# The costs of buying a majority government

Plinio Bicalho*

September 2, 2025

**Abstract**

Executive branch representatives must garner support from elected legislative officials to govern. Building a legislative majority is an important stepping stone in most executive-branch mandates. This majority-building process may, however, impose significant costs upon society. Using a regression discontinuity design, I show that municipalities in Brazil whose mayors hold few seats in the municipal chamber experience substantially more turnover in their bureaucracy. RD estimates demonstrate that non-tenured civil servants are hired (+46.7%) and fired (37.5%) at substantially higher rates under minority mayors. This turnover is not confined to high-ranking government positions but extends to roles filled by both skilled and unskilled public servants. Using teacher and school principal surveys, I show that these new hires are generally inexperienced workers who fall short in indicators of job performance. Ultimately, municipalities that elect a mayor with limited legislative support experience a significant drop in standardized test scores (-0.048 to -0.073 std. dev.). Heterogeneous causal estimates are consistent with politicians using government job appointments as bargaining chips to acquire legislative support when their coalitions hold limited seats in the municipal chamber.

*I am especially grateful to Soala Ekine and Ben Marx for our numerous discussions and helpful comments. I also thank John Fallon, Dilip Mookherjee, Martin Fiszbein, Ray Fisman, and Hiroaki Kaido for their helpful comments. Finally, I'd also like to thank the Boston University microeconomics reading group participants for their valuable comments. All errors are my own. Email: pliniob@bu.edu

# 1 Introduction

The principle of separation of powers has profoundly shaped the modern concept of the nation-state. Many democratic nations sought to instill checks and balances into their governments by separating the government into executive, legislative, and judicial branches (De Montesquieu (1989)). This separation demands that branches cooperate to ensure effective governance. In non-parliamentary systems, however, the legislative body and the executive branch need not be aligned. Often, the leader of the executive branch and the bulk of the seats in the legislative will belong to different political coalitions.

This paper investigates how a misalignment between the executive and legislative branches affects the bureaucracy and public service provision. The effects of such a leadership mismatch are theoretically ambiguous. On one hand, when the executive and legislative branches are controlled by different coalitions, this divergence may foster greater political competition (Besley et al. (2010)) and enhance accountability (Jones (2013)). On the other hand, misalignment may lead executive branch leaders to offer coveted government resources to other coalitions in exchange for legislative support.

To examine the effects of an executive-legislative misalignment, I employ a close-election regression discontinuity design. First, I identify municipalities where a coalition has secured a majority of the seats in the municipal chamber. Next, within each of these municipalities, I identify the mayoral candidate aligned with such a majority coalition. Finally, I compare outcomes between municipalities where the majority coalition's mayoral candidate narrowly lost to a minority coalition candidate (treatment) and narrowly won (control). To the best of my knowledge, this is the first study to utilize the margin of victory of the candidate from the majoritarian coalition in a mayoral election as the running variable in a regression discontinuity framework.

I leverage data from three main datasets to implement such a research design. Brazil's electoral court, the *Tribunal Superior Eleitoral*, provides me data on all the municipal elections from the 2004, 2008, 2012, and 2016 electoral cycles. The Ministry of Labor posts yearly the universe of formal employment in Brazil (*RAIS*). From this data, I leverage information on all hirings and firings of workers of municipal executive branches from 2003 to 2020. The Ministry of Education, finally, provides me with data on standardized test scores and principal-teacher surveys conducted every 2 years, from 2007 to 2019. After merging all these datasets at the municipal level, my main dataset consists of 7803 municipal elections in which a mayoral candidate represented a coalition with the majority of the seats in the municipal chamber.

To address potential concerns regarding the manipulation of the running variable, I run the density test proposed by Cattaneo et al. (2018). To further validate my research design, I also show that a range of pre-treatment and baseline covariates are continuous across the RD cutoff. Once the validity of the design is well supported, I establish three facts.

First, I show that electing a mayor from a minority coalition leads to a sharp increase in bureaucratic turnover during a mayor's first year in office. Hirings of high-rank occupations, such as municipal secretaries and public service directors, increase 30.3% due to the election of a minority mayor. Firings and hirings of non-tenured public servants increased by 37.5% and 46.7%, respectively. Notably, this increased turnover tends to be ubiquitous across non-tenured public servant occupations in the public sector. There are large estimated increases in the hiring numbers of accountants, construction workers, dentists, drivers, lawyers, nurses, public health officials, social workers, and teachers, among other occupations. While there are significant effects on non-tenured firings, these are generally smaller than the pronounced increase in hiring.

Second, I show that electing a mayor from the minority coalition leads to hiring inexperienced workers into the bureaucracies and a sharp decline in measures of public worker performance. Using principal and teacher surveys, I show that municipalities run by a mayor from a minority coalition increase the shares of first-time principals and first-time civil servant teachers employed in their bureaucracy by 0.154 and 0.182, respectively. Contemporaneously, measures of worker performance also plummet. On the one hand, principals are less often reported to "care about administration" by their teachers. On the other hand, teachers report more often feeling overwhelmed with work and that their students have fallen behind. Moreover, teachers report less often that they hold meetings to evaluate students' performance.

Third, I show that electing a mayor from the minority coalition leads to a considerable decrease in students' standardized test scores. Over the 4 year mandate of a minor government, 5th-grade students score 0.051 standard deviations less than students governed by a major coalition mayor in a mathematics and Portuguese country-wide test. The estimated impact is slightly higher for 9th-grade students as the test scores are estimated to be 0.066 standard deviations lower than their majority-ruled counterparts.

Finally, I explore the heterogenous treatment effects to test the hypothesis that governments use public sector jobs as a bargaining chip to buy legislative support. The idea behind this exercise is that under the hypothesis that the executive branch is buying a legislative majority by trading public sector jobs, bureaucratic turnover should diminish the more seats a mayor's coalition holds in the municipal chamber.

Under this observation, I propose the following strategy to assess this "majority buying" hypothesis. First, I broadened my sample to include not only municipalities where a coalition clinched a majority of legislative seats but also municipalities in which one coalition achieved a plurality of the legislative seats. I refer to coalitions that achieved a plurality of legislative seats as *major coalitions*. Coalitions running against major coalitions are referred to as *minor coalitions*. Second, I estimate the treatment effects of electing a minor coalition mayor at different levels of counterfactual legislative support.

If the majority buying hypothesis is true, it should be that the treatment effects on public sector turnover will be larger when the elected coalition holds more seats in the municipal legislative. To estimate these

heterogeneous effects, I propose an analogous design to the usual one-dimensional regression discontinuity design. To the best of my knowledge, this is also the first academic paper to propose such a multi-dimensional RDD.

This multidimensional empirical design simply extends the usual RDD to a multi-dimensional setting. For each share $s$ of total municipal legislative seats, I use local linear regression to non-parametrically estimate the impacts of electing a candidate of a minor coalition as opposed to a candidate of a major coalition that holds a share $s \in [0.35, 0.85]$ of seats on the municipal legislative chamber. Section 5 specifies the nuances of exactly how such a multi-dimensional regression discontinuity design was implemented.

I found that the results of this design support the majority buying hypothesis. Namely, electing a mayor with limited seats on the municipal legislative has small and often statistically insignificant effects on measures of bureaucratic turnover. In line with the majority buying hypothesis, however, these effects are progressively exacerbated as the share of seats held by the elected mayor's coalition increases. For instance, electing a mayor whose coalition holds a qualified majority decreases the hiring and firing of municipal secretaries and public sector directors by over 67% and 75%, respectively. The same progressive intensification of treatment effects emerges in the hiring and firing of all public sector employees, hiring of school principals and teachers, and hiring of inexperienced teachers and principals.

This progressivity, however, is less evident for lower-lever public service workers. The progressivity cannot be clearly observed in turnover metrics of total non-tenured civil servants, and standardized test scores. The lack of a consistent trend in these variables of lower-lever public service workers suggests that additional mechanisms may be at play. These mechanisms are discussed in Section 6.

This paper most closely relates to the literature on bureaucracy and public service delivery. Recently, scholars have focused on state capacity as a key element of development (Acemoglu and Robinson (2012)), Besley and Persson (2014), Besley et al. (2022)), putting bureaucracy at the center stage of academic research in political science and development economics. It is well-established that bureaucratic jobs are an important resource used by politicians to reward supporters (Grindle (2012)). Geddes (2023) documents that, anecdotally, rewarding supporters with bureaucratic positions traces back to at least the 1930s in Latin America. Recently, a set of books and papers has documented how exactly the bureaucracy is used to reward political supporters. I consider these papers to be the most closely related academic work to the one presented in this article. Among such papers, some have garnered significant attention from scholars recently. Barbosa and Ferreira (2023), Brollo et al. (2017), and Colonnelli et al. (2020) have shown how, in Brazil, patronage is widely present in public sector employment. They argue that party members, donors, and unelected politicians get rewarded for their support through public-sector employment. Akhtari et al. (2022) shows that political turnover leads to bureaucratic turnover and, potentially, lower standardized test

scores. Xu (2018) uses historical data on the British empire to demonstrate how patronage led to distortions in the allocation of public sector positions and ultimately distorted performance incentives. Iyer and Mani (2012) explore how in India politicians use reassignments to control the bureaucratic body. Brassiolo et al. (2020) show similar usage of public positions for patronage in Ecuador. Altogether, the use of bureaucratic jobs to reward allies seems to be a common occurrence of patronage across countries in history.

The mechanism proposed in this academic paper, however, is not patronage per se. While the tool used for rewarding might be the same (sought-after bureaucratic positions), majority buying and patronage are fundamentally different concepts. A patronage relation between two agents implies the existence of a hierarchy between these two agents. One agent will presumably be the supporter, while the other will be the patron. The process of majority buying is better described as a horizontal quid-pro-quo relation across political coalitions. While one coalition benefits through legislative support, the other gains access to sought-after bureaucratic positions in a transactional manner. This paper differs from the current patronage literature by presenting a new mechanism through which the political process distorts the allocation of bureaucratic jobs.

While patronage seems to be a relevant mechanism in the appointment of bureaucratic workers, there is evidence that political affiliation influences bureaucratic behavior in a more complex fashion. Brollo et al. (2020) shows that politically appointed headmasters influence the distribution of conditional poverty alleviation transfers close to election time. Spenkuch et al. (2023) shows that, in the United States, ideological misalignment between bureaucrats and politicians leads to greater costs, overruns, and delays in procurement contracts. Toral (2024) demonstrates how politicians replace several bureaucratic positions prior to leaving office. It is worth mentioning that there is a wide, mostly theoretical, literature that analyzes bureaucratic structures and how different organization structures affect performance (Ashraf and Bandiera (2018), Besley and Ghatak (2005), Bostashvili and Ujhelyi (2019), Dahlström and Lapuente (2022),Finan et al. (2015), Pepinsky et al. (2017), and Rauch and Evans (2000)). A common theme in this literature is the accountability-autonomy tradeoff when deciding the degree of insulation that government bureaucrats should have.

Finally, I relate my research to a body of literature that highlights the different incentives faced by legislative officials when carrying out their parliamentary functions. Fouirnaies and Hall (2022) find that legislators who can no longer seek reelection exert less effort in their mandate. Kroeger et al. (2017) and Bertrand et al. (2021) show how interest groups, at the very least, attempt to influence legislative voting patterns in the US. Baumgartner et al. (2009), Bertrand et al. (2014), and Drutman (2015) provide insights on which tools interest groups use to attempt to influence legislative officials.

# 2 Institutional Background

Brazil's political system is somewhat standard in Latin America. At the municipal level, the legislative branch is unicameral. All municipal legislative officials (*vereadores*) are elected for 4-year terms simultaneously in the entire country. The number of elected legislative representatives in each municipality is proportional to the its population. Municipal legislative chambers will host between 9 and 55 representatives. Although Brazilians vote for legislative officials and mayors in the same election, these elections are independent of each other. Separate ballots are cast for mayoral and municipal legislative candidates.

Mayoral terms coincide with municipal legislative terms. If a municipality has up to 200,000 registered voters (roughly equivalent to 400,000 citizens) the mayoral election follows a simple plurality rule. The candidate with the most votes will win the election. In bigger municipalities, Brazilians follow a plurality-runoff model. If no candidate achieves a simple majority of votes in the first round, the two best-voted candidates advance to a second-round election.

Any municipality will have a set of registered parties. In a municipal election year, these parties can choose to form coalitions with other parties to run for the mayoral office or to run by themselves. If a coalition is formed, the parties must choose a single candidate to represent the coalition in the mayoral election.

While parties may form coalitions to secure executive positions, they compete independently for legislative seats. Each party fields its own candidates for the municipal legislative chamber. The allocation of municipal legislative seats is determined through a complex proportional representation system. The number of seats a party wins is proportional to the total number of votes it receives. Within each party, these seats are allocated to the candidates who garner the most votes.

# 3 Datasets

My dataset combines 5 datasets collected by the Brazilian federal government. Most datasets are reported at the submunicipal level. Given that the treatment status is assigned at the municipal level, all variables are aggregated at the municipal level for my main analysis. The Data Appendix provides additional details on the aggregation and merging of these datasets.

## 3.1 TSE

The Brazilian electoral court (*Tribunal Superior Eleitoral*) organizes municipal elections in Brazil every 4 years. Since 1998, it has published data on the votes cast, coalitions formed, and victorious candidates for

every election year. These data allow me to identify mayoral winners and losers of any municipal election. Furthermore, it allows me to identify the share of seats in the legislative associated to each coalition formed in mayoral elections. The data is reported at the submunicipal (electoral-zone) level.

To match the data availability of the RAIS and SAEB datasets, I focus on data regarding the municipal election between 2004 and 2016.

## 3.2 RAIS

The Brazilian Ministry of Labor gathers yearly reports on formal employment in Brazil. At the end of the year, any private or public organization must send reports to the Ministry of Labor regarding contracts that were active at some point during that year. These data include information on how many workers an organization had in a given year, whether they were hired in that year, whether they were fired in that year, how many hours/months that worker was hired to work in that organization, whether a worker is a civil servant, and whether that civil servant has tenure. The dataset also allows me to identify different worker occupations. Each contract is associated with a particular occupation[1]. I can, therefore, derive variables at the municipal-occupation level.

Finally, I focus my analysis on organizations affiliated to the municipal executive branch[2]. The dataset is reported at the worker-contract level. Therefore, it must be aggregated for my empirical analysis. The data is aggregated to compute municipal turnover by simply using hirings and firings counts of organizations associated to that municipality. Effective labor supply is normalized so that a unit of labor supplied represents the equivalent of a 40 hours/week worker hired for 12 months in a year.

Even though the RAIS data has been collected since the 1980s, it has changed substantially over time. To avoid comparing different RAIS reports, I focus on the reports between 2003 and 2020. The dataset has remained virtually constant in this timeframe.

## 3.3 SAEB and Censo Escolar

In between the years of 2007 and 2023, the Ministry of Education conducted standardized tests and questionnaires with school workers to assess the quality of public education in Brazil. These standardized tests and questionnaires are jointly referred to as the *SAEB evaluations*. Every two years, 5th and 9th grade students took nation-wide standardized tests in Portuguese and Mathematics. Simultaneously, teachers and principals answered questionnaires regarding working conditions and performance. These evaluations are a census for all schools with over 20 students registered in the 5th or 9th grade in that year. A sample of

[1] identified through the Classification of Brazilian occupations (CBO)
[2] Natureza Jurídica 103-1

schools that have at least 10 students registered in the 5th or 9th grade in that year[3].

No aggregation is needed for standardized test scores. Based on students' performance in standardized tests, the Ministry of Education assigns representative average scores for each municipality's 5th and 9th grade public education. Principals' and teachers' questionnaire answers are aggregated at the municipality level, according to their school's municipality.

The school census (*Censo Escolar*) is a related dataset I rely on. Yearly, the Ministry of Education compiles a dataset regarding the housekeeping of public schools. It provides me data on how many teachers, students, and schools exist in any given year in Brazil. Moreover, it has self-reported basic information on the schools' infrastructure (access to clean water, access to a sewage system, access to electricity,...).

I harmonized and used data on the questionnaires and test scores for the years of 2007-2019. In 2021, the SAEB evaluation underwent profound changes, which made it impossible to harmonize across years.

### 3.4 IPEA

The Institute of Applied Economic Research (IPEA) is a governmental agency that collects economic data in Brazil. It provides me municipal-level variables regarding public finances, population, and GDP for the years of 2003-2020.

### 3.5 Sample selection

For my empirical analysis, four important changes were made to my dataset. First, section 4 considers only elections in which a coalition managed to clinch a majority in the municipal legislative[4]. Second, a non-trivial number of elections are annulled and redone every election year. Generally, elections are annulled if there are issues related to vote buying and illegal candidacies. To avoid issues related to these elections, I simply excluded elections that were annulled from my final sample. Third, to avoid issues related to the plurality-runoff model detailed in Section 2, I considered only municipalities with up to 200,000 registered voters (roughly equivalent to 400,000 citizens).

Fourth, and most importantly, the manipulation test proposed by Cattaneo et al. (2018) provides substantial evidence of manipulation around the cutoff for very small municipalities. Namely, it seems that in very small cities candidates of the majoritarian coalition disproportionally manage to find themselves elected around the cutoff. This discontinuity is particularly strong for municipalities with less than 7,000 citizens. Therefore, my final sample excludes municipalities with less than 10,000 inhabitants at the beginning of the

[3] 2007 is exceptional. Schools located in rural areas were not taken into account for the censitary part of the SAEB evaluation
[4] This requirement is dropped for section 5

election year. I emphasize that the results are robust to including all municipalities in my final sample. The choice of excluding small municipality purely relates to concerns of manipulation around the cutoff.

I discuss potential sources of this apparent discontinuity in section 4, and how it need not necessarily relate to explicit voter fraud. The final sample of section 4 contains data on the 4 election cycles that happened between 2004 and 2016, and it includes 7,803 municipal elections. 2,954 out of Brazil's 5,569 municipalities are represented in at least one of the election cycles considered. The final sample of section 5 contains data on 9,520 elections in which 3,037 municipalities are represented.

# 4 First Empirical Strategy

This section aims to estimate the effects of electing a minority government. Every municipality in my final sample had one mayoral candidate whose coalition achieved at least a simple majority in the municipal legislative chamber. In this context, I aim to estimate the causal impact of electing a mayor from one of the minority coalitions relative to a majority coalition mayor. To do so, I rely on a sharp regression discontinuity design.

## 4.1 Outcome Variables

First, many of the variables considered are municipal hiring and firing counts and, hence, they contain many zeros and are highly skewed. Following the literature, I propose the following transformation to count variables:

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

This inverse hyperbolic sine transformation allows me to interpret point estimates as approximate percentage changes. Furthermore, it helps with asymptotic approximations by correcting for the right skewness that often plagues count variables.

Second, I follow in the appendix Marx et al. (2022) and further standardize outcome variables. For every outcome variable $Y$ defined for municipality $m$ and election year $t$, I apply the following transformations:

$$\Delta Y_{m,t}^{short-run} = Y_{m,t+1} - Y_{m,t-1} \tag{2}$$

$$\Delta Y_{m,t}^{full-cycle} = \frac{1}{4}\left(\sum_{\tau=1}^{4} Y_{m,t+\tau}\right) - Y_{m,t-1} \tag{3}$$

Results following this formulation are generally stronger than the baseline results. The variable con-

structed in equation 2 is designed to capture the immediate effects of electing a mayor from a minority coalition. On the other hand, the variable constructed through equation 3 captures the average effect of electing a mayor from a minor coalition over an entire election cycle. In both types of outcome variables, I subtract a municipality's outcome variable baseline value $Y_{m,t-1}$. This subtraction increases the precision of my estimates as it allows me to control for different baseline variable values across the treatment and control groups. Such a specification has gained popularity in RD studies as standard errors are generally smaller relative to a canonical RD design.

Finally, an analogous *full-cycle* variable is derived for SAEB variables using years for which SAEB is available. SAEB outcome variables are only available for periods $t-1, t+1$, and $t+3$ for any election year $t$. Moreover, 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.

## 4.2 Regression discontinuity estimation

I rely on a close election design and the procedure proposed by Calonico et al. (2014) to non-parametrically estimate the effects of electing a mayor from a minority coalition relative to the candidate from the majority coalition. The design is run at the municipality-election year level $(m, t)$. The regression discontinuity design is implemented through the following equation:

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

where $X_{m,t}$ is the 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 for whether a minority coalition candidate has won the mayoral election of year $t$ and municipality $m$. $\Delta Y^*_{m,t}$ is an arbitrary outcome variable after I've applied the transformations described in the previous subsection. For every variable, I report the bias-corrected point estimate $\gamma$, the robust standard error, and the p-value associated with the robust confidence interval of $\gamma$.

The regression discontinuity design allows me to uncover the true effect of having a misalignment between the leadership of the executive and legislative branches around the cutoff $c = 0$. While the design does not guarantee external validity of the estimates away from the cutoff, I see it as the closest feasible design to an ideal experiment. To guarantee point estimates are generalizable, an ideal experiment would randomly assign municipalities mayors from the majority coalition (control) or mayors from a minority coalition (treatment) and compare outcomes following such an assignment.

## 4.3 Validity Checks

Any implementation of an RDD raises concerns regarding manipulations of the running variable. In this case, my main concern is that mayoral candidates who belong to the majority coalition are able to manipulate the running variable to find themselves disproportionally on the winning side of close elections. Indeed, there seems to be strong evidence that this is the case. Using the manipulation test proposed by Cattaneo et al. (2018), I find sufficient evidence (p-value of 0.04) that an unusually large number of mayoral candidates from the majority coalitions win municipal elections. I find that this discontinuity in the density of my running variable is driven by small municipalities (less than 10,000 inhabitants). The estimated manipulation is particularly strong in municipalities with populations of 10,000 or fewer. Even though explicit voter fraud seems appealing as a potential explanation, it need not be. It is possible that in these very small municipalities, popular coalitions manage to sway close elections in their favor by manipulating voter behavior[5].

My sample selection deals with such a potential manipulation by focusing on municipalities with a population of at least 10,000 at the beginning of the election year considered. By doing so, the p-value under the null hypothesis ($H_0 : p = 0.5$) sky-rockets to 0.7571. Although I effectively drop municipalities with a population of less than 10,000, I must emphasize that the results are robust to the inclusion of such municipalities.

Finally, I show in the appendix that there are no apparent discontinuities in baseline covariates. Furthermore, I selected key outcome variables (high-rank public sector worker turnover variables, principal and non-tenured teacher turnover variables, average test scores, etc.) and show that there are no apparent discontinuities around the design's cutoff in the year prior to the mayoral election.

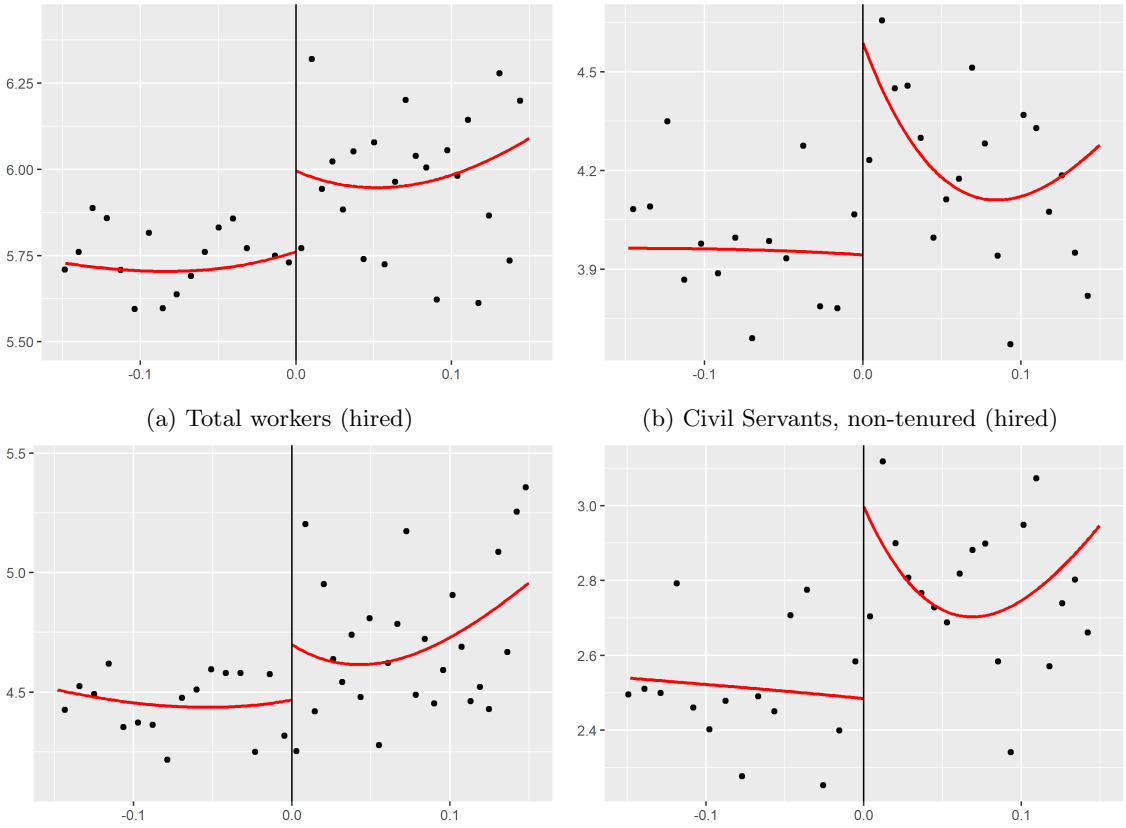
## 4.4 First Empirical Strategy: Results

I, first, present the RD treatment effect estimates ($\gamma$) estimated through equation (4) for government positions. Table 1 and panel 1 show how misalignments between the executive and legislative branches affect turnover across different types of government workers.

The results provide a clear picture: electing a mayor from the minority coalition leads to a substantial increase in bureaucratic turnover. The estimated increase in overall hirings was roughly 24.5%. Such an increase has been driven by the considerable increase in turnover for non-tenured civil servants. The number of non-tenured civil servants hired and fired increased by roughly 46.7% and 37.5% at the cutoff.

Once I established that a misalignment between the executive and legislative branches leads to an increase in bureaucratic turnover, I decomposed these effects by different occupations. This decomposition can be

[5]Vote buying, for instance, is anecdotally still present in small municipalities nationwide. As of 2024, over 300 cases of vote buying were under investigation by Brazil's federal police.




(a) Total workers (hired)

(b) Civil Servants, non-tenured (hired)

(c) Total workers (fired)

(d) Civil Servants, non-tenured (fired)

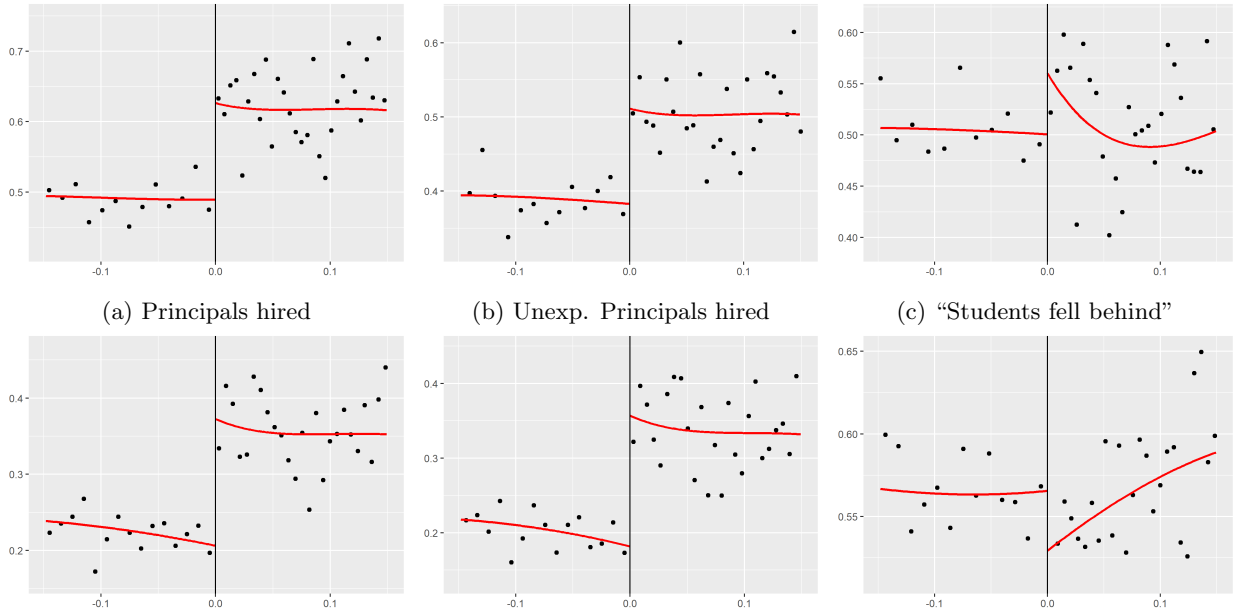
Figure 1: (RAIS) Hiring and firing patterns in the year after the election by contract type ($sinh^{-1}(counts)$)

seen in tables 2, and 3 and in panels 5 and 6.

Regression discontinuity design estimates show how the turnover induced by electing a mayor from the minor coalition seems ubiquitous across various occupations. If the mayor does not have a majority in the legislative chamber, the executive branch hires more lawyers[6] (+18.4%), public sector heads[7] (+30.3%).

Perhaps more surprisingly, such an increase in hiring can also be seen across a wide variety of occupations in the public sector. If the mayor is running a minor government, occupations associated with skilled labor, such as nurses (+30.1%), social workers (+14.4%), and teachers (+32.5%) experience a sharp increase in non-tenured civil-servant hiring in the year following an election. Moreover, occupations associated with unskilled labor, such as drivers (+36.2%), office assistants (+26.8%), and security guards (+49.5%) are hired more often under a minority municipal government.

Given the measured increased in municipal turnover, I turn my attention to the characteristics of the new hirees. The SAEB provides in-depth information regarding all public school principals and teachers. Table 4 and panel 2 provide an in-depth overview of how electing a mayor from a minority coalition impacts public schools.



(a) Principals hired (b) Unexp. Principals hired (c) "Students fell behind"

(d) Civil serv. teachers hired (e) Unexp. Civil serv. teachers hired (f) School council met this year

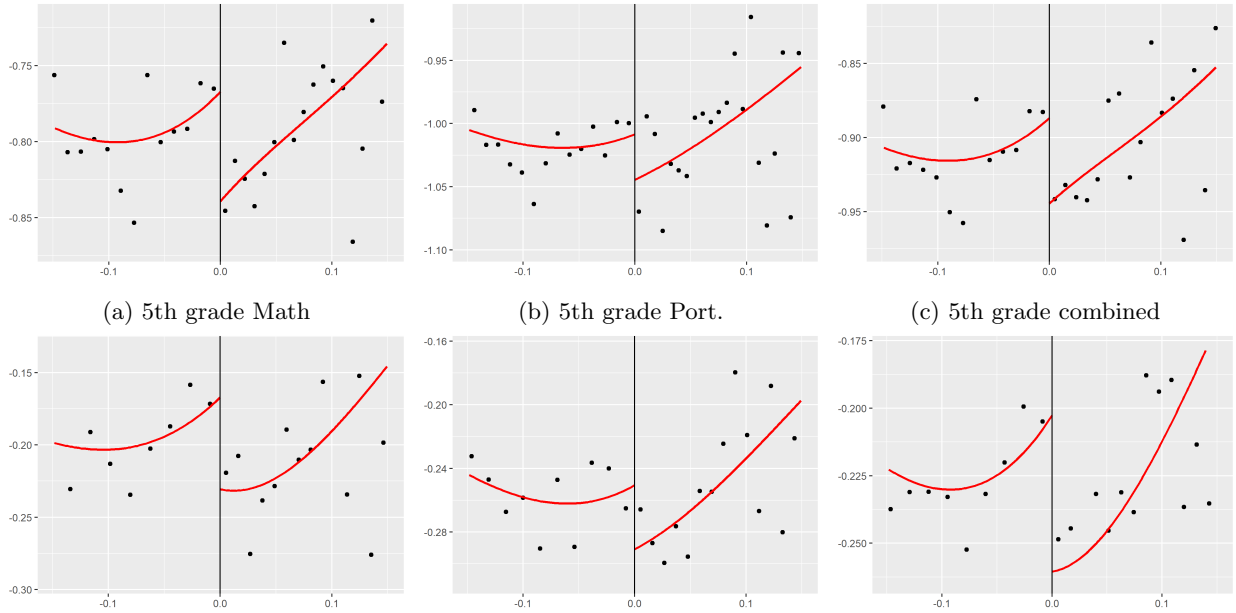
Figure 2: (SAEB) Effects of a minor coalition mayor on public schools in the year after an election (share of total employment)

Similar effects can be observed across teachers and school principals. Similar to the RAIS estimates, I demonstrate with the SAEB dataset that electing a mayor from a minority coalition induces bureaucratic

[6]tipically municipal prosecutors, public defendants, consultants, etc
[7]typically municipal secretaries, public service coordinators, and public service directors

turnover. A mayor from a minor coalition increases hiring in both top-tier (school principals) and downstream (civil servant teachers) government positions. Moreover, the increased turnover is explained by a sharp increase in the number of 1st-time teachers and 1st time-principals hired. Table 4 shows how the estimated increase in the share of recently hired principals (0.172) and civil servant teachers (0.18) is almost exactly the same as the estimated increase in the share of inexperienced principals (0.154) and inexperienced civil servant teachers (0.182). The magnitude of these treatment effects shouldn't be brushed over. The RD design estimates that electing a minority coalition mayor causes roughly 1 out of 6 schools to be exposed to an unexperienced principal/teacher.

This remarkable increase in inexperienced labor hiring is in line with the *majority buying hypothesis*. If sought-after public sector jobs are used as a bargaining chip by the mayor to gain support from other coalitions, one would presume these jobs would be filled by workers who would otherwise have a hard time getting hired on their own. Therefore, one would expect to see an increase in the hiring number of inexperienced and otherwise unqualified workers if mayors are using these job appointments to gain legislative support.



(a) 5th grade Math (b) 5th grade Port. (c) 5th grade combined

(d) 9th grade Math (e) 9th grade Port. (f) 9th grade combined

Figure 3: (SAEB) Effects of a minor coalition mayor on public schools standardized test scores (std. deviations)

Finally, I analyze the impact of electing a minority coalition mayor on worker performance indicators and student's test scores. Table 4 paints a clear picture. In line with the hiring of inexperienced workers, worker performance substantially worsens in municipalities ruled by a mayor that does not hold a majority

in the legislative chamber.

Principals in treated municipalities perform considerably worse than their control counterparts. Their schools are less likely to have a pedagogical plan, while teachers report less often that principals care about the school's administration and school's maintenance. Moreover, a smaller share of school teachers report that they "respect the principal professionally" and that "the principal cares about students' learning". Similarly, teachers report less having a school council[8] and report more often that their students have fallen behind.

Ultimately, in line with the previous results, table 5 shows how municipalities ruled by minor coalition mayors perform significantly worse in standardized test scores. Panel 3 visually depicts the magnitude of such causal effects. Treatment effects can be observed across both 5th and 9th grades. Treated municipalities around the cutoff experience a decrease of 0.065 and 0.07 standard deviations for 5th and 9th grades, respectively, in the year following an election.

### 4.5 Interpretation of the shape on the regression discontinuity

The turnover discontinuities in panels 1, 5 and 6 generally follow the same shape. A constant (sometimes mildly sloped) function approaches the cutoff from the left-hand side, followed by a positive discontinuity and a sharply negatively sloped function that converges to a constant on the right-hand side.

I propose a simple and intuitive explanation for such a discontinuity shape. Here the voting margin is better understood as a measure of the public's support for the mayor of the minority coalition. It is, therefore, intuitive that the left-hand side estimated function is seemingly constant. If a municipality elects a mayor from the majority coalition, there is no correlation between bureaucratic turnover and voters' support for the minority coalition candidate. As soon as the minority coalition candidate wins the election, however, the amount of popular support they have becomes relevant.

More specifically, bureaucratic turnover decreases at a decreasing rate the more popular support the elected mayor has. As the mayor's popularity increases, eventually turnover flattens or becomes slightly positive. Such a pattern can be easily explained under the *majority buying hypothesis*. Under this hypothesis, minority coalition mayors try to gain legislative support by offering coveted bureaucratic positions to other parties. It seems reasonable to assume a more popular mayor would find it easier to negotiate legislative support. To the extent that politicians and parties aim to gather popular support, vote shares capture an important asset for the mayor when bargaining with municipal legislators. On the flip side, if a minority coalition mayor was elected in a coin-toss election against a candidate from the majority coalition, legislators

[8] regular meetings with other teachers and parents

from the majority coalition can demand more bureaucratic positions in order to align themselves with the polarizing minority government.

### 4.6 Alternative mechanisms

Although the evidence seems to prima facie suggest that mayors use bureaucratic positions to gain legislative support, it is important to consider alternative explanations to the results presented in this section. I consider three alternatives to the majority buying hypothesis.

First, it could be that the true underlying mechanism involves political turnover. As established by Akhtari et al. (2022), political turnover leads to bureaucratic turnover. To the extent that incumbent mayors can plausibly elect more municipal legislators, it is reasonable to speculate that my results are merely driven by political turnover. The election of a minority government would simply be a proxy for incumbency overturning. I show in table 6 how this is not the case. Not only turnover effects still exist in elections where incumbency is not at stake but measured treatment effects are substantially greater. Effects of electing a minority mayor on the number of firings of non-tenured public servants, for instance, rises from 37.5% to 68.9% in elections without an incumbent candidate. The effects are even larger at 108.7% in election without an incumbent mayor or an incumbent party.

Second, it is possible that bureaucratic hiring and performance decrease not due to turnover per se, but a general expansion of public service delivery in municipalities ran by a minority coalition mayor. Although some of these concerns can be dismissed by observing the sizable increase in bureaucratic firings in table 1, I provide further evidence that public services are not expanded following the election of a minority mayor in table 7. The table is no evidence that the number of open schools, registered students, teacher on payroll or even measures of school infrastructure are affected by the treatment.

Third, I consider the possibility that changes in the municipal budget, GDP, or population causes such changes. Table 8 shows none of these alternative explanations are supported by a RD design. In the election cycle and in the year following an election, there is no measurable impact of electing a minority coalition mayor on population, GDP, intergovernmental transfers, a government's labor expenditure, or a government's education expenditure.

An argument can be made that the results are partially driven by corruption. In table 7, I find some evidence for such a claim. The share of schools that report insufficient infrastructure or insufficient funds increases in municipalities ran by a minority coalition mayor. These evidence, however, need not capture corruption. Novice principals and teachers likely struggle to manage school resources. The increase reporting of insufficient resources can, therefore, be explained by the mismanagement of schools induced by the

increased turnover and hiring of unexperienced school workers in table 4.

# 5 Second Empirical strategy: Heterogeneous Treatment Effects

Once causal results are established in the previous section, I turn my attention to the underlying mechanism. The previous section presented 3 pieces of evidence consistent with the use of bureaucratic jobs to gain legislative support. First, the fact that virtually all school bureaucratic turnover in panel 2 seems to come from the hiring of inexperienced workers, suggests public sector appointments are selecting bureaucrats who would otherwise have a hard time obtaining a government job through their own curriculum. Second, the recurring shape of the discontinuity across graphs that capture bureaucratic turnover suggests turnover effects fade away the more voter support the elected mayor has. These estimates are in line with the majority buying hypothesis if a mayor's popularity is an asset when negotiating local legislative support. Finally, estimated effects on students' standardized test scores and the performance of school teachers and principals suggest hiring done by mayors without legislative support does not have the public's interest at heart. Although all of these three facts support the majority buying hypothesis, I consider this evidence circumstantial. To assess more decisively whether mayors use public sector appointments to gain legislative support I suggest one final test.

This final test relies on a simple observation: if mayors use bureaucratic jobs to gain legislative support, turnover effects should be greater when the counterfactual mayor enjoys greater support from the legislative chamber. Two cases are presented below to illustrate this point:

**2004 Election in Nhandeara:** In 2004, mayoral candidate Nelson Magalhaes barely lost the election in Nhandeara (SP). In a coin-toss election, Nelson won by a margin of roughly 0.77% of total votes (only around 51 votes). His coalition, however, still managed to elect the most seats of the municipal council out of all coalitions. They fell short of a simple majority, however, as they only won 3 out of 9 possible seats.
**2008 election in Sooterama:** In 2008, the municipality of Sooterama (ES) mayoral candidate Joana Rangel the election by a mesmerizing number of 73 votes. Her coalition not only managed to clinch a majority, it won every seat in the municipal legislative chamber. Her coalition won 9 out of the 9 seats in her municipality.

In both cases, the coalition with the most seats in the legislative chamber barely won the mayoral election. If the majority buying hypothesis is true, one would expect that the reduction in bureaucratic turnover were the largest, however, in Sooterama. Had Joana lost the 2008 election not only would the runner-up, Esmael Loureiro, have established a majority in the municipal legislative, he would have had no

legislative representative in his municipal chamber. On the other hand, even after clinching a legislative plurality, mayor Nelson Magalhaes still had to negotiate the support of 2 more legislative officials to run a simple majority in the municipal chamber. Electing runner-up Dr. Odilon, therefore, would have had limited impact on bureaucratic turnover in the city of Nhandeara if the majority buying hypothesis is true.

Based on the thought experiment above, I propose estimating the treatment effects of electing a major coalition mayor, at different levels of their legislative support. As proposed in the previous paragraph, under the majority buying hypothesis estimated treatment effects should be larger the greater legislative support an elected mayor has.

## 5.1 Second Empirical strategy: Estimation

There are substantial challenges to separately estimating the treatment effects according to the amount of counterfactual legislative support a mayor would have had. 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 treatment effects of electing a minor coalition mayor for every point $p \in [0, 1]$, representing the share of counterfactual legislative support. The original tradition RD design, however, only yields a point estimate and, by definition, cannot be use to explore these heterogeneous treatment effects.

One strategies stand out from the economics literature for this proposed heterogeneity analysis. As in any other regression, one can simply use interaction terms to analyze how treatment effects vary according to a particular variable. The main issue with such an approach is that interaction terms will, necessarily, assume a particular functional form for treatment effects. Suppose the RDD estimates are interacted with a simple linear term. Equation 4 can be written as:

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

$Z_{m,t}$ captures the share of seats of a municipality's major coaltion of municipality $m$ during the election year $t$. This design imposes that the treatment effect varies linearly according to the share of seats held by the major coalition of a municipality, when nothing supports such a functional form.

Although the linear assumption can be relaxed by introducing higher degree polynomials, introducing such terms also impose strong functional form assumptions that may not hold for nth degree polynomial. Furthermore, as the degree of the polynomials increases, one minimizes the bias at the expense of the variance in the bias-variance tradeoff. Estimates, therefore, become less precise.

I propose an alternative estimation strategy. Instead of running a simple RD in a 2-dimensional plane, I propose estimating the treatment effects in a 3-dimensional plane. This 3 dimensional plane consists of the

usual running variable dimension (X), outcome dimension (Y), but it is augmented by a 3rd dimension (Z) that breaks down treatment effects across a particular variable.

First, I define a grid of points $\{p_n\}_{n=1}^{N} \subset [0,1]$ for which I aim to estimate treatment effects. For every single point $(0, p_n)$, the effects of electing a minor coalition mayor as opposed to a mayor with a share $p_n$ of the municipal legislative by estimating the following equation:

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

As before, $Z_{m,t}$ represents the share of seats of a municipality's major coalition[9] of municipality $m$ during the 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 for whether a minor coalition candidate has won the mayoral election of year $t$ and municipality $m$. In order to have a non-parametric estimation, the equation above is ran as a local linear regression. In other words, I only a select few observations $\{(x_i, y_i, z_i)\}$ to estimate equation 6 for a point $(0, p_n)$. Namely, observation $i$ is used to estimate the discontinuity at $(0, p_n)$ if and only if its distance to $(0, p_n)$ is smaller or equal than a bandwidth $b_n$. I measure distance of $d_i$ of an observation using the usual Euclidean distance, which in 2-dimensions collapses to the pythagorean theorem:

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

Finally, to mimic the triangular regression weights used in regression discontinuity settings, each local regression is ran with "conical weights". The weight $w_i$ of an observation $i$:

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

Figure **??** represents how I would leverage information from the observed underlying bivariate distribution $(X_{m,t}, Z_{m,t})$ to non-parametrically estimate treatment effects for 4 different points using illustrative bandwidths. To my knowledge, this is the first paper that proposes such a multideminsional extention to a regression discontinuity design to capture heterogeneous treatment effects.

Extending the usual RD setting to a 2-dimensional space presents two main complications. First, there is no notion of optimal bandwidths for RD designs in economics. While in recent years Calonico et al. (2014)

[9] as stated in the introduction, this refers to the coalition with the most seats in the legislative

became somewhat of a golden standard for bandwidth selection in RD designs, their algorithm is made for the canonical 1-dimensional analysis. Second, by doubling the dimensions of the RD, one needs to worry about the curse of dimensionality. In general, adding a second dimension will likely hinder one's estimation precision. Datapoints naturally become more scattered when analyzed in two-dimensions.

I choose in my main specification a bandwidth of 0.15. As observed in tables 1 to 4, optimal one-dimensional bandwidths are consistently between 0.09-0.12. The choice of 0.15 proposes a minor increase in the local linear regression bandwidth to counteract the curse of dimensionality. As this choice is arbitrary, I show in the appendix that results are generally robust to the use of the bandwidths 0.10 and 0.20. As to mimic the triangular weights of a 1-dimensional RD, I use "conical weights", that is the weight $w_i$ of observation $i$ follows:

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

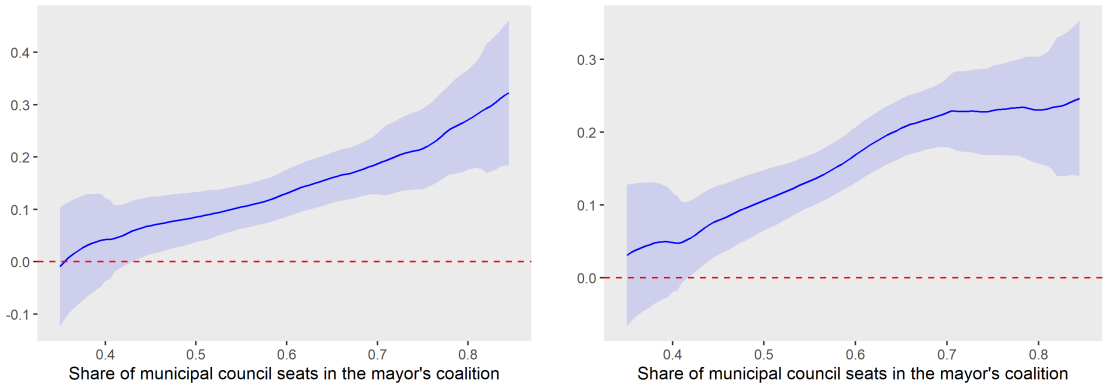
Finally, I restrict my estimates to the points $p_n \in \{0.325, 0.335, 0.345, ..., 0.825\}$. As displayed in the graph **??**, most observation are concentrated between the points 0.325 and 0.825. Point estimates and confidence intervals simply become too volatile outside of this interval. The choice of increments (0.01) can be refined, although it has negligible effect in the results showcased below and simply add computational burden to my analysis.

## 5.2 Second Empirical strategy: Results

I use the continuous RD described to estimate the causal effect of electing a minor government relative to a plural government with level of support $p_n$. These effects are captured by the parameter $\gamma_0$ in equation 6. The confidence interval derived uses the robust standard errors attached to such a coefficient. The first, and likely, most important set of outcome variables used in this strategy relate to bureaucratic turnover.

Figures 4 and 8 illustrate the heterogeneous effects of electing a minor coalition mayor. Overall, the effects appear broadly monotonic with respect to the share of seats held by the counterfactual mayor. Specifically, the increase in bureaucratic turnover associated with electing a minor coalition mayor is more pronounced when the counterfactual mayor would have enjoyed greater support from the municipal council. This trend holds for most categories of government jobs, except for non-tenured civil servants. In this category, turnover peaks when the counterfactual mayor would have held a qualified majority and then declines to a statistically insignificant level.

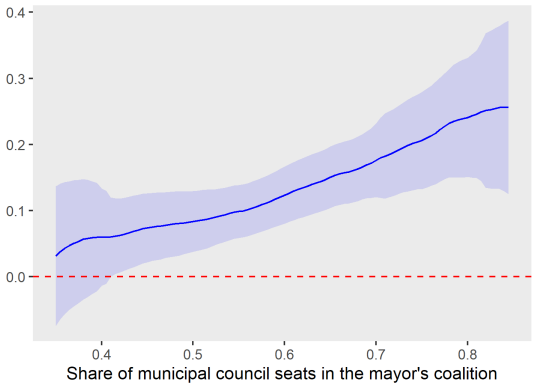
Figure 4 presents a similar pattern in the context of public school hirings. The turnover effects of electing a minor coalition mayor increase as the counterfactual mayor's support from the municipal council



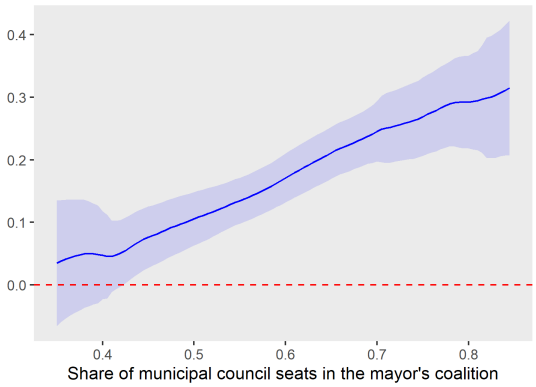

(a) School principals hired




(b) Civil serv. teachers hired




(c) Unexperienced school principals hired




(d) Unexperienced civil serv. teachers hired

Figure 4: Hetereogeneous effects of electing a minor coalition mayor on bureaucratic turnover - school hiring ($sinh^{-1}(counts)$)

rises. This panel also highlights that the almost monotonic rise in school principal hirings primarily involves inexperienced workers. In fact, the data in Figure 4 suggest that virtually all the increase in school staff hirings attributable to a minor coalition mayor stems from higher rates of hiring inexperienced personnel.

I interpret these results as strong evidence in favor of the *majority buying hypothesis*. Hiring and firing patterns are broadly consistent with mayors using government positions to gain legislative support. Moreover, apart from non-tenured civil servant turnover, the majority buying hypothesis suffices to explain all the estimates in panels 4, and 8.

Panel 9, similarly, generally supports that measures of bureaucratic performance and a mayor's legislative support go hand in hand. Measured impacts of electing a mayor with little legislative support tend to be largest when compared to a mayor with ample legislative support. As a mayor's counterfactual legislative support increases so does the share of teachers who report that "they feel overwhealmed", "their stundets fell behind", and "their student council met this year[10]". Similar patterns can be observed for measures of school principal performance, although they are not as strong as the ones observed for teachers.

Finally, I run my analysis to estimate the effects of electing a minor coalition mayor on standardized test scores. Panel 10 contains the results of such an analysis. In this case, however, the heterogeneous effects of electing a minor coalition mayor on students test scores do not clearly point to a single underlying mechanism. Treatment effects on 5th and 9th grade test scores are not monotonic and tend to follow different patterns across 5th and 9h grade scores. I discuss explanations for such shapes in the next section.

### 5.3 Discussion of the results

The RD causal estimates of bureaucratic hiring and performance are, generally, in line and can be fully explained by the "majority buying hypothesis". In this section, I discuss the two key concerns regarding the estimated relationship between legislative support and outcome variables: (1) monotonicity in bureaucratic turnover and (2) heterogeneity in treatment effects on non-tenured public servant turnover and test scores.

First, a potential concern is whether the relationship between legislative support and bureaucratic turnover outcomes should be monotonic. Some might argue that once a mayor secures a qualified majority (two-thirds of the municipal chamber), additional legislative support should no longer affect outcomes. However, even beyond this threshold, mayors can benefit from expanded support. For example, greater legislative backing may facilitate smoother negotiations in key committees or reduce the need to allocate bureaucratic positions to maintain coalition loyalty. Thus, the incentives for majority buying decrease but do not vanish entirely as support grows beyond two-thirds.

[10]school council refers to meeting between teachers (and sometimes parents) to discuss how to improve students' learning

Second, heterogeneity of the treatment effects on non-tenured public servant turnover and test scores paint a complex picture. A select few graphs presented stand out due to their non-monotonic shapes. Unlike the monotonic causal relationship estimated for all public sector workers and public sector heads in panel 8, turnover effects on public servants is an inverted U-shape. In other words, electing a minority mayor has increasingly positive impacts on non-tenured civil-servant turnover up until the counterfactual share of legislative support of roughly 0.63-0.67 of the legislators. Beyond a qualified majority, the measured increase in bureaucratic turnover rates attenuate until they are statistically indistinguishable from zero. Similarly, heterogeneous treatment effects on test scores also paint a complex picture in panel 10. Over the full election cycle 5th graders experience the greatest treatment effects at small values of $p_n$, when a major coalition mayor gets elected with limited support in the legislative chamber. Effects then ware off as the counterfactual mayor's support in the legislative branch increases. 9th graders, on the other hand, experience the greater treatment effects at counterfactual levels of support of small simple majorities and also at the highest levels of legislative majority (¿0.8).

Although carefully investigating the shape of these causal relationships is outside of the scope of this paper, I propose an explanation can be found based on the findings of this paper and the literature on political competition (Besley et al. (2010) and Jones (2013)). As increased legislative support decreases a mayor's incentives to exchange bureaucratic positions for political support, it also reduces the institutional oversight provided by political competitors. Without this oversight, mayors are freer to prioritize their personal agendas ans staff the bureaucracy with their own appointees. The inverted U-shape therefore results from the progressive fading away of both the majority buying and political competition mechanisms. As a mayor gains legislative support, the marginal benefit of this support (less majority buying) decreases while the marginal cost (less political competition) increases, leading to the observed inverted U-shape.

The estimated heterogeneous effects on standardized test scores, on the other hand, demands careful examination. Standardized test scores are generally seen as a proxy for education quality. Public education quality, in turn, is a function not only of the quality of the educational bureaucracy but also of the effort exerted by policy-makers. Improvements in public education require that policy-makers put effort towards it through careful oversight, resource allocation and planning. As a mayor enjoys greater support from the legislative branch, political competition decreases. Without the pressure of political competitors, politicians may favor their own private agendas (Besley et al. (2010)) at the expense of effort dedicated towards public education delivery. On the other hand, greater legislative support reduces a mayor's incentive to exchange public sector positions for political support. My conjecture, therefore, balances the labor selection effects of "majority buying" and organizational effects of "political competition".

When applied to 5th grade and 9th grade test scores heterogeneous treatment effects, however, these

two mechanisms might results in different dynamics. Over the entirety of the election cycle (t+1 to t+4), estimated treatment effects on 5th grader test scores are progressively attenuated as the counterfactual mayor increases their share of legislative support. In other words, the treatment effects are largest when mayors enjoy limited support from municipal legislators. This waring off is to be expected if the labor exerted by 1st-5th grade teachers and principals demands limited training and skills while it depends more heavily on the efforts and planning of policy makers.

Similarly, 9th grade test score point estimates can be rationalized through the interplay of the "majority buying" and "political competition" mechanisms. In this case, gaining political support from the legislative chamber would yield educational gains through better selection of 6th-9th grade teachers and improvements in their performance, as captured by panels 4 and 10. As one increases a mayor's legislative base, however, there are losses in education quality due to the diminished political competition in the municipality. Increasing a mayor's legislative support past 2/3rds of is likely to have reduced negative impacts on education due to political competition. Therefore, any additional legislative support gained beyond 2/3rds of the municipal chamber would yield net benefits to 9th graders' education.

# 6 Conclusion

The previous two sections establish several empirical facts regarding the causal effect of electing a mayor from a minor government. First, I show through a RDD that electing a mayor from a minor coalition leads to increased bureacratic turnover. Second, I show that in public schools the election of a minor government leads to increased hirings of unexperienced workers. Furthermore, electing a minor coalition mayor leads to worst measures of worker performance and test scores in public schools.

Regarding the underlying mechanism, I find an abundance of evidence to support the claim that mayor with little legislative support use coveted public sector positions to gain from municipal legislators. RD estimates show thay electing a minority coalition mayor sharply increases bureaucratic turnover measured by the number of workers hired and fired in a wide range of occupations. Furthermore, detailed municipal public school data shows how the increased number of hires is accompanied by increased hirings of unexperienced teachers and principals. Furthermore, I established through the same RD design that concurrent measure of school worker performance decrease substantially in a mayor's minority government. Finally, I show that students' standardize test scores fall considerably as a result of the minority mayor's administration.

Next, I show how RD evidence rules out 3 prominent alternatives to the "majority buying hypothesis". First, there is no evidence that the turnover is caused by incumbency losses studied in Akhtari et al. (2022). Second, the RD design shows no movement in the extensive margin of public service hirings or coverage that

could explain my initial causal estimates. Third, more "macro" variables at the municipal level (such as GPD, intergovernmental transfers, labor expenditure, education budget,...) are not affect by the election of an mayoral candidate of the minority coalition of the municipal legislative chamber.

Finally, using my novice strategy of a multi-dimensional RD, I show that heterogeneous treatment effects estimates further help solidify the "majority buying hypothesis". The impact of electing a minority coalition mayor on the hiring of overall number of new workers, public sector heads, school principals, and teachers monotonically increases in the number of counterfactual support a mayor would have had from the legislative chamber. Similar monotonically increasing treatment effects generally appear in the number unexperienced school worker hirings and measures of school worker performance.

There are two important nuances, however, to the majority buying hypothesis. First, while heterogeneous effects of electing a minor coalition mayor on bureaucratic turnover of high-rank government positions are perfectly in line with the majority buying dynamics, heterogeneity in measured turnover of non-tenured civil servants indicates other mechanisms may be at play. Second, measured heterogeneous treatment effects of electing a minor coalition mayor on student test scores also indicate that the majority buying hypothesis is not the only relevant mechanism that relates legislative-executive branch alignment and public service delivery.

# References


Acemoglu, D. and Robinson, J. A. (2012). *Why nations fail: The origins of power, prosperity and poverty*. Profile London.

Akhtari, M., Moreira, D., and Trucco, L. (2022). Political turnover, bureaucratic turnover, and the quality of public services. *American Economic Review*, 112(2):442–493.

Ashraf, N. and Bandiera, O. (2018). Social incentives in organizations. *Annual Review of Economics*, 10(1):439–463.

Barbosa, K. and Ferreira, F. (2023). Occupy government: Democracy and the dynamics of personnel decisions and public finances. *Journal of Public Economics*, 221:104856.

Baumgartner, F. R., Berry, J. M., Hojnacki, M., Leech, B. L., and Kimball, D. C. (2009). *Lobbying and policy change: Who wins, who loses, and why*. University of Chicago Press.

Bertrand, M., Bombardini, M., Fisman, R., Hackinen, B., and Trebbi, F. (2021). Hall of mirrors: Corporate philanthropy and strategic advocacy. *The Quarterly Journal of Economics*, 136(4):2413–2465.

Bertrand, M., Bombardini, M., and Trebbi, F. (2014). Is it whom you know or what you know? an empirical assessment of the lobbying process. *American Economic Review*, 104(12):3885–3920.

Besley, T., Burgess, R., Khan, A., and Xu, G. (2022). Bureaucracy and development. *Annual Review of Economics*, 14(1):397–424.

Besley, T. and Ghatak, M. (2005). Competition and incentives with motivated agents. *American economic review*, 95(3):616–636.

Besley, T. and Persson, T. (2014). Why do developing countries tax so little? *Journal of economic perspectives*, 28(4):99–120.

Besley, T., Persson, T., and Sturm, D. M. (2010). Political competition, policy and growth: theory and evidence from the us. *The Review of Economic Studies*, 77(4):1329–1352.

Bostashvili, D. and Ujhelyi, G. (2019). Political budget cycles and the civil service: Evidence from highway spending in us states. *Journal of public economics*, 175:17–28.

Brassiolo, P., Estrada, R., and Fajardo, G. (2020). My (running) mate, the mayor: Political ties and access to public sector jobs in ecuador. *Journal of Public Economics*, 191:104286.

Brollo, F., Forquesato, P., and Gozzi, J. C. (2017). To the victor belongs the spoils? party membership and public sector employment in brazil. *Party Membership and Public Sector Employment in Brazil (October 2017)*.

Brollo, F., Kaufmann, K., and La Ferrara, E. (2020). The political economy of program enforcement: Evidence from brazil. *Journal of the European Economic Association*, 18(2):750–791.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261.

Colonnelli, E., Prem, M., and Teso, E. (2020). Patronage and selection in public sector organizations. *American Economic Review*, 110(10):3071–3099.

Dahlström, C. and Lapuente, V. (2022). Comparative bureaucratic politics. *Annual Review of Political Science*, 25(1):43–63.

De Montesquieu, C. (1989). *Montesquieu: The spirit of the laws*. Cambridge University Press.

Drutman, L. (2015). *The business of America is lobbying*. Oxford University Press New York.

Finan, F., Olken, B. A., and Pande, R. (2015). The personnel economics of the state.

Fouirnaies, A. and Hall, A. B. (2022). How do electoral incentives affect legislator behavior? evidence from us state legislatures. *American Political Science Review*, 116(2):662–676.

Geddes, B. (2023). *Politician's dilemma: building state capacity in Latin America*, volume 25. Univ of California Press.

Grindle, M. S. (2012). *Jobs for the boys: Patronage and the state in comparative perspective*. Harvard University Press.

Iyer, L. and Mani, A. (2012). Traveling agents: political change and bureaucratic turnover in india. *Review of Economics and Statistics*, 94(3):723–739.

Jones, P. E. (2013). The effect of political competition on democratic accountability. *Political Behavior*, 35:481–515.

Kroeger, M. A. et al. (2017). How groups write the law: An empirical analysis of group influence in american state legislatures.

Marx, B., Pons, V., and Rollet, V. (2022). Electoral turnovers.

Pepinsky, T. B., Pierskalla, J. H., and Sacks, A. (2017). Bureaucracy and service delivery. *Annual Review of Political Science*, 20(1):249–268.

Rauch, J. E. and Evans, P. B. (2000). Bureaucratic structure and bureaucratic performance in less developed countries. *Journal of public economics*, 75(1):49–71.

Spenkuch, J. L., Teso, E., and Xu, G. (2023). Ideology and performance in public organizations. *Econometrica*, 91(4):1171–1203.

Toral, G. (2024). Turnover: How lame-duck governments disrupt the bureaucracy and service delivery before leaving office. *The Journal of Politics*, 86(4):000–000.

Xu, G. (2018). The costs of patronage: Evidence from the british empire. *American Economic Review*, 108(11):3170–3198.

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

| | $sinh^{-1}$**(number of recently hired)** | | | | $sinh^{-1}$**(number of recently fired)** | | | |
|---|---|---|---|---|---|---|---|---|
| | Total Workers | Civil Serv. | Civil Serv. (tenured) | Temp. Workers | Total Workers | Civil Serv. | Civil Serv. (tenured) | Temp. Workers |
| $\gamma$ | 0.245** | 0.467*** | -0.073 | -0.112 | 0.085 | 0.375** | -0.069 | -0.091 |
| SE | (0.121) | (0.172) | (0.189) | (0.19) | (0.152) | (0.162) | (0.113) | (0.177) |
| p-value | [0.044] | [0.007] | [0.699] | [0.555] | [0.575] | [0.021] | [0.544] | [0.608] |
| N | 7801 | 7801 | 7801 | 7801 | 7801 | 7801 | 7801 | 7801 |
| Eff. N | 3712 | 3581 | 3514 | 3024 | 3559 | 3549 | 4507 | 3007 |
| b | 0.113 | 0.108 | 0.105 | 0.088 | 0.107 | 0.107 | 0.145 | 0.087 |

Table 2: (RAIS) Hiring and firing patterns in the year after the election by occupation

| | Public sector heads | Lawyers | Nurses | Pub. Health Officials | Social Workers | Supervisors | Teachers |
|---|---|---|---|---|---|---|---|
| | $sinh^{-1}$**(number of recently hired)** | | | | | | |
| $\gamma$ | 0.303** | 0.184*** | 0.301*** | 0.18** | 0.144** | 0.133 | 0.325** |
| SE | (0.125) | (0.056) | (0.115) | (0.092) | (0.066) | (0.086) | (0.14) |
| p-value | [0.016] | [0.001] | [0.009] | [0.05] | [0.029] | [0.122] | [0.02] |
| N | 7549 | 7549 | 7549 | 7549 | 7549 | 7549 | 7549 |
| Eff. N | 3727 | 3766 | 3759 | 3359 | 3621 | 4187 | 3816 |
| b | 0.12 | 0.121 | 0.121 | 0.105 | 0.115 | 0.138 | 0.123 |
| | $sinh^{-1}$**(number of recently fired)** | | | | | | |
| $\gamma$ | 0.212* | 0.062 | 0.145 | 0.1 | 0.056 | 0.148** | 0.329** |
| SE | (0.109) | (0.043) | (0.097) | (0.068) | (0.05) | (0.07) | (0.135) |
| p-value | [0.053] | [0.145] | [0.135] | [0.138] | [0.261] | [0.034] | [0.015] |
| N | 7549 | 7549 | 7549 | 7549 | 7549 | 7549 | 7549 |
| Eff. N | 3492 | 3416 | 3615 | 3332 | 3845 | 3787 | 3429 |
| b | 0.11 | 0.107 | 0.115 | 0.104 | 0.124 | 0.122 | 0.108 |

(1) All Construction worker, driver, guard, maintenance worker and office assistant hirings are represented in the data as these positions are hired through both temporary contracts and civil servant contracts. Accountant, auditor, architect, dentist, and doctor hirings only represent non-tenured civil servant hirings.

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

| | Accountants | Auditors | Architects | Dentists | Doctors | Constr. Work. | Driver | Guards | Maint | Off. Assist. |
|---|---|---|---|---|---|---|---|---|---|---|
| | $sinh^{-1}$**(number of recently hired)** | | | | | | | | | |
| $\gamma$ | 0.104* | 0.02 | 0.038** | 0.122 | 0.104 | 0.109 | 0.362*** | 0.496*** | 0.099 | 0.27* |
| SE | (0.057) | (0.037) | (0.015) | (0.075) | (0.089) | (0.1) | (0.133) | (0.155) | (0.122) | (0.15) |
| p-val. | [0.071] | [0.6] | [0.012] | [0.103] | [0.242] | [0.275] | [0.007] | [0.001] | [0.42] | [0.072] |
| N | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 |
| N(b) | 3370 | 3359 | 3554 | 3109 | 3173 | 4064 | 2823 | 2501 | 3489 | 3729 |
| b | 0.105 | 0.105 | 0.113 | 0.095 | 0.097 | 0.133 | 0.086 | 0.074 | 0.11 | 0.12 |
| | $sinh^{-1}$**(number of recently fired)** | | | | | | | | | |
| $\gamma$ | 0.042 | 0.017 | 0.012 | 0.043 | 0.028 | 0.086 | 0.236** | 0.172* | 0.162* | 0.064 |
| SE | (0.037) | (0.029) | (0.008) | (0.059) | (0.074) | (0.086) | (0.109) | (0.101) | (0.095) | (0.129) |
| p-val | [0.262] | [0.561] | [0.118] | [0.467] | [0.706] | [0.318] | [0.03] | [0.091] | [0.089] | [0.622] |
| N | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 | 7551 |
| N(b) | 3588 | 3628 | 3044 | 3370 | 3468 | 3986 | 3044 | 4103 | 3635 | 3630 |
| b | 0.114 | 0.115 | 0.093 | 0.105 | 0.109 | 0.13 | 0.093 | 0.135 | 0.116 | 0.115 |

(1) All public sector heads and lawyer hirings are represented in the data as these positions are hired through both temporary contracts and civil servant contracts. All other occupation hirings only represent non-tenured civil servant hirings.

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

| | 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" | "Students fell behind" | "Principal cares about: students' learning" | "Principal cares about: management" | "Principal cares about: maintenance" |
| | **Share of municipal reports in the year after the election (t+1)** | | | | | | | | | | | |
| $\gamma$ | 0.172*** | 0.154*** | -0.028 | −0.047** | 0.18*** | 0.182*** | −0.078** | 0.037 | 0.093** | −0.087* | -0.051 | −0.066** |
| SE | (0.037) | (0.037) | (0.03) | (0.02) | (0.034) | (0.034) | (0.037) | (0.033) | (0.039) | (0.046) | (0.034) | (0.031) |
| p-value | [0] | [0] | [0.349] | [0.023] | [0] | [0] | [0.034] | [0.272] | [0.015] | [0.061] | [0.135] | [0.035] |
| N | 5637 | 5637 | 5637 | 5637 | 5551 | 5551 | 5551 | 5551 | 5551 | 5551 | 5551 | 5551 |
| N(b) | 2684 | 2512 | 2607 | 2007 | 2295 | 2324 | 2160 | 2848 | 2431 | 2287 | 1978 | 2552 |
| b | 0.116 | 0.106 | 0.111 | 0.08 | 0.096 | 0.098 | 0.09 | 0.127 | 0.104 | 0.096 | 0.081 | 0.111 |
| | **Share of municipal reports in the 4 years after the election** | | | | | | | | | | | |
| $\gamma$ | 0.097*** | 0.082** | 0.007 | -0.01 | 0.109*** | 0.103*** | −0.078** | 0.088* | 0.112** | -0.099 | −0.07** | −0.089** |
| SE | (0.033) | (0.034) | (0.028) | (0.021) | (0.023) | (0.022) | (0.038) | (0.05) | (0.052) | (0.061) | (0.035) | (0.044) |
| p-value | [0.004] | [0.016] | [0.798] | [0.62] | [0] | [0] | [0.041] | [0.075] | [0.031] | [0.103] | [0.044] | [0.043] |
| N | 4415 | 4488 | 5579 | 5579 | 5494 | 5494 | 5494 | 3678 | 3678 | 5494 | 5494 | 3678 |
| Eff. N | 2391 | 2003 | 2702 | 2329 | 2238 | 2261 | 2238 | 1618 | 1468 | 2294 | 2092 | 1648 |
| b | 0.133 | 0.104 | 0.118 | 0.097 | 0.095 | 0.096 | 0.095 | 0.101 | 0.09 | 0.097 | 0.087 | 0.103 |

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

| | **5th Grade** | | | **9th Grade** | | |
|---|---|---|---|---|---|---|
| | Math | Portuguese | Combined | Math | Portuguese | Combined |
| | **Standardized test scores (in the year after the election)** | | | | | |
| $\gamma$ | $-0.053^{**}$ | $-0.045^{***}$ | $-0.048^{**}$ | $-0.081^{**}$ | $-0.064^{*}$ | $-0.073^{**}$ |
| SE | (0.027) | (0.018) | (0.021) | (0.033) | (0.033) | (0.031) |
| p-value | [0.046] | [0.01] | [0.023] | [0.013] | [0.054] | [0.019] |
| N | 5754 | 5754 | 5754 | 4301 | 4301 | 4301 |
| Eff. N | 1736 | 1930 | 1804 | 1234 | 1153 | 1185 |
| b | 0.102 | 0.117 | 0.107 | 0.096 | 0.088 | 0.092 |
| | **Standardized test scores (in the 4 years after the election)** | | | | | |
| $\gamma$ | -0.05 | $-0.053^{***}$ | $-0.051^{**}$ | $-0.089^{**}$ | -0.043 | $-0.066^{*}$ |
| SE | (0.031) | (0.02) | (0.025) | (0.042) | (0.038) | (0.039) |
| p-value | [0.107] | [0.009] | [0.04] | [0.035] | [0.25] | [0.086] |
| N | 3760 | 3760 | 3760 | 2672 | 2672 | 2672 |
| Eff. N | 852 | 841 | 829 | 490 | 453 | 477 |
| b | 0.103 | 0.101 | 0.1 | 0.08 | 0.075 | 0.079 |

Table 6: Bureaucratic turnover effects by incumbency status in the year after the election

| | Non-ten. civil serv. hired | Non-ten. civil serv. fired | Pub. sect. heads hired | Pub. sect. heads fired | Lawyers hired | Lawyers fired | School principal | Unexp. principals | School teachers | Unexp. teachers |
|---|---|---|---|---|---|---|---|---|---|---|
| | **All elections (Main specification)** | | | | | | | | | |
| $\gamma$ | 0.467*** | 0.375** | 0.303** | 0.212* | 0.184*** | 0.062 | 0.172*** | 0.154*** | 0.18*** | 0.182*** |
| SE | (0.172) | (0.162) | (0.125) | (0.109) | (0.056) | (0.043) | (0.037) | (0.037) | (0.034) | (0.034) |
| p-value | [0.007] | [0.021] | [0.016] | [0.053] | [0.001] | [0.145] | [0] | [0] | [0] | [0] |
| N | 7801 | 7801 | 7463 | 7463 | 7463 | 7463 | 5599 | 5599 | 5513 | 5513 |
| Eff. N | 3581 | 3549 | 3727 | 3492 | 3766 | 3416 | 2684 | 2513 | 2298 | 2326 |
| b | 0.108 | 0.107 | 0.12 | 0.11 | 0.121 | 0.107 | 0.116 | 0.106 | 0.096 | 0.098 |
| | **Elections without an incumbent candidate** | | | | | | | | | |
| Estimate | 0.496** | 0.689*** | 0.331** | 0.286** | 0.156** | 0.045 | 0.191*** | 0.195*** | 0.211*** | 0.215*** |
| SE | (0.224) | (0.204) | (0.155) | (0.134) | (0.077) | (0.049) | (0.051) | (0.045) | (0.04) | (0.039) |
| p-value | [0.027] | [0.001] | [0.032] | [0.032] | [0.042] | [0.352] | [0] | [0] | [0] | [0] |
| N | 4248 | 4248 | 4069 | 4069 | 4069 | 4069 | 2893 | 2893 | 2861 | 2861 |
| Eff. N | 2002 | 1908 | 2360 | 1941 | 1943 | 1996 | 1439 | 1547 | 1436 | 1490 |
| b | 0.109 | 0.102 | 0.141 | 0.11 | 0.11 | 0.114 | 0.117 | 0.128 | 0.119 | 0.124 |
| | **Elections without an incumbent candidate or an incumbent party** | | | | | | | | | |
| Estimate | 0.957*** | 1.087*** | 0.199 | 0.274 | 0.194** | 0.072 | 0.248*** | 0.235*** | 0.257*** | 0.266*** |
| SE | (0.295) | (0.262) | (0.224) | (0.173) | (0.092) | (0.062) | (0.05) | (0.051) | (0.046) | (0.044) |
| p-value | [0.001] | [0] | [0.373] | [0.113] | [0.034] | [0.251] | [0] | [0] | [0] | [0] |
| N | 2637 | 2637 | 2518 | 2518 | 2518 | 2518 | 1842 | 1842 | 1822 | 1822 |
| Eff. N | 1128 | 1142 | 1232 | 1187 | 1070 | 1062 | 1152 | 1013 | 1019 | 1044 |
| b | 0.101 | 0.102 | 0.117 | 0.113 | 0.1 | 0.099 | 0.165 | 0.139 | 0.142 | 0.146 |

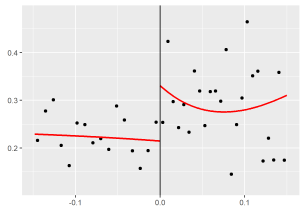
Table 7: Alternative mechanisms: Effects of electing a minority coalition mayor on other school outcomes

| | Open schools | K-12 regist. students | Teachers on payroll | Schools w/ access to clean water | Schools w/ access to energy | Schools w/ access to sewage syst. | Schools "insuffic. infrastruc." | Schools "insuffic. funds" | Schools "insuffic. staff" | Schools "insuffic. textbooks" |
|---|---|---|---|---|---|---|---|---|---|---|
| Estimate | 0.006 | 0.014 | 0.004 | -0.003 | 0.006 | 0.002 | 0.119** | 0.047* | 0.016 | 0.027 |
| SE | (0.018) | (0.013) | (0.016) | (0.01) | (0.006) | (0.006) | (0.055) | (0.027) | (0.025) | (0.027) |
| p-value | [0.754] | [0.288] | [0.796] | [0.769] | [0.327] | [0.738] | [0.03] | [0.082] | [0.524] | [0.318] |
| N | 5911 | 5911 | 5862 | 5911 | 5911 | 5911 | 3669 | 5543 | 5543 | 3669 |
| Eff. N | 2665 | 2411 | 2921 | 2711 | 2030 | 2663 | 1460 | 2563 | 2751 | 1804 |
| b | 0.106 | 0.094 | 0.122 | 0.109 | 0.076 | 0.106 | 0.089 | 0.11 | 0.121 | 0.117 |

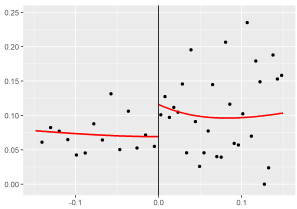
Table 8: Alternative mechanisms: Effects of electing a minority coalition mayor on the government's budget, GDP, and population

| | Log(munic. labor exp.) | Log(munic. running exp.) | Log(munic. educ exp.) | Log(munic. real GDP) | Log(munic. population) | Log(munic. intergov. transf.) |
|---|---|---|---|---|---|---|
| Estimate | -0.028 | -0.033 | 0.082 | 0.021 | -0.003 | 0.001 |
| SE | (0.048) | (0.066) | (0.101) | (0.015) | (0.004) | (0.018) |
| p-value | [0.55] | [0.616] | [0.413] | [0.169] | [0.446] | [0.955] |
| N | 7170 | 7170 | 7156 | 7801 | 5722 | 7163 |
| Eff. N | 4932 | 4252 | 4152 | 2886 | 2899 | 2825 |
| b | 0.201 | 0.156 | 0.151 | 0.083 | 0.121 | 0.094 |

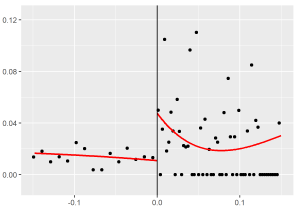
Figure 5: Hiring patterns in the year after the election by occupation ($sinh^{-1}(counts)$)



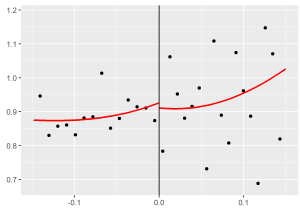
(a) Accountants (non-ten)



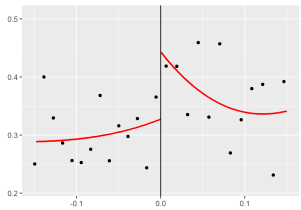
(b) Auditors (non-ten)



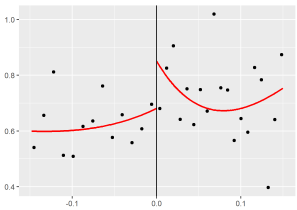
(c) Architects (non-ten)



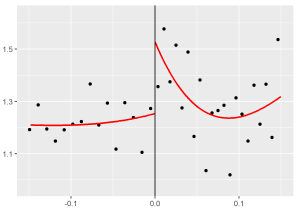
(d) Constr. Workers



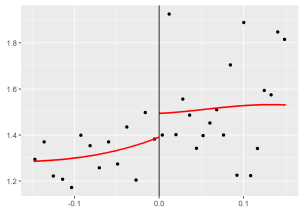
(e) Dentists (non-ten)



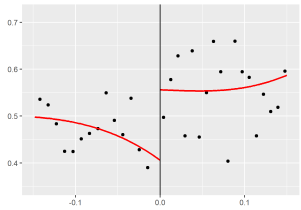
(f) Doctors (non-ten)



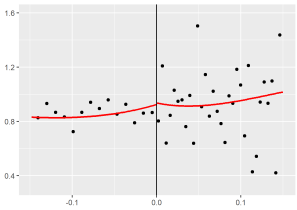
(g) Drivers



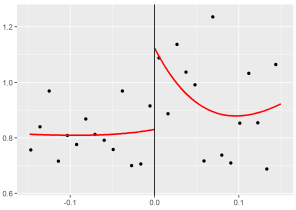
(h) Guards



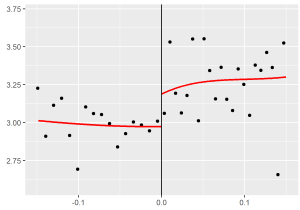
(i) Lawyers



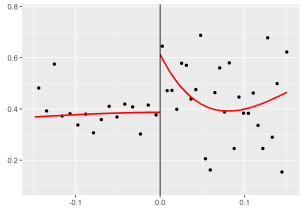
(j) Maint. workers



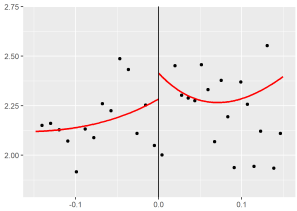
(k) Nurses (non-ten.)



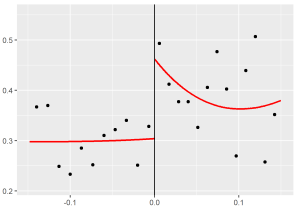
(l) Office assistants



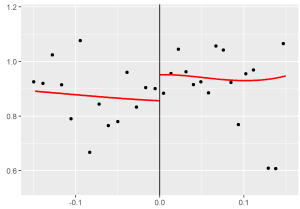
(m) Pub. health off. (non-ten)



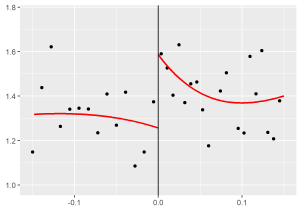
(n) Public sector heads



(o) Social Work. (non-ten.)

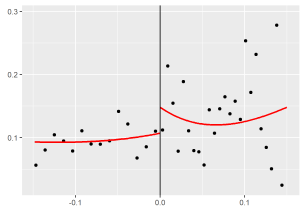


(p) Supervisors (non-ten)

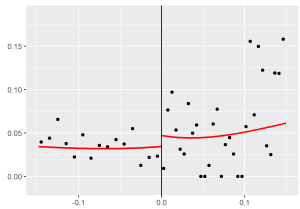


(q) Teachers (non-ten.)

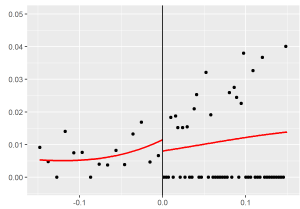
Figure 6: Firing patterns in the year after the election by occupation ($sinh^{-1}(counts)$)



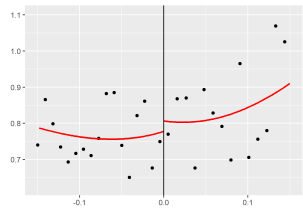
(a) Accountants (non-ten)



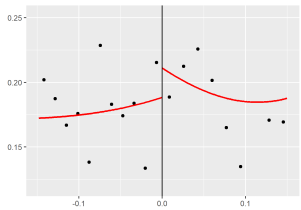
(b) Auditors (non-ten)



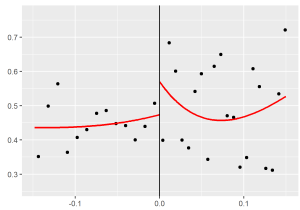
(c) Architects (non-ten)



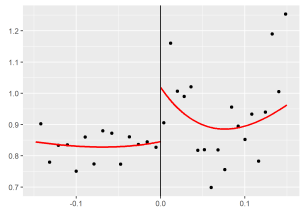
(d) Constr. Workers



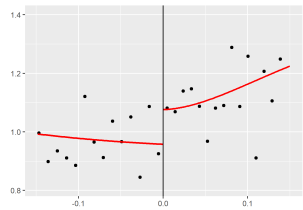
(e) Dentists (non-ten)



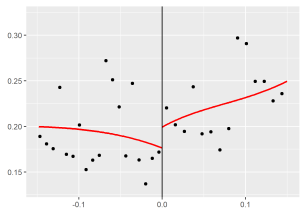
(f) Doctors (non-ten)



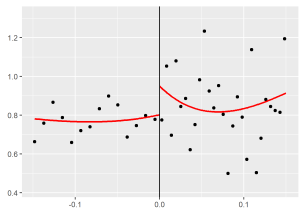
(g) Drivers



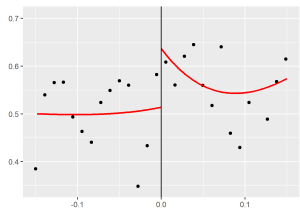
(h) Guards



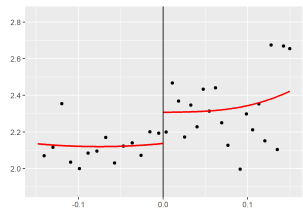
(i) Lawyers



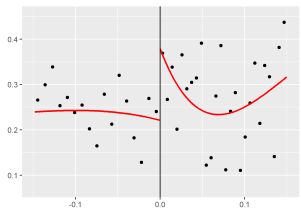
(j) Maint. workers



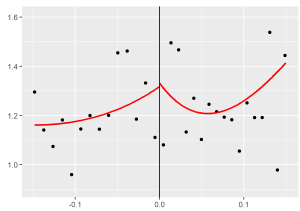
(k) Nurses (non-ten.)



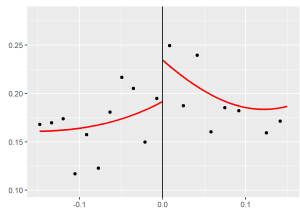
(l) Office assistants



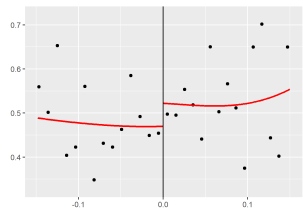
(m) Pub. health off. (non-ten)



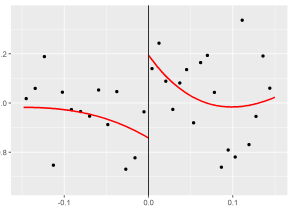
(n) Public sector heads



(o) Social Work. (non-ten.)

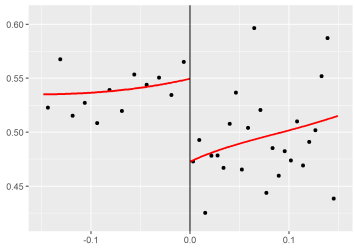


(p) Supervisors (non-ten)

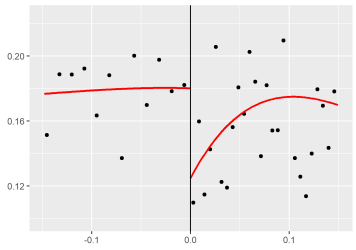


(q) Teachers (non-ten.)

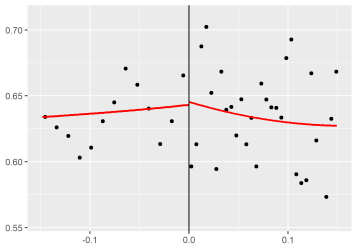
Figure 7: (SAEB) Teachers' and Principals' performance in the 4 years after the election ($sinh^{-1}(counts)$)



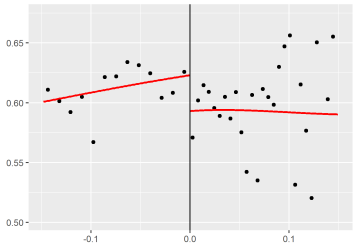
(a) Principal offers teach. train.



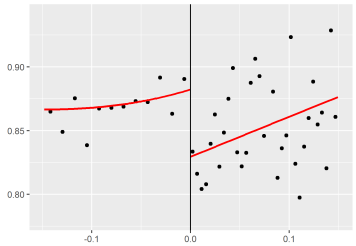
(b) Teachers: "princ. cares about stud. learning"



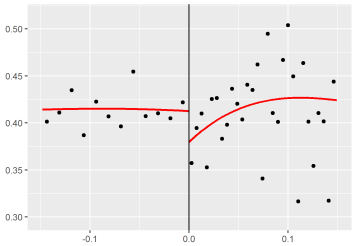
(c) Teachers: "princ. cares about admin."



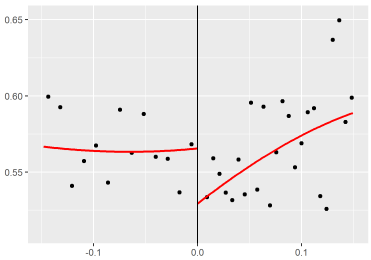
(d) Teachers: "princ. cares about maintenance"



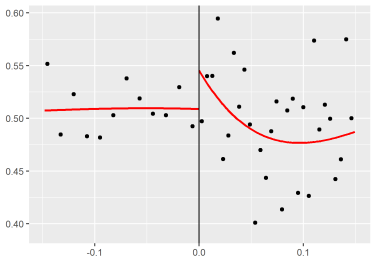
(e) Principal has a pedag. proj.



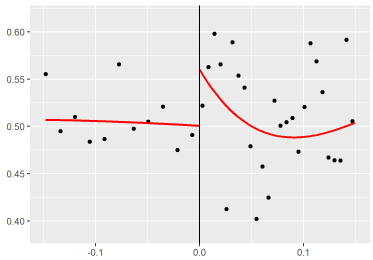
(f) Teachers: "I respect the principal professionally"



(g) School council met this year

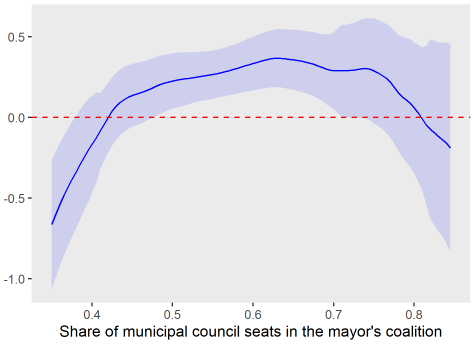


(h) "I feel overwhealmed"

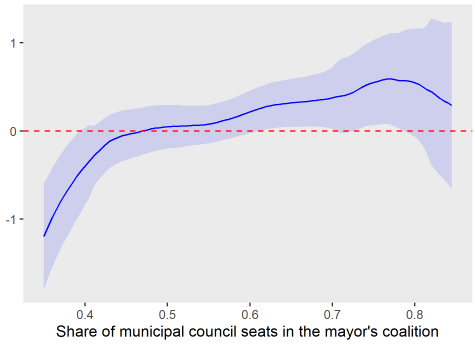


(i) "Students fell behind"

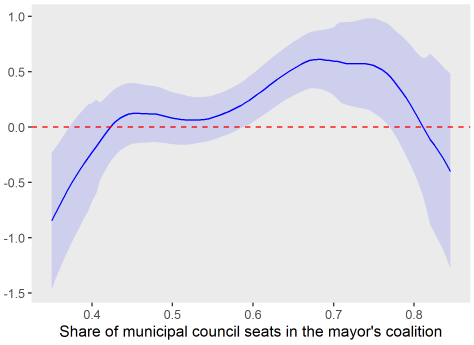
Figure 8: Hetereogeneous effects of electing a minor coalition mayor on bureaucratic turnover - hiring ($sinh^{-1}(counts)$)



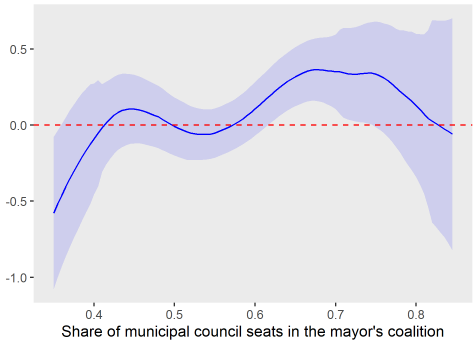

(a) Total workers (hired)



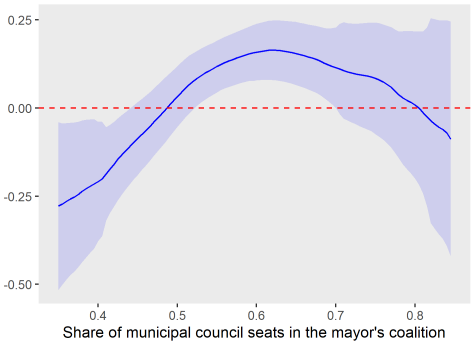

(b) Total workers (fired)



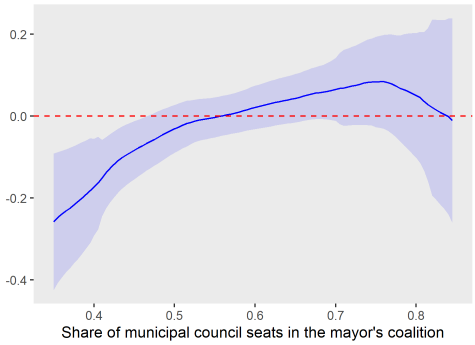

(c) Public sector heads (hired)




(d) Public sector heads (fired)

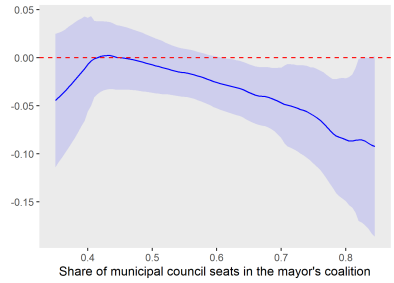



(e) Lawyers (hired)

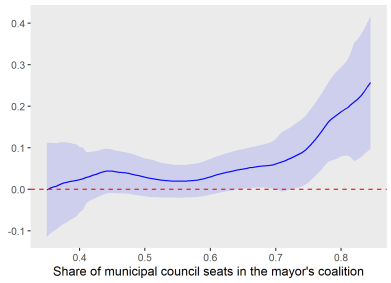



(f) Lawyers (fired)

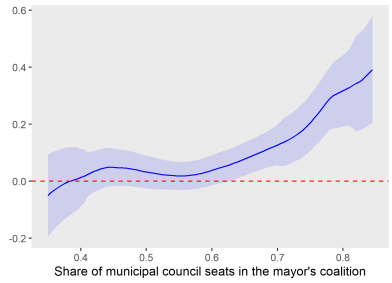
Figure 9: Hetereogeneous effects of electing a minor coalition mayor on bureaucratic performance in the 4 years after the election ($sinh^{-1}(counts)$)



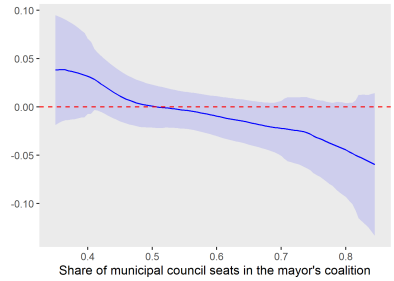

(a) School council met this year



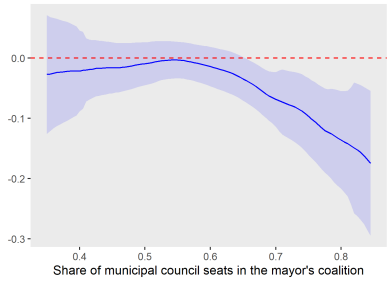

(b) "I feel overwhelmed"



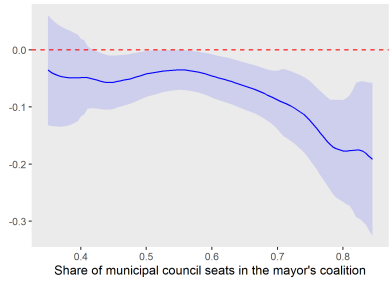

(c) "Students fell behind"



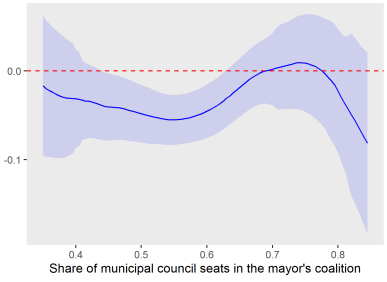

(d) Teachers: "princ. cares about admin."



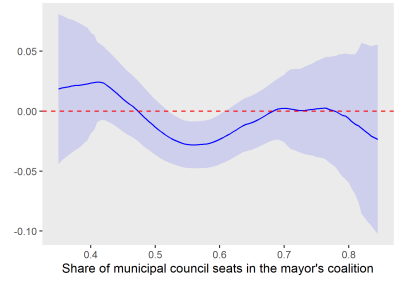

(e) Teachers: "princ. cares about maintenance"




(f) Teachers: "princ. cares about stud. learning"

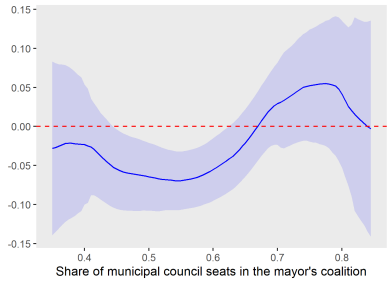



(g) Principal offers teach. train.

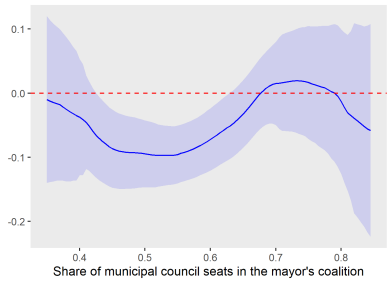



(h) School has a pedag. proj.

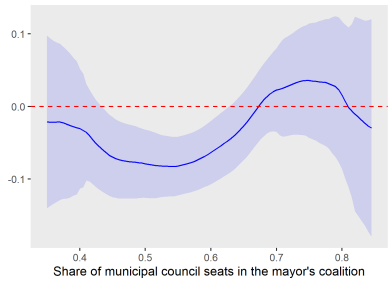
Figure 10: Hetereogeneous effects of electing a minor coalition mayor on standardized test scores (std. dev's)



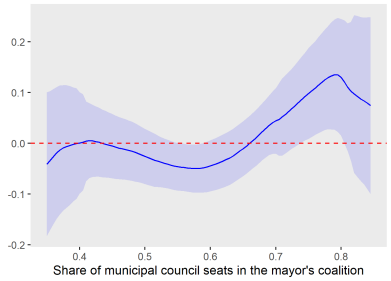

(a) Port. 5th grade (t+1)



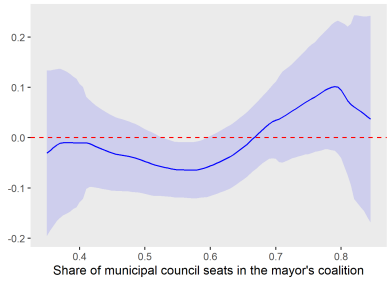

(b) Math 5th grade (t+1)



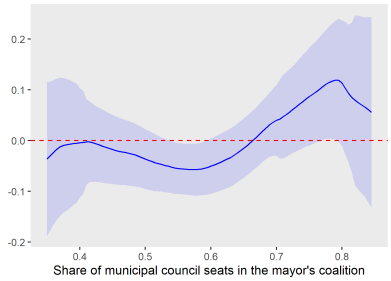

(c) Total 5th grade (t+1)



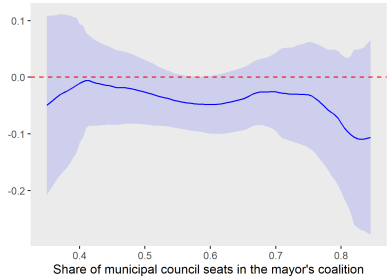

(d) Port. 5th grade (t+1 to t+4)



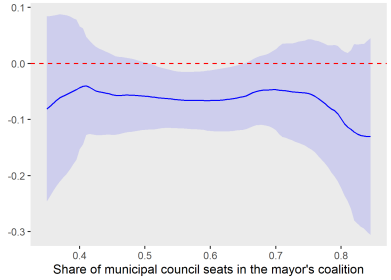

(e) Math 5th grade (t+1 to t+4)



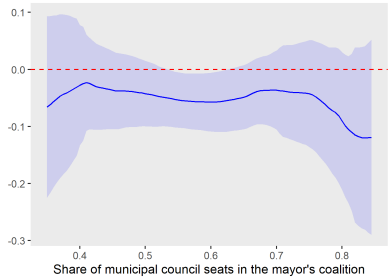

(f) Total 5th grade (t+1 to t+4)



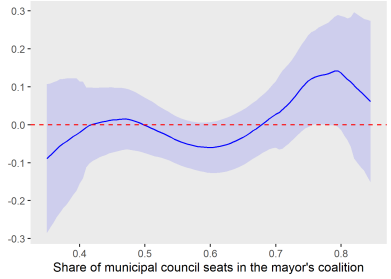

(g) Port. 9th grade(t+1)



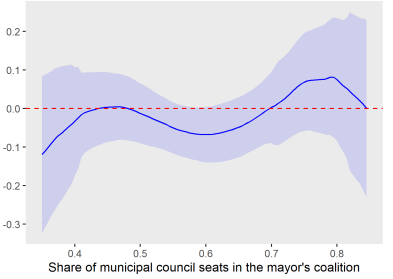

(h) Math 9th grade(t+1)



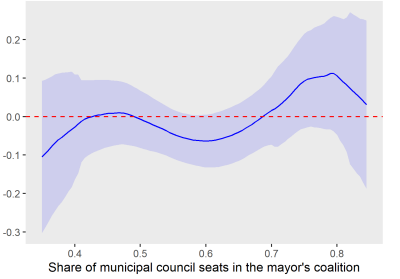

(i) Total 9th grade (t+1)




(j) Port. 9th grade (t+1 to t+4)




(k) Math 9th grade (t+1 to t+4)




(l) Total 9th grade (t+1 to t+4)