Audits and Bureaucratic Corruption: Evidence from Brazilian Municipalities *

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Abstract

Corruption is often a collective endeavor involving both politicians and bureaucrats, yet most research on anti-corruption policies focuses on disciplining politicians. We examine how audits affect bureaucrats using a randomized audit program of Brazilian municipalities. We establish that audits effectively punish corrupt bureaucrats: they increase dismissals and resignations, with effects concentrated among likely patronage hires. Leveraging a theoretical framework that views mayors as bureaucratic supervisors, we show that the mayor's decision to dismiss is driven by legal rather than electoral accountability concerns. Our findings have implications for the design of anti-corruption policies and suggest that while patronage appointments come with rents, they are also exposed to greater punishment.

JEL Codes: D73, H83, K42, J45
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1 Introduction

Corruption imposes significant economic and political costs, undermining both government legitimacy and economic growth (e.g., Rose-Ackerman and Palifka, 2016; Rothstein, 2011; Fisman and Svensson, 2007). To combat corruption, governments worldwide have adopted policies designed to detect and deter corrupt practices by public officials (Chen and Kung, 2018). Among these policies, audits have proven particularly effective at uncovering and sanctioning corrupt behavior (Nyblade and Reed, 2008; Ferraz and Finan, 2011; Bobonis et al., 2016; Gans-Morse et al., 2018).

Corruption, however, is rarely an individual action; it is often a collective endeavor involving both politicians and bureaucrats. Successful corruption schemes typically rely on bureaucrats' knowledge of public administration and rent-seeking opportunities (Ferrali, 2020; Larson, 2021; Di Tella and Schargrodsky, 2003; Ehrlich and Lui, 1999). In Brazil, from 2009 to 2020, bureaucrats accounted for 53% of public officials indicted for administrative irregularities, compared to 47% for mayors.[1] This highlights the need to evaluate anti-corruption policies with respect to bureaucrats, not just politicians.

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[1] Authors' calculations based on data from the Cadastro Nacional de Condenações Civis por Ato de Improbidade Administrativa e Inegibilidade, Conselho Nacional de Justiça.

While the effects of audits on politicians are well-documented, their impact on bureaucrats remains less understood. This question is particularly important because bureaucrats differ significantly from private-sector employees. They typically enjoy greater job security, which should, in principle, reduce their incentives for opportunistic behavior. Additionally, their wages are generally fixed, meaning that standard mechanisms such as performance-based pay cannot be employed to curb opportunistic actions. Further complicating the issue, politicians–who serve as bureaucrats' managers–differ from private-sector managers in that they are accountable to two distinct principals: citizens (political accountability) and enforcement agencies (legal accountability). Existing research demonstrates that audits reinforce both forms of accountability by uncovering evidence of corruption, thereby facilitating punishment by voters (Ferraz and Finan, 2008) and legal sanctions by enforcement agencies (Avis et al., 2018). However, how these dual accountability mechanisms shape bureaucrats' career trajectories remains an open question. This issue is particularly complex in developing countries, where corruption is more pervasive, and bureaucratic appointments are often made through patronage (Colonnelli et al., 2020; Toral, 2024a). Moreover, bureaucrats in these settings tend to have less secure employment contracts, further distinguishing their incentives and career dynamics from those of their counterparts in more developed settings.

This paper investigates the impact of anti-corruption audits on bureaucratic corruption in Brazil. Our objectives are twofold: (i) to evaluate whether audits effectively discipline corrupt bureaucrats and (ii) to analyze the mechanisms underlying mayoral punishment behavior and bureaucrats' responses.

We examine Brazil's municipal audits program, administered by the Comptroller General (CGU), an independent federal agency. This program offers several advantages: Brazil's decentralized governance and moderate corruption levels[2], the program's proven efficacy in reducing corruption (Avis et al., 2018), and the availability of rich micro-level data. We combine municipal audit data with employment records for over 1.1 million high-ranking municipal bureaucrats.[3] These records include individual characteristics, employment details, and party membership, allowing us to identify co-partisanship with mayors. The program's randomized, staggered roll-out enables causal identification of the audits' effects on bureaucratic career outcomes, as well as an exploration of temporal, institutional, and individual heterogeneity.

First, we find that audits effectively punish corrupt bureaucrats. On average, audits increase career interruptions (+5.6%) and slow wage progression. The heterogeneity of these effects is consistent with punishment of bad actors rather than scapegoating of good actors. First, the increase in career interruptions is driven by increased dismissals (+11.8%), rather than increased resignations, indicating punishment actions undertaken by the mayor. Second, punishment is stronger in more corrupt municipalities – as measured by the amount of corruption uncovered by audits. Third, at the individual level, bureaucrats likely hired through patronage – such as mayors' co-partisans, bureaucrats earning significantly more than their private-sector wage estimate,[4] or those with temporary contracts[5] – experience more negative career outcomes. Third, bureaucrats hired after the audit, who could not have been involved in the revealed corruption, experience fewer negative effects. However, audits are less effective in highly corrupt municipalities, where entrenched corruption may enable circumvention of sanctions.

Having shown that audits increase bureaucrats' dismissal rates, we examine the mechanisms underlying the mayor's decision to dismiss. Our second finding rationalizes bureaucrats' dismissals as the mayor's response to legal rather than electoral incentives. We develop a theoretical framework inspired by Tirole (1986) and Faure-Grimaud et al. (2003), that predicts that legal and political accountability have divergent implications for dismissals. Under electoral

[2]Brazil ranks 96th out of 180 countries in Transparency International's 2021 Corruption Perceptions Index.

[3]We focus on mayoral appointeess, the first-layer of accountability.

[4]We estimate counterfactual private-sector wages using our rich employment data.

[5]Temporary contracts in Brazilian municipalities are often used for patronage (Toral, 2024a).

accountability, first-term mayors – who can seek reelection – should impose harsher punishments than second-term mayors – who cannot seek reelection, – to offset electoral damage. Under legal accountability, second-term mayors should punish more severely to preempt legal sanctions upon leaving office. Consistent with legal accountability, our evidence shows that second-term mayors punish more and that punishment mitigates legal risks. We also rule out alternative mechanisms, demonstrating that the observed dismissals are not driven by selection effects, learning effects, or responses to federal budget cuts.

We make three main contributions. First, we contribute to a literature on the economics of corruption in organizations. In organizations, corruption requires agents to collude with their peers (Ferrali, 2020) or with their manager (Tirole, 1986; Faure-Grimaud et al., 2003) against their principal. Sánchez De La Sierra et al. (2024) have examined this issue empirically by considering a pure bureaucracy (the Kinshasa traffic police agency), where both the agent and the manager are bureaucrats. We extend this analysis to political hierarchies, where the manager is a politician. In this context, there are two principals: citizens (political accountability) and enforcement agencies (legal accountability). Both principals are constrained in the kinds of contracts that regulate their relationships with the agent and her manager, and thus cannot fully prevent corruption.

Second, we contribute to the literature on the mechanisms through which anti-corruption audits enable accountability. For mayors, existing research has shown that audits strengthen both political (Nyblade and Reed, 2008; Ferraz and Finan, 2011; Bobonis et al., 2016) and legal accountability (Avis et al., 2018). Bureaucrats, however, remain a relatively new area of interest. Prior studies on the same audit program focus on hiring practices, finding that audits improve the quantity and quality of hires through political accountability (Gonzales, 2022; Lauletta et al., 2022; Santos and Leon, 2024). Our study diverges in two key ways. First, we focus on bureaucratic retention rather than selection, allowing us to jointly analyze mayors' actions (e.g., dismissals) and bureaucrats' actions (e.g., resignations). Second, instead of considering all bureaucrats, we concentrate on high-level bureaucrats, who are more exposed to corruption and serve as the first layer of accountability. Our findings complement this existing literature: while previous studies document the hiring of better bureaucrats, especially among frontline service providers, we document the punishment of bad bureaucrats. Our results reveal a tradeoff in how punishment operates. Audits are more effective at disciplining politicians who are eligible for reelection, as they are subject to both electoral and legal accountability. However, audits are also more effective at disciplining bureaucrats serving under politicians who are not eligible for reelection, as their shorter time-horizon amplifies the pressure from legal accountability mechanisms.

Finally, we contribute to a broader debate on the relationship between politicians and bureaucrats. Previous research highlights the dynamics between these actors (Gulzar and Pasquale, 2017; Toral, 2024b; Brierley et al., 2022) and the limitations of accountability mechanisms (Grossman and Slough, 2022). There is substantial evidence that patronage appointments grant individuals privileged positions within bureaucracies (Colonnelli et al., 2020; Toral, 2024a). However, we show that these ties can also become liabilities: patronage appointees are more likely to face punishment following audits. This suggests that patronage appointments involve a tradeoff, as their privileged status makes them particularly vulnerable to accountability measures.

The paper is structured as follows. Section 2 provides details about the institutional context and the data used for municipal governments, bureaucracies and anti-corruption audits in Brazil. Section 3 documents our motivating stylized fact; namely, that audits successfully discipline corrupt bureaucrats, increasing their dismissal and resignation rates. Section 4 analyzes the mechanisms underlying the mayor's decision to dismiss bureaucrats. Section **??** analyzes the mechanisms underlying bureaucrats' decision to resign. Section 5 concludes.

2 Context and Data

2.1 Municipal Governments and Bureaucracies

Data. Our analysis of municipal bureaucracies leverages employer-employee data from the Relação Anual de Informações Sociais (RAIS), an annual micro-level census of all formal sector employees—both private and public—collected by the Brazilian Ministry of Labor.[6] Each year, municipalities are legally required to report detailed information on all employees, from cleaning staff to municipal cabinet members. Non-compliance or misreporting can result in legal sanctions and fines.

RAIS provides rich data on individual employees and their employment contracts, including age, wages, work experience, education level, and contract type. It also includes information on bureaucrats' occupational roles and seniority, and career outcomes such as dismissals, resignations, and hires. Our dataset covers the period from 2003 to 2015 for all municipalities included in the randomized audit program (see Section 2.2 for details). We further link this dataset with party membership records from the Supreme Electoral Court (TSE), enabling us to identify the partisan affiliations of municipal bureaucrats.

Institutional context. Brazil has a federal system of government with three levels—federal, state, and municipal—each with its own bureaucracy. Municipal governments, the first tier of government, comprise over 5,000 municipalities and employ more than half of Brazil's total bureaucratic workforce across all levels of government.[7] Most municipalities are small: as of 2021, 87% had fewer than 50,000 inhabitants.[8] Each municipality has an elected executive (mayor) and a legislative branch (city council), with elections held every four years. Mayors can serve a maximum of two consecutive terms but may run for office again after a mandatory cooling-off period of one term.

The 1988 decentralization reforms granted municipalities significant autonomy, giving mayors broad discretion over the hiring, dismissal, and wage-setting of municipal employees (Abrucio and Couto, 1996; Falleti, 2010). While Brazil's federal bureaucracy is characterized by competitive entry exams and strong job protections (Grindle, 2012; Pereira, 2001), municipal bureaucracies operate under labor contracts that offer protections that are comparable to the private sector.

Municipal employment is divided into two main contract types:

1. **Temporary contracts** (*temporário*), typically annual, governed by private-sector labor law.

2. **Permanent contracts** (*estatutário*), which may be subject to a civil service statute requiring a written exam for selection and offering some employment protections. However, these protections vary by municipality and do not fully insulate bureaucrats from dismissal.

Political dynamics and patronage. Mayors' broad discretion over personnel decisions has made municipal bureaucracies particularly susceptible to patronage and clientelism, especially in smaller and poorer municipalities (Brollo et al., 2017; Colonnelli et al., 2020). While patronage occurs at all levels, high-level bureaucrats—whom we analyze in this paper (see Section 2.4 for precise definitions)—are especially vulnerable. Figure 1 highlights politically driven turnover patterns for this subsample, with clear spikes around election periods.

[6] The RAIS dataset has been widely used in applied research on municipal bureaucracies (Brollo et al., 2017; Colonnelli et al., 2020; Akhtari et al., 2022; Toral, 2024b).

[7] Authors' calculations based on RAIS data.

[8] Authors' calculations based on census data.

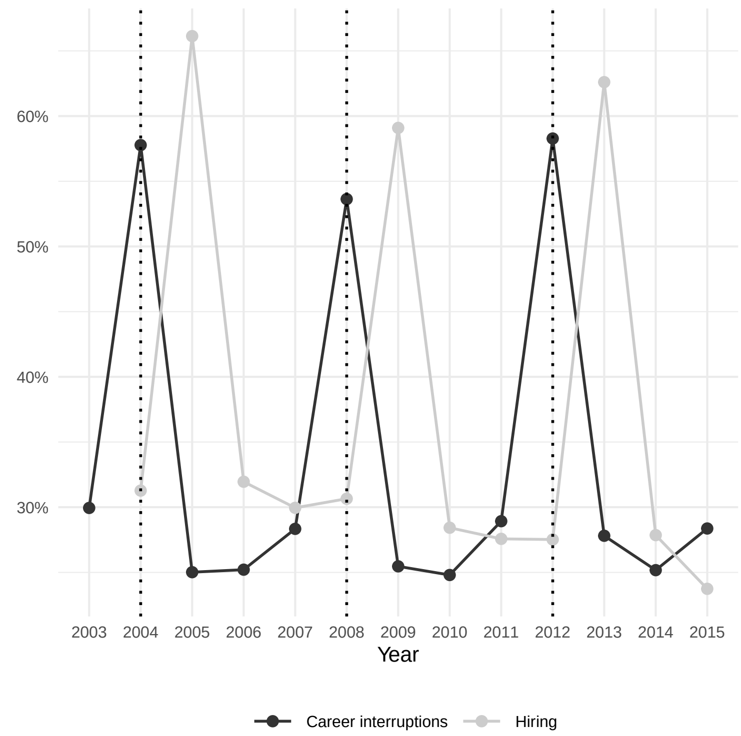

Figure 1: **Bureaucratic turnover over time.** The solid lines represent average career interruption (i.e., resignation and dismissal) and hiring rates for all municipalities in our sample. Dotted lines are election years. We do not report hiring rates in 2003 because the data is inconsistent for that year, owing to changes in reporting practices. There is a spike in turnover around elections.

High-level bureaucrats, including cabinet members, senior managers, and directors, hold key administrative positions within the municipal government and are directly appointed by the mayor. They often receive their positions as rewards for electoral support or personal loyalty. Of course, not all high-level bureaucrats are patronage appointees. Our dataset feature proxies that allow identifying likely patronage beneficiaries (see Section 2.4 for details).

2.2 Federal transfers and Anti-Corruption audits

Institutional context. Municipalities in Brazil rely heavily on federal transfers to fund their daily operations, including payroll expenses. This dependence is especially pronounced in smaller, poorer municipalities, where transfers can constitute over 90% of the local budget (Arretche, 1999; Prado, 2001). These funds are intended to support budget-constrained municipalities in delivering essential public services, such as constructing classrooms or purchasing medical supplies. However, given the broad discretion granted to local governments in managing these resources, instances of administrative mismanagement and outright corruption are widespread (Ferraz and Finan, 2007).

To mitigate the misuse of federal funds and enhance accountability, the Comptroller General of the Union (CGU) launched a nationwide audit program in 2003. This program employs an annual, publicly conducted state-level lottery to randomly select municipalities for audits. Between 2003 and 2015, selected municipalities were subjected to unannounced inspections, reducing the risk of evidence tampering. Audit teams—comprising 10 to 15 meritocratically recruited auditors—arrive within days of the lottery draw, conducting on-site investigations for approximately two weeks. Importantly, prior research has found no evidence that auditors manipulate reports to favor or target specific municipalities (Ferraz and Finan, 2008).These audits focus on service expenditures and potential irregularities in federally funded programs. In our sample, each audit covers an average of 16 budget items, amounting to approximately $6.7 million or 40.8% of the municipal budget (Table 1).

Although the CGU and its auditors lack direct enforcement power, their findings provide evidence that facilitates accountability. Detected irregularities are reported to the relevant federal ministries, which have the authority to impose sanctions, such as withholding future transfers until corrective actions are taken. Audits also facilitate electoral accountability: revelations of corruption reduce the likelihood of incumbents' reelection, as opposition candidates often leverage audit findings against them (Ferraz and Finan, 2008). Additionally, audits faciitate legal accountability by providing evidence to federal prosecutors and law enforcement agencies, which use these reports to guide investigations and build corruption cases (Avis et al., 2018).

Data. Our analysis of municipal anti-corruption audits relies on data collected and made publicly available by the CGU.[9] Beginning in 2006, the CGU introduced a standardized classification system for audit findings, categorizing irregularities—referred to as faults (falhas)—into three levels of severity:

1. Notices (avisos): Minor procedural issues.
2. Intermediate faults (falhas intermediárias): Administrative mismanagement without clear evidence of intentional wrongdoing.
3. Serious faults (falhas graves): Clear-cut cases of corruption involving intentional abuse of public office for private gain.

To validate this classification, we conducted a manual review of audit reports from a randomly selected sample of 30 municipalities, analyzing all recorded faults. We use the number of serious faults as our primary measure of corruption. These infractions typically involve over-invoicing, bid rigging, and embezzlement schemes such as the use of ghost employees. Appendix A.1 provides examples and the distribution for examples and a detailed breakdown (Figure A1).

2.3 Additional Data

We integrate supplementary data from multiple sources. Electoral outcomes are obtained from the Supreme Electoral Court (TSE), which provides information on mayoral characteristics such as incumbency status, age, gender, and education level. We use electoral data from the Base dos Dados initiative. Municipal budget data for the period 2006-2015 is sourced from Finanças Brasil (FINBRA), while demographic information is drawn from the 2001 census, compiled by the National Institute of Geography and Statistics (IBGE).

2.4 Sample, Outcomes, and Descriptive Statistics

Our analysis centers on high-level bureaucrats, which we define as the bureaucrats that are directly appointed by the mayor. This group includes members of the mayoral cabinet (e.g., Secretary of Education) as well as senior managers and directors.[10] We focus on these bureaucrats because they constitute the first layer of accountability beneath the mayor, playing a critical role in high-level decision-making regarding public resources. Table 1 provides descriptive statistics for our sample, which includes approximately 1.1 million bureaucrats.

[9] We are grateful to Fernanda Brollo for sharing historical data on the program for the 2003-2006 period.

[10] We define this group based on Brazilian Occupation Codes (CBO) 1112 to 1115, which correspond to "General directors of public administration." This classification closely follows Colonnelli et al. (2020).

Most municipalities are small, with a median of 13 high-level bureaucrats, though a few larger municipalities drive the mean cabinet size up to around 60 employees.

To evaluate the potential sanctions faced by bureaucrats, we analyze both career interruptions and wage progression. Let n_{jt} denote the total number of bureaucrats employed in municipality j during year t, and let i_{jt} represent the number of career interruptions. We define the *career interruption rate* as $\frac{i_{jt}}{n_{jt}}$.

Career interruptions can arise for various reasons, ranging from routine events—such as retirements or the expiration of temporary contracts—to more deliberate actions, including dismissals and resignations. To better capture the different drivers of career interruptions, we separately consider the *dismissal rate* and the *resignation rate*, which reflect the role of mayoral decisions and bureaucratic agency, respectively.

In addition to career interruptions, we examine wage progression. Let N_{jt} represent the set of employees in municipality j in year t, and let w_{ijt} denote the wage of employee i in municipality-year t. We define the average (log) wage progression in municipality j during year t as $y_{jt} = \frac{1}{|N_{jt} \cap N_{j,t-1}|} \sum_{i \in N_{jt} \cap N_{j,t-1}} \log \frac{w_{ijt}}{w_{ij,t-1}}$, where the denominator accounts for the set of bureaucrats who were employed in both year $t-1$ and year t. This measure captures the typical wage growth among continuing bureaucrats and allows us to assess whether audits influence earnings trajectories over time.

Bureaucratic careers in our sample tend to be relatively short, diverging from the lifelong tenure associated with Weberian bureaucrats. The expected tenure is 3.5 years, with 62.6% of bureaucrats serving for only a single term and 85.1% serving at most two terms. In the average municipality, 33.4% of high-level bureaucrats experience a career interruption, with 6.9% being dismissed and 5.4% resigning. The remaining 21.1% depart for other reasons, including termination of a temporaty contract or retirement. Among those who remain in office, real wages increase by an average of 1% per year.

Although audits are designed to uncover corruption, they occur infrequently. The annual probability of an audit is only 2.3%. However, when audits do take place, they have substantial scope: the scrutinized programs account for 61.3% of a municipality's budget. On average, audits uncover eight serious faults, and over 80% of audited municipalities exhibit at least one serious fault (see Section 2.2 for further details).

Our dataset also includes various individual-level characteristics such as gender, age, education, and tenure. The average bureaucrat is 40 years old, is predominantly male, and often lacks higher education.

Beyond standard demographics, we identify three key indicators that may capture patronage relationships with the mayor. First, we determine whether a bureaucrat is a *co-partisan* of the mayor, expecting this to signal political loyalty and potential patronage ties. Second, we estimate each bureaucrat's *counterfactual private-sector wage* within the same municipality using data from RAIS, employing a high-dimensional model with variable selection to predict private-sector wages based on age, gender, race, education, and municipal fixed effects (see Appendix A.2 for methodological details). Comparing actual public-sector wages to predicted private-sector wages allows us to identify overpaid and underpaid bureaucrats, with overpayment serving as a potential indicator of patronage appointments. Third, we examine *contract type*, distinguishing between permanent and temporary contracts. Temporary contracts are frequently used for hiring campaign supporters and serve as a common measure of patronage in the literature (Colonnelli et al., 2020; Toral, 2024a). While temporary contracts are prevalent at lower levels of the bureaucracy, they are relatively rare in our sample, averaging 7.2% in any given municipality-year. We posit that for higher-level, better-paid positions, patronage appointments are more likely to involve permanent contracts, as they provide longer-term rewards to key political supporters. Overall, while the majority of bureaucrats are overpaid relative to the private sector, both temporary contracts and co-partisanship remain relatively uncommon, each accounting for less than 10% of our sample.

Variable	Value
Outcomes	
Career interruptions	0.334
Dismissals	0.069
Resignations	0.054
Δ wages	0.010
Corruption	
Yearly audit probability	0.023
Amount audited as pct. of local expenditures	0.613
Number of serious faults	8.093
Individual characteristics	
Pct. females	0.411
Pct. co-partisans	0.098
Pct. overpaid	0.584
Age	41.712
Pct. higher education	0.308
Pct. permanent contracts	0.927
Expected tenure (in years)	3.518
Sample size	
N municipalities	5,108
N bureaucrats	1,132,006
N bureaucrats per municipality (mean)	60
N bureaucrats per municipality (median)	13
N municipality-year	53,999
N individual-year	3,411,500

Table 1: **Descriptive statistics.** This table reports descriptive statistics for high-level bureaucrats in all municipalities in our sample from 2003-2015 (see Section 2.4 for a definition of high-level bureaucrats). Unless otherwise specified, all measures are municipality-year averages. Career interruptions are the sum of dismissals and resignations among high level bureaucrats. Δ wage is the yearly wage progression rate. Over-paid is a binary variable that equals one if the bureaucrat's wage is above her counterfactual wage in the private sector (see Appendix A.2). Higher education refers to holders of a college degree or higher.

To assess whether these three indicators–co-partisanship, overpayment, and temporary contracts–indeed capture patronage ties, we first show that all they negatively correlate with public good provision in the health and education sectors (Appendix, Table A1). We then conduct an exploratory factor analysis that includes two placebo variables, gender and education, which should not be correlated with patronage. As detailed in Appendix A.3, our results indicate that the data is best described using two factors. Our patronage indicators all load more heavily on the first factor, while the placebo variables all load more heavily on the second factor. This suggests that our patronage indicators capture a distinct latent construct that is not explained by the placebo variables. Based on these findings, we construct a patronage index using the first factor.

3 Stylized Facts

We show that audits effectively punish corrupt bureaucrats. We first demonstrate that, on average, audits increase career interruptions and curtail wage progression. However, these findings alone are not sufficient to establish that audits serve as punishment. Following an audit that unveils corruption, honest bureaucrats might instead be scapegoated. To address

	Career interruptions	Dismissals	Resignations	Δ wages
Post-audit	0.017**	0.008**	0.004	−0.014***
	(0.007)	(0.003)	(0.003)	(0.005)
R^2	0.312	0.284	0.296	0.102
N	53 922	53 922	53 922	44 847
Mean outcome (control)	0.333	0.068	0.055	0.011

* p <0.1, ** p <0.05, *** p <0.01

Table 2: **Municipal-level analysis at the yearly level.** OLS estimates of Equation (1). Audits trigger an increase of career interruptions, evidenced by an increase in both dismissals and resignations. All models include municipality and year fixed effect. Standard errors are clustered at the municipality level in parenthesis.

this concern, we analyze the heterogeneity surrounding these average effects and find evidence more consistent with audits targeting and punishing corrupt bureaucrats than with alternative explanations.

Methods. We estimate the average treatment effect of random audits on outcomes of interest at the municipal level. These outcomes include career interruptions, dismissal rates, resignation rates, and average log wage progression (see Section 2 for details). Because audits are randomized, our setting constitutes a staggered roll-out design with randomization. In this design, the standard two-way fixed effects (TWFE) estimator recovers the average causal effect (Athey and Imbens, 2022). Our main specification is:

$$y_{jt} = \alpha_j + \alpha_t + \beta t_{jt} + \epsilon_{jt}, \tag{1}$$

where y_{jt} is the outcome measured in municipality j during year t, α_j and α_t are municipality and period fixed effects, respectively, t_{jt} is an indicator variable equal to 1 for the period t^* during which municipality j was audited and for all subsequent periods $t' \geq t^*$, and 0 otherwise. Finally, ϵ_{jt} is a random error term clustered at the municipal level. Our parameter of interest, β, captures the causal effect of being audited, averaged over all years following the audit. We subject this specification to a series of robustness tests, which we discuss at the end of this section.

Audits trigger career interruptions. Table 2 demonstrates that audits trigger career interruptions. On average, audits increase career interruptions by 1.7 percentage points (5.4%) in all subsequent time periods (column 1). These career interruptions arise from increased dismissals, but not from increased resignation rates (columns 2 and 3). Additionally, audits reduce wage progression by 1.4% (column 4).

Having established that audits trigger career interruptions, we now examine heterogeneity in these effects. Specifically, we show that the observed patterns are more consistent with audits punishing corrupt bureaucrats than with alternative explanations.

Punishment is stronger in more corrupt municipalities. To analyze heterogeneity with respect to pre-audit corruption levels, we modify the model in Equation (1). The audit data report the quantity of corruption uncovered during each audit. Because corruption is not measured in untreated municipalities, we restrict the analysis to treated municipalities. We measure corruption as the number of serious faults uncovered in an audit (see Section 2.2).[11]

[11] The criteria examined by audits have varied over time. To create a time-invariant measure of corruption, we adjust the number of serious faults by subtracting the yearly mean number of faults uncovered. Specifically, let f_{jt} represent the number of serious faults uncovered in municipality j in year t, and let $\overline{f}_t$ denote the mean number of faults uncovered across all municipalities audited in year t. Our corruption measure is $f_{jt} - \overline{f}_t$.

	Career interruptions	Dismissals	Resignations	Δ wages
Post-audit (β_1)	0.005	−0.005	0.002	0.002
	(0.014)	(0.007)	(0.006)	(0.009)
Post-audit × Med. corruption (β_2)	0.038**	0.018**	0.009	−0.025**
	(0.017)	(0.008)	(0.007)	(0.012)
Post-audit × High corruption (β_3)	0.014	0.007	0.000	−0.012
	(0.018)	(0.008)	(0.007)	(0.011)
R^2	0.301	0.286	0.297	0.103
N	9674	9674	9674	8020
Mean outcome (control, low corruption)	0.330	0.064	0.066	0.020
Mean outcome (control, med. corruption)	0.329	0.064	0.049	0.022
Mean outcome (control, high corruption)	0.366	0.049	0.032	0.028
$\beta_1 + \beta_2$	0.044***	0.014*	0.011*	−0.023**
	(0.015)	(0.008)	(0.006)	(0.011)
$\beta_1 + \beta_3$	0.019	0.002	0.003	−0.010
	(0.017)	(0.008)	(0.006)	(0.009)

* p <0.1, ** p <0.05, *** p <0.01

Table 3: **Heterogeneity with respect to pre-audit corruption levels.** OLS estimates of Equation (1) when adding an interaction term for corruption levels. The sample is restricted to audited municipalities. Dependent variables are in percentage terms. Corruption is a categorical variable based on the number of serious irregularities discovered during audits. The first trecile is the reference category. The effect of audits is concentrated in municipalities that exhibit moderate corruption levels. All models include municipality and year fixed effect. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

We then group treated municipalities into terciles based on corruption levels, creating groups of low, medium, and high-corruption municipalities.

To estimate treatment effects by corruption levels, we reestimate Equation (1) with an interaction term between treatment status and corruption levels. The inclusion of municipal-level fixed effects ensures that the estimated effects are not confounded by differences in pre-treatment outcome levels across corruption groups.

Table 3 shows that the effects of audits are concentrated in municipalities with intermediate levels of corruption, suggesting that audits are effective within certain bounds. As should be expected of an effective program, audits prompt no response in municipalities where little corruption is uncovered. However, audits also fail to prompt a response in high-corruption municipalities, indicating that these highly corrupt environments may successfully evade punishment. We discuss potential mechanisms underlying this last pattern in Section 4. We show that our results are robust to extending the sample to include non-audited municipalities and to using alternative measures of corruption (Appendix B.1).

Punishment is stronger for likely-corrupt bureaucrats. We now examine individual-level data and show that audits disproportionately punish likely patronage appointees compared to unlikely patronage appointees, and bureaucrats who may have been involved in the corruption uncovered by the audit compared to those who could not. To do so, we estimate a discrete-time proportional hazard model using logistic regression:

$$y_{ijt\tau} = \alpha_j + \alpha_t + \alpha_\tau + t'_{ijt}\beta + x'_{it}\gamma, \tag{2}$$

	Logit			OLS
	Career interruption	Dismissals	Resignations	Δ wages
Joined before the audit (β_1)	0.133**	0.216**	0.128	−0.006
	(0.052)	(0.087)	(0.082)	(0.004)
Joined after the audit (β_2)	0.085	0.149	0.048	−0.007*
	(0.056)	(0.093)	(0.092)	(0.004)
R^2	0.950	0.976	0.978	0.073
N	2 098 158	2 098 158	2 098 158	2 098 158
$\beta_1 - \beta_2$	0.049	0.067	0.080	0.001
	(0.038)	(0.063)	(0.062)	(0.003)

* p <0.1, ** p <0.05, *** p <0.01

Table 4: **Individual-level analysis.** Logistic regressions (models 1-3) report log-odds ratios of Equation (2). Column 4 reports OLS estimates of Equation (2). Audits trigger waves of dismissals that affect both bureaucrats who joined before the audit and those who joined afterwards. Only bureaucrats that joined before the audit see a significant increase in resignation rates. All models include tenure, municipality, and year fixed effects, and control for gender, age, age squared, education and contract type. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

where $y_{ijt\tau}$ is a binary outcome measured for individual i in municipality j during period t, corresponding to τ periods of tenure. We consider three binary outcomes that align with our aggregate career interruption measures: $y_{ijt\tau} = 1$ if individual i (a) experienced a career interruption, (b) was dismissed, or (c) resigned during period t, and $y_{ijt\tau} = 0$ otherwise. Consistent with our aggregated models (Equation (1)), we include municipality and year fixed effects α_j and α_t, and we model the hazard rate non-parametrically with tenure fixed effects α_τ.

To minimize unobserved heterogeneity, we include a vector of covariates x_{it}, comprising time-invariant characteristics (gender, a binary indicator for higher education, and contract type distinguishing between temporary and permanent contracts) and mechanically varying characteristics (a second-order polynomial for age). For consistency with our aggregate outcomes, we also consider log wage progression as an outcome, estimating Equation (2) using OLS.

The treatment variable t_{ijt} is a categorical indicator with the following categories: (a) bureaucrats who joined a treated municipality before the audit, and (b) bureaucrats who joined after the audit. The reference category includes bureaucrats in never-treated municipalities and bureaucrats in treated municipalities prior to the audit. This specification tests whether employees who joined after the audit—who could not have been implicated in the corruption uncovered—experience less severe consequences than those who joined before the audit. To ensure consistency with our aggregate-level results, we weight the sample such that each municipality-year receives equal weight.

Table 4 shows that bureaucrats who joined before the audit experience significantly higher career interruption rates, concentrated among increased chances of dismissal, and slower wage progression post-audit. Conversely, those who joined before the audit see no satistically significant change in ther career outcomes, as compared to never-audited bureaucrats. These findings suggest that audits only punish bureaucrats potentially involved in corruption (i.e., those who joined before the audit) amd do not affect bureaucrats that could not have been involved in the corruption uncovered by the audit.

We extend Equation (2) by interacting a moderator with each treatment category to test whether audits disproportionately punish likely patronage appointees. Moderators include

	Co-partisan	Over-paid	Temporary	Index	Female	Graduate
Joined before the audit (β_1)	0.200**	0.103	0.183**	0.181**	0.249***	0.181**
	(0.089)	(0.096)	(0.089)	(0.088)	(0.091)	(0.091)
Joined after the audit (β_2)	0.156*	0.197**	0.137	0.165*	0.166*	0.176*
	(0.093)	(0.098)	(0.094)	(0.094)	(0.094)	(0.094)
Moderator × Joined before (β_3)	0.118	0.188**	0.483**	0.103***	−0.088	0.104
	(0.120)	(0.076)	(0.217)	(0.037)	(0.063)	(0.074)
Moderator × Joined after (β_4)	−0.099	−0.073	0.207	−0.039	−0.043	−0.081*
	(0.090)	(0.053)	(0.171)	(0.026)	(0.042)	(0.048)
R^2	0.976	0.976	0.976	0.976	0.976	0.976
RMSE	0.234	0.234	0.234	0.234	0.234	0.234
N	2 098 158	2 098 158	2 098 158	2 098 158	2 098 158	2 098 158
$\beta_1 + \beta_3$	0.318**	0.290***	0.666***	0.284***	0.160*	0.285***
	(0.137)	(0.095)	(0.219)	(0.092)	(0.094)	(0.100)
$\beta_2 + \beta_4$	0.057	0.124	0.344*	0.127	0.123	0.095
	(0.126)	(0.096)	(0.187)	(0.096)	(0.097)	(0.100)
$\beta_1 - \beta_2$	0.044	−0.094	0.046	0.016	0.083	0.005
	(0.064)	(0.074)	(0.065)	(0.062)	(0.069)	(0.068)
$(\beta_1 + \beta_3) - (\beta_2 + \beta_4)$	0.261*	0.166**	0.322*	0.158**	0.038	0.190**
	(0.134)	(0.074)	(0.185)	(0.071)	(0.074)	(0.083)

* p <0.1, ** p <0.05, *** p <0.01

Table 5: **Individual-level analysis, heterogeneity.** Each model uses dismissals as an outcome. The column header refers to moderator = 1. For instance, in column 1, moderator = 1 for co-partisans and moderator = 0 for non co-partisans. The "Index" moderator corresponds to the scores associated with the first factor of an exploratory factor analysis (see Figure A2 for details). Audits increase dismissals for over-paid and temporary bureaucrats that joined before the audit, while female and graduate bureaucrats exhibit no such differential effects, consistent with punishment of likely patronage hires. Audits decrease dismissals for graduates that joined after the audit (parameter β_4), consistent with broader improvements in HR practices. Estimates are log-odds ratios of logistic regression models. All models include tenure, municipality, and year fixed effects, and control for gender, age, age squared, education and contract type. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

indicators for overpayment, co-partisanship, and temporary contracts, as well as an index of the three constructed from the first factor of an exploratory factor analysis (see Section 2.4 for a discussion of our validation of these indicators). We compare these moderators with placebo moderators–specifically gender and education–that are less likely to be indicative of clientelistic relationships.

Table 5 focuses on dismissal events, revealing that characteristics likely associated with patronage appointments–co-partisanship, overpayment, and temporary contracts–are significantly correlated with stronger punishment effects (β_3 significantly different from 0). The findings are robust when using the index derived from factor scores (column "Index"). Conversely, placebo moderators (gender and university education) do not show significantly larger punishment effects (β_3 not significantly different from 0). Moreover, audits affect bureaucrats who joined after the audit ($\beta_2 > 0$), suggesting that they induce broader, more long-term changes in HR practices. For these bureaucrats, effects are not significantly different for likely the effects are significantly larger for likely patronate appointees ($\beta_4 = 0$), while graduates experience comparatively longer careers ($\beta_4 < 0$). Additioanlly, consistent with punishment effects, likely patronage appointees who joined before the audit experience stronger punishment than similar patronage appointees who joined after the audit ($\beta_1 + \beta_3 > \beta_2 + \beta_4$, while $\beta_1 = \beta_2$).

These findings suggest that while audits have both punitive and long-term effects, they disproportionately target likely patronage appointees. Overall, audits appear to be somewhat effective in punishing corrupt bureaucrats. On one hand, they trigger dismissals that disproportionately affect likely patronage hires. On the other hand, they induce broader changes in HR practices, somewhat increasing dismissals for bureaucrats who joined after the audit. However, audits fail to prompt punishment in highly corrupt municipalities. We discuss this in Section 4.2.

Robustness. We first verify that our conclusion—that audits effectively root out corrupt bureaucrats—holds in a broader context (Appendix B.1). Specifically, we examine whether bureaucrats who experience career interruptions re-enter high-level bureaucratic positions elsewhere in the country or at any level of government. Less than 8.5% of these individuals return to the bureaucracy, a proportion that remains relatively constant over time. This pattern does not vary significantly based on whether the career interruption occurred before or after an audit, nor does it depend on whether the bureaucrat was dismissed, resigned, or interrupted her career for other reasons (Figure A3). Additionally, we confirm that bureaucrats who leave are not systematically replaced by new hires. Re-estimating our main specification (Equation (1)) with the percentage of new hires and the (log) number of employees as outcomes, we find no significant effect of audits on hiring rates, and the overall number of employees remains constant (Table A4). In other words, the personnel reductions we observe are too small to meaningfully affect the size of the bureaucracy.

We then demonstrate that our main result–namely, that audits induce career interruptions (Table 2)–is robust to alternative model specifications. In Appendix B.3, we show that the results hold when using monthly data instead of yearly data (Table A5) and when using log counts as the dependent variable instead of percentages (Table A6).

We also estimate a dynamic specification to examine the persistence of treatment effects, showing that the effects are long-lasting and consistent in magnitude with alternative staggered difference-in-differences estimators proposed by De Chaisemartin and d'Haultfoeuille (2024), Borusyak et al. (2021), and Callaway and Sant'Anna (2021) (Figure A4). This setting benefits from two key features that mitigate bias: the randomization of audits ensures comparability between treated and untreated units (see Table A7 for balance tests), supporting the parallel trends assumption, and the rarity of audits provides a large pool of never-treated units, ensuring a well-defined control group.

However, standard estimation methods for dynamic treatment effects do not account for the unique aspects of our setting, particularly the influence of the electoral calendar. Indeed, treatment effects vary depending on whether elections occur in the interim and on their outcomes. Figure 2 reports estimates where treatment effects vary as a function of the number of terms elapsed since the audit. Effects on wage progression and career interruptions are long-lasting, spanning two and three terms, respectively. The effect on dismissals is largely instantaneous, with statistically significant effects occurring immediately after the audit. The effect on resignations is more muted, with statistically significant effects occurring two terms after the audit. Note that this specification pools first- and second-term mayors, who face different reelection incentives. We explore this variation in detail in the next section, where we discuss the mechanisms underlying the mayor's decision to dismiss bureaucrats (Section 4).

4 Mechanisms underlying politicians' decision to dismiss

We now investigate the mechanisms underlying the effectiveness of audits. Specifically, we develop a theoretical model to distinguish the empirical implications of two widely studied mechanisms: electoral accountability and legal accountability (Section 4.1). In Section 4.2, we test these mechanisms empirically, demonstrating that legal accountability is the primary driver of audits' effectiveness, while providing evidence against several alternative explanations.

4.1 Theory

The model. We present a theoretical model inspired by the literature on supervision in mechanism design settings (Tirole, 1986; Faure-Grimaud et al., 2003). The game involves three players: a bureaucrat (she), a mayor (he), and a legal enforcer (it). The mayor acts as the supervisor in the relationship between the enforcer and the bureaucrat, using the information he gathers to potentially dismiss the bureaucrat, thereby sparing the enforcer the costs of an investigation. However, as highlighted in the literature, this supervisory advantage introduces the risk of collusion between the bureaucrat and the mayor.

Our objective is to analyze how legal and electoral accountability influence the likelihood of such collusion. To do so, we depart from the mechanism design framework. Indeed, under this framework, the principal may fully prevent collusion by designing an optimal contract. We assume, instead that the contract that governs the relationships between the principal, the agent, and the supervisor is fixed and investigate the extent to which changes in the technology affect outcomes.

Inside each period t, we consider the following timing:

1. The bureaucrat B chooses an amount $q_t \in \{0, 1\}$ of rent to extract. This level is observable to the mayor and the legal enforcer. Whereas the mayor directly obtain hard evidence for it, the legal enforcer does not, reflecting the benefit of supervision.

2. An audit occurs with probability p_A. When audits occur ($a_t = 1$), they reveal corruption to voters and help the enforcer find hard evidence.

3. The bureaucrat chooses the bribe $b_t \in [0, q_t]$ to offer to the mayor.

4. The mayor M chooses whether to retain ($r_t = 1$) or dismiss ($r_t = 0$) the bureaucrat. If he dismisses her, rents (q_t and b_t) are destroyed. If he retains her, he receives the bribe b_t but suffers the intrinsic cost for dishonesty, θ, assumed heterogeneous ($\theta \sim U[0, 1]$) and unobservable, in particular by the bureaucrat.

5. Observing the current level of corruption, the enforcer E chooses whether to investigate ($e_t = 1$) and obtain hard evidence. In case the investigation detects corruption – which occurs with probability λ, – both the mayor and the bureaucrat are fined. Fines are paid when leaving office.

We assume that whereas bureaucrats only serve for one term (consistently with empirical patters, see Section 2.4 and descriptive evidence (Section 2.4)), mayor can serve for up to two periods and discounts the future at rate $\delta \in (0, 1)$. At each period, he enjoys office rents $\rho > 0$. By the end of each period, the mayor is reelected with probability $\epsilon_t(q_t|a_t)$. Since second-term mayors cannot be reelected, we have $\epsilon_2(q_2|a_2) = 0$. First-term mayors may be reelected. As audits provide hard evidence of corruption, they reduce the mayor's reelection prospects if they indeed reveal corruption. As such, we have that $\epsilon_1(q_1|a_1) = \epsilon^0$ if $a_1 = 0$ or $q_1 = 0$ and $\epsilon_1(q_1 = 1|a_1 = 1) = \epsilon^1 < \epsilon^0$ otherwise.

The enforcer wishes to reduce rent extraction. It observes r_t, q_t and b_t, but has no hard evidence of q_t and b_t. The enforcer may trigger an investigation. If he decides to investigate, the investigation is *conclusive* with probability $\lambda(a)$; that is, it produces hard evidence of q_t and b_t. Investigations incur cost c. Audits increase the probability of gathering hard evidence from $\lambda(0)$ to $\lambda(1) > \lambda(0)$. Conclusive investigations lead to the destruction of the rent (i.e., $q_t = b_t = 0$) and to the bureaucrat and the mayor being fined $f_B > 0$ and $f_M > 0$ respectively. With probability $1 - \lambda(a)$, the investigation is inconclusive, in which case it does not produce hard evidence of q_t and b_t. In that event, the rent and bribe are not destroyed, and fines are not levied. Fines are paid by their respective players when they leave office. The assumption is inconsequential for bureaucrats, as they remain in office for only one period. For mayors, this matches the empirical observation that mayors are largely protected from prosecution when in office (Lambais and Sigstad, 2023).

Ex-post flow payoffs thus write

$$\begin{aligned}
u_{B,t} &= q_t r_t[(1 - e_t\lambda(a_t))(1 - b_t) - e_t\lambda(a_t)f_B] \\
u_{M,t} &= \rho + r_t[(1 - e_t\lambda(a_t))b_t - (\epsilon(q_t|a_t)\delta + (1 - \epsilon(q_t|a_t)))e_t\lambda(a_t)f_M - \theta q_t] \\
u_{E,t} &= e_t(\lambda(a_t) - c)
\end{aligned}$$

We make the following assumptions, in order to guarantee an interior solution:

Assumption 1 (Interior solution)**.** Assume that

1. enforcement is moderately capable; i.e., $c < \lambda(a) < \overline{\lambda}$
2. office rents are moderately large; i.e., $\underline{\rho} < \rho < \overline{\rho}$

The exact values of the bounds on parameter values are reported in Appendix C.1.

Moderate enforcement capabilities (assumption 1.1) ensure that the enforcer investigates with an interior probability yet incentivize the bureaucrat to extract rents. Likewise, moderate office rents (assumption 1.2) ensure that some mayors are incentivized to accept bribes yet care about retaining office.

The general case. We first describe equilibrium behavior, solving the game using backward induction. At each time period, after observing q_t, the enforcer sets e_t to maximize its payoff. As such, he chooses $e_0 = 0$ when $q_t = 0$. Assumption 1.1 guarantees that he chooses $e_t = 1$ when $q_t = 1$.

Consider now a mayor of type θ and denote $v_t(\theta)$ his continuation value in period t. Since second-term mayors cannot be reelected, we have $v_2(\theta) = 0$. Recall that a mayor's reelection prospects impact the time period at which he will be paying corruption-related fines (if any). If he is audited and retains the bureaucrat, then his chances of reelection are low. His chances of reelection are high otherwise. Assuming that $q_t = 1$, the mayor's payoff in period t is

$$U_{M,t}(r_t|b_t, a_t, \theta) = \rho + r_t[(1 - \lambda(a_t))b_t - (1 - (1 - \delta)\epsilon(q_t|a_t))\lambda(a_t)f_M - \theta] + \delta\epsilon(q_t|a_t)v_{t+1}(\theta)$$

Thus, for each type of mayor θ, there exists a bribe b_t^* that makes him indifferent between (1) retaining the bureaucrat, thus accepting some corruption and (2) dismissing the bureaucrat, thereby destroying the rent. Bribe b_t^* solves $U_{M,t}(1|b_t^*, a_t, \theta) = U_{M,t}(0|b_t^*, a_t, \theta)$. We obtain

$$b_t^*(\theta|a_t) = \frac{\theta + [1 - (1 - \delta)\epsilon_t(1|a_t)]\lambda(a_t)f_M - \delta[\epsilon_t(0|a_t) - \epsilon_t(1|a_t)]v_{t+1}(\theta)}{1 - \lambda(a_t)} \tag{3}$$

Provided b_t^* is increasing in θ, then, when facing bribe $b_t^*(\theta|a_t)$, mayors of type $\theta' \leq \theta$ accept b_t^* and choose $r_t = 1$, while mayors of type $\theta' > \theta$ reject b_t^* and choose $r_t = 0$. As such, the bureaucrat expects that she will be retained with probability θ at cost $b_t^*(\theta|a_t)$. Notice that b_t^* is indeed increasing in θ for second-term mayors (i.e., $t = 2$), as they have $v_{t+1}(\theta) = 0$. We will show that this also holds true for first-term mayors in the particular cases we consider below.

Since the bureaucrat does not know the mayor's type, she picks $\theta_t^*(a_t)$ that maximizes her expected payoff, conditional on a_t and q_t. Assuming that b_t^* is increasing in θ, she solves

$$\max_{\theta} u_{B,t}(\theta|a_t) \equiv \theta[(1-\lambda(a_t))\,(1-b^*(\theta|a_t)) - \lambda(a_t)f_B] \tag{4}$$

Finally, prior to a_t, the bureaucrat chooses

$$q_t^* = 1 \iff p_A u_{B,t}(\theta_t^*(1)|a_t = 1) + (1-p_A)u_B(\theta_t^*(0)|a_t = 0) \geq 0 \tag{5}$$

In this environment, $\theta_t^*(a_t)$ is an important quantity, as it captures equilibrium the bureaucrat's retention rate at term t. In other words, $1-\theta_t^*(a_t)$ is the probability that the bureaucrat is dismissed in equilibrium. As such, $\theta_t^*(1)-\theta_t^*(0)$ captures the effect of audits on bureaucrats' retention rate.

The legal accountability channel and its empirical implications. We first consider the effect of audits in an environment in which they only facilitate legal enforcement. In other words, we assume that audits have no effect on the mayor's reelection prospects. That is, we assume $\epsilon_t^0 = \epsilon_t^1$. In this context, Equation (3) becomes:

$$b_t^*(\theta|a_t) = \frac{\theta + [1-\epsilon_t(1-\delta)]\lambda(a_t)f_M}{1-\lambda(a_t)}$$

Since b^* is increasing in θ, when facing bribe $b^*(\theta|a_t)$, mayors of type $\theta' \leq \theta$ accept b^* and choose $r_t = 1$, while mayors of type $\theta' > \theta$ reject b^* and choose $r_t = 0$.

The bureaucrat then solves the maximization problem in Equation (4) to choose the optimal bribe and associated retention rate $\theta^*(a_t)$, then assesses the condition in Equation (5) to pin down the optimal quantity of rent q_t^*. The following lemma describes the solution:

Lemma 1. *In equilibrium, the optimal retention rate is given by*

$$\theta_t^*(a_t) = \frac{1-(1+f_B+(1-(1-\delta)\epsilon)f_M)\lambda(a_t)}{2},$$

with $\theta_t^*(a_t) \in (0,1)$ *and* $q_t^* = 1$.

Having characterized equilibrium, we can derive the main empirical implication:

Proposition 1. *Audits increase bureaucrats' dismissal rates; i.e.,* $\theta_t^*(1)-\theta_t^*(0) < 0$. *The effect of audits on bureaucrats' dismissal rates is decreasing in the probability of reelection:* $\frac{\partial|\theta_t^*(1)-\theta_t^*(0)|}{\partial\epsilon_t} < 0$.

Audits increase dismissal rates. Indeed, cheaper enforcement makes punishment more likely, which increases the bribe required to retain the bureaucrat. This, in turn, reduces the range of dishonesty that can support corruption; i.e., this increases dismissals. The effect gets larger as the probability of reelection decreases, as mayors that have slim reelection prospects will face punishment sooner, magnifying the effect of increased punishment. The effect is largest for second-term mayors, since they cannot be reelected, giving Proposition 1 a stark corollary:

Corollary 1. *The effect of audits on bureaucrats' dismissal rates is larger for second-term mayors than for first-term mayors; i.e.,* $|\theta_2^*(1)-\theta_2^*(0)| > |\theta_1^*(1)-\theta_1^*(0)|$.

The electoral accountability channel and its empirical implications. We now consider the effect of audits in an environment in which they only facilitate electoral accountability. That is, they reduce the mayor's electoral prospects conditional on retaining the bureaucrat. In other words, we assume that audits have no effect on the cost of enforcement; i.e., $\lambda(0) = \lambda(1) \equiv \lambda$. Note that electoral accountability may interact with legal accountability by affecting when mayoral fines are paid. We shut down this channel by assuming that corruption caries no legal cost to the mayor; i.e., $f_M = 0$.

In the second period, mayors cannot be reelected (i.e., $\epsilon_2^a = 0$). As such, audits become irrelevant, in the sense that $U_{M,2}(r_2|b_2, 1, \theta) = U_{M,2}(r_2|b_2, 0, \theta)$. Thus, the mayor trades-off expected bribe and dishonesty cost, and at $t = 2$, Equation (3) becomes

$$b_2^*(\theta) = \frac{\theta}{1-\lambda}$$

Since b^* is increasing in θ, mayors of type $\theta' \leq \theta$ accept b^* and choose $r_2 = 1$, while mayors of type $\theta' > \theta$ reject b^* and choose $r_2 = 0$. The bureaucrat picks θ to maximize her expected payoff. We obtain

$$\theta_2^* = \frac{1-(1+f_B)\lambda}{2},$$

with Assumption 1.1 guaranteeing that $\theta_2^* \in (0, 1)$ and $q_2^* = 1$.

Consider now the first period. If $a_1 = 0$, the mayor's reelection prospects are not affected by his actions. The mayor and the bureaucrat thus behave as in period 2: $b_1^*(\theta|a_1 = 0) = b_2^*(\theta)$ and $\theta_1^*(0) = \theta_2^*$.

If $a_1 = 1$, the mayor's reelection prospects are affected by his actions. In particular, retaining the bureaucrat reduces the likelihood that he enjoys his continuation value. The mayor's continuation value is itself dependent on his type. All mayors expect their office rent ρ. Sufficiently dishonest mayors also expect to pocket bribe b_2^*. We thus have

$$v_2(\theta) = \begin{cases} \rho + (1-\lambda)b_2^*(\theta_2^*) - \theta, \text{ if } \theta < \theta_2^* \\ \rho, \text{ otherwise} \end{cases}$$

Equation (3) becomes

$$b_1^*(\theta|a_1 = 1) = \begin{cases} \frac{\delta(\epsilon_t^0-\epsilon_t^1)(\rho+(1-\lambda)b_2^*)+(1-\delta(\epsilon_t^0-\epsilon_t^1))\theta}{1-\lambda}, \text{ if } \theta < \theta_2^* \\ \frac{\delta(\epsilon_t^0-\epsilon_t^1)\rho+\theta}{1-\lambda}, \text{ otherwise} \end{cases}$$

Notice that $b_1^*(\theta|a_1 = 1)$ is increasing in θ. Thus, all mayors such that $\theta' < \theta$ accept $b_1^*(\theta|a = 1)$, while all mayors such that $\theta' > \theta$ reject it. The bureaucrat then solves the maximization problem in Equations (4) and (5). The following Lemma pins down the solution:

Lemma 2. *In equilibrium, the optimal retention rate conditional on $a_1 = 1$ satisfies $\theta_1^*(1) \in (0, \theta_2^*]$, and the optimal quantity of rent is $q_1^* = q_2^* = 1$.*

Having characterized equilibrium, we can state the main empirical implication:

Proposition 2. *Audits increase bureaucrats' dismissal rates; i.e., $\theta_t^*(1) - \theta_t^*(0) \leq 0$. The effect of audits on the bureaucrat's dismissal rate is increasing as the electoral effect of audits gets larger: is decreasing in the probability of reelection: $\frac{\partial|\theta_t^*(1)-\theta_t^*(0)|}{\partial|\epsilon_1-\epsilon_0|} > 0$.*

Audits increase dismissal rates because they reduce the probability of reelection, incentivizing the mayor to remove the bureaucrat in order to restore her electoral prospects. Similar to the legal accountability channel, this increases the bribe required to retain the bureaucrat and, in turn, reduces the range of dishonesty that can support corruption; i.e., this increases dismissals. The effect grows larger as the effect of audits on electoral outcomes (i.e., $\epsilon_1 - \epsilon_0$) increases. These mayors see their chances of reelection reduced by the audit. The effect is smallest for second-term mayors, since they cannot be reelected, which gives Proposition 2 a stark corollary that runs opposite to Corollary 1:

Corollary 2. *The effect of audits on the bureaucrat's dismissal rate is larger for first-term mayors than for second-term mayors; i.e.,* $|\theta_2^*(1) - \theta_2^*(0)| < |\theta_1^*(1) - \theta_1^*(0)|$.

4.2 Empirical Results

We now empirically characterize the mechanisms underlying the effectiveness of audits. First, we test the empirical implications of our theoretical model. Next, we provide evidence against several alternative explanations.

Evidence supports legal accountability over electoral accountability. Corollaries 1 and 2 of our theoretical model provide distinct empirical predictions for the mechanisms of legal and electoral accountability. Legal accountability implies that second-term mayors are more likely to dismiss bureaucrats than first-term mayors, while electoral accountability predicts the opposite. To test these predictions, we compare the effect of audits on bureaucrats' dismissal rates between first- and second-term mayors.

Propositions 1 and 2 further refine these predictions within the subset of first-term mayors. Proposition 1 predicts that the effect of audits decreases with ϵ_0, the ex-ante probability of reelection (noting that $\epsilon_0 = 0$ for second-term mayors). Mayors with low reelection prospects are expected to dismiss bureaucrats more frequently, as they anticipate facing sanctions sooner. In contrast, Proposition 2 predicts that the effect of audits increases with $|\epsilon_1 - \epsilon_0|$, the change in the probability of reelection due to the audit (where $|\epsilon_1 - \epsilon_0| = 0$ for second-term mayors). If audits widely reduce reelection chances, then first-term mayors are expected to dismiss more bureaucrats to mitigate the negative electoral consequences.

A direct comparison between first- and second-term mayors is complicated by the persistence of audit effects over time. While audits are randomly assigned, ensuring that mayors' political standing is independent of treatment at term 0, audits can influence reelection prospects for first-term mayors (Ferraz and Finan, 2008), introducing post-treatment bias in comparisons during subsequent terms.[12] To address this issue, we focus on term 0, where the effects of audits can be compared without confounding by reelection dynamics. This is consistent with our theoretical model, which compares first- and second-term mayors at term 0 only.

We split the binary treatment variable t_{jt} into two components: t_{jt}^0, which captures treatment effects during term 0, and t_{jt}^1, which captures effects in subsequent terms. We also include a mediator, m_{jt}, to test theoretical predictions. Corollaries 1 and 2 classify m_{jt} as binary: $m_{jt} = \epsilon_0 = 0$ for second-term mayors and $m_{jt} = \mathbb{1}\{\epsilon_0 > 0\} = 1$ for first-term mayors. The resulting baseline model is:

$$y_{jt} = \alpha_j + \alpha_t + \beta_0 m_{jt} + \beta_1 t_{jt}^0 + \beta_2 t_{jt}^0 m_{jt} + \beta_3 t_{jt}^1 + \epsilon_{jt} \quad (6)$$

Here, β_1 captures the effect of audits on second-term mayors during term 0, while $\beta_1 + \beta_2$ captures the effect on first-term mayors during the same period. β_3 measures the persistence of audit effects in subsequent terms for all mayors.

We consider alternative specifications of m_{jt} to test the implications of Propositions 1 and 2. We test Proposition 1 by setting $m_{jt} = \epsilon_0$. We measure ϵ_0 in two ways. First, we use a proxy; namely, the mayor's margin of victory in the previous election, setting $\epsilon_0 = 0$ for second-term mayors. Second, we estimate it non-parametrically using a random forest model that predicts

[12] We show that for our sample – which spans more elections than Ferraz and Finan (2008), – audits have a negative, yet not statistically significant average effect on the probability of reelection of first-term mayors (Table A8).

	Term	Margin	$\widehat{\epsilon}_0$	$\lvert\widehat{\epsilon}_1 - \widehat{\epsilon}_0\rvert$
Post-audit, term 0 (β_1)	0.023***	0.016***	0.018***	0.009**
	(0.008)	(0.005)	(0.006)	(0.004)
Post-audit, term 0 × ϵ_0 (β_2)	−0.019**	−0.074***	−0.032***	
	(0.009)	(0.024)	(0.012)	
Post-audit, term 0 × $\lvert\epsilon_1 - \epsilon_0\rvert$ (β_2)				−0.076
				(0.138)
ϵ_0	−0.003**	−0.009	−0.031***	
	(0.001)	(0.006)	(0.002)	
$\lvert\epsilon_1 - \epsilon_0\rvert$				−0.083***
				(0.032)
Post-audit, term 1+	0.006	0.006	0.005	0.006
	(0.004)	(0.004)	(0.004)	(0.004)
R^2	0.288	0.291	0.295	0.291
N	51 750	50 979	50 961	50 961
Mean outcome (control, 1st-term mayor)	0.068	0.068	0.068	0.068
Mean outcome (control, 2nd-term mayor)	0.069	0.069	0.069	0.069
$\beta_1 + \beta_2$	0.004	−0.058***	−0.014	−0.067
	(0.005)	(0.021)	(0.008)	(0.138)

* p <0.1, ** p <0.05, *** p <0.01

Table 6: **Heterogeneity with respect to reelection incentives.** OLS estimates of Equation (6). Model 1: $\epsilon_0 = 1$ for first-term mayors, 0 for second-term mayors. Model 2: ϵ_0 proxies for the mayor's probability of reelection using his margin of victory in the previous election (with $\epsilon_0 = 0$ for second-term mayors). Model 3: ϵ_0 proxies for the mayor's probability of reelection using random-forest estimates. Model 4: $|\epsilon_1 - \epsilon_0|$ is the absolute value of the effect of audits on the mayor's probability of reelection, derived from causal forest estimates. Model 1: consistent with legal rather than electoral accountability, the effect of audits is concentrated in second-term mayors. Models 2 and 3: consistent with legal accountability, the effect of audits decreases in the mayor's electoral prospects. Model 4: inconsistent with electoral accountability, the effect of audits on dismissals does not vary with its effect on reelection prospects. All models include municipality and year fixed effects. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

mayoral reelection as a function of a number of observable characteristics, including audit status and additional covariates.[13] We test Proposition 2 by setting $m_{jt} = |\epsilon_1 - \epsilon_0|$. Recall that $\epsilon_1 - \epsilon_0$ is the treatment effect of audits on the mayor's probability of reelection. To obtain precise measures of this quantity, we estimate it non-parametrically using a causal forest model (Wager and Athey, 2018), a class of models that leverages random forests to provide precise estiamtes of heterogeneous treatment effects, using the same covariates as for the random forest estimates used to derive estimates of ϵ_0.

Table 6 supports the predictions of legal accountability over electoral accountability. Column 1 compares first-term ($\epsilon_0 = 1$) and second-term ($\epsilon_0 = 0$) mayors. Second-term mayors experience a significant increase in dismissal rates post-audit (parameter $\beta_1 > 0$), while no significant effect is observed for first-term mayors ($\beta_1 + \beta_2 = 0$). Columns 2 and 3 align with Proposition 1: mayors with weaker reelection prospects exhibit stronger dismissal effects ($\beta_1 > 0$), and the

[13]Specifically, local GDP, the mayor's age and gender, the number of candidates and of voters, his margin of victory in the previous election, as well as the state and election year.

	Conviction	Fees	Suspension	Conviction	Fees	Suspension
Post-audit, term 0 (β_1)	0.013	0.048	0.010	0.018	0.069	0.018
	(0.010)	(0.078)	(0.008)	(0.013)	(0.110)	(0.012)
Med. dismissal				−0.001	0.031	−0.001
				(0.006)	(0.045)	(0.005)
High dismissal				0.004	0.048	0.007
				(0.006)	(0.043)	(0.005)
Post-audit, term 0 × med. dismiss. (β_2)				−0.033*	−0.201	−0.024
				(0.018)	(0.142)	(0.015)
Post-audit, term 0 × high dismiss. (β_3)				0.001	0.043	−0.010
				(0.018)	(0.147)	(0.015)
Post-audit, term 1+	0.013	0.035	0.014	0.013	0.037	0.014*
	(0.009)	(0.075)	(0.008)	(0.009)	(0.075)	(0.008)
R^2	0.320	0.310	0.313	0.320	0.310	0.313
N	17 580	17 580	17 580	17 580	17 580	17 580
Mean outcome (control)	0.052	0.300	0.038			
Mean outcome (control, low fires)				0.055	0.329	0.042
Mean outcome (control, med. fires)				0.041	0.223	0.027
Mean outcome (control, high fires)				0.052	0.299	0.039
$\beta_1 + \beta_2$				−0.014	−0.132	−0.005
				(0.015)	(0.118)	(0.013)
$\beta_1 + \beta_3$				0.019	0.112	0.008
				(0.014)	(0.117)	(0.012)

* p <0.1, ** p <0.05, *** p <0.01

Table 7: **Effects of audits on legal sanctions.** This Table reports estimates of Equation (6), using legal sanctions as outcomes. Models 1 to 3 remove the interaction terms of Equation (6). "Conviction" equals one if the mayor faced legal sanctions after the current term. "Fees" correspond to the monetary legal sanction. "Suspension" equals one if the mayor was suspended from office after the current term. We group the percentage of dismissals in terciles, with the first tercile as the reference category. Moderate levels of dismissals post-audit alleviate legal sanctions. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

effect diminishes as electoral strength increases ($\beta_2 < 0$), disappearing entirely for mayors certain of reelection ($\beta_1 + \beta_2 = 0$). Conversely, Column 4 shows no support for Proposition 2, as dismissal rates do not vary with the impact of audits on reelection ($\beta_2 = 0$). We show in Table A9 that these results are robust to considering all forms of career interruptions as an outcome.

Importantly, legal accountability trickles down to lower echelons of the bureaucracy. In Table A10, we reproduce the analysis of Table 6, model 1, for other categories of municipal employees, including top bureaucrats who are not directly appointed by the mayor, as well as low-level bureaucrats (e.g., administrative assistants), high-level frontline providers (e.g., medical doctors), and low-level frontline providers (e.g., cleaners). For all categories but high-level frontline providers, results are consistent with those of Table 6: the effect of audits on dismissals is concentrated among second-term mayors, suggesting that legal accountability trickles down to other municipal employees. Audits have no effect on high-level frontline providers, presumably because they are more critical to the municipality and less likely to be patronage appointments.

Direct evidence of legal accountability. We further investigate legal accountability by examining the relationship between audits, mayoral convictions, and dismissals (Table 7). Columns 1-3 estimate Equation (6) without interaction terms and use various legal sanctions as outcomes. On average, audits do not significantly affect sanctions. However, Columns 4-6 incorporate dismissal rates (categorized into terciles) as a mediator. While these models are not causally interpretable due to conditioning on a post-treatment variable, they reveal that moderate dismissal levels correlate with reduced legal sanctions (parameter $\beta_2 > 0$). This finding supports our theoretical argument: dismissing bureaucrats disrupts rent-seeking, enabling mayors to avoid legal penalties. The effect disappears at high dismissal levels, which we explore further at the end of this section.

Evidence does not support alternative mechanisms. We consider three alternative mechanisms that may account for this Section's main result: namely, that audits prompt dismissals under second-term mayors but not under first-term mayors.

First, the result could be driven by *selection*: reelected mayors may differ systematically from those who are not reelected (e.g., they may be more competent or possess traits associated with being high-type), which are not fully observed in the data. When analyzing first-term mayors, we pool a large number of low-type mayors (i.e., mayors who will not get reelected) with a smaller number of high-type mayors (i.e., mayors who will get reelected), potentially masking the effect of low-type mayors. To evaluate selection effects, we re-estimate Equation (6) on a sample that excludes first-term mayors who were not reelected. This sample, consisting only of high-type mayors, allows us to isolate any differences. Table 8, column 1, shows that the estimates for this selected sample are consistent with those from the full sample in Table 6, column 1. This consistency suggests that selection effects cannot explain the main results.

Second, the result could be driven by *learning*: dismissing bureaucrats may require administrative expertise, such as familiarity with labor laws, which second-term mayors may acquire through experience. To test this, we allow the effect of audits to vary by the year of the term in which they occur. Specifically, we amend Equation (6) to include $p_{jt} \in \{0, \ldots, 3\}$, a discrete variable that indicates whether municipality j undergoes an audit in the first year of the electoral cycle ($p_{jt} = 0$), the second year ($p_{jt} = 1$), the third year ($p_{jt} = 2$), or the final (election) year ($p_{jt} = 3$). We further set $p_{jt} = 0$ if municipality j is not audited during the term. Equation (6) becomes:

$$y_{jt} = \alpha_j + \alpha_t + \beta_0 m_{jt} + \beta_1 t^0_{jt} + \beta_2 t^0_{jt} m_{jt} + \beta_3 t^1_{jt} + \beta_4 p_{jt} + \beta_5 p_{jt} m_{jt} + \epsilon_{jt}, \tag{7}$$

Parameter β_4 captures the effect of audits as they occur later in the term for second-term mayors, while $\beta_4 + \beta_5$ captures the corresponding effect for first-term mayors. If learning is driving the results, then the response to audits should become stronger as audits occur later in the term, *both* for first- and second-term mayors, since they become more experienced. If legal accountability is driving the results, the pattern sould hold stronger for second-term mayors, as lack of reelections implies that they are closer to facing legal sanctions. Results in Table 8, column 2, show that second-term mayors respond more strongly to audits conducted later in the term ($\beta_4 > 0$), consistent with the idea that they are closer to facing legal sanctions. However, the pattern does not hold for first-term mayors ($\beta_4 + \beta_5 = 0$), indicating that learning effects cannot explain the observed patterns.

Third, audits may lead to *budget cuts*. If audits reduce federal transfers, mayors may respond by decreasing the wage bill, leading to dismissals. To test this mechanism, we re-estimate our main specification (Equation (1)) using various measures of budget size as outcomes, including total federal transfers, discretionary federal transfers, and the wage bill. Table A11 demonstrates that audits do not lead to significant reductions in federal transfers or the wage bill. This indicates that reductions in budget size cannot account for the observed rise in dismissals.

	Selection	Learning
Post-audit, term 0 (β_1)	0.029***	0.000
	(0.009)	(0.010)
1st-term mayor	−0.019***	−0.003**
	(0.002)	(0.001)
Post-audit, term 1+	0.000	0.006
	(0.006)	(0.004)
1st-term mayor × Post-audit, term 0 (β_2)	−0.041***	0.008
	(0.011)	(0.011)
Year of audit, term 0 (β_4)		0.019**
		(0.008)
1st-term mayor × Year of audit (β_5)		−0.023**
		(0.009)
R^2	0.324	0.289
N	29 625	51 750
$\beta_1 + \beta_2$	−0.011*	0.006
	(0.007)	(0.011)
$\beta_4 + \beta_5$		−0.004
		(0.005)

* p <0.1, ** p <0.05, *** p <0.01

Table 8: **Alternative mechanisms.** This Table reports estimates of Equation (6) on a selected sample (model 1), and of Equation (7) (model 2). Model 1: results are qualitatively similar to Table 6, model 1, suggesting that selection cannot explain the observed patterns. Model 2: while second-term mayors respond more strongly to audits conducted later in the term ($\beta_4 > 0$), first-term mayors do not ($\beta_4 + \beta_5 = 0$), suggesting that learning effects cannot explain the observed patterns. All models include municipality and year fixed effect. Standard errors clustered at the municipality level in parenthesis. Hypothesis tests report the estimated value of the linear combination of parameters and the associated standard error in parenthesis.

Furthermore, as shown in Table A4, mayors counterbalance career interruptions by hiring new high-level bureaucrats, keeping the overall size of the bureaucracy constant.

Discussion. We have established that audits lead to the dismissal of corrupt bureaucrats and that these dismissals are primarily driven by legal, rather than electoral, accountability concerns. Additionally, we have shown that audits have no discernible effect in highly corrupt municipalities (Table 3). We attribute this to limited liability: in such municipalities, rent-seeking opportunities are more abundant (in our model, this corresponds to q taking values greater than 1), reducing the deterrent effect of fines imposed on mayors and bureaucrats. As a result, bureaucrats can transfer sufficiently large fractions of the rent to avoid dismissal.

Moreover, we find that large-scale dismissals following audits do not mitigate legal sanctions (Table 7). We interpret this as evidence that, in the most severe cases of corruption, dismissing bureaucrats is insufficient to shield the mayor from legal consequences. Audits provide hard evidence that is too incriminating to be offset by personnel changes. In the framework of our model, this implies that dismissing bureaucrats only partially eliminates the rent q. Consequently, when q is sufficiently high, mayors still opt for dismissals to reduce their exposure to fines, but they remain subject to substantial legal penalties.

5 Conclusion

Our study examined the impact of anti-corruption audits on bureaucrats, an often-overlooked yet crucial component of corrupt networks. While prior research has primarily focused on how audits discipline politicians, we showed that they also effectively punish bureaucrats, particularly those likely hired through patronage. We find that audits increase bureaucratic dismissals and resignations, with stronger effects in more corrupt municipalities and among overpaid bureaucrats or holding temporary contracts. Importantly, our findings suggest that audits do not merely lead to indiscriminate scapegoating but rather facilitate the targeted punishment of bad actors.

Beyond documenting these effects, we examined the mechanisms underlying both dismissals and resignations. We showed that mayors' decisions to dismiss bureaucrats are driven by legal rather than electoral accountability. Second-term mayors, who are no longer constrained by reelection concerns but are more vulnerable to legal prosecution, impose harsher sanctions following audits. Our results highlight the importance of legal accountability mechanisms in shaping political behavior, especially in contexts where electoral accountability may be weak or incomplete.

In addition, we showed that patronage hires are more likely to be dismissed post-audit, which suggests a tradeoff for patronage positions: while they offer lucrative incentives, these opportunities are contingent on political protection. Once audits weaken that protection, patronage hires exit the public sector.

Our results have two important policy implications. First, they suggest that anti-corruption audits are effective not only in disciplining politicians but also in reshaping bureaucratic incentives. However, their effectiveness depends on the institutional environment: while audits increase accountability in moderately corrupt municipalities, their effects are weaker in highly corrupt settings where limited liability may curtail the effectiveness of legal accountability, in the context of larger rent-seeking opportunities. This finding highlights the limits of transparency interventions in contexts where corruption is deeply embedded.

Second, our results highlight an important trade-off: audits are most effective in punishing mayors when they occur during their first term, as they compound legal and electoral accountability. However, their effectiveness in disciplining bureaucrats is higher when audits target second-term mayors, whose heightened legal accountability concerns make them more likely to impose sanctions on their subordinates. The optimal response to this trade-off depends on the production function of corruption. While audits demonstrate greater quantitative effectiveness in penalizing bureaucrats compared to politicians, the decision regarding whom anti-corruption authorities should prioritize for punishment hinges on identifying those who contribute the most to the production of corruption. This should be a focal point for future research.

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Appendices

A Data

A.1 Anti-corruption audits

We draw a random sample of 30 reports to verify how infractions were classified into two categories: *grave*, and *media*. *Falhas graves* are really the ones we focus on as evidence of corruption. In this category, auditors report practices that are clearly associated with corruption: collusion between companies and government, over invoicing of budgets, withholding of salaries or spending on staff members who are not allowed to be hired by the program.

In the category *falhas medias*, we have minor infractions that are not necessarily evidence of corruption, but procedural deficiencies. For instance, we have some municipalities that fail to respect a regular meeting of the health board, or that fail to provide enough books in school. We do not believe that constitutes enough evidence of corruption, since this seems to be rather weaknesses in administrative procedure that are meant to be identified by these audits.

- **Falhas médias:** Minor irregularities in the execution of programs. Mostly attributed to deficiencies in administrative procedure, rather than clear examples of corrupt behavior. Examples: 1) there is not a schedule for school bus maintenance, 2) not enough books in school; 3) poor infrastructure for healthcare facilities; 4) mismatch between registered beneficiaries and eligible families for Bolsa Família; 4) no formal procedure for legal actions in the health council.

- **Falhas graves:** The relevant category for corruption. In this category, we find evidence of over budgeting, illicit subcontracting practices, ghost employees, payment for services never provided. Examples: 1) requirements in audit that favor a particular company; 2) overspending of items in the budget, without justification; 3) charge for conditional cash transfers; 4) public servants receive Bolsa Família, when clearly above the income threshold.

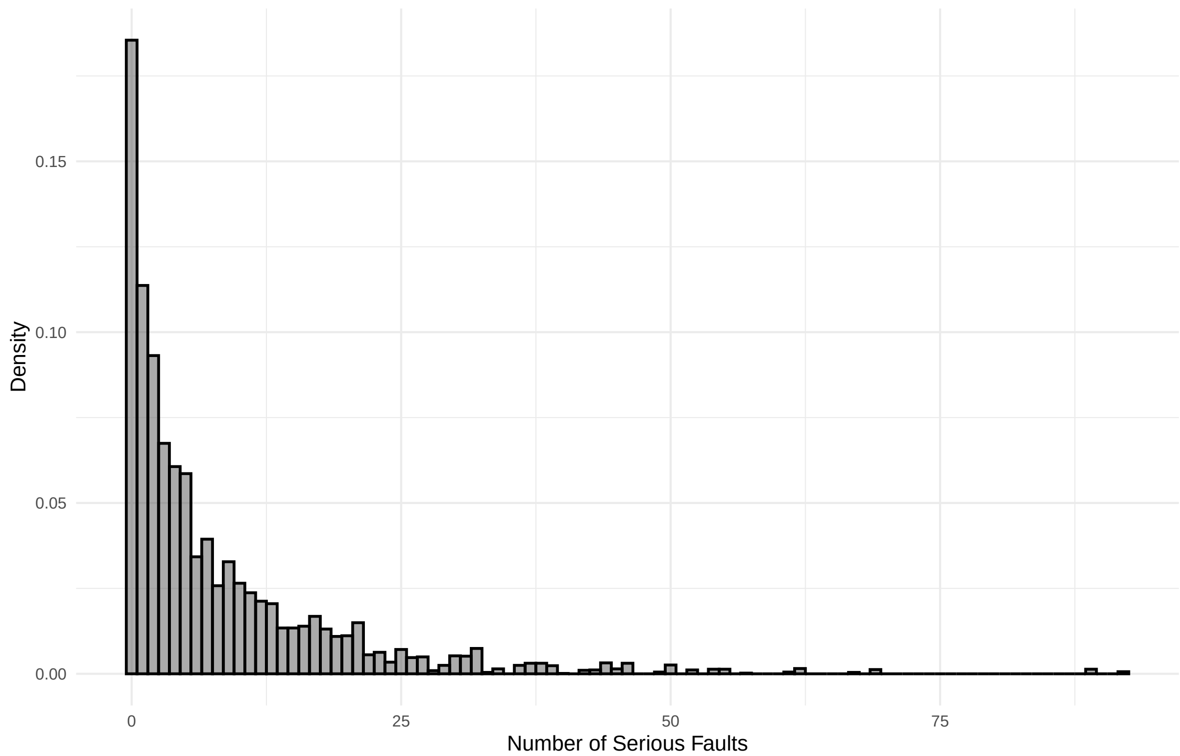

Figure A1: Distribution of the number serious faults reported in anti-corruption audits.

A.2 Estimating counterfactual private-sector wages

Our approach for estimating counterfactual private-sector wages is in the spirit of the Blinder-Oaxaca (Blinder, 1973) decomposition. We estimate a model that predicts the (log) wage of non-municipal bureaucrats; that is, private sector employees, who represent the bulk of the sample, as well as other public-sector employees, then use this model to predict the wage of municipal employees. Our goal is to predict the wages of non-municipal employees with as much much precision as possible, leveraging all the variables included in our data. As such, our model includes the following predictors:

- Age, modelled as an order-4 polynomial,
- Gender
- Race
- Education
- Public sector wage (logged),

and all two-way interactions. We further introduce municipal-level fixed effects. Because of the scale of the data, we estimate one model per year-state. In order to improve precision, we borrow from machine learning and estimate this model using elastic net regularization (Zou and Hastie, 2005), an approach that combines $L1$ regularization (LASSO) and $L2$ regularization (ridge regression).

A.3 Validating proxies for clientelism and patronage

	Schools				Healthcare		
	N	Pct. internet	Pct. library	Pct. lab	N	Beds	Equip.
Pct. co-partisans	−0.020	−0.008	0.001	−0.008*	−0.050*	0.072	−0.004
	(0.016)	(0.013)	(0.018)	(0.005)	(0.026)	(0.158)	(0.009)
Pct. over-paid	−0.023*	−0.017*	−0.062***	−0.004	−0.004	0.090	−0.009
	(0.012)	(0.009)	(0.012)	(0.003)	(0.019)	(0.101)	(0.007)
Pct. temporary	0.018	0.018	0.024	−0.011**	−0.075***	−0.116	0.010
	(0.021)	(0.014)	(0.016)	(0.005)	(0.028)	(0.129)	(0.012)
R^2	0.984	0.879	0.743	0.861	0.959	0.852	0.878
N	13 604	13 604	13 604	13 604	13 363	4086	13 679
Mean outcome (control)	2.049	0.471	0.244	0.038	2.059	1.046	0.231

* p <0.1, ** p <0.05, *** p <0.01

Table A1: **Correlation between proxies for patronage and public good provision.** All models regress outcomes averaged out at the term-level, include municipality and term fixed effects, and control for pct. graduates, females, average tenure, and career interruptions. Standard errors clustered at the municipality level in parenthesis. Outcomes include, for schools, the log number and the percentage of schools with internet access, a library, and a science lab; for healthcare, the log number of hosppitals, and the log number of beds and equipments averaged across hospitals. All three proxies for patronage are negatively correlated with public good provision.

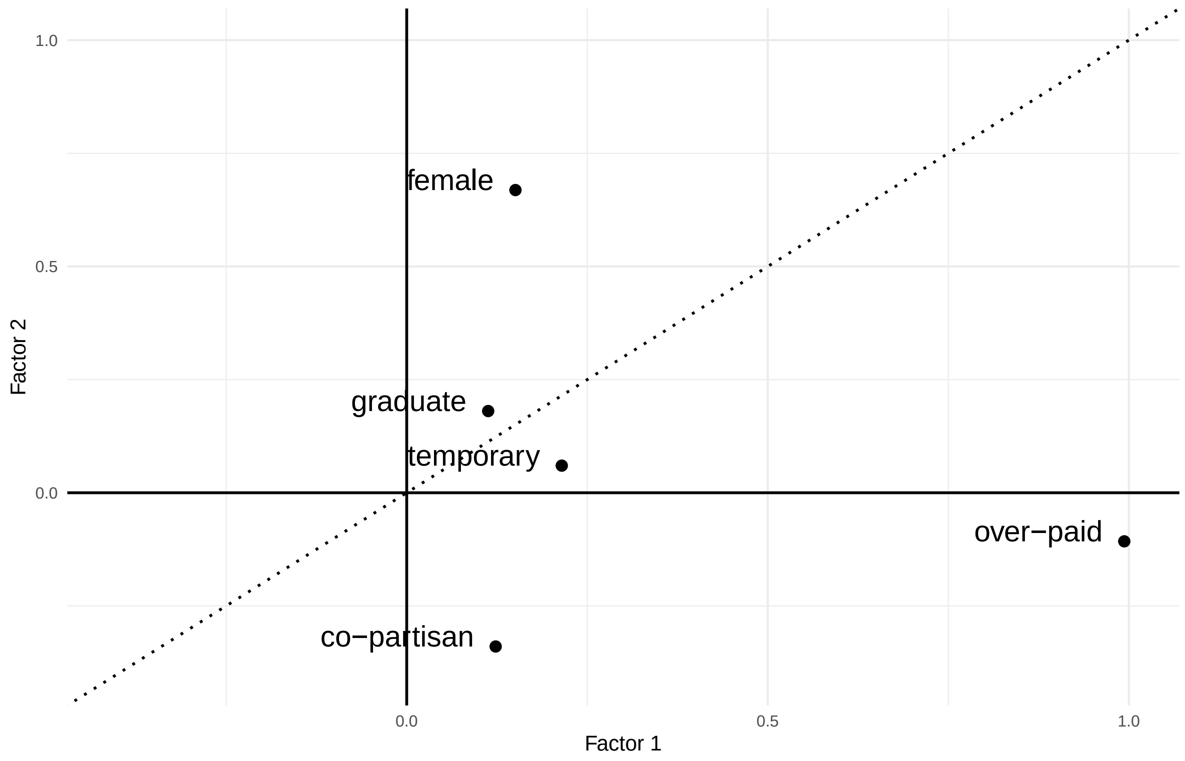

Figure A2: **Exploratory factor analysis.** This plot shows the factor loadings associated with exploratory factor analysis of 5 individual-level characteristics using tetrachoric correlations and varimax rotation. The number of factors (3) is selected using the Kaiser rule. Our three candidate variables for patronage (i.e., overpayment, co-partisanship, temporary contract) load more heavily on factor 1 and 3 than on factor 2, while the other two variables (female, graduate) load more heavily on factor 2 than factors 1 and 3. This suggests that our three candidate variables pick up a construct that cannot be captured by our placebo.

B Robustness checks on stylized facts

B.1 Heterogeneity with respect to corruption levels

	Career interruptions	Dismissals	Resignations	Δ wages
A. Number of serious faults, not de-meaned				
Post-audit (β_1)	0.017	-0.002	0.004	0.000
	(0.014)	(0.007)	(0.005)	(0.009)
Post-audit × Med. corruption (β_2)	0.013	0.012	0.007	-0.027**
	(0.017)	(0.009)	(0.007)	(0.013)
Post-audit × High corruption (β_3)	0.007	0.009	-0.003	-0.007
	(0.019)	(0.008)	(0.006)	(0.010)
R^2	0.301	0.286	0.297	0.104
N	9674	9674	9674	8020
Mean outcome (control, low corruption)	0.327	0.063	0.061	0.018
Mean outcome (control, med. corruption)	0.331	0.066	0.054	0.027
Mean outcome (control, high corruption)	0.369	0.047	0.028	0.025
$\beta_1 + \beta_2$	0.030**	0.009	0.012*	−0.027**
	(0.015)	(0.008)	(0.007)	(0.013)
$\beta_1 + \beta_3$	0.025	0.007	0.001	−0.007
	(0.018)	(0.008)	(0.006)	(0.008)
B. Number of serious faults per amount audited; de-meaned				
Post-audit (β_1)	0.007	-0.007	0.005	0.002
	(0.014)	(0.007)	(0.006)	(0.010)
Post-audit × Med. corruption (β_2)	0.033**	0.022***	0.004	-0.024**
	(0.017)	(0.008)	(0.007)	(0.012)
Post-audit × High corruption (β_3)	0.015	0.009	-0.002	-0.013
	(0.019)	(0.008)	(0.007)	(0.011)
R^2	0.301	0.287	0.296	0.103
N	9674	9674	9674	8020
Mean outcome (control, low corruption)	0.325	0.063	0.070	0.020
Mean outcome (control, med. corruption)	0.335	0.065	0.046	0.022
Mean outcome (control, high corruption)	0.365	0.049	0.032	0.027
$\beta_1 + \beta_2$	0.040***	0.015**	0.009	−0.022**
	(0.015)	(0.007)	(0.006)	(0.011)
$\beta_1 + \beta_3$	0.022	0.002	0.003	−0.010
	(0.017)	(0.008)	(0.006)	(0.009)
C. Number of serious faults per item audited; de-meaned				
Post-audit (β_1)	0.014	-0.003	0.002	-0.002
	(0.015)	(0.007)	(0.006)	(0.011)
Post-audit × Med. corruption (β_2)	0.023	0.015*	0.009	-0.016
	(0.017)	(0.008)	(0.007)	(0.012)
Post-audit × High corruption (β_3)	0.006	0.005	0.000	-0.008
	(0.019)	(0.008)	(0.007)	(0.012)
R^2	0.301	0.286	0.297	0.103
N	9674	9674	9674	8020
Mean outcome (control, low corruption)	0.326	0.068	0.064	0.016
Mean outcome (control, med. corruption)	0.333	0.060	0.050	0.026
Mean outcome (control, high corruption)	0.366	0.049	0.032	0.028
$\beta_1 + \beta_2$	0.036**	0.012	0.011*	−0.019*
	(0.015)	(0.008)	(0.006)	(0.010)
$\beta_1 + \beta_3$	0.020	0.002	0.003	−0.011
	(0.017)	(0.008)	(0.006)	(0.009)

* p <0.1, ** p <0.05, *** p <0.01

Table A2: **Robustness to alternative measures of corruption.** This table reproduces 3 but varies the measure of corruption used to construct tercile groups. Results are qualitatively similar.

	Career interruptions	Dismissals	Resignations	Δ wages
Post-audit (β_1)	0.021	0.015**	0.006	0.003
	(0.014)	(0.007)	(0.006)	(0.010)
Post-audit × Low corruption (β_2)	−0.023	−0.018**	−0.007	−0.006
	(0.018)	(0.009)	(0.008)	(0.013)
Post-audit × Med. corruption (β_3)	0.016	0.001	0.002	−0.031**
	(0.019)	(0.009)	(0.008)	(0.014)
Post-audit × High corruption (β_4)	−0.009	−0.011	−0.007	−0.018
	(0.020)	(0.009)	(0.007)	(0.013)
R^2	0.312	0.284	0.296	0.102
N	53 922	53 922	53 922	44 847
Mean outcome (control, never audited)	0.332	0.069	0.056	0.010
Mean outcome (control, low corruption)	0.330	0.064	0.066	0.020
Mean outcome (control, med. corruption)	0.329	0.064	0.049	0.022
Mean outcome (control, high corruption)	0.366	0.049	0.032	0.028
$\beta_1 + \beta_2$	−0.002	−0.002	−0.001	−0.003
	(0.012)	(0.006)	(0.005)	(0.008)
$\beta_1 + \beta_3$	0.037***	0.017***	0.009*	−0.027***
	(0.013)	(0.006)	(0.005)	(0.010)
$\beta_1 + \beta_4$	0.012	0.004	0.000	−0.015**
	(0.015)	(0.006)	(0.005)	(0.007)

* p <0.1, ** p <0.05, *** p <0.01

Table A3: **Heterogeneity with respect to the level of corruption (full sample).** This table reproduces Table 3 but also includes non-audited municipalities. As such, treatment effects are relative to a non-audited municipality with average levels of corruption. Results are qualitatively similar.

B.2 Contextualization of the main result

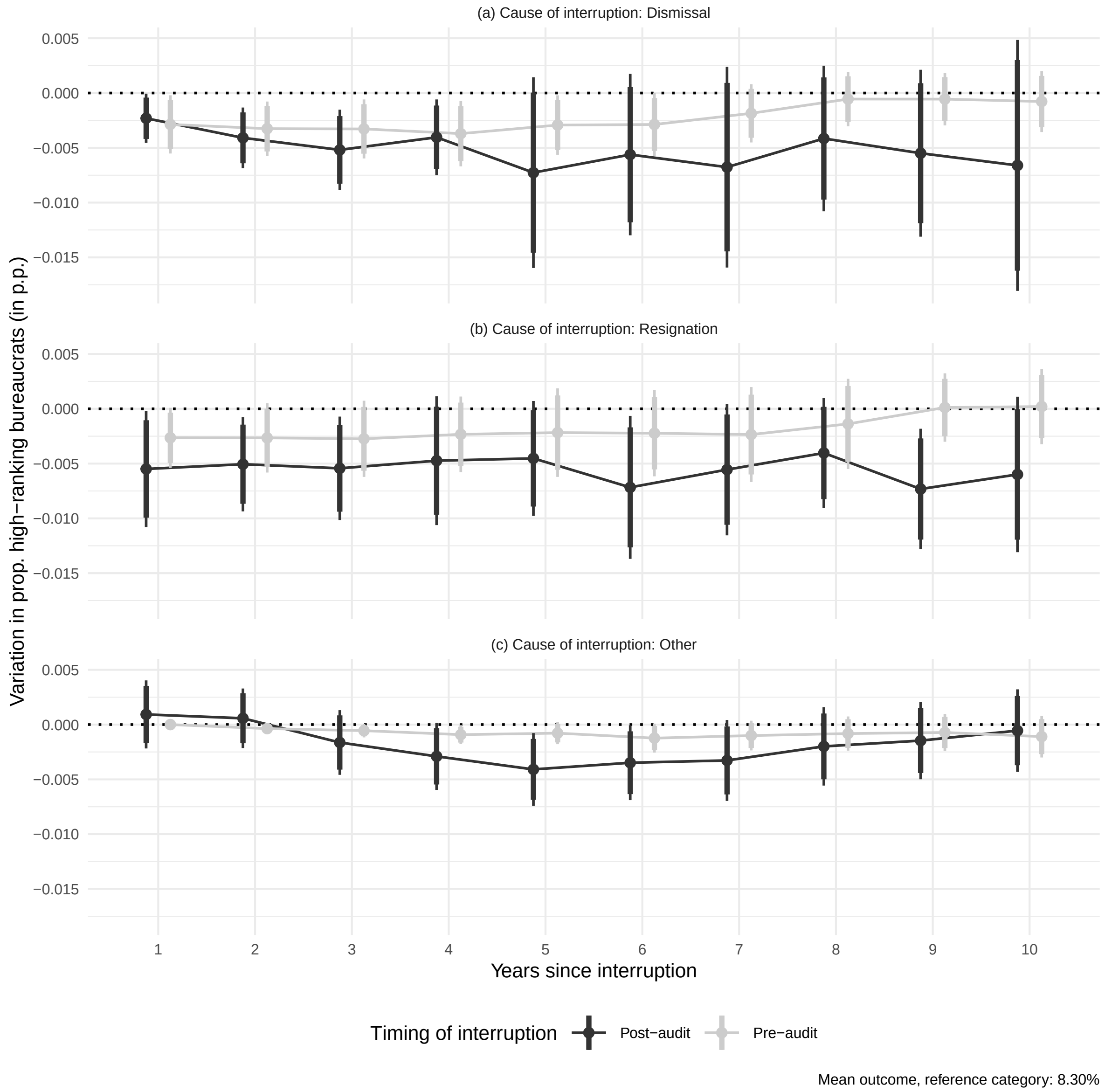

Figure A3: **Revolving door.** This Figure describes the percentage of in-sample bureaucrats that re-enter any high-level bureaucratic position (i.e., CBO 1) at any level of government following a career interruption event. We estimate saturated individual-level OLS model with municipality and year fixed-effect, in order to investigate the evolution of this rate relative to our reference category; namely, individuals who saw their career interrupted for "other" reasons, pre-audit, one year after the audit. We report 90 and 95% confidence intervals clustered at the municipal level. Revolving door phenomena are unlikely (8.3% in the reference cateogry), and do not meaningfully vary over time, nor as a function of the cause of career interruption, nor according to the timing of the interruption.

	Hiring	Size
Post-audit	0.001	0.031
	(0.008)	(0.028)
R^2	0.360	0.754
N	50 478	53 922
Mean outcome (control)	0.353	2.747

* p <0.1, ** p <0.05, *** p <0.01

Table A4: **Effect of audits on the size of the bureaucracy.** This Table reports OLS estimates of the model in Equation (1) using the percentage of hires and the log-number of bureaucrats as dependent variables. The model that uses the percentage of hires as a dependent variable excludes year 2003, as hiring data is not reliable for this year. Audits do not affect hiring patterns nor bureaucracy size.

B.3 Alternative specifications of the main result

	Career interruptions	Dismissals	Resignations
Post-audit	0.025***	0.018***	0.007***
	(0.003)	(0.002)	(0.002)
R^2	0.043	0.056	0.062
N	94 641	94 641	94 641
Mean outcome (control)	0.102	0.052	0.050

* p <0.1, ** p <0.05, *** p <0.01

Table A5: **Main result at the monthy level.** This Table re-estimates models 1 to 3 of Table 2 using monthly-level data. All models include municipality and month×year fixed effect.

	Career interruptions	Dismissals	Resignations
Post-audit	0.060**	0.036	0.021
	(0.027)	(0.022)	(0.017)
R^2	0.609	0.541	0.590
N	53 922	53 922	53 922
Mean outcome (control)	1.623	0.560	0.516

* p <0.1, ** p <0.05, *** p <0.01

Table A6: **Main result using (log) counts.** This Table re-estimates models 1 to 3 of Table 2 using log counts of each event as the dependent variable. Albeit weaker, results are qualitatively similar.

B.4 Balance tests

	Number of employees	Mean wage	Population	Illiteracy rate	Urban rate	Gini
Audited	−0.390	−0.024	0.050	0.003	0.009	0.003
	(6.794)	(0.022)	(0.039)	(0.002)	(0.008)	(0.002)
R^2	0.013	0.218	0.141	0.728	0.215	0.160
N	3434	3432	3390	3390	3390	3390

* p <0.1, ** p <0.05, *** p <0.01

Table A7: **Balance tests.** We compare 2003 outcomes for all in-sample municipalities. Since audits are randomized at the state level, we perform within-state comparisons by including state fixed effects. Heteroskedastic-robust standard errors in parenthesis. There are no significant differences in pre-treatment characteristics, suggesting that treatment and control municipalities are balanced on pre-treatment characteristics.

B.5 Dynamic effects

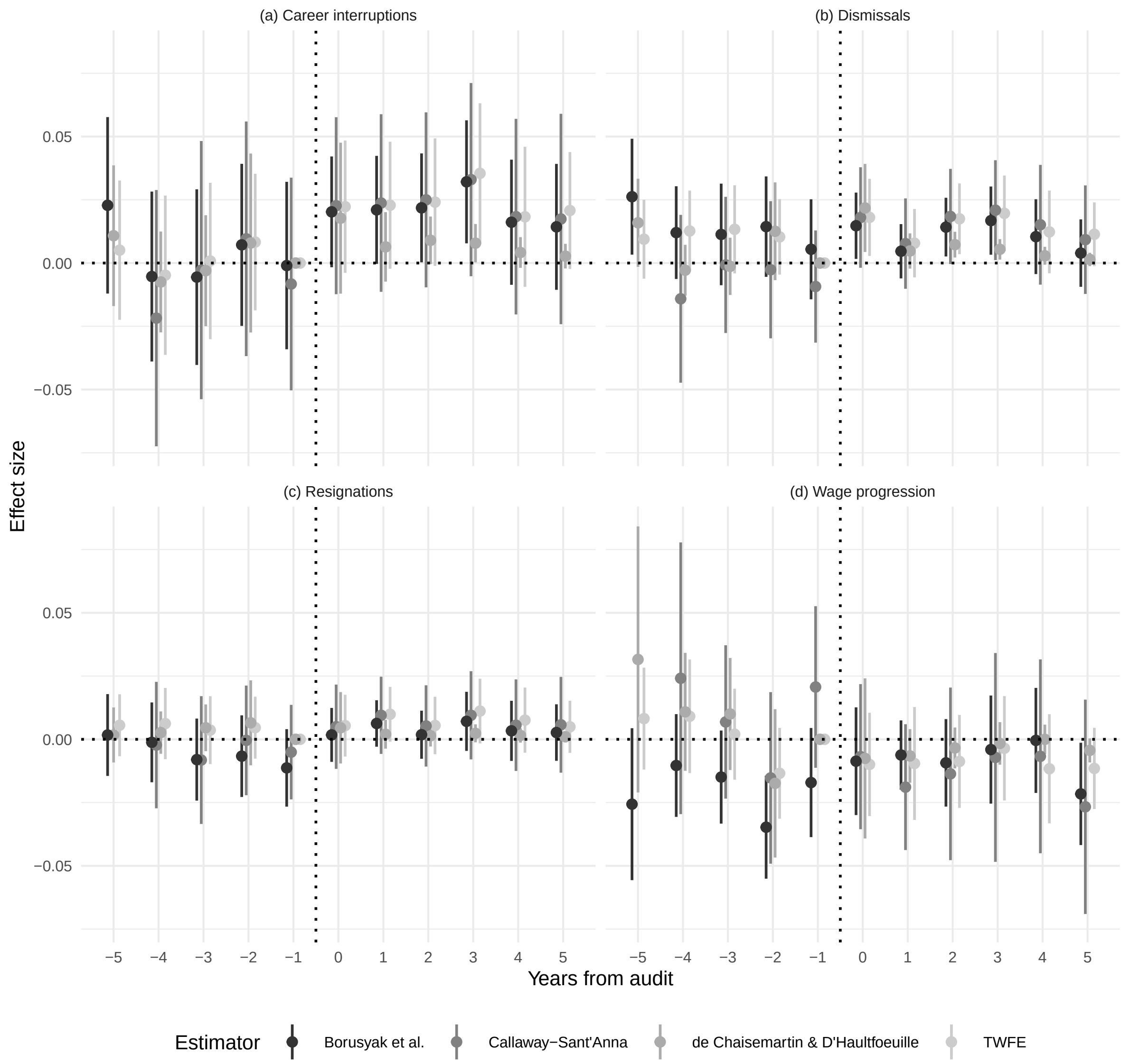

Figure A4: **Dynamic effects.** Panel (c) shows that audit have a short-, medium- and long-run effect on career interruptions, with significant effects within 1 year, 2 to 4, and 5 or more years of the event. Panels (a) and (b) reveal different timings for dismissals and resignations, with immediate, increases in dismissals, followed by a wave of resignations within 1 year of the audit, then a progressive slowdown in resignations which is offset by an increase in dismissals. Wages start to decline in the medium run (2-4 years after the event), with significant declines only observed in the long run, with a long-run decline in wage progression rate of about 5 percent. Similarly, hires tend to increase in the medium run, reaching a maximum three years after the audit.

C Robustness: mechanism underlying resignations

C.1 Proofs

Bounds on parameters for assumption 1 . The bounds are:

- $\overline{\lambda} = \frac{1}{1+f_B+f_M}$

- $\underline{\rho} = \frac{1-\lambda(1+f_B)}{2}$
- $\overline{\rho} = \frac{2-\delta(\epsilon_0-\epsilon_1)}{2\delta(\epsilon_0-\epsilon_1)}$

Proof of Lemma 1. We first solve the maximization problem in Equation (4) to derive $\theta_t^*(a_t)$. Notice that the objective function is quadratic and concave in θ. The first-order condition is thus sufficient and necessary. Assumption 1.1 implies that $\theta_t^*(a_t) \in (0,1)$.

We have $u_{B,t}(\theta^*(a_t)|a_t) = \left(\frac{1-(1+f_B+(1-(1-\delta)\epsilon)f_M)}{2}\right)^2$. Thus, Assumption 1.1 implies that $u_{B,t}(\theta^*(a_t)|a_t) > 0$ for any a_t which, in turn, implies that $q_t^* = 1$. □

Proof of Proposition 1. We have that $\theta_t^*(1) - \theta_t^*(0) = -\frac{(1+f_B+(1-(1-\delta)\epsilon)f_M)(\lambda(1)-\lambda(0))}{2} < 0$. This implies that $\frac{\partial\theta_t^*(1)-\theta_t^*(0)}{\partial\epsilon_t} > 0$. □

Proof of Lemma 2. We first solve the maximization problem in Equation (4) to derive $\theta_1^*(1)$. Notice that the objective function is continuous over its domain and quadratic and concave in θ over both regions. The first-order condition for $u_{B,1}(\theta|a_t = 1)$ over its first region obtains

$$\tilde{\theta} = \frac{(2-\delta(\epsilon_0-\epsilon_1))(1-\lambda(1+f_B)) - 2\delta(\epsilon_0-\epsilon_1)\rho}{4(1-\delta(\epsilon_0-\epsilon_1))}$$

Assumptions 1.1 and 1.2 imply that $0 < \tilde{\theta} < \theta_2^*$. Thus, over its first region, $u_{B,1}(\theta|a_t = 1)$ is maximized at $\tilde{\theta}$. Also notice that $\lim_{\theta\to\theta_2^{*+}} \frac{\partial u_{B,1}(\theta|a_t=1)}{\partial\theta} < 0$. Over its second region, $u_{B,1}(\theta|a_t = 1)$ is thus maximized at θ_2^*. As such, $\theta_1^*(1) = \tilde{\theta}$. Note finally that Assumptions 1.1 and 1.2 imply that $u_{B,1}(\theta_1^*(1)|1) > 0$. Since $u_{B,1}(\theta_1^*(0)|0) > 0$, we have $q_1^* = 1$. □

Proof of Proposition 2. We have $\theta_2^*(1) - \theta_2^*(0) = \theta_2^* - \theta_2^* = 0$, and $\theta_1^*(1) - \theta_1^*(0) = \theta_1^*(q_1^*|1) - \theta_2^* = -\frac{\delta(\epsilon_0-\epsilon_1)((1-\lambda(1+f_B))-2\rho)}{4(1-\delta(\epsilon_0-\epsilon_1))} < 0$, from the proof of Lemma 1. Thus, $\frac{\partial\theta_1^*(1)-\theta_1^*(0)}{\partial\epsilon_1-\epsilon_0} < 0$, implying $\frac{\partial|\theta_1^*(1)-\theta_1^*(0)|}{\partial|\epsilon_1-\epsilon_0|} > 0$. □

C.2 Additional tests

	(1)	(2)
Audit	−0.007	−0.013
	(0.018)	(0.018)
R^2	0.039	0.082
N	11 506	11 506
Controls	_	✓

* p <0.1, ** p <0.05, *** p <0.01

Table A8: **Effect of audits on the probability of reelection of first-term mayors.** On average, audits have a negative, yet statistically insignificant effect on mayors' reelection probability. All models include state and year fixed effect. Model 2 controls for local GDP, the mayor's age and gender, the number of candidates and of voters, his margin of victory in the previous election. Standard errors clustered at the state level in parenthesis.

	Term	Margin	$\widehat{\epsilon}_0$	$\lvert\widehat{\epsilon}_1 - \widehat{\epsilon}_0\rvert$
Post-audit, term 0 (β_1)	0.028**	0.022**	0.030***	0.015*
	(0.012)	(0.009)	(0.010)	(0.008)
Post-audit, term 0 × ϵ_0 (β_2)	−0.018	−0.076*	−0.055**	
	(0.014)	(0.040)	(0.023)	
Post-audit, term 0 × $\lvert\epsilon_1 - \epsilon_0\rvert$ (β_2)				−0.208
				(0.252)
ϵ_0	−0.021***	−0.075***	−0.100***	
	(0.003)	(0.012)	(0.004)	
$\lvert\epsilon_1 - \epsilon_0\rvert$				−0.149**
				(0.060)
Post-audit, term 1+	0.017**	0.018**	0.017**	0.018**
	(0.008)	(0.008)	(0.008)	(0.008)
R^2	0.315	0.317	0.326	0.317
N	51 750	50 979	50 961	50 961
Mean outcome (control, 1st-term mayor)	0.329	0.330	0.330	0.330
Mean outcome (control, 2nd-term mayor)	0.338	0.338	0.338	0.338
$\beta_1 + \beta_2$	0.009	−0.054	−0.025	−0.193
	(0.010)	(0.036)	(0.019)	(0.251)

* p <0.1, ** p <0.05, *** p <0.01

Table A9: **Heterogeneity with respect to reelection incentives, all career interruptions.** This Table reproduces 6 but considers all forms of career interruptions instead of dismissals only. Results are qualitatively similar.

	High bur. (all)	Low bur.	High front.	Low front.
Post-audit, term 0 (β_1)	0.018***	0.009***	0.001	0.004**
	(0.006)	(0.003)	(0.001)	(0.001)
Post-audit, term 0 × ϵ_0 (β_2)	−0.017**	−0.008**	−0.001	−0.003**
	(0.007)	(0.003)	(0.002)	(0.002)
ϵ_0	−0.001	0.001	0.001	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Post-audit, term 1+	0.004	0.001	0.000	0.002*
	(0.003)	(0.002)	(0.001)	(0.001)
R^2	0.295	0.290	0.273	0.305
N	64 767	69 479	68 589	68 975
Mean outcome (control, 1st-term, $\epsilon_0 = 1$)	0.060	0.031	0.015	0.017
Mean outcome (control, 2nd-term, $\epsilon_0 = 0$)	0.060	0.029	0.014	0.016
$\beta_1 + \beta_2$	0.001	0.000	−0.001	0.000
	(0.004)	(0.002)	(0.001)	(0.001)

* p <0.1, ** p <0.05, *** p <0.01

Table A10: **Effects of audits on other municipal employees.** This Table reestimates Table 6, model 1 on other types of municipal employees; specifically, all high-level bureaucrats (CBO 1), instead of the subset of high-level bureaucrats we consider in the main analysis, low-level bureaucrats (CBO 4), high-level frontline providers (CBOs 2 and 3), and low-level frontline providers (all other CBOs). Results show that the legal accountability mechanism evidenced for our sample trickles down to other municipal employees, except for high-level frontline providers, presumably because the latter are more likely to be critical to the municipality's operations and less likely to be patronage appointments.

	Total revenues	Discrete federal transfers	Wage bill
Post-audit	0.012**	0.106	0.005
	(0.005)	(0.117)	(0.003)
R^2	0.966	0.349	0.733
N	52 451	52 451	53 887
Mean outcome (control)	15.765	9.422	2.230

* p <0.1, ** p <0.05, *** p <0.01

Table A11: **Impact of audits on budget.** OLS estimates of Equation (1) using budget-related outcomes. Wage bill is the sum of wages paid to high level bureaucrats. We follow Brollo and Troiano (2016) by focusing on discretionary transfers for capital investments from federal and state governments, for which mayors have a significant effect on the allocation of these transfers. The effects of audits on career interruptions are not driven by budget cuts. All models include municipality and year fixed effect. Standard errors clustered at the municipality level in parenthesis.

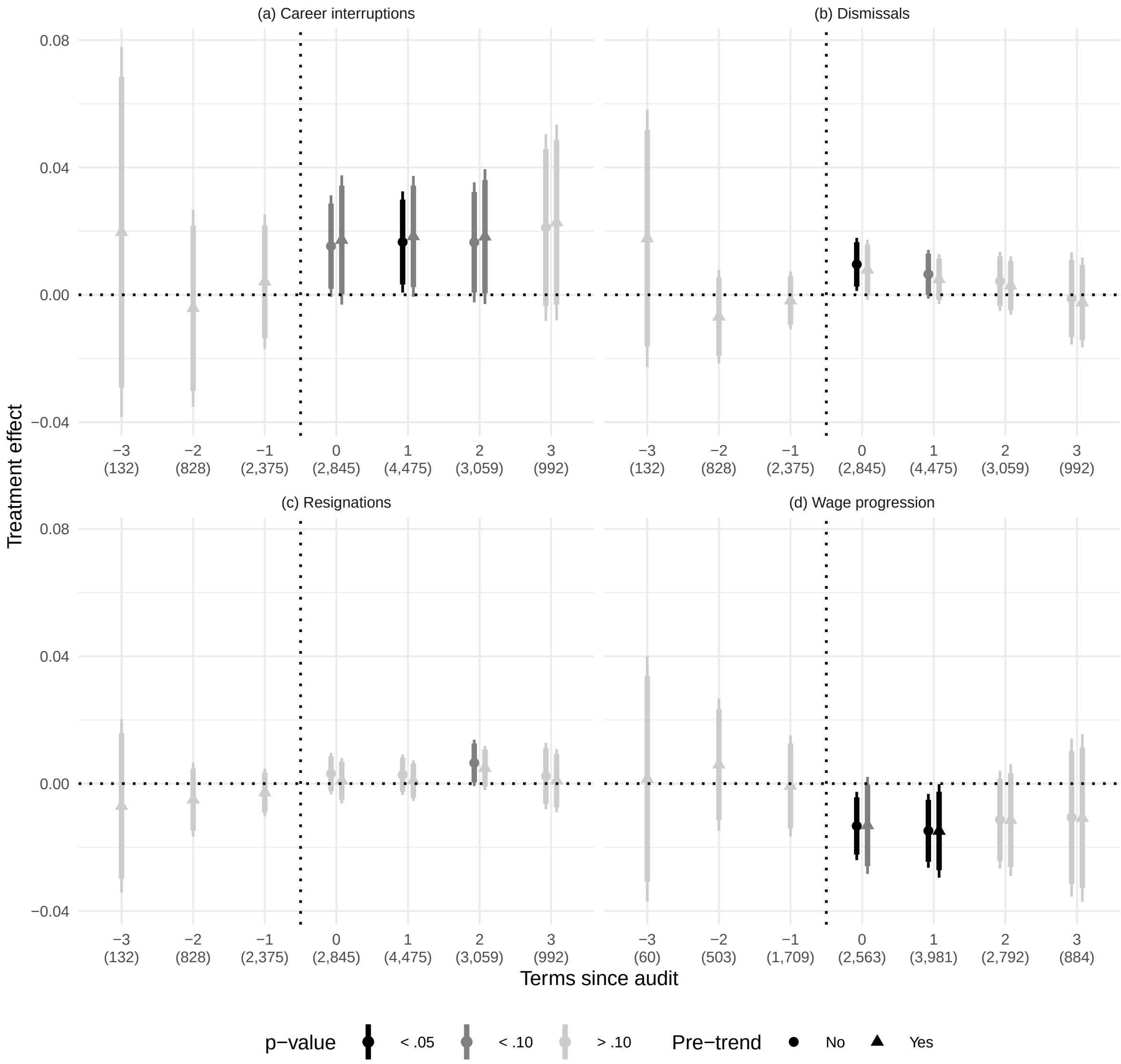

Figure 2: **Dynamic effects.** Term 0 refers to the term during which the audit occurred. We report estimates from a model that includes pre-trends and a model that does not. Bars represent 90 and 95% confidence intervals clustered at the municipal level. Sample sizes in parenthesis. Audits have immediate effects on dismissals and long-term effects on career interruptions, resignations, and wage progression.