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Essays on Development Economics and Political Economy

by

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Committee in charge:

Professor Frederico Finan, Chair
Professor Ernesto dal Bó
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Abstract

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This dissertation studies the impacts of *quid pro quo* relationships between politicians and their supporters on public policy and the labor market of public servants.

Chapter 2 investigates the effects of a temporary shock in the likelihood of detecting officeholders' misbehavior on the composition of public expenditures and reelection rates of incumbent politicians, taking into consideration the existence of an ongoing relationship between lawmakers and campaign contributors. This shock derives from an experiment developed by Zamboni and Litschig (2018) that temporarily and randomly increased the likelihood of selection of Brazilian municipalities into a preexisting audit program against corruption. I document a reduction in the probability that treated municipalities engaged in construction projects during the experiment, with a partial reversal in the reelection year, as the audit risk returned to its baseline mark. I report that the threat of audit mostly impacted the actions of politicians in locations with previous experience in the anti-coruption program and where mayors had close relationships with historically corrupt campaign donors, such as construction companies. Moreover, I estimate a large contraction in the likelihood of contribution from these donors to the reelection campaigns of treated mayors and a sizeable reduction in their reelection rates.

Chapter 3, co-authored with Thiago Scot, analyzes the effects of a change in power at the local level on the employment of health care workers in Brazil. It compares municipalities where the incumbent party barely won a bid for reelection with those where the incumbent party barely lost, in order to estimate the effects of political turnover on the labor market for public servants. It reports a larger rate of termination of employment contracts in municipalities affected by these shocks, in comparison to the control group. It shows that employees are more likely to leave the health care labor force permanently after political turnover and that public employees on unstable career tracks drive most of the disruption in the employment contracts.

To all the loved ones that supported me through this journey

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Acknowledgments

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Chapter 1

Dissertation Introduction

Democracies all over the world rely on regular elections as the fundamental tool to hold politicians accountable to their performance in office. Frequently, politicians depend on the support from individuals, parties, and firms in order to ascend to office, enact policies, and eventually succeed in reelection races. Their supporters, in turn, usually expect to be compensated by lawmakers once the electoral process is concluded.

This dissertation investigates the implications of this *qui pro quo* relationship on the behavior of officeholders, and its consequences for policies, in a broad sense, and its impacts on the labor market of public servants. It is divided into two main chapters, both of which use empirical methods and data within the Brazilian political framework.

Chapter 2 explores how a temporary threat of audit impacts the profile of public expenditures and reelection rates of officeholders, taking into account the existence of a relationshp between politicians and campaign contributors. It does so by using an experiment developed by Zamboni and Litschig (2018) that exposed a subset of mayors to a temporary shock in the probability of selection into a pre-existing audits program against corruption, and released information to treated mayors about their status. Among the 5,500 municipalities eligible to participation in the program, 120 were exposed to an increase in the likelihood of audits of 25% for a year, versus 5% in the control group. I estimate difference-in-differences regressions and quantify treatment effects on discretionary federal transfers to the local government, as well as presenting OLS estimates of treatment effects on campaign contributions and reelection outcomes in the election that followed the experiment.

I report sizeable reductions in the probability that treated municipalities execute construction projects during the experiment year, with a subsequent partial rebound in the reelection year, at the same time as the audit risk returned to the baseline value. I observe that the threat of audits mainly impacts the behavior of politicians in locations with previous experience in the anti-corruption program and where mayors had strong ties with historically corrupt campaign donors, such as construction companies. Furthermore, I estimate a large contraction in the likelihood that these donors contribute to the reelection efforts of treated mayors and a substancial reduction in their reelection rates.

In chapter 3, co-authored with Thiago Scot, we analyze the effects of change in power at the local level on the employment of health care workers. We compare municipalities where the incumbent party barely won a bid for reelection with those where the incumbent party barely lost in our estimation of the effects of political turnover on public servants. We exploit a dataset with information of all contracts for Brazilian health care employees and investigate changes in their employment status and hours provided, after the political turnover shocks, using variation from three different election cycles.

We detect a larger rate of cessation of employment contracts in municipalities affected by these shocks, compared to the control group. We observe considerable effects on the employment status of health care providers in the first semester of the new government. Additionally, we find that employees are more likely to leave the health care labor force altogether following the political turnover event. Finally, we show evidence that public employees on less stable careers than their counterparts drive most of the disruption in the employment contracts.

Chapter 2

Corrupt Politicians, Campaign Donors, and the Power of a Threat: Evidence from a Policy Experiment in Brazil

2.1 Introduction

Rational agents respond to incentives: a higher likelihood or intensity of punishment inhibits wrongdoing (Becker, 1968; Becker and Stigler, 1974). In recent years, the literature has emphasized the effectiveness of audits as instruments against political corruption (Avis et al., 2018; Zamboni and Litschig, 2018) and misbehavior more broadly (Kleven et al., 2011; Olken, 2007). Another strand of the literature has provided theoretical and qualitative evidence for the reasons that politicians engage in corruption. Besides the self-enrichment motive, corruption serves as a bargaining tool for officeholders, with the aim to garner resources from firms to support their costly political campaigns (Boas et al., 2014; Weschle, 2015; Fisman, 2001; Ramalho, 2007; Fleischer, 1997).

Although there is an increased understanding of the effects of audits on corruption and election outcomes (Ferraz and Finan, 2008; Zamboni and Litschig, 2018), there is scant empirical evidence regarding their impact on the relationship between corrupt politicians and campaign donors, as well as on the provision of public goods. Furthermore, the long-term effects of policies that provide transitory shocks to the likelihood of punishment for misbehavior are not well understood.

In this chapter, I investigate the effect of the threat of an audit in the provision of public goods and reelection rates of politicians, mediated by the deterioration of the relationship between local politicians and campaign contributors. I exploit an experiment that temporarily increased the likelihood that municipalities are selected into an anti-corruption audits program in Brazil in May of 2009 for a year (Zamboni and Litschig, 2018). Mayors in the group with a higher audit risk received letters that disclosed their treatment status. The treatment group consisted of 120 municipalities that faced an audit probability of 25%

in a year, whereas the control group included the remaining 5,400 municipalities with a 5% annual audit risk.[1] I estimate difference-in-differences specifications to assess treatment effects on discretionary federal transfers to municipalities using a panel dataset from 2000 to 2012. Additionally, I analyze OLS estimates of treatment effects on political outcomes for the election that followed the experiment in 2012.

I find a reduction of 21p.p. in the likelihood that treated municipalities carry out construction projects in the year of the experiment, and a rebound of +8.8p.p. in the year when reelection campaigns happen, when the audit risk returned to the baseline. The group responsive to the threat consisted of municipalities with previous experience in the anti-corruption program and with mayors that relied on historically corrupt campaign donors, such as construction companies (Fleischer, 1997). Moreover, I detect a decline of 23p.p. in the probability that these firms contribute to the reelection campaigns of incumbent politicians and a decrease of 50p.p. in reelection rates of mayors.

Zamboni and Litschig (2018) report that these letters provoked a reduction of 10p.p. in corruption findings. I interpret the fall in the expenditure in construction works as a side effect of the decrease in wrongdoing reported by these authors.[2] Politicians that had strong ties with developers avoid promoting construction projects during the experiment as a way to not engage in corruption with these campaign donors, who were not aware of the experiment. Although mayors reverse their course of action with respect to developing new construction projects in the election year, this shift is not enough to compensate for the initial fall in contracts to these companies. Because construction firms received lower returns in public contracts on their campaign donations, they choose not to finance these politicians for their reelection campaigns. Furthermore, mayors' political prospects deteriorate and the likelihood that they will keep their seat plunges.

The findings of this chapter relate to five growing literatures. First, this study contributes to a literature on how economic agents respond to changes in punishment. For example, Bar-Ilan and Sacerdote (2001) show that both and increase in the probability of punishment and in the values of fines for traffic violations in Israel reduces violations for a diverse pool of drivers. Di Tella and Schargrodsky (2003) analyze the effects of a crackdown on corruption in the public hospitals in Argentina and report a reduction in prices paid by these facilities for basic and homogenous inputs.

A second and related branch of the literature consists of studies that investigate the effects of audits on political corruption. Avis et al. (2018) study long-term effects of the Brazilian anti-corruption program and find a reduction of 8p.p. on objective measures of corruption. Moreover, they structurally estimate a political agency model and show that their reduced-form results are mainly driven by perceived nonelectoral costs of engaging in

[1] The threat was fulfilled and 30 treated municipalities were audited in May of 2009.

[2] The provision of goods that rely on constructions tend to be more affected by political corruption compared to other projects due to a relatively small competition among supplier firms and high-value per project (Mauro, 1998; Shleifer and Vishny, 1993).

corruption. Furthermore, Zamboni and Litschig (2018) conducted the experiment I use in this chapter and present evidence that a higher audit risk causes a decrease of 10p.p. in the share of audited resources involved in corruption. Olken (2007) uses data from a policy experiment that increased the probability of government audits from 4% to 100% in Indonesia to show a decline of 8p.p. in missing expenditures for road projects as evidence of a fall in corruption. Finally, Bobonis et al. (2011) reports that releasing audit results before election in Puerto Rico reduces corruption in the short term, but mayors explore their reputational gains in their second term as officeholders by engaging in more wrongdoing, so effects on corruption are short-lived. While these studies present evidence that audits have discipline effects on economic agents, I contribute to the literature by investigating how they can change the provision of public goods and the relationship between corrupt politicians and campaign donors.

Third, there is an emerging literature on the effect of threats of punishment for misbehavior through letters. As mentioned above, Zamboni and Litschig (2018) set up the experiment that temporarily raised audit risk to a subset of municipalities in Brazil and sent letters to treated mayors to make them aware of their status. Kleven et al. (2011) present evidence that Danish taxpayers respond to both audits and threat of audits, by disclosing higher self-reported incomes and therefore incurring in less tax evasion. Additionally, Pomeranz (2015) shows that announcing additional monitoring to taxpayers in Chile results in a reduction in tax evasion. Also, they find a smaller impact on transactions that depend on paper trail, such as VAT, which implies that VAT can serve as a deterrent to tax evasion. I contribute to this literature showing that Brazilian mayors respond to a letter with a threat of audits by changing their effort in applying for federal transfers and reducing their expenditure in construction projects.

Another relevant branch of the literature concerns the relationship between political connections, especially through campaign donations, and corruption. For example, Boas et al. (2014) use a regression discontinuity approach to show that firms specialized in construction projects that donate to candidates who barely win their elections for Brazilian Congress receive up to 14 times returns in contracts on the previously donated resources. Weschle (2015) present a theoretical model for the drivers of corruption: self-enrichment motives, financial support for political campaigns, and as a way to ensure job opportunities after leaving office. Using data from India, he shows that these drivers are substitutes and in places where electoral races are more competitive there is less opportunity for corruption for mayors' personal wealth and more dependence on campaign donors. Fleischer (1997) provides a historical overview of the impeachment of a Brazilian president in 1992 and presents evidence on the relationship between corrupt politicians and construction companies in Brazil. He reports that the political campaign for the president managed a massive corruption scheme with Brazilian firms, especially construction companies, and their relevance as one of the main peers in the campaign donation market in that country. Furthermore Ramalho (2007) study how corrupt and politically connected firms suffered from the impeachment of the Brazilian president in 1992; they relied on government contracts and report short term losses after the

political change, but return back to baseline in the medium-term. Finally, Fisman (2001) constructs a measure of political connectedness during a dictatorial regime in Indonesia to provide empirical evidence that connected firms were able to extract rents because of their close relationship with the government. I provide a contribution to this literature by documenting a relationship between corrupt politicians and firms with known histories of illicit activities such as developers in the Brazilian setting.

The fifth related branch of the literature focuses on how political corruption distorts the provision of public goods. In this regard, Brollo et al. (2013) shows that Brazilian municipalities with revealed corruption findings through the anti-corruption audits program receive fewer resources from the central government for projects of capital investments such as construction works. Nevertheless, Zamboni and Litschig (2018) do not find effects of a threat of audits on the provision of health services at the local level. In turn, Mauro (1998) uses cross-country data to show that places with higher corruption levels also invest less in education. My contribution to this literature consists of providing empirical evidence that politicians respond to a higher likelihood of punishment for corruption by reducing their expenditure on construction projects, therefore changing the profile of public goods provided for their constituents.

The rest of the chapter is structured as follows. Section 2.2 provides background on the Brazilian anti-corruption program, the experiment of increasing the probability of audits, and presents the data used in the empirical analysis. Section 2.3 discusses the research design and Section 2.4 present the results. Section 2.5 concludes the chapter.

2.2 Institutional Background and Data

2.2.1 Brazilian anti-corruption program

In 2003, Brazil created as institution with autonomous power to investigate misuse of government resources, named Controladoria Geral da União (CGU). In that year, they started a randomized audits program (Programa de Fiscalização por Sorteios Públicos) that selected municipalities for a thorough investigation of their public accounts. CGU mobilized a team of auditors to visit each municipality drafted in the lotteries for around 2 weeks, covering a period of approximately 3 years of information on public accounts.

Over time, the number of investigated municipalities and frequency of the lotteries varied, but for the relevant period for this chapter, 60 municipalities were selected for each lottery, that happened once per quarter. [3] This program focused on municipalities of small and medium sizes, with a population of up to 500,000 people, excluding state capitals. Of the 5,570 municipalities in Brazil, around 5,500 qualified for participation in these lotteries.

[3] In Section 2.2.2, I present a timeline of the political cycle and the relevant time frame for the empirical analysis.

In a given year, each municipality had an approximate 5p.p. probability of being drafted to this program.[4]

Ferraz and Finan (2008) analyze the effects of the release of the results of the audits on reelection outcomes of incumbent mayors and find that the challenger uses this information strategically by revealing it to the voters, leading to a decrease in the reelection rates of corrupt politicians. Moreover, Avis et al. (2018) show that CGU's anti-corruption program results in a long-term reduction of corruption levels in municipalities of up to 8 percent.

2.2.2 Experiment of increasing the probability of participation in the audits' program

In 2009, Zamboni and Litschig (2018) partnered with CGU to conduct an experiment to assess how corruption levels change when the probability of participation in the audits' program is temporarily increased. In order to do so, they randomly selected 120 municipalities to participate in this policy evaluation in May of 2009 and sent letter to mayors in this group to disclose the fact they had a higher likelihood of being audited. Out of the 120 municipalities in this group, 30 were audited at the end of the experiment (in the lottery of May of 2010), which implies on an annual audit risk of 25p.p for the duration of the experiment. [5] Therefore, the experiment generated an temporary raise in the probability of participation in the audits program of 20p.p., from a baseline of 5p.p. in the control group. From May of 2010 and onwards, the annual audit risk for the treatment group returned to the baseline of 5p.p.

Zamboni and Litschig (2018) show that the letters reduce the share of audited resources involved in corruption by 10p.p. However, because they can only observe corruption in the municipalities that were actually audited in the program, their results focus on comparing 30 municipalities in the treatment group with 90 municipalities in the control group. [6] In this chapter, I do not rely on the corruption data for the empirical analysis, which allows the use of the 120 municipalities as a treatment group and the remaining 5,100 as a control group. [7] While Zamboni and Litschig (2018) report that politicians respond to the increased audit risk by reducing corruption, there is less understanding on the background effects of it. In the Section 2.4, I provide evidence that the experiment impacts the profile of public

[4]The probability that a municipality was selected for a given lottery was around 1.25p.p., taking into account the fact that there is a grace period in which municipalities are exempt of participation to a few lotteries after they were drafted in the program.

[5]For the remainder of the chapter, I consider that the period of the experiment consists of time frame between May of 2009 and April of 2010, and that the period post-experiment starts in May of 2010.

[6]The special audit that happened in May of 2010 selected 30 municipalities in the treatment group and 30 in the control group. They also include an additional set of 60 municipalities in the control audited in March of 2010 to increase the power in the estimation.

[7]Around 200 municipalities are discarded from the estimations as a result of not receiving any campaign donations in 2008, which prevents the computation of the heterogeneity described in Section 2.3.

goods provided and reelection rates of the mayors, through a degeneration of the relationship between politicians and campaign donors.

Figure 2.1 shows the timeline of the experiment and political cycle, for the relevant period for the empirical analysis. First, elections at the local level in Brazil happen once every four years and mayors have a two-term limit. [8] Mayors received campaign contributions up until October of 2008, when elections happened. Their terms started on January 1st of 2009 and ended on December 31st of 2012. First-term mayors can run for reelection in October of 2012 and receive campaign donations until that date. The experiment started in May of 2009, in the first semester of the political term, when treated mayors received the letters informing them of the experiment. The experiment formally ended in May of 2010, when 30 out of the 120 municipalities where selected to participate in the audits program.

2.2.3 Discretionary federal transfers to municipalities in Brazil (block grants)

The outcomes analyzed in Section 2.4.1 are discretionary transfers from the federal government to municipalities (block grants). These earmarked grants are fundamental resources for capital expenditure at the local level: from new constructions to renovations of public facilities, as well as the purchase of equipments and inputs for the provision of public services. Azulai (2017) explains that 89% of the overall budget in municipalities were directed to payment of personnel, pensions and regular operations in the municipality, leaving a small amount to capital expenditure .[9] Thereupon, discretionary transfers have been historically the main source of funds to construction and renovation projects.

Moreover, mayors need to submit a lengthy application to specific ministries in order to be considered for block grants using an online system knows as SICONV. In that regard, the effort they put in this process play a substantial role on the likelihood of receiving these federal transfers. In fact, Panel A of Table 2.2 shows that 26% of the observations at the municipality-year level report no amounts of block grants. The values are considerable higher for construction works (45%) and non-construction projects (41%).[10] Panel B reveals average values of construction or renovation projects of around R$1 million per year and Panel C reports that these activities correspond to an average of 53% of the total block grants received by the municipalities.

[8] Politicians who governed for two consecutive terms can run for office again, as long as they respect a grace period of four years outside the office.

[9] Local taxes and constitutional federal transfers are mainly used for these expenses.

[10] I used Text Mining techniques in order to categorize block grants into construction and non-construction uses. For more details on that and in the most common keywords in the projects that block grants financenwq21, check A.1.1.

2.3 Research Design

I examine whether the threat of audits impacts the transfers received from the federal government, the profile of donations to mayors' reelection campaigns, and the reelection outcomes of mayors by using the random variation induced by the letters with the audits' threat. The treatment group is composed by the 120 municipalities that received the letters in May of 2009 and the control group contains all the remaining municipalities.[11] Although Zamboni and Litschig (2018) document a reduction in corruption findings caused by the increase in the probability of audits, it is uncertain whether this experiment would change politicians' behavior regarding the request of discretionary federal funds. Given the panel structure of the data on block grants and the fact that these outcomes depend heavily on the mayors' effort when applying for them, I test whether the letter results in changes in block grants received by local politicians using the following model for municipality m and year t:

$$Block\ grants_{mt} = [\sum_{k \in K} \beta_{1,k}\ Letter_m \times \lambda_k] + \gamma Z_{mt} + c_m + \lambda_t + u_{mt} \tag{2.1}$$

where $Block\ grants_{mt}$ is either an indicator for a municipality m that receives any block grants in a year t. [12] [13] The variable Z_{mt} refers to population of the municipalities and controls for size effects on block grants, and the terms c_m and λ_t are fixed effects of municipalities and year, respectively. The error term u_{mt} captures unobserved determinantes of the block grants. The set K varies with our specifications: for simplicity when presenting the results, I use $K = \{2009, \text{after}\}$ in the tables, in which $\beta_{1,k}$ coefficients with $k = 2009$ reflect contemporaneous treatment effects while the experiment was happening, whilst coefficients with $k =$ after refer to average treatment effects in the same political term but after the experiment was over (for years 2010, 2011, and 2012). I adopt a more flexible specification in figures that plot the estimates and confidence intervals for the treatment effects, estimating yearly treatment effects, so that $K = \{2009, 2010, 2011, 2012\}$ in these regressions.

I also test whether treatment effects vary with respect to two key factors. The first one refers to whether the municipality had experienced the anti-corruption audits program before the experiment took place in 2009, and captures the fact that previous experience in the audits program serve as a credibility shock to the threat of audits in the experiment. If politicians are not familiar with audits program when the experiment happens, a threat of audits might not lead to any changes in their behavior.

[11] I also excluded state capitals and municipalities with population above 500,000 people, because they do not participate in this audits program.

[12] Because of frequent zeros in block grants, I use the inverse hyperbolic sine transformation (IHS) for evaluating the treatment effects on the nominal values of block grants in A.2.1.

[13] In the panel dataset with block grants, years were shifted to start in May and end in April, as a result of the experiment starting in May of 2009.

The second factor involves the intensity to which mayors relied on donations from construction companies to their campaigns in 2008 elections. In the Brazilian political framework, construction firms are important peers in the market of campaign donations to politicians, contributing both directly to mayors campaigns and indirectly through the mayors' political parties. In the data used in this chapter, I can only observe direct contributions to the politicians and adopt the share of direct donations from construction firms as a measure of mayors' dependence on these companies. Throughout Brazilian history, construction companies have financed political campaigns of a wide set of parties and politicians in a *quid-pro-quo* fashion: once politicians get elected, these firms expect to receive illicit benefits regarding the provision of services to the government (Boas et al., 2014; Weschle, 2015; Fisman, 2001; Fleischer, 1997; Geddes and Neto, 1992; Power and Taylor, 2011; Ramalho, 2007). The Car-Wash Operation (*"Operação Lava Jato"*) is a recent example of this type of relationship, that took place from 2014 to 2021. This was a massive political scandal that involved corrupt politicians, public servants, and more than 50% of the biggest 50 construction firms in the country, which financed politicians and received illegal resources from the government. In Figure 2.2, I show a positive correlation between the logarithm of corruption findings in municipalities in the period of 2005-2008 and the share of construction donations received by mayors in the 2008 elections, once the effect of population size is removed from both variables. I interpret the share of donations from construction companies in 2008 as a proxy for the type of politician and the strength of their commitment to these firms: corrupt mayors score higher on the share of campaign donations received from these companies and rely more heavily on them in order to get elected. These incumbents might react differently to receiving the letter with a threat of audits against corruption: on one hand, they might aim to reduce corruption in order to minimize the chance of criminal and electoral punishment, on the other hand, they need to honor their commitment with campaign donors, such that they secure funds for their reelection campaigns in 2012.

I extend Equation 2.1 to include heterogeneous treatment effects with respect to participation in the audits program before the experiment and the share of donations from construction companies in the 2008 elections:

$$\begin{aligned} Block\ grants_{mt} = & [\sum_{k \in K} \beta_{1,k}\ Letter_m\ \lambda_k + \beta_{2,k}\ Letter_m\ \lambda_k\ A_m + \beta_{3,k}\ Letter_m\ \lambda_k\ C_m \\ & + \beta_{4,k}\ Letter_m\ \lambda_k\ A_m\ C_m + \alpha_{1,k}\lambda_k\ A_m + \alpha_{2,k}\lambda_k\ C_m + \alpha_{3,k}\lambda_k\ A_m\ C_m] \\ & + \gamma Z_{mt} + c_m + \lambda_t + u_{mt} \end{aligned} \tag{2.2}$$

where A_m is an indicator variable on whether the municipality had experienced the audits program before the experiment took place in 2009, C_m is the share of campaign donations in 2008 elections of an elected mayors that originated from construction companies (in p.p.), and the remaining variables are defined in the same way as Equation 2.1. The interaction coefficient $\beta_{4,k}$ captures differential sensitivities of the treatment effect to respect to construction donations, between previously audited and not previously audited munici-

palities. If politicians respond differently to the threat of audits when their municipality had experienced the audits program and they were committed to construction companies, then $\beta_{4,k}$ will be different than zero. It is important to note that both the variables $Letter_m$ and A_m are exogenous variables, because the source of variation for both is random. [14]

Furthermore, I estimate heterogeneous treatment effects on the donations to mayors' reelection campaigns in 2012 and their reelection outcomes. The data is structured in a cross section and the specification used for the estimation of these treatment effects is:

$$\begin{aligned} Political\ outcomes_m = &\beta_1\ Letter_m + \beta_2\ Letter_m\ A_m + \beta_3\ Letter_m\ C_m + \beta_4\ Letter_m\ A_m\ C_m \\ &+ \alpha_1\ A_m + \alpha_2\ C_m + \alpha_3\ A_m\ C_m + \gamma Z_m + u_m \end{aligned} \tag{2.3}$$

where $Political\ outcomes_m$ reflects either the value of campaign donations that mayors received in 2012 or indicator variables to respect to reelection outcomes of mayors that were not term-limited. [15] All the other variables are defined in the same fashion as Equation 2.2.

I perform a balance check with demographic and political variables in Table 2.1. There are no statistical differences between the treatment and control groups for any of the demographic variables from the 2010 Census and the political variables from the 2008 elections. Regarding the variables used in the computation of heterogeneous treatment effects (A_m and C_m), I also do not observe statistical differences between groups. Additionally, even conditioning the sample on the municipalities that received some donations from construction companies, there are no statistical differences in the amount of donations received in the elections of 2008 according to treatment status. Moreover, in Figure 2.3, I show that around approximately 1/4 of municipalities had participated in the audits program before the experiment happened in 2009.

2.4 Results

2.4.1 Effects of the threat of audits on block grants

Table 2.3 presents fixed effect (FE) regression results from estimating Equations 2.1 and 2.2. The dependent variables are indicators for whether the municipality received block grants in a given year. I categorized block grants according to their use: for construction or non-construction projects.[16] Columns 1, 2, and 3 report contemporaneous and post-

[14] The variable $Letter_m$ characterizes the treatment status in the experiment and the variable A_m reflects random previous participation in the audits program.

[15] In Brazil, only first-term mayors can run for reelection. The exception is for mayors that served two terms but took a gap in the office for at least one term.

[16] For more details on the process of text mining and the creation of categories for block grants, check the appendix.

experiment average treatment effects.[17] The results suggest no effects of the experiment on the probability of receiving block grants. I insert the heterogeneity of previous contact with the audits program in columns 4, 5, and 6. At a first glance, mayors in municipalities that had been previously audited do not respond differently to the treatment compared to politicians in places without past involvement with the audits program. In columns 7, 8, and 9, I include the share of construction firms' donations in 2008 as a second dimension in the estimation of heterogeneous treatment effects, according to Equation 2.2. The results show that the difference in the sensitivity of the contemporaneous treatment effect with respect to construction donations between municipalities previously and not previously audited is -6.5p.p., for block grants directed to construction projects and -6.1p.p. to non-construction works.[18] There is a reversal of the estimates for the coefficient $\beta_{4,post}$, for construction federal transfers after the experiment ended, with an estimate of 1.6p.p. whereas the effects for non-construction projects stay negative, at -4p.p. These results suggest that there are significant changes in the profile of federal transfers with the audits' threat, mainly for municipalities that had previous experience with the audits program and where mayors relied on construction companies for their election in 2008.[19]

In Figure 2.5, I relax the assumption that treatment effects are the same for the remainder of the mayors' term, after the experiment was over (years 2010, 2011, and 2012), and estimate coefficients for each of the four years in the political term. Moreover, I compute the total treatment effects for four different groups of municipalities, with two dimensions that correspond to the two heterogeneities in Equation (2.2): the rows split the sample according to previous experience in the audits program and the columns separate mayors into a group that did not receive any donations from construction companies in 2008 and another group that received the average of donations from construction companies in these elections (1.82p.p.). The graphs show that the group mostly affected by the treatment consists of the places with past experience with the audits program and where politicians had a relationship with construction companies through campaign donations: they reduce the probability of starting construction projects by more than 20p.p. while the experiment was happening in 2009, and partially reverse it in the electoral year of 2012, with an estimate of +8.8p.p. Figure 2.6 complements the information of Figure 2.5 by reporting the difference in treatment effects in places where the mayors received the average donations from construction companies and municipalities where mayors did not rely on these firms in the 2008 elections, for both previously audited and not previously audited municipalities. For previously audited municipalities, differential treatment effects are negative in 2009 in places where mayors were committed to construction companies, with a reversal happenning as soon as the experiment is over in 2010, until the end of the term in 2012. Figures 2.7 and 2.8 replicate previous

[17] The contemporaneous treatment effect refers to effects of the experiment on the outcomes in the (shifted) year 2009. The post-experiment treatment effects incorporate the letter's effect on the values of the outcomes for the remainder of the mayor's term, after the experiment was over (for years 2010, 2011, and 2012).

[18] These are the coefficients $\beta_{4,2009}$ in Equation (2.2).

[19] I will refer to municipalities with previous experience in the audits program and where mayors relied on construction companies' donations as the "sensitive group", for simplicity.

ones for the probability of receiving block grants directed to non-construction projects. They report permanent negative treatment effects for the municipalities previously audited and where the mayor had ties with construction firms.

Zamboni and Litschig (2018) show a reduction in corruption findings as a result of the experiment at its end in May of 2010. The results of Table 2.3 and Figures 2.5, 2.6, 2.7, and 2.8 suggest that mayors additionally respond to the experiment by reducing the total amount of projects in the municipalities in 2009, but attempt to compensate construction companies after the experiment by partially increasing the amount of construction projects in their precincts, at the expense of keeping non-construction projects at a lower level compared to their baselines. The complementarity between block grants directed to construction and non-construction projects described in Figure 2.4 is consistent with treatment effects for 2009 but the letter breaks this relationship after the experiment ended.

Table 2.4 modifies the dependent variable to the inverse hyperbolic sine of the value of block grants, in order to measure both intensive and extensive margin effects of the experiment.[20] The estimates of $\beta_{4,2009}$ and $\beta_{4,post}$ retain the same sign compared to the linear probability models of Table 2.3, but the magnitudes are much higher. Therefore, the audits threat lead to a relatively modest decrease in the number of public projects in the municipalities, and a much larger reduction in the nominal values in block grants while the experiment happened in 2009. This result is consistent with the findings of lower corruption by Zamboni and Litschig (2018): mayors respond to the letter mainly through using block grants more wisely and reducing the average values of projects, rather than generating a major decrease in the number of construction and non-construction projects and potentially disrupting the provision of public goods for their constituents. [21]

2.4.2 Effects of the threat of audits on campaign donations and reelection outcomes in 2012

Table 2.5 reports OLS estimations from Equation (2.3) on the probability of receiving donations from different sources in the 2012 reelection campaigns, restricting the sample only to mayors that decided to run for reelection.[22] Figure 2.9 compiles the results from Table 2.5 and show treatment effects for the four relevant groups. Once again, the experiment impacts mainly the group of previously audited municipalities where mayors had ties with construction companies. There is a reduction of 23p.p. in the probability that a mayor in that group receive any donations from construction companies, consisting in evidence that these firms punish mayors in subsequent elections for the reduction in construction projects

[20] Bellemare and Wichman (2019) shows how to interpret regressions with the inverse hyperbolic sine (IHS). Differently than the logarithm specification, the IHS(y) allows the use of zeros in the regression, which are frequent in the block grants data.

[21] In the section A.2.1, I include figures with results for the inverse hyperbolic sine of block grants, as a measure of both intensive and extensive margin effects.

[22] Almost 46% of mayors run for reelection in 2012.

after the experiment. Moreover, there is a reduction of virtually 100p.p. in the probability that mayors contribute to their campaigns with their own funds, which reveals that these politicians might have updated their beliefs regarding their reelection outcomes, after the experiment. There is no effect on the probability of receiving any donations in 2012, from a baseline of 97.1%.

Table 2.6 shows treatment effects on the values of donations received in 2012. Figure 2.10 summarizes treatment effects by groups. The magnitudes of treatment effects for the group in the upper-left corner are above 100%, in absolute values.[23] I interpret these large coefficients as evidence that mayors in this group do not report any donations from construction companies, political party, or own funds for their reelection campaign.[24] Although there is partial substitution towards receiving more nominal donations from other firms, besides developers, the overall value of donations suffers a sizeable reduction as well. Overall, these findings suggest that the temporary threat of audits results in persistent effects in the political scene: there is evidence of a breakage in the relationship between politicians and construction companies, leading to an overall reduction in donations for the reelection campaign of mayors. These results support an additional interpretation to the findings in Section 2.4.1. The reduction in construction projects in 2009 (while the threat of audits was active) for the sensitive group might be explained by hesitant mayors that choose to be cautious and not carry out these projects in that year. The fact that construction companies choose not to finance the reelection campaign of mayors in that group serves as evidence against collusion between politicians and campaign donors while the experiment was in place in 2009.

Table 2.7 presents treatment effects on indicators for reelection outcomes in 2012, using Equation (2.3). Figure 2.11 reports treatment effects by groups. The upper left graph in Figure 2.11 shows that reelection rates decrease by 50p.p. for the sensitive group, conditioning the sample to first-term mayors that could run for reelection. Furthermore, treatment effects on reelection rates for mayors that chose to run for reelection are stronger, as a result of positive treatment effect on the likelihood that mayors run for reelection. A potential explanation for positive treatment effects on the decision of running for reelection is that these politicians make this choice before they observe the amount of donations for their reelection campaigns and underestimate the impact of the experiment on their expected campaign donations.[25] Taken together, the results indicate that the temporary threat of audits induce long term effects in the political system: a reduction in mayors' campaign donations and reelection rates in the sensitive group, two years after the end of the experiment.

[23] In the IHS(y) specification, a coefficient of x can be interpreted as $(100 \times x)\%$

[24] By observing the mean of the dep. variables in Table 2.5, commonly mayors do not receive any resources from a specific source for their reelection campaigns, which justifies the interpretation of coefficients above 100% in absolute value as a shift towards zero donations from a given source in 2012.

[25] There is a strong seasonality in campaign donations in the electoral year and the majority of donations happen after mayors decide whether they will run for reelection.

2.4.3 Robustness checks

Figures 2.12, 2.13, A.5, and A.6 extend Equation (2.2) to include treatment effects before 2009 as a falsification test. There is no evidence of treatment effects before the experiment started, for both block grants directed to construction and non-construction projects. [26]

2.5 Conclusion

This chapter shows that a temporary threat of audits against corruption impacts the provision of public goods, and tears the relationship between corrupt politicians and campaign donors. In the context of an experiment that temporarily raised the audit risk for a random set of municipalities, I find that the threat of scrutiny causes a reduction of the 21p.p. in the likelihood that construction projects are carried out in the year of the experiment. Furthermore, the threat letters mainly effect municipalities with previous experience in the audits' program and where mayors had financial ties with historically corrupt campaign donors (construction firms). Finally, identify a decline of 23p.p. in the probability that these firms refinance the reelection campaign of politicians, as well as a reduction of 50p.p. in reelection rates of incumbent mayors in the treatment group.

These findings offer several policy implications. First, a temporary threat of punishment to illicit behavior can result in a long-term disruption of the political equilibrium, changing the relationship between officeholders and the private sector, and the composition of politicians elected to office. While the literature has shown that economic agents respond to higher probability or intensity of punishment for misbehavior (Becker, 1968; Di Tella and Schargrodsky, 2003; Avis et al., 2018; Zamboni and Litschig, 2018; Kleven et al., 2011; Olken, 2007), there is sparse evidence on the channels thorugh which they can disrupt a pre-existing equilibrium. Furthermore, political corruption often produce inefficiencies in the allocation of resources in the provision of public goods. While my results suggest that corrupt politicians change amount of government's resources directed to capital expenditure, such as construction and renovation projects, more research is needed to better understand whether corruption can partially function as a grease in the wheels in the government operations or whether it causes over-provision of these types of public goods.

[26] In A.2, I include additional results where I replace the heterogeneity of previous experience with the audits program (before 2009) with an indicator variable on whether the municipality experiences audits after the experiment's end.

Figures

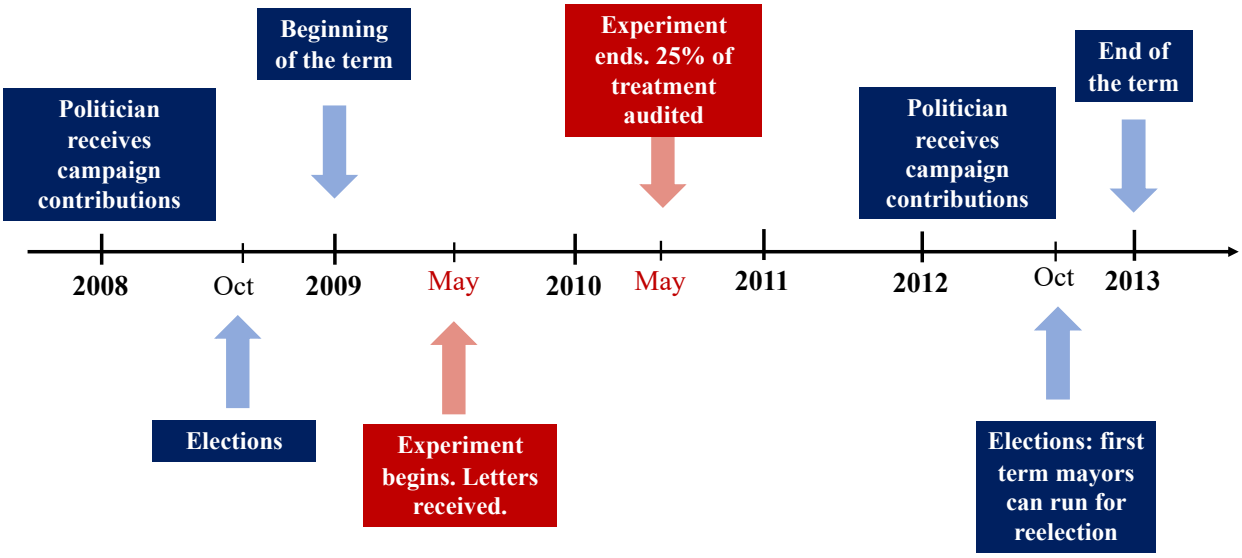
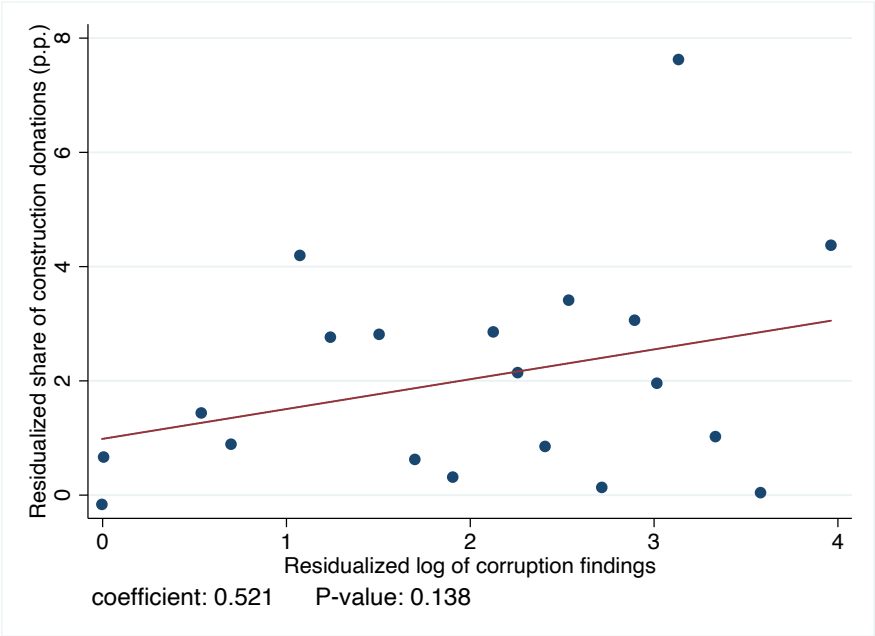
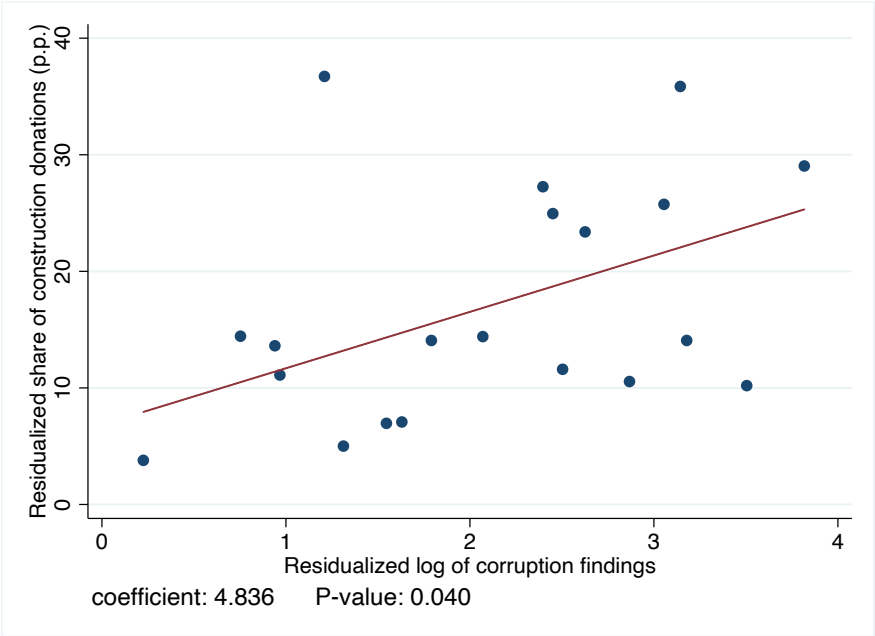


Figure 2.1: Timeline of the experiment and political cycle in Brazil

Notes: Figure 2.1 shows the relevant timeline for the empirical analysis in this chapter. The start of the mayors' term is on January 1st, 2009 and the end of the term is on Dec 31st, 2012.



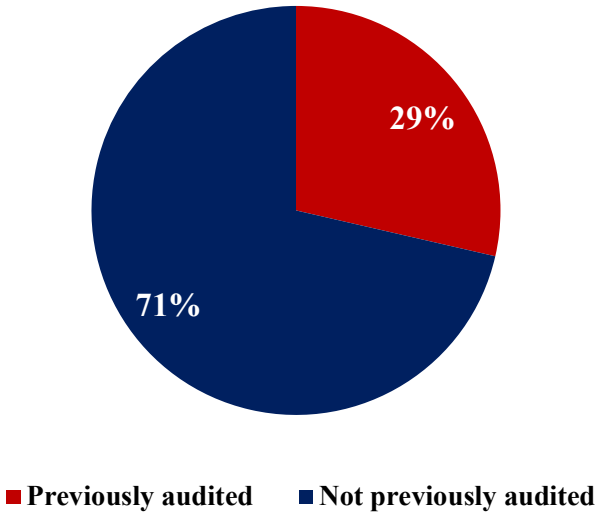
(a) Full sample



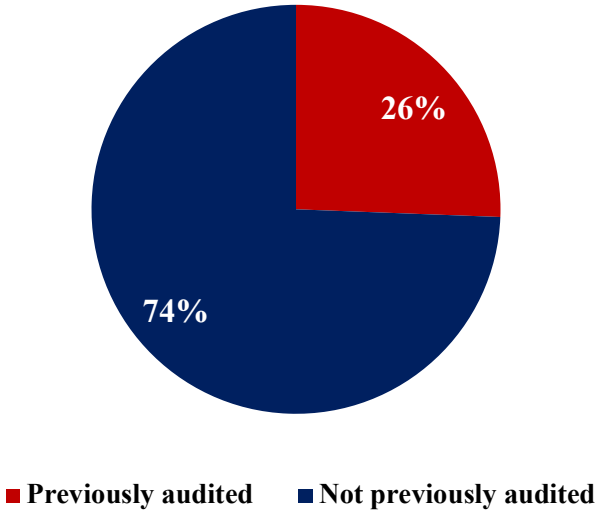
(b) Conditional on receiving donations from construction companies

Figure 2.2: Relationship between corruption and campaign donations from construction companies

Notes: Panel 2.2a plots the share of construction donations in 2008 election and the logarithm of corruption findings between 2004 and 2008. Both variables are expressed as residual changes, once the municipality's population is taken into account. Panel 2.2b plots the same variables, restricting the sample to municipalities that received any donations from construction companies for 2008 elections.



(a) Treatment group



(b) Control group

Figure 2.3: Share of municipalities that had participated in the audits program before the experiment started in 2009

Notes: Panel 2.3a plots the share of municipalities in the treatment group that had participated in the CGU's audits program before May of 2009, when the letter experiment took place. Panel 2.3b plots the same variable for the control group.

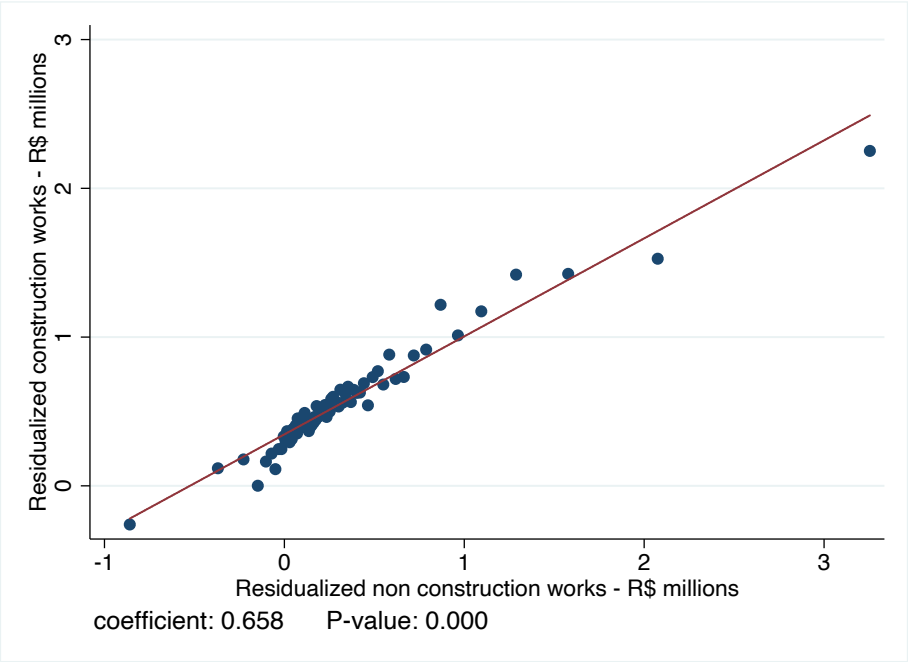


Figure 2.4: Relationship between block grants for construction projects and non-construction projects

Notes: Figure 2.4 shows the association between block grants directed to construction projects and other projects for municipality-year levels. Both variables are expressed as residual changes, once the municipality's population is taken into account. R$ means nominal values in the Brazilian currency ("Reais").

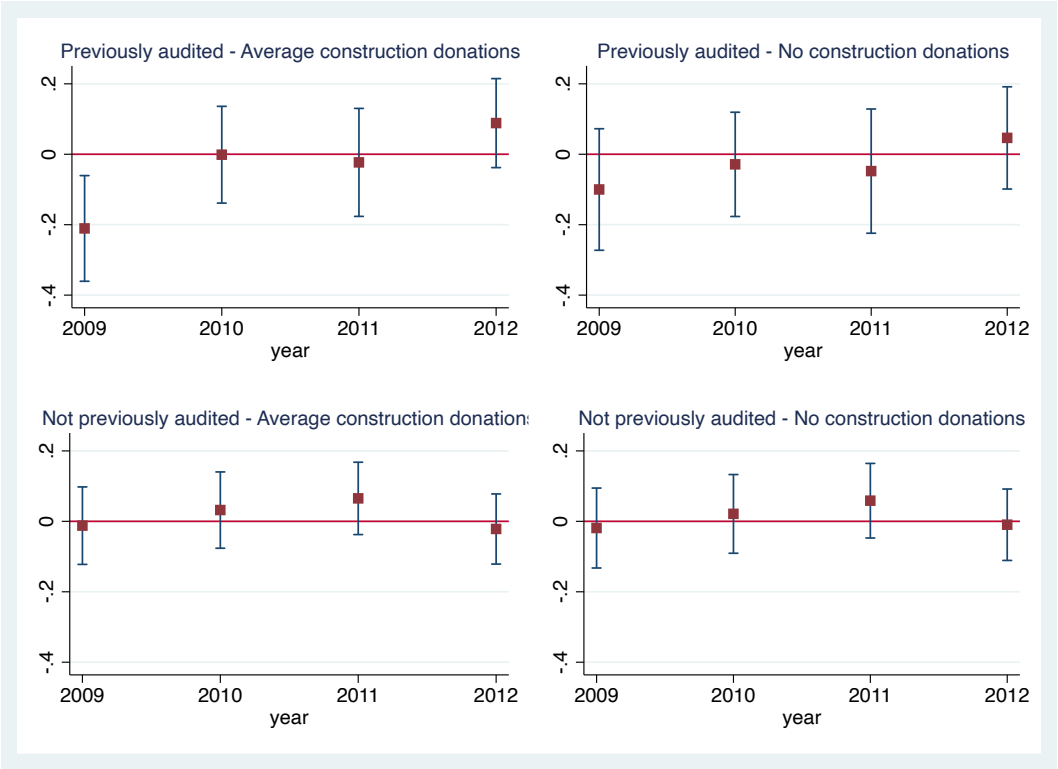


Figure 2.5: Heterogeneous treatment effects on the probability of receiving block grants for construction projects - by groups

Notes: Figure 2.5 reports heterogeneous treatment effects on the municipality's probability of receiving block grants for construction projects in each year of the mayors' term. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

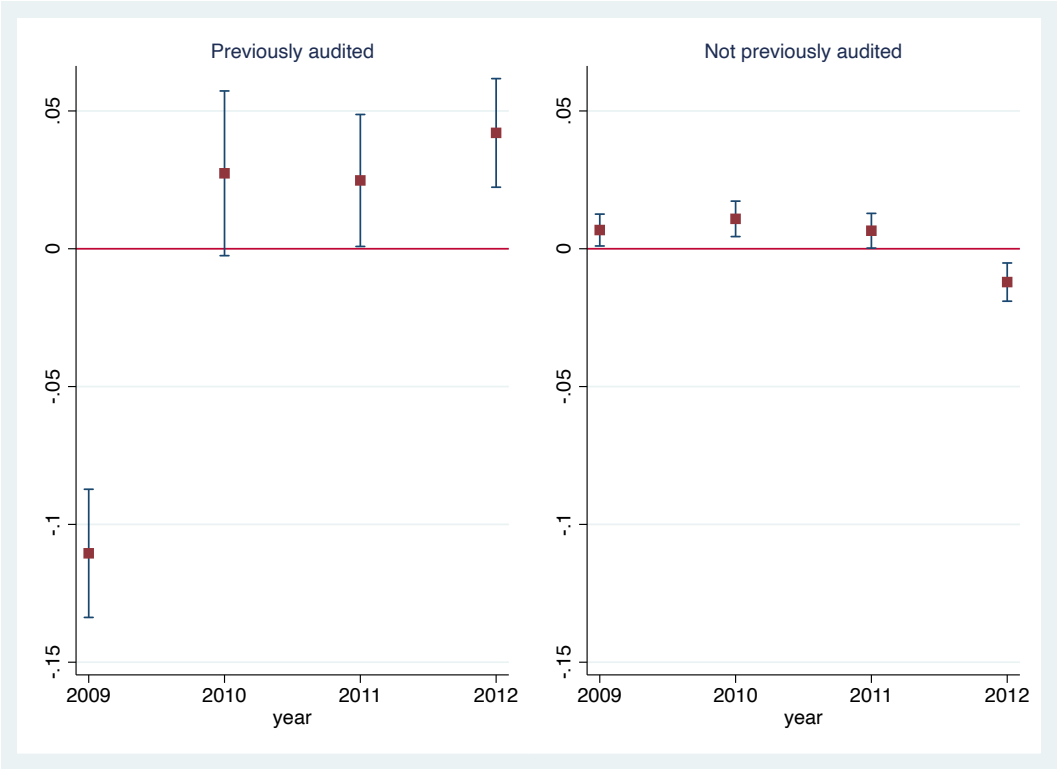


Figure 2.6: Difference treatment effects on the probability of receiving block grants directed to construction projects between municipalities financed or not financed by construction companies in 2008

Notes: Figure 2.6 reports heterogeneous treatment effects on the probability of receiving block grants directed to construction projects in each year of the mayors' term . The first (second) graph focus on municipalities that had (not) been previously audited before the experiment. Each coefficient reflects the difference in treatment effects between places where the mayors received the average value of construction donations (1.82%) and municipalities where they did not receive any dontations from construction companies. Point estimates and 95% confidence intervals are reported for each year in the graphs.

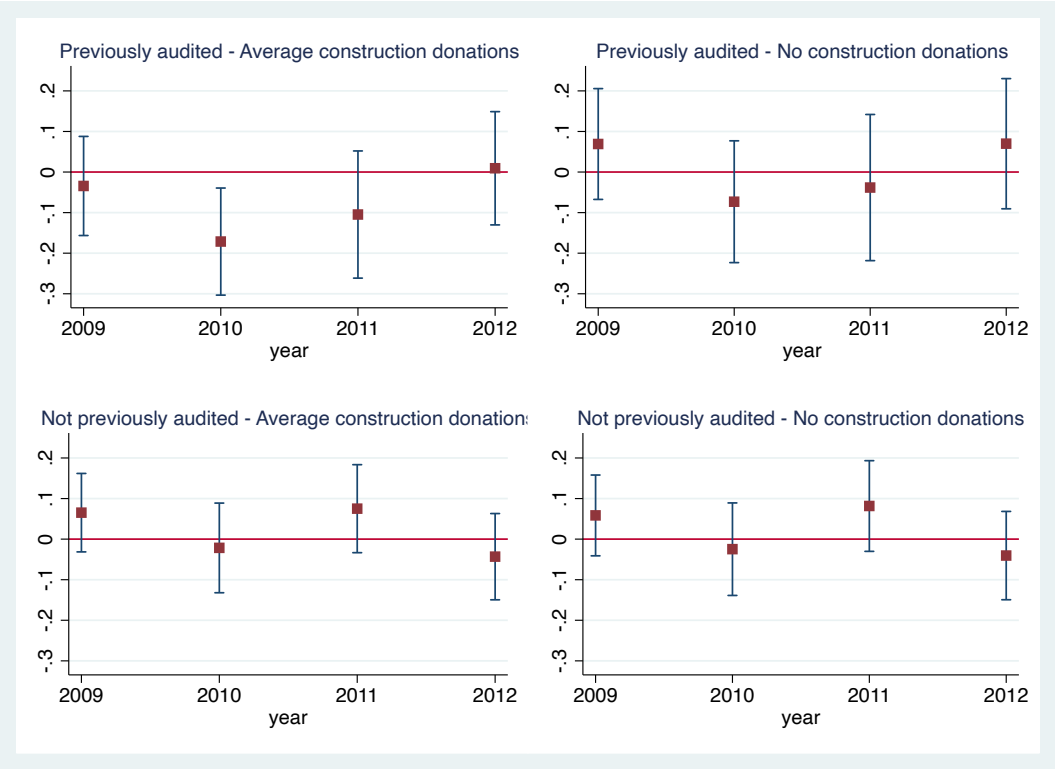


Figure 2.7: Heterogeneous treatment effects on the probability of receiving block grants for non-construction projects - by groups

Notes: Figure 2.7 reports heterogeneous treatment effects on the municipality's probability of receiving block grants for non-construction projects in each year of the mayors' term. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

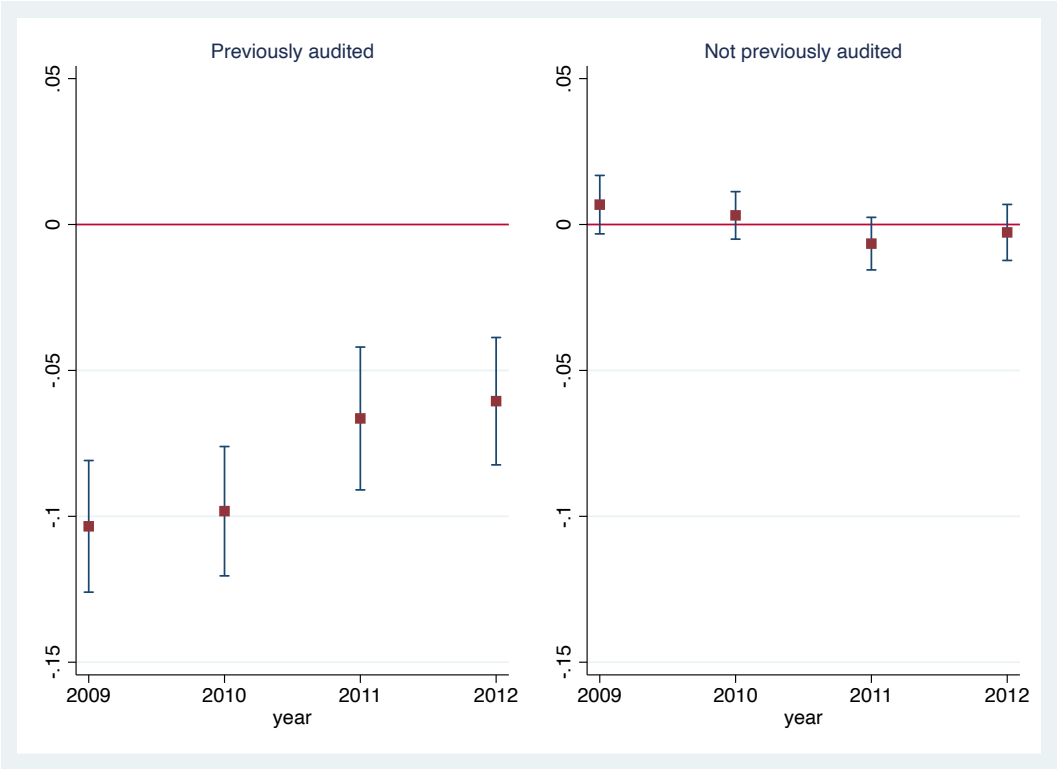


Figure 2.8: Difference treatment effects on the probability of receiving block grants directed to non-construction projects between municipalities financed or not financed by construction companies in 2008

Notes: Figure 2.8 reports heterogeneous treatment effects on the probability of receiving block grants directed to non-construction projects in each year of the mayors' term . The first (second) graph focus on municipalities that had (not) been previously audited before the experiment. Each coefficient reflects the difference in treatment effects between places where the mayors received the average value of construction donations (1.82%) and municipalities where they did not receive any dontations from construction companies. Point estimates and 95% confidence intervals are reported for each year in the graphs.

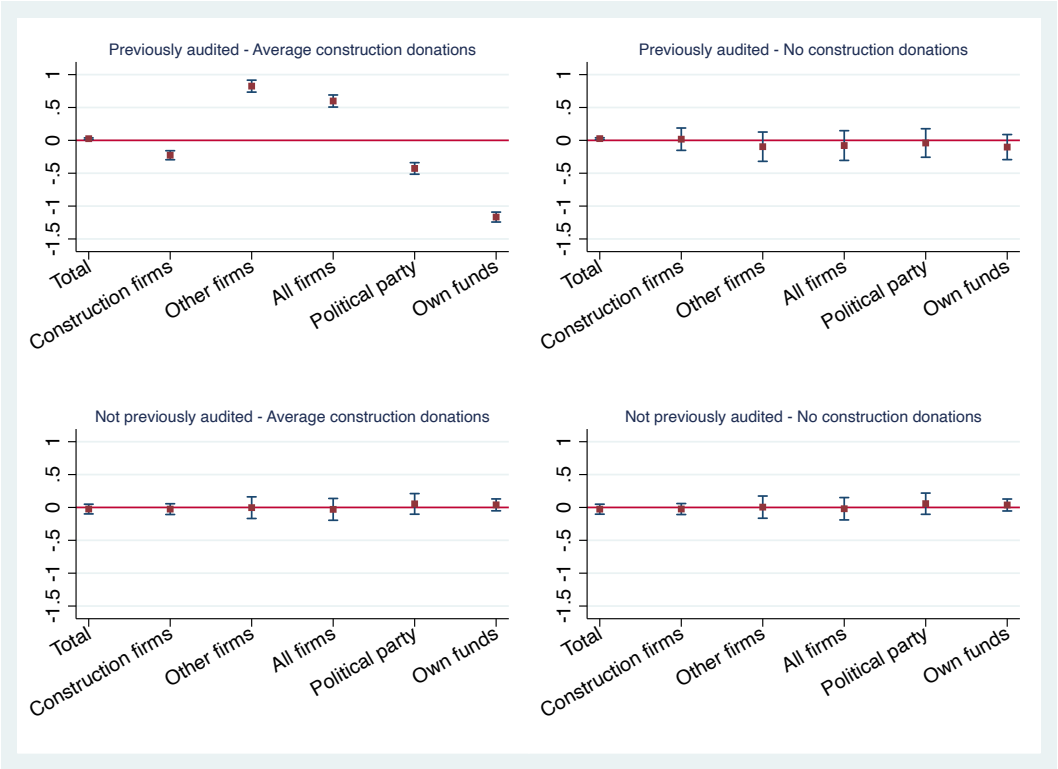


Figure 2.9: Heterogeneous treatment effects on the probability of receiving campaign donations in 2012 - by source of donations

Notes: Figure 2.9 reports heterogeneous treatment effects on the mayor's probability of receiving campaign donations from different sources in 2012. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

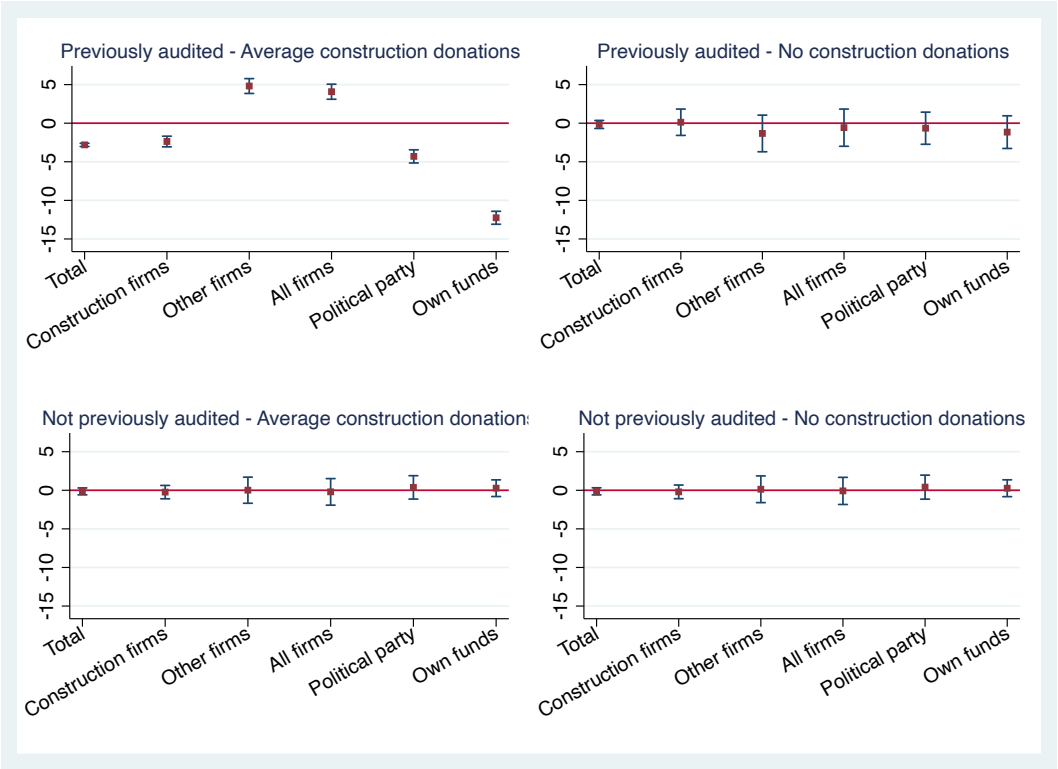


Figure 2.10: Heterogeneous treatment effects on the values of campaign donations in 2012 - by source of donations

Notes: Figure 2.10 reports heterogeneous treatment effects on the mayor's values of campaign donations from different sources in 2012. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

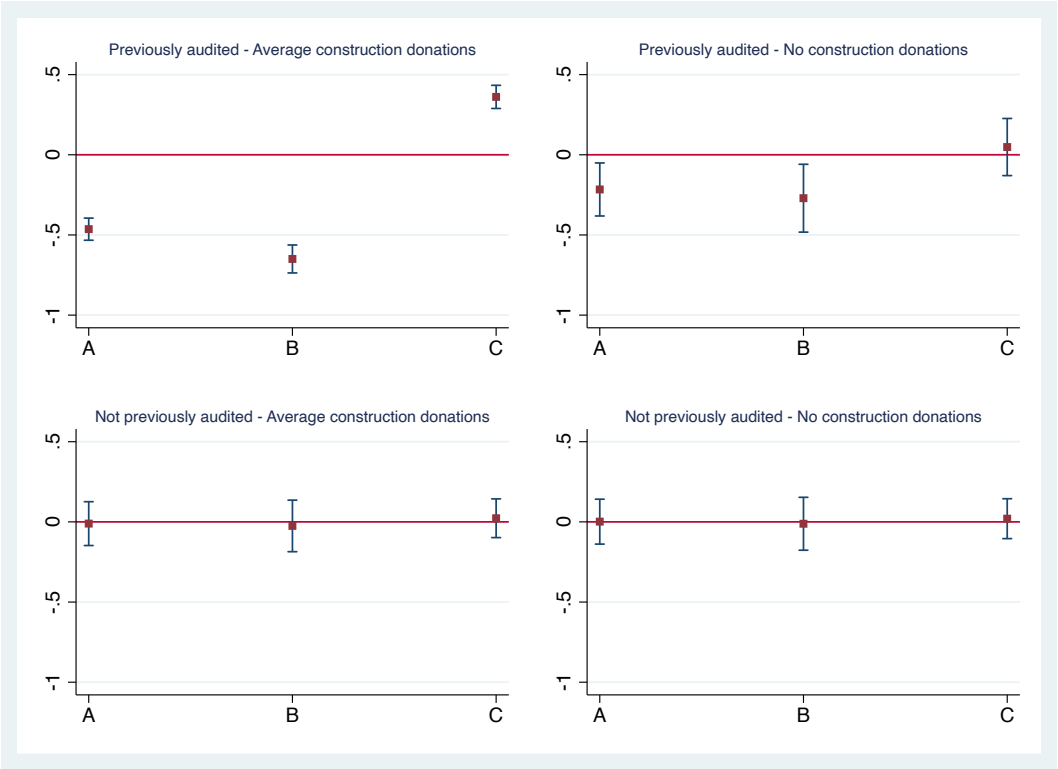


Figure 2.11: Heterogeneous treatment effects on the reelection outcomes in 2012

Notes: Figure 2.11 reports heterogeneous treatment effects on the municipality's reelection outcomes in 2012. The values A in the horizontal axis refer to treatment effects on reelection rates conditional on mayors that are in first term (following column A of Table 2.7. The values B refer to treatment effects on reelection rates, conditional on mayors that run for reelection. The values C refer to treatment effects on the decision of running for reelection, for mayors in first term. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

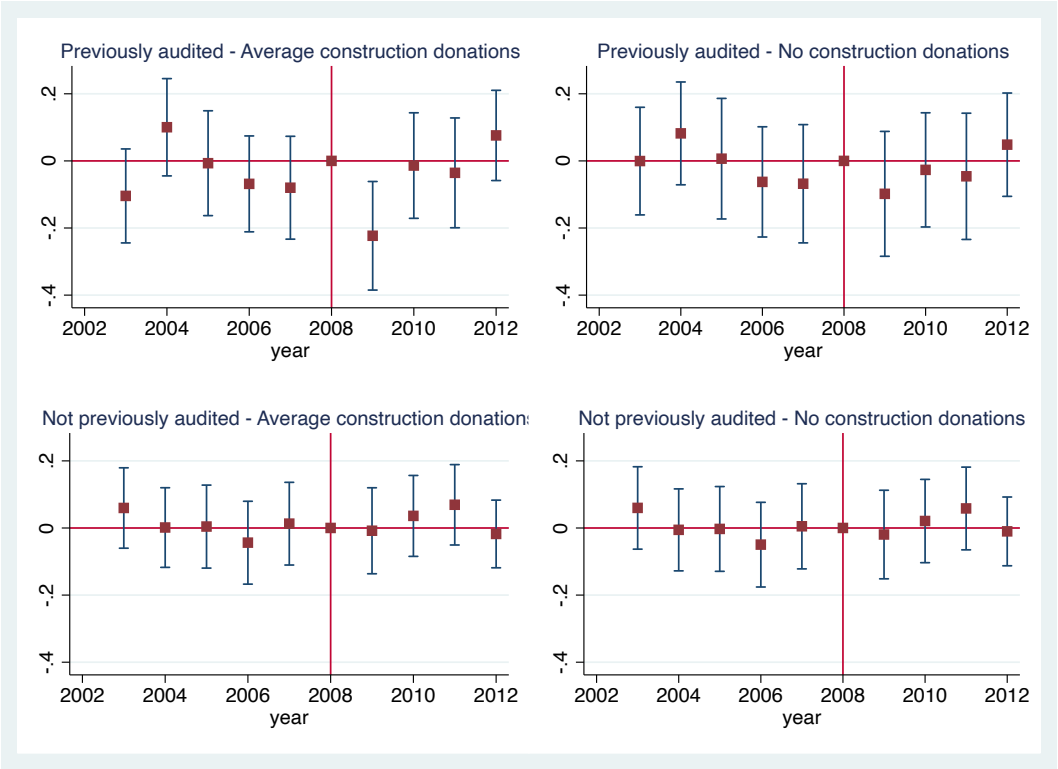


Figure 2.12: Falsification tests: heterogeneous treatment effects on the probability of receiving block grants for construction projects

Notes: Figure 2.12 reports heterogeneous treatment effects on the municipality's probability of receiving block grants for construction projects in each year of the mayors' term. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

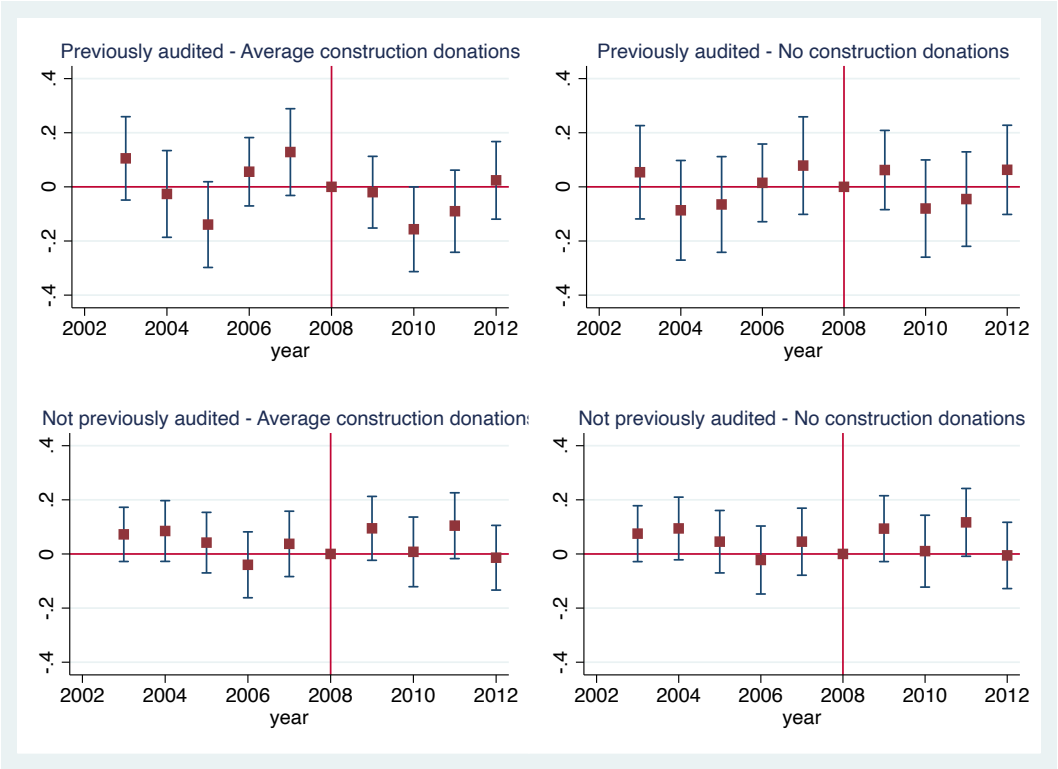


Figure 2.13: Falsification tests: heterogeneous treatment effects on the probability of receiving block grants for non-construction projects

Notes: Figure 2.13 reports heterogeneous treatment effects on the municipality's probability of receiving block grants for non-construction projects in each year of the mayors' term. The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Tables

	Control	Treatment	Difference	P-value
Panel A - Demographic variables:				
Life expectancy (years)	73.08	72.79	0.28	[0.27]
Child Mortality (under 5 years old)	19.27	20.09	-0.83	[0.24]
Years of schooling at 18 years old	9.46	9.41	0.06	[0.58]
Illiteracy Rate (%)	20.61	21.89	-1.28	[0.29]
College graduate (%)	5.41	5.08	0.33	[0.23]
Gini coefficient	0.49	0.50	-0.01	[0.12]
Per capita income (R$)	489.41	464.68	24.73	[0.26]
Households with eletricity (%)	97.18	96.62	0.56	[0.39]
Human Development Index	0.66	0.65	0.01	[0.18]
Population	23,951	26,276	-2,325	[0.59]
Radio (%)	0.55	0.59	-0.04	[0.40]
Panel B - Political variables:				
Municipality previously received audits program (%)	25.60	28.60	-2.89	[0.49]
Share of construction $ in 2008 (in p.p)	1.81	2.01	-0.20	[0.83]
Share of construction $ in 2008 (in p.p) - only positive values	16.67	23.36	-6.69	[0.47]
Total revenue of construction donations in 2008 polls (R$)	4,143	5,706	-1,563	[0.68]
Total revenue of construction donations in 2008 polls (R$) - only positive values	26,267	48,914	-22,646	[0.48]
Receiving any donations from construction companies (%)	10.2	8.3	1.9	[0.45]
Total revenue of firms' donations in 2008 polls (R$)	20,165	21,644	-1,479	[0.67]
Total revenue of campaign donations in 2008 elections (R$)	72,874	83,849	-10,976	[0.57]
First term mayor (%)	0.61	0.65	-0.04	[0.41]
Number of campaign donations	26.67	32.65	-5.98	[0.44]
Number of candidates to mayor in 2008	2.61	2.61	0.01	[0.95]
Win margin of the elected mayor in 2008 (%)	0.20	0.22	-0.02	[0.37]
Mayor's Gender (male=1)	0.91	0.94	-0.03	[0.14]
Mayor's education (years of schooling)	12.81	12.78	0.03	[0.92]
Mayor with former white collar occupation (%)	0.37	0.38	-0.01	[0.78]
Sample Size	5401	120		

Table 2.1: Balance check for demographic and political variables

Notes: Table 2.1 presents demographic and political variables for the control and treatment groups. The P-values for the test for difference in means are reported in the last column. The demographic variables come from the Census of 2010 and the political variables refer to 2008 elections.

	Mean	SD	Min	Max
Panel A - Indicator for receiving block grants:				
Total	0.74	0.44	0.00	1.00
Construction projects	0.55	0.50	0.00	1.00
Non- construction projects	0.59	0.49	0.00	1.00
Panel B - Values of block grants (millions of R$):				
Total	1.42	6.36	0.00	636.88
Construction projects	0.92	5.61	0.00	628.72
Non Construction Projects	0.49	2.29	0.00	137.66
Panel C - Share of block grants in p.p.:				
Construction projects	52.64	38.81	0.00	100.00
Observations		22,036		
Number of municipalities		5,509		

Table 2.2: Descriptive statistics for block grants (2009-2012)

Notes: Table 2.2 shows descriptive statistics for block grants between 2009 and 2012. Each observation is a municipality-year entry. Panel A reports nominal values of block grants in millions of R$. Panel B reports indicator for receiving block grants in a specific year for a municipality. Panel C shows the share in p.p. of the values of block grants for construction projects.

Dependent Variable:	Total (1)	Construction (2)	Non construction (3)	Total (4)	Construction (5)	Non construction (6)	Total (7)	Construction (8)	Non construction (9)
letter×λ_{2009}	0.030	-0.045	0.061	0.044	-0.010	0.068	0.044	-0.019	0.058
	(0.028)	(0.047)	(0.041)	(0.033)	(0.055)	(0.049)	(0.034)	(0.058)	(0.051)
letter×λ_{post}	-0.016	0.017	-0.009	-0.013	0.025	0.002	-0.012	0.023	0.005
	(0.027)	(0.030)	(0.031)	(0.033)	(0.037)	(0.039)	(0.034)	(0.039)	(0.041)
letter×λ_{2009}×previously audited				-0.048	-0.116	-0.025	-0.018	-0.081	0.011
				(0.062)	(0.105)	(0.087)	(0.056)	(0.105)	(0.086)
letter×λ_{post}×previously audited				-0.009	-0.027	-0.035	-0.016	-0.033	-0.019
				(0.056)	(0.063)	(0.063)	(0.058)	(0.065)	(0.063)
letter×λ_{2009}×construction donations in p.p.							0.000	0.004**	0.004
							(0.001)	(0.002)	(0.003)
letter×λ_{post}×construction donations in p.p.							-0.000	0.001	-0.001
							(0.001)	(0.001)	(0.001)
letter×λ_{2009}×previously audited× construction donations in p.p.							-0.070***	-0.065***	-0.061***
							(0.005)	(0.007)	(0.007)
letter×λ_{post}×previously audited× construction donations in p.p.							0.011***	0.016***	-0.040***
							(0.004)	(0.005)	(0.004)
Mean of dep. variables	.805	.585	.670	.805	.585	.670	.805	.585	.670
Observations	67,657	67,657	67,657	67,657	67,657	67,657	67,657	67,657	67,657
R-squared	0.093	0.080	0.091	0.093	0.080	0.091	0.093	0.080	0.092
Number of municipalities	5,208	5,208	5,208	5,208	5,208	5,208	5,208	5,208	5,208

Table 2.3: The effects of the audits threat on the probability of receiving block grants

Notes: Table 2.3 reports the effects of the audits threat on the probability of receiving block grants from the federal government, in a panel of Brazilian municipalities between 2000 and 2012 (Equations (2.1) and (2.2)). In columns (1), (4), and (7), the dependent variable is an indicator on whether the municipality received any block grants in a given year. Columns (2), (5), and (8) present the experiment's impacts on block grants used for construction works. Columns (3), (6), and (9) present the effects of the letters on block grants direct to non-construction projects. The variable letter is an indicator for the municipalities selected to the group with higher audits' probability. The variables λ_{2009} and λ_{post} are indicators for the years 2009 and 2010,2011,2012. Coefficients with λ_{2009} reflect contemporanous effects of the experiment and λ_{post} show treatment effects after the end of the experiment. All regressions include municipality and year fixed effects. Clustered standard errors(at the municipality level) in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Total (1)	Construction (2)	Non construction (3)	Total (4)	Construction (5)	Non construction (6)	Total (7)	Construction (8)	Non construction (9)
letter×λ_{2009}	0.567	-0.818	1.024*	0.694	-0.490	1.029	0.671	-0.612	0.896
	(0.404)	(0.675)	(0.537)	(0.466)	(0.793)	(0.650)	(0.487)	(0.819)	(0.674)
letter×λ_{post}	-0.142	0.213	-0.054	-0.128	0.317	0.075	-0.112	0.283	0.146
	(0.381)	(0.422)	(0.418)	(0.470)	(0.520)	(0.522)	(0.490)	(0.542)	(0.543)
letter×λ_{2009}× previously audited				-0.446	-1.108	-0.015	0.036	-0.601	0.479
				(0.925)	(1.498)	(1.149)	(0.843)	(1.504)	(1.136)
letter×λ_{post}× previously audited				-0.046	-0.338	-0.435	-0.126	-0.429	-0.255
				(0.790)	(0.876)	(0.845)	(0.818)	(0.901)	(0.843)
letter×λ_{2009}× construction donations in p.p.							0.013	0.055**	0.050
							(0.020)	(0.027)	(0.033)
letter×λ_{post}× construction donations in p.p.							-0.003	0.016	-0.022
							(0.023)	(0.021)	(0.019)
letter×λ_{2009}× previously audited× construction donations in p.p.							-1.030***	-0.898***	-0.830***
							(0.068)	(0.098)	(0.089)
letter×λ_{post}× previously audited × construction donations in p.p.							0.142***	0.240***	-0.513***
							(0.054)	(0.064)	(0.055)
Mean of dep. variables (R$)	978,531	601,128	377,402	978,531	601,128	377,402	978,531	601,128	377,402
Observations	67,657	67,657	67,657	67,657	67,657	67,657	67,657	67,657	67,657
R-squared	0.101	0.089	0.089	0.101	0.089	0.089	0.101	0.089	0.089
Number of municipalities	5,208	5,208	5,208	5,208	5,208	5,208	5,208	5,208	5,208

Table 2.4: The effects of audits threat on the values of block grants transferred to municipalities

Notes: Table 2.4 reports the effects of the audits threat on the nominal values of block grants from the federal government, in a panel of Brazilian municipalities between 2000 and 2012 (Equations (2.1) and (2.2)). In columns (1), (4), and (7), the dependent variable is the inverse hyperbolic sine of total block grants received by a municipalitiy in a specific year. Columns (2), (5), and (8) present the experiment's impacts on the IHS of block grants used for construction works. Columns (3), (6), and (9) present the effects of the letters on the IHS of block grants direct to non-construction projects. The variable letter is an indicator for the municipalities selected to the group with higher probability of audits. The variables λ_{2009} and λ_{post} are indicators for the years 2009 and 2010,2011,2012. Coefficients with λ_{2009} reflect contemporanous effects of the experiment and λ_{post} show treatment effects after the end of the experiment. All regressions include municipality and year fixed effects. Clustered standard errors(at the municipality level) in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Total (1)	Construction firms (2)	Other firms (3)	All firms (4)	Political party (5)	Own funds (6)
letter × previously audited	-0.002*	-0.134**	0.513***	0.379***	-0.212**	-0.589***
construction donations in p.p.	(0.001)	(0.065)	(0.085)	(0.086)	(0.083)	(0.073)
letter × previously audited	0.053	0.042	-0.101	-0.059	-0.098	-0.139
	(0.039)	(0.097)	(0.143)	(0.144)	(0.138)	(0.108)
letter × construction donations in p.p.	0.001	-0.001	-0.004**	-0.005	-0.002	0.001
	(0.001)	(0.005)	(0.002)	(0.005)	(0.005)	(0.002)
letter	-0.027	-0.024	0.004	-0.020	0.057	0.037
	(0.039)	(0.043)	(0.086)	(0.087)	(0.082)	(0.046)
Mean of dep. variables	.971	.126	.428	.555	.319	.878
Observations	2,388	2,320	2,320	2,320	2,320	2,320
R-squared	0.001	0.133	0.004	0.041	0.022	0.022

Table 2.5: The effects of audits threat on the probability of receiving donations for mayors' reelection campaigns in 2012

Notes: Table 2.5 reports OLS estimates of the effect of the letters on the probability that mayors received donations from various sources for their reelection campaigns in 2012 (Equation (2.3)). All dependent variables are indicators on whether a mayor received any financing from a specific source. Column (1) indicator refers to receiving any donations in 2012. Columns (2), (3), and (4) show effects on construction companies, non-construction companies, and all firms, respectively.Column (5) and (6) reflect impacts of the experiment in the donations from parties to mayors and mayors' own funds, respectively. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Total (1)	Construction firms (2)	Other firms (3)	All firms (4)	Political party (5)	Own funds (6)
letter × previously audited construction donations in p.p.	-1.458***	-1.363**	3.461***	2.642***	-2.002**	-6.133***
	(0.199)	(0.653)	(0.905)	(0.920)	(0.793)	(0.811)
letter × previously audited	-0.025	0.330	-1.459	-0.497	-1.055	-1.425
	(0.354)	(0.981)	(1.499)	(1.522)	(1.326)	(1.215)
letter × construction donations in p.p.	-0.004	-0.018	-0.067**	-0.069	-0.016	0.004
	(0.007)	(0.047)	(0.033)	(0.044)	(0.051)	(0.028)
letter	-0.138	-0.204	0.132	-0.080	0.406	0.265
	(0.234)	(0.451)	(0.883)	(0.897)	(0.794)	(0.556)
Mean of dep. variables (in R$)	150,241	7,473	29,362	36,836	18,602	38,028
Observations	2,320	2,320	2,320	2,320	2,320	2,320
R-squared	0.264	0.167	0.091	0.098	0.049	0.011

Table 2.6: The effects of audits threat on the donations' values for mayors' reelection campaigns in 2012

Notes: Table 2.6 reports OLS estimates of the effect of the letters on the values of donations that mayors received from various sources for their reelection campaigns in 2012 (Equation (2.3)). All dependent variables are the inverse hyperbolic sine of a source of campaign donations. Column (1) refers to all donations received in 2012. Columns (2), (3), and (4) show effects on construction companies, non-construction companies, and all firms, respectively.Columns (5) and (6) reflect impacts of the experiment in the donations from parties to mayors and mayors' own funds, respectively. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Reelection rates		Run for reelection rates
Dependent Variable	Conditional on 1st term (A)	Conditional on running (B)	Conditional on 1st term (C)
letter × previously audited	-0.130**	-0.202**	0.171**
construction donations in p.p.	(0.063)	(0.080)	(0.068)
letter × previously audited	-0.218**	-0.259*	0.028
	(0.111)	(0.137)	(0.111)
letter × construction donations in p.p.	-0.007***	-0.007***	0.001
	(0.002)	(0.003)	(0.002)
letter	0.001	-0.012	0.020
	(0.071)	(0.084)	(0.063)
Mean of dep. variables	.393	.537	.731
Observations	3,217	2,358	3,217
R-squared	0.003	0.004	0.003

Table 2.7: The effect of audits threat on reelection rates in 2012 polls

Notes: Table 2.7 reports OLS estimates of the effect of the letters on the reelection rates and rates of running for reelection in 2012 (Equation (2.3)). All dependent variables are indicator variables. Columns (1) and (2) show effects on reelection rates: conditional on mayors in 1st term that could run for reelection, and conditional on mayors that chose to run for reelection, respectively. Column (3) reflect impacts of the experiment in the decision of running for reelection, for mayors that could chose to do so. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

2.6 Transitional Section Between Chapter 2 and Chapter 3

In Chapter 2, I focused on understanding how the relationship between politicians and supporters impacts the behavior and political careers of the first, when there is an exogenous increase in the likelihood of punishment for misbehavior. There, I presented evidence that politicians highly supported by historically corrupt campaign donors respond more to this shock and suffer bigger political consequences with the loss of support for their reelection campaigns.

In Chapter 3, with Thiago Scot, we change the focus from the politicians to public servants, in order to investigate how a shock to the political party in power can change the career outcomes for the last. Instead of using a shock on the likelihood of detection of corruption, like I did in Chapter 2, we will explore a quasi-random change in the political power at the local level by investigating locations with close-races in Chapter 3.

Chapter 3

The Career Effects of Political Shocks: Evidence from Health Workers in Brazil[1]

3.1 Introduction

Politicians exploit their power to appoint bureaucrats as a way to reward their supporters in election efforts (Brollo et al., 2017; Barbosa and Ferreira, 2019; Iyer and Mani, 2012). However, patronage frequently leads to the selection of less competent people to public office and may impact the quality of public service delivery (Colonnelli et al., 2020). Moreover, democracies are subject to regular changes in bureaucracy as a result of political turnover, which can also contribute to a disruption in the provision of public services (Akhtari et al., 2022; Toral, 2022).

Although the literature has documented the effects of political turnover on patronage and the provision of public services, there is less evidence on its effects on the careers of public servants, who might lose their jobs to potential supporters of a new administration. Additionally, there is scant evidence on the transition costs incurred by workers that get displaced after a switch in the officeholder's party.

In this chapter, we investigate the effects of political turnover on the employment of health care workers in Brazil. We exploit the variation in vote margins for Brazilian mayors in three different election cycles and estimate regressions for a set of municipalities where the incumbent parties either barely won or lost their reelection races. We make use of a dataset that compiles information on the universe of contracts for Brazilian health care workers in order to quantify disruptions in the employment status and hours provided by employees in this sector as a result of the political turnover.

We find a higher rate of termination of employment contracts in treated municipalities, where there was a change in the party in power, compared to the control, where the incum-

[1]This chapter is joint work of Thiago Scot and Murilo Ramos. All permissions to reprint this material as a chapter of the present dissertation have been obtained.

bent party won the reelection race. More specifically, we find sizeable effects on the turnover of health care workers in the two initial quarters of the new administration, ranging from 7p.p. to 18p.p. We also observe that 4p.p. of this rise in turnover represent the difference in permanent exits from the health care labor force, between the treatment and control groups. Additionally, we present evidence that most of the observed disruption in the employment of health care workers come from individuals who are public employees on a track with less stability and fewer benefits than tenured public servants.

Furthermore, we document that workers in the treated municipalities suffer a disruption in their employment both through a termination of contract at their original workplace and through an adjustment in their hours of service at that location. Although we do not observe individuals leaving the public health care sector for its private counterpart, we find evidence that a considerable share of employees increase the number of hours worked in the neighboring municipalities as a way to compensate for the disruption caused by the political turnover at their original location.

This chapter aims to contribute to two different branches of the literature. The first consists of documenting the *quid pro quo* relationship between politicians and the bureaucracy, its relationship with the electoral cycle, and implications for public service delivery. Colonnelli et al. (2020) make use of a regression discontinuity design in close races in Brazil to show that patronage plays a substancial role on employment in public institutions, for both higher and lower ranked positions, and that it can lead to the selection of individuals with lower qualifications for these jobs. Brollo et al. (2017) explore a similar setup in Brazil to document that politicians use discretionary appointments as a way to influence policy-making while also rewarding supporters. Contrarily to Colonnelli et al. (2020), they report that bureaucrats hired by the ruling party are usually at least as qualified than members from other parties. Barbosa and Ferreira (2019) use a regression discontinuity design with Brazilian data to report that elections provide some degree of cyclicity to patronage: winning parties or coalitions are able to increase their share among public sector workers through discretionary appointments.

Akhtari et al. (2022) explore the same variation in political turnover as we use in this chapter, providing insights about its impacts on public service provision. They focus their analysis on outcomes in the education sector and use the fact that politicians have the discretion to change personnel at municipal schools as a source of variation in the management equilbrium in order to show negative effects of this disruption on test scores of students. Furthermore, Toral (2022) uses a dataset on procedures provided by the public health sector in Brazil to show a decline in health services in the period right after elections, during the last weeks of the term of lame-duck politicians [2].

A second strand of the literature investigates the career effects of shocks that are com-

[2]Some authors have investigated the allocation of health workers and its impact in the quality of health services in several countries. Some examples are Carrillo and Feres (2019); Okeke (2021); Bartel et al. (2014); Akosa Antwi and Bowblis (2016)

mon to a large set of workers. For instance, Autor et al. (2014) use import shocks of chinese manufactured goods in the U.S. economy to show that lower skilled workers suffer significant drop in earnings and that the labor adjustment can exacerbate inequality between individuals with high and low levels of human capital. Walker (2013) reports that a regulatory shock related to environmental policies can generate significant transition costs to workers, both through temporary unemployment and reduction in lifetime earnings. Finally, Dix-Carneiro and Kovak (2019) use variation in tariffs during the 1990s trade liberalization in Brazil to show effects in regional labor markets that persist long after the initial shock.

The rest of the chapter is structured as follows. Section 3.2 provides background on the Brazilian public health care system, and presents the data used in the empirical analysis. Section 3.3 discusses the econometric framework and Section 3.4 present the results. Section 3.5 concludes the chapter.

3.2 Institutional Background and Data

3.2.1 Overview of the public healthcare system in Brazil

Brazil has the biggest system of public health in the world, known as Unified Health System ("Sistema Único de Saúde" or SUS). This system provides access to free health services to all the population in Brazil, for both citizens and non-citizens of the country, and relies on a cooperation between the three main levels of government: federal, state, and municipalities.

SUS provides a wide range of health services for the population: from primary care services and vaccination, to more sophisticated treatments for complex conditions. In general, municipalities manage the primary care health services, in small local clinics known as "Basic Units of Health" (Unidades Básicas de Saúde or UBS), whereas the states and the federal government usually concentrate their efforts on public hospitals or more advanced centers for more severe conditions, such as cancer treatments or complex surgeries.

In this chapter, we focus on the primary care health sector, mostly managed by the local government, and consists of the most used service by the population, with services ranging from preventive health and vaccination, to small surgeries and image exams. At this level of services, there are four most common occupations: physicians, nurses, nurse assistants, and community health workers(CHW). The first two groups consist of individuals with at least a college degree [3]. Nurse assistants complete a technical degree in nursing, equivalent to high-school level, and community health workers are required to complete a high-school program in order to qualify for the job. CHW are present at the public health services in Brazil since the 1990's, but they have grown in importance since the beginning of the 21st century. For the period of our study, the majority of CHW come from the community they

[3]In Brazil, physicians and nurses receive training directly at the college level and do not need to attend graduate school in order to serve in the health sector.

are serving and have close contact with the population, by visiting homes and performing basic health checks on individuals.

Moreover, Brazilian healthcare workers in the public sector can be hired into two main tracks: civil servants ("estatutários") and non-civil servants ("não estatutários"). Civil servants usually enjoy a wide range of benefits, such as tenure, special pension, better baseline wages, and these tend to be, on average, more generous than the ones offered to other workers in similar jobs, both inside and outside the public sector. For this reason, usually the selection process for civil servants' positions is considerably more competitive than for the other tracks. There is also more flexibility in the hiring process of non-civil servants, and mayors have historically used their power to benefit their allies with public servant positions in this track [4].

3.2.2 Healthcare data in Brazil - CNES

In this chapter, we use a dataset know as National Registry of Health Establishments ("Cadastro Nacional de Estabelecimentos de Saúde or CNES), which includes information about the physical structure, services, and workers in the healthcare sector in Brazil. Created in 2003, CNES initially only included public institutions and its services, but expanded in the following years to include private institutions as well. We collected the data on all contracts between workers and healthcare facilities, both in the public and private sectors.

In table 3.1, we present descriptive statistics for this dataset, using as reference the first quarter of the election years in our study. In panel A, we use contracts between workers and a healthcare institution as the unit of observation, and find a proportion of 85.4% contracts involve public healthcare services, with an average of 27.5 weekly hours per contract. Panel B uses each healthcare worker as the unit of observation. On average, each worker provides 40.3 weekly hours in total and women correspond to almost 74% of the healthcare workforce. Additionally, we present the share of the four most common occupations in the dataset, listed here in descending order: nurse assistants, nurses, community health workers, and physicians.

Panel C restricts the data to primary health care professionals, excluding staff in hospitals and other complex services. These services are provided mostly managed by municipalities, at a share of 88.3%. We observe that the share of women in this workforce increases to almost 80%, that an average worker holds 1.38 contracts and provides around 42 hours of work per week. We also detect a change in the share of occupations in comparison to panel B. Now community health workers correspond to almost 40% of the labor force, followed by nurse assistants, nurses, and physicians, at shares of 22.5%, 17%, and 10.6%, respectively.

Finally, panel D reports the proportion of contracts observed two years after our baseline, for a measure of overall turnover by each occupation over this period. Overall, 67% of

[4] One of the most common ways used for politicians to hire allies is through commissioned positions ("cargos comissionados"). They exist in virtually all areas of public service, in the local, state, and federal levels.

contracts are still observed at the end of the 24-month span, from a low of 48.7% and 53.5% for physicians and nurses, and a high of 78.1% for community health workers [5].

3.3 Econometric Framework

Below we present estimates of the impact of political turnover, defined as a change of party in power at the municipal level, on the turnover of PHC workers. We restrict our sample to "close elections", defined as those where the victory margin of the winner was less than 5 percentage points, and estimate equations of the form:

$$Y_{ismo} = \alpha + \beta \mathbf{1}\{\text{Political Turnover}\}_m + \gamma_s + \psi_o + \epsilon_{ismo} \tag{3.1}$$

where $\mathbf{1}\{\text{Political Turnover}\}$ is an indicator variable on whether the municipality $\underline{m}$ experienced a political turnover, and γ_s and ψ_o are fixed-effects for state and occupations, respectively [6][7].

We estimate this equation separately for each quarter, starting from two quarters before the election until nine quarters after the election, for three different election cycles: 2008, 2012, and 2016. For each election cycle , we set our sample of workers using as baseline for the panel dataset the workers with active contracts in the first quarter of each election year. We then follow these workers for several quarters after the baseline quarter. The main outcomes in this chapter are binary variables that denote whether a contract for a given worker $\underline{i}$, in a state $\underline{s}$, in municipality $\underline{m}$, with an occupation $\underline{o}$ is terminated in different periods before and after the election. In some of our specifications, the outcomes will be the hours worked in the main and neighboring municipalities.

Our measure of political turnover is constructed by using the margins of vote for the challenger in the Brazilian local elections for 2008, 2012, and 2016, using data from the Brazilian electoral authority ("Tribunal Superior Eleitoral" or TSE). We use figure 3.1 to show that there is a lot of regional variation on the political turnover variable, using the elections of 2012 as an example. In figure 3.2a, we plot the histogram of the margin-of-victory for the candidates that challenge the incumbent party. In figure 3.2b we present the McCrary density test and do not observe evidence of manipulation of the margins of vote, calculated using a local linear regression smoothing function.

[5] From our conversations with the public healthcare management team in some municipalities, this is consistent with the fact that CHW are attached to their local community where they come from, have low mobility and less job opportunities, compared to the other professionals, who frequently switch working locations.

[6] The political turnover indicator variable can also be defined as $\mathbf{1}\{\text{Incumbent margin} < 0\ \}_m$.

[7] We will refer to treatment group for municipalities where there was a political turnover and control for the ones where the incumbent party remained in power.

3.4 Results

3.4.1 Effects of political party change on the healthcare sector turnover

Our regressions focus on estimating the effect of the change of party in the local government on the turnover of workers in the primary health care public sector in Brazil. Our main coefficient of interest is the β from Equation 3.1, which is estimated on a quarterly basis starting two quarters before the election, up until 8 quarters after it. For most of our specifications, this coefficient will reflect the difference in termination of employment between municipalities where there was a change in the party in power, compared to places where the incumbent party succeeded in the reelection campaign.

Figure 3.3 shows the effects of political turnover on the termination of contracts between health workers and the primary health care units. We do not observe statistically significant estimates for the quarters that precede the election. We do observe a relatively small effect of almost 2p.p. of more terminations of contracts in the treated municipalities compared to the control group, which demonstrates that the effects of political turnover start to happened as soon as the election result is known to individuals. The effects of political turnover peak at the second quarter in the term of the new administration at 7p.p. and then reach an asymptote at 5p.p. value.

The next four graphs focus splits the sample into the four most common occupations in the primary care sector, in order to analyze how the effects of political turnover vary by the work activities. In figure 3.4 we observe relatively small coefficients for community health workers, that reach a highest estimate of 3.5p.p. in the second quarter of the first year of government, and stabilize at 3p.p. in the long run. Figure 3.5 reports the effects on physicians' contracts, that approach a high of 6p.p. after the election and down to 4p.p. in the eighth quarter of the administration. In figure 3.6, we detect the highest effects among all occupations, for the nurses with college degree. The coefficients hit the 10p.p. mark and stabilize above 5p.p. in the long run. Similar, but smaller effects are observed in figure 3.7, with a maximum effect of 8p.p. in the second quarter after the election.

CHW tend to stay much longer in their contracts compared to other occupations, independent of the political turnover in the municipality, and also are less affected by the change of party in power. For this reason, we focus the rest of our analysis in this chapter on the sample that excludes these workers. In figure 3.8, we plot the coefficients for the contracts of healthcare workers, except CHW. We observe estimates close to 10p.p. at the peak of the curve, which decreases to 6p.p. at the right tail. Furthermore, many workers hold more than one contract at a time and can change contracts within the same municipality, with virtually no changes in their work activity and responsabilities. We modify the dependent variable in figure 3.9 such that it codifies only terminations of all contracts that a worker has at a given municipality, for each relevant quarter. Most point estimates are similar to the ones in figure 3.8, except for a larger coefficient of 3p.p. in the quarter when the election

happened.

Figure 3.10 presents coefficients on the effects of political turnover on a binary variable that indicates whether a worker is not observed working in the health care sector, including non primary care and private health sector as well, for different periods. Although the estimates are smaller in magnitude compared to the previous figures, we still observe a considerable effect, that fluctuates around the 5p.p. value for all quarters after the election[8]. In figure 3.11, we refine the measure of work turnover, by focusing on heath care providers that leave the labor force and are not seen anymore in our dataset, as evidence that their exit of that sector was permanent. We observe that workers in the treated municipalities leave the health care labor force at a rate of 4p.p. higher than municipalities in the control group. Considering that the unconditional 2-year turnover rate for nurses and doctors are around 50p.p., our effects due to the political turnover are expressive.

In section 3.2.1, we emphasized the existence of two main tracks to the public service in Brazil: the tenured employees as the civil servants and the non-tenured workers. As expected, turnover is much higher for the non civil servants track, as figure 3.12 reports. Non civil servants are more likely to suffer termination of contracts and to leave the health care sector, with rates that reach 18p.p. and 9p.p., respectively for these outcomes.

3.4.2 Effects of political party change on the hours worked in healthcare

In this section, we evaluate the effects of political turnover on the hours supplied by health workers in their main municipality and additional municipalities. In figure 3.13, we keep our restriction in the sample to non community health workers and combine both intensive and extensive margin effects on the hours reported for the employees. Workers are employed, on average, for three less hours in a week at their main municipality, and partially substitute them for an additional hour worked in neighboring municipalities.

Figure 3.14 focuses on the hours worked by non civil servants. In graph 3.14a we do not impose further restrictions in the sample, and observe an average decrease in hours worked by these employees at their main municipality on a range from four to eight hours per week, with a slight increase in supplied hours in other municipalities at one additional hour. As we restrict the sample to employees that do not leave their main municipality after the change in the political power in figure 3.14b, we observe a symmetrical effect: workers substitute around three hours of their service in the main municipality to virtually the same amount in other municipalities. We take this result with caution, due to the selection bias that can appear as we restricted our sample to these employees that staed at their main jobs.

[8]It is relevant to note that workers that left the health care sector for some quarters, and return after some time are still considered in this graph.

3.4.3 Additional results

In figure 3.15, we test whether the political turnover impacts the likelihood that health care workers switch their main contracts to the private health sector. We do not see evidence of that in our regressions. However, when we investigate the effects of political turnover on the distance between the original municipality and the new location of service for the main contract where the employees are supplying their hours, we observe an increase of around 3p.p. in the likelihood that workers are traveling more than 10km to their new workplace, as shown in figure 3.16. Therefore, as we connect our main results, we observe that the political turnover increases the termination of contracts for health care workers in a given municipality. Workers do not switch to the private health care sector, but either increase their hours supplied in neighboring municipalities or leave the health care labor force altogether.

We present a brief robustness check in figure 3.17 regarding the margins of vote used for the creation of the indicator variable of political turnover and the limitation on what we define as close elections. We focus on the coefficients in the quarter before elections and eight quarters after it, and how they change as our measure of close elections change. Our benchmark incumbent loss margin is 0.05. As we vary it along the horizontal axis, we do not observe considerable changes in the point estimates, but mostly an increase in the range of the confidence intervals as the loss margin narrows, and thus the number of observations in our sample decreases.

3.5 Conclusion

This chapter documents the effects of political turnover on the careers of health care workers in Brazil. We explore a dataset with information on all employment contracts for health workers, in order to measure changes in the employment stauts and hours supplied, as a result of the switch of administrations in the local government. We make use of a close-races design in order to divide municipalities into a treatment group, where challengers barely won the election, and a control group, in which the incumbent party marginally succeeded in the reelection campaign.

Overall, we find larger rates of termination of employment contracts for workers in treated municipalities compared to the control group. This differential effect starts in the lame-duck period, right after the election, peaks at the first two quarters of the new administration, and later stabilizes at lower levels in a two year window, for most of the outcomes of interest. Moreover, we detect more pronounced effects of the political turnover for the employment of non civil servants, which is consistent with the fact these workers have less job security and stability, compared to the tenured employees. Furthermore, we estimate substantial effects of the change in administration on the likelihood that a employee permanently leaves the health care labor force. We also observe a change in the profile of hours supplied at the baseline workplace, with some degree of substitution towards employment in neighboring municipalities.

The next natural step towards understanding the effects of political turnover on the careers of health care workers is to quantify whether it impacts the lifetime earnings, long term wages, and job activities of these individuals. Additionally, we foresee the possibility of a more detailed analysis on costs beared by these workers associated with the increase in their commuting distances to work. Finally, additional evidence on why we observe turnover effects for employees in jobs not considered traditionally "political" could provide a more complete account on the mechanisms behind the reduced form results in this chapter.

Figures

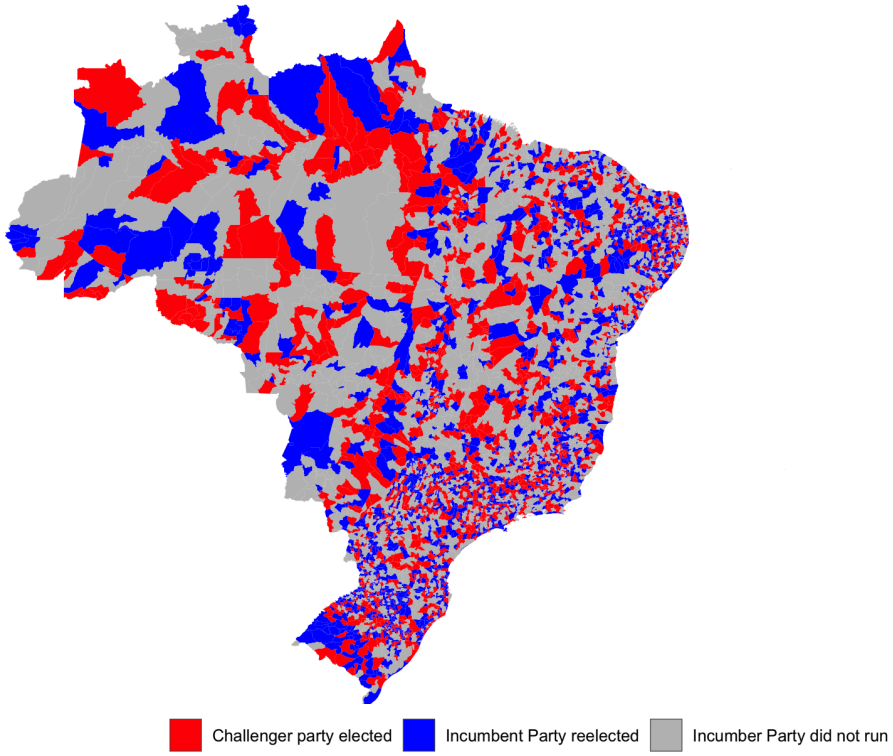
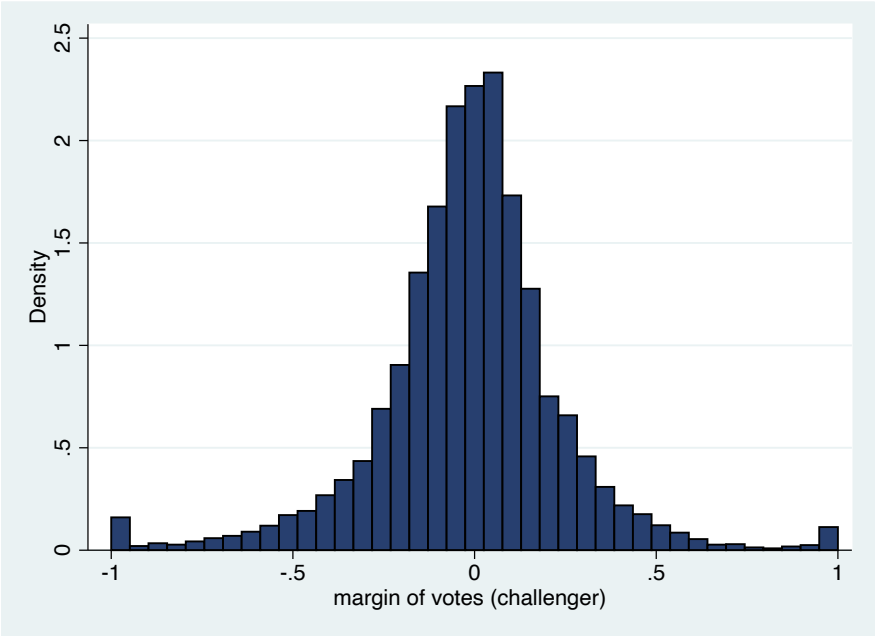
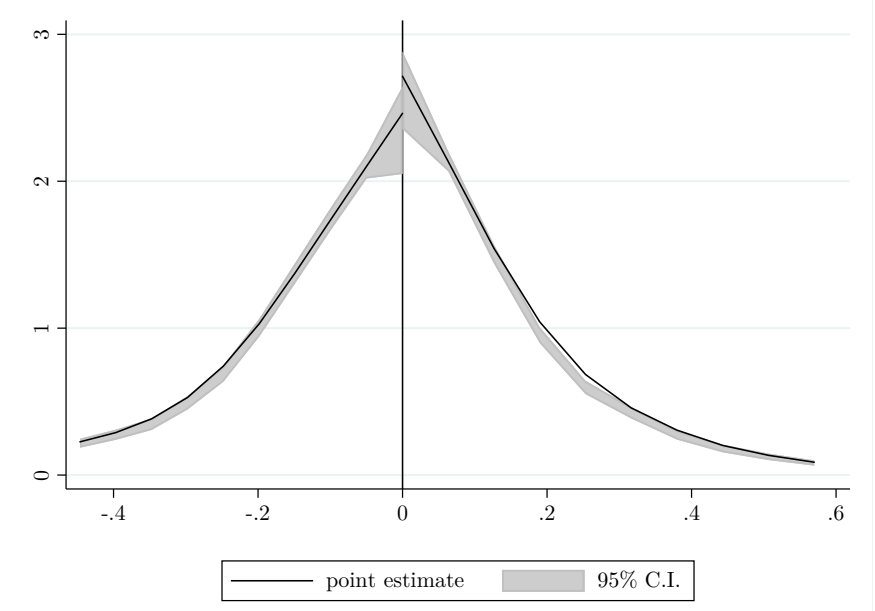


Figure 3.1: Political turnover for local elections in Brazil (2012)

Notes: Figure 3.1 shows a map of Brazilian municipalities according to the results of local elections in 2012. Municipalities are divided into three groups: red where there is political turnover; blue where the incumbent party wins the reelection race; and grey where the incumbent party does not run for reelection.



(a) Histogram



(b) Local linear regression

Figure 3.2: Density of the voting margins for the challenger in Brazilian municipalities

Notes: Panel 3.2 plots the margin of votes for the challengers at the local elections in Brazil. The data includes the elections in years 2008, 2012, and 2016. Panel 3.2a denotes the histogram of the votes margins and panel 3.2b plots the estimated non-parametric density function of the same variable, with a 95% confidence interval for this function.

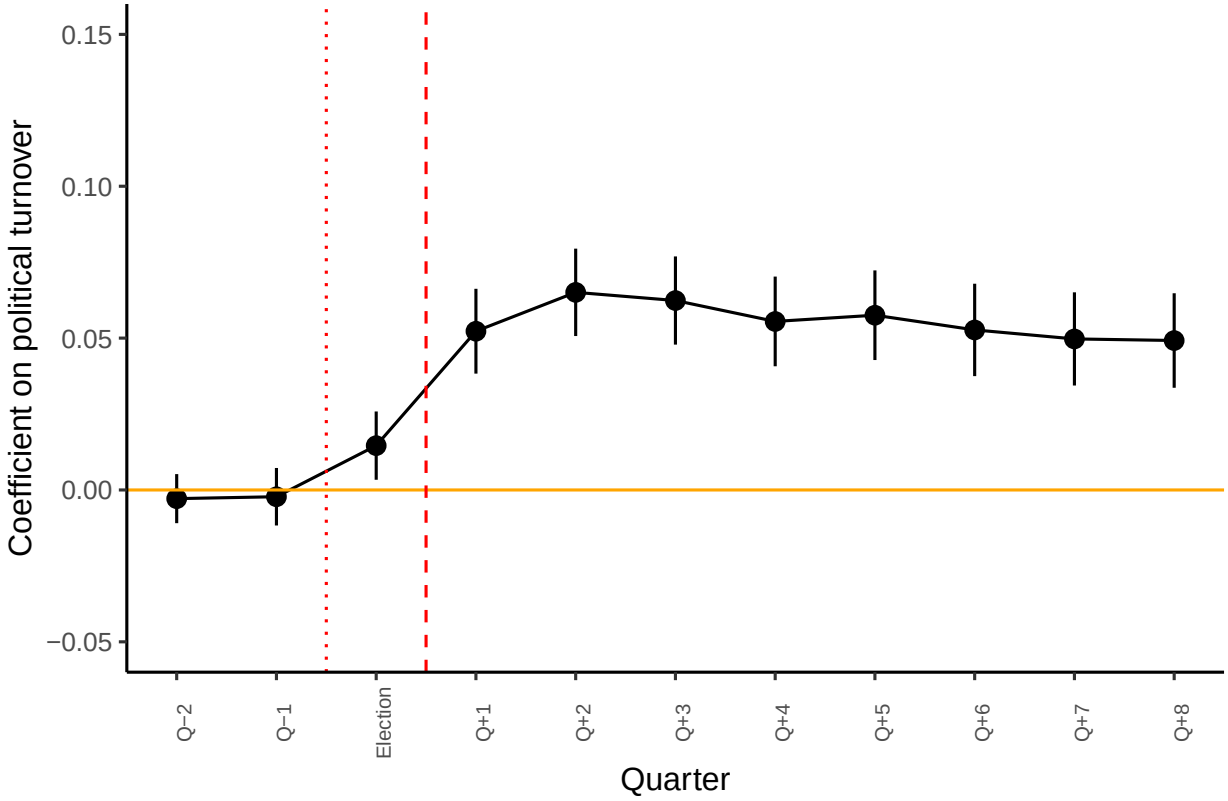


Figure 3.3: Effects of political turnover on contracts for healthcare workers

Notes: Figure 3.3 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given worker in a municipality, for each relevant quarter. Only healthcare workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

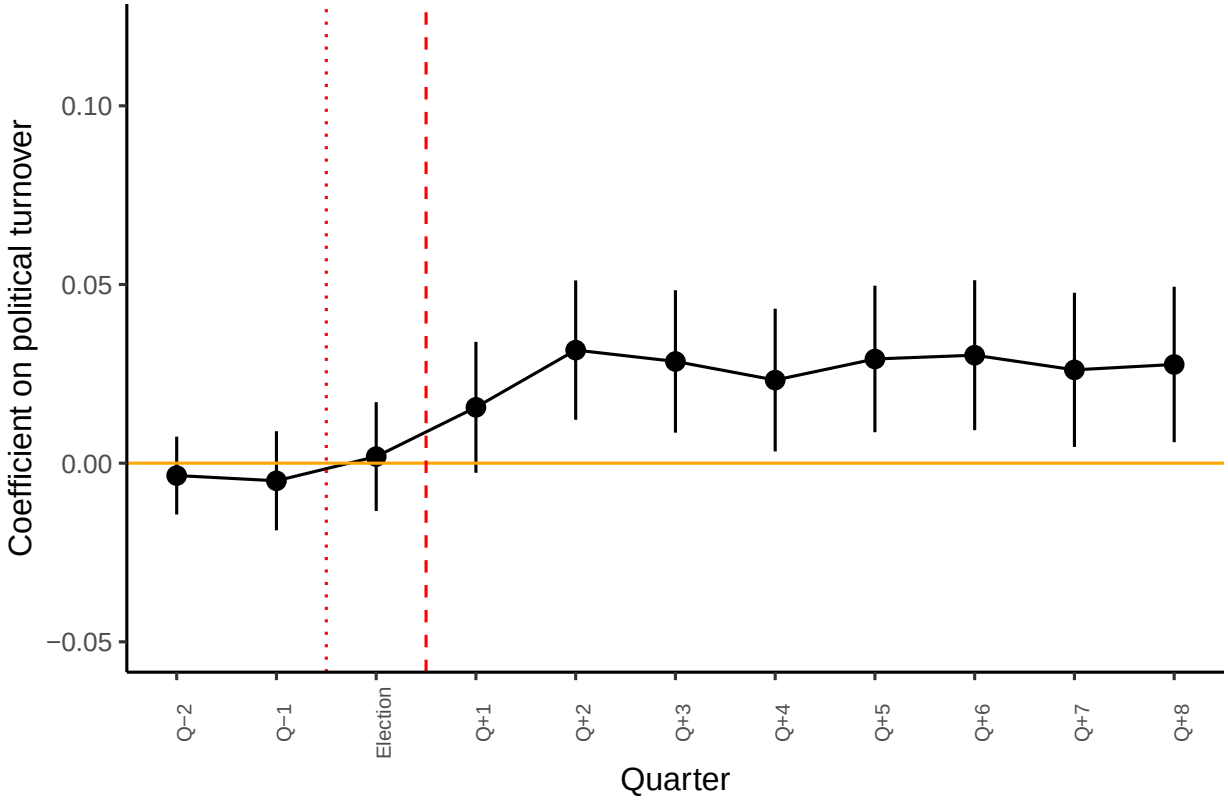


Figure 3.4: Effects of political turnover on contracts for community health workers

Notes: Figure 3.4 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given community health worker in a municipality, for each relevant quarter. Only healthcare workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

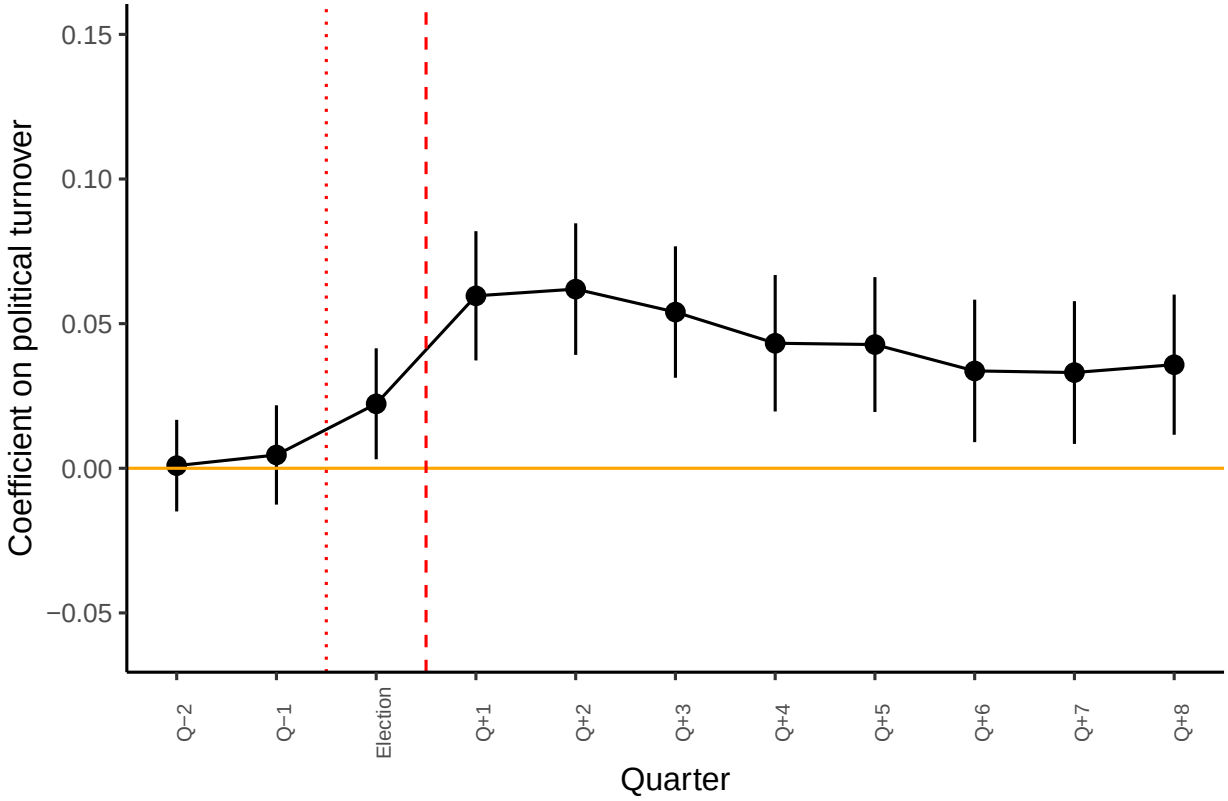


Figure 3.5: Effects of political turnover on contracts for physicians

Notes: Figure 3.5 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given physician in a municipality, for each relevant quarter. Only healthcare workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

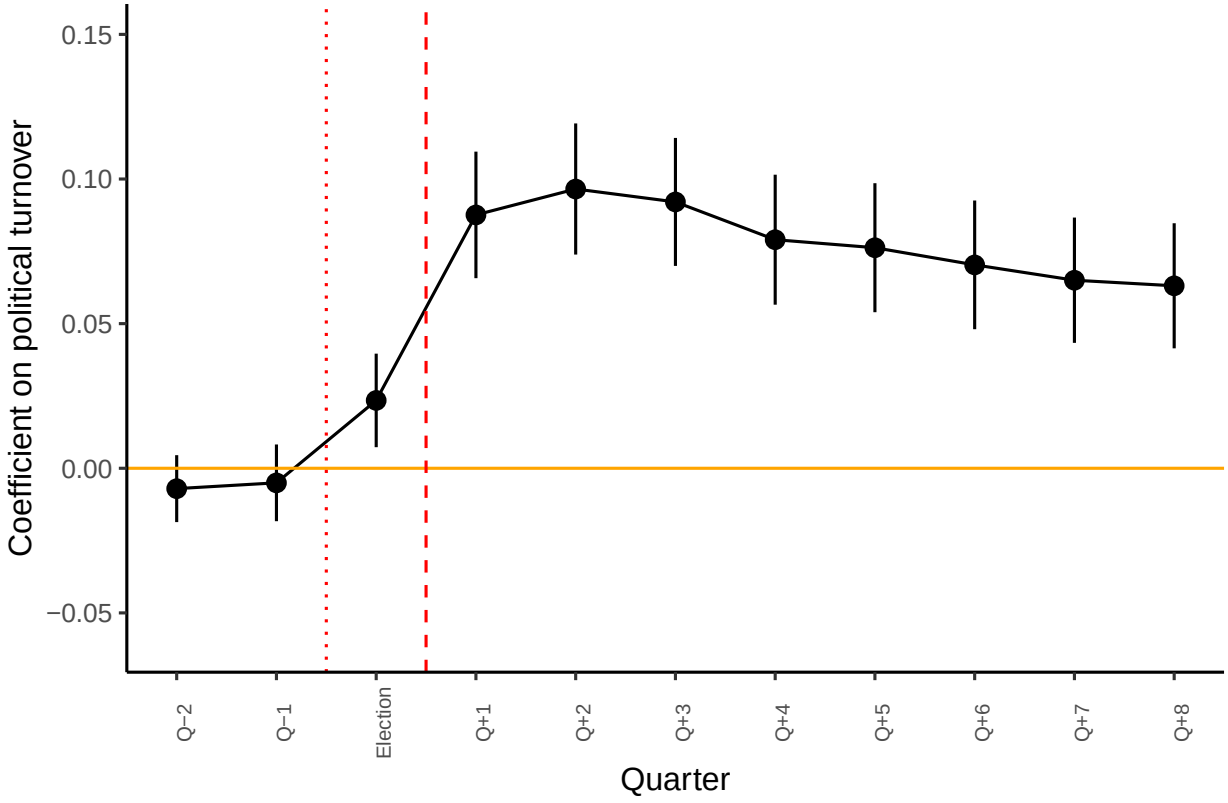


Figure 3.6: Effects of political turnover on contracts for nurses

Notes: Figure 3.6 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given nurse in a municipality, for each relevant quarter. Only healthcare workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

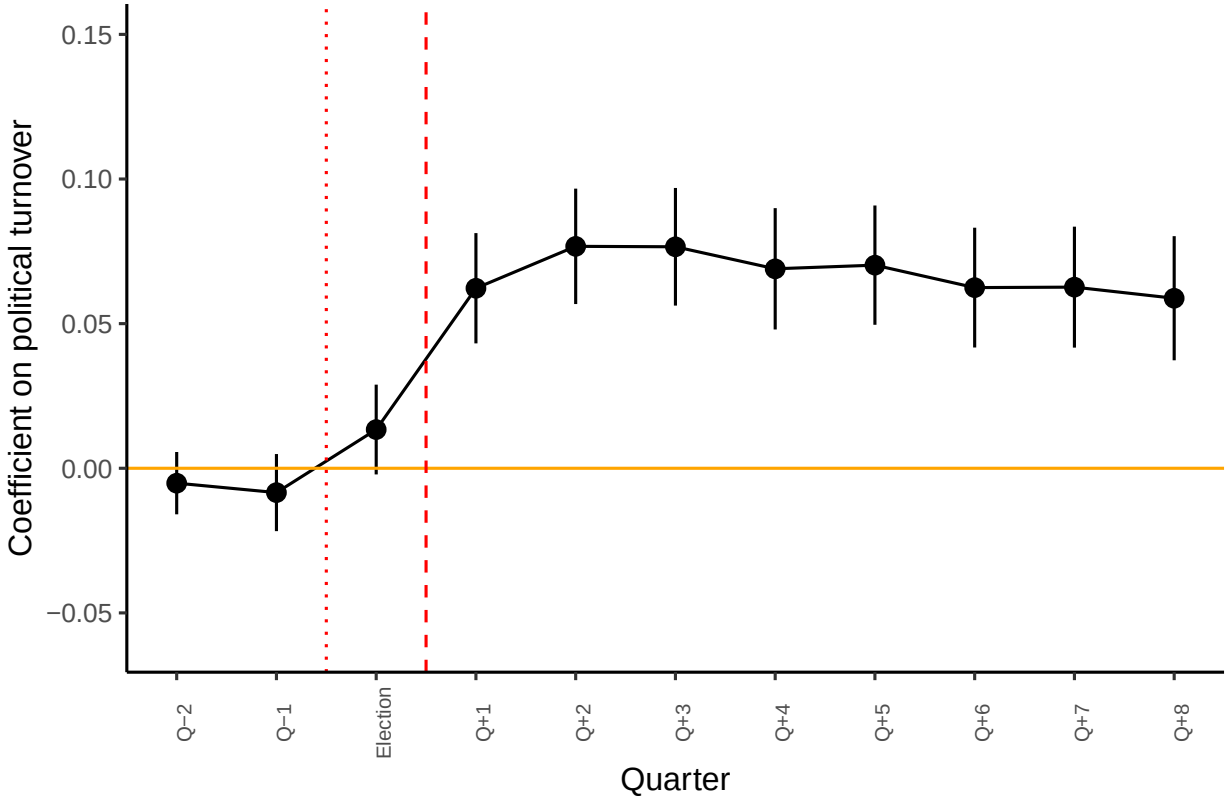


Figure 3.7: Effects of political turnover on contracts for nurse assistants

Notes: Figure 3.7 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given nurse assistant in a municipality, for each relevant quarter. Only health care workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

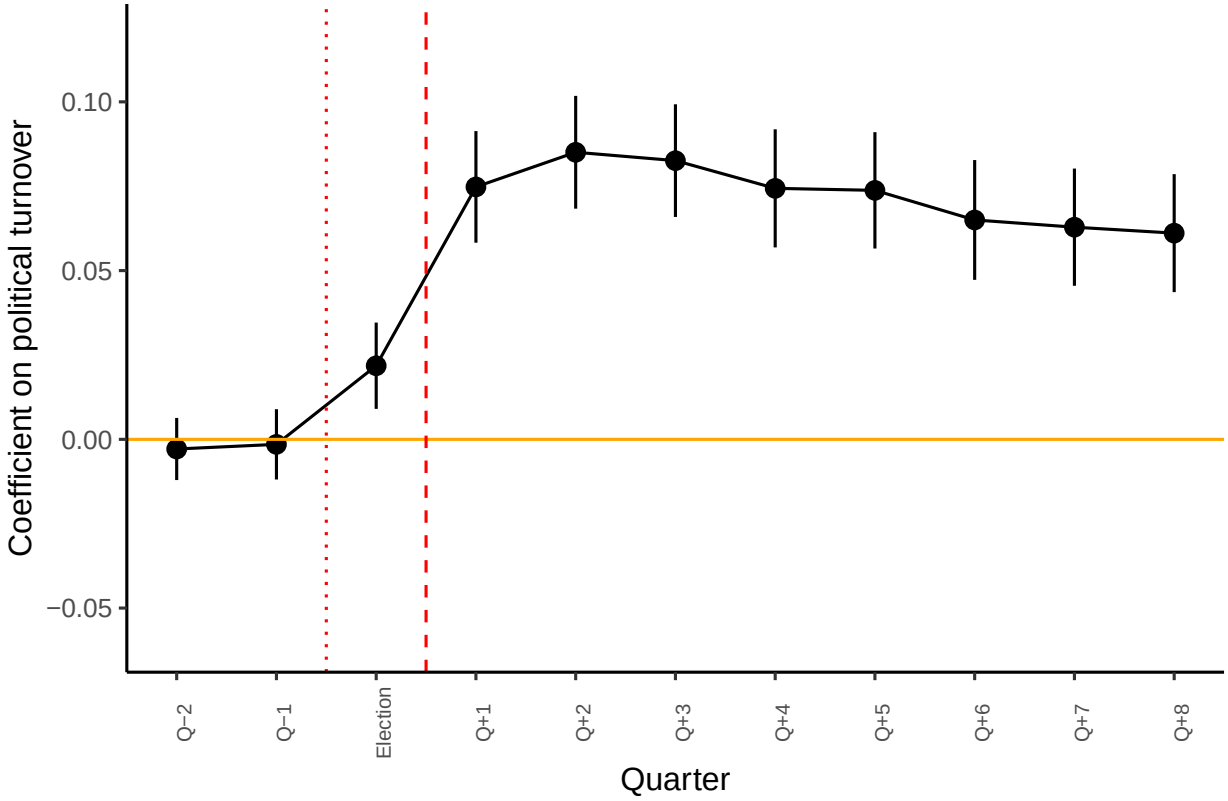


Figure 3.8: Effects of political turnover on contracts for healthcare workers - except CHW

Notes: Figure 3.8 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether the same contract is not observed for a given healthcare worker in a municipality, for each relevant quarter, and excludes community health workers. Only health care workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

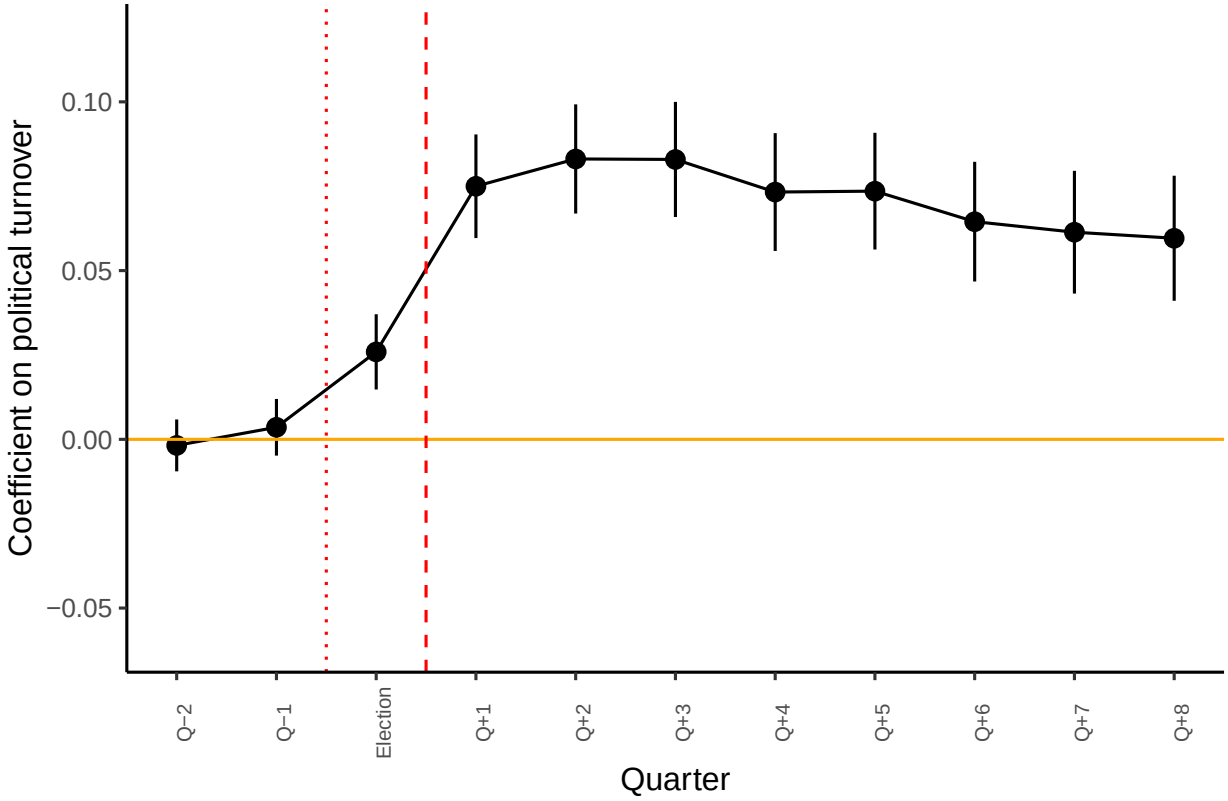


Figure 3.9: Effects of political turnover on the employment of healthcare workers in a given municipality - except CHW

Notes: Figure 3.9 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether a worker is not working in a municipality, for each relevant quarter, and excludes community health workers. Only health care workers in the public healthcare clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

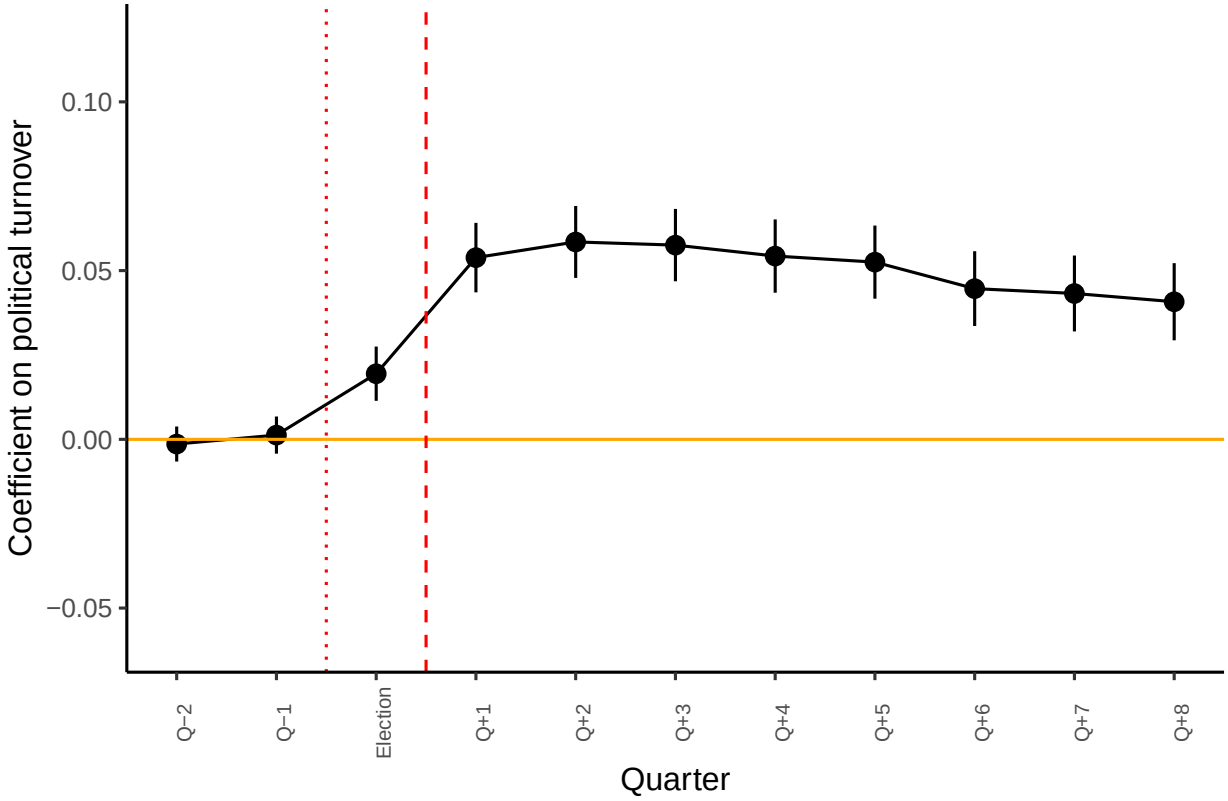


Figure 3.10: Effects of political turnover on the employment of healthcare workers - except CHW

Notes: Figure 3.10 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether a worker is not observed working in the healthcare sector, for each relevant quarter, and excludes community health workers. Only health care workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

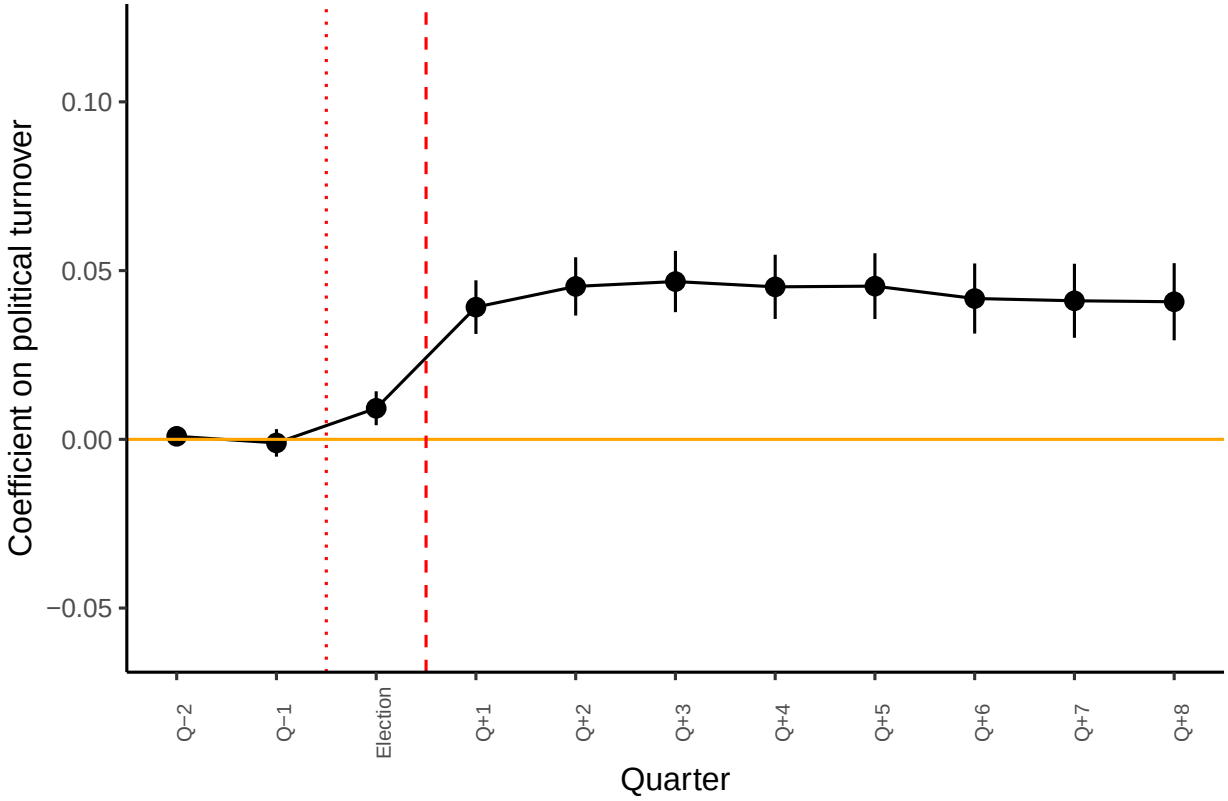
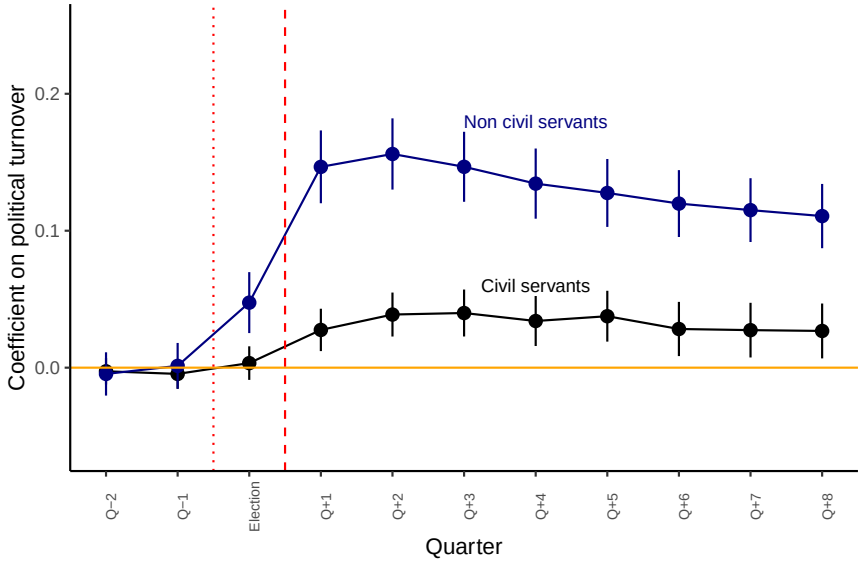
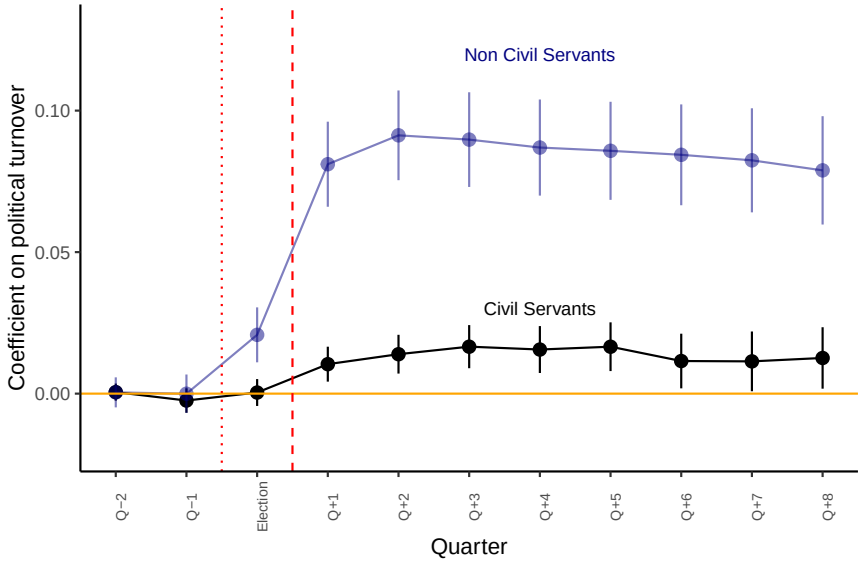


Figure 3.11: Effects of political turnover on the heathcare labor force - except CHW

Notes: Figure 3.11 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that indicates whether a worker leaves the labor force in the healthcare sector , and excludes community health workers. Only health care workers in the public healthcare clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.



(a) Contracts' turnover



(b) Workers leaving labor force

Figure 3.12: Effects of political turnover on the heathcare workers - by civil servants' status

Notes: Figure 3.12 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016, for civil servants and non civil servants. In figure 3.12a, the dependent variable is a binary variable that indicates whether a contract is not observed for a given worker and municipality in each quarter. In figure 3.12b, the dependent variable denotes whether a worker is leaving the healthcare labor force. Only health care workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

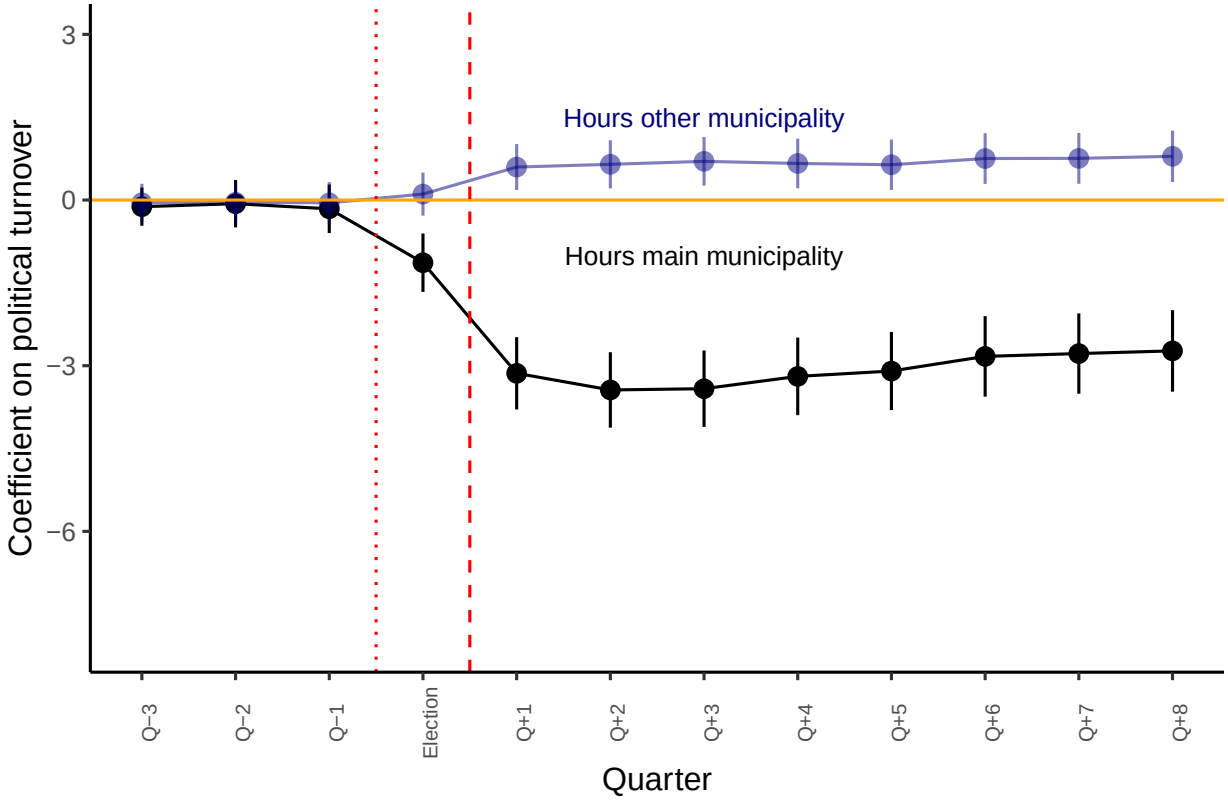
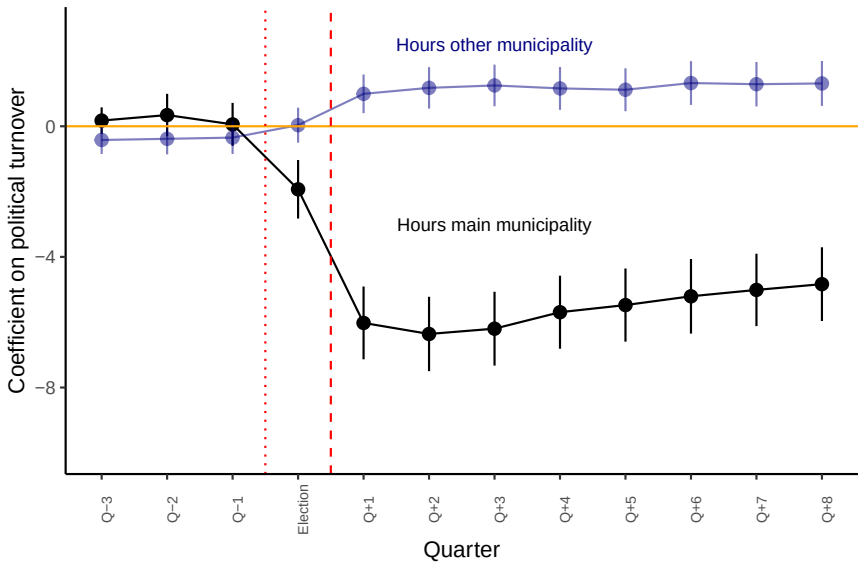
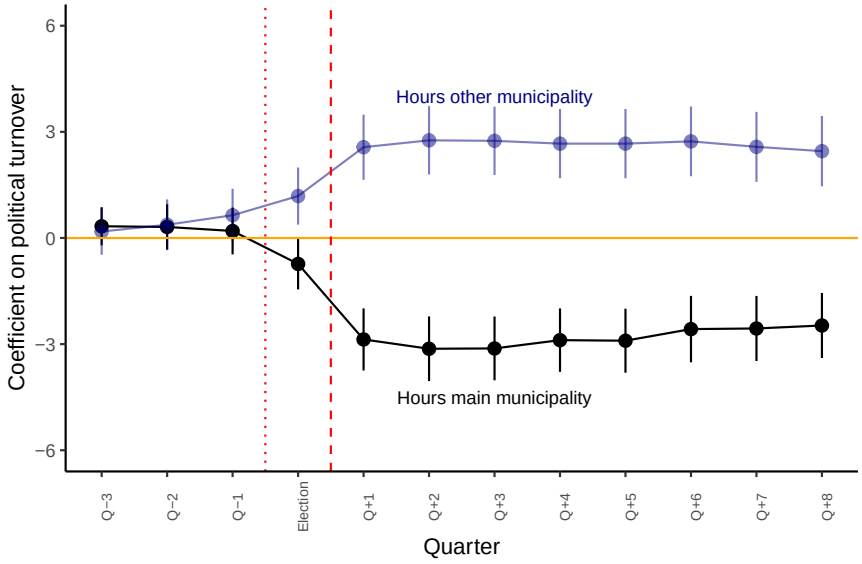


Figure 3.13: Effects of political turnover on the hours worked in a municipality - except CHW

Notes: Figure 3.13 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is the number of hours provided by a healthcare worker in a given municipality,and excludes community health workers. Only healthcare workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.



(a) Extensive and intensive margins



(b) Only intensive margin

Figure 3.14: Effects of political turnover on the hours provided by healthcare workers - only non civil servants

Notes: Figure 3.12 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016, for civil servants and non civil servants. The dependent variable is the number of hours provided by a healthcare worker in a given municipality,and excludes community health workers and civil servants. Only health care workers in the public health care clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

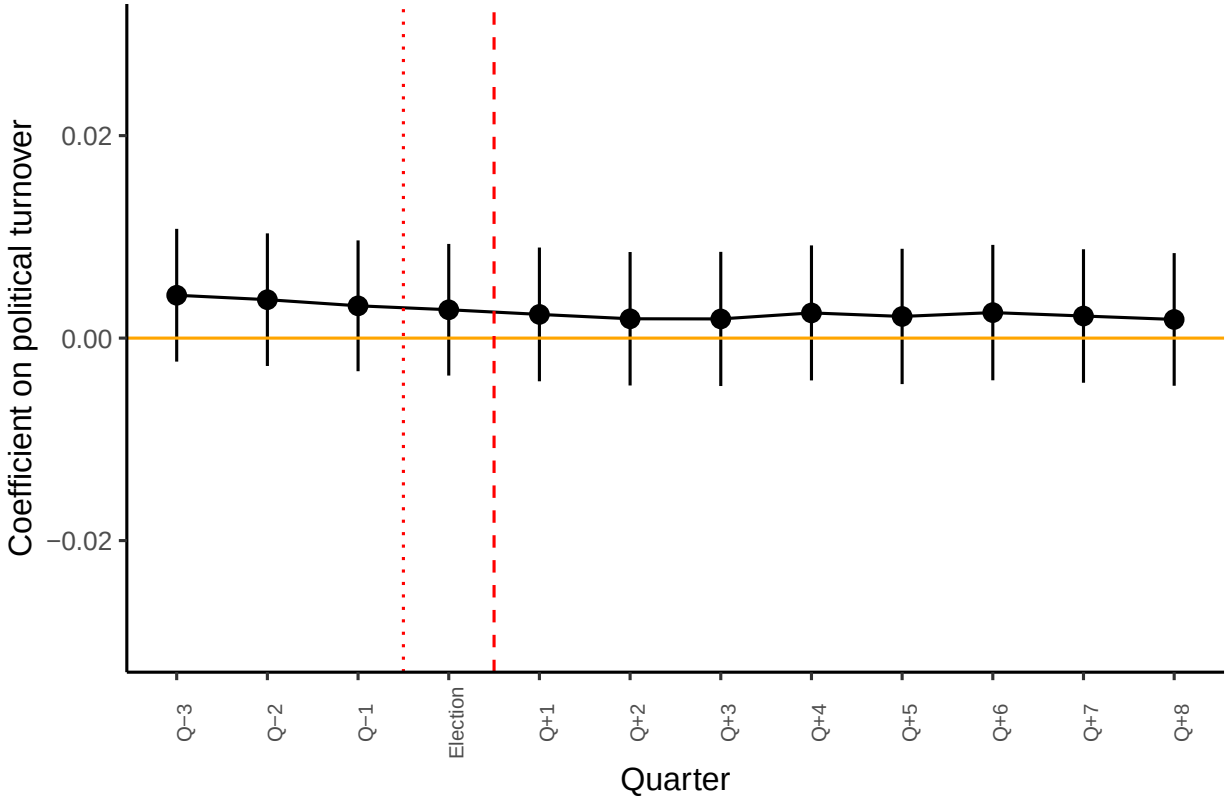


Figure 3.15: Effects of political turnover on the likelihood of switching to private sector

Notes: Figure 3.15 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that represents individuals that switch their main contract to the private sector. We use active contracts for health care workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

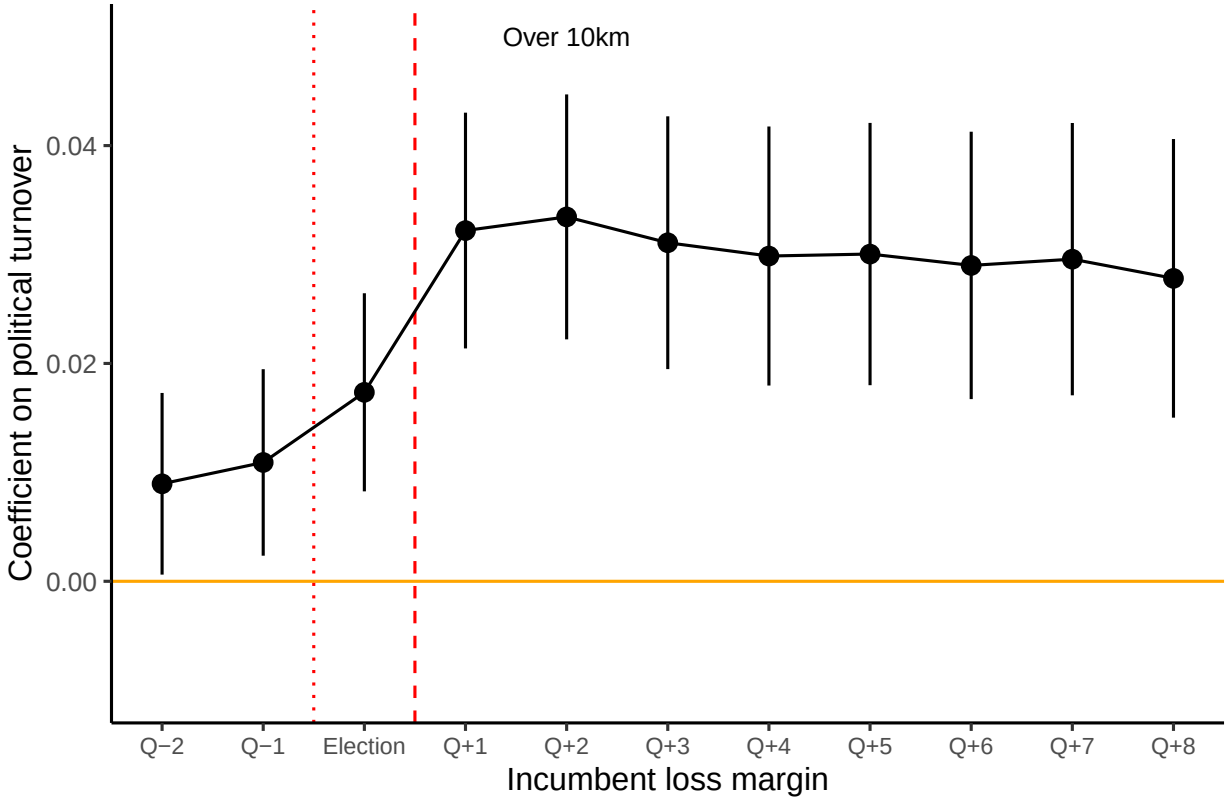


Figure 3.16: Effects of political turnover on the distance to the workplace for healthcare workers

Notes: Figure 3.16 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the elections of 2008, 2012, and 2016. The dependent variable is a binary variable that represents individuals that switch to jobs in places where the distance is above 10km of their original workplace. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

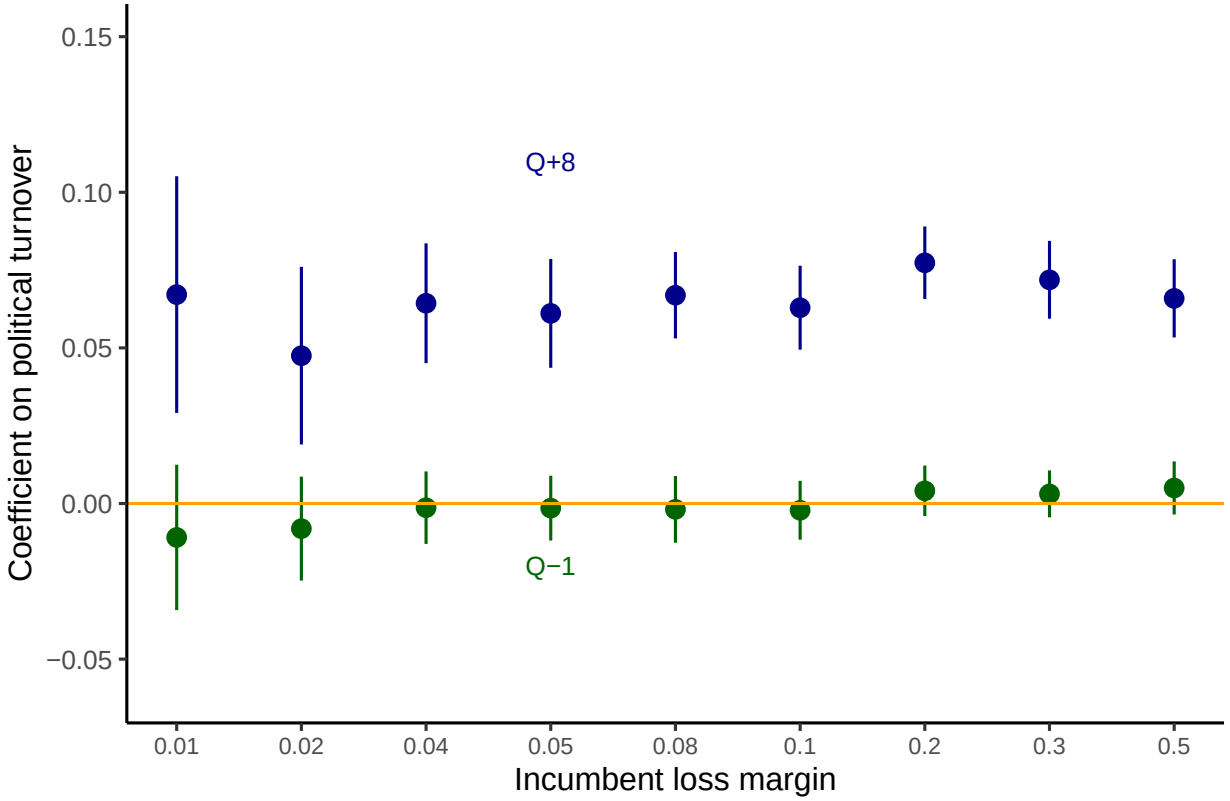


Figure 3.17: Robustness of political turnover effects with different ranges for close elections

Notes: Figure 3.17 reports the coefficients for the "Political Turnover" variable in equation 3.1, for the turnover of contracts for the quarter before the election and for eight quarters after the election. The dependent variable represents whether a contract is not observed in these periods. The horizontal axes represents different margins of vote used to characterize close elections. Only healthcare workers in the public healthcare clinics (Unidade Básica de Saúde) are considered in graph. Private sector workers or workers in public hospitals are excluded from the data. We use active contracts for healthcare workers in the first quarter of each electoral year as the baseline of individuals that are followed in our panel.

Tables

Statistic	N	Mean	St. Dev.	Median	Max
Panel A: All contracts					
Public sector employee (SUS)	2,568,856	0.854	0.353	1	1
Number of hours	2,568,856	27.513	14.208	30	440
Panel B: All health professionals					
Male	1,753,716	0.265	0.441	0	1
Total hours worked	1,753,716	40.301	15.612	40	470
CHW	1,753,716	0.157	0.364	0	1
Physician	1,753,716	0.147	0.354	0	1
Nurse	1,753,716	0.194	0.396	0	1
Assistant	1,753,716	0.305	0.461	0	1
Panel C: Primary care health professionals					
Male	684,185	0.216	0.412	0	1
Number contracts	684,185	1.374	1.098	1	53
Number unique establishments	684,185	1.320	0.888	1	53
Total hours worked	684,185	42.500	13.124	40	400
Hours SUS	684,185	41.599	12.514	40	400
Hours private	684,185	0.901	4.941	0	240
Municipal level	684,185	0.883	0.322	1	1
CHW	684,185	0.397	0.489	0	1
Assistant	684,185	0.224	0.417	0	1
Nurse	684,185	0.169	0.375	0	1
Physician	684,185	0.106	0.308	0	1
Panel D: PHC contracts - Share observed 24 months later					
All	676,013	0.669			
CHW	272,469	0.781			
Physician	65,113	0.487			
Nurse	115,233	0.535			

Table 3.1: Descriptive statistics of CNES dataset

Notes: Table 3.1 describes the dataset of healthcare workers' contracts (CNES). Panel A describes all contracts observed in March of 2012. Panel B restricts the sample workers instead of contracts. Panel C focuses on primary care health professionals and exclude individuals that work outside the municipal clinics, such as in state hospitals. Panel D presents a measure of work turnover for different occupations in the health care sector: community health workers, physicians, and nurses.

Chapter 4

Dissertation Conclusion

The main goal of this dissertation was to investigate a wide set of implications that arise as a result of relationships between politicians and supporters on the behavior of officeholders, its impacts on public policies enacted, and its consequences on the labor market for public employees.

In Chapter 2, I presented evidence on the impacts of a temporary shock in the probability of detection of misbehavior for politicians on the profile of public expenditures and the career of officeholders. My findings suggest that a temporary threat of punishment to illicit behavior might disrupt the long-term equilibrium, altering the relationship between politicians and firms, as well as the composition of politicians elected to office. Additionally, I present some evidence that political corruption change the composition of public spending, especially in the capital expenditure projects. There is still a long avenue ahead in order to understand the effects of political corruption in the provision of public goods. A considerable challenge consists of measuring the quality and even quantity of public service or goods delivery, which I could not still assess in this dissertation.

In Chapter 3, which is co-authored with Thiago Scot, we presented evidence that the political turnover at the local level produces sizeable effects on the careers of public servants in the health care sector. Although we were able to document changes in employment status and hours of work, future projects could analyze longer term effects of this shock on lifetime earnings and costs borne by individuals that leave the labor force.

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Appendix A

Appendix for "Corrupt Politicians, Campaign Donors, and the Power of a Threat: Evidence from a Policy Experiment in Brazil"

A.1 Text Mining

A.1.1 Block grants

The data of block grants provides a variable that describes the project that is financed through them. When filling the application for these grants, municipalities describe in a few sentences the scope of the project, which provides rich information regarding the destination of the federal resources. Using software R, I constructed a vector of keywords in order to quantify the most common projects and categorize block grants between construction and non-construction works.

A.1.2 Campaign Donations

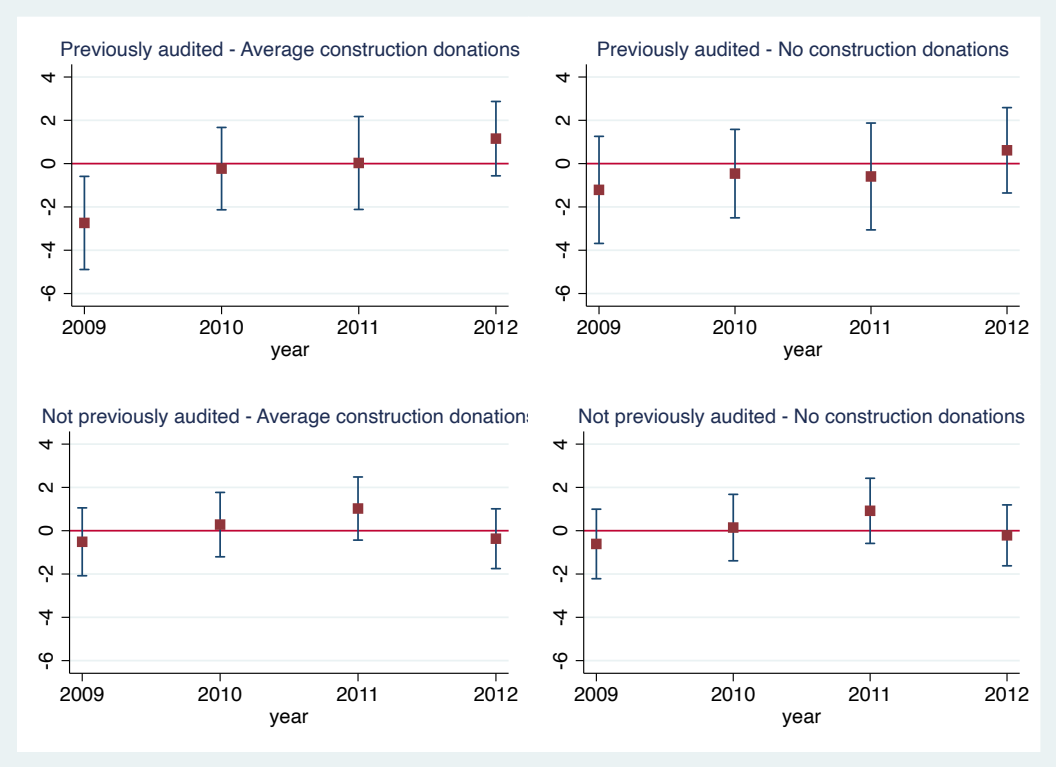
The Supreme Electoral Court (TSE) in Brazil provides data on the legal campaign donations to all politicians that run for office in Brazil. They construct a variable that describes the origin of donations: own funds from politicians; firms; individuals; and political parties. However, they do not provide sub-categories for the donor firms. I use the variable of the names of donors (which include names of people for individual contributions and names of firms for these types of donations) in order to search for most common words of firms. Using software R, I constructed a vector of keywords in order to quantify the most common donor names and to categorize donations that come from construction firms.

A.2 Additional Results

A.2.1 Inverse hyperbolic sine transformation for block grants

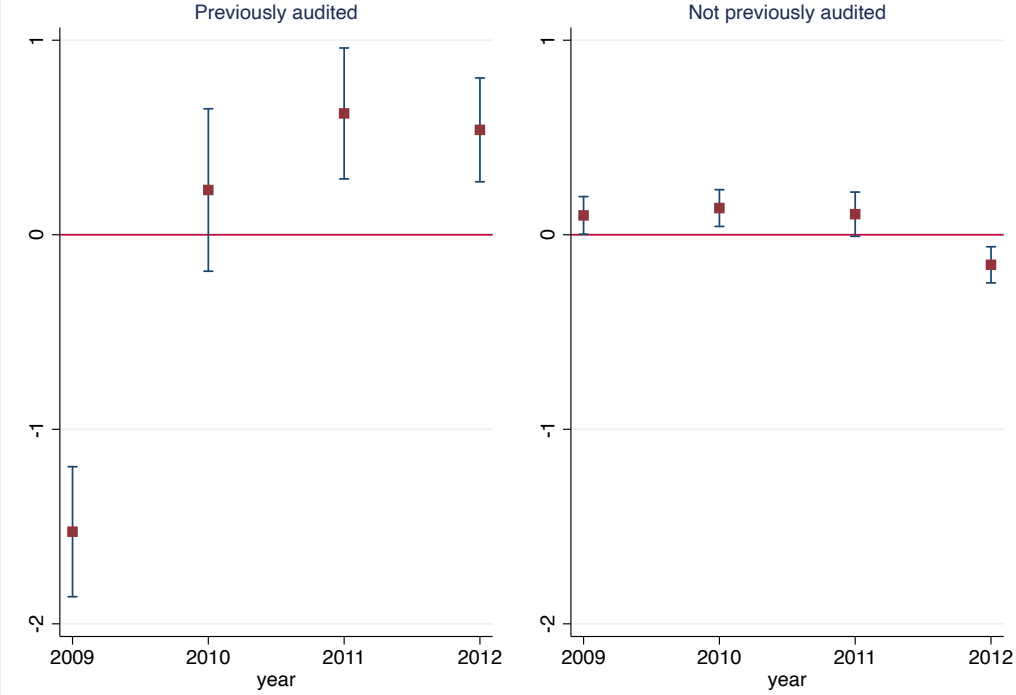
Figures A.1, A.2, A.3, and A.4 show treatment effects by groups on the intensive and extensive margin of block grants, by year. The direction of the estimates are similar to the ones for the extensive margin, just with higher magnitudes, consistent with the findings of Table 2.4.

Figure A.1: Heterogeneous treatment effects on the values of block grants directed to construction projects - by groups



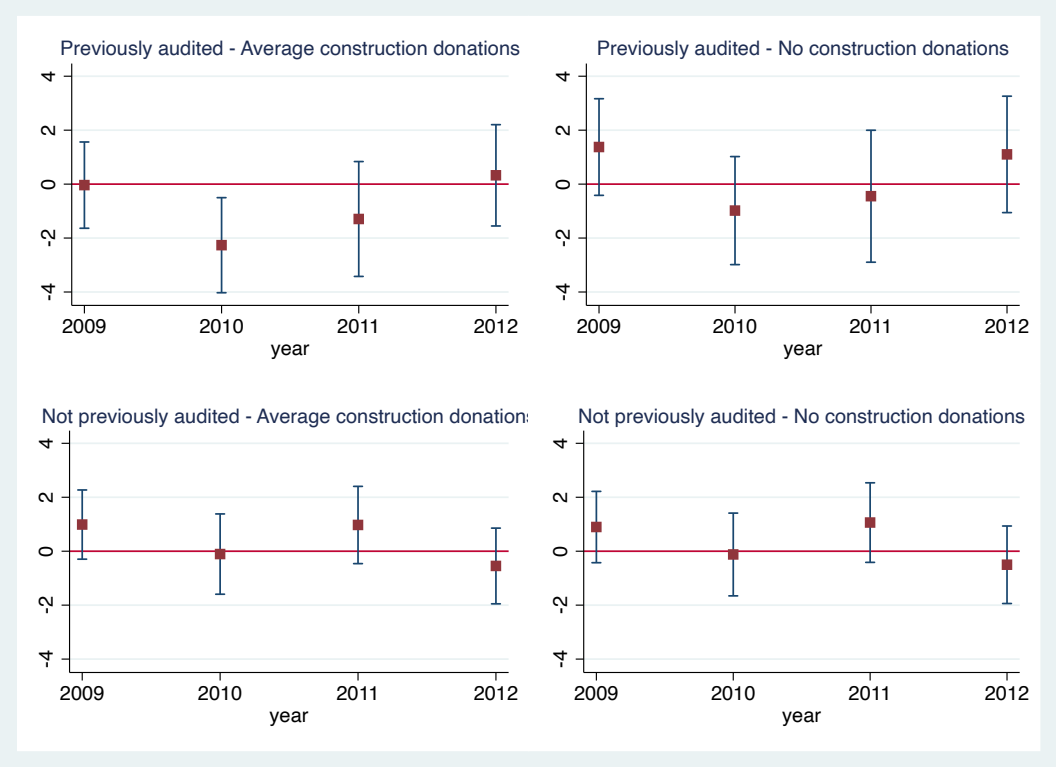
Notes: Figure A.1 reports heterogeneous treatment effects on the value of block grants the municipalities directed to construction projects in each year of the mayors' term (using the IHS functional form). The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Figure A.2: Difference treatment effects on the values of block grants directed to construction projects between municipalities financed or not financed by construction companies in 2008



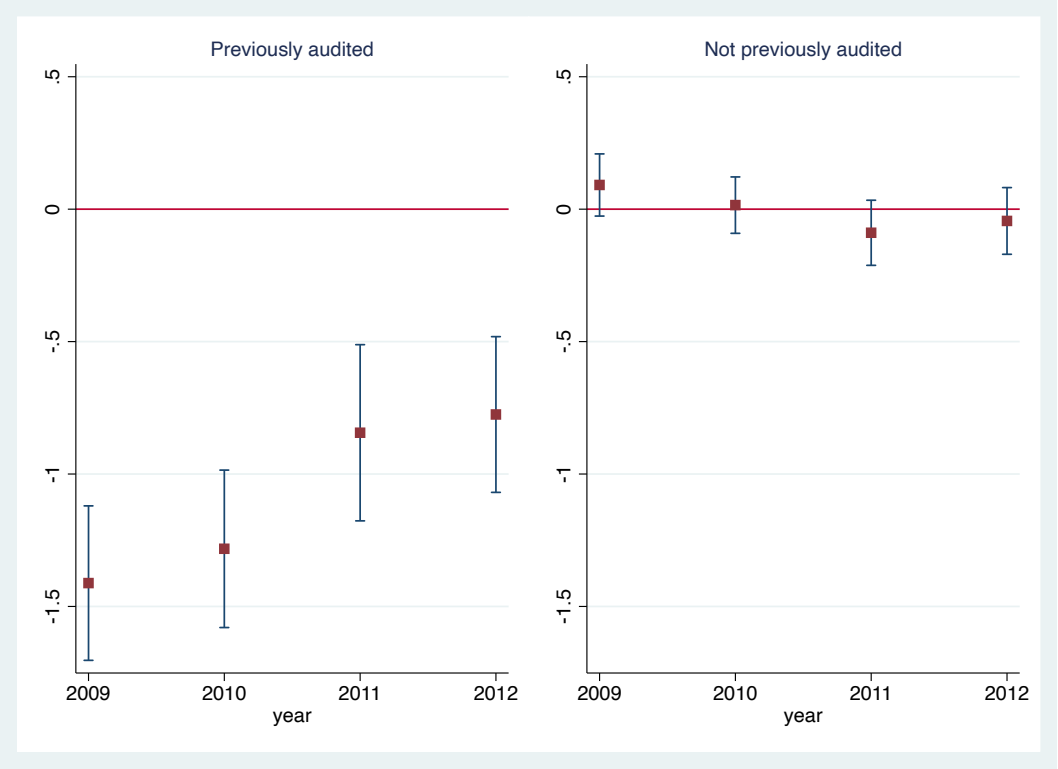
Notes: Figure A.2 reports heterogeneous treatment effects on the values of block grants directed to construction projects in each year of the mayors' term . The first (second) graph focus on municipalities that had (not) been previously audited before the experiment. Each coefficient reflects the difference in treatment effects between places where the mayors received the average value of construction donations (1.82%) and municipalities where they did not receive any dontations from construction companies. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Figure A.3: Heterogeneous treatment effects on the values of block grants directed to non-construction projects



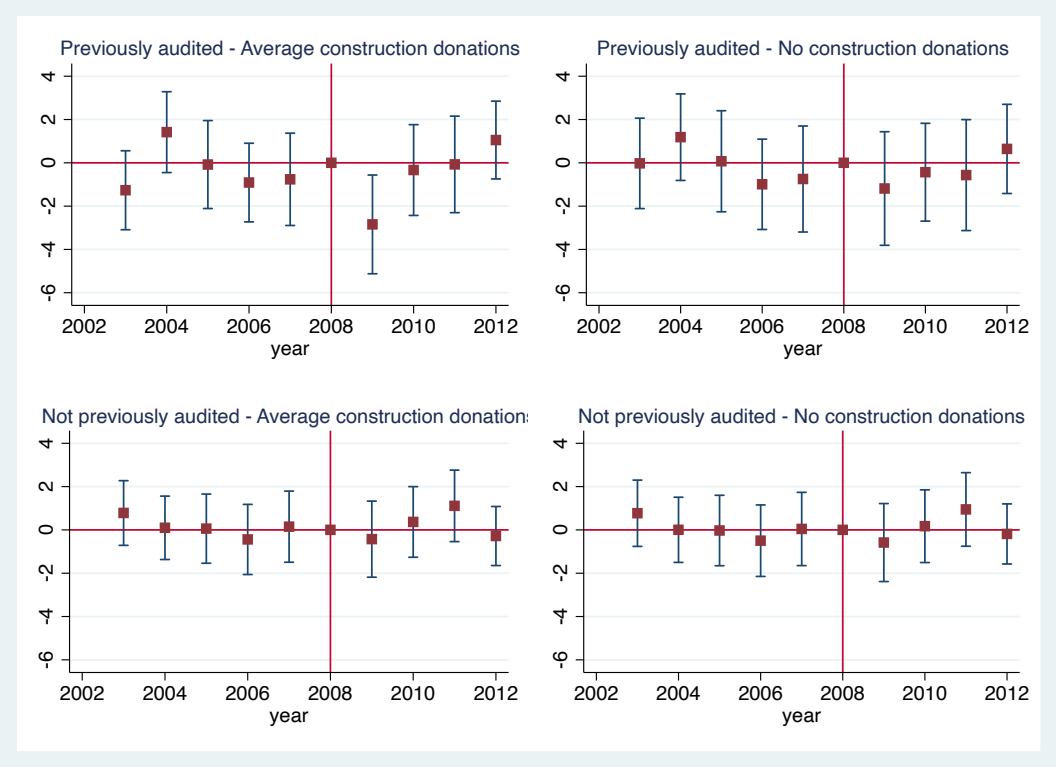
Notes: Figure A.3 reports heterogeneous treatment effects on the value of block grants the municipalities directed to non-construction projects in each year of the mayors' term (using the IHS functional form). The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Figure A.4: Difference treatment effects on the values of block grants directed to non-construction projects between municipalities financed or not financed by construction companies in 2008



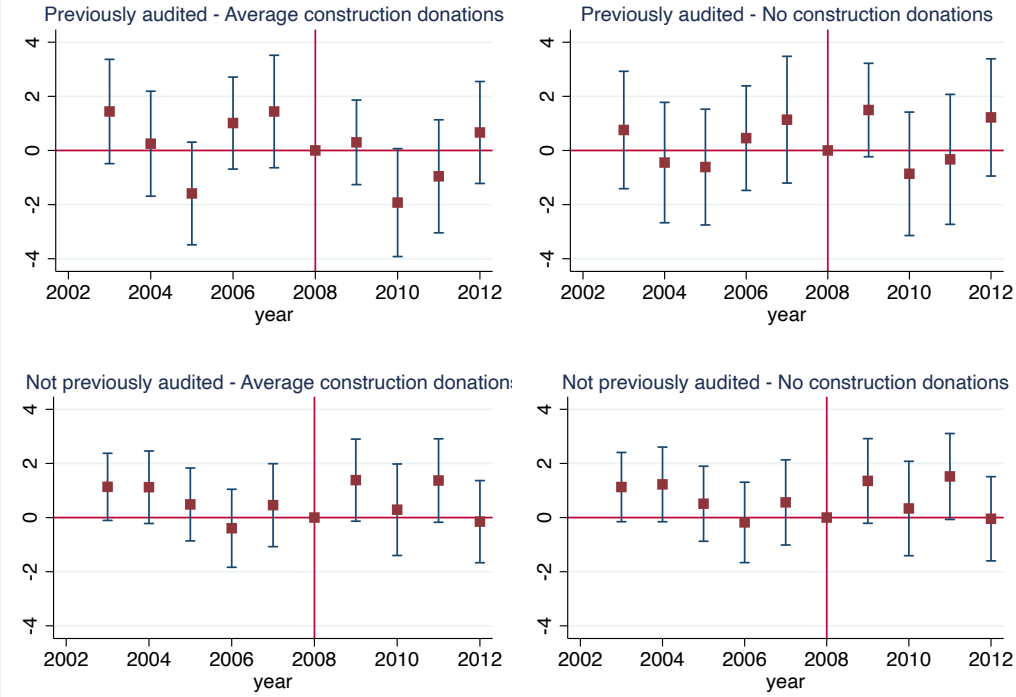
Notes: Figure A.4 reports heterogeneous treatment effects on the values of block grants directed to non-construction projects in each year of the mayors' term . The first (second) graph focus on municipalities that had (not) been previously audited before the experiment. Each coefficient reflects the difference in treatment effects between places where the mayors received the average value of construction donations (1.82%) and municipalities where they did not receive any dontations from construction companies. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Figure A.5: Falsification tests: heterogeneous treatment effects on the values of block grants directed to construction projects



Notes: Figure A.5 reports heterogeneous treatment effects on the value of block grants the municipalities directed to construction projects in each year of the mayors' term (using the IHS functional form). The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

Figure A.6: Falsification tests: heterogeneous treatment effects on the values of block grants directed to non-construction projects



Notes: Figure A.6 reports heterogeneous treatment effects on the value of block grants the municipalities directed to non-construction projects in each year of the mayors' term (using the IHS functional form). The graphs in first (second) row refer to municipalities that had (had not) previously participated in the audit program before the experiment happened, respectively. The graphs in first (second) column refer to municipalities for which the mayor received the average share (did not receive any amount) of construction donations in 2008 elections. Point estimates and 95% confidence intervals are reported for each year in the graphs.

A.2.2 Audited after experiment as heterogeneity

In this section, I present results using Equations (2.2) and (2.3), while substituting the heterogeneity A_m (previously audited municipalities) to municipalities that received audits after the experiment took place. We can see that sign of the main estimates remain the same, but magnitudes in general are smaller than before. This might be caused by the fact that effects of the letters decrease after the end of the experiment, but remain present for the municipalities in which the audits materialized.

	Probability of receiving block grants			IHS of block grants		
Dependent Variable:	Total (1)	Construction (2)	Non construction (3)	Total (4)	Construction (5)	Non construction (6)
letter×λ_{post}× audited after experiment × construction donation in p.p.	-0.019***	-0.022***	-0.012**	-0.172***	-0.193***	-0.098
	(0.004)	(0.005)	(0.005)	(0.059)	(0.066)	(0.069)
letter×λ_{post}	-0.041	0.005	-0.041	-0.483	0.003	-0.427
	(0.032)	(0.036)	(0.035)	(0.446)	(0.500)	(0.459)
letter×λ_{post}× audited after experiment	0.127**	0.092	0.170**	1.724*	1.367	2.110*
	(0.064)	(0.076)	(0.083)	(0.951)	(1.069)	(1.159)
letter×λ_{post}× construction donations in p.p.	0.001	0.002	-0.001	0.006	0.028	-0.022
	(0.002)	(0.002)	(0.001)	(0.025)	(0.024)	(0.017)
Mean of dep. variables	.805	.585	.670	978,531	601,128	377,402
Observations	67,657	67,657	67,657	67,657	67,657	67,657
R-squared	0.093	0.080	0.092	0.101	0.090	0.089
Number of municipalities	5,208	5,208	5,208	5,208	5,208	5,208

Table A.1: Robustness with heterogeneity audited after the experiment : The effects of audits threat on the block grants transferred to municipalities

Notes: Table A.1 reports the effects of the audits threat on block grants from the federal government, in a panel of Brazilian municipalities between 2000 and 2012 (Modified Equation (2.2), substituting the heterogeneity "audited before the experiment" with "audited after the experiment"). In columns (1), (2), and (3), the dependent variable is indicator of receiving any block grants by a municipalitiy in a specific year. Columns (4), (5), and (6) present the experiment's impacts on the IHS of block grants. The variable letter is an indicator for the municipalities selected to the group with higher probability of audits. The variables λ_{2009} and λ_{post} are indicators for the years 2009 and 2010,2011,2012. Coefficients with λ_{2009} reflect contemporanous effects of the experiment and λ_{post} show treatment effects after the end of the experiment. All regressions include municipality and year fixed effects. Clustered standard errors(at the municipality level) in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Total (1)	Construction firms (2)	Other firms (3)	All firms (4)	Political party (5)	Own funds (6)
letter × audited after experiment	-0.001	-0.035***	-0.001	-0.035***	0.035***	-0.062***
construction donations in p.p.	(0.001)	(0.010)	(0.008)	(0.011)	(0.011)	(0.002)
letter × audited after experiment	0.017	0.152	-0.352**	-0.201	0.101	0.158**
	(0.032)	(0.137)	(0.158)	(0.180)	(0.179)	(0.067)
letter × construction donations in p.p.	0.001	0.000	-0.004**	-0.004	-0.001	0.003**
	(0.001)	(0.005)	(0.002)	(0.005)	(0.005)	(0.002)
letter	-0.016	-0.031	0.040	0.009	-0.012	-0.046
	(0.032)	(0.039)	(0.078)	(0.076)	(0.071)	(0.057)
Mean of dep. variables	.971	.126	.428	.555	.319	.878
Observations	2,388	2,320	2,320	2,320	2,320	2,320
R-squared	0.002	0.131	0.005	0.041	0.023	0.022

Table A.2: Robustness with heterogeneity audited after the experiment : The effects of audits threat on the probability of receiving donations for mayors' reelection campaigns in 2012

Notes: Table A.2 reports OLS estimates of the effect of the letters on the probability that mayors received donations from various sources for their reelection campaigns in 2012 (Modified Equation (2.3), substituting the heterogeneity "audited before the experiment" with "audited after the experiment"). All dependent variables are indicators on whether a mayor received any financing from a specific source. Column (1) indicator refers to receiving any donations in 2012. Columns (2), (3), and (4) show effects on construction companies, non-construction companies, and all firms, respectively.Column (5) and (6) reflect impacts of the experiment in the donations from parties to mayors and mayors' own funds, respectively. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	Total (1)	Construction firms (2)	Other firms (3)	All firms (4)	Political party (5)	Own funds (6)
letter × audited after experiment	0.033	-0.378***	-0.354***	-0.427***	0.018	-0.646***
construction donations in p.p.	(0.023)	(0.091)	(0.093)	(0.105)	(0.110)	(0.042)
letter × audited after experiment	-0.449	1.306	-2.977*	-2.294	0.886	0.887
	(0.398)	(1.307)	(1.653)	(1.764)	(1.763)	(0.906)
letter × construction donations in p.p.	-0.003	-0.008	-0.051	-0.054	-0.008	0.030
	(0.007)	(0.048)	(0.032)	(0.044)	(0.051)	(0.024)
letter	-0.115	-0.253	0.325	0.308	-0.192	-0.470
	(0.208)	(0.426)	(0.795)	(0.800)	(0.690)	(0.650)
Mean of dep. variables (in R$)	150,241	7,473	29,362	36,836	18,602	38,028
Observations	2,320	2,320	2,320	2,320	2,320	2,320
R-squared	0.263	0.167	0.090	0.097	0.050	0.011

Table A.3: Robustness with heterogeneity audited after the experiment : The effects of audits threat on the donations' values for mayors' reelection campaigns in 2012

Notes: Table A.3 reports OLS estimates of the effect of the letters on the values of donations that mayors received from various sources for their reelection campaigns in 2012 ((Modified Equation (2.3), substituting the heterogeneity "audited before the experiment" with "audited after the experiment"). All dependent variables are the inverse hyperbolic sine of a source of campaign donations. Column (1) refers to all donations received in 2012. Columns (2), (3), and (4) show effects on construction companies, non-construction companies, and all firms, respectively.Columns (5) and (6) reflect impacts of the experiment in the donations from parties to mayors and mayors' own funds, respectively. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Reelection rates		Run for reelection rates
Dependent Variable	Conditional on 1st term (1)	Conditional on running (2)	Conditional on 1st term (3)
letter × audited after experiment construction donations in p.p.	-0.009	-0.020*	0.021**
	(0.008)	(0.010)	(0.009)
letter × audited after experiment	0.001	0.138	-0.137
	(0.144)	(0.184)	(0.146)
letter × construction donations in p.p.	-0.005***	-0.005***	0.002
	(0.002)	(0.002)	(0.001)
letter	-0.061	-0.117	0.058
	(0.064)	(0.075)	(0.055)
Mean of dep. variables	.393	.537	.731
Observations	3,217	2,358	3,217
R-squared	0.003	0.004	0.002

Table A.4: Robustness with heterogeneity audited after the experiment : The effect of audits threat on reelection rates in 2012 polls

Notes: Table A.4 reports OLS estimates of the effect of the letters on the reelection rates and rates of running for reelection in 2012 ((Modified Equation (2.3), substituting the heterogeneity "audited before the experiment" with "audited after the experiment"). All dependent variables are indicator variables. Columns (1) and (2) show effects on reelection rates: conditional on mayors in 1st term that could run for reelection, and conditional on mayors that chose to run for reelection, respectively. Column (3) reflect impacts of the experiment in the decision of running for reelection, for mayors that could chose to do so. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.