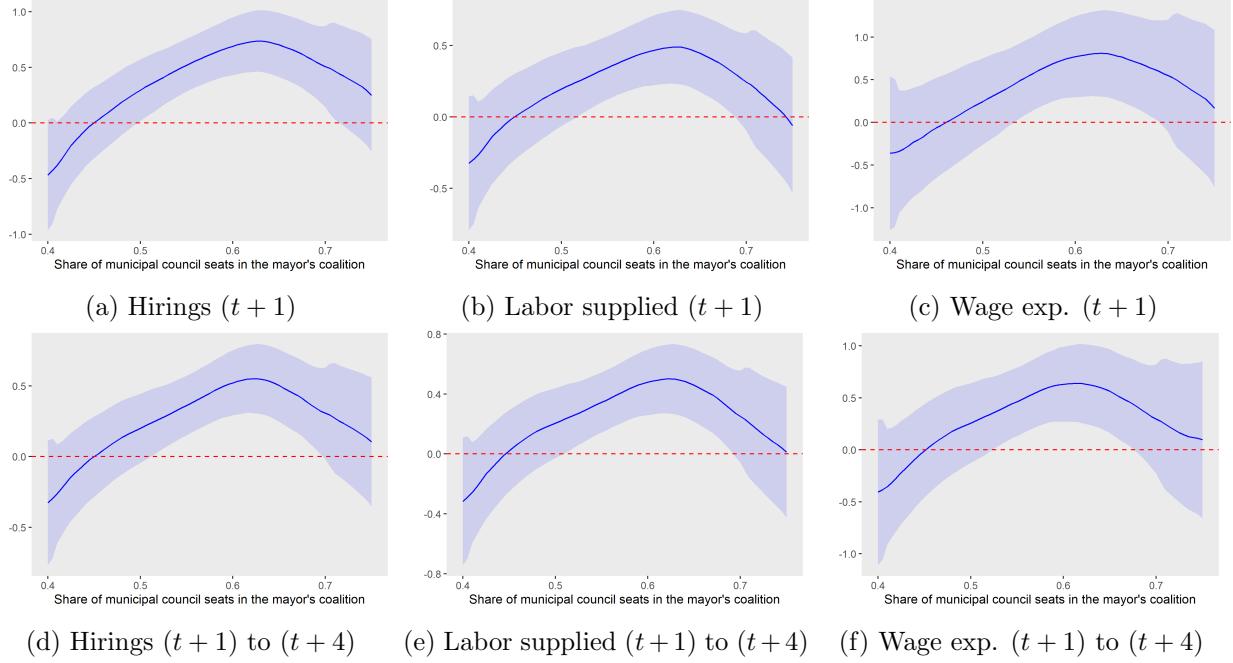


Figure 6: Heterogeneous effects of electing a minor coalition mayor on non-tenured CS hiring

Finally, I extend the analysis to examine the effects of electing a minority-coalition mayor on standardized test scores. Panel 11 reports the results. Similarly to the estimates of RAIS non-tenured civil servant hirings, treatment effects are not monotonic. Instead, 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 and wage expenditure of non-tenured civil servants. Second, I demonstrate that in public schools, the increase in civil servant

hirings is accompanied by a proportional increase in inexperienced civil servant hiring. Third, I show that these inexperienced bureaucratic hirings are accompanied by a persistent decrease in bureaucratic performance indicators.

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 variables—such as GDP, intergovernmental transfers, and education budgets—are unaffected by the election of a minority-coalition mayor.

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

However, the evidence from RAIS data on non-tenured civil-servant hiring and from standardized test scores paints a more nuanced picture. The heterogeneous treatment effects in these cases warrant further investigation to better understand the underlying mechanisms.

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

| | $\sinh^{-1}(\text{number of recently hired})$ | | | $\sinh^{-1}(\text{number of recently fired})$ | | | | |
|----------|---|----------------|---------------|---|---------|----------------|---------------|------------------|
| | Total | Non-ten. CS | Tenured CS | Temp. Workers | Total | Non-ten. CS | Tenured CS | 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 B: 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

| $\sinh^{-1}(\text{counts})$ | | | | | | | | | | |
|-----------------------------|---------------------|----------|----------|----------------------------|-------------------|----------|----------|---------|---------|---------|
| 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

| <i>Share of all municipal reports</i> | | | | | | | | | | | | |
|---|---------------------|-------------------------|-------------------------|----------------------------|-----------------------------------|------------------------------|-------------------------|---------------------------|---|-------------------------------------|--------------------------------------|---------|
| Principals' Survey | | | | | | | Teachers' Survey | | | | | |
| Principals hired | Inexp. princ. hired | Offers teacher training | Sch. has a pedag. proj. | Civil serv. Teachers hired | Inexp. civil serv. teachers hired | School council met this year | "I feel overwhealmed" | "My students fell behind" | "Principal cares about: students' learning" | "Principal cares about: management" | "Principal cares about: maintenance" | |
| Panel A: Short-run ($t + 1$) | | | | | | | | | | | | |
| 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-run ($t + 1$ 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. | Combined | Math | Port. | Combined |
| 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 elections | | | | | | | | | | | |
| 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 incumbent candidate | | | | | | | | | | | |
| 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 incumbent candidate or incumbent 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: Medium 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. running exp.) | Log(munic. educ. exp.) | Log(munic. real GDP) | Log(munic. population) | Log(munic. transfers) |
|----------|---------------------------|-----------------------------|---------------------------|-------------------------|---------------------------|--------------------------|
| 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 |

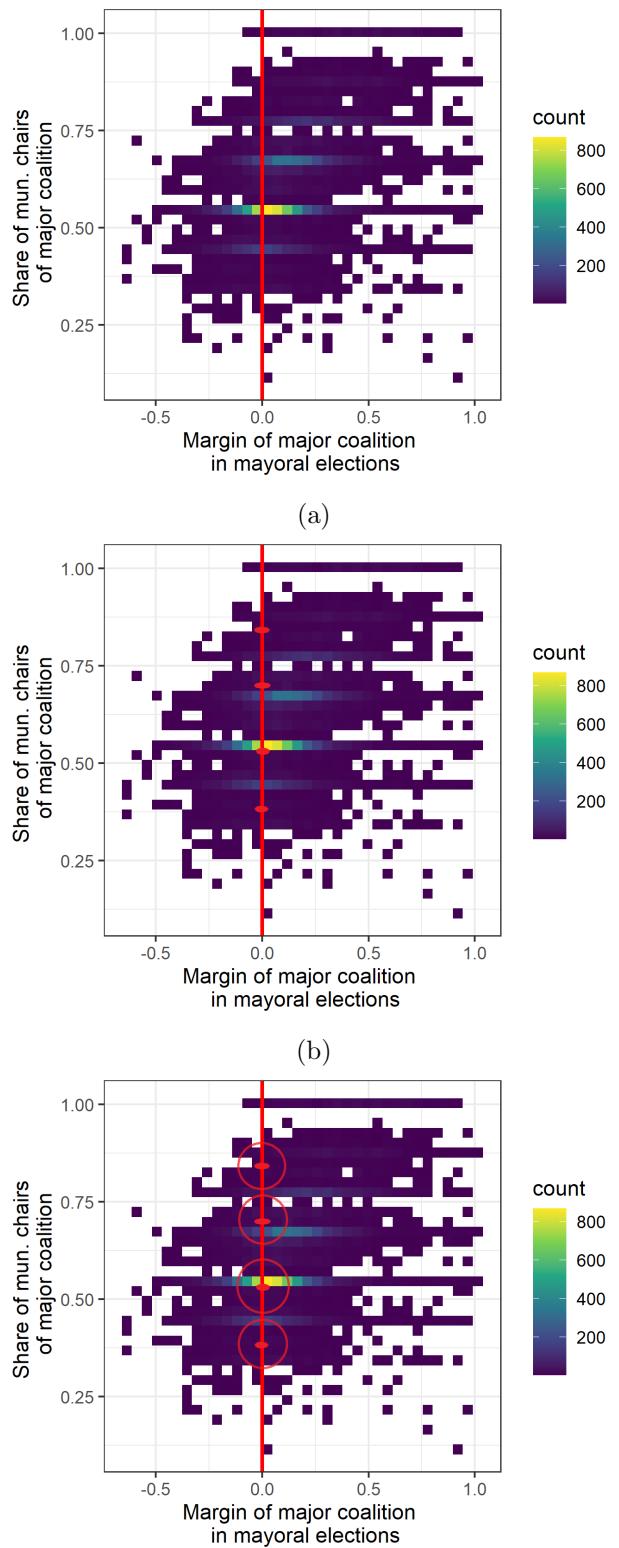
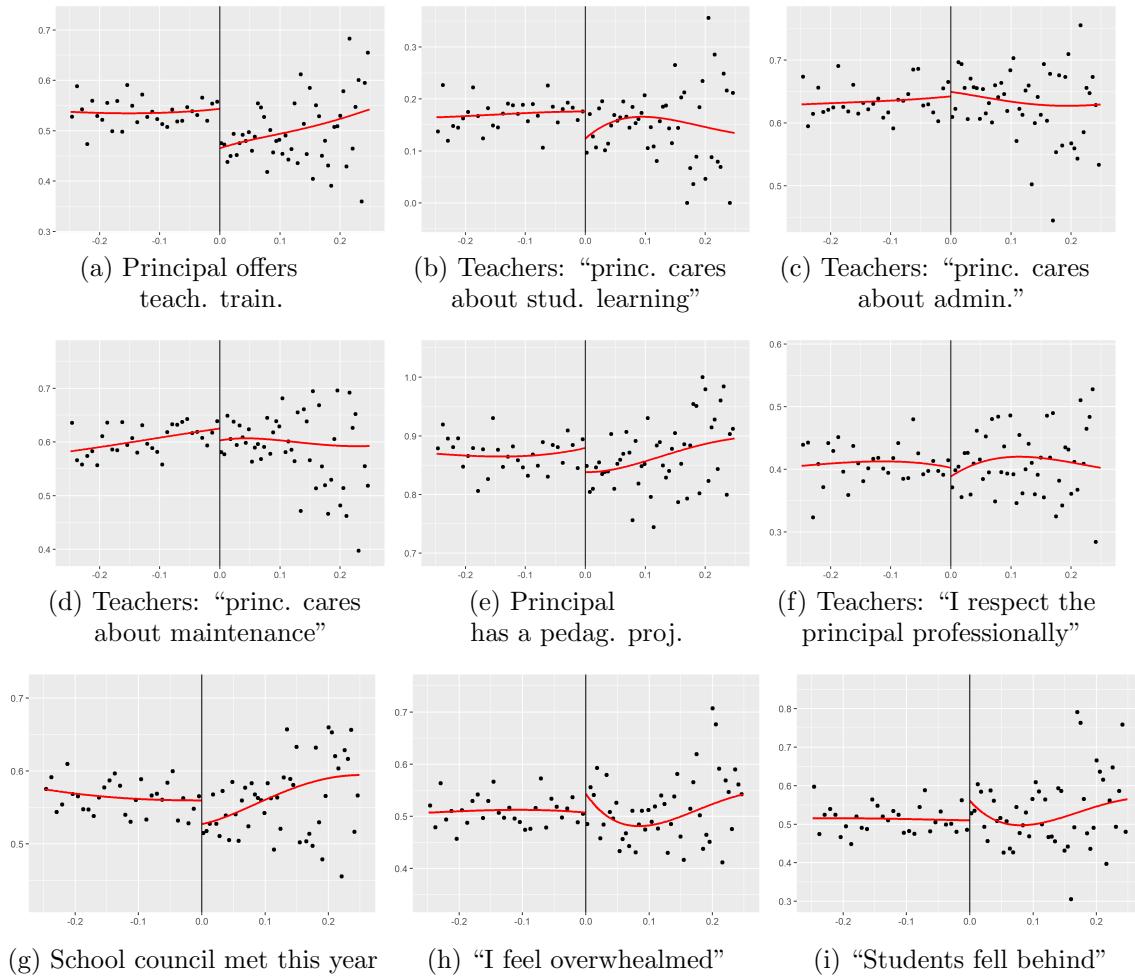


Figure 7: Heterogeneous Treatment effects generalization

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



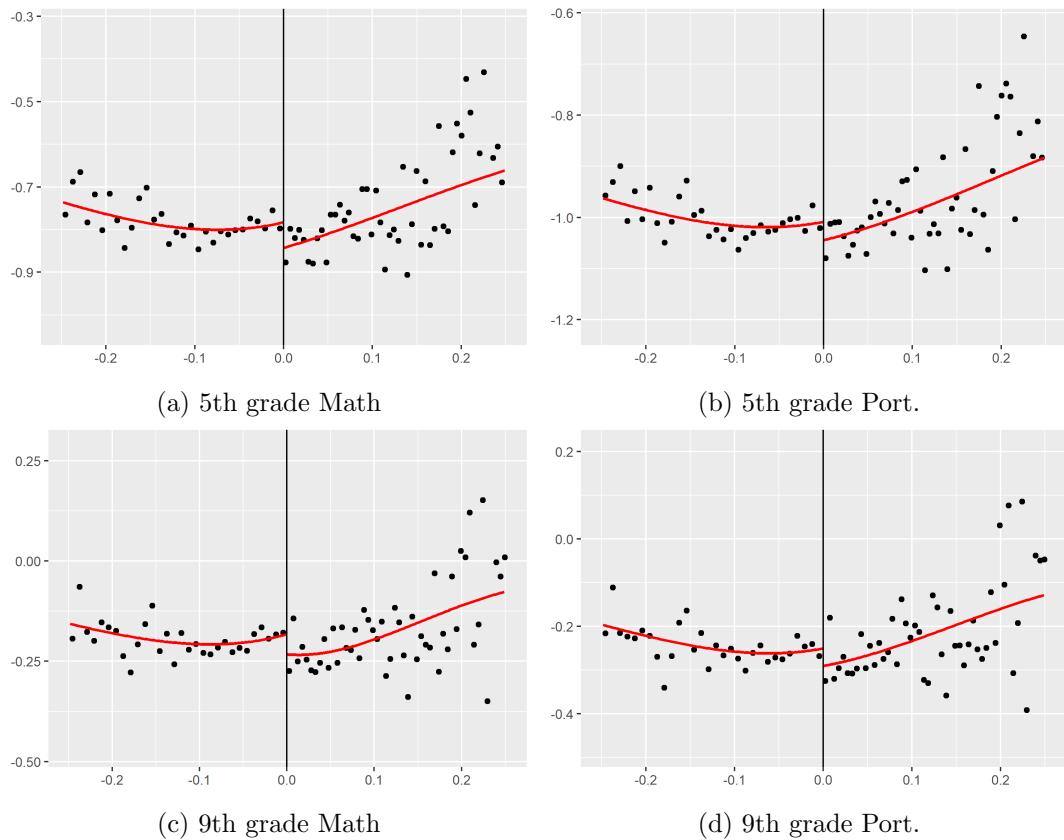


Figure 9: (SAEB) Effects of a minor coalition mayor on public schools' standardized test scores in $t + 1$ (std. deviations)

Figure 10: More heterogeneous effects of electing a minor coalition mayor on bureaucratic performance in the 4 years after the election ($\sinh^{-1}(\text{counts})$)

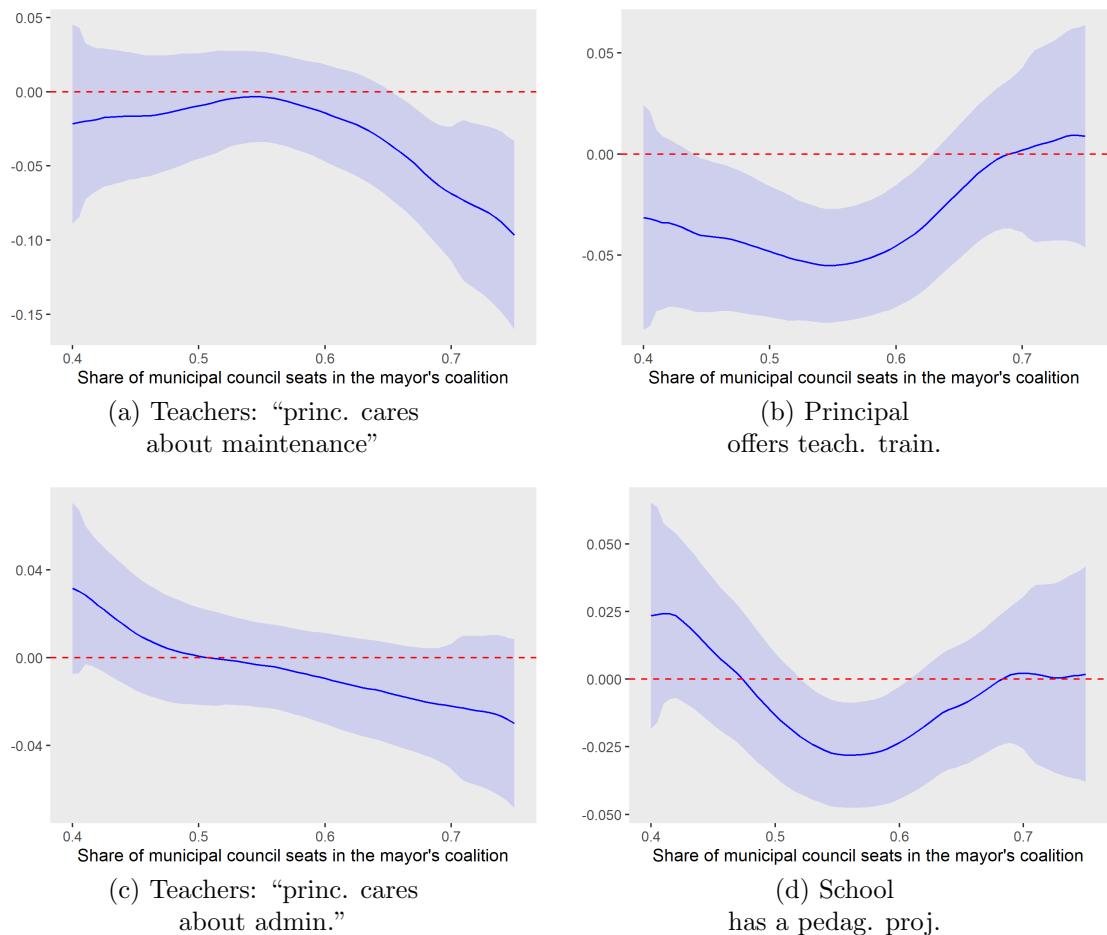


Figure 11: Heterogeneous effects of electing a minor coalition mayor on standardized test scores (std. dev's)

