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ESSAYS IN DEVELOPMENT ECONOMICS

By

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DISSERTATION

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2022

To my Mother, Elisabeth, and my Father, Roberto. I love you.

Contents

Abstract

This dissertation studies potential causes for questions of particular relevance in developing countries.

In the first chapter, I use a Regression Discontinuity Design on close elections in Brazil to analyze how the election of a female mayor affects the gender composition gap of top municipal executives. I show that electing a female mayor on a close race increases the share of female managers by 17 %. This increase doesn't come at the cost of observed quality of employees. I also present evidence that public sector-specific and supply-side channels are unlikely to explain my findings; instead, an increase in gender-inclusive policies within female-led governments suggests that homophily may be a driver of the results.

In the Second chapter, focusing on the 2016 Brazilian election, my coauthors and I perform a similar analysis to chapter one but using race as subject of analysis. We estimate the impact of electing a non-white mayor on a given municipality over the share of non-white municipal managers. We find that electing a non-mayor is not associated with any significant increase on the share of non-white managers.

In the third chapter, we use a random corruption audit program in Brazil to cast light on the relationship between corruption and political participation. Different from other studies, we analyze corruption impacts at a different electoral level from where the corruption act took place. Empirical analysis shows that while being a standard deviation away from the mean of corruption violations and having random audits released before the election is not associated with a decrease in null voting on the local level, being in the same position of the corruption violation distribution and having random audits released prior to the election is associated with a 4% decrease in null voting in gubernatorial elections.

CHAPTER 1

Leadership and Gender Composition in Top Positions

1.1. Introduction

Over the past sixty years, most countries in the world have seen a reduction in gender gap indicators in their labor markets ([**48**]). Despite this overall convergence, women remain heavily underrepresented in high status positions. In the private sector, as of 2020, only 37 of the companies on Fortune 500 were led by female CEOs. [1] In the public sector, the female share in managerial positions is still below 50% for both OECD (43 %, Figure 1.1) and non-OECD countries (33 %).

Given their position of power and discretion in decision making, leaders, defined as agents in charge of making human resources allocation decisions, are often pointed out as a potential path to reducing the top-position underrepresentation problem. In the past decade, several gender quotas and other inclusive policies have been implemented in the hope that leadership changes can generate gender-inclusive spillovers to their subordinates.[2] [3] Yet, while the general literature on gender gaps in labor markets is extensive, the literature on gender gaps in top positions and their relationship with leadership is limited. This article seeks to contribute to this narrow field.

This article analyzes the impact of electing a female mayor on the composition gap among top managers in the local public sector. To evaluate this relation, I use electoral data from four local elections and labor market data from more than 5 500 Brazilian municipalities to implement a Regression Discontinuity design to access the impact of a woman winning a close election on the gender composition of managers in the public sector at the local level. Furthermore, I analyze the channels that could affect the gender gap in these positions, differentiating between public sector specific mechanisms, supply and demand shifts.

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[1]https://fortune.com/2020/05/18/women-ceos-fortune-500-2020/

[2]Idea International reports that, as of 2021, more than 130 countries have gender quotas in some form in their polical system. For more see https://www.idea.int/data-tools/data/gender-quotas/country-overview

[3]Belgium, France, Germany, Iceland, India, Israel, Italy, Norway, Spain, Germany, and India have introduced some type of board gender quota for private companies, according to [**8**]

In theory, female mayors can affect the managerial gender composition gap in ambiguous ways. If leaders (in this case, mayors) prefer working with employees of their own gender ([34]), we may see that a switch to a female leader leads to a decrease in the gender composition gap ([60]). If, however, the supply of qualified women on the local level is small or non-existent, a female politician may have no impact on reducing the gender inequality gap. Proponents of this theory have pointed out a few reasons to why supply may be limited at the top: For example, female workers may face more frequent career interuptions ([12]) or shy away from competitive work settings disproportionately more than men ([1], [41]). Hence, a priori, the relationship between leadership and the gender composition of employees is unknown.

My results show that a woman winning a close mayoral race is associated with a significant 17 % increase in the share of female managers with temporary contracts at the local level. This increase represents 71 % of the gender composition gap. This result is robust to a series of robustness and falsification tests. It is strong enough to increase composition on the entire top quintile of wages in municipal public sectors. I interpret these results as evidence that direct supply-limiting mechanisms are not the main cause of the gender gap. Next, I evaluate the common concern on the gender equality literature that gender inclusiveness may come at the cost of less qualified employees. I show that the increase in managerial share doesn't seem to come at the expense of observable quality, as I find no effects on managers' average level of education.

Furthermore, I evaluate wether public sector-specific channels could explain my results. I show that the results are unlikely to be driven by differential characteristics associated with mayoral gender, asymmetries in policy preferences, patronage, or strategic behavior. Finally, I analyze if an indirect supply channel or demand channels could explain the increase in female managerial share. While I find limited evidence of an indirect supply channel, demand channels cannot be ruled out as drivers of my main results. I show that female mayors are indeed more concerned with gender equality, as they are significantly more likely to implement gender-inclusive policies. I interpret this as suggestive evidence that demand-side preferences may play a role in driving the gender composition gap. Finally, while analyzing effects on heads of local boards, I show that these effects are consistent with homophilic preferences of leaders and that, although not being the dominant effect, sector-specific supply limitations may be present. These results are important from the policy-making perspective, as changing the gender of leadership may, at least in this case, narrow

the gender composition gap in top positions by 71 %, suggesting that gender quotas in leadership positions with discretion may trickle down to managerial positions narrowing, representation gaps at the top.

This study provides three main contributions to the literature on female leadership and gender gaps in top positions. First, I analyze leadership in a context where leaders have complete discretion over the employee composition selection. This analysis is different from previous studies that focus exclusively on directors' board changes. While extremely relevant for the board quotas policy debate, conclusions from these analyses are drawn from cases where cases in which leaders have limited discretion to make decisions. Boards of directors often face internal company policies and labor market regulations, limiting their members' decision-making power. Applying conclusions drawn from contexts where female leaders have constrained discretion to other cases can lead to misleading predictions and incorrect policy recommendations, especially in circumstances where authors find that leadership has limited impacts ([11]).

Second, to the best of my knowledge, this is the first study to use a Regression Discontinuity design to evaluate gender composition gaps in top managerial positions. So far, studies that have examined the relation between leadership and female employee share in top executive positions have used either instrumental variables to find null results ([11]) or fixed-effects models to find positive impacts on the share of female employees ([60]). In either case, the exogeneity of treatment is limited at best, and factors such as unobservable within-company institutional changes can confound the interpretation of results. Thus, finding an institutional setup where one can use a methodology that is not subject to these confounding factors is crucial for properly identifying effects, particularly given the contrasting nature of previous results in this literature. Third, due to the nature of my data and institutional setup, this study is able to analyze mechanisms behind the increase in female composition and show suggestive evidence that demand-side preferences drive the effect. Finally, this study contributes to the broad literature that studies the impact of electing female politicians by showing that beyond the impact that electing a female official has on politics and provision of public goods, electing female politicians also impacts the gender composition of top employees and the implementation of gender-inclusive policies in local public sectors.

This paper is organized as following: Section 1.2 presents a brief literature review on gender gaps in top positions and impacts of gender-mixed elections, Section 1.3 provides context for the

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Brazilian public sector and electoral system, Section 1.4 describes the data, Section 1.5 describes the methodology, Section 1.6 presents the results, Section 1.7 concludes.

1.2. Related Literature & Contribution

Several articles have examined the impact of having direct or indirect female superior over gender-based labor market outcomes of top employees in the private sector. While some studies in this area focus on promotion rates ([**54**], [**17**], [**55**]) others focus on wage gaps [**40**]. Only two studies focus explicitly on composition at top positions: [**60**] use a fixed-effects model to show evidence that, for U.S. companies in the period ranging from 1997 - 2009, the female share of directors is a significant positive predictor of the female share among top executives. Authors suggest that their results indicate that quota-style affirmative action policies, such as the one being implemented at the time in Norway, should have a significant impact over the gender composition of employees. Using a instrumental variable approach, [**11**] evaluates the consequences of said policy for managerial gender composition and finds no effects. In both cases, impacts over the share of top-earning employees can be limited by lack of discretion of directors. Moreover, both of these methodologies do not control for potential unobservable within organization changes that could affect both boards and managers. This study makes three contributions to the discussion in this literature: First, it is the first work to analyze gender composition in top-earning positions in a context where leaders have full discretion. Second, it is the first to make this analysis using a regression discontinuity approach. Third, it is the first to, within the same context, analyze the driving mechanism of the change in composition.

This study is also related to the literature that analyzes the policy consequences of electing female politicians. Since [**28**] seminal paper showing that women invest more in the provision of public goods that affect their own gender this developed branch of literature has used mostly regression discontinuity designs to access the influence of female politicians in several different areas such as education, health, labor markets and politics ([**39**], [**31**], [**31**], [**15**] [**19**], [**3**], [**4**], [**77**]). For example, [**39**] shows that for the U.S., women mayors exhibit similar spend patterns to those of men. [**31**] show that female political representation increases the probability that an individual will attain primary education in urban areas, and [**19**] find that an increase in women's representation results in a reduction in neonatal mortality and strategic behavior. This article contributes to this literature not only by analyzing the consequences of female mayors for employment of top executives

but also by analyzing consequences for gender policies.

1.3. Context

1.3.1. Brazilian Public Sector.

1.3.1.1. *Managers.* Managers in local governments are responsible for leading and directing boards on various areas: from education to transportation, from health to gender equality. They represent the highest form of management of the Brazilian public sector. As such, they tend to be more educated and exhibit higher wages than the average public employee. Table 1.1 compares characteristics of managers in Brazilian municipalities with average characteristics of quintiles of public employees wage distribution. There are a few points to notice about this table. First, the average manager composition is similar to that of employees on the top quintile of the wage distribution regarding wages and education. On average, managers are paid a mean of 3.83 Brazilian minimum wages (R$ 510,00 a month as of 2010 [4]) across municipalities, the top quantile of the wage distribution has a mean of 4.315 BRMW, indicating that managers are indeed on the top of the municipality wage distribution. Second, managers are also very similar to the top quintile in terms of educational outcomes. On average, the share of employees in a municipality that has completed elementary school or less is only 15.5% for managers and 16.2 % for the fifth quintile, 43 % for the employees on the first quintile. On the opposite end of the instruction composition, managers and the fifth quintile are also similar. On average across municipalities, 31.3 % of managers have completed superior education compared to 45.7 % of the members of the top quintile.

Where managers and the top quintile are very different, however, is on the contract type composition. Managers, on average, have a higher share of temporary contracts (55.3 %) than any other quintile of the wage distribution, including the top one. This is crucial for this study, as temporary contracts are those where mayors have a higher discretion over hiring and separation. Subsection 1.3.1.2 will provide a deeper discussion about differences in contract types.

Finally, it is crucial to notice the abrupt decline in female participation at higher quintiles of the wage distribution seen in public sectors across the world is also present in Brazilian municipalities.

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[4] 1 USD = R$1.7438, hence $ 293.10 as of January 2010

Female participation goes from a 68.9 % overrepresentation on the lowest quintile to an underrepresentation of 45.6 % on the highest one, a significant gap considering that women account for 52 % of the Brazilian population. Managers also display a significant gender gap: Only 41 % of them are women. This difference in composition is more striking when we look at characteristics within the managerial position (Figure 1.3). Female managers are clearly more educated than men: On average, female managers are more likely to have completed high school and superior education but less likely to have completed only elementary school or less. This suggests that a lack of qualified supply is not the main driver of the gender gap at a managerial position. Finally, it is worth noticing that despite being overqualified, female managers still face a wage gap of around 10 % even within the managerial position.

1.3.1.2. *Hirings and Separations.* Brazilian Municipalities are responsible for the provision of a broad range of public goods in areas such as health and education. Funding for this provision comes primarily from state and federal governments through direct transfers [38]. Consequently, not only do mayors have a lot of discretion for policy implementation and budget use, but local public sectors represent a large share of the total public sector employment: 56 percent as of 2014.

Selection processes for most public sector jobs generally follow a direct and objective, albeit slow, selection criteria called "Concurso Publico" (public civil service exams). First, applicants for a position present their credentials and undertake formal examination, which varies considerably across jobs and sectors, taking the form of written or oral tests. Candidates are then ranked based on grades obtained in these examinations, with the ones with the highest grades being offered the position. Public Servants hired through these civil service exams face considerable stability on the job: They acquire tenure after only three years on the position and can only be fired upon proof of severe misconduct.

To allow elected public officials to have some discretion over the hirings of members of their offices, the Brazilian constitution also allows hirings to be done without public civil service exams through temporary contracts. In the case of this type of contract, mayors have almost full discretion in the selection. This type of hiring is restricted to some top positions and is rather dynamic, as selection criteria don't have to follow any rules. Job stability is also non-guaranteed, with individuals in these positions facing separation laws similar to those in the private sector.

Figure 1.2 displays separations and hirings for the managerial position at the municipality level over the period of my analysis (2004 - 2018). There are three points to notice about this figure: First, most of the movement of separations and hirings is driven by managers with temporary contracts, displaying the dynamism of this type of contract. Second, hirings tend to happen in the first year of the electoral cycle (2005, 2009, 2013, 2017) and separations on the last (2004, 2008, 2012, 2016). This reinforces the idea that temporary contracted managers are associated with a choice of the elected politician, as their movement closely follows the electoral cycle.

Finally, managers who enter and exit the public sector through civil service exams also exhibit a small cyclicality. This is not unexpected; employees have discretion to leave the position anytime they desire and may choose to do so more frequently at the of an electoral cycle. Furthermore, although mayors have officially no discretion on who to hire on public civil service exams, they do have discretion on when to hold these examinations and when to hire. Hence the small cyclicality observed on the stable contract manager time series should not be seen as an issue.

1.3.2. Brazilian Electoral System. Brazil is a democratic country with all of its 5,570 municipalities being governed by a mayor elected every four years in direct elections. Municipalities with more than 200,000 registered voters feature a run-off in case no candidate receives a single majority in the first round. Elections are typically held in October with the elected candidate taking charge in office alongside members of its government on the first of january of the following year. Mayors are term-limited and are allowed to be in office for a maximum of two consecutive terms (eight years).

Given the system of direct transfers, mayors have a considerable discretion over the spending of municipality funds and hiring for board compositions. As in most areas of Brazilian politics women are extremely underrepresented politically on the mayoral position. On average in my sample, only slightly more than 26% of elections have a female candidate an only 9 % have a female winner. I will focus on the consequences of electing these mayors.

1.4. Data

1.4.1. RAIS. RAIS is an administrative dataset reporting all formal work contracts in Brazil, and it is managed by the Brazilian Ministry of Labor (MTE). As one of the main sources of data for the analysis of Brazilian labor markets, it provides information on wages, hours worked, hiring and

separation dates, demographic characteristics (gender, age, and education) for all formal private and public sector workers. I use this information to restrict my analysis to temporary managerial contracts where the employer is the local government.

Crucial to this study, each employer-employee match is classified into an occupation, which falls into one of the 2,511 categories (Classificação Brasileira de Ocupações 2002 (CBO)). I rely on this classification to define managers, and since this classification was created in 2002, I start my analysis on the subsequent year.

1.4.2. Electoral Data. I obtain publicly-available electoral records for the 2000, 2004, 2008, 2012 and 2016 municipal elections from the Brazilian Superior Court (TSE), the highest judicial body of the Brazilian Electoral Justice. This dataset provides information on election results, both for mayoral and local councils candidates. It also includes information on essential demographic characteristics such as age, race, and gender.

To conduct my analysis, I first exclude municipalities with a run-off. Using the information on gender, I then restrict my sample to municipalities with at least one female candidate. Then, I construct a variable that equals the the share of vote of the most voted female candidate minus the share of vote of the most voted male candidate to be used as the running variable in my analysis.Other methodological details are discussed on section **??**.

1.4.3. Complementary Datasets. I obtained two datasets publicly available from the Brazilian Institute of Geography and Statistics (IBGE) for auxiliary analysis. First, I use data from the Brazilian Census in 2000 and 2010 to construct my covariates. To evaluate gender-related policies, I use Perfil Município for the years of 2005, 2009, and 2013. This dataset is a policy survey sent to high public executives on local governments. In these particular years, the survey possessed an appendix asking additional questions related to gender policies.

I extract four particular indicator variables of interest from this dataset. First, a variable that indicates if a municipality has a local woman's rights council. In these types of councils, members of local public administration meet with ordinary citizens to discuss policies related to woman's rights. Although these councils have no legislative power, suggestions made here can be later implemented as policies by the municipality.

Second, I extract a variable that indicates if a municipality has a women's rights board. Boards are areas of the government responsible for implementing policies for a certain topic. Nonetheless, the definition of "area" is arbitrary. Local governments can report any group of people as a board, even if they are subjected to other administrative areas. Hence, I also extract a variable that reports boards if that administration has a board with an independent budget, which I interpret as a more objective indicator of an area of that government being committed to women's rights. Third, I also extract a variable that indicates if a municipality has a gender equality bill. These bills are official documents with clear policy goals to be adopted by the municipal administration. Although the implementation of these policies is not binding, the document itself can be interpreted statement of commitment to the gender equality cause. Finally, I match these indicators to mayors elected in the year previous to the survey.

I also use data from the System of Information about Public Educational Budget (SIOPE) and System of Information about Public Health Budget (SIOPS). SIOPE/SIOPS are federal administrative systems whose purpose is to record local educational and health funds spending. Crucially for this study, these systems record names for all heads of boards of education, health, and finance of municipalities for all years in my sample. Using these names, I predict the gender of these executives by comparing it to data on the number of females and males with the same name in Brazil from the IBGE's 2010 Census. I consider a name to be female or male when its frequency in the census related to that gender is higher than 90 % .

1.5. Methodology

Identifying the impacts of electing female politicians on labor market outcomes may be a challenging task. Simply comparing municipalities with a female mayor with those with a male mayor is likely to generate biased estimates due to endogeneity issues. For example, municipalities with a female mayor are likely to be more liberal, which tends to enact policies that reduce the gender gap. Ideally, to analyze the role of gender on the composition gap in managerial positions, we would like to randomly expose some municipalities to female mayors while keeping others unexposed. Unfortunately, this design is not feasible. Nonetheless, given the institutional setup in Brazil, it is still possible to use a regression discontinuity design to make causal inferences.

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To do so, I first restrict my sample to electoral races where only one female candidate is present. This is equivalent to 89 % of races in my final sample, and it is necessary to interpret the results as a close female victory versus a close female loss. I then calculate the margin of victory/loss between the female candidate and the most voted non-female candidate in a certain municipality. Using this margin of victory of a female candidate as the running variable, I compare the gender composition of managers in municipalities where a female candidate barely won versus a female candidate barely lost.

Formally, I am interested in estimating the difference in potential outcome in mixed-gender races, that is $E\left(\tau_{i,t}\left(1\right)-\tau_{i,t}\left(0\right)|i\epsilon\Omega\right)$ where $\tau\left(1\right)$ is the potential outcome if the mayor is a woman and $\tau\left(o\right)$ if it is not. Unfortunately, at a given point in time, we cannot observe both potential outcomes. That is the observed outcome is: $\tau_{i,t}=\tau_{i,t}\left(1\right)*F_{it}+\tau_{i,t}\left(0\right)*\left(1-F_{it}\right)$, where F_{it} defines the treatment status: $F_{it}=1$ if mayor is a woman, $F_{it}=0$ otherwise. Hence, our object of interest is the ATE $=E\left(\tau_{i,t}\left(1\right)-\tau_{i,t}\left(0\right)\right)$. We define treatment for mixed gender races:

(1.1) $$FemaleWinner_{it}=1\left\{MVF_{it}>0\right\}$$

Where MVF_{it} is the female candidate margin of victory in municipality i and electoral year t [5] and 1[.] is the indicator function. Hence, this variable will take positive values if the mixed-gender electoral race resulted in a female mayor winner and negative if it resulted in a male mayor winner. When $MVF_{i}=0$, the gender of the mayor F_{i} sharply changes from zero to one. If we then assume that potential outcomes are a continuous function of the running variable at the threshold, the LATE for our female sample will be:

(1.2) $$\gamma_{F}=E\left(\tau_{i,t}\left(1\right)-\tau_{i}\left(0\right)|\,MVF_{i}=0\right)=\lim_{x\uparrow MVF_{i}}Y_{i}-\lim_{x\downarrow MVM_{i}}Y_{i}$$

In order to estimate γ_{F} I use a local linear regression approach as proposed by [50], where I restrict our sample to municipalities in the interval $MVF\epsilon[-h,+h]$ and estimates the model:

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[5] MVF_{i} is specified as the vote share of the female candidate minus the vote share of the most voted male candidate

$$(1.3) \qquad Y_{it} = \beta_0 + \beta_1 MVF_i + \beta_2 FemaleWinner_t + \beta_3 MVF_i \cdot FemaleWinner_t + \mu_t + \varepsilon_{it}$$

Where the coefficient of interest is β_2. Following the most recent developments in the literature, I implement local polynomial Regression Discontinuity (RD) point estimates with robust bias-corrected confidence intervals and inference procedures developed by [21] [22]. To compute the bandwidth h, I use one common CER-optimal bandwidth selector for the RD treatment effect estimate. However, my main results are also robust to CER-sum bandwidths and MSE optimal selectors. Standard errors are clustered at the municipal level.

The identification strategy relies on the hypothesis that absent of treatment, the running variable would be continuous around the cut-off. To provide evidence in favor of this hypothesis Figure 1.4 shows a series of regression discontinuity estimates on municipality characteristics around the bandwidth specified on my main specification of section 1.6. With no estimate being significantly different from zero, I conclude that there is no evidence of a discontinuous jump around the cut-off for these characteristics. Figure 1.5 performs the same analysis but for local public sector characteristics on the year prior to the elections. Within the bandwidth of my main results, municipalities that elect a male mayor and those that elect a female mayor are not significantly different in terms of share of temporary contracts, the share of female workers, the share of managers, among others. Since municipalities on both sides of the cut-off are not significantly different in terms of public sector attributes and overall characteristics, I conclude that it is reasonable to assume smoothness around the cut-off in the absence of the treatment.

Furthermore, I also test manipulation of the running variable using a McCrary test, as suggested by [62]. Figure 1.6 displays the result of the test. I fail to reject the hypothesis of continuity of the running variable around the cut-off. This test, along with the balance test of my covariates, increase the confidence that my estimates are respecting the necessary conditions for a causal effect interpretation.

1.6. Results

In this section, I present my results for the analysis of the consequences of electing politicians on female managerial share. Section 1.6.1 presents my main results, discusses its interpretations, and examines robustness checks. Section 1.6.3 analyzes the role of possible public sector specific mechanisms in explaining my results. Finally, section 1.6.2 investigates other mechanisms that are not unique to the public sector in explaining results.

1.6.1. Main Results.

1.6.1.1. *Female Managerial Composition.* Table 1.2 displays local linear regression discontinuity estimates for the impact of winning a mixed-gender election on the share of female managers [6] at the end of the first year of government at the municipality level. Odd columns report results for linear specifications; even columns report results for quadratic specifications. When using a general Convergence Error Ratio bandwidth selector method, a woman's victory in a close election is associated with a 7.0 percentage point increase in female managers' share. The magnitude of the effects are meaningful: 17.5 % increase in the share of female from a mean of 40.6 % or 70.9% of the 9.4 percentage points composition gap. This effect can also be seen in the total number of female managers, as displayed in Table 1.5, suggesting that the driving mechanism is not simply a disproportionally shrink in government size.

As a robustness check, column three displays the estimates one common CER-optimal bandwidth selector for the sum of regression estimates. The bandwidth size is very similar to the original CER selector. Once again, a woman victory in a close election is associated with a 7.0 percentage points increase in the share of female managers, a magnitude similar to the original CER case. The negligible difference between estimates is reassuring that the bandwidth selector algorithm is not driving both magnitude and significance of my results.

A larger difference between estimates is found when we compare different functional forms. A quadratic local linear estimate leads to an 11 percentage points estimate. Even though this estimate has a slightly greater magnitude than the linear estimate, an intuition for this difference can be found in Figure 1.7. Data appears to follow a linear path around the cut-off. Hence, a

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[6]More precisely: total female managers with temporary contracts with the municipality/total managers with temporary contracts with the municipality

quadratic polynomial tends to overestimate the impact of a female mayor. For the rest of this study, I will report local linear and quadratic estimates. Nonetheless, results are generally robust to other polynomials (and bandwidths), and additional robustness checks can be found in Figure 1.8.

To test if my estimates are indeed capturing the causal effect of a female winning a close race on the share of female managers, I estimate the effect of a female victory on the managerial share one year before the election. Finding null effects increases the validity of my estimates. Table 1.3 follows the exact structure of table 1.2 but shows estimates for the female managerial composition one year before the election. None of the specifications display significant estimates, and the magnitude and signs differ from post-election effects. This table should be interpreted as suggestive evidence that my regressors are not capturing a spurious relation.

Figure 1.9 presents a broader way to make use of time on the analysis of effects. I present estimates for effects of electing a female politician on a close race on the share of female managers from 3 years prior to the election to 4 years after it. For the pre-election period, no estimate is significant at 95 % confidence level, with some being negative. The electoral year, t = 0, sees a positive estimate. Since this estimation is made for a period post-election and represents the share of female managers at the end of the previous administration that will continue to the next administration, one shouldn't necessarily expect it to be zero.

At the end of the first electoral year, t = 1, we see the effect already discussed in Table 1.2. At this point, the new administration already had one complete year to make hirings and separations. An estimate of similar magnitude and size can be seen for the second year of the administration, a result that increases the causal interpretation of these results. For the third and fourth years, results are still positive albeit non-significant. Considering the positive effects on the last year, it is possible that electing a female politician has an impact that extrapolates the term of the elected mayor. A exlpanation for this decline in estimates on the third year is that managers are anticipating the end of the government by leaving one year early. Another theory is that female managers leave to participate in state-level positions since gubernatorial elections take place two years after local ones. One way or another, the effect on the first two years is clear.

Next, I evaluate if characteristics associated with being a female mayor are driving the results. Since male and female politicians differ in terms of both observable and unobservable characteristics,

it is relevant to investigate if results are driven by characteristics that are associated with gender in this context. For example, anecdotal evidence suggests that female politicians are more educated and tend to be associated more frequently with left-leaning parties. To evaluate this question, I use a regression discontinuity model with the same running variable (margin of votes) but replace the outcome variable by an indicator that takes the unitary value if the winner mayor possesses a certain characteristic.

Results for this analysis can be found in Figure 1.10. Characteristics are grouped into three categories: Educational, Marital, and Political. Winning female mayors are very similar to winning male mayors in most observable characteristics: Politically, close female victories are not associated with a significantly higher probability of the winner belonging to any of the major Brazilian political parties.[7] In terms of marital status, these two groups also do not significantly differ in the probability of being single, married or having other marital statuses.[8]

Closely elected male and female mayors present different educational characteristics, where close female winners are almost 20 percentage points more likely to have a college degree. To evaluate if this is a driver of my main results, I include this (and other characteristics) as a control in my main specification. When I include only educational characteristics, point estimate drops to .5 percentage points. This result is still significant at 10 % significance level, which suggests that education may be responsible for explaining part of the effect of the increased female managerial composition. However, most of the effect still seems to be explained by pure gender differences.

Two follow-up questions of interest are: (1) How meaningful is this effect for the entire public sector? And (2) Does this increase in representation at the top generate a trickle-down effect? Table 1.4 attempts to answer these questions. When I analyze the composition of all temporary workers, including managers, we see that the close election of a female mayor is associated with a 3.3 percentage points increase in female composition (column 1, panel A). This effect is considerably smaller than the effect on managers (described in table 1.2), yet significant at 10 % significance level. When we analyze the effect by quintiles of the wage distribution, we see that all quintiles exhibit positive coefficients. These coefficients are statistically significant for the second, fifth quintiles and the entire distribution. When we managers are excluded from the analysis (Panel B), results

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[7]Major political parties in Brazil defined as PT, PSDB, PMDB.

[8]Even though one may find these results surprising considering how males and females differ in these characteristics on the Brazilian population, these results are similar to the ones reported in [3]

lose significance and considerable change magnitude (from 0.041 to - 0.001 p.p.), suggesting that managers mainly drive the effect on the top quantile. In other words, effects for the managerial position are so meaningful that they completely drive results for the fifth quintile and significantly affect the result for the entire public sector. These results are also relevant to mitigate concerns of the relevancy of managers in this context: Even if one is still skeptical of the role played by managers on the local public sector in Brazil, electing a female politician has an impact on the entire distribution.

Results may lead one to conclude that closely electing a female politician leads to a trickle-down effect, where female mayors hire more female managers and these managers hire more female employees. Nonetheless, Panel B may show that this interpretation is precipitated. Effect for the entire public sector, as well as the first four quintiles, remain positive, albeit non-significant. Hence, despite these results suggesting that there may be an effect, it is impossible to statistically claim that electing a female politician leads to a trickle-down effect.

Having established that a female victory on a close election is associated with a significantly, both statistically and economically, a higher share of female managers, it is important to discuss the implications of this result for the causes of the gender composition gap. First, I interpret this result as evidence that mechanisms that imply that a change in leadership would not lead to a change in composition in top positions are not binding in this case.[9] Do these results necessarily consist of evidence of taste-based discrimination? Although a similar claim has been made elsewhere in this literature, I take a more conservative approach, and on sections 1.6.3 and 1.6.2 I analyze several different mechanisms that could explain these results.

1.6.1.2. *Effects on Managers Quality.* A general concern of the affirmative action literature is that a focus inequality may come at a cost in terms of the quality of employees. For example, female mayors could give preference to managers from their own gender even if they are not the most qualified for the position, promoting equality but reducing managerial quality. This quality reduction is unlikely in this case, given that, in my sample, women managers are on average more educated than their male counterparts (Figure 1.3). Yet, the pool of candidates may differ from

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[9]For example, career interruptions due to childbearing may limit women's professional advancements ([**13**]), this, in turn, may lead to a limited supply of female candidates in top positions. We may also observe limited supply because women may shy away from competition for promotions ([**66**]). However, if limited supply is the cause we observe a gender gap in top positions, changing a mayor should not directly reduce the gap.

managers actually hired, leading the overall effect to be unknown. Given that manager is a high-skill occupation, an obvious choice as a proxy to quality is to use educational composition.

Table 1.6 - Panel A displays estimates for the impact on the educational composition of all employees. On average, managers are less likely to have completed elementary school or less by 3.2 percentage points on governments where a female mayor had a close victory. They are also more likely to have at least a high school degree but less likely to have at least a college degree. None of these coefficients are significant, and magnitudes also indicate that there seems to be no reason to believe that the narrowing of the gender gap is coming at the cost of efficiency for managerial employees.

One possible concern with the analysis presented in Panel A is that female mayors are favoring female managers, but this effect is averaged across all managers leading to null effects. To mitigate these concerns, Table 1.6 - Panel B displays estimates for the impact on the educational composition of female employees. On average, women are less likely to only have completed elementary school or less by 1.2 percentage points on governments led by a female mayor. They are also more likely to have graduated college but less likely to have graduated high school. None of these results are significant, providing further evidence that, in this context, there is no reason to believe that female leaders are favoring women in detriment to the quality of public service provision.[10]

To conclude my analysis of managerial quality, I examine the impact of gender on the probability of being reelected. This analysis assumes that if managers hired by female mayors lower the quality of public service provision through managers, female mayors should face significantly lower reelection probabilities. One limitation of this analysis is that it is possible that despite managers providing lower-quality public goods, female mayors are still able to make up in other areas to the extent that there are no electoral consequences of these managerial practices. However, this is unlikely to be the case if managers are extremely less qualified. Table 1.7 displays the analysis of reelection probabilities. The dependent variable is an indicator variable for reelection, and the running variable is still the margin of votes of the most voted female candidate. A female victory is

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[10]These results also mitigate concerns that results are driven by gender asymmetries in patronage. [32] have already shown that Brazilian mayors engage in patronage by hiring managers with temporary contracts. If female mayors are more likely to engage in patronage than their male counterparts, electing a female mayor could reduce the gender composition gap at the managerial position. However, if this mechanism was the main driver of results, one would expect the quality of female managers to go drastically down with the election of female employees. Furthermore, [19] suggests that if anything, women are less likely to engage in strategic behavior. I analyze other public sector specific mechanisms in section 1.6.3.

associated with a non-significant 3.6 % decrease in reelection probability. This analysis, along with the previous ones, at least suggests that even if there is an impact on the quality of employees, it is unlikely to be one of a meaningful economic magnitude.

1.6.2. Supply and Demand Mechanisms. As mentioned previously, it is precipitated to interpret the reduction in the gender composition gap as direct evidence of demand side preferences. This section examines if supply side or demand side could explain the reduction in this gap.

1.6.2.1. *Demand Side Preferences.* If male mayors hire more male employees because they prefer working with managers of their own gender, switching to a female mayor may reduce the composition gap. Alternatively, suppose women are indeed being discriminated against in the Brazilian public sector, and female mayors are willing to remediate this issue. In both cases, one expects female mayors to also act against discrimination in other areas. To analyze this mechanism, I examine if female mayors are more likely to implement gender equality policies. This shouldn't be interpreted as a sufficient condition for taste-base or statisticaldiscrimination. Finding that female mayors implement more gender-inclusive policies does not imply that previous results are driven by taste base discrimination. Yet, finding that women are not more likely than men to pass such policies would provide suggestive evidence that taste-based discrimination is not the driving mechanism. In this sense, one can interpret gender-inclusive policies as a necessary but not sufficient condition for taste-based discrimination.

To make this analysis, I use data from the municipal government policy survey described in section 1.4.3. I present results for the probability of observing four types of gender policies on local governments: Creation of women's rights council (Panel A), creation of a woman's rights right board (Panel B), creation of a woman's rights board with independent budget (Panel C) and creation a municipal plan for gender equality (Panel D). Table 1.8 follows the same structure as others in this article.

First, it is important to notice that all coefficients are positive, suggesting a positive relation between electing a female politician and the probability of observing these gender policies. Panel A shows the probability of observing a women's rights council one year after the election. A close female victory is associated with an average of 10.8 percentage points, or a 57.4 % of the mean, increase on the probability of seeing one of these councils post-election.

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Panel B shows effects on the probability of observing a woman's rights board. All specifications display positive, albeit non-significant results. It is possible that women do not create more gender-related boards than men. Another possibility is that given the lack of precise definition of the word "board" in this context and the self-declared nature of this survey, mayors report any organization related with gender equality as a "board". Panel C displays more objective ways to define a board, that is, an organization within the local governments with their own budget. In this case, we see that a close victory by a woman is associated with a significant increase of 5.3 percentage points, or 91 % of the mean, in the probability of observing these boards.

Finally, I analyze the probability of observing a municipal plan of gender equality. A close victory by a woman is associated with an increase of 1.98 percentage points, or 37.8 % of the mean, of observing such policy. Point estimates are large, but the coefficient is not significant due to large standard errors, independently of the specification chosen.

There are three ways to interpret these results. First, one can conclude that increase in demand comes from the need of female workers to participate and implement these councils and boards. This interpretation is unlikely for two reasons: First, while boards with their own budgets could potentially use this budget to hire more female managers, councils don't have this power, as they only serve as organizations where members of the elected government and citizens discuss and suggest policies. Hence, it is unlikely that a larger share of municipalities with councils is hiring more managers to work with gender policies directly. Second, gender boards, organizations that potentially would demand new managers, are present only on a small share of municipalities (5.81 %), making it unlikely to drive the demand. Hence, I interpret these results as suggestive evidence that the increase in the share of female managers is a consequence of a stronger desire of female mayors to create a more gender-inclusive government.

An alternative way to interpret these results is as evidence of the presence of discrimination in a municipality, and female mayors' commitment to reduce it. If male mayors discriminate and female mayors act on a way to try to reduce gender gaps, or even simply do not engage in discrimination we expect to see an increase on the female share of managers after the election of a women. Nonetheless, these results are also compatible with a gender homophily theory. Homophily refers to a tendency of various types of individuals to associate with others who are similar to themselves ([34]), and it may

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impact hiring decisions through networking. If this association makes women care disproportionally more about gender inequalities, then homophily may also lead to an increase in policies in that field.

To detangle between these two demand-side channels I use data of gender of different boards' heads. Assume that discrimination by male mayors is the main reason for the increase in share of female managers when a female mayor is elected. Then we would expect to see a larger increase in areas in which there is a stronger stigma against women (finance, comptroller general) and no effects in areas women are historically overrepresented (health, education). If homophily is the reason why we see a higher share of females among managers then we would expect to see positive for health and education.

Table 1.9 presents this analysis. Column (1) shows results for the increase in probability of observing a female as the head of the local board of education. Columns (2), (3), (4) perform the same analysis for the board of health, finance and municipal comptroller general. A female close victory is associated with a significant 10 percentage points increase in the probability of observing a female head of board of education and a significant 20 percentage points increase in the probability of observing a female head of board of health. In comparasion, no significant effects are found for head of board of finance or municipal comptroller general, areas in which woman are underrepresented by more than 20 percentage points .

Given that effects are oberved in areas where stigmatzaion of female abilities, and likely discrimination, is relative smaller, I interpret these results as suggestive evidence that homophily, and not discrimination, is the main channel driving my results. However, it is important to recognize the limitations of this type of analysis. Since heads of boards are a subset of managers, these two samples can be different and effects may differ accross them.

Another interesting result of Table 1.9 is the estimate for head of board of finance and comptroller general. In the case of both an homophily and discimination based increases, we would expect these coefficients to be positive. One possible explanation for these null results are sector-specific limitation in supply. That is, if mayors prefer to hire or promote heads within their networks but they are not enough qualified female applicants, it is possible that the hiring will happen within the opposite gender. The same logic can be applied for the comptroller general position. Either way, anaylsis in this section finds suggestive evidence that necessary conditions for demand side

mechanisms, especially homophilic networks, to be present. I analyze theories that could lead to a supply side shift in the next secion.

1.6.2.2. *Supply Side Preferences.* One reason why we might observe an increase in the share of female managers may be supply-related preferences. For example, suppose women find it more desirable to work for other women. In that case, a female mayor may increase the number of applicants for positions in that given government. In this case, even if the demand or the hiring process is completely indifferent to gender, we would see a narrowing of the gender composition gap. This mechanism is particularly relevant as supply has already been pointed by theoretical models as one of the reasons for women overrepresentation in other areas of the public sector ([42])

To test this hypothesis, I look at stable public servants. As previously mentioned, once hired, this type of employee quickly obtains tenure and faces considerable stability. Given the inability of municipal governments to unilaterally separate jobs of public servants with this type of contract, I analyze the gender composition of continuing workers with this type of contract at the end of the first year of the electoral cycle. Table 1.10 shows this analysis. When using a linear specification, a close female victory is associated with an increase in the share of continuing female public servants with stable contracts of 4.3 percentage points. The estimates' magnitude is smaller for the quadratic form, columns (3) and (4) display results for a CER-sum bandwidth. In all cases, albeit positive, estimates are not significant and with a magnitude between one-third and half of the main result, showing that this channel cannot explain the larger part of the estimated effect.

These results seem to suggest that it is unlikely that a supply shift fully explains the increase in female share. It is important, however, to consider two issues on the interpretation of these results. First, the positive sign may capture the decisions of promoting already stable public servants to managerial positions and reporting them as still on the same contract. This could lead to positive estimates if we believe that women are disproportionally more likely to promote women. Second, this is a different category of workers, particularly less dynamic than managers with temporary contracts. It is possible that women still prefer being part of governments led by women, but this preference is not strong enough to impact the decision to change jobs or retire once on the job. One way or another, the small non-significant magnitude of the estimates indicates that while a small supply effect may exist, it is unlikely to be the main driver of results.

1.6.3. Public Sector Specific Mechanisms. In this section, I evaluate if different public sector specific mechanisms could drive the increase in female managers' composition. My analysis show that differences in strategic behavior, or public goods provision are unlikely to fully explain results.

1.6.3.1. *Differences in Strategic Behaviour.* Next, I evaluate if strategic could potentially explain these results. It already has been shown, for a subsample of the data used in this study, that women hire fewer overall temporary workers due to their lower engagement in strategic behavior ([19]). If male mayors hire more temporary managers due to strategic behavior and these managers are more likely to be male, strategic behavior could explain the results. Finally, I analyze if female managers have fewer total temporary managerial workers on table 1.5 and if this possible reduction in total temporary manager workers is what leads to an increase in the share of female workers. I find that on average, female lead governments have 7.13 more temporary managers per ten thousand habitants. One way to conciliate this with [19] is that even though female mayors may have fewer total temporary workers, they employ more total temporary managers. I conclude that strategic behavior is unlikely to explain the increase in the share of female temporary managers.

1.6.3.2. *Differences in Public Goods Provision.* Another public sector specific mechanism of interest concerns the increase of demand for areas of public goods provision that are female-dominated. There is broad evidence that men and women act differently as policymakers, especially regarding good public provision. For example, women policymakers are associated with higher educational ([31]) and health outcomes ([15], [19]). If to do so, women hire more employees in these areas, and employees in these areas are disproportionally more likely to be female; an increase in the supply of public goods in these areas by local governments may lead to an increase in demand for female workers, including managers. Unfortunately, we do not observe the area of managerial workers. I do observe, however, the area of other public sector workers. If the female mayors really hire more managers in these areas, you would expect to see an impact not only for managers but also for other workers. I will analyze this channel next.

Table 1.11 show the effect of a close female victory on the share of workers of areas that previous literature has already shown to be disproportionally impacted by a women victory: education and healthcare. Columns 1 and 2 show the effect on the share of workers in this area respective to

the total number of workers at the end of the first year of government. Columns 3 and 4 perform the same analysis but for the logarithm of the total number of workers. Not only are coefficients non-significant but also negative, suggesting that although previous literature finds an association between female mayors and an increase in quality of these public services, this doesn't necessarily translate into more workers in this area. Hence, similar to other public sector specific mechanisms, differences in public service provision are also unlikely to explain the increase in female managerial composition.

1.7. Conclusion

This article analyzes the underrepresentation of women in top positions. In Brazil, as in most countries in the world, women are underrepresented in top positions in the public sector. To the best of my knowledge, this is the first work to have exogenous variation on the leadership position and find that changes in leadership have significantly reduces the gender composition gap in top positions.

I present evidence that electing a female mayor in a close electoral race increases the share of female managers by 17 %. This result is robust to a series of falsification and robustness checks. The positive impact on the female share can, in fact, be extended to the entire top quintile of the public sector distribution. I show that there is no evidence that this decrease in the gender composition gap comes at the cost of the quality of managers: A close female victory leads to managers who are as just as educated as the managers in a close male victory.

I show that supply-side preferences are unlikely to be the main driver for the results, as stable public servants do not significantly choose to supply more work for female-led government. I also present evidence that this result is unlikely to be explained by gender-specific mayoral characteristics or by increases in demand for managers in sectors where women are overrepresented. Instead, a demand-side preference channel in which a female mayor demands more female managers due to homphilic preferences is supported by my data. These results also point to the potential of sector-specific supply limitations to reduce the leaders' impact.

These results have direct implications for the problem of female underrepresentation in top positions. First, to the more than 50% of countries in which women are underrepresented in public sector managerial positions, it suggests that that promoting female leaders, through either quotas

or elections, may reduce the gender composition gap. Second, results also imply that these gains in equality come at no cost of quality. Third, to the private sector, it suggests that policies that increase female presence in leadership positions with discretion, such as certain gender board quotas or female entrepreneurship promoting policies, may have positive spillovers to top executives. Finally, results also show that despite these positive spillovers, policymakers shoul not abandon policies that target sectors in which women are underrepresented, as supply limitations can reduce leaders' impact.

Figures

FIGURE 1.1. Average Share of Female in the Public Sector by Occupation type
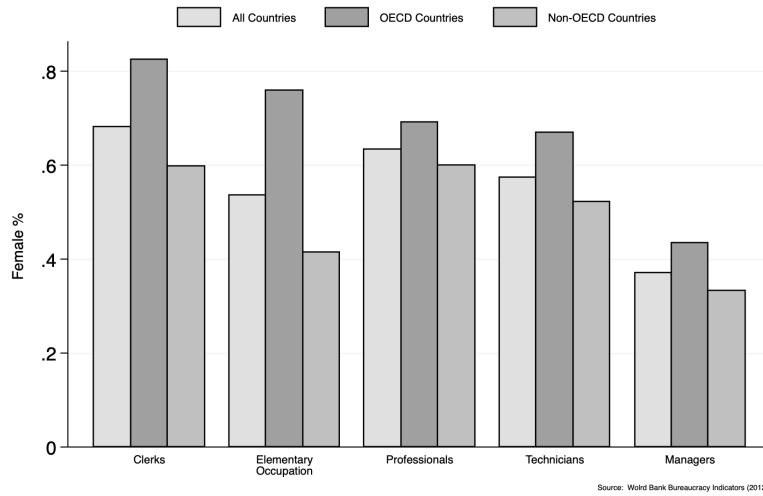
Note: Figure displays average share of female in national public sectors for OECD and non-OECD countries for 2012. OECD partner countries included as non-OECD countries.

FIGURE 1.2. Hirings and Separations by Contract type per Year
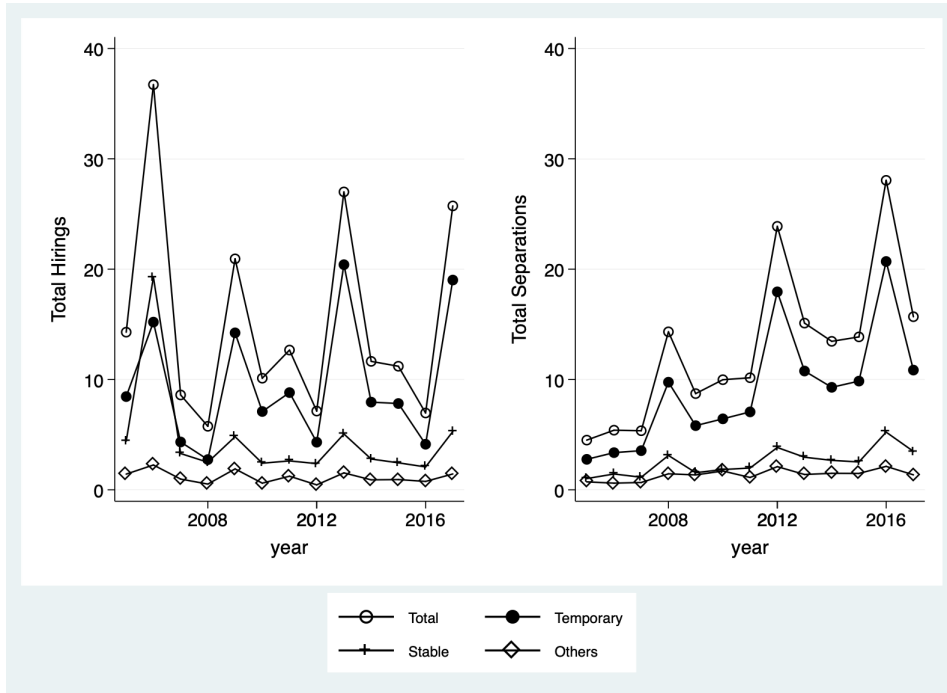
Note: Figure displays total hirings and separations of managers by contract type per year. Stable contracts defined as RAIS contract type code 30 or 31.

FIGURE 1.3. Managers Characteristics by Gender
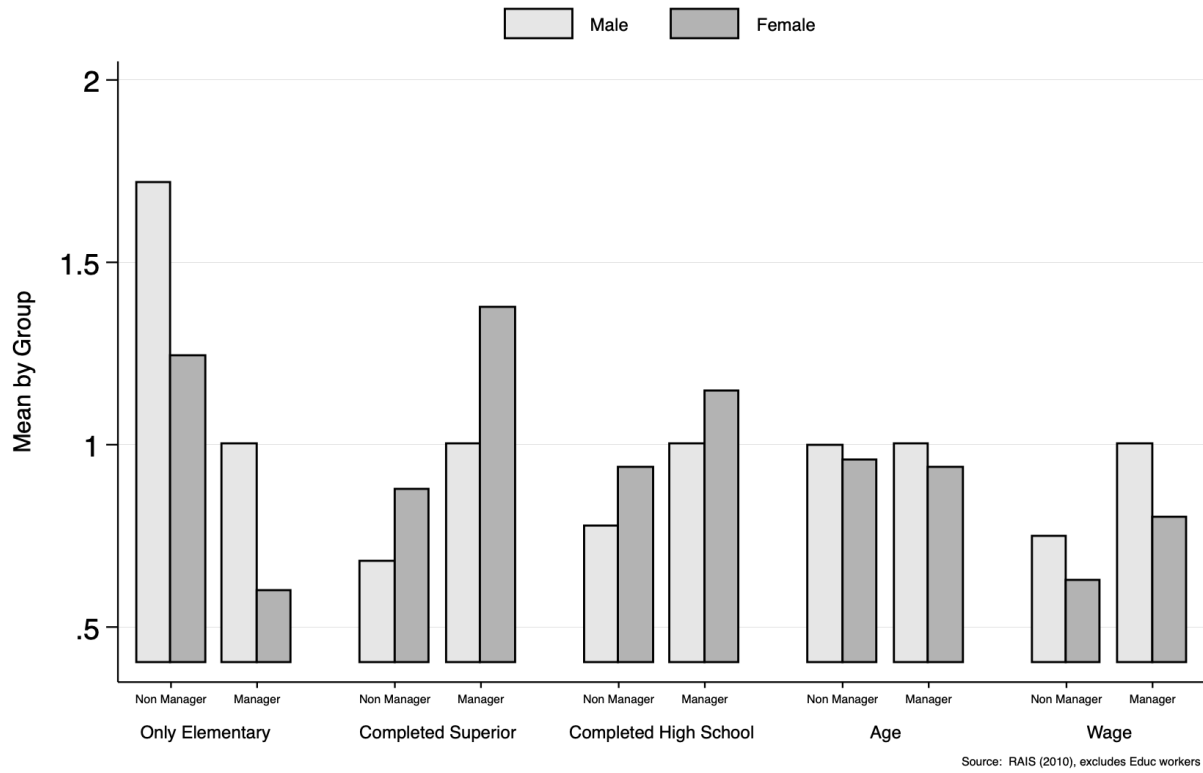
Note: Figure displays mean characteristics by group, using male managers as reference. Figure excludes educational workers, who tend to be overrepresented in municipalities since elementary education is a Municipality competency in Brazil.

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FIGURE 1.4. Smoothness around the Cut-off for Covariates at Municipal Level
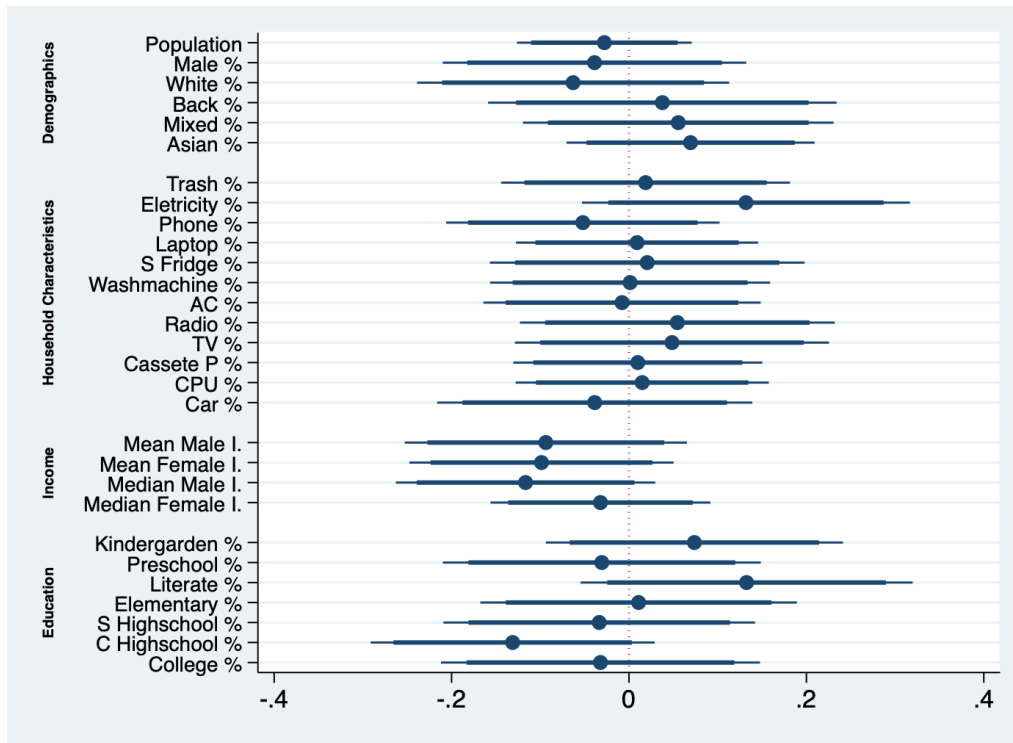
Note: Figure displays results obtained by estimating RD models with margin of win of the female candidate as running variable on different municipality level characeristics. Models use Triangular Kernel, Linear Polynomial and bandwidth selected using methodology proposed by [**21**], [**22**] for main results, [-0.106,0.106] . Standard errors clustered at municipality level. Thin lines indicate 95 % confidence intervals, thicker lines indicate 90 % confidence intervals.

FIGURE 1.5. Smoothness around the Cut-off for Covariates at Local Public Sector
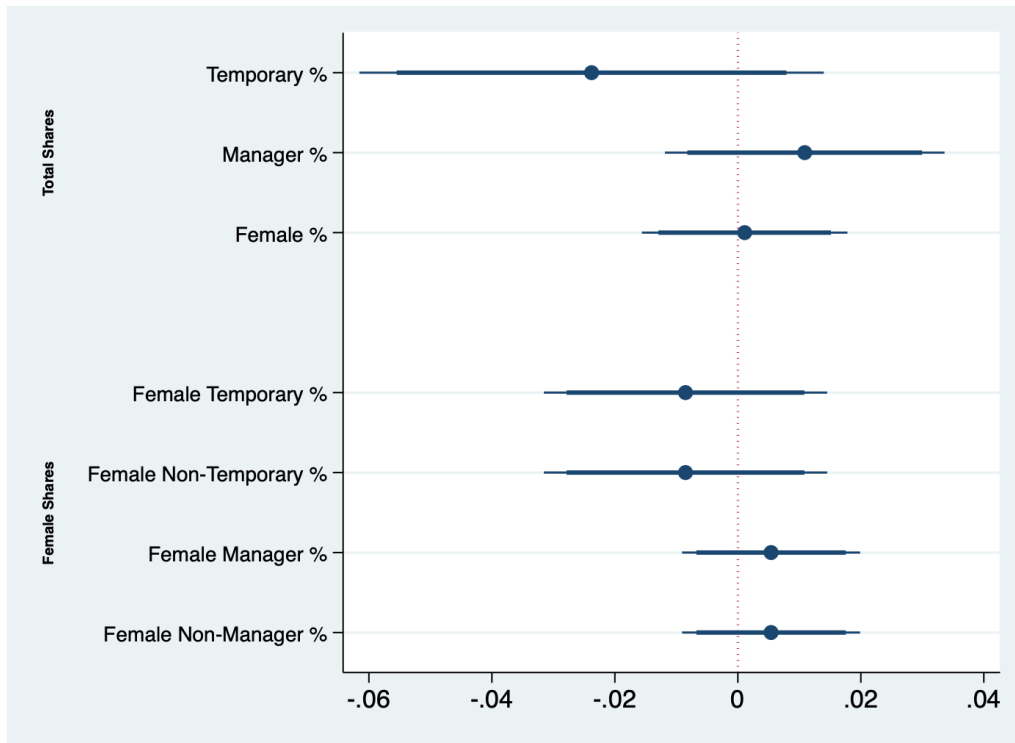
Note: Figure displays results obtained by estimating RD models with margin of win of the female candidate as running variable on different local public sectot level characeristics. Models use Triangular Kernel, Linear Polynomial and bandwidth selected using methodoly proposed by [**21**], [**22**] for main results, [-0.116,0.116]. Standard errors clustered at municipality level. All percentages calculated over total number of local public sector employees. Thin lines indicate 95 % confidence intervals, thicker lines indicate 90 % confidence intervals.

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.	.5182089	.6461088
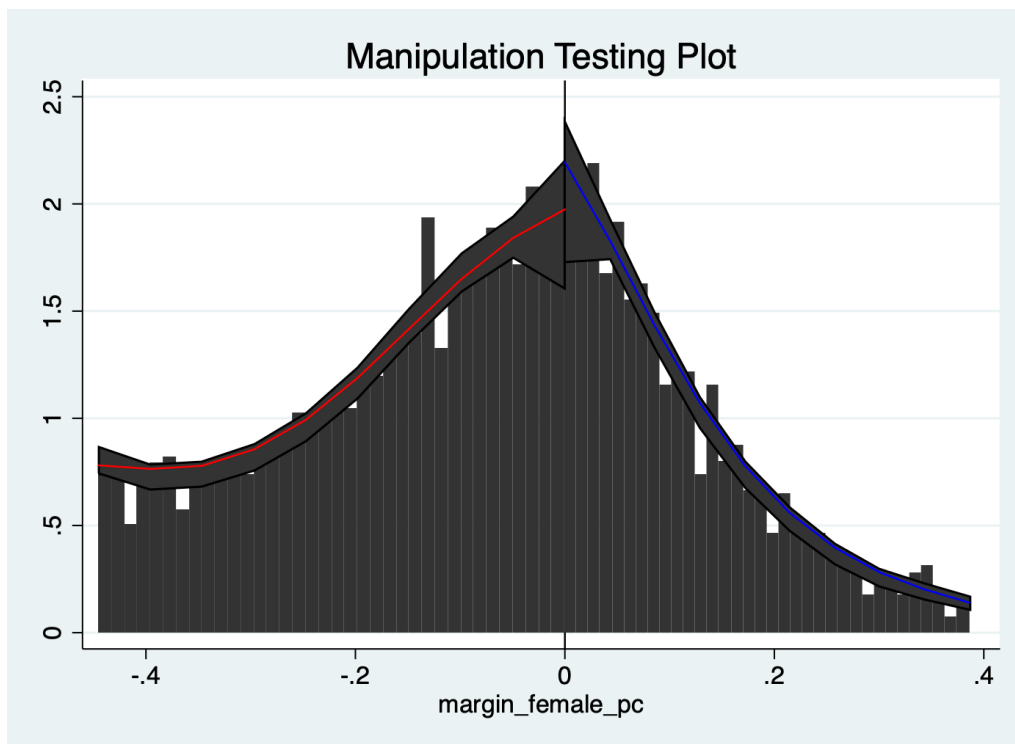
FIGURE 1.6. Mccrary test

FIGURE 1.7. Regression Discontinuity using CER Bandwidth
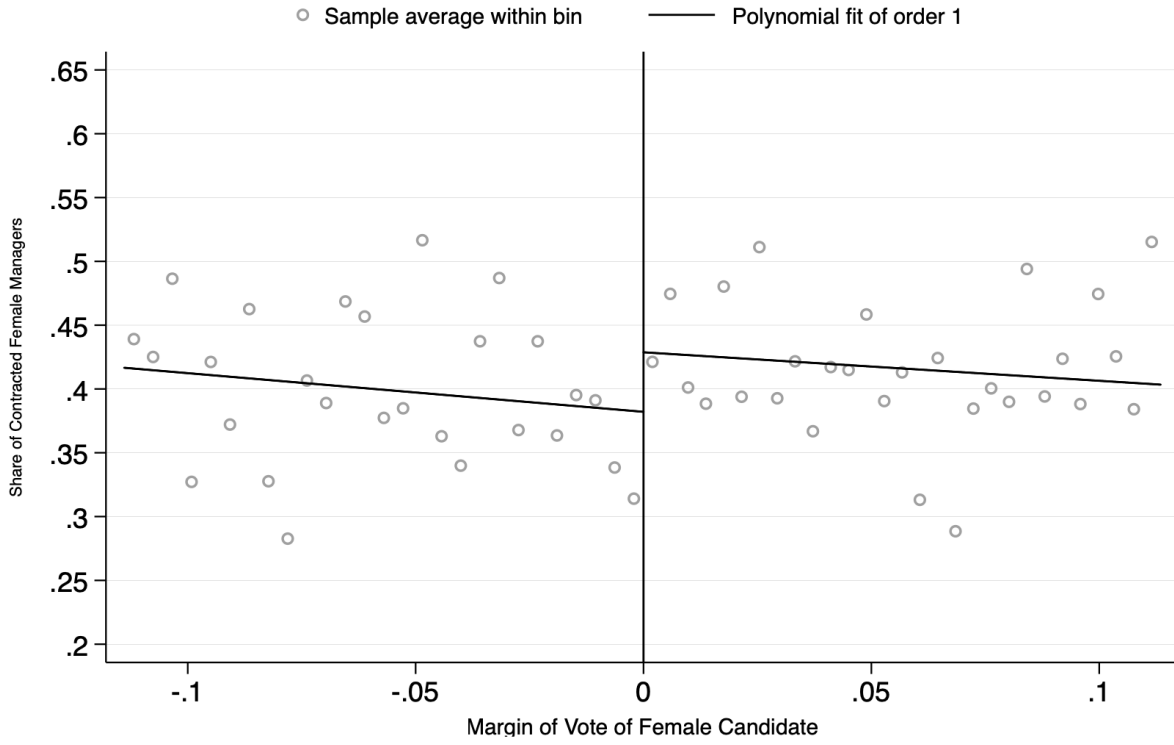
Note: Figure displays results obtained by estimating RD models with margin of win of the female candidate as running variable on share of female temporary managers as dependent variable. Models use Triangular Kernel, Linear Polynomial and bandwidth selected using methodology proposed by [**21**], [**22**] for main results, [-0.116,0.116]. Standard errors clustered at municipality level. All percentages calculated over total number of local public sector employees. Thin lines indicate 95 % confidence intervals, thicker lines indicate 90 % confidence intervals.

FIGURE 1.8. Robustness Check for Estimates
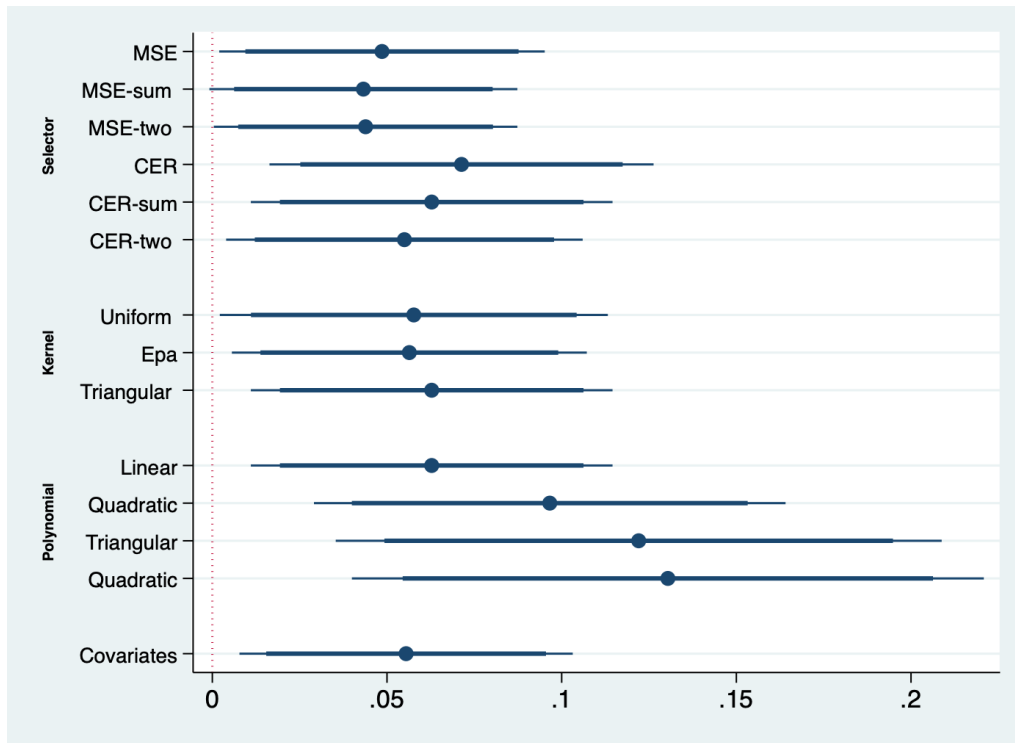
Note: Figure displays changes in specifications to the prefered model that uses CER selector, Triangular Kernel and Linear Polynomial. For example, uniform kernel uses CER selector, Linear Polynomial but uniform Kernel. Standard errors clustered at municipality level. Thin lines indicate 95 % confidence intervals, thicker lines indicate 90 % confidence intervals.

FIGURE 1.9. Effects Over Time
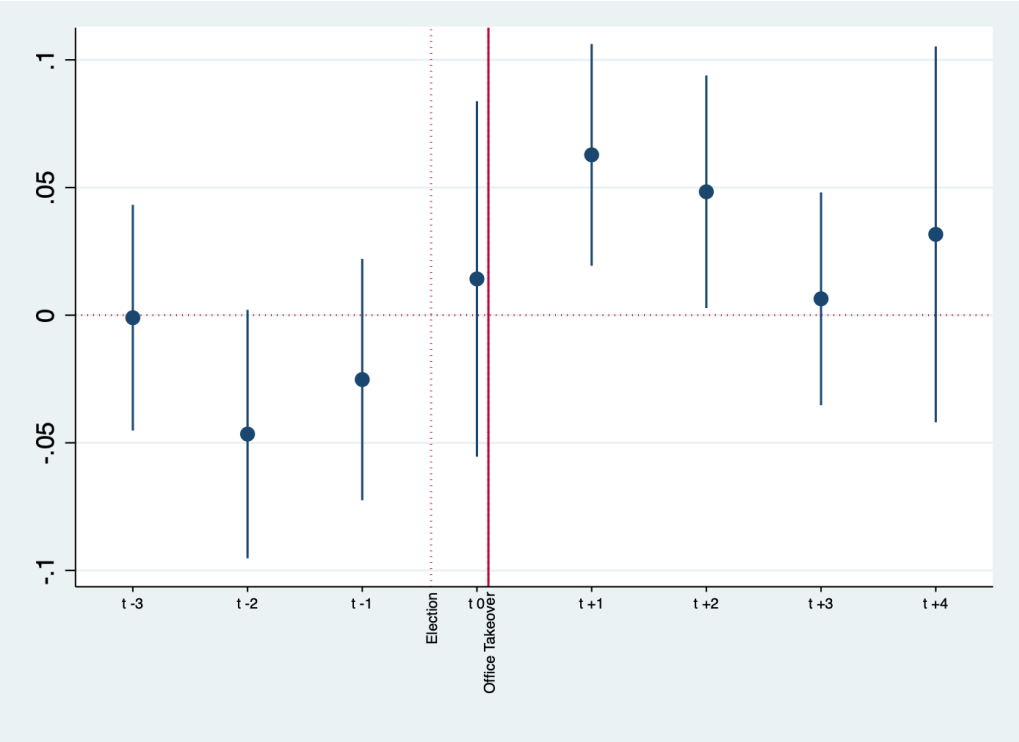
Note: Figure displays yearly effects of electing a female mayor on share of female temporary managers using electoral year as t = 0. Share of Female Managers calculated using the stock of contracts on 31st of December of each year. Standard errors clustered at municipality level. Lines indicate 95 % confidence intervals.

FIGURE 1.10. Characteristics associated with a female win
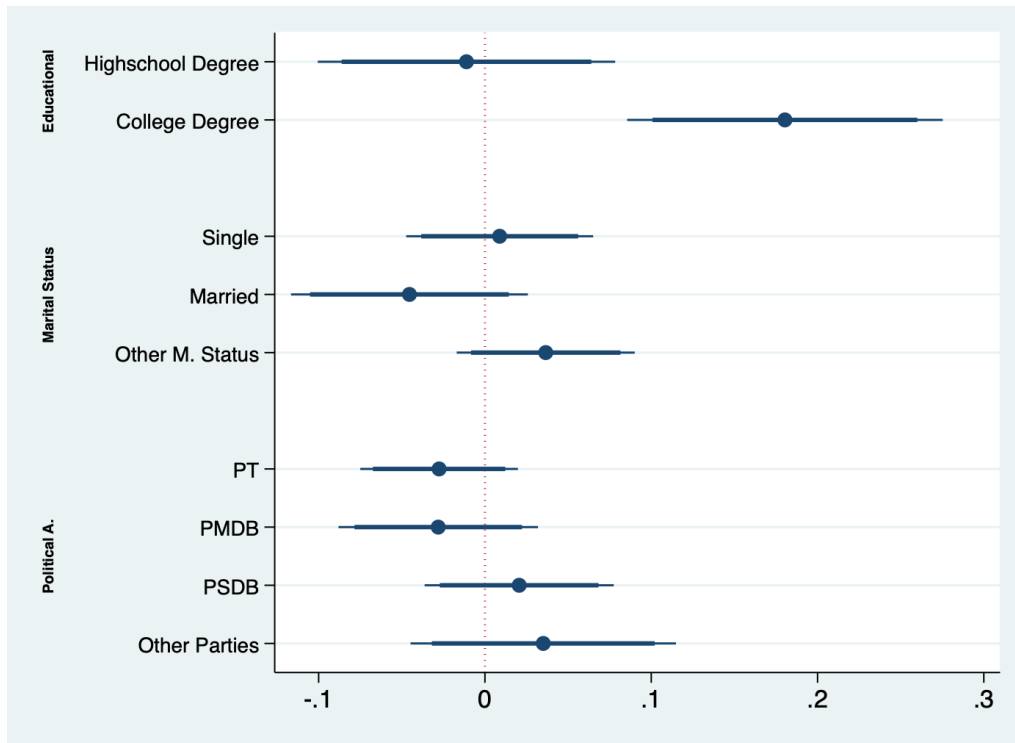
Note: Figure displays RD estimates using margin of votes of the female candidate as the running variable and different indicator variables for educational, marital status and political characteristics as dependent variable. Thin lines indicate 95 % confidence intervals, thicker lines indicate 90 % confidence intervals. Standard errors clustered at municipality level.

FIGURE 1.11. Estimates Sensitivity to inclusion of politicians characteristics
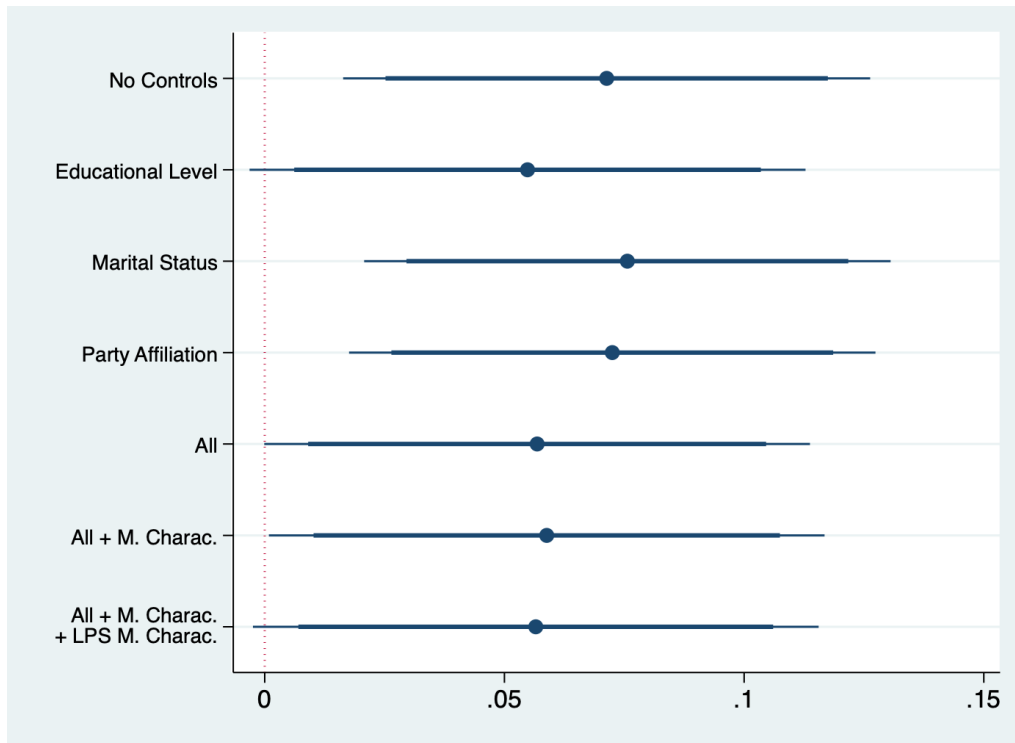
Note: Figure sensitivity to the inclusion of individual politician characteristics. All models estimated using CER selector method, triangular kernel and linear polynonmial. Standard errors clustered at municipality level.

TABLE 1.1. Managers Compared to Quintiles of Wage Distribution on the Public Sector

	Managers	1st Q	2nd Q	Wage Quint.. 3rd Q	4th Q	5th Q
General:						
- Wage (BRMW)	3.830	1.043	1.349	1.637	2.130	4.315
- % Female	0.414	0.689	0.626	0.506	0.435	0.456
- Age	41.710	38.273	40.435	41.311	41.645	41.570
- % Manager	1.000	0.053	0.047	0.053	0.067	0.141
- % Full Time	0.838	0.844	0.851	0.839	0.819	0.758
Education:						
- % Elementary Comp. or less	0.155	0.430	0.474	0.444	0.371	0.162
- % HS Comp. or more	0.807	0.507	0.469	0.503	0.578	0.808
- % at least Superior Comp. or more	0.313	0.043	0.047	0.070	0.133	0.457
Contract Type:						
- Stable	0.377	0.634	0.720	0.734	0.727	0.636
- Temporary	0.553	0.218	0.157	0.157	0.174	0.257
- Other	0.070	0.148	0.123	0.109	0.099	0.107
Years on Position:						
- < than 1	0.191	0.211	0.126	0.111	0.101	0.136
- 2 to 5	0.539	0.431	0.332	0.306	0.295	0.348
- > than 5	0.270	0.359	0.542	0.582	0.604	0.515

Note: Table displays mean of municipalities average for each variable for the year of 2010. Columns (2) - (6) indicate quintiles of wage distribution in the local public sector. Stable contracts defined as RAIS contract type equal to categories 30 or 31. Full-time workers defined as those with contracts with more than 40 work hours a week. Wages displayed as a function of Brazilian federal minimum wages.

TABLE 1.2. Impact on Share of Female Managers

	(1) % Female	(2) % Female	(3) % Female	(4) % Female
Win	0.070**	0.125***	0.070**	0.081**
	(0.029)	(0.042)	(0.029)	(0.033)
Mean	0.406	0.406	0.406	0.406
Selector	cerrd	cerrd	cersum	cersum
Polynomial	Linear	Quadratic	Linear	Quadratic
Bandwidth	0.116	0.120	0.115	0.205
N_b	1317	1343	1308	1960
N	3286	3286	3286	3286

Note: Female share defined as female temporary employees over total temporary employees one year after electoral race. Sample restricted to mixed gender elections with one female candidate. Triangular Kernel used to to construct the local-polynomial estimator. Standard errors clustered at municipality level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.3. Impact on Share of Female Managers (Placebo)

	(1) % Female	(2) % Female	(3) % Female	(4) % Female
Win	-0.023	0.013	-0.027	0.000
	(0.030)	(0.040)	(0.028)	(0.038)
Mean	0.391	0.391	0.391	0.391
Selector	cerrd	cerrd	cersum	cersum
Polynomial	Linear	Quadratic	Linear	Quadratic
Bandwidth	0.121	0.150	0.142	0.170
N_b	929	1100	1064	1200
N	2233	2233	2233	2233

Note: Female share defined as female temporary employees over total temporary employees one year prior to electoral race. Sample restricted to mixed gender elections with one female candidate. Triangular Kernel used to to construct the local-polynomial estimator. Standard errors clustered at municipality level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.4. Impacts by Quintiles of Wage Distribution

	(1) All	(2) 1st Quint	(3) 2nd Quint	(4) 3rd Quint	(5) 4th Quint	(6) 5th Quint
Panel A: All Temporary workers						
Win	0.033*	0.024	0.048*	0.020	0.014	0.041*
	(0.019)	(0.026)	(0.029)	(0.027)	(0.025)	(0.024)
Mean	0.565	0.643	0.600	0.547	0.524	0.480
Bandwidth	0.093	0.098	0.096	0.105	0.112	0.111
N_b	1529	1602	1299	1513	1634	1612
N	4661	4661	3889	4175	4303	4252
Panel B: Temporary without Managers						
Win	0.028	0.014	0.042	0.037	0.010	-0.001
	(0.021)	(0.028)	(0.032)	(0.028)	(0.028)	(0.027)
Mean	0.584	0.650	0.609	0.561	0.545	0.513
Bandwidth	0.094	0.098	0.099	0.112	0.116	0.124
N_b	1515	1534	1262	1510	1606	1652
N	4594	4499	3741	4015	4131	4049

Note: Female share defined as female temporary employees over total temporary employees one year prior to electoral race. Panel A includes managers in all quintiles. Sample restricted to mixed gender elections with one female candidate. Triangular Kernel used to to construct the local-polynomial estimator. Standard errors clustered at municipality level in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table 1.5. Impact of Female Victory on Temporary Workers Composition

	(1) Total p/10k	(2) Female p/10k	(3) Male p/10k
Win	7.133	4.202	2.886
	(5.253)	(2.851)	(2.498)
Mean	13.417	6.453	6.964
Selector	cerrd	cerrd	cerrd
Bandwidth	0.105	0.110	0.099
N_b	1940	2014	1834
N	5261	5261	5261

Note: Table dislpays RD estimates for total, male and femle temporary employees per 10,000 habitants . Sample restricted to mixed gender elections with one female candidate. Triangular Kernel and linear function used to construct the local estimate. Standard errors clustered at municipality level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.6. Impact on Educational Outcomes

	(1) % Elementary	(2) % Graduated HS	(3) % Graduated Coll
Panel A: All Managers			
Win	-0.032	0.013	-0.003
	(0.025)	(0.027)	(0.037)
Mean	0.127	0.835	0.342
Bandwidth	0.114	0.113	0.084
N_b	1287	1282	993
N	3266	3266	3266
Panel B: Female Managers			
Win	-0.012	-0.006	0.008
	(0.022)	(0.026)	(0.046)
Mean	0.078	0.890	0.406
Bandwidth	0.111	0.114	0.084
N_b	1067	1086	833
N	2778	2778	2778

Note: Table dislpays RD estimates for share of workers with different educational levels. Column (1) estimates the impact of a female victory on share of workers with at most elementary education. Columns (2) and (3) estimate the impact of a female victory on share of workers with at at least secondary and tertiary education respectively. Sample restricted to mixed gender elections with one female candidate. Triangular Kernel, CER selector method and linear function used to to construct the local estimate. Standard errors clustered at municipality level in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 1.7. Impact of Female Victory on Reelection Probability

	(1) Reelected %	(2) Reelected %	(3) Reelected %	(4) Reelected %
Win	-0.036	-0.033	-0.036	-0.036
	(0.040)	(0.048)	(0.039)	(0.046)
Mean	0.172	0.172	0.172	0.172
Selector	cerrd	cerrd	cersum	cersum
Polynomial	Linear	Quadratic	Linear	Quadratic
Bandwidth	0.101	0.151	0.108	0.171
N_b	1472	1975	1546	2155
N	3967	3967	3967	3967

Triangular Kernel, CER selector method and linear function used to to construct the local estimate. Politcians matched accross electoral cycles using CPF. Standard errors clustered at municipality level in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 1.8. Impact of Female Victory on Gender Policies

	(1)	(2)	(3)	(4)
Panel A: Councils				
Win	0.108***	0.107***	0.103***	0.110***
	(0.0342)	(0.0363)	(0.0328)	(0.0388)
Mean	0.188	0.188	0.188	0.188
Bandwidth	0.0884	0.186	0.0976	0.157
N_b	1346	2365	1463	2118
Selector	cerrd	cerrd	cersum	cersum
Panel B: Boards				
Win	0.0400	0.0495	0.0396	0.0495
	(0.0467)	(0.0577)	(0.0470)	(0.0581)
Mean	0.239	0.239	0.239	0.239
Bandwidth	0.0841	0.122	0.0827	0.120
N_b	1303	1744	1292	1723
Selector	cerrd	cerrd	cersum	cersum
Panel C: Independent Budget				
Win	0.0528**	0.0609**	0.0597**	0.0627**
	(0.0226)	(0.0291)	(0.0243)	(0.0281)
Mean	0.0581	0.0581	0.0581	0.0581
Bandwidth	0.113	0.107	0.0829	0.122
N_b	1083	1027	832	1144
Selector	cerrd	cerrd	cersum	cersum
Panel D: Municipal Plan				
Win	0.0198	0.0286	0.0200	0.0253
	(0.0224)	(0.0301)	(0.0228)	(0.0291)
Mean	0.0523	0.0523	0.0523	0.0523
Bandwidth	0.107	0.126	0.103	0.137
N_b	1573	1790	1516	1919
Selector	cerrd	cerrd	cersum	cersum

Note: "Councils" denote indicator variable for municipalities who declare to have a Public Council to debate woman's rights."Boards" denote indicator variable for municipalities who declare to have a specific board to implement gender equality policies. "Independent Budget" denotes variable for gender equality board with independent budget. Finally, "Municipal plan" represent indicator variable for municipalities who declare to have a formal municipal plan for gender equality. Standard errors clustered at municipality level in parentheses. Odd columns use local linear specification, evens use local quadratic.
* p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 1.9. Impact on Gender of Head of Board

	(1) Share Female	(2) Share Female	(3) Share Female	(4) Share Female
Win	0.100*	0.203***	-0.045	-0.028
	(0.058)	(0.071)	(0.065)	(0.058)
Board	Education	Health	Finance	Compt. Gen.
Mean	0.702	0.479	0.304	0.268
Selector	cerrd	cerrd	cerrd	cerrd
Polynomial	Linear	Linear	Linear	Linear
Bandwidth	0.119	0.099	0.103	0.115
N_b	1068	855	953	1035
N	2549	2383	2557	2534

Triangular Kernel, CER selector method and linear function used to construct the local estimate. Standard errors clustered at municipality level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.10. Impact on Share of Stable Female Managers

	(1)	(2)	(3)	(4)
	% Female	% Female	% Female	% Female
Win	0.043	0.029	0.051	0.033
	(0.054)	(0.066)	(0.050)	(0.064)
Mean	0.516	0.516	0.516	0.516
Selector	cerrd	cerrd	cersum	cersum
Polynomial	Linear	Quadratic	Linear	Quadratic
Bandwidth	0.091	0.132	0.105	0.141
N_b	787	1077	900	1145
N	2451	2451	2451	2451

Triangular Kernel, CER selector method and linear function used to to construct the local estimate. Dependend variable defined as share of stable female managers excluding new hirings at the end of first year of term. Stable managers defined as manangers with RAIS contract type code 30 and 31. Standard errors clustered at municipality level in parentheses.

 * p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 1.11. Impact of Female Victory on Areas of Public Goods Provision

	(1) Educational %	(2) Healthcare %	(3) Educational (Ln)	(4) Healthcare (Ln)
Win	-0.015	0.000	-0.036	0.043
	(0.013)	(0.001)	(0.115)	(0.105)
Mean	0.251	0.020	4.727	2.333
Selector	cerrd	cerrd	cerrd	cerrd
Polynomial	Linear	Linear	Linear	Linear
Bandwidth	0.099	0.133	0.095	0.116
N_b	1800	2275	1664	1668
N	5168	5168	4934	4197

Triangular Kernel, CER selector method and linear function used to construct the local estimate. Standard errors clustered at municipality level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER 2

Leadership and Racial Composition in Top Positions

2.1. Introduction

In several other countries of the world, black workers face worst labor market outcomes than their non-black counterparts ([9], [16], [67]). This underrepresentation is particularly prominent at the top. In America only 5 out of 279 executives S&P 100 are black. In Brazil, black workers are underrepresented on top positions leading white workers to make on average 70 % more than blacks ([23]). On local public sectors, non-white workers make up only 33 % of workers on the managerial position. This study investigates the contribution of race of leadership to the racial representation gap.

Both in Brazil and Worldwide, the difference in outcomes by ethnicity may be attributed to several different causes, making a deep and comprehensive analysis of each potential channel crucial for solving the underrepresentation problem. One channel associated with the low share of non-white workers in top positions is the lack of non-white workers on leadership and decision making positions above them. For example, on a recent interview, former Times Warner African American CEO Dick Parsons mentioned that black candidates "don't get looked period." for openings in top positions by other CEO's. [1]

Since [14] seminal work on discrimination on labor markets, many studies have provided observational evidence of the impact of a leader's race on probability to hire or retain minority applicants ([45] [46], [47], [5]). This study uses a quasi-random experiment in Brazil to provide a causal estimate of the impact of the race of leadership over hirings and retention of minority managers.

Under the presence taste base discrimination ([10]) or racial homophily, electing a non-white leader can lead to a higher share of non-white managers. Nonetheless, if the supply of non-white candidates for the managerial position is limited or if agents do not exhibit these types of preferences electing a an non-white leader may not lead to any increase on the share of minority mangers. Hence,

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[1]https://www.usatoday.com/in-depth/money/business/2020/08/20/racism-black-america-corporate-america-facebook-apple-netflix-nike-diversity/5557003002/

a priori, the effects of electing a non-white leader are unknown. Using a regression discontinuity on mixed raced electoral races in Brazil, we are able to compare municipalities similar on both observable and non-observable characteristics thus being able to access causation of the election of a non-white mayor.

Focusing on the 2016 Brazilian election, we estimate the impact of electing a non-white mayor on a given municipality over the share of non-white managers. On average, a non-white mayor increases the share of non-white temporary contracted managers by a non-significant 0.3 percentage points. This effect is robust to restricting our sample to contracts only initiated during the mayor's term and to electoral races where the mayor has no reelection probability.

We provide two main contributions to the literature: First, this is the first study to use a regression discontinuity design to analyze the impact of race of leadership on racial gaps on top positions. While with this methodology extrapolation for other contexts can be more challenging due to its local nature, regression discontinuity designs also produce estimates that have extremely high internal validity. Second, this is the first study to so for the public sector, a sector known to be a stepping stone for white collar positions and public life.

This paper is organized as follow: Section 2.2 describes data and empirical strategy. Section 2.3 describes results and Section 2.4 concludes.

2.2. Data and empirical strategy

2.2.1. Data and Institutional Context. In this study, we will use three primary sources of information: The Superior Electoral Court (TSE), the Annual Social Information Report (RAIS), and the Profile of Municipalities (MUNIC/IBGE).

Brazil holds municipal elections every four years. The municipal election records are made public by TSE.[2] The election data contains information about the candidates, such as age, marital status, gender, occupation, and party affiliation, and information about elections, including the number of votes received by each candidate. As of 2014, the TSE began to publish a self-declaration of candidates' races. Therefore, we have information on the race of mayoral candidates for the 2016 municipal elections. To classify the groups of races, we must consider the Brazilian context. Brazilian demographic census considers five groups of races (White, Black, Mixed-race ("pardo"),

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[2] `http://www.tse.jus.br/eleicoes/estatisticas/repositorio-de-dados-eleitorais-1/`

Asian ("amarelo"), and Indigenous). But, given other demographic characteristics (such as economic status and formal education), Brazilian racial policies recognize that both blacks and mixed-races suffer from discrimination and that it is important to make efforts to improve their social and economic positions in society [64]. For this reason, we follow the recent literature on racial gaps and racial discrimination in Brazil [43,51,52,58,71], classifying race in two main categories: whites and non-whites (blacks and mixed-race).[3]

The labor market information from RAIS that we use consists of an administrative matched employer-employee dataset managed by the Ministry of Labour (MTE). This data provides information on the universe of workers in both public and formal private sectors. Data are released annually, and we use data from 2003 to 2018. The Annual Report of Social Information (RAIS) contains all formal employment contracts in Brazil. In addition, this database provides us with information on wages, hours worked, dates of hire and dismissal, and demographic information on workers, such as education, age, gender, race, and worker's social security number.

In Brazil, there are two ways to enter the municipal public sector. Most public sector jobs require applicants for the position to undertake a public civil service exam, and its format and content vary according to the job description. Then candidates are ranked according to their performance, and the best-ranked candidates get the job vacancy in the public sector. Public servants hired through the public civil service exam have reasonable job stability. Given the public hiring legislation, dismissals in this type of contract are rare, occurring only in cases of extreme misconduct. A downside of this form of hiring is the length of time to fill the vacancy.

The second form of public hiring is through temporary contracts. This type of contract allows elected officials to choose their office members since there is no need to conduct a public civil service exam. Instead, the public administration of the municipality sets the requirements to fill in the job. Temporary contracts are most common in leading job positions and, for this reason, the individuals in theses position, usually work closely with the mayor. Since temporary contracts face similar separation laws to those in the private sector, the turnover rates are higher for this type of contract.

Given our research question, RAIS provides us with four important pieces of information: the legal nature of the employer, Brazilian Classification of Occupations (CBO), the type of contract, and race. These variables make it possible to identify municipal workers in leadership positions

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[3]Only 0.26% mayors candidades in the election of 2016 are Asian or Indigenous. We opt to exclude these observations from our sample.

with temporary contracts. Therefore, following [59], we consider municipal workers' contracts with employers that classified their legal nature as "Public Body of the Municipal Executive Power" or "Municipalities." In its turn, we define a managerial position as those in at least one of the following CBO codes: "111220 - Executive Secretary" and "111415 - Director of the municipal public service".

Table 2.1 presents information about the public municipal workers, by type of contract (temporary or stable) and job position (manager or non-manager), for the year 2017, i.e one year after the 2016 election. As expected, temporary workers spend less time in their position than those under stable contracts. Further, managers spend less time in their position than non-managers. One possible explanation for this fact is that, since the manager is a position of trust of the public administration, there is more turnover in these job positions due to political interests. We can also see in Table 2.1 that temporary and stable managers have higher mean wages than non-managers under the same contract. Taken together, the tenure in position and the mean of wages, we have evidence that our definition of temporary managerial positions is adequate.

Since the employer declares the worker's race in RAIS, it could be the case that different employers' records race differently for the same worker. To deal with this fact, we define the race of the individual as the modal race across all their observations in the RAIS sample, as proposed by [43]. With the worker's race information, it is possible to construct our variable of interest, the proportion of non-white workers in municipal managerial positions with temporary contracts.

For further investigations on the effect of a black mayor over racial policies, we will use data from Profile of Municipalities (MUNIC/IBGE). This data set provides information on several characteristics of Brazilian municipalities. For our purposes, we use 2018 and 2020 MUNIC to get information on the race of municipal managers in the following offices: health, education, culture, and social welfare. Further, for 2014 and 2019, MUNIC has information on policies to promote racial equality and fight racism.

2.2.2. Empirical Strategy. Estimating the impact of electing a non-white mayor is not a straightforward task. The unobserved characteristics of politicians and municipalities may mislead our conclusions if we compare municipalities with white mayors with non-white mayors. The ideal setup to perform this estimation would be a random assignment of mayors. However, since this setup is not plausible, we will use close elections to induce a kind of random variation. To estimate

the causal impact of the mayor's race over the proportion of non-white managers in public service, we will employ a Regression Discontinuity Design, following the steps suggested by [57].

To perform our RDD, we will restrict our sample to municipalities in which, of the two most voted candidates, one declares himself as non-white, and the other declares himself white. Using voting data, we will build the running variable that will equal the percentage of votes of the black candidate minus the percentage of votes of the white candidate. We also use TSE information on demographic characteristics and party affiliation to perform balance checks and check the validity of our empirical strategy. The main identification assumption is that, for the specific subset of competitive electoral races that we consider, whether the candidate gets the higher or lower rank is as good as random. In other words, the idea is that, in close elections, the odds of being on one side or the other of the voting threshold are as good as random. In this case, the counterfactual for the proportion of non-white managers if the elected mayor were white are the non-white candidates that were close runners-up.

Formally, the equation that we estimate is given by:

$$(2.1) \quad Y_c = \beta NonWhiteMayor_c + \gamma f(VoteShare_c) + \tau[NonWhiteMayor_c * f(VoteShare_c)] + \alpha_c$$

Where the variable $NonWhiteMayor_c$, indicates if the mayor who won the mayor election at municipality c, is non-white. Our coefficient of interest is β, which measures the impact of having a non-white mayor on the race of public servants in top positions. We also include a measure of the candidate's victory margin $f(VoteShare_c)$, and cluster the standard errors at the municipal level.

The dependent variable in our main specification is the proportion of non-white municipal workers in managerial positions in each municipality. We construct this variable based on the information of the active employment contracts in December of 2017, at the end of the first year of the mayor's term. We also perform alternative specifications looking only at new employers (temporary contracts from January 2017 - the first month of the mayor's term - till December 2017). Further, we look at the race of managers of specific areas, such as education, health, culture, social welfare, and policies that aim to promote racial equality, using the MUNIC data, as mentioned in the previous session.

Following the most recent developments in RD design, our main estimations will use a local linear specification with a triangular kernel, as suggested by [26]. We will also use the optimal bandwidth selector to minimize an approximation to the coverage error (CER) of the confidence

interval and robust bias correction for constructing confidence interval as proposed by [20] and [24]. To check the robustness of our estimations, we test for different bandwidths, kernel functions, and polynomial degrees specifications.

2.2.2.1. *RD validity and descriptive statistics.* The context of close elections is one of the most common applications of regression discontinuity designs. The validity of RD assumptions had been tested and confirmed in different contexts, as described by [36] and [27]. Despite this evidence, we proceed to show that our regression discontinuity approach to measuring the impact of electing a non-white mayor over the race of temporary managers is adequate. To this end, we need to provide strong evidence that the regression functions are smooth functions of the score at the cutoff. Since the assumption of continuity of the conditional distribution of potential outcome relies on unobservable features, we can not directly test it. But, as suggested by [24], we can test empirical implications of the unobservable RD assumptions, providing evidence of its validity.

First, we perform a manipulation test based on the idea proposed by [63]. This test aims to check if there is a manipulation of candidates' votes around the cutoff by examining if candidates' density is continuous near the cutoff value. Figure 2.1 presents the plot of the distributional density of a candidate's margin of victory for the continuity test proposed by [25]. We cannot reject the null hypothesis of no manipulation in our sample, indicating no discontinuity around the zero vote margin of victory.

The second test consists of a falsification test that examines whether treated units are similar to control units in observable pre-treatment characteristics near the cutoff. The key idea is very straightforward: if we find systematic differences between candidates with similar vote margins, we have evidence that the runner-ups are not an adequate control group for the winners. We perform the same regression as our main specification but use the predetermined covariates as the dependent variable. Here we analyze two dimensions: characteristics of the candidates (as education, party affiliation, etc., for non-whites and whites candidates), and characteristics of the municipality (such as income per capita, the share of men, illiteracy rate, and the existence of racial equality policy in the year of 2014). For each of these regressions, we use its respective CER-Optimal bandwidth. Figure 2.2 presents the point estimation with the 95% confidence interval to each regression. We can see that none of the covariates presented in each sample is statistically significant. Thus, we can argue that there is no manipulation of the running variable near the cutoff. Given these results,

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we can state that our estimation is indeed the causal effect of electing a non-white mayor over the race of managers in the municipal public sector.

The falsification test discussed above showed no difference in observable characteristics between non-white winners and non-white runners-up. But, since our interest relies on actions taken by the mayor, it is also important to compare the attributes of non-white and white winners. Table 2.2 performs this comparison for the full and the close election sample, with its respective mean test p-value. We note that the groups differ by gender, education, and age composition for the full sample. In addition, white winners groups have a larger proportion of mayors affiliated with the MDB party (a center-oriented political party).

When we restrict the sample to elections with a margin of victory of at most 10%, we see that the gender and higher education composition difference between the two groups disappear. Still, then the groups remain distinct when we look at age profile and the proportion of mayors with high school education. Regarding party affiliation, there is no statistical difference between the MDB members in the close election sample. But now, in this sample, we observe a larger proportion of non-white mayors (8,39%) affiliated to left-wing party, PT, than in the white mayor's group (3,61%). Despite this difference in party affiliation, we can conclude that the two groups are similar when comparing winners in close elections. The next section presents the results of our estimations to verify the impact of electing a non-white mayor over the racial composition of municipal managers with temporary contracts.

2.3. Results

2.3.1. Proportion of non-white managers. We begin this section by presenting the estimates of the impact of electing a non-white mayor over the proportion of non-white managers with temporary contracts. Table 2.3 show us the results when we look at managers with temporary contracts in December of 2017 and in December of 2018, that is, the first two years of the four-year mandate. In panel A we are considering the overall proportion of temporary contracts. According to our estimates, there is no effect of electing a non-white mayor over the proportion of non-white managers in municipal public service. The point estimate of our variable of interest is close to zero and identical in module for both years, 0.003.

One possible explanation for this null result could be that the change in managerial composition implemented by the new mayor is insufficient to significantly impact the overall proportion of non-white workers. For example, it could be the case that new mayors face difficulties dismissing employees who already occupied these jobs before starting the new mandate. Although this kind of explanation is unlikely to occur given the legislation of temporary contracts, as mentioned in the previous section, the estimates on panel B of Table 2.3 aims to address this concern. Therefore, in these regressions, we consider only managers with temporary contracts initiated during the new mandate, i.e., starting from January 2017 and active in December 2017 (column 1) and December 2018 (column 2). As a result, we still have no effect of electing a non-white mayor, with point estimates similar for both years and close to zero.

In Brazil municipal elections, each mayor candidate runs for a four-year mandate. The electoral legislation allows the incumbent to run for at most one reelection. That is, a mayor could have at most two consecutive mandates. In panel C of Table 2.3, we consider the overall proportion of non-white managers but restrict our sample only to municipalities where the incumbent was unable to run in the 2016 elections. By looking only at municipalities with new candidates, we hope to exclude the possibility that what drives the absence of effect is the changes in the composition of municipal managers with temporary contracts made in the previous mandate. Again, the estimated effect of electing a non-white mayor over the racial composition of managers with a temporary contract is statistically equal to zero. Figures 2.3, 2.4 and 2.5, presents the RD plot for each estimation aforementioned.

In the context of a regression discontinuity design, the result may be sensible to model specification choices, such as bandwidth, local polynomial and kernel weight. Therefore, it is necessary to verify if the null result previously presented is robust to different estimation choices. We began by showing that the absence of impact of electing a non-white mayor over the proportion of non-white municipal managers with temporary contracts is independent of bandwidth choice. Figure 2.6 presents the point estimate (with 95% confidence intervals) for different bandwidths, and the null result remains, both for 2017 and 2018. The null result for different bandwidths also remains if we look at the other dependent variables of Table 2.3(Figure B.1 in the appendix).

In addition, to test for several bandwidths, Table 2.4 presents the results for using different estimation specifications in terms of optimal bandwidth method, local polynomial, and Kernel weight.

Also, in columns 2, 3, and 5, we include state-fixed effects in our estimation. Although we showed in the previous section that we have a balanced sample on several municipalities characteristics on the two comparison groups, the inclusion of state fixed effects can increase the efficiency of our estimates [24]. In all of the six specifications presented in Table 2.4, we still have zero effect, with similar point estimations.

With the results presented above, we may conclude that there is no effect of electing a non-white mayor over the proportion of non-white managers with temporary contracts in Brazilian municipalities.[4] To explore similar dimensions, we check for the impact of having a non-white mayor over the race of managers of specific areas and on implementing racial policies.

2.3.2. Head of municipal offices. The public administration of Brazilian cities includes the municipal offices. The municipal offices are responsible for planning, organizing, directing, and coordinating policies related to their areas (health, education, culture, for example). The mayor chooses the head of each municipal office, that works under a temporary contract, as mentioned in section 2.1. Heads of municipal offices work close to the mayor on important municipalities' subjects and, for this reason, are under the spotlight of local governments. The data from MUNIC have the information on the race of some of the heads of municipal offices. This information allows us to investigate if electing a non-white mayor impacts the probability of the head of offices of Health, Education, Culture, Social assistance, and Environment being non-white. We present the results of our estimation in Table 2.5.

Our estimations show no impact (and point estimates close to zero) for all the offices, except for Social assistance. Somewhat surprisingly, we have a negative effect: electing a non-white mayor reduces the probability of having a non-white head of the Social assistance office. The magnitude of the negative effect is not negligible. A close win of a non-white mayor is associated, on average, with a 19.9 percent points (or 38.8% of the mean) decrease on the probability of having a non-white head of Social assistance office. Unfortunately, since we have information on the race of the head of the office only for one year (2018), we cannot exclude the possibility that our sample is unbalanced on this variable. Likewise, we cannot test if the founded effect persists during the entire mandate.

2.3.3. Racial policies and actions. Given the results presented so far, we have evidence that the mayor's race does not influence the race of the municipal managers with temporary contracts

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[4]The null effect is also found when we use the proportion of Black managers as the dependent variable.

nor the race of the head of municipal offices. A natural question that arises given this result is if there are differences in racial policies and actions implemented by non-white mayors compared to white mayors, despite the absence of differences in the races of the managers. We use the MUNIC data on certain racial policies in each Brazilian municipality to investigate this fact.

The dependent variable in the estimation of the first column of Table 2.6 equals one if the municipal education office adopts measures against racial discrimination. The second column refers to health actions aimed at the non-white population in the municipalities. The dependent variable on the third column would equal one if the municipalities followed the policies recommendations of the Federal Government policy in its health plan for improving the health of the non-white population.[5] Finally, on the last column of Table 2.6, we use the information if the current municipal administration have policies that aims to promote racial equality and fight racism. All four estimations indicate no difference between white and non-white mayors for the policies and actions that we analyze.

Therefore, our results indicate that the race of the winner of a close mayoral election does not impact the municipal racial policies. However, it is important to highlight that our analysis of racial actions and policies is exploratory, given data availability. For example, we do not have information on municipal policies that aim to promote racial equality in other areas that are especially relevant when discussing racial issues, such as public security.

2.4. Conclusion

This chapter analyzes the underrepresentation of non-white in top positions. In Brazil, as in most countries in the world, racial minorities are underrepresented in top positions in the public sector. To the best of my knowledge, this is the first work to have exogenous variation on the leadership position and find that changes in leadership have no impact on the racial composition gap in top positions.

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[5]The Federal health pan for improving the health of the non-white population is a document for municipal health managers, with the objective of providing guidance regarding the implementation of public policies to combat health inequalities that affect the Non-white population. The document defines principles, objectives, guidelines and possible strategies for improving the health conditions of the non-white population.

Figures
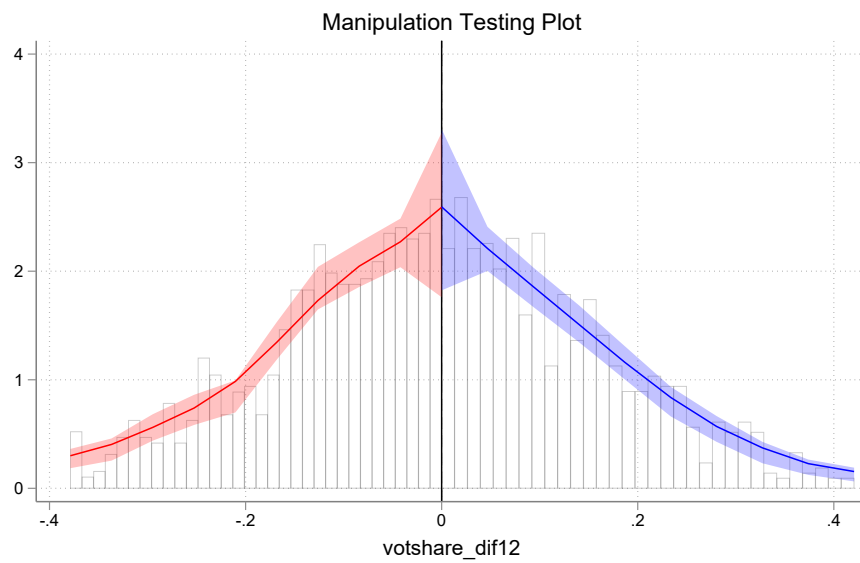
FIGURE 2.1. Density tests

Note: Manipulation test proposed by [**25**]. The null hypothesis is that there are no manipulation with the chosen bandwidth (here, bandwidth selection was based on MSE of sum of densities). The robust bias-corrected p-value is 0.913 and its respective bandwidth is 0.126.
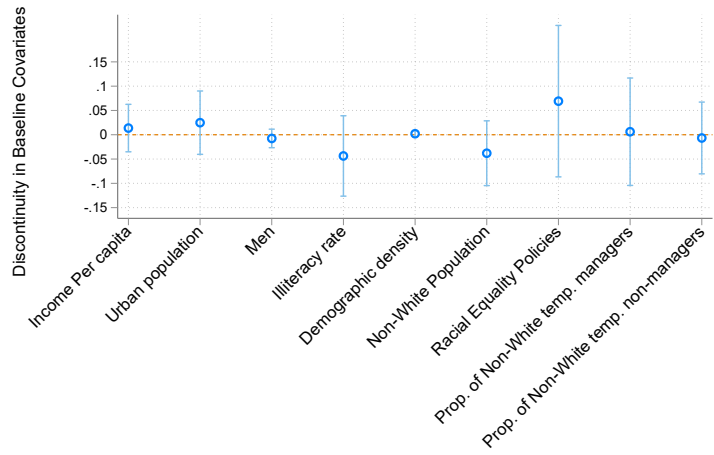
(a) Municipality characteristics
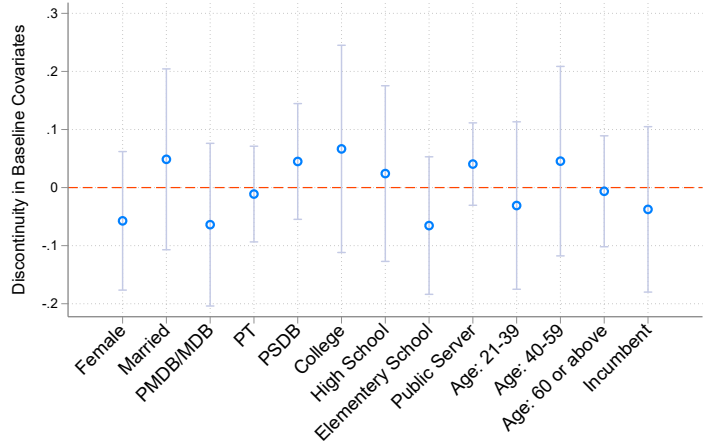
(b) Non-white candidates
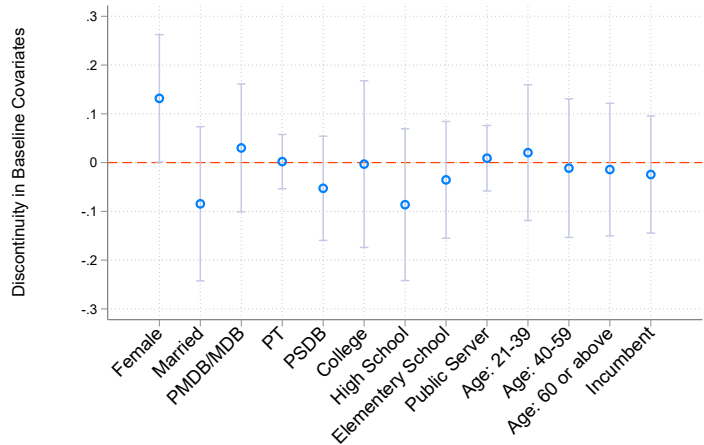
(c) White candidates

FIGURE 2.2. Test for discontinuities in covariates

Note: Point estimate and 95% confidence intervals of RDD estimates using baseline covariates as the dependent variable and the margin of victory as the running variable.
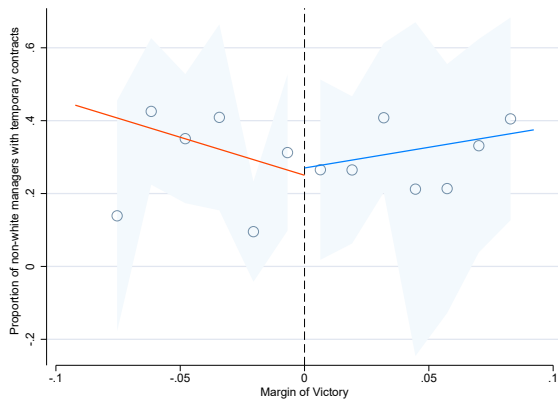
(a) Temporary contracts - 2017
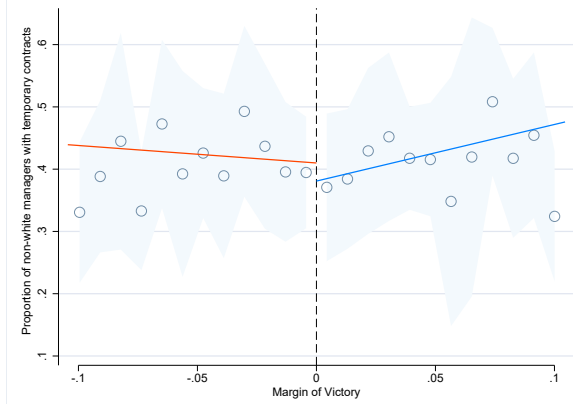
(b) Temporary contracts - 2018

FIGURE 2.3. RD plot for impact over overall proportion of non-white managers with temporary contracts.

Note: Estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level.
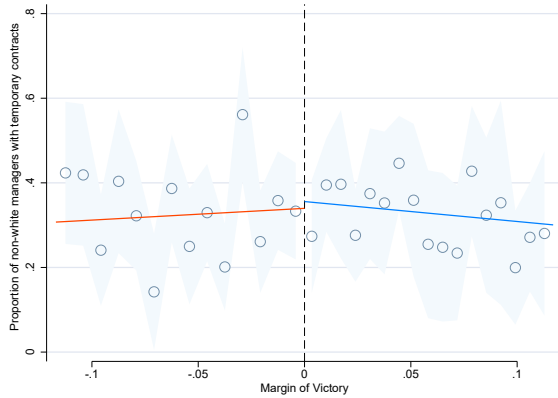
(a) Temporary contracts - 2017
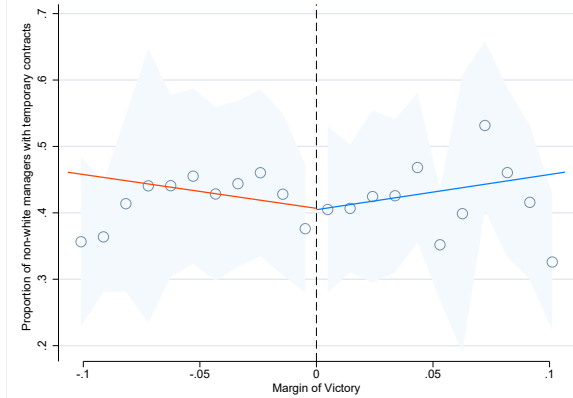
(b) Temporary contracts - 2018

FIGURE 2.4. RD plot for impact over proportion of non-white managers with temporary contracts initiated during mayor's term.

Note: Estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level.
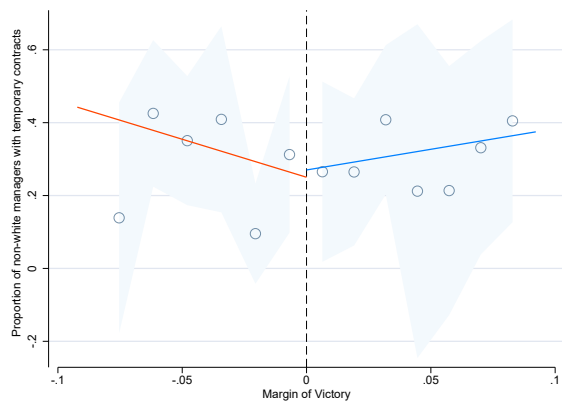
(a) Temporary contracts - 2017
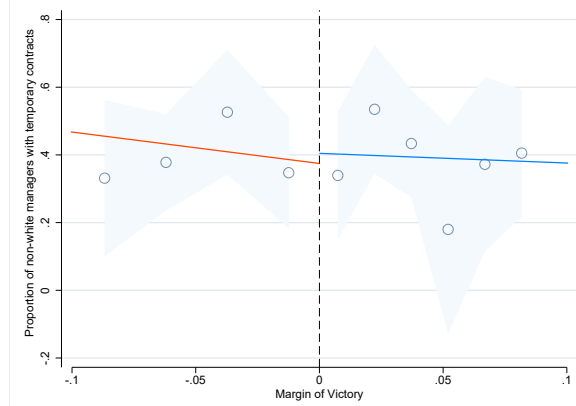
(b) Temporary contracts - 2018

FIGURE 2.5. RD plot for impact over overall proportion of non-white managers with temporary contracts in races without reelection possibility.

Note: Estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level.
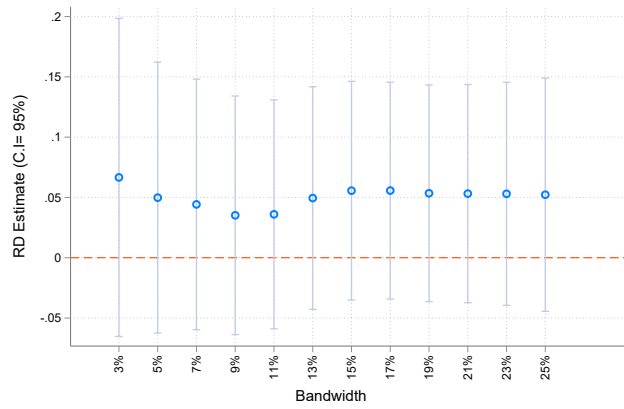
(a) 2017
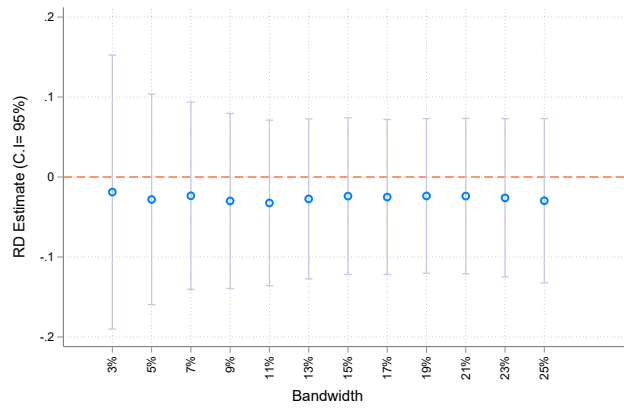
(b) 2018

FIGURE 2.6. Bandwidth Sensibility - Proportion of non-white managers.

Note: Point estimate and 95% confidence intervals. Dependent variable is the porportion of non-white managers with temporary contracts.

TABLE 2.1. Characterisct of municipal workers, by type of contract and job position.

	Temporary		Stable	
	man-agers	**non-managers**	**managers**	**non-managers**
Female (%)	51.15	38.66	38.92	32.61
Non-white (%)	42.44	41.07	40.08	35.54
Wage (R$)	3,003.34	2,418.52	3,397.78	3,119.31
(s.e)	(2,535.95)	(2,193.91)	(3,055.43)	(2,679.69)
Tenure in position (in months)	29.26	68.13	113.73	151.82
(se)	(56.95)	(68.13)	(114.19)	(106.13)
Age	41.31	41.53	44.3	44.48
(se)	(11.73)	(41.53)	(10.18)	(106.13)
Elementary school	6.33	8.29	6.41	7.16
High School	44.8	40.07	37.26	32.71
Higher education or more	34.75	36.63	41.99	46.23
N	109,936	1,014,928	113,738	3,046,395

TABLE 2.2. Descriptive statistics of Mayors elected in 2016.

Variables	Full Sample			Close Elections (BW = 0.1)		
	White Mayor	Non-White Mayor	p-value test	White Mayor	Non-White Mayor	p-value test
Female	16.92	11.94	0.004***	18.05	14.16	0.16
Married	70.32	72.27	0.387	70.27	69.36	0.79
MDB party	19.18	14.63	0.015**	18.61	14.16	0.111
PT party	4.65	6.16	0.177	3.61	8.39	0.007***
PSDB party	13.47	13.73	0.876	11.39	12.43	0.671
Higher education	54.47	49.55	0.048**	53.61	48.84	0.206
High school	23.96	30.17	0.005***	25.00	32.37	0.03**
Elementery school	11.91	12.19	0.864	11.11	11.85	0.759
Public Server	6.55	8.86	0.082*	5.83	7.23	0.454
Age: between 21 and 39 years old	23.36	23.11	0.904	24.72	21.09	0.252
Age: between 40 and 59 years old	58.64	65.98	0.002***	56.94	69.66	0.005***
Age: 60 years old or more	18.00	10.91	0.000***	18.34	9.25	0.000***
Incumbent	21.93	21.82	0.958	18.05	22.25	0.165

TABLE 2.3. Estimates of the effect of electing a non-white mayor over the proportion of non-white managers with temporary contract

Panel A: Temporary contracts

	2017	*2018*
	(1)	(2)
Non-White Mayor	0.003	-0.003
	(0.052)	(0.053)
Dependent variable mean	0.332	0.325
BW	0.115	0.104
Eff. Number of Obs	516	495
Number of Obs	1070	1093

Panel B: Temporary contracts initiated during the mayor's term

	2017	*2018*
	(1)	(2)
Non-White Mayor	0.020	0.010
	(0.055)	(0.055)
Dependent variable mean	0.334	0.330
BW	0.117	0.107
Eff. Number of Obs	491	480
Number of Obs	1003	1043

Panel C: Temp. contracts in races without reelection possibility

	2017	*2018*
	(1)	(2)
Non-White Mayor	0.026	0.053
	(0.118)	(0.108)
Dependent variable mean	0.319	0.304
BW	0.092	0.100
Eff. Number of Obs	103	113
Number of Obs	258	258

Note:. All estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level are reported in parentheses.

TABLE 2.4. Different specifications of estimation model for impact on proportion of non-white municipal managers.

	Prop. of non-white managers with temp. contract (2017)					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-White Mayor	-0.031	0.035	0.056	-0.011	0.031	0.035
(s.e)	0.069	0.068	0.449	0.062	0.046	0.048
BW	0.123	0.123	0.142	0.183	0.11	0.156
BW selector	CER	CER	MSE	MSE	CER	MSE
Kernel	Triangular	Triangular	Triangular	Triangular	Epachnikov	Epachnikov
Polynomial	Quadratic	Quadratic	Linear	Quadratic	Linear	Linear
State Fixed Effect	No	Yes	Yes	No	Yes	No
Eff. Number of Obs	551	551	627	740	497	683

Note: Robust standard errors clustered at the individual level are reported in parentheses. MSE is bandwidth that optimizes the mean squared error of the local polynomial RD point estimator. The CER-optimal bandwidth minimizes the asymptotic coverage error rate of the robust bias-corrected of the confidence interval for the point estimator [20]

TABLE 2.5. Effect over having a non-white head of municipal board.

	Health	Education	Culture	Social assistance	Environment
	(1)	(2)	(3)	(4)	(5)
Non-White Mayor	0.0168	-0.021	-0.065	-0.199**	-0.089
(s.e)	0.0835	0.087	0.091	0.087	0.087
Dep. variable mean	0.5118	0.562	0.57	0.529	0.534
BW	0.107	0.098	0.091	0.091	0.1
Eff. Number of Obs	756	671	609	643	628
Number of Obs	1600	1597	1516	1600	1463

Note: Estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level. Race of the Health, Education, Culture and Social Assistance head of office on the year of 2018. Race for the Head of Environment office on the year of 2020.

TABLE 2.6. Effect over racial policies and actions.

	Education Actions	Health Actions	Health Policies	Racial Equality Policy
	(1)	(2)	(3)	(4)
Non-White Mayor	-0.099	0.029	-0.06	0.000
(s.e)	0.065	0.043	0.068	0.0788
Dep. variable mean	0.893	0.048	0.309	0.292
BW	0.095	0.083	0.143	0.108
Eff. Number of Obs	590	611	533	762
Number of Obs	1388	1617	1588	1616

Note: Estimates presented uses bandwidths CER-Optimal, local linear regression and triangular kernel. Robust standard errors clustered at the individual level. Columns 1, 2 and 3 refer from MUNIC/2018 data. Column 4 refer to MUNIC/2019 data.

CHAPTER 3

Corruption and Political Participation

3.1. Introduction

Corruption is a complex political, social, and economic phenomenon present in all parts of the globe. The burden it imposes in modern societies is such that former World Bank president James Wolfensohn once referred to it as the "Cancer of Corruption." Fighting it will likely be done on several different fronts, and increasing information and transparency will be one of those. Think thank International Transparency lists information as one of the critical factors in the struggle against corruption, calling information "a human right that can act as a safeguard against corruption."

The relation between corruption and information have been widely investigated by political scientists and economists. Several works focus on the consequences of corruption-related information for incumbent politicians. For example, incumbents who are found to be more corrupt face lower re-election probability ([**56**], [**33**], [**37**]) and audit programs are associated with a reduction in corruption [**38**].

Less is known, however, about the impacts of corruption on voter participation. While some articles have casted light on this question by showing causal evidence that corruption may have no effect [**7**], [**49**] or decrease political participation ([**29**], [**35**], [**44**]) this literature focuses on elections where the information released directly involves one candidate. Although valid to analyze the consequences of cases where politicians are directly responsible for the misuse of public funds, this setup is not ideal for evaluating theories suggesting that corruption reduces thrust in the political system. The corrupt politician may affect the supply of candidates confounding effects and mechanisms. For example, suppose a mayor is found to be corrupt. In that case, he may pull out of a subsequent election, leading the race to be less competitive and voters to have lower participation not due to lower trust in the system but because of the level of competitiveness.

There are several reasons to why corruption may lead to a decrease in participation: It may undermine voters confidence in public institutions ([**18**], [**30**]) or compromise thrust in the political

system in general ([65], [69], [70]). Using a simple protest voting framework, I show that if corruption leads voters to reevaluate the utility attributed to each candidate victory by attributing a higher probability that one of them is more corrupt than the other, then the utility gap between candidates may increase, which in turn would lead to higher participation. This may be true to any election, not only those in which the corrupt politician is directly involved.

I show empirical evidence that supports this theory. Using an anti-corruption program that randomly audits municipalities for irregularities in their federal transfer funds, I show that municipalities in which information of violations was released prior to the elections had lower levels of null and blank votes for both mayor and governor (and hence higher participation). This result is interesting for two main reasons: First, while some papers have shown that corruption may have no effect or decrease participation, this is the first cataloged causal case of an increase in participation. Second, I also show that the effects spill over to other positions (Governors) who were are not directly accountable for the funds at a local level.

3.2. Literature & Contribution

The existing literature linking corruption to turnout is still small, and findings present mixed results. The predominant view in the literature is that corruption affects citizens' participation negatively. Advocates of this relation generally propose that a certain level of trust in politicians and government officials is necessary for political participation. This argument is made more clear in [76] and [72], where authors argue that if citizens lower levels of thurst or satisfaction with politicians at various levels of the government, they have less interest in leaving home on election day.

This theoretical argument finds mixed support on empirical analyses. [61], for example, analyses national-level survey data and establishes that individuals who think that political corruption is more widespread are less likely to vote in elections than individuals who think that there is less corruption in the political system. Other correlational studies have found similar evidence for individual countries and regions ([53], [73], [75]).

Causal studies supporting this view are scarce, albeit existent. Making use of an experiment in Mexican local elections [29] shows that voters react to the provision of corruption information

by withdrawing from the political process. Focusing on Italian municipalities for the period 1999-2014, [44] also suggests that exposure to corruption has general and negative effects on political participation. Finally, [35] conduct an experiment in the Brazilian city of Sao Paulo. After informing voters about the challenger's record of corruption, the authors find that the treatment reduced turnout by 1.9 percentage points.

A second branch of literature suggests that there is no relation between turnout and corruption. [68] focus on the U.S. and reports that there is no correlational evidence that corruption and a individuals' likelihood to show up on Election Day are linked. Similarly, [6] find that releasing information about candidates' criminal records prior to the election in India has no causal impact on participation. [74] is the only study to this date to establish a positive relationship between corruption and participation. While analyzing voting data for Portuguese legislative elections in 2005 and 2009, authors find a positive relation between turnout and corruption. Results should be understood as correlational ones, but authors provide an explanation to conciliate their findings with previous literature. They suggest that the increase in mobilization on the sub-national level may be due to citizens using low corruption municipalities as a benchmark for corruption levels, increasing mobilization to reach those levels. While most studies on the correlation between corruption and participation seem to rely to some extent on the idea that corruption reduces thrust on public servants and politicians ([2] [70] [65]), there is no reason to assume that this reduction in confidence is made homogeneously among all politicians on the political spectrum. I provide a simple framework to analyze the effects of corruption on turnout.

Furthermore, I analyze the effects of corruption on a different national level to the one violations took place. While analyzing data at the local level when corruption takes place at the local level may have provided valuable contributions to the literature, it has some limitations. First, local-level elections are subject to first-order effects where several effects can be confounded. For example, it is an established result that voters punish corrupt politicians [37], if this punishment leads elections to be less contested, participation may decrease because voters attribute a lower probability to their vote being pivotal and not because they are actually attributing it to their entire political class.

Second, it is subject to supply effects. Upon receiving information about corruption, it is reasonable to assume that not only voters' priors are updated but also candidates. For example, more corrupt candidates may decide to apply if corruption is higher than expected, discouraging the

voter that cares about corruption. Hence, it is crucial to find a setup not subjected to supply-side effects.

3.3. Background

3.3.1. Brazilian Electoral System. Brazil is a democratic country, with all of its 5,570 municipalities being governed by a mayor elected every four years in direct elections. Elections in municipalities with more than 200,000 registered voters feature a second-round run-off in case no candidate receives a single majority in the first round. Mayors are term-limited and are allowed to be in office for a maximum of eight years (or two consecutive terms). Voting in Brazil is compulsory, with small sanctions applied in case of absence on election day. Absent voters face a fine of R$3.51 (about US$0.65 in August 2020). However, they are also allowed to "justify" their absence exempting themselves from the paying fine (valid justifications include sickness or being out of the country). Hence, given these light penalties, it is not surprising that even with compulsory voting, a little more than 30 million Brazilians (over 20% of registered voters) did not vote at the 2018 election. If a voter decides to be present on election day, he faces the choice of voting for a candidate, blank or null. Blank and null votes usually represent a meaningful share of total votes cast by Brazilian on polls; In the 2010 presidential election, Blank and Null votes represented 8.64 % of all votes, with states like Alagoas casting a total of 11.68 % of Blank and Null votes.

Even though Blank and Null are terms usually used interchangeably by Brazilian voters and the media, they are slightly different by some technicalities. A null vote is a vote for a non-existent party or candidate, while a blank vote is a form of a valid vote. If a voter in the voting booth presses the numbers of a nonexisting party, that will count as a Null vote. Since 1998, however, this is unlikely to be done by mistake as voting in Brazil is done electronically. After typing in the number of the candidate, a picture of the candidate, the numerals typed, and its basic pieces of information are displayed to the voter before him, or she confirms the vote. Hence, mistakes are unlikely, and most of the time, these votes are cast intentionally. Blank votes are cast using a "Blank Vote" button on the electronic voting machine. In theory, these are valid votes but for no particular candidate. Since both of these votes are used with similar intentions and in both cases, voters abstain from choosing a candidate, I pool them together and interpret them as a non-participatory vote.

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3.3.2. Random Audit Program. Each year, Brazilian municipalities receive large transfer amounts from the federal government to guarantee local population access to basic public services such as health care, education, and sanitation. These federal transfers are made with usually little to no oversight. Mayors and legislative bodies have the discretion to allocate most of these resources according to their will, opening a sizeable door to mismanagement and corruption. CGU (Controladoria Geral da União) is the federal agency responsible for investigating the proper use of government funds and ensuring transparency of public finances. Targeting a decrease in municipality corruption levels, CGU launched an anti-corruption program aimed at municipal governments in 2003. The so called Programa de Fiscalização por Sorteios Públicos (Monitoring Program with Public Lotteries), consists of random audits of municipaly use of federally transfered funds. During each round of the program, 60 municipalities were chosen by a publicly held draw in Brasília, where all noncapital municipalities with a population of up to 500,000 inhabitants are eligible for selection. Upon selection, the CGU compiles information on any federal fund transferred to the given municipal government within the past four years. Following that compilation, CGU creates an audit task force for randomly selected specific government projects. Around 15 auditors are then dispatched to the audited municipality for one to two weeks to verify the general delivery of public services associated with the project. Auditors then analyze relevant documents and receipts associated with transferred funds, interview the local population, policymakers, and attempt to find any evidence of misgovernance. It is noteworthy to mention that incentives for corruption are low among auditors; First, the fact that audits are not performed by an individual auditor already makes bribing extremely more costly. Second, auditors earn above-average salaries and are hired based on open public examinations. Upon completion of inspections, a final report describing all the irregularities is submitted to the CGU office in Brasília. This report is made available online by CGU; subsequentially, it is coded and added to a dataset containing other reports and managed internally by CGU.

3.4. Data

I use this dataset as the source to build a corruption measure for the period of 2006 to 2015. Hence my dataset encompasses three Brazilian Gubernatorial Elections (2006, 2010, 2014) and 20 draws (draw 20 to 40). (Figure 3.1 displays the number of audited municipalities per year. For this

period, internal CGU data contains a detailed description of all irregularities found by the auditors for each inspection order. Moreover, this data contains information on the sector, the amount audited, a description of the irregularity, and a classification of the irregularity.

This classification is made into three categories (1) Formal violation, (2) Moderate violation, or (3) Extreme Violation. Formal violations are clearly the mildest of the three and do not implicate corruption. Examples of this type of violation are documents that were not properly filled out or even not properly formatted. Moderate violations and extreme violations, however, can be interpreted as acts of corruption or mismanagement and are most of the time hard to be separated in terms of intensity. Consider the reports about Nova Glória in draw # 34: Overprice in the purchase of medicines for a public pharmacy was classified as an extreme violation. Meanwhile, several students for which the local government had been receiving federal transfers were found to be non-existent, and this violation was considered moderate. Even though one could argue that overpriced is a more clear act of corruption, both violations seem to imply at least some sort of mismanagement. Hence, In this paper, I use the combination of both as a measure of corruption. For completeness, I also display individual results for some specifications.

Figure ?? displays a histogram for the number of corruption violations found by the municipality. Notice that all municipalities have at least one medium or extreme violations. The distribution is also skewed to the right, showing us that there are some outlier municipalities with extremely high corruption levels. I do not exclude these from the analysis.

Electoral data comes from *Tribunal Superior Eleitoral* (TSE), the Brazilian Superior Electoral Court. This data contains the number of votes as well as personal characteristics of candidates such as gender, race, education, and income for all governors in all governors that participated in races in all 26 Brazilian States. Table 3.1 shows descriptive statistics for all governors in our sample. Governors are, on average, more educated than the median Brazilian (at least 75% of them attended college in any particular year). They are also more likely to be male and older. The greater age is not surprising, as similar to other countries in the world, the governor is a prestigious position in Brazil for which candidates are usually experienced, mid-career politicians.

Finally, municipality level data comes from the 2000 National census and includes demographic, economic, and social characteristics of households in each Brazilian municipality. To supplement this, I use data from Perfil Municipio for the availability of media in Brazilian municipalities.

3.5. Methodology

My main objective is to test if corruption disclosure affects the level of null and blank voting in municipalities where it takes place. Literature has shown that disclosing corruption may increase blank and null voting through protest voting; however, according to the model developed in section 3, the relation between these two is ambiguous. To estimate this relation, ideally I would randomly assign disclosure of corruption across municipalities and then measure the differences in corruption levels across both groups. Since unfortunately, this experiment is unfeasible, given the cross-sectional nature of my data a design of the random audit program, I instead, compare municipalities that have their corruption violations disclosed months before the elections with municipalities whose corruption violations are disclosed months after using the following regression:

$$VS_{msyl} = \alpha + \beta_0 Release_{msyl} + \beta_1 Violations_{msyl} + \beta_2(Release_{msyl} * Violations_{mysl}) + X_{ms} + \vee_s + \omega_y + \theta_l + \epsilon_{msyl}$$

Where VS_{msyl} is the null or blank vote share in municipality m, state s, electoral cycle y, lottery l, $Release_{msyl}$ released before elections dummy and $Violations_{msyl}$ is the Z-score of number of violations. The term ϵ denotes unobserved variables that determine vote share. In order for our estimate of interest, β_2, to be consistent, a necessary condition is that the timing of the release of corruption audits be uncorrelated with elections timing. Two main reasons contribute to believing that this hypothesis holds true: First, the design of the program guarantees audit reports are released altogether by an independent federal agency (free from any local influence) only a few months after the draws. Second, if this hypothesis doesn't hold, we should see differences in observables between municipalities audited before and after elections. Results for that analysis can be seen in Table **??** Column (3) displays simple differences between municipalities audited before and after the election. If the timing of audits release is really random, then we shouldn't see any consistent significant difference in characteristics between municipalities audited before and after. Municipalities audited before and after only differ significantly in terms of percentage of the population with high school and in the percentage of households with TV. Nonetheless, when we look at the specification used

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for my main analysis, none of these point estimates are significant. One way or another, to increase precision, I include these variables as controls in my main specification.

Another challenge faced in estimating corruption effects over participation is isolating the direct effect of dissatisfaction. Voters punishing corrupt politicians is an already well-established result in literature (Ferraz & Finan (2008)). Hence, in municipal elections, it is possible that voters switch to null simply as a substitute to having their preferred politician convicted. I focus on gubernatorial elections in order to isolate this mechanism. Governors have no responsibility for local budgets and hence shouldn't be held accountable for corruption that takes place at the municipality level. Furthermore, focusing on gubernatorial elections isolates supply-side effects also documented in the literature (Giacomini (2020)). In order to estimate the relation between violations and the presence of media, I use a model that adds an interaction of violations with the presence of radio, TV, and the internet. I estimate the following specification:

$$VS_{msyl} = \alpha + \beta_0 Release_{msyl} + \beta_1 Violations_{msyl} + \beta_2 Media_{msyl} + \beta_3 (Release_{msyl} * Violations_{mysl}) +$$

$$\beta_4 (Release_{msyl} * Media_{mysl}) + \beta_5 (Violations_{mysl} * Media_{mysl}) +$$

$$\beta_6 (Violations_{mysl} * Media_{mysl} * Release_{msyl}) + X_{ms} + \vee_s + \omega_y + \theta_l + \epsilon_{msyl}$$

Where the coefficient of interest is β_6 and $Media_{mysl}$, denotes the presence of either internet, TV, or AM radio in the municipality.

3.6. Results

Table 3.3 presents the estimation results of models described in the previous section. Both Linear and quadratic specifications present estimators of similar signs and magnitude. On average, a one standard deviation increase on the violations distribution combined with having these violations being released before the election is associated with a significant 0.638 percentage points decrease in null and blank votes on the linear specification and 0.691 percentage points on the quadratic form. The magnitude of this effect is also significant, a 6.5 % and a 7% decrease of a 9.8 percentage points baseline, respectively.

Column (3) displays the results of the semi-parametric estimation. Except for the 4th quintile point estimates increase in absolute magnitude, an result in line with the previous models. Analysis

of this specification contributes to the evidence that our results are not driven by the functional form chosen.

One problem with interpreting this decrease in null votes as an increase in participation is that it can be generated by a decrease in turnout. That is, if null votes are decreasing, but turnout is also decreasing, the overall effect on the proportion of the population that casts valid votes is uncertain. Moreover, one could argue that null votes decrease because voters that would vote null are now staying at home. Hence, it is important to analyze the impacts of corruption on turnout.

This analysis is presented in Table 3.4. Being a further standard deviation from the mean and having audits released before the election is associated with an increase in turn out of 1.79 and 3.57 percentage points for the linear and quadratic model, respectively. Moreover, both estimators are statistically non-significant, which suggests that it is unlikely that a decrease in turnout is the driver of the decrease in null votes.

Tables 3.6 and 3.7 display the results of robustness checks of my two preferred specifications. On the quadratic case (3.6), removing lottery fixed effects increases standard errors and leads to a small absolute reduction in the point estimate (-0.00682 to -0.00614). The same reduction relative to the fully saturated model is observed win the model without State Fixed Effects and controls and without State and Lottery Fixed Effects. I interpret these estimators as contributing to the evidence that my main results are not being driven by a singular choice of model.

Interactions of media are shown in Table 3.8. I only display the coefficients for the triple interaction. On top of being one extra standard deviation away from the mean and having violations released before the election, having a TV station in the municipality reduces null votes by an extra 1.2 percentage points while having an AM Radio reduces by further by 0.957 percentage. In both cases, estimators are significant at 10 % level. The magnitude of these estimators is also meaningful. When compared to the double interaction, the media interaction coefficients are twice as big (and in the AM Radio case, the only one significant). These findings are in line with the literature: Ferraz and Finan (2008) also find larger effects on the presence of radio.

Local TV stations and AM radios are the most common mean of communication for local news in mid-sized Brazilian municipalities. It is important to notice that even though my sample encompassed the 2006, 2010, and 2014 elections, most of the draws are concentrated in the 2006 and 2010 elections, a time where not many Brazilians didn't have access to the internet. For example,

in the 2000 census, less than 3% of households declared to have a computer **??**, a few years later in 2012, only 49% of Brazilian declared having accessed internet in the past three months [1]. Hence it is not surprising that we find no effect for the internet.

What could possibly be responsible for this increase? 3.9 provides a possible answer. Our linear and quadratic models show a positive relationship between corruption and voting in new candidates. Moreover, the linear model displays an increase in votes for new candidates of 0.515 percentage points, a magnitude very similar to the decrease in null votes (0.630), and we cannot reject that these estimators are different from each other statistically. The estimator magnitude is also extremely meaningful. It represents a 16 % increase in votes for new candidates.

We also cannot reject that this estimate is different from zero. However, the semi-parametric model suggests that this is a mere consequence of the functional form imposed as three out of four quintiles displays statistically significant increases when compared to the first quintile. Notice, however, that this is not the only dimension in which candidates may be different. Any dimension in which candidates differ, and it is perceived as more likely to be attributed to corruption may lead voters to

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[1]https://www.avellareduarte.com.br/fases-projetos/conceituacao/demandas-do-publico/pesquisas-de-usuarios-atividades-2/internet-no-brasil-2015-dados-e-fontes/internet-no-brasil-2012-dados-e-fontes/

3.7. Conclusion

Using a large national-level anti-corruption program in Brazil, I analyze the impact of corruption disclosures on voter participation. Different from all of the works in previous literature, I analyze the effects of corruption disclosures on an electoral level different from the one where violations took place. I argue that this is an ideal analysis if one wants to evaluate the consequences for the entire political system. I show that an extra standard deviation on the corruption violation distribution and having violations released before the elections is associated with a decrease in null and blank votes of 6.5 %. This increase is even greater for municipalities that have either a local TV station or a local A.M. radio station.

Furthermore, I also show that this increase in voting is accompanied by an increase in voting in new candidates. I suggest that these factors are related as voters use this information to update upwards the probability that a candidate that is part of the political system to be corrupt. The new/old dichotomy, however, may not be the only dimension in which voters updates their believes. Any characteristic that leads to systematic uneven updates from voters may lead voters to may lead to voters reevaluate utility attributed to one candidate's victory, possibly leading to an increase in mobilization.

Figures
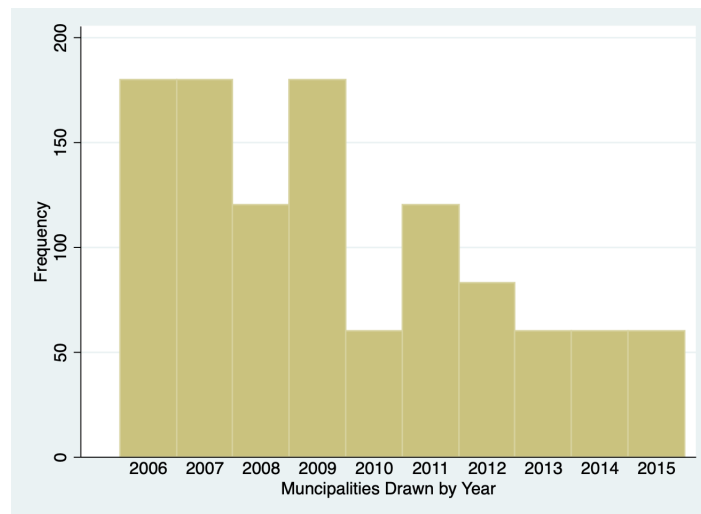
FIGURE 3.1. Distribution of Audited Municipalities per Year
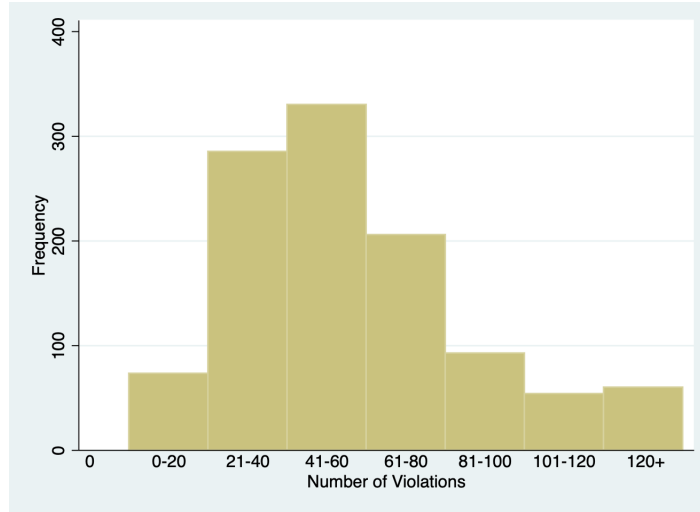
FIGURE 3.2. Distribution of Audited Municipalities per number of Violations

Tables

TABLE 3.1. Descriptive Statistics by Electoral Cycle

	2006		2010		2014	
	mean	sd	mean	sd	mean	sd
Age	54.429	10.815	52.874	10.005	47.767	10.519
Attended College %	0.739	0.440	0.756	0.431	0.789	0.409
Attended HS %	0.972	0.167	0.963	0.191	0.989	0.104
Male %	0.886	0.318	0.894	0.309	0.881	0.325
Single %	0.147	0.355	0.175	0.381	0.151	0.359
PT_PSDB_PMDB	0.232	0.423	0.237	0.427	0.259	0.440
Extreme Left %	0.213	0.411	0.294	0.457	0.243	0.430
Extreme Right %	0.090	0.287	0.031	0.175	0.043	0.204
Observations	211		160		185	

TABLE 3.2. Balance Checks

	Audited Before	Audited After	Difference	Interaction Difference
Population Total	25942.336	23298.481	2643.855	-5779.593
	.	.	(3996.588)	(6364.279)
White %	0.497	0.473	0.023	-0.019
	.	.	(0.018)	(0.023)
Mixed %	0.424	0.448	-0.024	0.022
	.	.	(0.017)	(0.021)
Black %	0.064	0.060	0.004	-0.005
	.	.	(0.004)	(0.004)
Others %	0.016	0.019	-0.003	0.001
	.	.	(0.003)	(0.002)
Literate %	0.865	0.865	-0.000	-0.001
	.	.	(0.003)	(0.005)
High School %	0.158	0.147	0.011**	-0.006
	.	.	(0.005)	(0.008)
College %	0.026	0.025	0.001	-0.001
	.	.	(0.002)	(0.002)
Single %	0.558	0.569	-0.011	0.005
	.	.	(0.008)	(0.012)
Married %	0.377	0.368	0.009	-0.004
	.	.	(0.007)	(0.010)
Income Mean	351.966	343.283	8.684	0.015
	.	.	(10.817)	(14.695)
Income Median	199.395	194.519	4.876	-1.112
	.	.	(4.819)	(6.003)
Phone %	0.162	0.147	0.015	-0.004
	.	.	(0.011)	(0.014)
Wash Machine %	0.143	0.124	0.018	-0.003
	.	.	(0.011)	(0.014)
Radio %	0.803	0.791	0.011	0.008
	.	.	(0.009)	(0.012)
TV %	0.738	0.718	0.021*	0.005
	.	.	(0.014)	(0.021)
CPU %	0.028	0.025	0.003	-0.003
	.	.	(0.002)	(0.003)

Standard Error in parenthesis

Interaction Difference shows β_3 of the following model:

$Var\_msyl = \alpha + \beta\_0 Release\_msyl + \beta\_1 Violations\_msyl + \beta\_3(Release\_msyl * Violations\_mysl) + \epsilon\_msyl$

TABLE 3.3. Effects of Corruption on Null Votes

	(1) Null %	(2) Null %	(3) Null %
Violations*Released	-0.00630**	-0.00682***	
	(0.00281)	(0.00262)	
Violations	-0.000959	-0.000798	
	(0.00114)	(0.00129)	
Released Before	-0.00533	-0.00701	0.00629
	(0.00578)	(0.00592)	(0.00630)
$Violations^2$ *Release		0.00228	
		(0.00156)	
$Violations^2$		-0.000202	
		(0.000566)	
Release* 2nd Quintile			-0.00772
			(0.00520)
Release* 3rd Quintile			-0.0156***
			(0.00501)
Release* 4th Quintile			-0.00388
			(0.00530)
Release* 5th Quintile			-0.0206***
			(0.00606)
Controls	Yes	Yes	Yes
Draw FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean	0.098	0.098	0.098
Observations	1153	1153	1153
R2	0.598	0.598	0.602

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.4. Effects of Corruption on Turnout

	(1) Turn Out %	(2) Turn Out %	(3) Turn Out %
Violations*Released	0.00209	0.00397	
	(0.00448)	(0.00472)	
Violations	-0.00652***	-0.00900***	
	(0.00244)	(0.00267)	
Released Before	-0.00735	-0.00436	-0.0172*
	(0.00942)	(0.00985)	(0.0102)
$Violations^2$ *Release		-0.00324	
		(0.00245)	
$Violations^2$		0.00248**	
		(0.00110)	
Release* 2nd Quintile			0.00760
			(0.00912)
Release* 3rd Quintile			0.0190**
			(0.00946)
Release* 4th Quintile			0.00854
			(0.00926)
Release* 5th Quintile			0.00844
			(0.0110)
Controls	Yes	Yes	Yes
Draw FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean	0.808	0.808	0.808
Observations	1153	1153	1153
R2	0.481	0.483	0.484

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.5. Robustness Test

	(1) Null %	(2) Null %	(3) Null %	(4) Null %
Released Before	0.00629	0.00341	0.00484	0.00511
	(0.00630)	(0.00629)	(0.00359)	(0.00423)
Release* 2nd Quintile	-0.00772	-0.00451	-0.00415	-0.00345
	(0.00520)	(0.00531)	(0.00527)	(0.00573)
Release* 3rd Quintile	-0.0156***	-0.0123**	-0.0118**	-0.0166***
	(0.00501)	(0.00495)	(0.00491)	(0.00594)
Release* 4th Quintile	-0.00388	-0.00160	-0.000914	-0.000476
	(0.00530)	(0.00531)	(0.00517)	(0.00593)
Release* 5th Quintile	-0.0206***	-0.0191***	-0.0173***	-0.0156**
	(0.00606)	(0.00602)	(0.00574)	(0.00715)
R2	0.6021	0.5734	0.5700	0.3977
Observations	1153	1162	1162	1153
Lottery FE	Yes	No	No	Yes
State FE	Yes	Yes	No	No
Controls	Yes	Yes	Yes	No

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.6. Robustness Check - Null & Blank

	(1) Null %	(2) Null %	(3) Null %	(4) Null %
Violations*Released	-0.00682***	-0.00614**	-0.00535**	-0.00567**
	(0.00262)	(0.00265)	(0.00248)	(0.00288)
$Violations^2$ *Release	0.00228	0.00154	0.00158	0.00294*
	(0.00156)	(0.00160)	(0.00155)	(0.00161)
Violations	-0.000798	-0.000604	-0.000498	0.00216
	(0.00129)	(0.00129)	(0.00123)	(0.00133)
$Violations^2$	-0.000202	-0.000247	-0.000249	0.000644
	(0.000566)	(0.000592)	(0.000559)	(0.000621)
Released Before	-0.00701	-0.00694	-0.00421*	-0.00544*
	(0.00592)	(0.00577)	(0.00246)	(0.00294)
R2	0.5983	0.5696	0.5663	0.3949
Observations	1153	1162	1162	1153
Lottery FE	Yes	No	No	Yes
State FE	Yes	Yes	No	No
Controls	Yes	Yes	Yes	No

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.7. Robustness Check - Null & Blank

	(1) Null %	(2) Null %	(3) Null %	(4) Null %
Released Before	0.00629	0.00341	0.00484	0.00511
	(0.00630)	(0.00629)	(0.00359)	(0.00423)
Release* 2nd Quintile	-0.00772	-0.00451	-0.00415	-0.00345
	(0.00520)	(0.00531)	(0.00527)	(0.00573)
Release* 3rd Quintile	-0.0156***	-0.0123**	-0.0118**	-0.0166***
	(0.00501)	(0.00495)	(0.00491)	(0.00594)
Release* 4th Quintile	-0.00388	-0.00160	-0.000914	-0.000476
	(0.00530)	(0.00531)	(0.00517)	(0.00593)
Release* 5th Quintile	-0.0206***	-0.0191***	-0.0173***	-0.0156**
	(0.00606)	(0.00602)	(0.00574)	(0.00715)
R2	0.6021	0.5734	0.5700	0.3977
Observations	1153	1162	1162	1153
Lottery FE	Yes	No	No	Yes
State FE	Yes	Yes	No	No
Controls	Yes	Yes	Yes	No

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.8. Effects of Media Interaction on Voting

	(1) Null %	(2) Null %	(3) Null %
Violations*Released	-0.00294	-0.00502*	-0.00445
	(0.00424)	(0.00298)	(0.00314)
Violations*Released*Internet	-0.00595		
	(0.00500)		
Violations*Released*TV Station		-0.0120*	
		(0.00648)	
Violations*Released*AM Radio			-0.00957*
			(0.00518)
Mean	0.0979	0.0979	0.0979
Controls	Yes	Yes	Yes
N	1153	1153	1153

Standard errors in parentheses

Media Information Source: Perfil Munic 2009

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.9. Effects of Corruption on New Politicians

	(1) New Cand. %	(2) New Cand. %	(3) New Cand. %
Violations*Released	0.00478	0.00515	
	(0.00629)	(0.00709)	
Violations	-0.00423	-0.00371	
	(0.00304)	(0.00394)	
Released Before	-0.000395	0.000763	-0.0205
	(0.0124)	(0.0133)	(0.0171)
$Violations^2$ *Release		-0.00126	
		(0.00320)	
$Violations^2$		-0.000415	
		(0.00108)	
Release* 2nd Quintile			0.0234**
			(0.0116)
Release* 3rd Quintile			0.0420*
			(0.0223)
Release* 4th Quintile			0.0230*
			(0.0123)
Release* 5th Quintile			0.00376
			(0.0182)
Controls	Yes	Yes	Yes
Draw FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean	0.031	0.031	0.031
Observations	903	903	903
R2	0.613	0.613	0.616

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
Total Population	30506.6	19739.8	23266.3	22621.1	24803.2	18770.0
	(184920.7)	(30247.3)	(36000.1)	(34982.5)	(37053.6)	(26218.6)
Total Voters.	23208.7	15089.4	17885.8	17394.1	19948.7	15097.2
	(142601.7)	(22262.9)	(26280.1)	(25621.2)	(28803.8)	(20348.2)
Male %	0.508	0.508	0.508	0.508	0.508	0.508
	(0.0177)	(0.0174)	(0.0181)	(0.0180)	(0.0184)	(0.0180)
Electricity %	0.869	0.868	0.853	0.854	0.868	0.860
	(0.165)	(0.165)	(0.171)	(0.171)	(0.161)	(0.163)
White %	0.526	0.525	0.471	0.477	0.500	0.490
	(0.255)	(0.256)	(0.245)	(0.246)	(0.246)	(0.246)
Income Total	14193.2	8765.7	10170.4	9923.8	11098.4	8197.0
	(95799.4)	(14351.3)	(16834.5)	(16428.7)	(17560.3)	(12379.7)
Income Mean	371.1	365.7	354.8	356.4	376.9	350.5
	(171.6)	(164.4)	(171.8)	(172.1)	(172.3)	(176.1)
Mean Male I.	433.5	426.7	411.6	413.9	440.2	409.6
	(222.9)	(214.1)	(222.1)	(222.9)	(223.7)	(233.3)
Mean Female I.	270.9	266.8	262.9	263.4	274.4	254.4
	(104.9)	(97.94)	(103.0)	(102.2)	(103.1)	(90.09)
Income Median	205.8	203.4	199.8	200.2	207.8	195.0
	(72.69)	(69.01)	(70.25)	(69.64)	(70.97)	(61.78)
Median Male I.	234.3	231.2	224.2	224.9	235.9	219.8
	(99.86)	(95.64)	(96.11)	(95.28)	(95.32)	(84.94)
Median Female I.	169.2	167.5	167.7	167.5	170.0	162.3
	(42.17)	(38.51)	(40.17)	(39.45)	(41.16)	(31.83)
Observations	27485	27108	6985	6206	3266	1306
Run-off	Yes	No	No	No	No	No
Single Gender Race	Yes	Yes	No	No	No	No
Multiple F. Cand.	Yes	Yes	Yes	No	No	No
Unreported Managers	Yes	Yes	Yes	Yes	No	No
Outside Bandwidth	Yes	Yes	Yes	Yes	Yes	No

Municipality Characteristics by electoral race. Standard deviation in parenthesis

TABLE A.2. Robustness Checks for Impact on Head of Board Gender

|	(1)	(2)	(3)	(4)
Panel A: Education Board				
Win	0.100*	0.115	0.100*	0.113
	(0.0576)	(0.0727)	(0.0575)	(0.0735)
Mean	0.702	0.702	0.702	0.702
Bandwidth	0.119	0.165	0.120	0.161
N_b	1068	1357	1069	1341
Selector	cerrd	cerrd	cersum	cersum
Panel B: Education Board				
Win	0.203***	0.263***	0.189***	0.238***
	(0.0708)	(0.0917)	(0.0678)	(0.0829)
Mean	0.479	0.479	0.479	0.479
Bandwidth	0.0986	0.123	0.109	0.155
N_b	855	1003	924	1215
Selector	cerrd	cerrd	cersum	cersum
Panel B: Finance Board				
Win	-0.0445	-0.0163	-0.0352	0.0227
	(0.0649)	(0.0800)	(0.0667)	(0.0864)
Mean	0.304	0.304	0.304	0.304
Bandwidth	0.103	0.147	0.0975	0.127
N_b	953	1266	912	1119
Selector	cerrd	cerrd	cersum	cersum
Panel D: Compt. General				
Win	-0.0283	-0.00727	-0.0298	-0.0147
	(0.0582)	(0.0779)	(0.0578)	(0.0751)
Mean	0.268	0.268	0.268	0.268
Bandwidth	0.115	0.146	0.117	0.158
N_b	1035	1245	1043	1305
Selector	cerrd	cerrd	cersum	cersum

Standard errors clustered at municipality level in parentheses. Odd columns use local linear specification, evens use local quadratic.
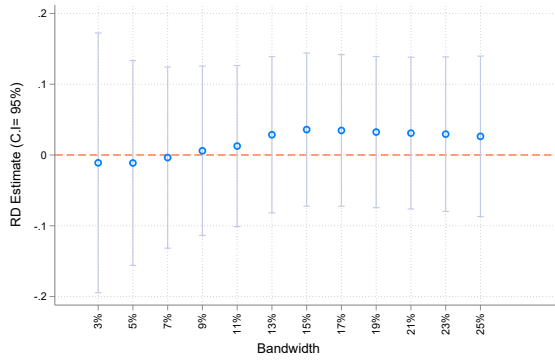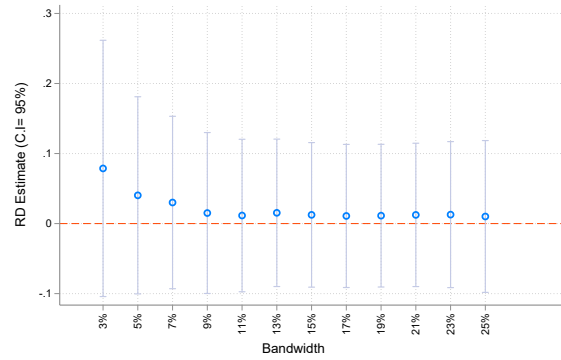* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
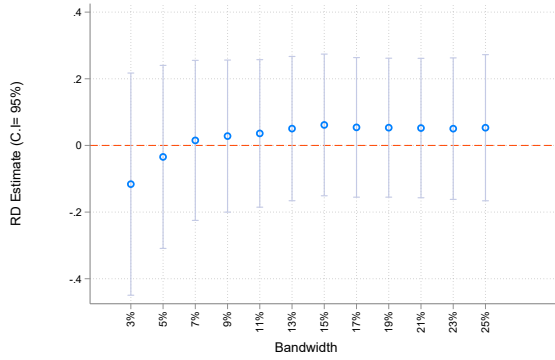
(a) Races without reelection possibility - 2017
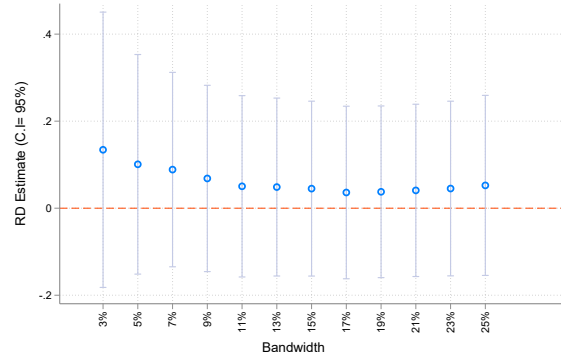
(b) Races without reelection possibility - 2018

(c) Temp. Contracts during mayor's term - 2017

(d) Temp. Contracts during mayor's term - 2018

FIGURE B.1. Bandwidth Sensibility.

Note: Point estimate and 95% confidence intervals. Dependent variable is the porportion of non-white managers with temporary contracts.

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