Politics never end: Public Employment Effects of Increased Transparency

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Abstract

Transparency that provides voters with negative information about politicians leads to electoral penalties. Do politicians strategically respond when confronted with this type of potential electoral backlash? I answer this question using exogenous variation generated by randomized audits to local governments in Brazil. I show that the execution of an audit leads to an increase in the number of public employees hired by the mayor. This effect is greater in municipalities where auditors uncovered higher levels of corruption. I find evidence consistent with mayors hiring more employees as a form of patronage to compensate for the loss of electoral support resulting from the audit. I closely examine the education sector using additional detailed data and find that hiring more school employees does not improve student outcomes, revealing limited direct consequences of audits on public goods production. Moreover, I show that an audit increases the share of payroll expenditures but decreases capital investment and that this substitution translated into a deterioration in the quality of school assets. These results suggest that patronage enables politicians to offset the potential electoral penalty of an audit by hiring employees who do not contribute much to public goods production.

Keywords: Electoral Accountability, Bureaucracy, Corruption, Education, Audits

JEL codes: D02, D72, D73, G38, H1, H41, H52, H83

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1 Introduction

Evidence suggests that transparency that reveals political malfeasance to the public negatively affects the incumbents' electoral performance (Bobonis et al., 2016; Ferraz and Finan, 2008). However, transparency may be of little value if it does not improve politicians' behavior. On the one hand, these reforms might realign dysfunctional politicians' incentives with voters' interest, pushing politicians to take corrective actions (e.g., dismissing "bad apple" employees to signal that they are not corrupt), thereby increasing welfare (Shleifer and Vishny, 1993). On the other hand, politicians might unanticipatedly use policy for their political benefit as a response to increased transparency (Lucas, 1976), as they have various means to achieve their goals. For instance, to compensate for the loss of electoral support, politicians might use policy for their political benefit and increase patronage—that is, distribute public jobs in exchange for votes (Weingrod, 1968).

In this paper, I ask: do politicians strategically respond to potential electoral backlash from political transparency? Studying politicians' responses to transparency has important policy implications, as creating better institutional incentives in one area might lead to a deterioration in another (Holmstrom and Milgrom, 1991). For example, if politicians increase patronage without considering the quality of the new hires, there may end up being fewer qualified people in the bureaucracy. Perhaps one of the most influential papers on how transparency leads to better outcomes is the work by Ferraz and Finan (2008). Using data from the first few waves of a new government audit program in Brazil, the authors found that disclosing corruption information to the public very close to the election enhanced electoral accountability by reducing the likelihood of reelection of corrupt mayors. In the current paper, I go beyond this by looking at public employment outcomes to examine how the politicians respond to the audit. In particular, I examine the same reform as Ferraz and Finan, but over a longer span and four full mayoral terms. Later waves occur at different points throughout the political cycle, during which time the incumbent can respond strategically before the next election. With this design, I study whether politicians change their political strategy by increasing the number of hires to achieve their reelection goal when faced with increased transparency. So far, there is little empirical evidence on whether politicians use policy for their political benefit as a strategic response to increased transparency.

I provide two main contributions. First, I show that, faced with an audit, which generates electoral backlash (Ferraz and Finan, 2008), the incumbent increases his patronage engagement to compensate for the loss of electoral support. This increase in patronage can be interpreted as "forced action" pushed by the audit. This first contribution is consistent with theoretical predictions (Acemoglu et al., 2008) as I show that politicians invest in different patronage avenues when facing new policy constraints. Second, I provide evidence that even though the audit increases the number of public employees, they do not improve public goods production. My paper is the first study that examines the downstream consequences of anti-corruption audits on public education. The evidence I present is in line with arguments made by Stigler (1971), which predict that a policy reform that induces the policymaker to substitute means to achieve the same goal (i.e., reelection) would reduce efficiency.[1] In my paper, I provide evidence on such efficiency reduction by showing that after an audit, politicians hire more employees who do not contribute much to public goods production.

In 2003, the Brazilian federal government launched an anti-corruption program designed to detect and punish irregularities in municipal expenditures. The program selects municipalities at random and audits their expenditures of federally transferred funds from previous years. My primary analysis employs a staggered difference-in-differences design focusing on how an audit changes public employment under the jurisdiction of the mayor. I compare employment outcomes in a municipality that was randomly drawn to be audited to those in eligible municipalities that were either never audited or were randomly chosen to be audited in later years. I confirm this design's validity, showing that a range of local economic, demographic, and public labor characteristics cannot predict which municipalities get audited.

Based on these analyses, I provide three main findings. First, examining the contracts of employees working for the municipal government across all sectors, I show that an audit leads to a larger number of hires of public employees. The effect on hires is about 15%, and it is greater for employees with temporary work contracts, which is where the mayor has considerable discretion. Importantly, I verify that the number of hires outstrips dismissals, resulting in a net rise in employees. The effects on hires are robust to recent advances in differences-in-differences method. Focusing on education and using identified data on the universe of school employees (consisting of teachers and teacher aides), I show that an audit increases the number of new school employees, mostly inexperienced ones. Municipal governments play a crucial role in this sector, given that they manage most primary schools and spend one-third of their budget on them. In addition, one-fourth of municipal employees work in this sector. Moreover, I find that the effect on hires in the municipal bureaucracy and on new school employees is greater among municipalities in which an audit uncovered higher corruption levels.

Second, I investigate two possible mechanisms that could explain why an audit leads to additional hires. One mechanism is that the corruption revealed by an audit decreases

[1] Coate and Morris (1995) formalized a model in which a policy reform will induce the policymaker to choose a less efficient transfer mechanism.

mayors' reelection chances, leading mayors to increase their patronage engagement to compensate for the loss of electoral support. Another alternative is that the information revealed by an audit improves the ability of mayors to take corrective actions (i.e., dismissing perceived "bad apples" working in the municipality), thereby allowing the mayor to hire new employees to replace the dismissed ones. I find evidence consistent with the first mechanism: Mayors increase the number of hires not as a form of corrective action, but rather as political leverage to shore up their reelection chances. Consistent with this interpretation, I show that the effect on hires is larger in the presence of electoral incentives, such as prospects for reelection, where there are greater gains from improving popularity. To do this, I compare the treatment effect from municipalities where the mayor was audited in its first term with those audited in its second term (who face a term limit). In addition, I examine whether the release of audit results differentially changes the reelection rates for a mayor who hired more or fewer employees during his term. I provide evidence showing that hiring additional employees offsets the negative effect of an audit on mayoral reelection rates.

To investigate the second mechanism, I exploit the audit data's richness from almost 800 audit reports and codify whether the schools located in an audited municipality were linked to irregularities or not. This data allow me to test directly whether additional hires occur in the specific municipal public schools where auditors uncovered irregularities. I observe greater and statistically significant positive effects on hires among schools where auditors did not uncover irregularities located in corrupt municipalities. This evidence rules out the idea that mayors are increasing hires to take corrective actions.

I go on to show that alternative explanations unrelated to patronage are also unlikely to account for these findings. I start by considering explanations that would directly affect hires (e.g., political turnover, resignations after an audit). Then, I consider explanations related to changes in the public finances of the municipality (e.g., larger inflows of transfers following an audit). In total, I rule out six alternative explanations.

Third, I examine the effects of an audit on public goods outcomes, in particular, the quality of education. To do this, I first use data on students in municipal public schools in Brazil. I do not observe improvements in students' educational performance, as measured by grade repetition, dropout rates, and standardized test scores, suggesting that hiring additional school employees does not improve educational outcomes. Moreover, I show that an audit increases the share of payroll expenditures but decreases capital investment and that this substitution translated into a deterioration in the quality of school assets. This implies that the direct consequences of audits on public goods production are limited, thereby reinforcing the idea that patronage is the mechanism explaining the effect on hires. In particular, the lack of improvement in public goods rules out the possibility that better public goods, rather than the patronage itself, explain the gains in reelection rates for hiring more employees.

This paper relates and contributes to four main areas of literature. First, it adds to the vast body of work studying transparency and political accountability.[2] Previous research suggests that voters punish politicians in the polls after adverse electoral shocks, such as political scandals (Hirano and Snyder Jr., 2012) and corruption disclosure (Bobonis et al., 2016; Buntaine et al., 2018; Dunning et al., 2019; Ferraz and Finan, 2008). Using the same reform examined in this paper, Ferraz and Finan (2008) show that audits conducted close to elections improve accountability by reducing the election of corrupt politicians. However, my work shows that the picture is more complicated because some politicians can respond strategically and offset the potential electoral penalty, specifically by hiring employees who do not contribute much to public goods production.

The research on how politicians respond to negative electoral shocks is largely theoretical and focuses on two areas. On the one hand, it addresses politicians experimenting with risky policies as a gamble for resurrecting their electoral chances (Izzo, 2020; Majumdar and Mukand, 2004). On the other hand, it addresses the choice of the politician communicating or not with the public when allegations initially surface (Basinger and Rottinghaus, 2012). Empirically, Poblete-Cazenave (2021) examines an alternative margin of politician's responses, campaign spending. Instead, I focus on a different margin of response: politicians distributing public jobs in exchange for votes to compensate for the loss of electoral support. In this regard, my results lend empirical support to models showing that, under certain conditions, politicians will turn to authoritarian practices or coercion when faced with threats to their political survival (Bueno de Mesquita and Smith, 2010; Fergusson et al., 2018).

Second, this study contributes to the literature on the personnel economics of the state. Previous research has focused mainly on financial incentives on the applicant pool, recruitment, and screening, and on incentives to improve performance (Finan et al., 2017). This paper links this literature with those related to corruption and electoral accountability.[3] My work highlights the role of public employee hires as the politician's strategic response when confronted with electoral backlash. It also suggests that patronage is a politically successful tool, as it effectively offsets the electoral penalty of disclosing corrupt acts from an audit on reelection rates. My results are consistent with other research showing that governments

[2]For a recent overview on the literature of political accountability, see Ashworth (2012). For a literature review on political scandals, see Invernizzi (2016).

[3]In a recent paper, Lauletta et al. (2020) examines Brazilian audits from 2003–2004 to study the link between audits and quality of government. In contrast to their findings, I show that an audit leads to inflation in the size of the bureaucracy. This difference arises from differences in the dataset on labor outcomes, in time frame examined, and in research design used.

use jobs for patronage and redistribution purposes (Alesina et al., 2000; Brollo et al., 2017; Colonnelli et al., 2020; Robinson and Verdier, 2013).

Third, my findings fill a gap in the literature on the determinants of quality in public services. While past work implies that audits enhance welfare Avis et al. (2018), the evidence on the causal effect of anti-corruption audits on downstream consequences, and in particular quality of public goods, is limited and concentrated on public health (Dizon-Ross et al., 2017; Zamboni and Litschig, 2018) and roads (Olken, 2007).[4] I contribute to this literature by showing that audits do not improve student outcomes. My results also speak to the literature connecting public services and turnover. In particular, my findings contrast with those of Akhtari et al. (2020), who found that municipalities with a new party in office experience higher turnover in schools and lower student test scores. One reason for this difference is that the present study shows that an audit leads to an inflation of the number of school employees and not simply a "reshuffling" of employees across schools. Thus, even if there is a negative effect on test scores due to turnover, it might be compensated for by the additional employees.

Lastly, my paper adds to the literature on the design of policies to deter corruption. Some of these studies show that audits are effective in reducing future corruption (Avis et al., 2018; Olken, 2007), but are uninformative about what happens to the bureaucrats linked to corrupt acts. I add to this body of work by showing that mayors do not dismiss "bad apple" employees after the audit. Instead, they hire more employees in exchange for votes, undermining the audits' effectiveness. Related emerging literature shows that audits can backfire (e.g., Gerardino et al., 2020).[5] Chong et al. (2015) show that providing voters with corruption information disengages them from the political process. In my setting, I show that audits backfire as they lead to a larger number of employees with no corresponding improvements in public goods. I also find that anti-corruption audits backfire regarding electoral accountability: Hiring additional employees offsets the negative effect of disclosing corruption information from an audit on mayoral reelection rates. Overall, my results provide empirical evidence of a seesaw effect (Acemoglu et al., 2008), demonstrating how an effective reform in one dimension may lead to a deterioration in others.

[4] Ferraz et al. (2012) find a negative association between corruption detected in audits and educational outcomes. (Colonnelli and Prem, 2021) focus on private sector outcomes and find that anti-corruption audits increase the number of firms concentrated in sectors that heavily rely on public procurement, such as manufacturing and transportation. According to their classification, private education is one of the least government-dependent sectors.

[5] For more literature related to audits, see Duflo et al. (2013); Gonzalez-Lira and Mobarak (2019); Lichand and Fernandes (2019); Wong (2021).

2 Institutional Background

This section provides relevant details about municipal governments and municipal elections in Brazil. It describes the different types of public sector workers under municipality jurisdiction and discusses differences in their selection and exit processes. It also delineates the structure of public education. Finally, it describes the randomized anti-corruption audits of municipal expenditures.

2.1 Municipal Bureaucracy

Public employees represented 16% of the workforce in Brazil in 2015 (more than 70 million employees), and more than 50% of public employees worked under municipality jurisdiction. A mayor (Prefeito) and a council of local legislators (Camara de Vereadores) govern municipalities. The mayor and legislators are simultaneously elected every four years. Mayors face a two-term limit.[6]

There are two main types of municipal public employees: tenure-track and temporary.[7] Among municipal public employees in 2015, 72% were employees with tenure-track, while 28% were temporary employees.[8] Selection and exit processes, as well as the type of work contract, vary widely among these types of workers. Selection of either type of municipal public employee depends exclusively on the corresponding municipality authority (e.g., director), with no state or federal government influence).

After three years of service, tenure-track employees get tenure, which provides job security. These employees are selected based on objective criteria (i.e., academic and professional credentials and a job-specific entrance exam). The selection process (*Concurso Publico*) starts with a public announcement of the required job, along with a job description, salary, and academic requirements. It ends with a public statement listing all candidates, selected candidates, and examination scores. Depending on the number of listed jobs, the process can last from a few months to a year.[9] Among the hires municipal public employees, only 34% were tenure-track employees in 2015. Dismissals can occur only after a judicial ruling

[6]Mayors can hold office for only two consecutive terms. However, they can return after a one-term hiatus or run for higher offices. In practice, it is very uncommon (Ferraz and Finan, 2011).

[7]This does not include elected positions (e.g., mayor). It is also possible to hire employees through a private company (e.g., janitors). These employees are not in the current sample because they work for private companies and are neither employed nor dismissed by the municipal authorities.

[8]The share of temporary employees among the final sample of municipalities is lower, about 19%.

[9]For example, in October 2011, the municipality of *Camalaú* started a Concurso Publico to fill a position for a doctor (Edital PMC 001/2011). The process ended in December 2011. A much larger process occurred in the municipality of *Damião* in May 2019, when a Concurso Publico was put in place to fill 35 positions across 22 occupations (Edital 001/2019). By January 2020, most jobs were filled, although some of them required extra time).

and only for a limited number of reasons (e.g., misconduct).

Second, the municipal government has the discretion to select and hire temporary employees directly and without examination. Moreover, the exit process is fast and does not require judicial ruling nor oversight. Temporary jobs are usually exclusive to positions of managers, directors (e.g., school principals), or advisors. However, they can also be filled by health and education staff and other occupations when the municipality determines the job "meets a temporary need of exceptional public interest" (Article 37 of the Brazilian Constitution). Even though the need for the position should be temporary in theory, in reality, there are no limits on employment length. Among the hires of municipal public employees, 66% are temporary. Similarly, among municipal public employees who left their positions, 67% were temporary.

2.2 Public Education

Municipalities are responsible for providing public services such as primary education, health care, and sanitation. In 2015 32% of municipalities' expenditures was allocated to education and 24% to health. The rest was distributed among other sectors.

Municipalities manage most primary schools (*ensino fundamental*, first to eighth grade), while state governments manage most secondary schools. By 2015, municipal governments managed 66% of primary schools, while state governments managed 68% of secondary schools.[10] Municipalities are responsible for the infrastructure and operations of the school: distributing school lunches, providing school transportation, hiring, paying salaries, and training of teachers. However, most funding for public education comes from the federal government (Akhtari et al., 2020) in the form of block grants based on student enrollment (fund called FUNDEF/FUNDEB).[11]

In 2015, 24% of municipal employees worked in the education sector. Schools' payrolls include a principal, teachers, and support staff (e.g., teacher aides). Most principals are temporary employees, directly appointed by mayors. Among municipal public schools in 2015, 82% of employees were teachers, while 18% were teacher aides. 74% of teachers were employees with tenure-track, while 26% were temporary employees. Support staff is temporary.

[10]Private entities managed about 16% of primary schools.

[11]At least 60% of FUNDEF revenues must be spent on teachers' salaries (Ferraz et al., 2012).

2.3 Randomized Anti-corruption Audits

In 2003, the federal government created an autonomous anti-corruption office, the Office of Comptroller-General (CGU). In the same year it was formed, the CGU launched an anti-corruption program (*Programa de Fiscalização por Sorteios Públicos (FPSP)*) designed to detect and punish any irregularities in municipal expenditures. The program selects municipalities at random and audits their expenses from federally transferred funds during previous years.[12] The program consisted of 40 rounds of randomized lotteries over the 2003–2015 period, each of which was independent, meaning that a municipality could be drawn more than once (see Appendix Figure A3). In each round, about 60 municipalities were randomly selected. The number of rounds per year varied significantly over time. Appendix Figure A1 shows the number of municipalities audited at least once over time and illustrates the annual variation of the program.[13] During this period, 40% of municipalities were audited at least once (see Appendix Figure A2).

The CGU gathers information on all federal funds transferred to every selected municipality during the previous three to four years and issues a random selection of inspection orders (e.g., school construction). Each order stipulates an audit task for a specific project within a specific sector. These orders focus on financial management and documentation, the procurement process, wages, and quality of service funded by the transfer. After that process, about ten auditors are sent to the municipality for one or two weeks to examine and verify the information in those inspection orders. Auditors perform interviews, review documentation, visit hospitals or schools, among other activities.[14] The time between the lottery announcement and the audit execution can be anywhere from one month to a year. On average, auditors visit the municipality one month after the announcement. After the completion of inspections, auditors submit a full report to the central CGU office in Brasília. A summary of each audited municipality's main findings is released to the media and published on the Internet. The federal police, prosecutors (MPF), and the federal courts of accounts (TCU) also receive this summary, resulting in legal consequences as warranted. On average, the summary is published seven months after the audit announcement.

[12] All municipalities with a population of up to 500,000 inhabitants are eligible for selection. The program randomly selects a fixed number of municipalities per state in each lottery. For smaller states, only one or two municipalities are selected in a single lottery, while for larger states, several municipalities are selected (up to seven in the recent lotteries).

[13] The program has changed in other regards over time. From August 2005 (17th lottery), the CGU targeted a limited number of randomly selected sectors in municipalities with larger populations, while smaller municipalities were still subject to audits in all sectors.

[14] Auditors are hired based on a competitive public examination and earn highly competitive salaries (Avis et al., 2018).

3 Empirical Strategy

In this section, I present the data sources and research design. I leave the description of the complementary strategy for the study of the mechanism of hiring to correct irregularities for the following section 5.

3.1 Data

This paper uses municipal employees, anti-corruption audits, education, and election data from different sources. I present summary statistics for all eligible municipalities and audited municipalities in Appendix Table A1.

Municipal Bureaucracy. The main source of data for employees' movements across the entire municipal bureaucracy is *Relação Anual de Informações Sociais* (RAIS), which is managed by the Ministry of Economy (*Ministério da Economia*). It is an annual survey that reports administrative information on the universe of labor contracts in both the public and formal private sectors. This survey is used for various social insurance programs in Brazil; there are consequences if misreporting occurs(Akhtari et al., 2020). It includes the worker's demographic characteristics, such as education level, gender, age, and whether they left employment voluntarily or were dismissed. It also includes information on the type of contract (i.e., temporary or tenure-track), hiring date, and end date of employment. Finally, in RAIS, each worker is assigned an occupational category specific to their current job (according to the Brazilian classification of occupations 2002 CBO). I use annual data from 2000 to 2015 to build a panel of employment outcomes by municipal government (employer). I use annual data from 2000 to 2015 to build a panel of employment outcomes by municipal government (employer). One limitation of this dataset is that it is anonymized, which impedes tracking the same employee overtime.

Education. The data on education come from several sources. First, I use the School Census (*Censo Escolar*) dataset from the National Institute for Research on Education *(*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira, INEP). It is an annual survey of the universe of public schools in Brazil. The census is conducted in May and is available from 2007 (the academic year begins in March and ends in December). This census includes characteristics of the school, enrollment, student characteristics, and a panel of school employees with individual and school identifiers. I use this panel to compute the movement of individual school employees. Specifically, I compute the number of new employees to a school by taking the pool of employees in a given school year and checking to see if those same employees were present at the same school the year prior. I compute the number of employees that left a school by taking the pool of employees in a given school year and checking to see if those same employees were present in the same school the next year. The data do not provide information regarding whether employees who left a school did so voluntarily or were dismissed or transferred. I build a panel of school-level movement of employees from 2008 to 2015. Second, I use *Prova Brasil*, a nationwide, standardized exam administered biannually in November since 2007 to all 4th and 8th graders in public schools. [15] I use *Prova Brasil* data from 2007 to 2015 to measure student achievement. To facilitate interpretation, I standardize math and Portuguese language test scores according to the individual level distribution of test scores for students in eligible but never-audited municipalities.[16] Finally, I use school-level data on dropout rates from the National Institute for Educational Research and Study and Executive Secretary (INEP).

Audits. The data on irregularities and audits come from the CGU. The data from lotteries 2–40[17] include the audited municipalities and the number of inspection orders for each municipality and lottery. Although all audit reports are public, the CGU only began to code the information from the reports starting with the 20th lottery in March 2006. For the most recent 20 lotteries, I obtain a description of detected irregularities by inspection order; the total number of detected irregularities is 122,397.

Following the CGU split between minor and more severe irregularities and the language of Avis et al. (2018), some examples of detected corruption include the following: fraud in the selection of beneficiaries of welfare programs (above the limit on household income), over-invoicing of goods and services, abandonment of public works by the contractor (without application of contractual sanctions), and fraud in the procurement of goods and services (e.g., not advertising the request for the work).[18] Appendix Figure A4 shows the share of audited inspection orders with at least one detected irregularity categorized as corruption over time.

Municipal Elections. The electoral data come from the Tribunal Superior Eleitoral (TSE). This dataset provides public records on election results in municipal elections in 2000, 2004, 2008, 2012, and 2016. Elections are held every four years in October, with the mayor taking office in January of the following year.

On this point, while Ferraz and Finan (2008) examine the electoral outcomes of the 2004 mayoral election, I use data from audits that occur during four full mayoral terms to study

[15] *Prova Brasil* is administered only in schools with at least 20 students enrolled in that particular grade level.

[16] CGU can perform audits outside the scope of FPSP that are not considered in this work.

[17] Lottery 1 was a pilot.

[18] Some examples of detected mismanagement are inconsistencies on the registered information of beneficiaries of welfare programs (e.g., not located at registered addresses), lack of documentation (e.g., absence of records of incoming stock on hospitals' warehouses), and keeping expired inventory (e.g., expired medicine in hospitals).

whether the mayor responds strategically when provided with more time before the next election.

Other Data. Socioeconomic and demographic data at the municipality level correspond to the 2000 census by the Brazilian Institute of Geography and Statistics (IBGE). I also uses municipal-level public finance data related to sectorial expenses and federal transfers drawn from Institute for Applied Economic Research (IPEA).

3.2 Research Design

I aim to estimate the causal effect of the random audits on municipal public employment. For this purpose, I examine exogenous variation resulting from anti-corruption audits that were conducted randomly across municipalities over time. The nature of this randomization lends itself naturally to a staggered difference-in-difference analysis with municipality and year fixed effects.

Even though previous work has strongly established the validity of audits' randomization (Colonnelli and Prem, 2021; Ferraz and Finan, 2008,1; Zamboni and Litschig, 2018), I also test this in the data. Appendix Table B1 shows the results of cross-sectional regressions where the dependent variable, an indicator of whether the municipality was audited between 2003 and 2015, regressed on state fixed effects and a set of local economic and demographic characteristics.[19] Based on observable characteristics, audited municipalities are similar to never-audited municipalities. The test for joint orthogonality shows that the coefficients are not statistically significant.

I estimate the following flexible event-study model:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{y=-4}^{-2} \eta_y D_{i,t}^y + \sum_{y=0}^{5} \beta_y D_{i,t}^y + \epsilon_{i,t} \tag{1}$$

Here, $Y_{i,t}$ is the outcome variable in municipality i at year t, α_i is a municipality fixed effect, and λ_t is a year fixed effect. $D_{i,t}^y$ is an indicator variable taking the value of 1 if it is year y relative to the audit announcement year in municipality i. These indicator variables are always 0 for eligible but never-audited municipalities. I normalize $\eta_{-1} = 0$, so all other coefficients η_y and β_y represent differences in outcomes relative to the year prior to the audit announcement. Observations more than three years before or more than four years after the audit announcement are represented by dummy variables $D_{i,t}^{-4} = \mathbb{1}(t - T_i^* \leq -4)$ and $D_{i,t}^{5} = \mathbb{1}(t - T_i^* \geq 5)$, respectively. The set of coefficients β_y represents the average difference

[19]The implied audit probability in any given round is constant within a state

in the outcomes between audited municipalities and non-audited municipalities relative to what that difference was before the audit announcement, conditional on the set of fixed effects.

I cluster the error term $\epsilon_{i,t}$ at the municipality level to allow for arbitrary correlation within municipalities.

I remove the few municipalities that were ineligible for the program. Then, I remove those municipalities without employment data for each year within the studied period. Finally, I exclude municipalities that were audited but for which I do not observe a full window of event-years within -3 to 4. That results in a set of treated (at different points in time) and never-treated municipalities. The final sample using municipality-level data includes a balanced panel of 4,057 eligible municipalities over the 2000–2015 period, consisting of 73% of all the eligible municipalities.

Appendix G provides more details on the research design when studying heterogeneous effects and summarizing results.

School Employees. In addition to the primary municipality-level analysis, I also conduct a school-level analysis by adapting the same empirical design to the more disaggregated dataset from the School Census. I use the following specification:

$$Y_{s,t} = \alpha_s + \lambda_t + \eta D_{i,t}^{-2} + \sum_{y=0}^{3} \beta_y D_{i,t}^{y} + \epsilon_{s,i,t} \tag{2}$$

$Y_{s,t}$ is the outcome variable (new and separated school employees). α_s, and λ_t are school and year fixed effects, respectively. The rest of the variables are analogous to those in equation (1). Observations more than two years before or more than three years after the audit are represented by dummy variables $D_{i,t}^{-2} = \mathbb{1}(t - T_i^* \leq -2)$ and $D_{i,t}^{3} = \mathbb{1}(t - T_i^* \geq 3)$, respectively. $\epsilon_{s,i,t}$ is an error term clustered at the municipality level.

The endpoints are different from the main specification since I only observe individual school employees' movements from 2008 to 2015. I remove the few municipalities that were ineligible for the program and schools without outcome data for each year within the studied period.[20]

Appendix G provides more details on the research design.

[20]The DD's specification is the following:

$$Y_{s,t} = \beta \text{PostAudited}_{i,t} + \alpha_s + \lambda_t + \epsilon_{s,i,t} \tag{3}$$

4 Results: Municipal Employment

In this section, I present evidence on how audits affect hires of the entire municipal bureaucracy. Next, using the more disaggregated data at the school level, I provide an estimated effect of audits on municipal public school teachers and principals. I then evaluate the heterogeneity of hires by detected levels of corruption in the municipality.

4.1 Municipal Bureaucracy

Figure 1 shows the causal effect of the audits on hires. The triangle markers show point estimates of log hires relative to the year before an audit along with 95% confidence intervals of β_c in equation (1). The estimates for prior to the audit are small and statistically insignificant (p-value for a joint hypothesis test of 0.8992). The estimates for after the audit show that audits lead to a persistent increase in hires, starting from one year after the audit announcement. As previously mentioned, on average, auditors visit municipalities one month after the announcement and a summary of findings is published about six months after. Therefore, it is not surprising that the effect on hires manifests later. The audits led to an increase in hires of 9% for years 1–2 and of 11% for years 3–4. The estimates are precise and statistically significant.

Table 1 presents estimates of equation (6) using log hires as the dependent variable. The results show that audits lead to more hires for all types of contracts and positions, with estimates (column 1) showing an increase in hires of 15.5%. As discussed earlier, temporary employees face shorter and more discretionary selection processes than do tenure-track employees. Therefore, it is reasonable to assume that the estimates for hires would be greater for temporary employees. To test this assumption, I change the dependent variable to hires of workers with temporary and tenure-track contracts (columns 2 and 3, respectively). The effect of an audit on temporary hires (15.8%) is twice as large as that for tenure-track (8.8%). To compare the effects across managerial positions (e.g., directors) and non-managerial positions (e.g., teachers), I change the dependent variable to hires by occupation (columns 4 and 5, respectively). The effect on hires for non-managerial positions (15.5%) was higher than that for managerial ones (10%). All coefficients are statistically significant at the 5% level.

Appendix Table C1 displays the results of several robustness tests on the effect of audits on hires. The point estimates are similar and still significant when I use Callaway and Sant'Anna (2020b) estimator (column 1), observations with a positive number of hires (column 2), and first-time hires (column 3). Estimates are also large and significant when I change the dependent variable to the natural logarithm of hires (column 4) or the number of hires in levels (column 5).[21]

Finally, the results in Appendix Table C3 show that the number of hires outstrips the number of dismissals, resulting in a net rise in employees. Column 1 shows that audits also led to an increase in dismissals. It is important to note that dismissals correspond to employees who the municipality government fires. The point estimate for dismissals (9.4% increase) is smaller than that for hires (15.5%). Column 2 shows a point estimate of 3.7% for the number of employees at the end of the year. These data show that the audits inflated the bureaucracy's overall size.

4.2 Municipal Public School Employees

This subsection presents results showing how audits affect hires of school employees. For these analyses, I use granular data on the universe of school employees in Brazil. This data provides two advantages in comparison with the previous one. First, the data include individual and school identifiers, which allows me to conduct a more precise calculation on the number of new employees and a richer analysis at the school-level. Second, this data can be connected with student outcomes, such as test scores. Education is a substantial sector for municipal government, receiving 32% of expenditures and housing 24% of municipal employees in 2015. It also represents a challenge in the fight against corruption, with 29% of detected corruption being linked to education.

School Employees. Figure 2 shows the effect of audits on new school employees, consisting of teachers and teacher aides. The markers show the new school employees relative to the year before the audit (omitted year). The data show a significant and persistent increase (annual average of 4.7%) in the number of new employees following the audit.

Appendix Table E1 presents estimates of equation (3) and several robustness tests on the effect of audits on new school employees. The estimate in column 1 shows that an audit leads to a 6.5% increase in the number new school employees. The point estimates are similar and still significant when I use the Callaway and Sant'Anna (2020b) estimator (column 2), excluded observations with zero new employees (column 3), and excluded schools without student test scores results (column 4). Estimates are also significant and positive when I change the dependent variable to the natural logarithm of the number of new employees (column 5) or the number of new employees in levels (column 6). As a placebo test, column 7 shows that the audits have an effect close to zero on new employees in state and federal

[21] Appendix Table C2 shows the DD estimates (Goodman-Bacon, 2021). It is worth noting that less than 10% of the identifying variation is a result of the treatment timing. Rather, the majority of variation comes from comparisons to municipalities with no audits during the sample period. The comparison of later-treated municipalities in hires is negative, on average, and accounts for a bias in the overall DD estimate (however, the weight is minimal).

schools that are not audited by CGU. Changes in employee payroll seem more plausible in public schools managed by municipalities directly affected by the audits.

The results in Appendix Table E2 show that the number of new employees outstrips the number of separated employees, resulting in a net rise in school employees. Column 1 shows a point estimate for dismissals of 2.2% increase, which is smaller than that for hires (6.5%). Column 2 shows a point estimate of 3.1% for the number of employees per school. Lastly, using *RAIS* data from 2000-2015, I show that the effect of the audits on the aggregated number of new school employees at the municipal level is 9% (column 3). This result shows that the observed effect of a net rise in school employees cannot be explained by transfers or "reshuffling" of employees across schools but instead corresponds to inflation of the education bureaucracy's overall size.

School Principals. Appendix Table E3 shows estimates of the effect of audits on school principals. Audits have a small effect on the share of new principals (column 1), but a negative and significant effect of -4.1 percentage points (pp) in the share of tenure-track principals (column 2). This result suggests greater direct contracting for principals after the audit.

4.3 Heterogeneity by Detected Levels of Corruption in Municipalities

In this subsection, I present results of the evaluation of the heterogeneity of the effect on hires by detected levels of corruption in the municipality. I estimate equation (5). The group segmentation is by the level of detected corruption. High corruption municipalities correspond to those whose share of audited inspection orders with irregularities labeled as corruption is higher than the median. [22]

Figure 3 shows point estimates for log hires relative to the year before an audit along with the 95% confidence intervals of $\tilde{\beta}_y^k$ in equation (5). The estimates for before the audit were small and statistically insignificant for both groups, but there was a persistent increase in hires for the high corruption group after the audit: 22% for years 1–2 and 19% for years 3–4. These estimates are precise and statistically significant. Estimates are close to zero for low corruption municipalities for all time periods, indicating a null response to the audit. Importantly, estimates for the high and low corruption municipalities are statistically different from each other. Overall, these results indicate that municipalities with higher detected corruption levels are more responsive to the audit.

[22] The sample in this exercise excludes municipalities that were audited for the first time during lotteries 1–19, a period for which the data on corruption were unavailable.

Appendix Table C4 shows the results of a series of robustness analyses on the previous results. Column 1 shows that the audits led to a significantly larger effect on hires in high corruption municipalities than low corruption ones. The effects are similar in magnitude when I split the sample to compare only high corruption and never-audited municipalities (column 2) and only low corruption and never-audited municipalities (column 3). Estimates are also large and significant when I sequentially change the definition of high corruption as municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the percentiles 60, 70, and 80 (columns 4-6).

Appendix Figure C1 shows heterogeneous estimates for hires for temporary and tenure-track employees. The markers in Panel A show log hires of temporary employees relative to the year before the audit. There was an increase in hires in high corruption municipalities after the audit, while there was almost no change in low corruption municipalities. Panel B shows log hires of tenure-track employees relative to the year before an audit. The estimates are small and statistically insignificant for both groups. Similar to the results presented earlier, these findings show that audits lead to a larger effect in hires for temporary employees, for whom the mayor has more discretion to appoint. This effect is driven by the most corrupt municipalities.

Finally, I examine the analogous heterogeneity for new school employees. Table 2 shows the heterogeneous effect of the audits on new employees in municipal schools. The estimate in column 1 indicates that an audit leads to a significant increase (10%) in new school employees in high corruption municipalities; the effect is close to zero in low corruption municipalities. To test whether this effect holds for just those individuals who are novices in the sector, I changed the dependent variable in column 2 to new employees who are novices in the sector (i.e., those who had never previously worked in a school). This resulted in a significant increase (15.5%) in high corruption municipalities, while no significant effect in low corruption ones. Instead, the dependent variable in column 3 corresponds to the new employees with previous experience working in a school. This resulted in a small and not significant change in neither low nor high corruption municipalities. Finally, I changed the dependent variable in column 4 to new employees holding multiple jobs in public schools. The effect is a significant increase (3.5%) in high corruption municipalities, suggesting that the audit also increases the number of new employees who handle a heavy workload.

Overall, these results indicate that municipalities with higher detected corruption levels drive the overall effect on new school employees. Moreover, the audit increases the number of inexperienced and with heavy workload new employees.

5 Mechanisms: Patronage versus Hiring to Correct Irregularities

This section investigates mechanisms by which audits lead to additional hires of public employees. I consider two main competing mechanisms that could explain the previously reported results. One mechanism is that the corruption revealed by audits decreases mayors' chances of reelection; consequently, mayors increase their patronage engagement to compensate for the loss of electoral support. Another possibility is that the information revealed by audits improves the ability of mayors to take corrective actions and dismiss of perceived "bad apples,", thereby allowing them to hire new employees to replace the dismissed ones. I provide evidence supporting the first mechanism and ruling out the second. Then, I provide other evidence that further rules out other alternative explanations.

5.1 Patronage Engagement

A first potential mechanism that could explain why audits lead to additional hires is that mayors increase their patronage engagement to compensate for the loss of electoral support due to what audits reveal. On this point, Ferraz and Finan (2008) examine the same reform and find that disclosing corruption information to the public decreased reelection rates. The authors use data from the first thirteen waves of lotteries that occurred in 2003 and 2004. They compare the electoral outcomes of municipalities for which audit results were published just a few months before versus after the 2004 mayoral election (see Appendix Figure A5). I study the audits during a longer-time scale and four full mayoral terms. Later waves occur throughout the entire political cycle, which gives the incumbent more time, even years during his first term, to respond strategically before the next election (see Appendix Figure A6). In particular, I examine the 40 waves of lotteries conducted between 2003 and 2015 and evaluate electoral outcomes over four mayoral terms (2004-2016).

This mechanism requires the presence of electoral incentives, such as prospects for reelection, which are outlined by political agency models and rent-seeking (Persson and Tabellini, 2000). Considering this, I test if the effect of audit on hires is heterogeneous by whether a mayor may be up for reelection and thus have electoral incentives. Testing this empirically is complicated, as political outcomes may be affected by the audits. To overcome this, I define the variables "first term" and "second term" as a fixed characteristic in a municipality, where "first term" takes the value of one in municipalities where the mayor was audited in its first term. Analogously, "second term" takes the value of one in municipalities where the mayor was audited in its second term.

Table 3 shows that the effect of audit on hires is heterogeneous by whether a mayor was audited in its first or second term. A first-term mayor may be up for reelection and thus have electoral incentives, while a second-term mayor faces a term limit. Column 1 shows that the effect of the audit on hires in municipalities where the mayor was audited in its first term is 30% larger than that where he was audited in its second term. Then, to assess the robustness of this result, I sequentially restrict the sample (columns 2-4). In column 2, I restrict the sample to include only mayors who remain in office during two terms, as first-term mayors who survived a second term are more comparable than first-term mayors who do not. The effect on hires for first-term mayors remains positive and significant, while the effect for second-term mayors becomes small and not statistically significant. In column 3, I restrict further the sample and exclude those mayors following the audited one. In column 4, I exclude mayors who run for office after a one-term hiatus as their electoral incentives in the second term differ from those who do not run again. Overall, results are very similar, showing that mayors who are up for reelection and who have more to gain (i.e., reelection) from shoring up their popularity drive the effect of audit on hires.

Then, I examine whether the release of audit results differentially changes the reelection rates for a mayor who hired more or fewer employees during his term. I estimate (7), presenting the results in Table 4.[23] The dependent variable is the likelihood of a mayor getting reelected. I first examine the heterogeneous effect of the audit release across the most corrupt municipalities (columns 1 and 2). In column 1, the baseline estimate shows a significant drop of 34 percentage points in the likelihood of being reelected. In contrast, reelection rates decrease less in municipalities that were audited and hired more employees. In column 2, I also control for whether a mayor was eligible for reelection. Results are similar: the estimate of the interaction between the audits and hires is positive and significant. I then examine the heterogeneous effect for municipalities where auditors uncovered less corruption (columns 3 and 4), finding that the audits do not have a significant effect on reelection rates. Moreover, there was no differential effect by whether a mayor hired more people. The interaction estimate is statistically indistinguishable from zero. The coefficients in columns 1–4 show that hiring more employees is positively related to reelection rates. Overall, these results indicate that hiring additional employees offsets the negative effect of audits on mayoral reelection rates. This effect is significant for the most corrupt mayors, for whom the audit represents a more significant political burden.[24]

[23] Appendix Table F1 shows that the release of the audits has a significant effect on the likelihood of mayoral reelection in municipalities that have been audited more than once. To avoid dealing with this margin of endogeneity, I restrict the sample of audited municipalities to those audited only once.

[24] Appendix Table F2 shows that, in net, and considering the mayor's strategic response, being audited in current term does not lead to a decrease in reelection rates.

5.2 Hiring to Correct Irregularities

Bureaucracies are typically unable to monitor their employees perfectly. The information revealed by audits may improve the ability of mayors to take corrective actions against perceived "bad apples" who work in their municipality (i.e., dismissing or transferring them) (Finan et al., 2017).[25] The mayor would then need to hire new employees to replace the dismissed ones, which would explain the observed increase in hires.

Detected Irregularities in Schools. To investigate this mechanism, I processed nearly 800 audit reports to examine the audit data's richness, which allowed me to test directly whether additional hires occur in municipal public schools where auditors uncovered irregularities. After the audit, a mayor who takes hire to correct irregularities would be expected to make employment changes in schools where auditors uncovered irregularities. To test whether the observed effects on new employees come from those schools, I estimate the following equation:

$$Y_{s,t} = \alpha_s + \lambda_t + \eta_{-2} D_{i,t}^{-2} \times \text{Detected Irregularities}_{s,t} + \sum_{y=0}^{3} \beta_y D_{i,t}^{y} \times \text{Detected Irregularities}_{s,t} + \eta_{-2} D_{i,t}^{-2} \times \text{Not Detected Irregularities}_{s,t} + \sum_{y=0}^{3} \beta_y D_{i,t}^{y} \times \text{Not Detected Irregularities}_{s,t} + \epsilon_{s,t} \quad (4)$$

α_s, and λ_t are school and year fixed effects, respectively. $D_{i,t}^{y}$ is an indicator variable taking the value of 1 if it is year y relative to the audit year in municipality i. Detected Irregularities$_{s,t}$ is a dummy variable that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities$_{s,t}$ is a dummy variable indicating whether auditors did not uncover irregularities: either because auditors did not find any or because they did not investigate this school.

Figure 4 shows the differential effects of audits on log new employees in schools where auditors uncovered irregularities compared to the other schools conditional on a set of school and year fixed effects. Given the previous results showing high heterogeneity of the effects of audits based in levels of corruption in municipalities, I estimate equation 4 separately for high and low corruption municipalities. Panel A shows estimates in high corruption municipalities. Estimates in schools where auditors detected irregularities were minor (close to zero) and imprecise (the estimate for two or more years before the audit is statistically

[25]This mechanism would be consistent with research showing that audits generate incentives to reduce corruption through their disciplining effect (Avis et al., 2018).

significant). On the other hand, estimates after the audit in schools where auditors did not uncover irregularities are positive and significant.

Panel B shows estimates in low corruption municipalities. Estimates after the audit in schools where auditors detected irregularities are negative and precise. These results suggest that hires remained frozen after audits. One plausible explanation is that mayors might want to investigate detected irregularities before hiring new employees. On the other hand, after the audit, estimates in schools where auditors did not uncover irregularities are small and insignificant.

Overall, the evidence does not support the idea that mayors are increasing employment as a means of taking corrective actions. If it were driven by corrective actions, the effect on hires would be larger in schools where auditors uncovered irregularities, which is not what was observed.

5.3 Alternative Channels

In this subsection I consider several alternative channels could explain the effects of audits on hires. Then, I rule them out.

First, I consider alternative channels that could explain the effects on hires by directly leading to changes in employment in general. Appendix Table C5 shows the results of the examination of these channels.

Political turnover. Using data from the first few waves, Ferraz and Finan (2008) show that Brazilian audits reduce the probability of reelection of the incumbent mayor. In the same context, Akhtari et al. (2020) show that a new party in the mayor's office leads to upheaval in employee turnover, and the effect is concentrated mainly in the first year after the election. Therefore, the results on hires may be driven by a new mayor who comes to power due to the audits and who hires new employees.

Considering this, I first re-estimate the baseline results, excluding from the sample those municipalities that were audited in the third or fourth year of the mayoral term. By doing this, I compare only those municipalities audited early in their mayoral terms with non-audited municipalities. By excluding the municipalities audited late in the mayoral term, I avoid the possibility that my estimates are driven by a short period during a term, right before a new election happens. Instead, in this exercise, my estimate on hires incorporates the effects of three to four years in the audited mayor's term before a new election occurs. Column 1 of Appendix Table C5 shows that the estimate for hires using this sub-sample drops relative to the baseline estimates, but it is still a significant increase of 10.4%.

Then, I re-estimate the baseline results, excluding the years corresponding to the mayor's first year in office, in which most of the new hires due to political turnover occur. Reassuringly, column 2 shows that the estimate for hires is very similar to the baseline result in column 1 of Table 1. Overall, while political turnover is a relevant factor, the results in this section show that changes in mayoral turnover do not account for a large share of the effect on hires.

Resignations. Malfeasance revealed by the audits might lead bureaucrats to resign. Plausibly, the effects would be larger in those municipalities with higher levels of detected corruption. A larger number of resignations would require subsequent employee turnover, which could at least partially explain the main results. Column 3 of Appendix Table C5 provides evidence that audits do not lead to changes in the number of resignations of employees, which rules out the possibility that the findings are driven by decisions taken by the employee.

Inadequate employees' educational qualifications. An alternative explanation is that audits reveal that employees' education qualifications are inadequate, resulting in the mayor dismissing them to replace them with more qualified individuals. Columns 4 and 5 of Appendix Table C5 show the effect of audits on the share of hires and dismissals of individuals with a college degree. The effects are not statistically significant and are close to zero, suggesting that audits do not change the education level of new or dismissed employees, which rules out employee qualification as a contributing factor on hires.

Inability to dismiss employees. Another explanation is that, because mayors might be unable to fire employees due to institutional constraints, they might take other actions (i.e., hiring new employees) to reduce corruption. As mentioned previously, mayors have considerable discretion to fire temporary employees. To test this possibility, I looked at schools, where 55% of employees are temporary. Column 1 of Appendix Table E4 shows that audits lead to an increase in separations of about 4% in the most corrupt municipalities. Moreover, as column 2 shows, most of the effect of audits on separated employees is concentrated in schools where auditors did not detect irregularities. Together, these findings indicate that mayors are able to fire presumably corrupt employees (column 1), but do not seem to base their dismissals on corruption (column 4), which rules out the possibility that they hire employees to reduce corruption.

Finally, increases in hires could be related to the public finances of the municipality. I examined two such factors.

Changes in funds from the Central Government. An alternative explanation for an increased number of hires after an audit is that the federal government reallocates funds to audited municipalities; after all, in Brazil, municipalities receive large transfers from the federal government, receiving 51.46 billion dollars from the federal government in 2015[26], which represented 47% of their total expenditures. In line with Colonnelli and Prem (2021), I find no evidence that municipalities experienced changes in the inflow of federal transfers after the audits (Panel A in Appendix Figure D1), which rules out the possibility that the increase in hires owes to an increase in federal funds.

Changes in the allocation of expenses across sectors. A final alternative explanation is that audits generate a change in the budget allocation across different sectors. A mayor might allocate more or fewer funds to particular sectors in response to the irregularities detected through the audits. Panels B-D in Appendix Figure D1 show the effects of the audits on municipal expenditures in education, health, and social welfare. The effects after the audit are close to zero and not statistically significant, ruling out the possibility that hires are driven by changes on the allocation of expenses across sectors.

6 Public Goods Production

This section examines the effect of audits on public goods—that is, the quality of various goods and services offered by municipal governments and employees—and culminates in an answer to the question of where the budget for hiring additional employees is coming from.

6.1 Educational Performance of Students

Audits may produce positive consequences for public goods production due to their effects on hires. To examine this, I use school level data to examine the effects on dropout and failure rates. Then, I use data on students' universe in municipal public schools in Brazil to show how audits affect students' math and language scores.

Figure shows event study estimates of the effect of an audit on student performance of 1st–8th graders in municipal schools. Panel A of Figure 5 shows event study estimates for grade-level failure rates in municipal schools. Estimates are small and imprecise. The average failure rate in the sample is 10.7%. Panel B of Figure shows event study estimates on dropout rates of 1st–8th graders. The average dropout rate in my sample is 3.1%. Estimates are small (close to zero) and estimates are not significant at conventional levels.

Finally, Appendix Figure E2 shows event study estimates of the effect of an audit on student test scores in math and Portuguese language in municipal public schools. I present scores separately for 4th and 8th graders because, while all municipalities offer elementary schools, not all offer middle schools. Estimates of the effect of audits on test scores are close

[26] The nominal exchange rate in 2015 was LCU 3.33.

to zero and statistically insignificant.[27]

Overall, these results show that, while audits lead to additional hires, they do not lead to better public services. That is, audits did not improve student performance. As I showed before, patronage enables politicians to offset the potential electoral penalty of audits by hiring employees who do not contribute much to public goods production.

These findings serve to strengthen the argument that patronage is the main factor driving increases in hires. This also suggests that the gains in reelection rates for hiring more employees (Table 4) are not explained by better public goods, but by the patronage itself.

6.2 Public Finances and Capital Investment

As shown in Panel A of Figure D1, municipalities did not receive larger inflows of transfers following an audit. This raises the question of where the budget for hiring additional employees is coming from. Column 1 of Table 5 shows that audits increase payroll expenditures (as the share of total expenditures) by 2 pp for the most corrupt municipalities and by 1 pp for the less corrupt ones. In contrast, audits decrease the share of capital investment expenditure by 1 pp for the most corrupt municipalities (column 2). The effects are significant at conventional levels.

Consistently, this substitution from capital to payroll expenditures translated into a deterioration in the quality of school assets. Appendix Table E5 shows that the quality of computers and Internet for teacher and student use decreased following an audit. The estimate in column 1 implies that an audit led to a decrease of 4 pp in the share of principals who state that the quality of the computers available for students is high or good in the most corrupt municipalities. The estimate in column 2 shows that an audit led to a decrease of 5.5 pp in the share of principals who state that the quality of Internet for student use in the most corrupt municipalities. Similarly, columns 3 and 4 show that principals who state that the quality of computers and Internet for teacher use are lower in the most corrupt municipalities.

Overall, these results are consistent with patronage being a politically successful tool. Exchanging votes for jobs directly benefits voters and their families, while the political benefits of providing public goods may be more diffused and materialize in a longer-term horizon.

[27]Appendix Figure E3 shows the event study estimates for an alternative set of treated municipalities (audited in odd years), showing similar results.

7 Concluding Remarks

This paper studies the strategic response of mayors to the electoral backlash of government audits. It provides three main findings. First, audits increase the number of public employee hires and that effect is greater among municipalities in which audits uncovered higher corruption levels. Second, when considering the mechanisms that drive these results, the evidence is consistent with mayors hiring additional employees as a form of patronage to compensate for the electoral support lost due to audits. Finally, even though audits lead to additional hires, they do not positively affect public goods and services provision.

The effect of disclosing corruption on electoral accountability has been analyzed in field experiments and studies in which the information was revealed only a few days (Buntaine et al., 2018) or a few months (Ferraz and Finan, 2008) prior to the election. However, the timing of the information being released to voters is relevant, as an extended gap between the audit and the election provides the incumbent with sufficient time to respond strategically. Gratton et al. (2018) show that politicians' strategic behavior drives the timing in which political scandals are released to the public. I consider a different margin of how time matters. In particular, I exploit the gap between the time when damaging information about politicians is released and the next election. Depending on this gap, the incumbent has more or less time to react.

My findings show, given sufficient time, that politicians are able to respond strategically to electoral backlash from government audits. Through patronage, politicians offset the potential electoral penalty of the audit by hiring employees who do not contribute to public goods production. As a result, policymakers must be cautious about the unintended consequences of audits. The effectiveness of audits depends on the reactions (which are sometimes unanticipated) of those politicians to the new policy, which is, in principle, the "Lucas critique" (Lucas, 1976).

Moreover, the findings show that anti-corruption audits do not have real effects on student outcomes, which is surprising, given that Avis et al. (2018) find that audits are effective in reducing corruption. More work is needed to understand why reducing corruption levels does not translate into a real improvement in public services. The results on capital expenditures show that the audits lead to a substitution from investment to payroll spending. Future research should focus on studying whether there are long-term effects due to reducing investment spending.

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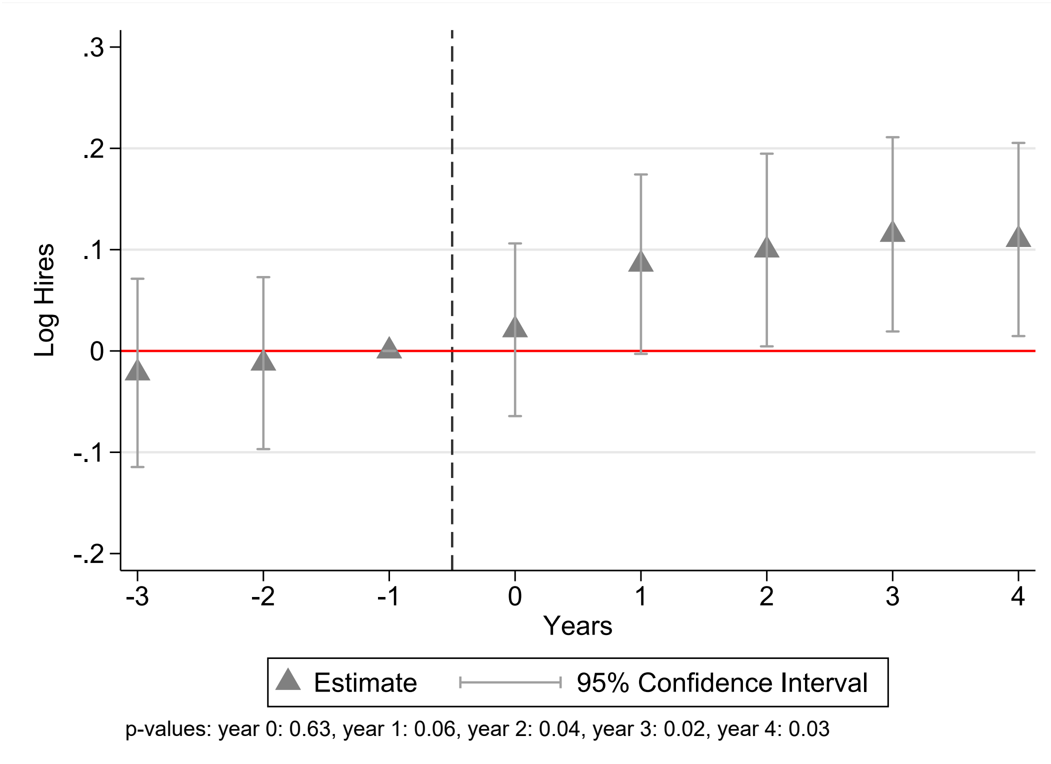
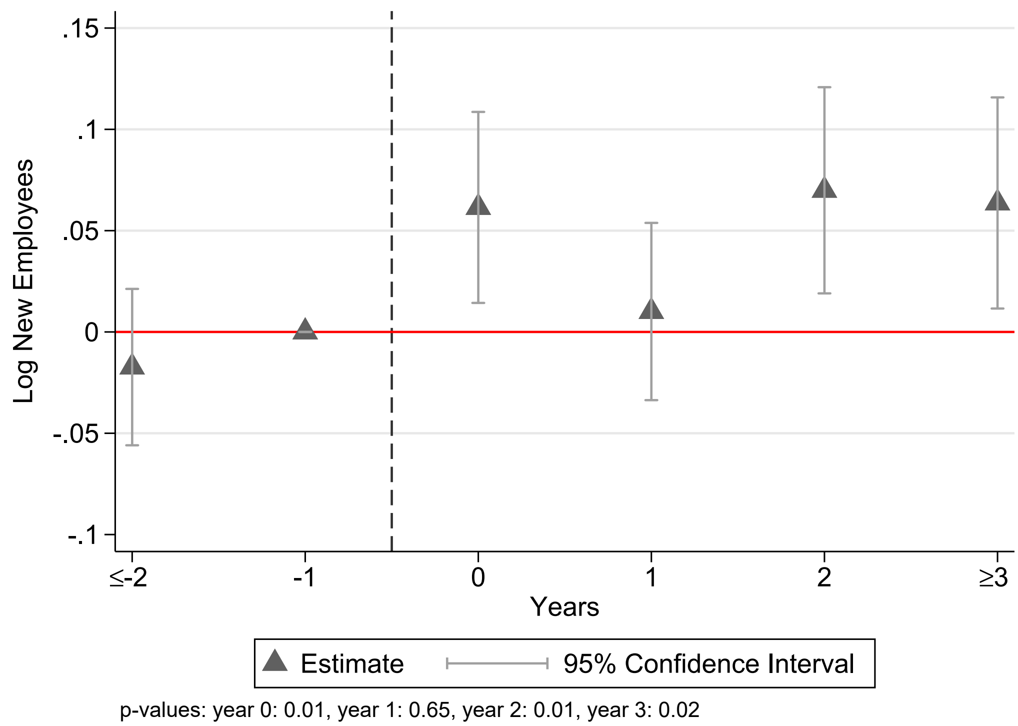
Figure 1: The Effect of the Audit on Hires of Municipal Public Employees



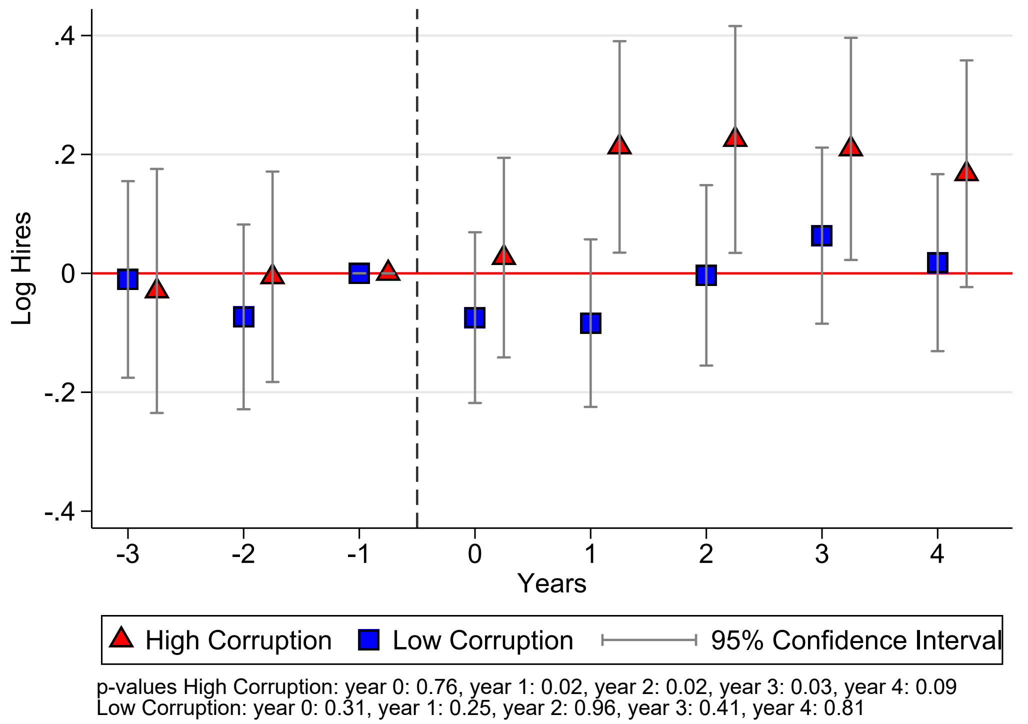
Notes: Graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the audit. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. The unit of observation in this figure is a municipality-year. Regression includes municipality and year fixed effects. Regression includes 64,912 observations from 4,057 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 3). Standard errors clustered by municipality.

Figure 2: The Effect of the Audit on Hires of Municipal School Employees



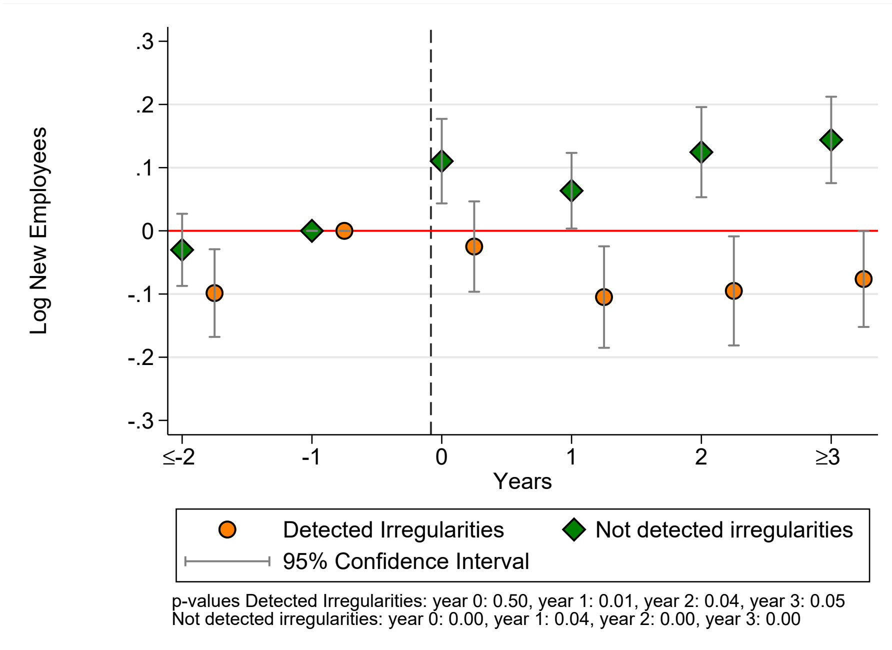
Notes: Graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of new employees. The unit of observation in this figure is a school-year. Regression includes school and year fixed effects. Regression includes 467,849 observations from 3,531 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

Figure 3: The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees

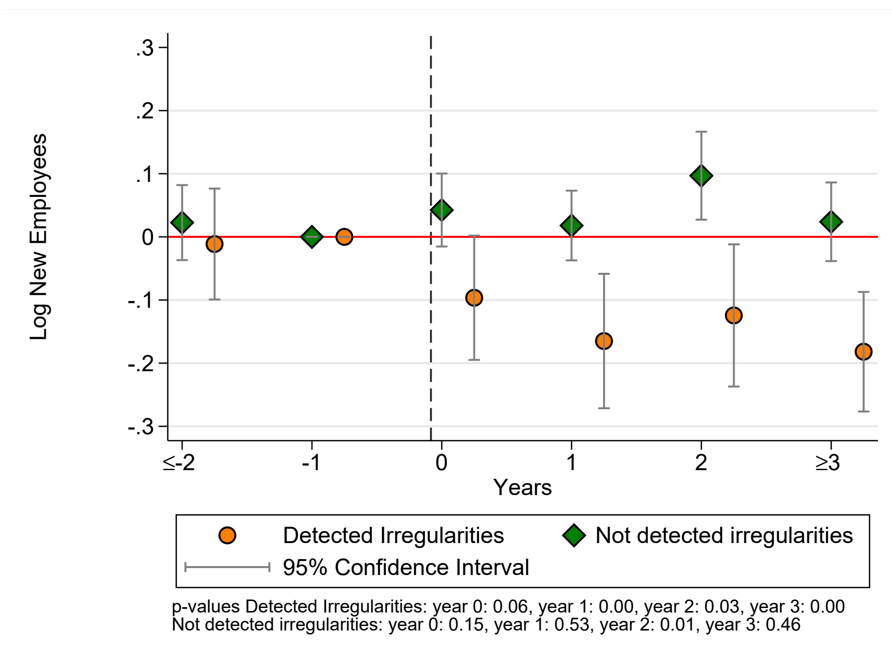


Notes: Graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit interacted with respective dummies for high and low corruption municipalities. The omitted time category is the year before the audit for each group of municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on new labor contracts added. The unit of observation in this figure is a municipality-year. Regression includes municipality and year fixed effects. Regression includes 56,192 observations from 3,512 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 3). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Standard errors clustered by municipality.

Figure 4: The Heterogeneous Effect of Hires in Municipal Schools by Detected Irregularities



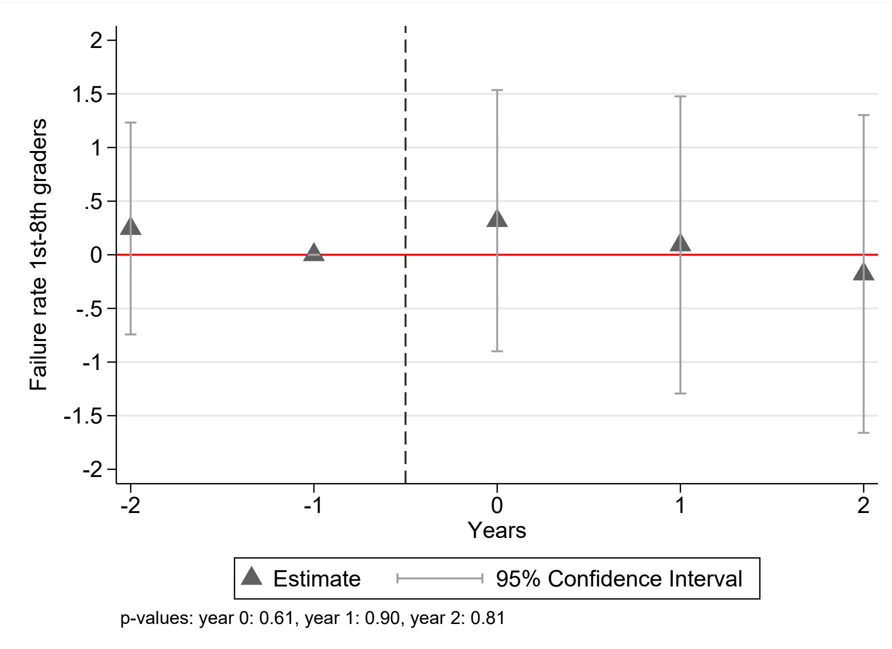
(a) High Corruption Municipalities



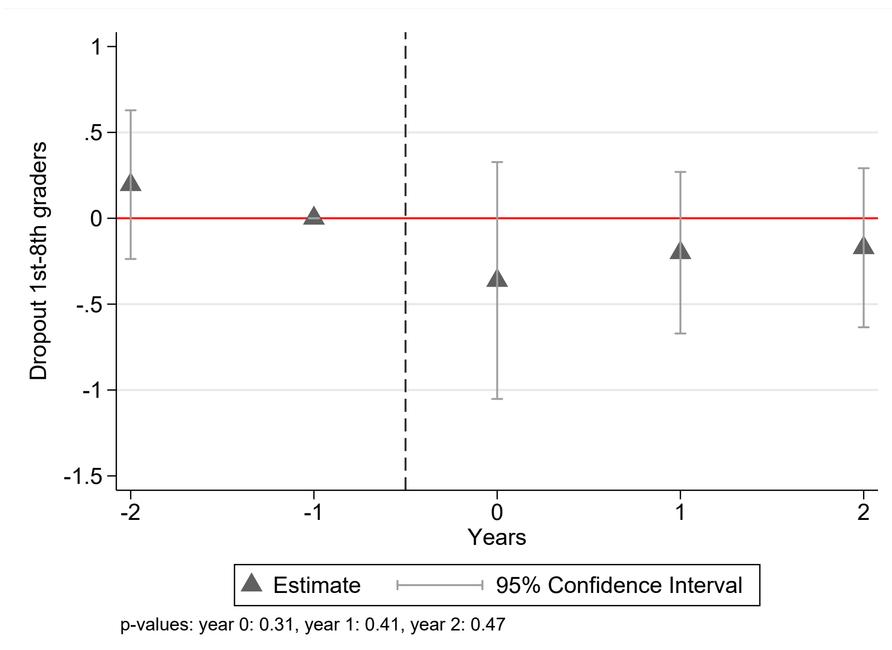
(b) Low Corruption Municipalities

Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of new employees. Detected Irregularities is a dummy that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities is a dummy indicating whether auditors did not uncover irregularities either because auditors did not find any or because they did not investigate this school. The unit of observation in this figure is a school-year. Regressions include school and municipality-year fixed effects. Regressions include 165,107 observations from 2,547 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

Figure 5: The Effect of the Audit on Additional Schooling Outcomes (1st-8th grade)



(a) Failure Rates



(b) Dropouts

Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Regressions include school and year fixed effects. Regressions use data from 2007 to 2015 and includes 203,523 observations from 3,712 municipalities. Sample covers a balanced panel of municipalities with a the window of [-3,3] around the audit year. Standard errors clustered by municipality.

Table 1: The Effect of the Audit on Hires of Municipal Public Employees

Dependent Variable:	Log Hires				
	Baseline	Type of work status		Type of Position	
		Temporary	Tenure-track	Managers	Non-managers
	(1)	(2)	(3)	(4)	(5)
PostAudited	0.155***	0.158***	0.088**	0.100***	0.155***
	[0.037]	[0.053]	[0.044]	[0.035]	[0.040]
Observations	64,912	64,912	64,912	64,912	64,912
Municipalities	4057	4057	4057	4057	4057
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	4.632	3.176	3.351	1.904	4.439

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-5 was done by estimating the inverse hyperbolic sine transformation$(log(y_i + (y_i^2 + 1)^{0.5}))$ on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. Post second-time Audited takes the value of 1 after the municipality was audited for a second time. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table 2: The Heterogeneous Effect of the Audit on Hires of Municipal School Employees

Dependent Variable:	New employees			
	All	Novice in education	Experience in education	Multiple school jobs
	(1)	(2)	(3)	(4)
PostAudited × Low Corruption	-0.006	-0.007	-0.016	-0.014
	[0.021]	[0.026]	[0.017]	[0.015]
PostAudited × High Corruption	0.102***	0.158***	0.016	0.035**
	[0.025]	[0.024]	[0.018]	[0.014]
Observations	447,264	447,264	447,264	447,264
Municipalities	3415	3415	3415	3415
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
p-value H_0: a-b=0	0.001	0.000	0.198	0.017
Mean of dep. var	1.463	0.796	1.057	0.444

Notes: Dependent variable in the header. Transformation of dependent variable in columns 1-4 was done by estimating the inverse hyperbolic sine transformation ($log(y_i + (y_i^2 + 1)^{0.5})$). PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Regressions in columns 1-4 use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a window of [-2,3] around the audit year. The unit of observation in this table is a school-year. Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of the Audit on Hires for First-term versus Second-term Mayors

Dependent Variable:	Log Hires			
	(1)	(2)	(3)	(4)
[a] PostAudited × First term	0.165***	0.217**	0.213**	0.199**
	[0.051]	[0.089]	[0.089]	[0.090]
[b] PostAudited × Second term	0.126*	0.034	0.012	0.005
	[0.071]	[0.072]	[0.073]	[0.076]
Observations	55,215	29,454	28,846	26,688
Municipalities	3681	3311	3311	2872
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample	All			
Ony Two term mayors		Yes	Yes	Yes
Excludes mayor following audited one			Yes	Yes
Excludes mayors who run for other office				Yes
p-value a-b=0	0.651	0.103	0.074	0.092
Mean of dep. var	4.721	4.589	4.577	4.613

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. First term takes the value of 1 for municipalities in which the audit occurs during the mayor's first term. Second term takes the value of 1 for municipalities in which the audit occurs during the mayor's second term. Column 2 restricts the sample to electoral periods in which the mayor is observed during his first and second term. Column 3 additionally excludes mayors following the audited mayor. Column 4 additionally excludes mayors who run for mayor in other municipalities or for mayor after a one-term hiatus. All regressions use data from 2001 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Sample uses data from 2001 to 2015 and and include municipalities with complete data on elections. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table 4: The Heterogeneous Effect of the Audit on Mayoral Reelection

Dependent Variable:	D(reelected mayor) Mean: 0.278			
	High Corruption municipalities		Low Corruption municipalities	
	(1)	(2)	(3)	(4)
Audited in Current Term	-0.339**	-0.218*	-0.075	0.048
	[0.140]	[0.117]	[0.138]	[0.100]
Audited in Current Term × Log Hires	0.054**	0.036**	0.007	-0.007
	[0.022]	[0.018]	[0.021]	[0.016]
Log Hires	0.043***	0.012**	0.046***	0.011**
	[0.005]	[0.005]	[0.005]	[0.005]
Eligible for reelection		0.520***		0.521***
		[0.006]		[0.005]
Observations	13,898	13,898	14,636	14,636
Municipalities	3491	3491	3659	3659
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. Log Hires is the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the total new labor contracts added during years 1-4 in the term. All regressions use election data from 2004, 2008 and 2012 and 2016. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19), municipalities that have been audited more than once, and municipalities without full employment data. The sample excludes municipalities that are not included in the balanced panel used to generate Figure 1. Sample in columns 1-2 includes never-audited and High Corruption audited municipalities. Sample in columns 3-4 includes never-audited and Low Corruption audited municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Variables High Corruption and Low Corruption always take the value of 0 for municipalities that have never been audited. The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

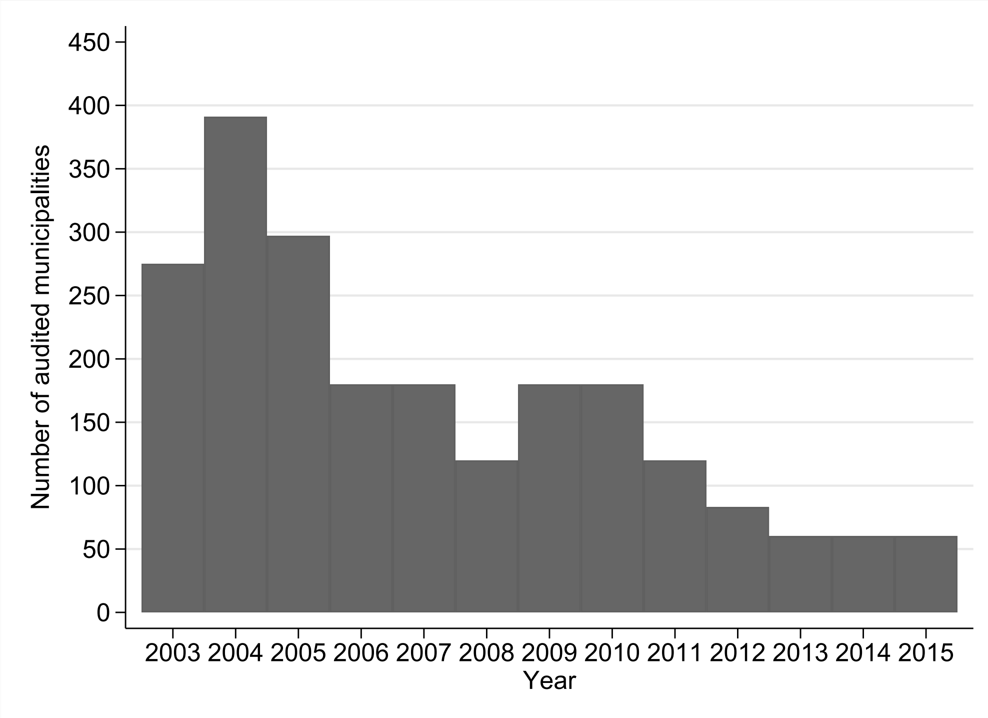
Table 5: The effects of the Audit on Payroll and Capital Expenditures

Dependent Variable:	Share of Expenditures	
	Payroll	Capital
	(1)	(2)
PostAudited × Low Corruption	0.010**	-0.004
	[0.005]	[0.004]
PostAudited × High Corruption	0.020***	-0.010**
	[0.005]	[0.004]
Observations	41,675	41,675
Municipalities	2780	2780
Municipality FE	Yes	Yes
Year FE	Yes	Yes
p-value a-b=0	0.098	0.262
Mean of dep. var	0.611	0.166

Notes: Dependent variable in the header is expressed as the share of total municipal expenditures. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. All regressions use data from 2000 to 2015, except for 2004, for which I do not have reliable data on expenditures on payroll or capital. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Sample excludes municipalities with missing data. The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

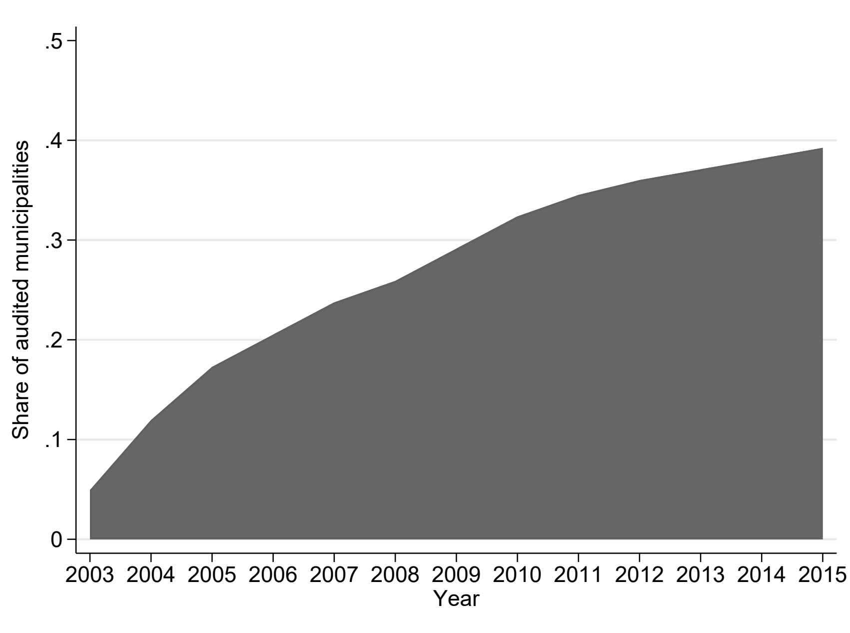
A Additional Background Information

Figure A1: Number of Municipalities Audited Per Year



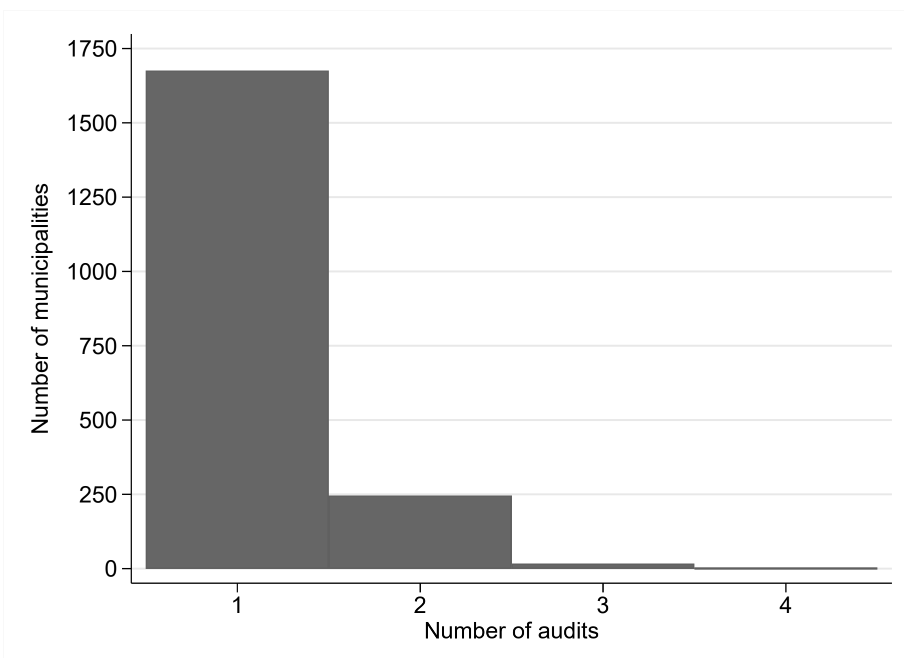
Notes: This figure plots the number of municipalities that were audited for the full duration of the program (from 2003 to 2015).

Figure A2: Share of Municipalities that were Audited



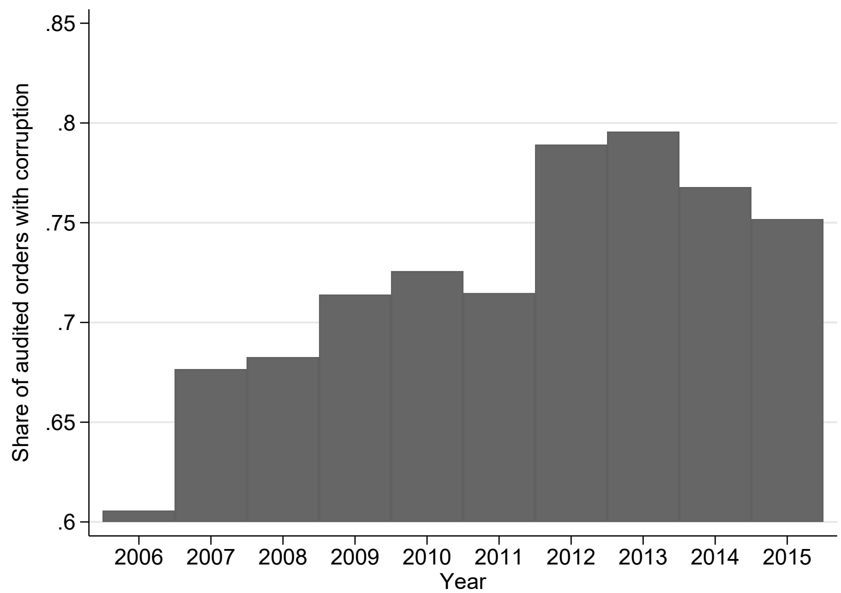
Notes: This figure plots the number of municipalities that were audited at least once for the full duration of the program (from 2003 to 2015), as the share of the total number of eligible municipalities.

Figure A3: Distribution of Times a Municipality has been Audited



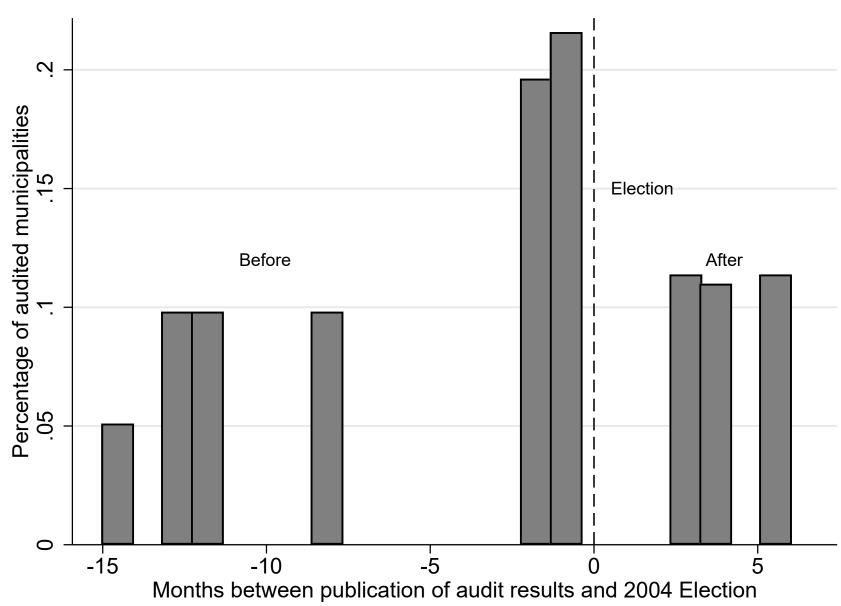
Notes: This figure plots the distribution of the number of times a municipality has been audited for the full duration of the program (from 2003 to 2015).

Figure A4: Distribution of Irregularities Associated with Corruption



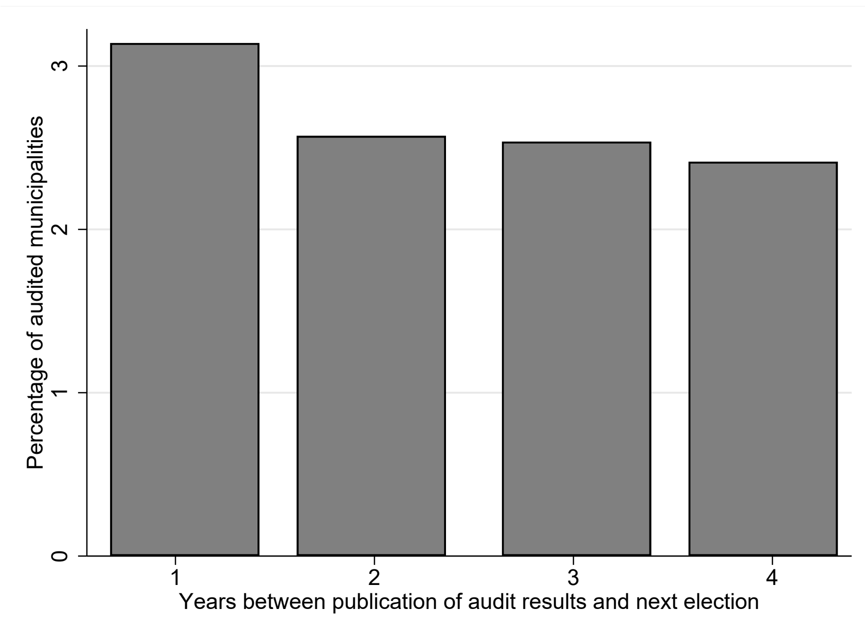
Notes: This figure displays the distribution of irregularities per service order associated with corruption.

Figure A5: Timing between the Release of Audit Results and 2004 Mayoral Election



Notes: Figure shows the share of audited municipalities by the number of months between the release of the audit reports in the first thirteen lotteries and the 2004 Mayoral Election. These lotteries correspond to the ones used by Ferraz and Finan (2008).

Figure A6: Years between the Release of Audit Results and Mayoral Elections, 2004-2016



Notes: Figure shows the share of audited municipalities by the number of years between the release of the audit reports and the following Mayoral Election.

Table A1: Summary Statistics

	N	Mean	Std. Dev.	Min.	Max.
Panel A. Eligible municipalities					
	A1. Public employees in municipalities				
Employees at end of year	64,912	671.0	990.6	1.0	15,500.0
Hires	64,912	164.0	327.5	0.0	9,743.0
Dismissals	64,912	31.9	120.1	0.0	6,073.0
Temporary employees at end of year	64,912	123.0	333.4	0.0	10,686.0
	A2. Other Characteristics				
Illiteracy rate 2000	4,056	0.207	0.119	0	1
Gini 2000	4,056	0.553	0.067	0	1
Income per capita 2000	4,056	5.670	0.570	4	8
Population 2000	4,056	9.358	1.063	7	13
Urban rate 2000	4,056	0.604	0.226	0	1
	A3. Reelection				
Reelected mayor	60,447	0.320	0.467	0	1
Panel B. Audited municipalities					
	B1. Public employees in municipalities				
Employees at end of year	17,616	722.6	1,035.8	1.0	14,703.0
Hires	17,616	177.2	367.6	0.0	9,743.0
Dismissals	17,616	31.2	103.3	0.0	2,728.0
Temporary employees at end of year	17,616	132.9	336.8	0.0	10,686.0
	B2. Other Characteristics				
Illiteracy rate 2000	1,101	0.218	0.120	0	1
Gini 2000	1,101	0.561	0.066	0	1
Income per capita 2000	1,101	5.622	0.576	4	7
Population 2000	1,101	9.462	1.057	7	13
Urban rate 2000	1,101	0.601	0.225	0	1
	B3. Reelection				
Reelected mayor	16,364	0.321	0.467	0	1

Notes: This table reports summary statistics at the municipality level, using RAIS, electoral and Census data for the period 2000–2015. The sample in Panel A includes all eligible municipalities, including those that are audited. The sample in Panel B includes only municipalities audited as part of the CGU anti-corruption program. Variables summarizing information on public employees in municipality correspond to number of labor contracts. Income per capita in log local currency per capita. Population in natural logarithm.

B Are audits random?

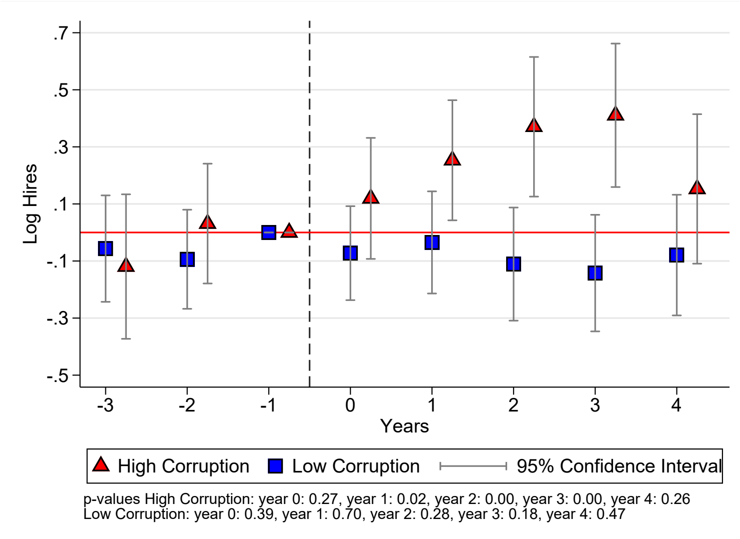
Table B1: Are Audits Random?

Dependent Variable:	Audited
	(1)
Hires 2002	0.000
	[0.000]
Dismissals 2002	-0.000
	[0.000]
Employees at end of year 2002	-0.000
	[0.000]
Temporary Employees at end of year 2002	0.000
	[0.000]
Share of Illiterate population 2000	0.000
	[0.001]
Income Gini 2000	0.119
	[0.112]
Income per capita 2000	-0.009
	[0.030]
Population 2000	0.017
	[0.011]
Share Urban 2000	0.064
	[0.041]
GDP 2002	-0.008
	[0.017]
Observations	5,271
State FE	Yes
p-value: Joint orthogonality	0.316
Mean of dep. var	0.344
R-squared	0.034

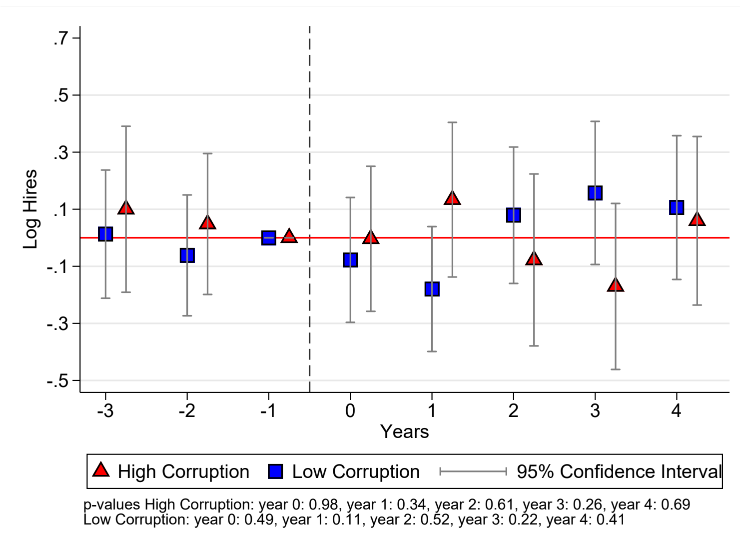
Notes: This table illustrates the randomness in the selection of municipalities to audit. Column 1 presents coefficients from a cross-sectional regression $\text{Audited}_{i,s} = \alpha_s + \gamma \times X_i + \epsilon_{i,s}$. The outcome variable, $\text{Audited}_{i,s}$, is an indicator for whether the municipality is audited between 2003-2015. All specifications include state fixed effects. The sample includes all eligible municipalities. RAIS variables and natural logarithm of estimated GDP are measured in 2002. Demographic variables from the 2000 Census. Robust standard errors are presented in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

C Municipal Public Employees: Additional Results

Figure C1: The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees by Type of Contract



(a) Log Hires - Temporary



(b) Log Hires - Tenure-track

Notes: Each graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit interacted with respective dummies for high and low corruption municipalities. The omitted time category is the year before the audit for each group of municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on new labor contracts added by type of contract (i.e. tenure-track or temporary). The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Regressions include 56,192 observations from 3,512 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 3). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Standard errors clustered by municipality.

Table C1: Main Results: Robustness

Dependent Variable:	Log Hires			Ln Hires	Hires
	Callaway et. al. (2020)	Hires>0	First-time Hires		
	(1)	(2)	(3)	(4)	(5)
PostAudited	0.100**	0.134***	0.135***	0.183***	14.966**
	[0.043]	[0.030]	[0.051]	[0.054]	[7.610]
Observations	64,912	61,833	64,912	64,912	64,912
Municipalities	4057	4057	4057	4057	4057
Municipality FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Mean of dep. var	4.632	4.863	3.159	3.749	164

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. The dependent variable in column 4 is the natural logarithm of new labor contracts plus 0.01. The dependent variable in column 5 is new labor contracts. PostAudited takes the value of 1 after the municipality was audited. Estimator in column 1 corresponds to the aggregate ATT estimated using Callaway and Sant'Anna (2020a). Sample excludes observations with zero hires in column 2. Dependent variable in column 3 corresponds to first-time hires only, and excludes transfers or re-employment. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table C2: Main Results: Goodman-Bacon Decomposition

Dependent Variable: Log Hires		
DD Comparison	Weight	Average DD Estimator
Earlier T vs. Later C	0.033	-0.036
Later T vs. Earlier C	0.052	0.023
T vs. Never treated	0.915	0.169
DD Estimate		0.155

Notes: Dependent variable corresponds to the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. DD estimators were estimated using Goodman-Bacon et al. (2019). Regressions uses data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Regressions includes municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table C3: The Effect of the Audit on Dismissals and Municipal Public Employees at end of year

Dependent Variable:	Dismissals	Employees at end of year
	(1)	(2)
PostAudited	0.094**	0.037***
	[0.043]	[0.014]
Observations	64,912	64,912
Municipalities	4057	4057
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Mean of dep. var	2.030	6.667

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of contracts where the employee was dismissed, and total contracts at the end of the year. PostAudited takes the value of 1 after the municipality was audited. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table C4: The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees: Robustness

Dependent Variable:	Log Hires					
	Baseline	Split Sample		Changing cutoffs: High Corruption		
	(1)	(2)	(3)	(4)	(5)	(6)
PostAudited		0.335***	0.103*			
		[0.080]	[0.057]			
PostAudited × Low Corruption	0.098*			0.075	0.077	0.104**
	[0.057]			[0.054]	[0.051]	[0.049]
PostAudited × High Corruption	0.334***			0.430***	0.573***	0.714***
	[0.080]			[0.090]	[0.110]	[0.147]
Observations	56,192	51,088	52,400	56,192	56,192	56,192
Municipalities	3512	3193	3275	3512	3512	3512
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
p-value H_0: a-b=0	0.014			0.001	0.000	0.000
Mean of dep. var	4.606	4.624	4.602	4.606	4.606	4.606
Percentile	50	50	50	60	70	80
Sample		High Corruption	Low Corruption			

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Sample in column 2 includes never audited municipalities and High Corruption municipalities. Sample in Column 3 includes never audited municipalities and Low Corruption municipalities. In columns 4-6 I change the definition of high corruption as municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the percentiles 60 (75%), 70 (80%), and 80 (84%). All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

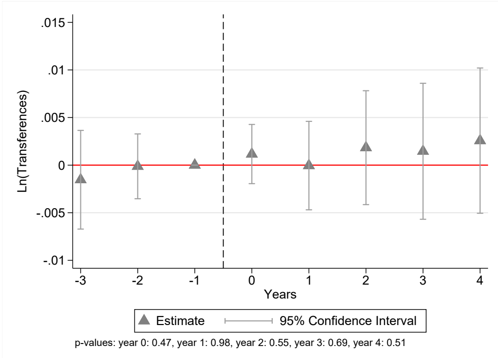
Table C5: Alternative Channels for the Effect of Audit on Hires

Dependent Variable:	Hires		Resignations	Share Hires w/ college	Share Dismissals w/college
	(1)	(2)	(3)	(4)	(5)
PostAudited	0.104**	0.148***		-0.003	0.001
	[0.049]	[0.039]		[0.004]	[0.007]
PostAudited × Low Corruption			0.044		
			[0.057]		
PostAudited × High Corruption			-0.040		
			[0.069]		
Observations	56,416	48,684	56,192	61,833	41,855
Municipalities	3526	4057	3512	4057	3857
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
p-value H_0: a-b=0			0.334		
Mean of dep. var	4.612	4.520	2.201	0.318	0.332

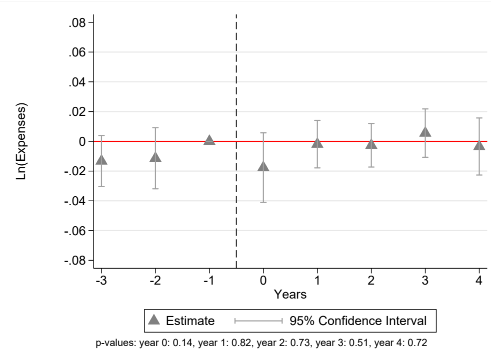
Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added (columns 1-2) and on the number of work contracts of employees who resigned (column 3). PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Sample in column 1 excludes municipalities that were audited during years 3 or 4 of the mayoral term. Sample in column 2 excludes observations that correspond to the first year of a mayoral term. Sample in column 3 excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). All regressions use data from 2000 to 2015, except column 1. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

D Public Finances

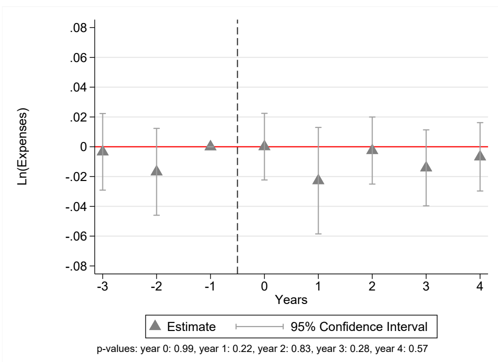
Figure D1: Effects of the Audit on Federal Transfers and Municipal Expenditures



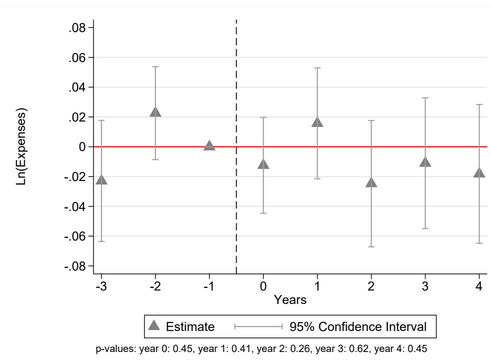
(a) Federal Transfers



(b) Education Expenses



(c) Health Expenses

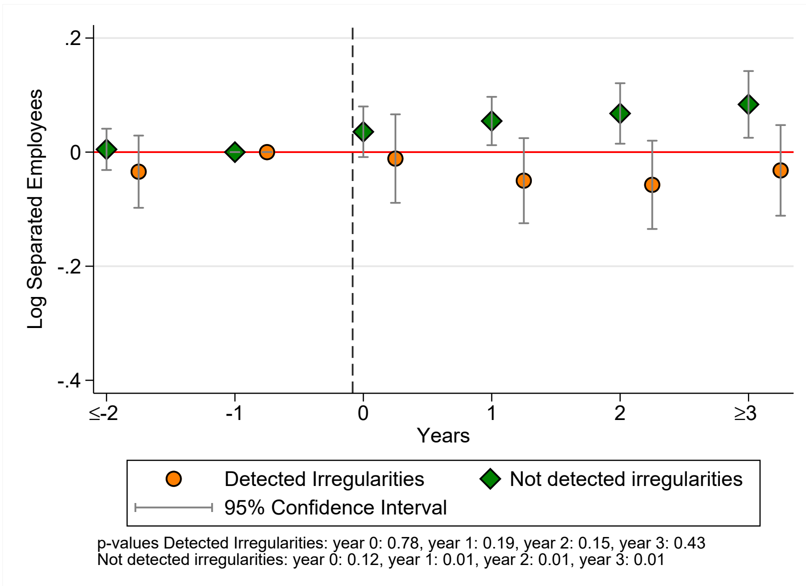


(d) Social Welfare Expenses

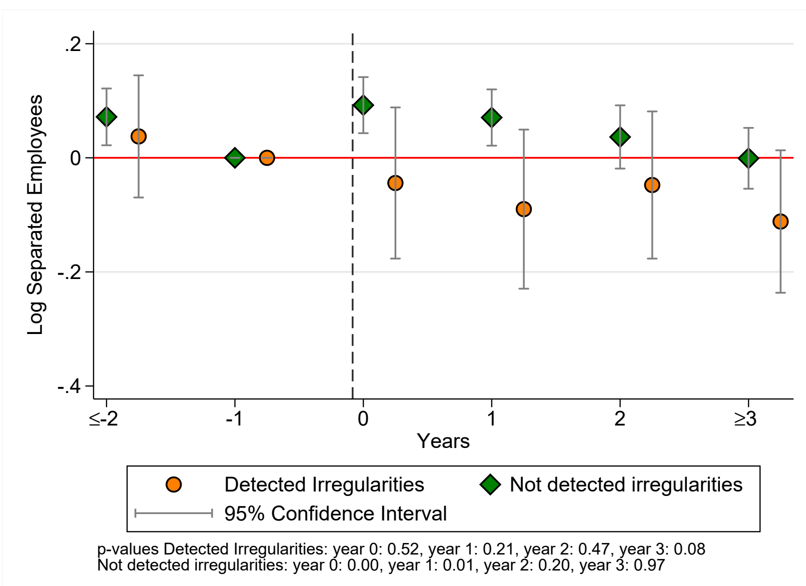
Notes: Each graph shows point estimates and 95% confidence intervals of regression of municipality level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Dependent variable is expressed in log points of constant local currency units. All regressions use data from 2000 to 2015 and include municipalities with complete data on transfers for the full sample. Regressions include municipality and year fixed effects. Regressions in Panels A and B include 87,360 observations from 5,460 municipalities. Regressions in Panels C-D include 45,472 observations from 2,842 municipalities. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

E Public Education: Additional Results

Figure E1: The Heterogeneous Effect of Separations in Municipal Schools by Detected Irregularities



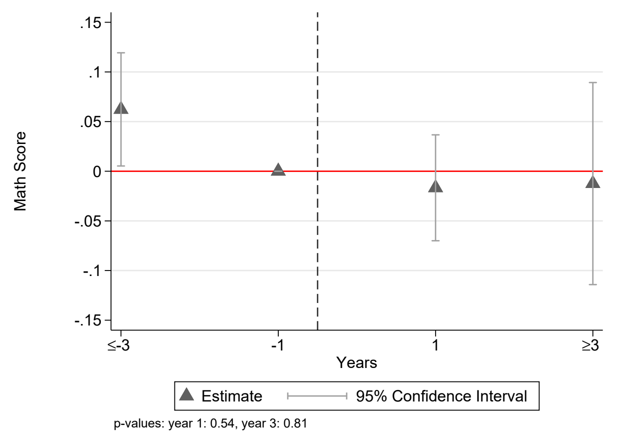
(a) High Corruption Municipalities



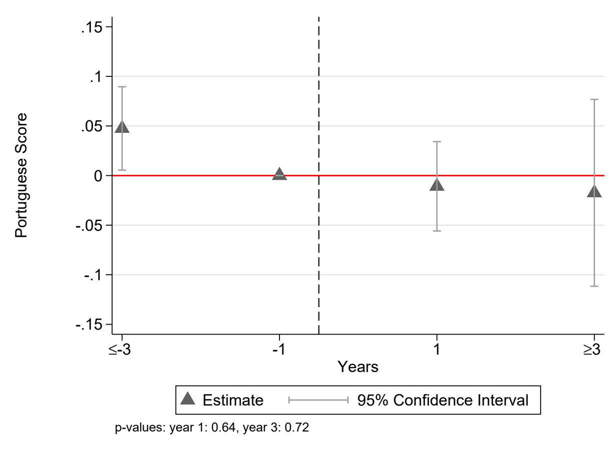
(b) Low Corruption Municipalities

Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of separated employees. Regressions include school and municipality-year fixed effects. Regressions include 165,107 observations from 2,547 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

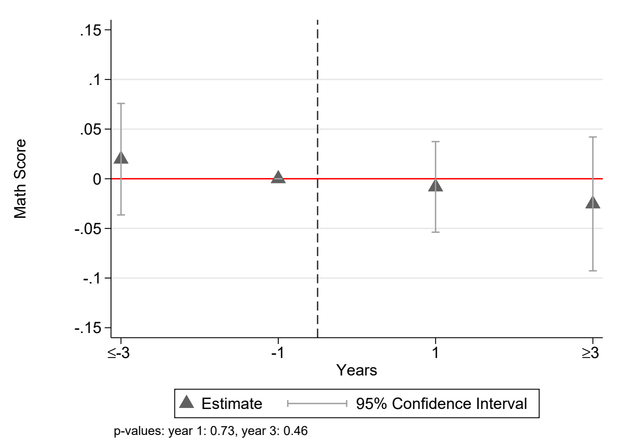
Figure E2: The Effect of the Audit on Student Test Scores in Municipal Public Schools



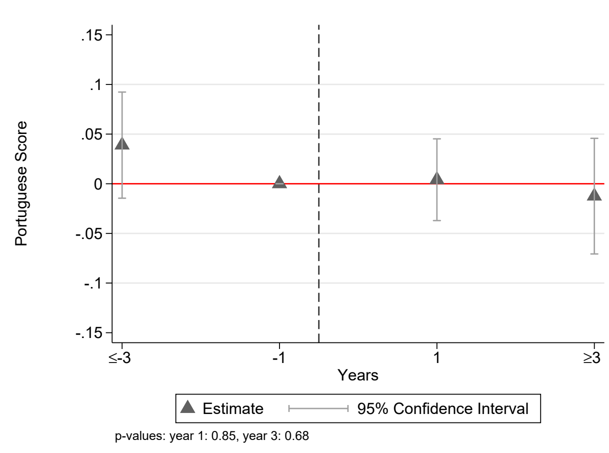
(a) 4th grade - Math



(b) 4th grade - Portuguese Language



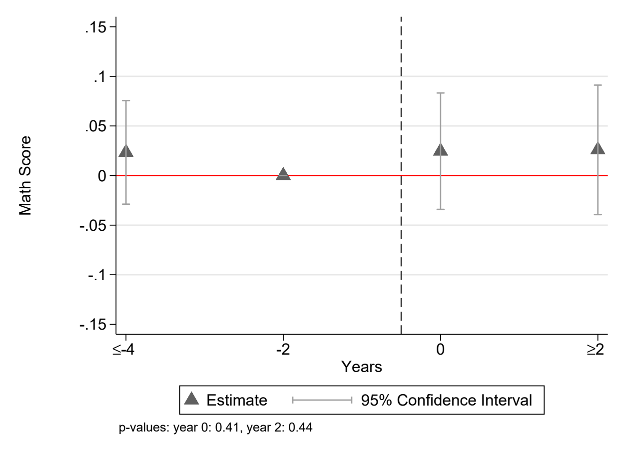
(c) 8th grade - Math



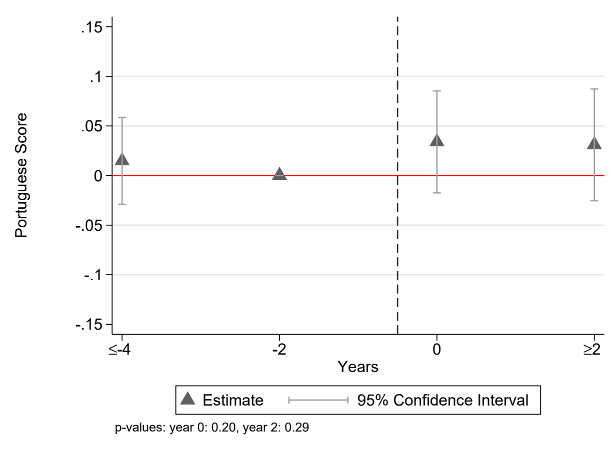
(d) 8th grade - Portuguese Language

Notes: Each graph shows point estimates and 95% confidence intervals of a regression of individual level test scores on a full set of dummy variables for years -3, -1, 1 and 3 relative to the audit. The omitted category is the year before the first audit. Individual Test scores correspond to the Prova Brasil exams (2007, 2009, 2011, 2013 and 2015) and are standardized based on the distribution of individual level test scores from students in non-audited municipalities up to 2015. The unit of observation in this figure is a student-year. Regressions include school and year fixed effects. They also include student level controls (an indicator variable for gender, age, whether the student is white, and whether the student's mother reads). Regressions on 4th grade tests scores include 2,887,564 observations from 3,203 municipalities. Regressions on 8th grade tests scores include 1,206,038 observations from 2,074 municipalities. Sample covers a balanced panel of municipalities. Sample of audited municipalities include those drawn in lotteries 31-33, and 36-37. Standard errors clustered by municipality.

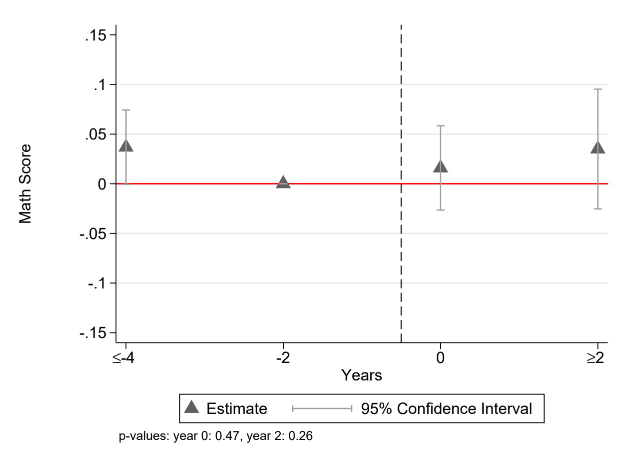
Figure E3: The Effect of the Audit on Student Test Scores in Municipal Public Schools



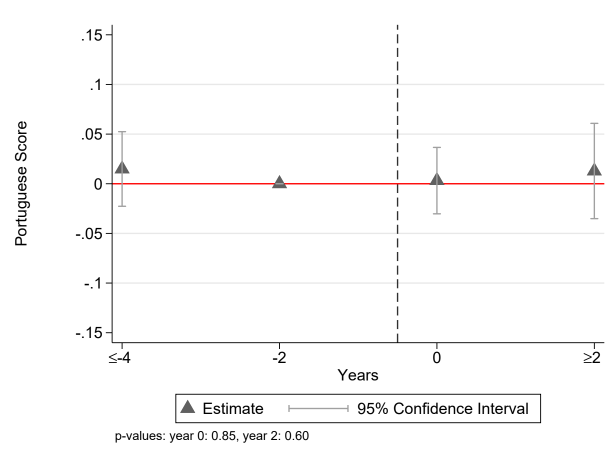
(a) 4th grade - Math



(b) 4th grade - Portuguese Language



(c) 8th grade - Math



(d) 8th grade - Portuguese Language

Notes: Each graph shows point estimates and 95% confidence intervals of a regression of individual level test scores on a full set of dummy variables for years -2, 0, 2 and 4 relative to the audit. The omitted category is the year of the audit. Individual Test scores correspond to the Prova Brasil exams (2007, 2009, 2011, 2013 and 2015) and are standardized based on the distribution of individual level test scores from students in non-audited municipalities up to 2015. Regressions include school and year fixed effects. They also include student level controls (an indicator variable for gender, age, whether the student is white, and whether the student's mother reads). Regressions on 4th grade tests scores include 2,887,564 observations from 3,203 municipalities. Regressions on 8th grade tests scores include 1,206,038 observations from 2,074 municipalities. Sample covers a balanced panel of municipalities. Sample of audited municipalities include those drawn in lotteries 34-35, and 28-30. Standard errors clustered by municipality.

Table E1: Effects of the Audit on New Municipal Public School Employees: Robustness

Schools:	Municipal						State&Federal
Dependent Variable:	Log New Employees				Ln New Employees	New Employees	Log New Employees
		Callaway et. al. (2020)	New Employees>0	*Prova Brasil*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PostAudited	0.065***	0.072*	0.046***	0.087***	0.151***	0.259***	-0.010
	[0.019]	[0.037]	[0.013]	[0.026]	[0.047]	[0.069]	[0.024]
Observations	447,264	447,264	336,551	170,568	447,264	447,264	105,896
Municipalities	3415	3415	3414	3291	3415	3415	3383
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	1.463	1.463	1.939	2.159	-0.226	3.726	2.505

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-4 and 7 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of new employees in municipal schools. The dependent variable in column 5 is the natural logarithm of new employees plus 0.01. The dependent variable in column 6 is new employees. PostAudited takes the value of 1 after the municipality was audited. Estimator in column 1 corresponds to equation 3. Estimator in column 2 corresponds to the aggregate ATT estimated using Callaway and Sant'Anna (2020a). Sample excludes observations with zero new employees in column 3. Sample excludes observations from schools that did not participate in *Prova Brasil* in column 4. *Prova Brasil* is administered only in schools with at least 20 students enrolled in that particular grade level. Sample in columns 1-6 includes all municipal schools, sample in column 7 includes only state and federal schools. All regressions use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table E2: Effects of the Audit on Municipal Public School Employees

Dependent Variable:	Separated employees	Employees at time of Census	Hires in education
	(1)	(2)	(3)
PostAudited	0.022	0.031***	0.090**
	[0.015]	[0.009]	[0.044]
Observations	447,264	447,264	64,912
Municipalities	3415	3415	4057
Years	2008-2015	2008-2015	2000-2015
Data source	School Census	School Census	*RAIS*
School FE	Yes	Yes	No
Year FE	Yes	Yes	Yes
Municipality FE	No	No	Yes
Mean of dep. var	1.401	2.588	2.871

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$). Dependent variables in columns 1-2 correspond to the number of separated employees, employees at time of Census in municipal schools, respectively. Dependent variable in column 3 corresponds to the new labor contracts added in the sector of education and where the municipal government was the employer. PostAudited takes the value of 1 after the municipality was audited. Regressions in columns 1-2 use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Regressions in columns 1-2 include school and year fixed effects. Regression in column 3 uses data from 2000 to 2015 and the sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year. Regression in column 3 includes municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table E3: The Effect of the Audit on Municipal Public School Principals

	Share of Principals	
Dependent Variable:	New	Tenure-track
	(1)	(2)
PostAudited	-0.003	-0.041***
	[0.015]	[0.012]
Observations	53,705	53,705
Municipalities	3154	3154
School FE	Yes	Yes
Year FE	Yes	Yes
Mean of dep. var	0.410	0.107

Notes: Dependent variable in the header. PostAudited takes the value of 1 after the municipality was audited. Regressions use data from individual survey responses from the Prova Brasil school principal questionnaire (2007, 2009, 2011, 2013 and 2015). New principals correspond to principals that stated that started working in the school less than 2 years ago. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table E4: Effects of the Audit on Separated Municipal Public School Employees

Dependent Variable:	Separated employees	
	All	High Corruption
	(1)	(2)
PostAudited × Low Corruption	-0.005	
	[0.016]	
PostAudited × High Corruption	0.038***	
	[0.013]	
PostAudited × Not detected irregularities		0.041***
		[0.015]
PostAudited × Detected irregularities		0.013
		[0.022]
Observations	540,384	482,512
Municipalities	4010	3569
School FE	Yes	Yes
Year FE	Yes	Yes
p-value H_0: a-b=0	0.032	0.208
Mean of dep. var	1.387	1.387

Notes: Dependent variable in the header. Transformation of dependent variable was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of separated employees in municipal schools. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Detected Irregularities is a dummy that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities is a dummy indicating whether auditors did not uncover irregularities either because auditors did not find any or because they did not investigate this school. Regressions use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table E5: Effects of the Audit on School Assets Quality

	Good quality			
	Students use		Teachers use	
Dependent Variable:	Computer	Internet	Computer	Internet
	(1)	(2)	(3)	(4)
PostAudited × Low Corruption	0.008	0.024	0.063***	0.035
	[0.023]	[0.024]	[0.022]	[0.025]
PostAudited × High Corruption	-0.040**	-0.055***	-0.039**	-0.040**
	[0.019]	[0.016]	[0.020]	[0.018]
Observations	50,614	50,538	50,462	50,470
Municipalities	3154	3154	3154	3154
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
p-value a-b=0	0.097	0.004	0.000	0.013
Mean of dep. var	0.464	0.350	0.494	0.461

Notes: Dependent variable in the header. Dependent variable takes the value of 1 if the principal states that the quality is good or very good, and 0 otherwise. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Regressions use data from individual survey responses from the Prova Brasil school principal questionnaire (2007, 2009, 2011, 2013 and 2015). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

F Reelection: Additional Results

Table F1: The Effect of Multiple Audits on Mayoral Reelection

Dependent Variable:	D(reelected mayor) Mean: 0.278	
	(1)	(2)
[*a*] Audited in Current Term	-0.010	-0.001
	[0.024]	[0.019]
Audited in Previous Term	-0.013	-0.023
	[0.020]	[0.022]
[*b*] Audited in Current Term $\times$ Audited in Previous Term	0.073	0.105*
	[0.080]	[0.062]
Eligible for reelection		0.524***
		[0.005]
Observations	16,484	16,484
Municipalities	4121	4121
Municipality FE	Yes	Yes
Year FE	Yes	Yes
p-value H_0: a+b=0	0.393	0.077

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. All regressions use election data from 2004, 2008 and 2012 and 2016. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table F2: The Effect of the Audit on Mayoral Reelection

Dependent Variable:	D(reelected mayor) Mean: 0.279	
	(1)	(2)
Audited in Current Term	-0.018	0.002
	[0.027]	[0.021]
Eligible for reelection		0.523***
		[0.005]
Observations	15,171	15,171
Municipalities	3810	3810
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19), and municipalities that have been audited more than once. The sample excludes municipalities that are not included in the balanced panel used to generate Figure 1. All regressions use election data from 2004, 2008 and 2012 and 2016. The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

G Empirical Appendix

This section provides detailed information on the the research design.

Heterogeneous Effects. When studying heterogeneous effects, I replace the event-year dummies in equation 1 with $\sum_k(\sum_{y=-4}^{-2} \tilde{\eta}_y^k D_{i,t}^y D_i^k + \sum_{y=0}^{5} \tilde{\beta}_y^k D_{i,t}^y D_i^k)$. The estimation equation is as follows:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_k(\sum_{y=-4}^{-2} \tilde{\eta}_y^k D_{i,t}^y D_i^k + \sum_{y=0}^{5} \tilde{\beta}_y^k D_{i,t}^y D_i^k) + \epsilon_{i,t} \tag{5}$$

D_i^k is equal to 1 if a municipality belongs to group k.

Summarizing results. In the tables, I summarize the magnitudes and joint statistical significance of the event study estimates in a DD's specification according to the following specification:

$$Y_{i,t} = \beta \text{PostAudited}_{i,t} + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{6}$$

where $\text{PostAudited}_{i,t}$ is an indicator variable taking the value of 1 for all years after the audit in the audited municipality, and 0 otherwise. $\text{PostAudited}_{i,t}$ is always 0 for never-audited municipalities. The fixed effects are analogous to those in equation (1). The coefficient of interest is β, which captures the average causal change in the outcome variable (e.g., percentage change of hired municipal public employees) of audited municipalities compared to the eligible but never-audited municipalities, conditional on the set of municipality and year fixed effects.[28]

Prova Brasil. In addition to the primary municipality-level analysis, I also conduct a student-level and principal-level analysis by adapting the same empirical design to the more disaggregated dataset from Prova Brasil. I use the following specification:[29]

$$Y_j = \alpha_{s(j)} + \lambda_{t(j)} + \eta D_{i,t}^{-3} + \beta_1 D_{i,t}^1 + \beta_3 D_{i,t}^3 + \gamma X_j + \epsilon_j \tag{9}$$

[28] When studying heterogeneous effects based on differential preexisting characteristics, I estimate the following interaction specification:

$$Y_{i,t} = \beta_1 \text{PostAudited}_{i,t} + \beta_2 \text{PostAudited}_{i,t} \times \text{C}_i + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{7}$$

where C_i is a characteristic of the municipality measured pre-audit, unless otherwise specified.

[29] Alternatively, I could include event-year dummies for -2, 2, and 4, a full set of dummies (0 is omitted).

$$Y_i = \alpha_{s(i)} + \lambda_{t(i)} + \eta D_{i,t}^{-4} + \beta_0 D_{i,t}^0 + \beta_2 D_{i,t}^2 + \gamma X_i + \epsilon_i \tag{8}$$

Y_j is the outcome variable for student or school principal j. $\alpha_{s(j)}$ and $\lambda_{t(j)}$ are school and year fixed effects, respectively. X_j are control variables (e.g., student's age, student's gender). The rest of the variables are analogous to those in equation (1). Observations more than three years before or more than three years after the audit are captured by dummies $D_{i,t}^{-3} = \mathbb{1}(t - T_i^* \leq -3)$ and $D_{i,t}^{3} = \mathbb{1}(t - T_i^* \geq 3)$, respectively. ϵ_i is an error term clustered at the municipality level.

The endpoints are different from the main specification since I only observed data points in 2007, 2009, 2011, 2013, and 2015. Given that the Prova Brasil data are biannual, to have a balanced panel of audited municipalities, I only include event-year dummies for -3, 1, and 3; a full set of dummies (-1 is omitted).