# The impact of anti-corruption audits on bureaucratic careers: Evidence from Brazilian municipalities

Romain Ferrali[1] and Galileu Kim[2]

[1]NYU Abu Dhabi
[2]Princeton University

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**Abstract**

How can governemnt policies deter corruption by public officials? Previous studies find that anti-corruption audits are effective in disciplining politicians, but their impact on bureaucrats is unclear. In this paper, we model corruption as a long-term career decision. Leveraging 10 years of randomized audits on the careers of 275 thousand municipal officials, we show that audits do not cause ex-post dismissals, even when strong evidence of corruption is found. To investigate audits' multiple effects, we propose a model of corruption with career concerns, estimating these effects structurally. We simulate counterfactuals, finding that audits lead to a 4 percent overall decrease in corruption. Our findings suggest that, as is, audits have limited effects, and we propose a set of policy redesigns that maximize their efficiency.

# 1 Introduction

How can government policies reduce corruption by public officials? The stakes are high: corruption has significant economic and political costs, undermining legitimacy and economic growth. (e.g. Rose-Ackerman and Palifka, 2016; Rothstein, 2011; Fisman and Svensson, 2007). In response, states have adopted policies designed to detect and punish corruption by public officials. (Chen and Kung, 2018). Audits have grown increasingly popular, with previous studies documenting their effectiveness in reducing corruption and sanctioning politicians (Avis, Ferraz and Finan, 2018; Ferraz and Finan, 2011). But political corruption captures only part of the story. Our understanding of *bureaucrats*' response to anti-corruption policies is far more limited.

The career incentives politicians and bureaucrats face are starkly different, ultimately shaping how they respond to policy interventions. Politicians are subject to electoral accountability and short-term mandates, with reelection serving as a disciplining mechanism (Besley, 2006; Ferraz and Finan, 2007). Bureaucrats, on the other hand, do not face electoral sanctions, and their long-term careers are open to transitions outside of the public sector. As such, a policy that may be well-suited for reducing corruption by politicians may be ineffective for a bureaucrat. Designing an effective anti-corruption policy requires taking into account this heterogeneity in incentives, identifying how it affects a bureaucrat's decision to engage in corruption.

In this paper, we analyze the conditions under which audits effectively reduce corruption by bureaucratic officials. Our empirical strategy focuses on Brazil, a democracy in the developing world with median levels of corruptions [1] We leverage a granular administrative panel data of 10 years of audits and the careers of over 275 thousand municipal bureaucrats. Taking advantage of the randomized nature of these audits, we first investigate what happens immediately after an audit. We find that, ex-post, high-level bureaucrats do not respond to audits, remaining in office even when strong evidence of corruption is found or their political officials are removed. The only short run improvement we find is in managerial practices, which lasts for four years after an audit and is limited to highly-corrupt municipalities. These results are surprising, given that the same audits are effective at removing corrupt politicians from office and that, as per Brazilian law, corrupt bureaucrats may face potentially severe sanctions, ranging from dismissal to imprisonment.[2]

These initial results raise more questions than answers. An immediate conclusion would be that audits are ineffective. However, it may also be that audits have strong *disciplining* effects, pushing bureaucrats to refrain from corruption because of the *threat* of

[1] According to Transparency International, Brazil ranks 106 out of 180 countries in the Corruption Perceptions Index.

[2] The CGU has conducted a set of crackdown operations – *operações especiais* – which has led to the arrest of bureaucrats found to be engaging in coruption, see articles.

an audit. It may also be that ex-post effects are unobservable, as bureaucrats temporarily refrain from stealing. Yet this first empirical approach can only capture ex-post, observable effects. We build a model of corruption with career concerns in order to enumerate the full range of effects audits may potentially have. This model has the additional benefit of breaking down this complex policy into a set of mechanisms: 1) audit *frequency* 2) *monitoring*, the auditor's capacity to detect and dismiss corrupt bureaucrats and 3) *clean-up* of the bureaucracy that could stem from those improvements in managerial practices we identify empirically and temporarily reduce rents. We find that as the severity of audits decreases, their effect moves from disciplining to ex post. In the limit, audits are not severe enough to curb corruption.

We apply our theoretical model the observed data through structural estimation. Our estimates suggest that the audits program, as is, causes a three percent decrease in overall corruption, compared to a baseline counterfactual in which audits are never implemented, in line with previous findings by Avis, Ferraz and Finan (2018). We find that this effect is limited due to: 1) high rents from corruption, 2) poor monitoring and 3) limited clean-up effects. We then explore how to redesign audits to improve their effectiveness, modifying each of these channels and comparing that to the same baseline counterfactual. In isolation, increasing the audit's frequency is the most effective deterrent to corruption, consistent with findings from Zamboni and Litschig (2018). However, pulling all the levers simultaneously provides the greatest effect: a 25% increase in all channels leads to the same reduction in coruption (25%) as auditing each and every municipality every year. Overall, these findings suggest a high complementarity between the policy's components: a multi-pronged attack is the most effective way to stave off bureaucratic corruption.

Our paper contributes to scholarly research on corruption by public officials in the developing world (Treisman, 2007; Olken and Pande, 2012). While previous studies have focused on how politicians engage in corruption (Ferraz and Finan, 2011; Nyblade and Reed, 2008), we focus on bureaucrats' decision, showing that the same policy may have different outcomes on politicians and bureaucrats. While Ferraz and Finan (2011) show that this same program is effective at removing corrupt politicians from office, we find much more limited effects on bureaucrats' careers and corrupt behavior. In the conclusion, we discuss how this may owe to different career incentives.

Our study also contributes to a growing body of literature on how public policies can improve bureaucratic quality. At the macro-level, scholars have analyzed how national-level reforms may improve state capacity (Evans, 1995; Grindle, 2012), but often failed to break down these complex policy bundles into their constitutive components (Centeno, Kohli and Yashar, 2017). At the micro-level, previous studies have shown, using experimental or quasi-experimental settings, that focalized policy interventions can improve bureaucratic quality (Duflo, Hanna and Ryan, 2012; Dal Bó, Finan and Rossi, 2013) but focused on improving a single component of a complex reform. Our study decomposes a

complex, national-level policy into a set of simpler components, highlighting how we can exploit their complementarity to enhance the policy's effectiveness.

The paper is structured as follows. Section 4 outlines the theoretical model that guides our analysis of corruption in bureaucratic careers. Section 2 provides the institutional context and descriptive summary of the data for municipal governments, bureaucracies and anti-corruption audits in Brazil. Section 3 discusses our empirical strategy and reduced-form results, while 5 presents the results of our structural estimation and counterfactuals. Section 6 concludes.

# 2 Context and Data

This section describes the careers of municipal bureaucrats and the municipal anti-corruption audits program in turn providing, for each, contextual information and descriptive statistics of the data, summarized in Table 1.

## 2.1 Municipal bureaucracies and management

### Context

Brazil is a presidential democracy with a decentralized federal system of government (Falleti, 2010). Municipalities are the lowest tier of government, counting over 5,000 units. Each municipality is composed of an executive (mayor) and a legislative (city council) branch, both elected simultaneously at four year intervals.

With the process of democratization in 1988, much of the social policy responsibilities were delegated to municipalities (Abrucio and Couto, 1996). As a result, the 1990s and 2000s saw a rapid expansion of local bureaucracies to manage and deliver these public goods and services services (Cardoso Jr et al., 2011). Currently, over half of Brazilian bureaucrats are hired and paid by municipalities.

Managerial practices are seldom meritocratic, as mayors enjoy wide discretion that they often use for patronage appointments and spoils distribution (Brollo, Forquesato and Gozzi, 2017; Colonnelli, Prem and Teso, 2017). As a result, the boundary between the private and public sectors is rather porous with, every year, around 20 percent of high-level bureaucrats leaving for the private sector.[3]

Similarly, managerial practices are also a municipal prerogative, leading to a wide variation in the extent and types of administrative practices implemented locally. In fact, the audits program was designed to assist municipalities in improving their managerial practices.

[3]More details on our classification of high-level bureaucrats below.

## Data

| | Value |
|---|---|
| **Dependent variable** | |
| Pct. departures | 30% |
| Pct. dismissals | 22% |
| Pct. hires | 59% |
| **Corruption** | |
| Management index | 0.405 |
| Number of intermediate faults | 48 |
| Number of serious faults | 8 |
| Number of audited items | 16 |
| Amount audited as pct. budget | 68% |
| **Employees** | |
| Amount audited (m$2010) | 6.825 |
| N employees | 84 |
| Pct. females | 55% |
| Pct. higher education | 23% |
| Age | 40 |
| Experience (years) | 22 |
| Median wage ($2010) | 328 |
| Pct. tenured contracts | 42% |
| **Sample size** | |
| N municipalities | 1,121 |
| N individuals | 276,303 |
| N individual-years | 847,161 |

Table 1: **Descriptive statistics.** This table reports descriptive statistics about the 1,112 municipalities that have been audited between 2006 and 2015 and their employees. Unless otherwise specified, all measures are municipality-year averages.

Employment data on municipal bureaucrats is gathered by the *Relação Anual de Informaçoes Sociais* (RAIS), an annual census of all employees, private or public, collected by the Ministry of Labor in Brazil. Every year, employers are mandated to file in information including, among others, age, wage, work experience and education[4] for all the employees on payroll. Irregularities are sanctioned by law, with fines being imposed on organizations found misreporting. Our dataset spans from 2006 to 2015.

Our analysis of career choices considers four outcomes. Directly related to the theory (section 4), we consider departures and dismissals from the bureaucracy. We consider two related concepts: the quality of management practices, which is an important theoretical mechanism in our model, and new hires into the bureaucracy. Our analysis focuses on

[4]This dataset has been widely used in other studies (e.g. Colonnelli, Prem and Teso, 2017; Brollo, Forquesato and Gozzi, 2017).

high-level bureaucrats, who are responsible for the top-level decisions in the management of public resources, and enjoy a direct connection to politicians.[5] Additionally, we only focus on those municipalities that have been audited during the period covered by our dataset, leaving us with 1,121 municipalities and 276,303 unique bureaucrats.

Our management index uses data from the *Pesquisa de Informações Básicas Municipais* (Munic), an annual survey conducted by the Institute of Brazilian Geography and Statistics (IBGE) that reports the presence of a set of institutional features (see Appendix B.2 for details). Following Grindle (2004); Bloom and Van Reenen (2007), the index is a simple count of good management practices implemented in the municipality, with higher scores denoting better management. We select practices that fall into three dimensions based on principles of "good governance": planning (e.g. does the municipality draft a transportation or city planning?), accountability (i.e. are there institutionalized accountability mechanisms, such as education boards or civil society consultations?) and operations (i.e. are there formal procedures to register transactions, contracts with third-parties?). This data comes with three important limitations: first, not all years in our sample are covered by the Munic;[6] second, the set of practices measured by the survey changes from year to year; and third, the survey uses self-reported data, opening the way for misreporting. We address these limitations by verifying that (1) good management correlates with low corruption (Appendix B.2), and (2) that our results are robust to including only those items that appear most frequently in the surveys (Appendix C.3).

Table 1 shows that turnover is relatively high. This largely owes to seasonality in staff rotation, with spikes in departures, dismissals and hiring around election years (see Appendix B.3 for additional details). On any given year, 52% of our sample drops out of the bureaucracy through departures or dismissals, and 59% bureaucrats are new hires. Additionally, we use the Blinder-Oaxaca decomposition **??** to predict the counterfactual wage of public-sector employees had they joined the private sector (see Section 5.1 for a discussion). Figure 1 shows the distribution of the ratio public / private sector wage for all bureaucrats in our sample, showing that 89% of bureaucrats enjoys a public-sector premium, with the median employee enjoying a 14% premium.

[5] As identified by the *Classificação Brasileira de Ocupações* (CBO) occupation classification, we subset our data to all public employees who belong to group 1. This category includes high-level staff in public administration, such as cabinet members, senior managers and directors.

[6] Data is not available for the years 2008, 2010 and 2014-5.

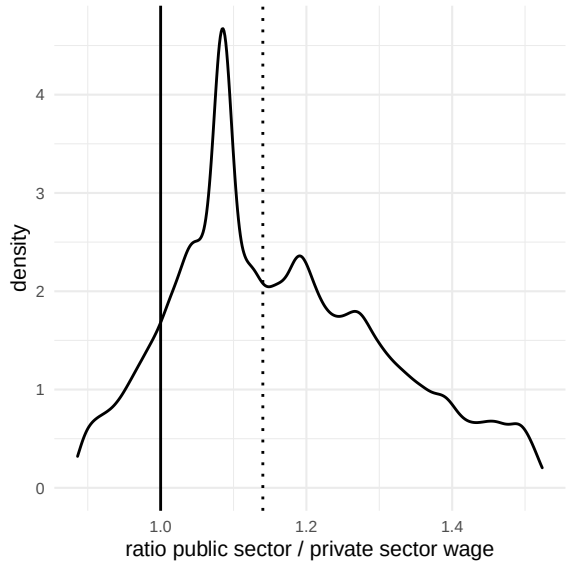


Figure 1: **Distribution of the ratio public/private sector wage.** The dotted bar represents the median.

## 2.2 Federal transfers and anti-corruption audits

### Context

Brazilian municipalities rely to a large extent on federal transfers to fund their operations and payroll (Arretche, 1999). For some, these constitute over 90 percent of their local budget (Prado, 2001). As such, the Comptroller General of the Union (CGU) institutionalized in 2003 a nation-wide audits program aiming at tackling mismanagement and corruption identify irregularities in municipalities in Brazil.[7] Municipalities to be audited are selected at random through a yearly, state-level lottery that selects 60 municipalities at random. Shortly after the lottery, teams of ten to fifteen auditors are sent to municipalities with a legal mandate to inspect service items and report potential irregularities in the programs that are funded through federal transfers. In our sample, auditors inspect an average of 16 items, corresponding to about $6.8 million, or 68% of municipal budget (Table 1). Auditors are meritocratically recruited and are sent for two weeks to ensure a short time-horizon to complete the audit, as well as reducing the potential for capture. Previous studies have found no evidence that auditors manipulate reports (Avis, Ferraz and Finan, 2018; Ferraz and Finan, 2008)

Neither auditors nor the CGU have direct sanctioning power over municipalities. Irregularities are reported to the federal-level ministry responsible for the particular problematic item, and it is incumbent on that ministry to punish corruption, e.g. withholding transfers until irregularities are addressed. The CGU only has jurisdiction over the au-

[7]For a description of the program, see `https://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos`.

diting of local budgets. Audits also often trigger informal political sanctions. Ferraz and Finan (2007) show that findings of corruption are often used by the opposition against the incumbent, thereby decreasing the incumbent's chances of reelection. Finally, audits may have indirect legal consequences. Recently, the federal police, in conjunction with the CGU, began to increase efforts in cracking down on municipalities guilty of more egregious cases of corruption, such as over-invoicing or fraudulent public procurement.[8] Similarly, the local city council may use these findings as a basis to impeach mayors.[9]

**Data**

Our data reports the irregularities reported for each service order controlled by auditors from 2006 to 2015, as well as the amounts corresponding to each of these service orders. Irregularities fall into three categories: (1) notices, (2) intermediate faults, and (3) serious faults.

We construct a series of municipal-level indicators of corruption from reported irregularities. Audits report an average of 50 intermediate faults and 8 serious faults per municipality (Table 1). However, while our reading of reports suggest that serious faults tend to pick up corruption, we also found, in line with Avis, Ferraz and Finan (2018) that the difference between intermediate and serious faults is quite blurry, as some intermediate faults also capture cases of intentional abuse of public office for private gain such as over-invoicing, shadow employees, and rigged public procurement (see Appendix B.1 for examples). Furthermore, since larger municipalities have larger budgets, they tend to report more irregularities.

Since there is no a priori good reason to select a particular corruption metric over another, we derive a variety of such metrics and carry our analysis over the least correlated among those. Specifically, we use, following Avis, Ferraz and Finan (2018), the count of intermediate and serious faults and the count of serious faults only. We then normalize these two metrics by the number of items audited, and also by the amount audited. Figure 12a reports the correlation among those metrics. While many of those metrics are highly correlated, the metrics that are normalized by amount stand out. We end up selecting two simple measures (*all faults*, and *serious faults*), and the two normalized measures that least correlate with those (*all faults by amount*, and *serious faults by amount*). Additionally, the stringency of auditing criteria has varied over time, with audits picking up increasingly many intermediate faults until lottery 25, and fewer after lottery 35. We remove this time trend by de-meaning irregularity counts by lottery (see Appendix B.1 for details), and validate our four measures by verifying that they

[8] See `http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/acoes-investigativas/operacoes-especiais`.

[9] See `https://g1.globo.com/sp/sao-paulo/noticia/2018/12/13/prefeito-continuou-chefiando-esquema-de-propina-em-maua-mesmo-apos-prisao-em-maio-diz-pf.ghtml`.

reproduce the main finding of Avis, Ferraz and Finan (2018); namely, that municipalities that have been audited twice show less corruption on their second audit. In the analysis, we group municipalities into terciles, creating equal-sized groups of low-, moderate- and high-corruption municipalities.

### 2.3 Additional data

To supplement our estimation, we collect additional data from a variety of sources. Information on electoral outcomes are gathered from the Supreme Electoral Court (TSE), containing mayor covariates such as incumbency status, age, gender and education level. Municipal budget from 2006 to 2015 is gathered from *Finanças Brasil* (FINBRA), and demographic data from the 2001 census, collected by National Institute of Geography and Statistics (IBGE).

## 3 Reduced-form estimation

In this section, we ask a simple question: after an audit occurs, what happens to the careers of those bureaucrats that are currently employed in the bureaucracy? We also investigate whether audits impact managerial practices, since their improvement may indirectly curb corruption, by subjecting bureaucrats to an environment in which engaging in corruption is more difficult. We leverage the randomized nature of these audits to compare municipalities that have been audited and municipalities that have not been audited yet. Doing so, we causally identify the extent to which audits trigger waves of departures/dismissals, and improvements in managerial practices.

**TODO: something on political models ??**

We find that audits have no impact on career outcomes; in other words, they fail to trigger waves of departures/dismissals. We find, however, that audits lead to modest improvements in management in highly corrupt municipalities, suggesting that audits might improve the environment in which bureaucrats operate, hence reducing corruption. Overall, results therefore suggest that audits have no observable ex-post effects on bureaucratic careers and corruption.

In what follows, we describe our approach in more details, present the results, and finally discuss how, by focusing on the observable, ex-post effects of audits, this approach is insufficient to pin down whether audits manage to curb corruption.

### 3.1 Approach

We evaluate the short-term effects of those audits on careers by estimating their average treatment effect on three outcomes: career interruptions (through dismissal or voluntary departure), and management practices.

The effect of audits should differ depending on whether the municipality was found guilty of corruption or not. In other words, one should not expect a municipality that was not found guilty of corruption to dismiss any of its bureaucrats. As such, we gear our empirical strategy towards estimating heterogeneous treatment effects. To accomplish this, we restrict our analysis to the municipalities that have been audited over the period, because the outcome of the audit is only observable in those municipalities, and construct a time-invariant municipality *type* from the result of that audit.[10] For our estimation, we construct a trichotomous variable $c_j \in \{0, 1, 2\}$ that determines whether municipality $j$ shows low, moderate, or high corruption ($c_j = 0, 1, 2$ respectively), using terciles of the distribution of corruption.

We observe municipalities for years ranging from $\underline{t} = 2006$ to $\bar{t} = 2015$. During this period, each municipality in our sample is treated by a random anti-corruption audit at least once. Let $\tau_{jt}$ be a binary variable that equals 1 if municipality $j$ has been audited during or prior year $t$, and equals 0 otherwise. Suppose municipality $j$ was audited on year $t_j \in \{\underline{t}, ..., \bar{t}\}$. For every municipality $j$, we observe a sequence $(\tau_{j\underline{t}}, ..., \tau_{j\bar{t}})$ such that $\tau_{jt} = 0$ for any $t < t_j$ and $\tau_{jt} = 1$ for any $t \geq t_j$. We compare, within-year, our four outcomes in municipalities that have been audited to those same outcomes in municipalities that have not been audited yet, for municipalities with the same level of corruption – low, medium, or high. With $1\{.\}$ the indicator function, our main specification reads as follows:

$$y_{jst} = \alpha_t + \alpha_s + \beta_2 \tau_{jt} + \sum_{k=1}^{2} \beta_{1k} 1\{c_j = k\} + \beta_{3k} \tau_{jt} 1\{c_j = k\} + \beta_4 x_j' + \epsilon_{jst}, \qquad (1)$$

with $y_{jst}$ one of our three outcomes measured in municipality $j$ within state $s$ during year $t$. Therefore, $y_{jst}$ is either the log number of voluntary departures, the log number of dismissals, or a management index ranging between 0 and 1. The vector $x_j$ contains time-invariant controls; namely, the log number of employees in 2006, as well as their median wage, and the municipality-level illiteracy rate, urbanization rate and gini measured in the 2001 census, to which we add the number of audited items. Finally, $\epsilon_{jst}$ is an error term.

The model in equation 1 identifies the effect of an audit on the municipality-level outcome $y_{jst}$. Parameter $\beta_2$ identifies the average treatment effect of an audit on municipalities with little corruption, while parameters $\beta_2 + \beta_{3,1}$ and $\beta_2 + \beta_{3,2}$ identify the average treatment effect of an audit on municipalities with moderate and high corruption respectively. Since audits are randomized at the state level, we include a state fixed effect $\alpha_s$ and make within-year comparisons using a year fixed effect $\alpha_t$. Additionally, we cluster standard errors at the municipality level.

[10] If municipality $j$ has been audited twice, we construct that variable using the results of the first audit.

The effect of audits may present specific time dynamics. One might hypothesize that audits lead to swift waves of departures immediately after they occur or, conversely, that it takes several years to be able to dismiss tenured bureaucrats. To consider these possibility, we amend the specifications in equation 1 and parametrize the treatment effect flexibly. We turn our treatment indicator $\tau_{jt}$ into a categorical variable that equals to 0 prior treatment in year $t_j$, and then counts the years after treatment: $\tau_{jt} \equiv \max\{0, t - t_j + 1\}$. We therefore compare, within year, the bureaucrats that have not been audited to bureaucrats that have been audited that year, one year ago, two years ago, and so forth.

Our last set of results shows that the electoral accountability mechanisms that we know affect politicians' careers do not trickle down to bureaucrats. To do so, we show that the hypothesis fails to pass an easy test. We focus our analysis on the cohort of bureaucrats hired by a mayor in her first term, which largely correspond to patronage appointments. If a mayor in her first term – who therefore may run for reelection – is found corrupt, she may have an incentive to dismiss her clients in order to wither down future electoral sanctions. Should she lose the elections, her successor also has an incentive to dismiss those bureaucrats. Intersecting these considerations is when the audits take place: presumably, audits that occur later on in the term for a first mayor, or in a more recent past for the second mayor, should have a stronger effect on bureaucratic personnel. We estimate these effects simultaneously.

We use a flexible parametrization to estimate treatment effects conditional on the political cycle. Recall that elections occur every four years. We account for a political trend that varies by municipality type using year-corruption type fixed effects. Additionally, we track, over time, the effect of having been audited on year 1, 2, 3, and 4 of the political cycle using a series of dummy variables.

We check the robustness of our findings by conducting a series of tests, either probing the substance of the theory, or the statistical validity of the findings (results reported in Appendix C). Regarding the substance of the theory, it might be that audits affect other segments of the bureaucracy. In other words, it might be that only a small number of key bureaucrats get dismissed. Conversely, it might be that audits trigger mass layoffs among less important employees. We show that our results extend to other categories of employees (namely, low bureaucrats, as well as high and low frontline workers, Appendix C.4), as well as to the most important high bureaucrats (i.e. municipal secretaries, Appendix C.5). It might also be that audits affect the composition of the pool of bureaucrats operating in the bureaucracy, and push mayors to hire more honest types. As such, we probe into hiring practices, by considering the number of hires following an audit. We find that audits have no significant effects on hiring patterns (Appendix **??**).

We also conduct tests that aim at verifying the statistical validity of the findings. We verify the randomization of audits by conducting a balance test comparing municipali-

ties that were audited early match municipalities that were audited later on (Appendix C.1). We show that our results are robust to the four corruption metrics outlined in section 2.2. We show that results are robust to using a measure of personnel turnover that uses percentages instead of log counts (Appendix C.2), and measures of management that use only the items that occur most frequently (Appendix C.3). They also We also show robustness to considering the subset of municipalities that have not been audited prior to 2006, the beginning of our period (Appendix C.6). Finally, we consider individual-level outcomes instead of municipal-level aggregates and show robustness to such disaggregation (Appendix C.7).

## 3.2 Results

Table 2 shows our main results, for the simplest corruption metric (total number of faults). Departure and dismissal rates for moderate- and high-corruption municipalities are indistinguishable from those of non-corrupt municipalities (columns 2 and 4). Audits induce, however, significant improvements in management in highly corrupt municipalities (models 7 and 8). While audits have no effect in low and moderate corruption municipalities, they have a positive effect on the quality of management in highly corrupt municipalities ($\beta_{32} > 0$), and the overall effect of audits in high-corruption municipalities is statistically significant ($\beta_2 + \beta_{32} > 0$). The effect is, however, substantively small, whith audits increasing the quality of management by 2.2 percentage points; that is, a 5% increase relative to the sample mean. Figure 2 shows that results extend to all the corruption metrics we consider.

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | No. of departures (log) | | No. of dismissals (log) | | No. of hires (log) | | Management index | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Audited ($\beta_2$) | 0.134* | 0.068 | −0.075 | −0.111 | 0.019 | −0.061 | 0.011 | 0.0003 |
| | (0.078) | (0.068) | (0.087) | (0.073) | (0.092) | (0.072) | (0.009) | (0.007) |
| Moderate corruption | 0.152 | 0.123 | 0.076 | 0.090 | 0.105 | 0.095 | 0.006 | 0.004 |
| | (0.107) | (0.088) | (0.111) | (0.090) | (0.130) | (0.089) | (0.011) | (0.009) |
| High corruption | 0.125 | 0.039 | 0.053 | 0.070 | 0.018 | −0.035 | −0.008 | −0.020* |
| | (0.113) | (0.104) | (0.117) | (0.108) | (0.139) | (0.110) | (0.012) | (0.011) |
| Audited × Moderate corruption ($\beta_{31}$) | −0.134 | −0.104 | −0.001 | 0.039 | −0.068 | −0.027 | −0.016 | −0.009 |
| | (0.108) | (0.095) | (0.121) | (0.102) | (0.129) | (0.101) | (0.012) | (0.010) |
| Audited × High corruption ($\beta_{32}$) | 0.008 | −0.020 | 0.103 | 0.115 | 0.219* | 0.197* | 0.023* | 0.022** |
| | (0.109) | (0.094) | (0.109) | (0.095) | (0.127) | (0.101) | (0.012) | (0.010) |
| Controls | _ | ✓ | _ | ✓ | _ | ✓ | _ | ✓ |
| $\beta_2 + \beta_{31}$ | 0 | -0.036 | -0.076 | -0.072 | -0.049 | -0.088 | -0.006 | -0.008 |
| $\beta_2 + \beta_{32}$ | 0.142 | 0.048 | 0.028 | 0.004 | 0.237** | 0.136 | 0.033*** | 0.022*** |
| Observations | 5,053 | 5,053 | 5,053 | 5,053 | 5,053 | 5,053 | 5,053 | 5,053 |
| $R^2$ | 0.148 | 0.300 | 0.132 | 0.269 | 0.237 | 0.473 | 0.316 | 0.441 |
| *Note:* | | | | | | | | *p<0.1; **p<0.05; ***p<0.01 |

Table 2: **Main results.** On average, audits have no effect on career interruptions (models 1 to 4). Audits do not decrease the number of new hires for municipalities with low and intermediate corruption either (models 5 and 6). Finally, audits are effective in improving management practices in highly corrupt municipalities (models 7 and 8). In rows $\beta_2 + \beta_{31}$, $\beta_2 + \beta_{32}$, significance stars are derived from an F-test that tests the null hypothesis $\beta_2 + \beta_{\cdot} = 0$. All models include year and state fixed effects, and measure corruption using all faults. Standard errors clustered at the municipality level. See section 3.1 for details about controls.

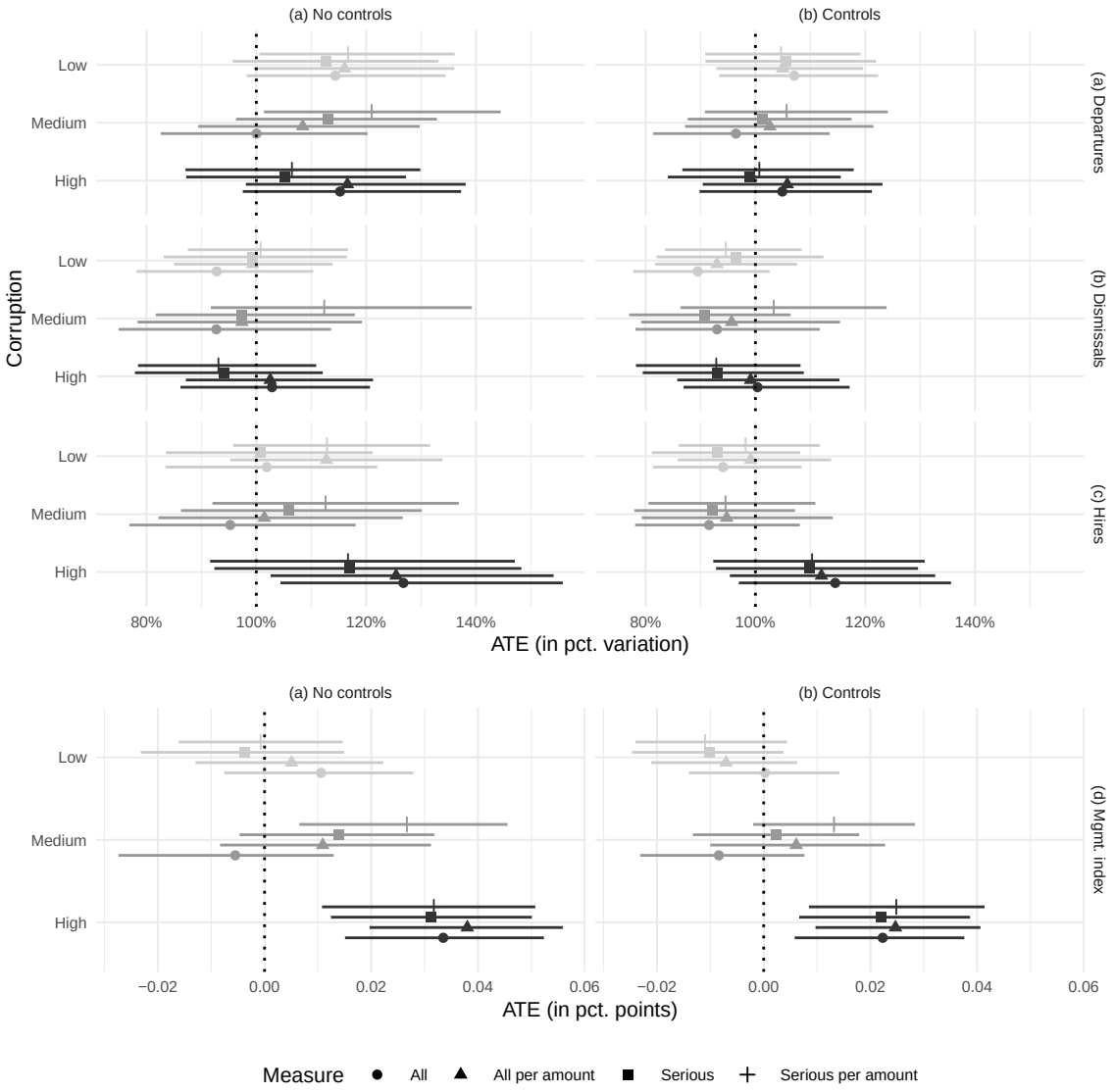


Figure 2: **Main result, different corruption metrics.** This table reestimates the specifications in Table 2 using all four corruption metrics. For each specification, we report the parameters $\beta_2$, $\beta_2 + \beta_{2,1}$, and $\beta_2 + \beta_{3,1}$. The top panel exponentiates these parameters to report the percentage of variation. Bars are 95 percent confidence intervals derived using semi-parametric bootstrap. Irrespective of the corruption metric, audits have a significant positive effect on the management index only for high-corruption municipalities. All other effects are not consistently significantly different from zero.

Analyzing the effects of audits over time (Figure 3) confirms that audits have no discernible effects on career interruptions nor hiring patterns (top three panels): for all three types of municipalities, departure and rates are comparable to pre-audit levels. Highly corrupt municipalities, however, sustain improvements in managerial practices of 0.04 percentage points immediately after an audit, with a sustained effect of 4-5 years.

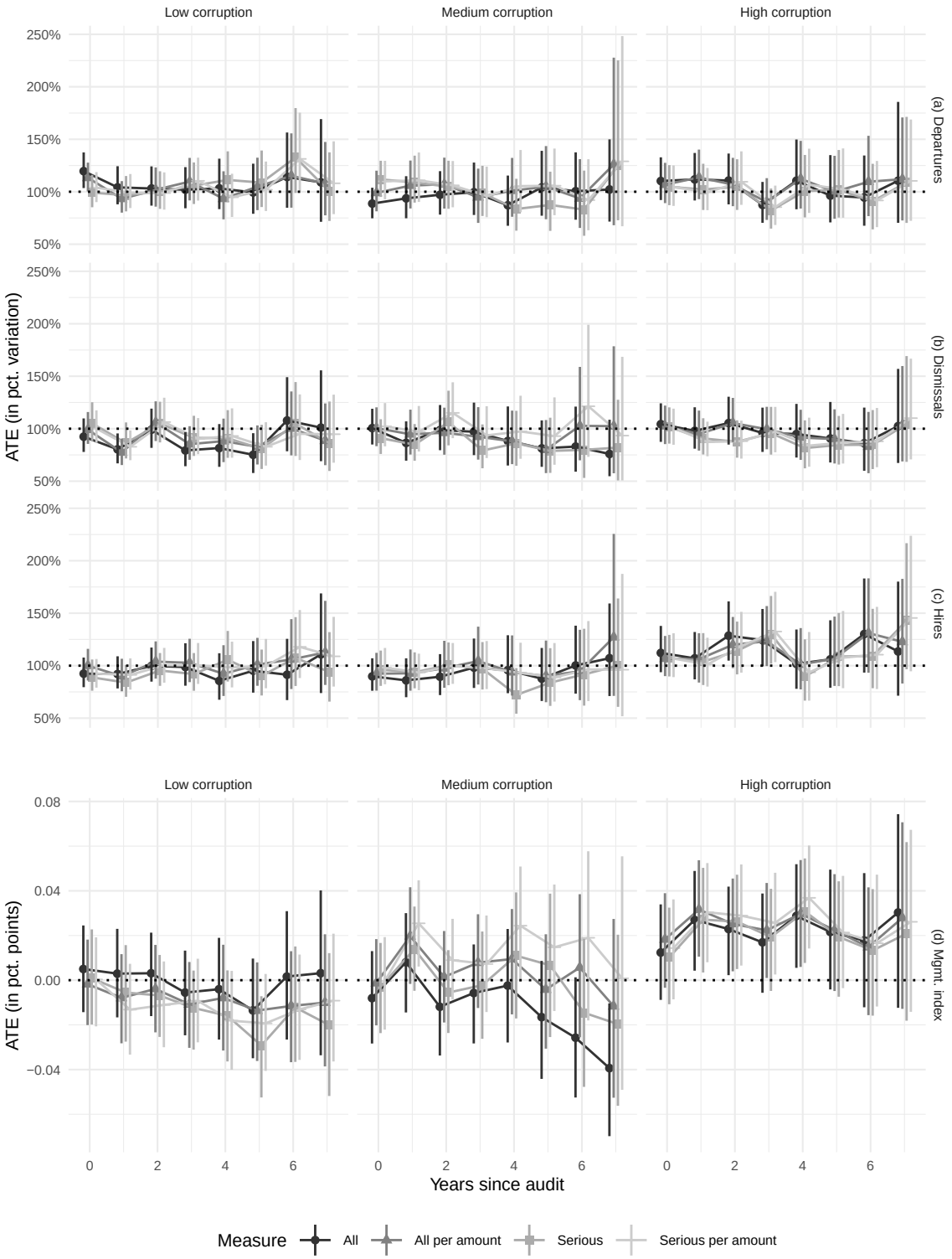


Figure 3: **Treatment effect over time.** The y-axis represents the average marginal effect of some number of years after the audit on the row outcome. It is measured in percentage points for the top and bottom panels, and in percentage of variation for the middle panel. Bars are 95 percent confidence intervals clustered at the municipality level. All specifications include the controls discussed in section 3.1. Audits significantly improve management practices in high-corruption municipalities 1 to 5 years after the audit, irrespective of the corruption metric being used. All other effects are not consistently significantly different from zero.

We finally show that electoral accountability mechanisms do not trickle down to bureaucrats. Figure 4 reports the effects of audits during a mayor's first term, using all faults as a measure of corruption. The figure uses year 1 of the electoral term as a reference category, and plots effect sizes relative to the reference category, set to the first year of term in a non-audited municipality of the type reported in the columns. As such, the red line in the top-left panel indicates variation in departures in a non-audited municipality, relative to year 1 of the term. The dark lines capture variation in departures in a municipality audited in years 1, 2, 3, and 4 of the term, again relative to relative to year 1 of the term in a non-audited municipality. While dismissals and departures do exhibit seasonality, with spikes in the first and last years of the term, audits do not significantly affect those patterns. In Appendix C.8, we report similar effects for the subsequent mayor, and show that the findings extend to the remaining three corruption metrics.

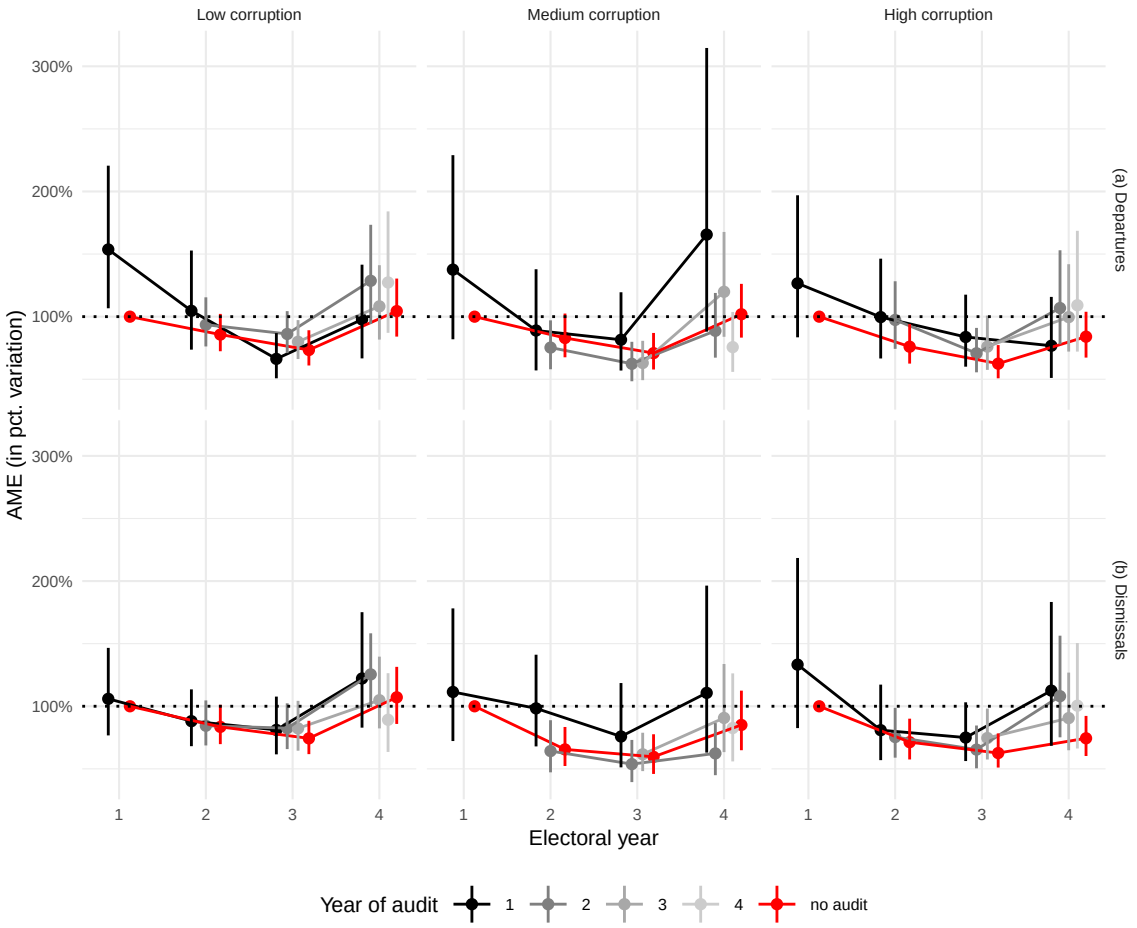


Figure 4: **Treatment effect as a function of the political cycle during the first term.** The y-axis represents the average marginal effect of audits the row outcome. The x-axis represents years in the political cycle, with year 1 being the first year of mandate. Colors indicate the year of the political cycle during which the audit occurred. Bars are 95 percent confidence intervals clustered at the municipality level. All models use the controls discussed in section 3.1. There is no evidence that audits lead to greater dismissals or departures of high-level bureaucrats.

## 3.3 Discussion

We have seen that audits not trigger waves of departures or dismissals, but lead to small, temporary improvements in managerial practices. These results are, however, insufficient to tell us whether audits effectively curb corruption. At first sight, the fact that audits trigger no career interruptions and have modest effects on the quality of managerial practices suggests that they have no effect on bureaucratic careers and corruption. However, two alternative explanations might both be consistent with these findings and the fact that audits effectively reduce corruption. First, those improvements in the quality of management might be substantial enough to trigger an *unobservable, ex-post* effect; specifically, to prompt bureaucrats to temporarily switch away from dishonest behavior by making it more difficult to steal. Second, it may be that audits have *disciplining*

effects; in other words, they would trigger no career interruptions because even highly corrupt municipalities are not very corrupt – recall that our measure of corruption is relative, – because the program has successfully deterred bureaucrats from engaging in corruption. This alternative would be consisted with the fact that the program is, by the end of our period, about 15 years old, giving ample time for bureaucrats to learn about the potentially severe consequences of engaging in corruption.

Yet, our current empirical strategy makes it difficult to assess these alternative explanations. Indeed, we currently leverage the fact that audits are randomized to compare audited municipalities to municipalities that have not been audited yet. As such, the strategy may only identify *ex-post, observable* effects. Probing whether audits have disciplining effects using randomization would require manipulating the *threat* of an audit, and not the occurrence of audits themselves.[11] Similarly, with the current approach, assessing whether audits trigger temporary switches away from dishonest behavior poses a measurement problem, since it would require measuring corruption without an audit.

We use structural estimation to circumvent these difficulties. We first develop a model that intersects bureaucratic careers with randomized audits, in order to highlight the full range of effects that these audits may have on both observable, and unobservable outcomes. We then structurally estimate the model on our data, treating corruption in time-periods during which the bureaucracy is not audited as a latent variable. In what follows, we first describe the model and derive a series of theoretical intuitions in simple settings, and then describe our estimation procedure and results.

# 4 Theory

In this section, we introduce a model that highlights all potential ways in which anti-corruption audits may impact bureaucrats' careers and reduce corruption. Indeed, our reduced-form estimates revealed that audits have no observable ex-post effects on bureaucratic careers suggesting that, at first sight, audits fail to curb corruption. Yet, our discussion highlighted a series of potential alternative explanations that would be consistent with both the observed patterns and the fact that audits do reduce corruption. The goal of this model is twofold: first, to exhaust all potential channels through which audits may reduce corruption, in order to ascertain that we are not missing any; and second, to devise a setting that lends itself to structural estimation, in order to identify said channels in the data.

In the model, a bureaucrat is employed in a bureaucracy at $t = 0$ and decides on a career plan that maximizes her permanent income. At each time period, she decides

[11] This approach has been explored for the current program (Poulsen, 2019), leveraging a one-year experiment within the program. Results suggest that these audits have indeed no significant disciplining effects, but are severely constrained by lack of power.

whether to simply remain employed, engage in corruption, or depart to the private sector. At each time period, the bureaucracy may get audited.

Ex-post, audits impact the agent's environment through two channels. First, with some probability, audits punish corruption that occurred in the previous period, and lead to the agent's dismissal, which we capture by a *monitoring technology* parameter. The agent then joins the private sector but incurs a temporary *wage penalty*, capturing a potential "red mark" that limits the agent's capacity to find a private-sector job. Second, audits may trigger a *clean-up* of the bureaucracy that could stem, among others, from the improvements in the quality of management that we identified empirically in the previous section. Clean-ups temporarily reduce the profitability of engaging in corruption. We say that an environment in which these impacts are severe is an environment in which audits have high *bite*.

We show that, depending on how much bite they have, audits may have three kinds of consequences on bureaucrats careers and corruption. First, if audits have low bite, then they have *no impact* on either careers or corruption: the bureaucrat remains in the public sector and always engages in corruption. If audits have intermediate bite, they have *ex-post effects*; that is, they lead to changes in behavior in response to the auditing event itself. Those include the bureaucrat getting caught for corruption and getting dismissed and, because audits may trigger improvements in management that make engaging in corruption less profitable, either temporarily refraining from corruption until the audit is over, or a departure to the private sector. Finally, if audits have high bite, they have *disciplining effects*. In other words, they lead to permanent changes in behavior that reduce corruption, *in anticipation* of the audits. Those include either refraining well-paid bureaucrats from stealing, or triggering preemptive departures from the bureaucracy.[12]

In what follows, we first describe the setting, and then derive optimal behavior under three specifications of the model. First, we examine a baseline in which audits have no bite. We then increase bite one channel at a time and allow, in turn, for audits to induce a wage penalty and to trigger clean-ups. All proofs are available in Appendix A. We conclude by discussing what these cases tell us about the permanent and ex-post impacts of audits on careers and corruption, and discussing model assumptions.

## 4.1 Setting

In the model, an agent is employed in a bureaucracy at $t = 0$. At each time period $t \geq 0$, she chooses an action $a_t \in \mathcal{A} = \{0, 1, 2\}$, with $a_t = 0$ corresponding to no action, $a_t = 1$ to engaging in corruption, and $a_t = 2$ to departing to the private sector, which corresponds to state $s_t = P \in \mathcal{S}$. Furthermore, at each time-period, the bureaucracy gets

[12]This latter result, although not apparent in the simple cases we analyze in this section, becomes apparent when introducing a baseline probability of getting dismissed $q_0 \neq 0$.

audited with probability $p$. If the bureaucracy is not audited, the agent is in the *normal* state $s_t = N$. She is in state $s_t = A$ otherwise. As a bureaucrat, at any time-period, the agent may also get dismissed, in which case she joins the private sector, but incurs a one-period penalty (state $s_t = P'$) before joining state $P$.

Overall, there are two occupations (public and private sector); four states ($N$ and $A$, which correspond to the public-sector occupation; and $P$ and $P'$, corresponding to the private-sector occupation); and three actions ($\mathcal{A} = \{0, 1, 2\}$).

Transitions between states depend on the current state and actions. If the agent is in the public-sector occupation and chooses to depart, she moves to the private-sector occupation: $\Pr(s_{t+1} = P | s_t, a_t = 2) = 1$ for $s_t \in \{A, N\}$. Departures are definitive, so that $\Pr(s_{t+1} = P | s_t = P, a_t) = 1$ for any $a_t \in \mathcal{A}$. If she chooses to stay, she may get dismissed with baseline probability $q_0$.[13] Additionally, if the agent is audited and stole in the previous time period, she gets detected and dismissed with probability $q$, which captures the *monitoring technology* associated with audits. In other words, the agent enters the punishment state with probability $\Pr(s_{t+1} = P | s_t, a_t = 0) = q_0$ and $\Pr(s_{t+1} = P' | s_t, a_t = 1) = q_0 + (1 - q_0)pq$, for $s_t \in \{A, N\}$. Since punishment lasts only one period, $\Pr(s_{t+1} = P | s_t = P', a_t) = 1$ for any $a_t \in \mathcal{A}$. The agent enters the normal state with probability $\Pr(s_{t+1} = N | s_t) = (1 - q_0)(1 - p)$ for $s_t \in \{A, N\}$, and the audited state with probability $(1 - q_0)p(1 - q)$.

If the agent is employed in the bureaucracy and chooses not to depart to the private sector (i.e. if $a_t \neq 2$), she earns her public sector wage $w > 0$. Additionally if she engages in corruption (i.e., if $a_t = 1$), she pockets the illegal *rent* $b \geq 0$. Audits, however, may lead to a temporary *clean-up* of the bureaucracy, stemming, for instance, from the improvements in the quality of management we identified in the previous section. Clean-ups reduce the benefits from corruption by $c \in [0, b]$ for one period. In the private sector, in period $t$, she earns private sector wage $\overline{w} > 0$. However, when the agent is punished, she undergoes private-sector *wage penalty* $k \in [0, \overline{w}]$. The agent's payoff at period $t$ is $u : \mathcal{A} \times \mathcal{S} \to \mathbb{R}$ writes

$$
\begin{aligned}
u(0, s_t) &= w \text{ for } s_t \in \{A, N\} \\
u(1, N) &= w + b \\
u(1, A) &= w + b - c \\
u(a_t, P) &= \overline{w} \text{ for any } a_t \in \mathcal{A} \\
u(a_t, P') &= \overline{w} - k \text{ for any } a_t \in \mathcal{A}
\end{aligned}
$$

[13] Although not relevant for a theoretical exercise, this parameter improves model fit when conducting structural estimation. In the data, bureaucrats may get dismissed even in the absence of an audit. Parameter $q_0$ captures these events in a reduced form.

The agent is infinitely-lived, discounts the future with rate $\delta \in (0,1)$, and maximizes her permanent income. In other words, she chooses a policy $\pi : \mathcal{S} \to \mathcal{A}$, which maps states $s_t$ to actions $a_t$.[14] With $\Pi$ denoting the set of possible policies, our agent solves the following dynamic programming problem

$$\max_{\pi \in \Pi} \mathbb{E}\left[(1-\delta)\sum_{t=0}^{\infty} \delta^t u(\pi(s_t), s_t)\right]$$

for initial state $s_0 \in \{A, N\}$.

In what follows, we solve this problem under a few additional assumptions that we will relax when estimating the model structurally.[15] Throughout the section, we assume that the baseline probability of dismissal $q_0 = 0$. Additionally, how public and private sector wages compare has important implications. There are three possible cases:

$$w < w + b < \overline{w} \tag{2}$$

$$w < \overline{w} < w + b \tag{3}$$

$$\overline{w} < w < w + b \tag{4}$$

Case 2 is not interesting in the context of this model, because departing to the private sector dominates both behaving honesty in the bureaucracy and engaging in corruption. In case 3, bureaucrats are underpaid relative to the private sector, but corruption is more profitable than private-sector employment. In case 4 on the other hand, bureaucrats are overpaid relative to the private sector.

We start by analyzing the simplest model, setting $k = c = 0$. We then examine how introducing a wage penalty changes the results, and analyze the case in which $k > 0$, $c = 0$. We finally examine how introducing clean-ups affects the results, focusing on the case in which $k = 0$, $c > 0$.

## 4.2 A baseline model

We first analyze the case where $k = c = 0$. Note that in this case, states $A$ and $N$ are payoff-equivalent, and so are states $P$ and $P'$. As such, we only need consider policies in which the agent steals in both states ($\pi(N) = \pi(A) = 1$), in neither state ($\pi(N) = \pi(A) = 0$), or quits preemptively ($\pi(N) = \pi(A) = 2$).

When $w < \overline{w} < w + b$, the only reason to stay in the public sector is to pocket rents. Since engaging in corruption is more profitable than quitting and incurs no penalty, it is optimal for the agent to steal in every period until she gets dismissed. When $\overline{w} < w <$

[14] Since payoffs $u$ are bounded and stationary, and transition probabilities are also stationary, and $\mathcal{S}$ and $\mathcal{A}$ are finite, a stationary policy $\pi : \mathcal{S} \to \mathcal{A}$ is optimal.

[15] Specifically, the structural model makes no additional assumptions on $q_0, k, c$.

$w+b$, the agent would rather stay in the public sector than depart to the private sector. As such, if corruption is very profitable (i.e. if $b$ is large enough), then the expected benefits from corruption are higher than the risk of joining the private sector, and stealing is optimal. Conversely, when corruption is not profitable enough, not stealing is optimal (see Figure 5 for an illustration). Formally:

**Proposition 1.** *If $k = c = 0$, then $\pi^*(N) = \pi^*(A) = 1$ is optimal if equation 3 holds. If equation 4 holds, then there is $b_0 > 0$ such that $\pi^*(N) = \pi^*(A) = 0$ is optimal whenever $b \leq b_0$, and $\pi^*(N) = \pi^*(A) = 1$ is optimal whenever $b \geq b_0$. Other stationary policies are not optimal.*

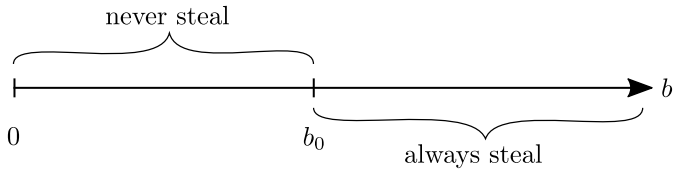


Figure 5: **Graphical illustration of proposition 1 in the case where $\overline{w} < w < w+b$.** The strategies "always steal" and "never steal" correspond, respectively, to $\pi(N) = \pi(A) = 1$ and $\pi(N) = \pi(A) = 0$.

## 4.3 Introducing a wage penalty

We now consider the case where $k > 0$ and $c = 0$. Compared to the baseline, states $A$ and $N$ are still payoff-equivalent, but states $P$ and $P'$ are not. Here engaging in corruption carries the additional cost of a one-time wage penalty $k$ after being dismissed.

When $w < \overline{w} < w + b$, then stealing is always optimal. The only reason why the agent would switch to its second most attractive policy – that is, quitting preemptively – is because the penalty from stealing $k$ is too high. Yet, such deterrence would require a disproportionately high outside option, i.e. it would require $\overline{w} \gg w + b$.

When $\overline{w} < w < w + b$, then

**Proposition 2.** *If $k > 0$ and $c = 0$, then there is $k_0$ such that $\pi^*(N) = \pi^*(A) = 1$ is optimal whenever $k \leq k_0$, and $\pi^*(N) = \pi^*(A) = 0$ is optimal whenever $k \geq k_0$. If equation 3 holds, then $k_0 > \overline{w}$. If equation 4 holds, then there are $b_0, b_1$ with $0 < b_0 < b_1$ such that $k_0 < 0$ if $b < b_0$, $k_0 \in [0, \overline{w}]$ if $b \in [b_0, b_1]$, and $k_0 > \overline{w}$ otherwise. Other stationary policies are not optimal.*

Proposition 2, illustrated graphically in Figure 6, tells us that if the wage penalty is sufficiently high, it has a deterrence effect, pushing agents to never steal. Conversely, it if is not high enough, then the agent engages in corruption until she gets detected and punished. Again, since $w < \overline{w} < w + b$, never stealing is not attractive.

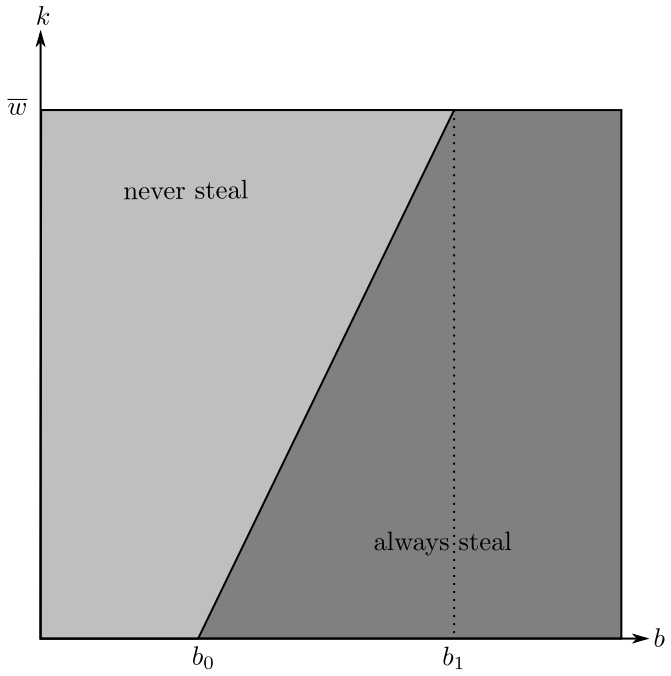


Figure 6: **Graphical illustration of proposition 2 in the case where $\overline{w} < w < w+b$.** The strategies "always steal" and "never steal" correspond, respectively, to $\pi(N) = \pi(A) = 1$ and $\pi(N) = \pi(A) = 0$.

## 4.4 Introducing a clean-up effect

We finally consider the case where $k = 0$ and $c > 0$. Compared to the baseline, states $P$ and $P'$ are still payoff-equivalent, but states $A$ and $N$ are not. Here, audits trigger a temporary clean-up of the bureaucracy, which make engaging in corruption less profitable after an audit.

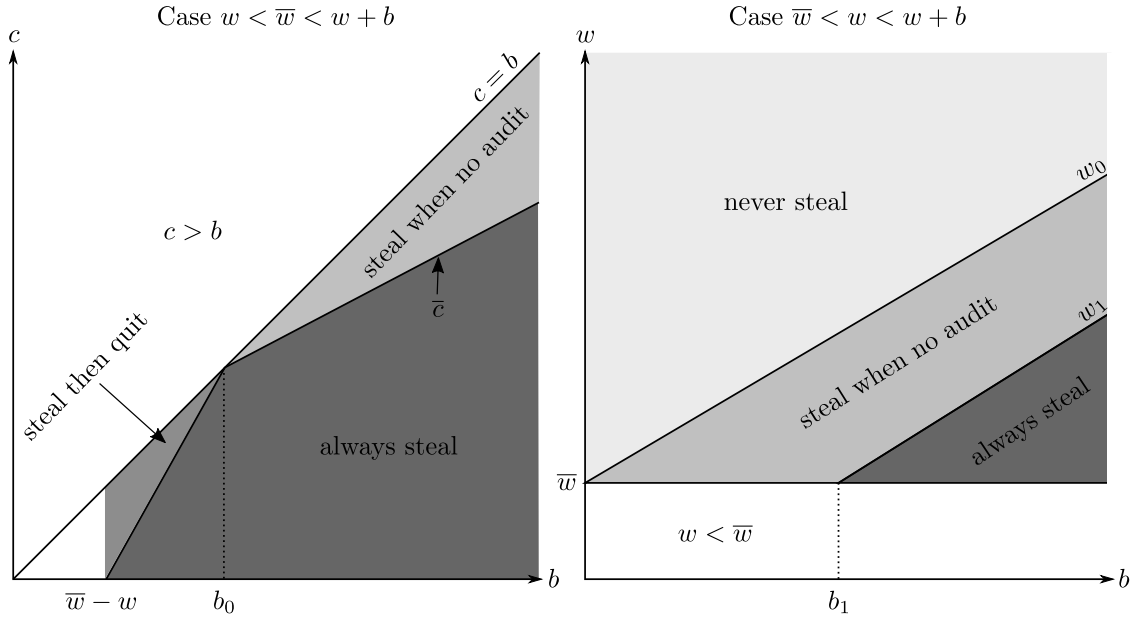


Figure 7: **Graphical illustration of proposition 3.** The policies "never steal," "always steal," "steal when no audit," and "steal then quit" correspond, respectively, to $\pi(A) = \pi(N) = 0$; $\pi(A) = \pi(N) = 1$; $\pi(A) = 0, \pi(N) = 1$; and $\pi(A) = 2, \pi(N) = 1$.

When $w < \overline{w} < w + b$ (Figure 7, left panel), always stealing is optimal if those the clean-up is sufficiently small. When those make the benefit from corruption too small, other policies become optimal, depending on the size of the benefit $b$. If both $b$ and $c$ are large, then the agent has an incentive to refrain from stealing for the one period during which the clean-up effect lasts (i.e. $\pi(A) = 0$), and resume afterwards (i.e. $\pi(N) = 1$). When $b$ is small, increases in $c$ make corruption less profitable overall, and agents retrench to the private sector after the first audit (i.e. $\pi(A) = 2$).

When $\overline{w} < w < w + b$ (Figure 7, right panel), always stealing is optimal if $w$ is sufficiently small and $b$ sufficiently large to both offset the clean-up effect and the risk of being dismissed to the less attractive private sector. As $w$ increases and $b$ decreases, agents first revert to only stealing after the clean-up has worn off. As $w$ further increases and $b$ further decreases, engaging in corruption is not worth the risk, so agents refrain from stealing. Formally:

**Proposition 3.** *Suppose $k = 0$ and $c > 0$. If equation 3 holds, then there is $\overline{c}(b) \leq b$ such that $\pi^*(N) = \pi^*(A) = 1$ is optimal whenever $c \leq \overline{c}$. The policy $\pi^*(N) = 1, \pi^*(A) = 2$ is optimal whenever $c \geq \overline{c}$ and $b \leq b_0$ such that $w + b_0 \geq \overline{w}$. Finally, the policy $\pi^*(N) = 1, \pi^*(A) = 0$ is optimal whenever $c \geq \overline{c}$ and $b \geq b_0$. If equation 4 holds, then the policy $\pi^*(N) = \pi^*(A) = 0$ is optimal whenever $w \geq w_0 \geq \overline{w}$. The policy $\pi^*(N) = 1, \pi^*(A) = 0$ is optimal whenever $w \in [w_0, w_1]$, with $w_1 \geq \overline{w} \iff b \geq b_1 > 0$. Finally, the policy $\pi^*(N) = \pi^*(A) = 1$ is optimal whenever $w \leq w_1$ and $b \geq b_1$. Other stationary policies are not optimal.*

## 4.5 Discussion

The model tells us that audits affect bureaucrats' careers through multiple channels. A useful way to categorize these effects is to separate them according to (1) personnel's behavior: whether bureaucrats depart the bureaucracy, get dismissed, or simply refrain from corruption while remaining employed, and (2) the timing of those effects: whether agents alter their behavior ex-ante, i.e. disciplining effects, or after the audit occurs, i.e. ex-post effects.

Audits may have disciplining effects, which occur when audits have high bite; that is, when $k, c$, or $q$ are high. Immediately, they may permanently deter corruption because the risk of getting dismissed and forced to join the private sector – perhaps compounded with ensuing wage penalties – outweighs the benefit. Such effect, however, requires that public sector wages be higher than private sector wages. Another disciplining effect, not seen in the above discussion, becomes apparent when considering a non-zero baseline probability of dismissal (i.e. $q_0 > 0$): audits trigger preemptive waves of departures.

As the bite of audits decreases, audits move from disciplining to ex-post effects. The most obvious such effects is the dismissal of employees that engage in corruption. Moreover, bureaucratic clean-ups immediately after an audit can prompt additional effects. Specifically, for those bureaucrats for whom the private sector is a poor option, while large improvements trigger permanent effects, smaller improvements push those bureaucrats to refrain from stealing temporarily. Clean-ups also have ex-post effects on those bureaucrats for whom the private sector is attractive: they steal until the clean-up occurs and then leave.

If audits are ineffective, then bureaucrats simply engage in corruption throughout their careers, and get dismissed if they ever get caught. In the context of our model, audits can fail to induce observable changes in bureaucratic behavior because they have little bite: $c$, $q$ or $k$ are too small to deter bureaucrats from always stealing.

This model makes a series of stark assumptions. Specifically, (1) we abstract away from a series of well-known determinants of individual bureaucratic behavior, especially as far as corruption is concerned, and (2) we consider an individual bureaucrat in isolation. Regarding the first point, the model does not explicitly feature public sector motivation, nor moral costs of corruption. These dimensions can, however, be easily integrated in the model by interpreting parameters $b$ and $w$ as reduced-forms. Specifically, parameter $w$ could incorporate both a monetary wage, as well as a latent public sector motivation. Conversely, parameter $b$ could incorporate both a (positive) monetary reward from corruption and a (negative) moral cost of corruption. In this setting, it may then be that, contra model assumptions, $b \leq 0$. In this case, the model becomes trivial: agents have no incentive to engage in corruption; they simply compare their public- and private-sector wages, and join the occupation that is most rewarding.

The second point, namely that the model considers an individual bureaucrat in isolation, is a simplifying choice. The setting does not explicitly feature other bureaucrats, nor a political principal. A more realistic model would have a political principal and other bureaucrats affect parameters $b, c, q$. More bureaucrats engaging in corruption may, for instance, impose externalities on the size of rents $b$, either reducing them (e.g. through crowding-out effects), or increasing them (e.g. through cooperation; see Shleifer and Vishny (1993) for a discussion of when either effect could materialize). Similarly, the political principal may engage in corruption herself, hence adopting lenient responses, and setting low $c, q$, or instead be tough on corruption, hence setting high $c, q$ (see e.g. Ferraz and Finan (2011) for a discussion of when each of these may occur). We interpret our results as partial equilibrium results in which we explore the full range of parameter values for $b, c$ and $q$. Doing so is in the spirit of the exercise, whose goal is to explore the full range of effects that audits may have. In other words, moving the model from decision theory to game theory by introducing additional players would eliminate some of the solutions we characterized above. We try instead to characterize all of them and leverage the data to identify which are the most likely using structural estimation.

# 5 Structural estimation

## 5.1 Approach

In the model examined in section 4, bureaucrat $i$ makes at each period $t$ the career decision $y_{it} \in \{0, 1, 2\}$, with 0 corresponding to not engaging in corruption, 1 to engaging in corruption, and 2 to departing to the private sector. Her decision varies depending on the state $s_t$, that captures whether the bureaucracy has been audited or not. In making this decision, agent $i$ takes into account the vector of parameters $\theta = (\delta, p, q_0, q, b, c, w, \overline{w}, k)$, with $\delta$ the discount factor; $p$, the probability of being audited; $q_0$, the baseline probability of being dismissed; $q$, the probability of being dismissed during an audit; $b$, the benefits from engaging in corruption; $c$, decreases in the benefits from corruption occurring after an audit; $w$, her public-sector wage; $\overline{w}$, her counterfactual, private-sector wage; and $k$, a wage penalty incurred upon dismissal.

The econometrician observes some of these variables and parameters. The states audited ($s_t = A$) and not audited ($s_t = N$) are observed, and career decisions are partially observed. The econometrician observes whether the agent decided to depart ($y_{it} = 2$), or whether she got dismissed ($y_{it} \equiv 3$). However, if the agent decided to leave, the econometrician does not observe whether she engaged in corruption or not ($y_{it} \in \{0, 1\}$). Similarly, the econometrician observes $p, q_0$, and $w$, but does not observe $b, c, q, \delta, \overline{w}, k$. We estimate the parameters $\delta, \overline{w}$, and $k$ in auxiliary models, and estimate jointly the latent variables $b, c, q$, as well as the underlying policy function $y = \pi(s_t, \theta)$. Additionally, we

allow all observed or pre-estimated parameters to vary at the individual level; i.e. we have $\theta_i = (\delta_i, p_i, q_{0i}, w_i, \overline{w}_i, k_i)$ for individual $i$, and allow unobserved parameters $\theta_j = (b_j, c_j, q_j)$ to vary at the level of municipality $j$.

Additionally, since the model is fully deterministic conditional on $\theta$ and the state, we introduce a source of random variation in the private sector wage by assuming that at each time period, the agent confronts a random offer from the private sector: $\overline{w}_t = \overline{w} + \epsilon_t$, with $\epsilon_t$ a random shock with mean 0 that is observed by the agent, but not by the econometrician. Specifically, we assume that $\epsilon_t$ follows a logistic distribution. With these assumptions, we can structurally estimate the model. In what follows, we first describe how we calibrate the model, then the auxiliary models we used for individual-level parameters $\theta_i$.

### 5.1.1 Approach for calibration

Recall that in the model examined in section 4, payoffs were bounded and stationary, transition probabilities were also stationary, and the state space $\mathcal{S}$ and action space $\mathcal{A}$ were finite. As such, a stationary policy $\pi : \mathcal{S} \to \mathcal{A}$ is optimal. Furthermore the action and state spaces are small. As such, given parameters $\delta, p, q_0, q, b, c, w, \overline{w}, k$, one can easily derive the value function $v_\pi(s_0) = (1-\delta)\left[u(\pi(s_0), s_0) + \mathbb{E}_\pi\left[\sum_{t=1}^{\infty} \delta^t u(\pi(s_t), s_t)\right]\right]$ for all potential policies by solving, for each policy, the system implied by its Bellman's equation. The optimal policy is, among all stationary policies $\Pi$ the policy $\pi^* \in \Pi$ with the highest value $v_{\pi^*}$.

Adding random shocks to $\overline{w}$ introduces complexity, because states now depend on $\epsilon$. In other words, a sufficiently bad offer from the private sector may push agents to stay in the bureaucracy one more period even though the optimal policy was to leave. Conversely, a sufficiently good offer from the private sector may push agents leave the bureaucracy permanently one more period even though the optimal policy was to stay. However, since $\epsilon$ is i.i.d. with mean 0, its impact on future periods cancels out in expectation. Decisions in period 0 only consider $\epsilon_0$, and one can view the model without random shocks on wages as a special case where $\epsilon_0 = 0$. As such, if policy $\pi^*$ is optimal when $\epsilon_0 = 0$, with action $\pi^*(s, \epsilon = 0) = a$, for $\epsilon_0 \neq 0$, the agent compares the value of action $a$ to the value of an alternative.

If $a \in \{0, 1\}$, the alternative is $a' = 2$, with value $(1-\delta)\left[(\overline{w} + \epsilon_0) + \mathbb{E}\left[\sum_{t=1} \delta^t(\overline{w} + \epsilon_t)\right]\right] = \overline{w} + (1-\delta)\epsilon_0$. If $a = 2$, the alternative $a' \in \{0, 1\}$ is the one that generates the highest value from a one-shot deviation from $a = 2$. As such, the optimal policy $\pi^*$ specifies an optimal action $y_{\pi^*}(s) \in \{0, 1\}$ with value $\omega(y_{\pi^*}(s), s)$ such that $y_{\pi^*}(s) = \pi(s, 0)$ and $\omega(y_{\pi^*}(s), s) = v_{\pi^*}(s)$ when $\pi(s, 0) \in \{0, 1\}$. When $\pi(s, 0) = 2$, $y_{\pi^*}(s) \in \{0, 1\}$ maximizes $\omega(y_{\pi^*}(s), s)$, the value of a one-shot deviation from $\pi^*$ to action $y$ in state $s$. Importantly, the decision taken in period $t$ satisfies

$$y_{it}(s) = \begin{cases} y_{\pi^*}(s), \text{ if } \omega_{\pi^*}(y_{\pi^*}(s), s) > \overline{w} + (1-\delta)\epsilon_t \\ 2 \text{ otherwise} \end{cases} \tag{5}$$

The optimal policy varies in function of parameters $\theta$. Some policies however are suboptimal for any value of $\theta$. Candidate policies for the bureaucrat to engage in are:

1. Never steal ($\pi(N,0) = \pi(A,0) = 0$)
2. Always steal ($\pi(N,0) = \pi(A,0) = 1$)
3. Steal when no audit ($\pi(N,0) = 1, \pi(A,0) = 0$)
4. Steal then quit ($\pi(N,0) = 1, \pi(A,0) = 2$), deviate to stealing ($y_\pi(A) = 1$) for $\epsilon_t$ sufficiently low in state $A$
5. Steal then quit ($\pi(N,0) = 1, \pi(A,0) = 2$), with $y_\pi(A) = 0$
6. Quit preemptively ($\pi(N,0) = \pi(A,0) = 2$), with $y_\pi(A) = y_\pi(N) = 1$
7. Quit preemptively ($\pi(N,0) = \pi(A,0) = 2$), with $y_\pi(A) = y_\pi(N) = 0$
8. Quit preemptively ($\pi(N,0) = \pi(A,0) = 2$), with $y_\pi(A) = 0, y_\pi(N) = 1$

Since $\epsilon_t$ follows a logistic distribution, and with $\text{logit}(x) = \frac{\exp x}{1+\exp x}$ the logistic function, it follows from equation 5 that the probability of observing action $y_{it} = 2$ in state $s$, with parameters $\theta$ and conditional on the agent not getting dismissed (i.e. $y_{it} \neq 3$) and parameters $\theta$ writes:

$$\Pr(y_{it} = 2 | y_{it} \neq 3, s_t, \theta) = \text{logit}\left(\frac{\overline{w} - \omega_{\pi^*(\theta)}(y_{\pi^*(\theta)}(s_t), s_t)}{1-\delta}\right)$$

Additionally, the probability of the agent getting dismissed writes:

$$\Pr(y_{it} = 3 | s_t, s_{t-1}, \theta) = q_0 + (1-q_0)1\{y_{\pi^*(\theta)}(s_{t-1}) = 1\}1\{s_t = A\}q$$

For municipality $j$, we can consider the tally of departures and dismissal as the total number of successes generated from a binomial distribution. As such, the likelihood contribution of municipality $j$ with individuals indexed by $i$ observed for $t_i$ periods writes:

$$\mathcal{L}_j(\theta_j) = \prod_i \prod_{t=0}^{t_i} \Pr(y_{ijt} = 3 | s_{jt}, s_{jt-1}, \theta_j)^{1\{y_{ijt}=3\}} \Big[ [1 - \Pr(y_{ijt} = 3 | s_{jt}, s_{jt-1}, \theta_j)]$$
$$[1 - \Pr(y_{ijt} = 2 | y_{ijt} \neq 3, s_{jt}, \theta_j)]^{1\{y_{ijt}\neq 2\}} \Pr(y_{ijt} = 2 | y_{ijt} \neq 3, s_{jt}, \theta_j)^{1\{y_{ijt}=2\}} \Big]^{1\{y_{ijt}\neq 3\}}$$

We estimate parameters $\theta_j$ by maximizing the log-likelihood function $\log \mathcal{L}_j(\theta_j)$. Identification chiefly comes from comparing levels of departures and dismissals in states $A$ and $N$, which allow us to learn about $b, c$, and $q_0$. If levels of departures are low and constant across states, then $b$ must be high and $c$ must be low. If, on top of this, levels of dismissals are comparable in states $A$ and $N$, then $q$ must be low. Note that for estimation, we normalize all parameters by the mean sample wage. In other words, we rescale all variables so that mean $w = 1$. Additionally, we now express $c$ as a percentage rather than as an absolute amount. In other words, during an audit, benefits from corruption shrink down to $(1 - c)b$.

In the remainder of this section, we detail how we estimate the observed, individual-level parameters $\theta_i$.

### 5.1.2 Estimation of individual-level parameters

Recall that the vector of individual-level parameters is $\theta_i = (\delta_i, p_i, q_{0i}, w_i, \overline{w}_i, k_i)$. Estimating parameter $p$ is straightforward: since $p$ is the probability of an audit in a given year, we simply use the probability of an audit in each state as per the lottery procedure. Other parameters are more challenging, largely because these parameters are time-invariant, while our data structure is time-varying. As such, we turn parameters that are essentially time-varying into time-invariant parameters by predicting their value for individual $i$ with characteristics $x_i$ over its lifecycle; that is, from the first year this person is observed in the dataset until mandatory retirement age. Vector $x_i$ includes time-invariant characteristics such as gender, education, the municipal-level controls used in the reduced-form models (section 3), state-level fixed effects, as well as deterministic time-varying variables such as age and work experience.

We estimate the discount factor $\delta$ by predicting the probability of individual $i$ retiring from the labor market using a logistic regression. We estimate the baseline probability of getting dismissed $q_0$ by predicting the probability of individual $i$ being dismissed in municipality-years such that $s_{jt} = N$. We estimate public and private-sector wages $w_i$ and $\overline{w}_i$ using the Blinder-Oaxaca procedure (Blinder, 1973); i.e. we regress the (log) wages of public sector employees over predictors $x_i$, and estimate a second model for private sector employees.[16] Parameter $w_i$ is therefore the average salary of employee $i$ if she stayed in the public sector from the first year we observe her until retirement, while $\overline{w}_i$ is the average salary of employee $i$ should she depart to the private sector on the first year we observe her until retirement. Finally, we estimate the dismissal penalty $k$ by comparing, for an individual with characteristics $x_i$, the private sector wage $\overline{w}_1$ that follows a voluntary departure to the private sector wage $\overline{w}_2$ that follows a dismissal, with $k_i = \frac{1}{t_i} \sum_{t=1}^{t_i} \widehat{\overline{w}_2(i,t)} - \widehat{\overline{w}_1(i,t)}$.

[16] Predictors include municipal employment and wage, education, age, gender, work experience.

## 5.2 Estimates, validation and counterfactual experiments

The model yields four distributions of interest. For each municipality, we estimate the distributions of $b$, the size of the benefits from corruption; $c$, the percent decrease in $b$ that follows audits; and $q$, the probability of getting dismissed for corruption after an audit. Figure 8 shows the distribution of these parameters. The rent from corruption $b$ ranges from 0 to 10 times mean monthly wage, with a median at 3.5 times mean monthly wage. Examining parameters $c$ and $q$ shows that most audits do not have significant bite. The distribution of $c$ is bimodal but tilted towards little improvements, with $c > 50\%$ in about 30% municipalities. Similarly, most municipalities only experience a small increase in the probability of being dismissed after an audit.

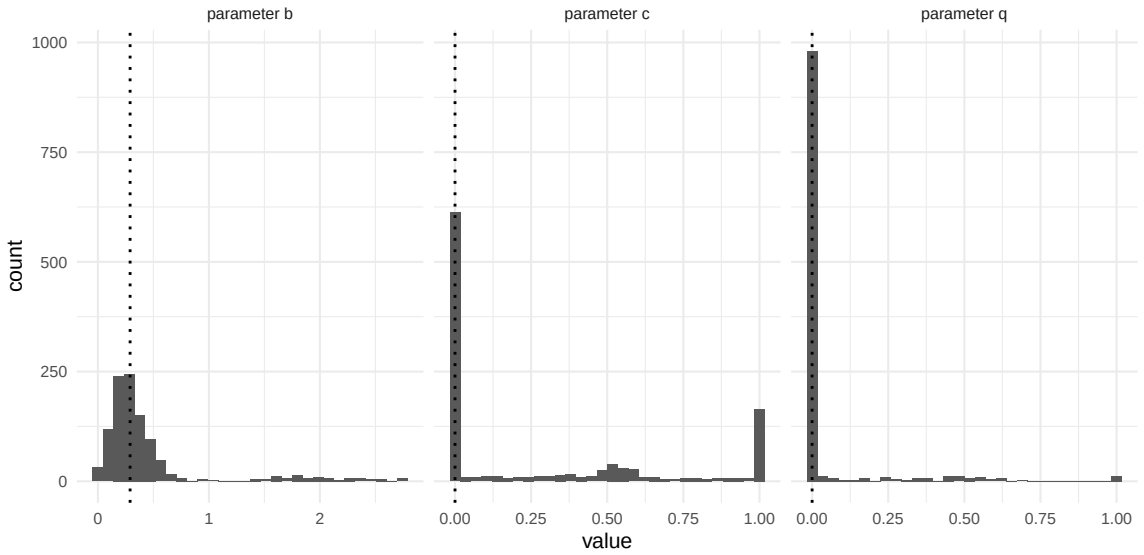


Figure 8: **Distribution of estimated parameters.** The rent from corruption $b$ ranges from 0 to 10. Audits have little bite: $q$ is minuscule for an overwhelming majority of municipalities, and $c$ is less than 50% for about 70% municipalities.

Figure 9 examines the distribution of optimal policies across agents. Given that audits have little bite, and that benefits are substantial, it is unsurprising that most bureaucrats engage in corruption irrespective of whether the bureaucracy is audited or not.

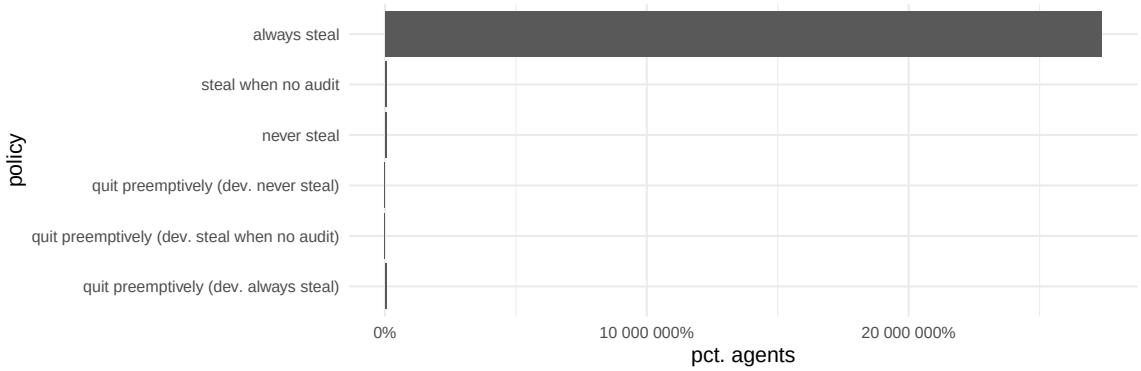


Figure 9: **Distribution of optimal policies.** Always stealing is overwhelmingly the preferred policy.

We validate these estimates by checking that they match patterns that we have previously established in the data. We show in Appendix D that the parameter $b$ correlates with the levels of corruption measured by audits: municipalities with a higher $b$'s are indeed more corrupt according to the different corruption metrics outlined in 3 (Figure 30). We further show that the channel we identified in the reduced-form model – namely, that audits lead to improvements in management in highly corrupt municipalities – is also partially borne out by those estimates. We show some support for the that in municipalities classified as highly corrupt as per audits, a significantly larger share of employees refrain from stealing during audits (i.e. use policy "steal when no audit").

We finally evaluate the impact of each of our main parameters on the careers of our sample by estimating a series of counterfactual scenarios. We first consider a "baseline"; that is, predictions under the estimated parameter values to observed data, to check whether this baseline matches reality. We then compare the baseline against a benchmark, "no audits" scenario, which sets both $c$ and $q$ to zero. This allows evaluating how much corruption has been deterred by the program. We then estimate the maximum potential of the audits program by increasing both $c$ and $q$ to their 95th percentile value for all those municipalities such that $c$ or $q$ is below this value. This "$c + q$" scenario estimates how much corruption would have deterred had all municipalities performed as the best in class.

We also consider both mechanisms in isolation by setting each parameter separately (i.e., a "$c$" and a "$q$" scenario). Finally, we consider a scenario that decreases the size of the rent $b$. We interpret $b$ as an indicator of the broader institutional environment in which bureaucrats evolves, that in fine impacts the size of potential gains from corruption. We consider $b$ relative to the municipality's mean public sector wage $\tilde{w}$, and set to the 5th percentile in the distribution of $b_j/\tilde{w}_j$ all municipalities above the 5th percentile. All scenarios consider each sample bureaucrat on the year they enter the sample and estimate their optimal policy given parameter values. This allows us to derive their probability of departure, and expected amount stolen over the period. Aggregating to the sample

level allows deriving the predicted size of the bureaucracy, cumulative amount stolen, and policy distribution with respect to expected behavior.

Examining the size of the bureaucracy (Figure 10) we see that our baseline overestimates real data, mostly because it does not accurately predict the large waves of departures and dismissals occurring in years 2008 and 2012. It performs remarkably well in other years. Comparing the baseline to the benchmark in which we remove audits, we also observe that it has little impact on either corruption or careers. Setting to one the amount total amount stolen under the benchmark, the program itself only reduced corruption by 4 percentage points. Increasing the bite of these audits increases their effectiveness, although increasing $q$ is more effective than increasing $c$, because $q$ reduces personnel size. Reducing $b$, however, is by far the most effective way to curb corruption, suggesting that an audits program is not sufficient to curb differences in levels of corruption.[17]

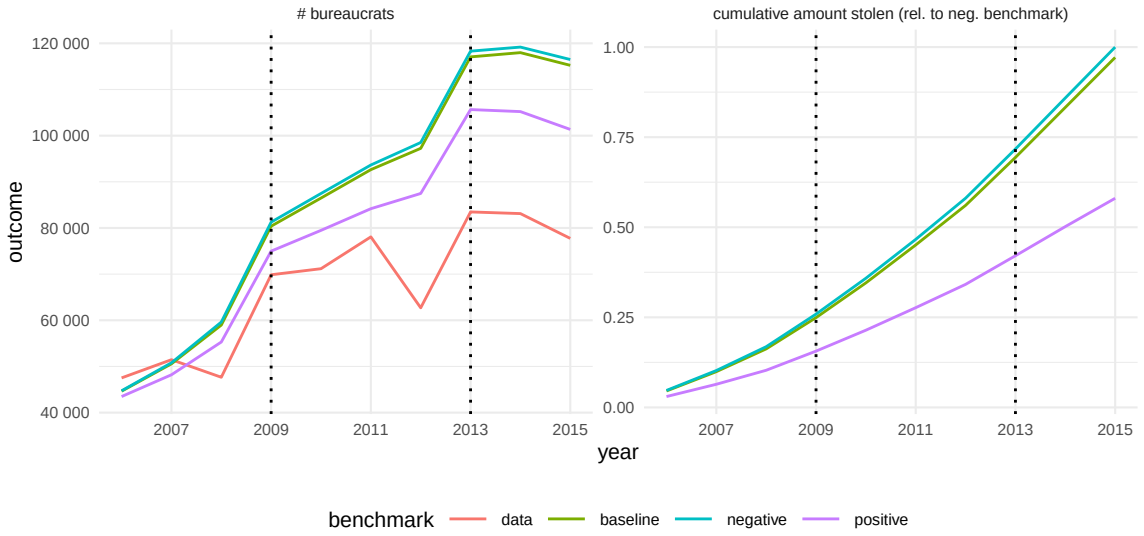


Figure 10: **Impact of counterfactual experiments on bureaucracy size and corruption.** The baseline matches the data relatively well except during election years (2008, 2012). Decreasing $b$ has a drastic impact on both outcomes.

Examining the distribution of optimal policies (Figure 11) allows for better understanding of the mechanisms underlying movements in the workforce. While very effective at curbing corruption, decreasing $b$ does not significantly alter the distribution of bureaucratic responses to audits. As such, the sharp decreases in the size of the bureaucracy observed in this scenario are likely because decreases in $b$ make public sector positions less attractive, hence triggering massive waves of departures. Conversely, scenarios $c$ and $q$ shift about 20% of staff towards stealing when no audit. While this strategy explains

[17] Other measures, such as increasing the probability of an audit $p$, could perhaps increase their effectiveness.

why the policy causes some reduction in corruption, it also suggests that the moderate decreases in the size of the bureaucracy observed under policy $q$ are due to increased dismissals.

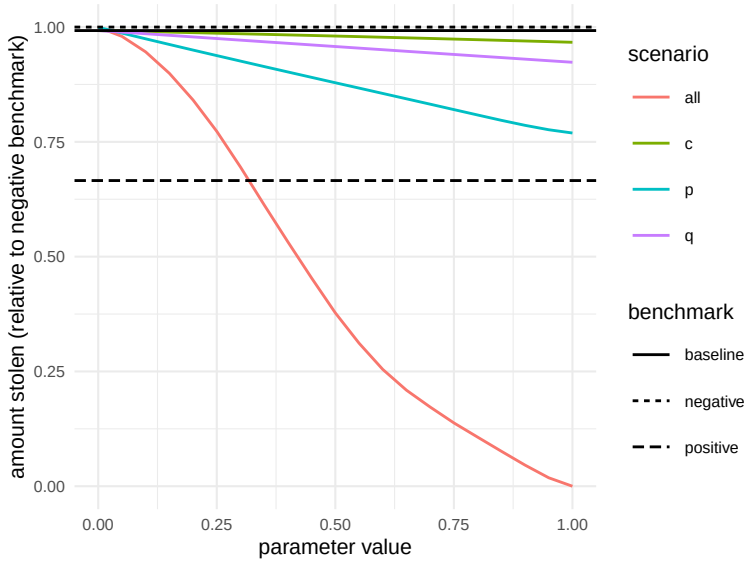


Figure 11: **Impact of counterfactual experiments on the distribution of optimal policies.** Scenarios $c$ and $q$ provoke a 20% shift towards "steal when no audit."

Overall, our results paint a nuanced picture of the effect of audits programs on bureaucratic careers. Although both structural and reduced-form results show that audits trigger some reductions in corruption, perhaps through short-term improvements in managerial practices, these improvements fail to leave a dent on corrupt behavior. Moreover, strengthening the bite of audits – either through threats of dismissal or temporary improvements in management – has only a limited impact, as bureaucrats never, in fact, stop stealing. Ultimately, there seems to be no more effective policy at reducing corruption than to induce changes in the local institutional environment that curb private gains from stealing.

# 6 Conclusion

Corruption is endemic across the developing world. Policies designed to reduce it have to carefully consider how they will affect the behavior of different types of public officials. Politicians have short-term electoral mandates, making their interactions with governmental institutions – and opportunity to engage in corruption – short-lived. In contrast, bureaucrats weigh exit options against an extended career time-horizon, leading to short and long-term effects of audits. To parse out these effects, we deploy a theoretical model that allows us to estimate how bureaucrats' career choices respond to an audits program.

We show that in Brazil, an audits program designed to curb corruption ultimately has

only limited effects on the careers of high-level municipal bureaucrats. The only short-term effect we detect – a temporary improvement in management for the most corrupt municipalities – fails to curb widespread corruption. In our structural estimation, we find that bureaucrats continue to steal, regardless of being audited. Even strengthening the policy's bite fails to reduce bureaucratic misbehavior. Ultimately, anti-corruption audits are no panacea: even the best case scenario will not eliminate corruption. Significant improvements can only occur through long-term reductions in opportunities for corruption, which requires a far more costly and extended effort.

Our paper demonstrates the value of theory in assessing policy effects. On the one hand, without theory we are unable to contextualize the findings with respect to policy benchmarks, allowing us to construct best case scenarios and removing the program altogether. On the other hand, we may miss potentially important long-term effects that may result from the policy as we restrict our analysis to ex post estimations. This paper provides a compelling case for complementing causal identification with theory to guide us through an assessment of the effectiveness of anti-corruption programs.

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# Appendices



## A Proofs

In this section, we denote by $v_\pi(s) = \mathbb{E}_\pi\left[(1-\delta)\sum_{t=0}^{\infty}\delta^t u(\pi(s_t), s_t)|s_0 = s\right]$ the continuation value implied by a strategy in state $s$. We denote by $\pi_0, \pi_1, \pi_2$ the policies that consist in playing 0, 1, 2 respectively in both states $A$ and $N$. Note that $v_{\pi_0} = w$ and $v_{\pi_2} = \overline{w}$. For concision, we also define $\overline{p} \equiv pq$. Since the action and state spaces are both small, we check for optimality by comparing the value functions of all possible policies.

*Proof of proposition 1.* Note that

$$v_{\pi^*}(N) = v_{\pi^*}(A) = (1-\delta)(w+b) + \delta[(1-\overline{p})v_{\pi^*}(N) + \overline{pw}]$$

Solving for $v_{\pi^*}(N)$, we get

$$v_{\pi^*}(N) = \frac{(1-\delta)(w+b) + \delta\overline{pw}}{1-(1-\overline{p})\delta}$$

Note that $v_{\pi^*}(N) > \overline{w} \iff w + b > \overline{w}$, which is true by assumption. So $\pi^*$ is always preferred to $\pi_2$. Also note that $v_{\pi^*}(N) > w \iff b(1-\delta) + \overline{p}(\overline{w} - w)\delta > 0$. As

such, if $\overline{w} > w$, then $\pi^*$ is always preferred to $\pi_0$. If $\overline{w} < w$, $\pi^*$ is preferred to $\pi_0$ iff $b \geq \frac{\overline{p}(w-\overline{w})\delta}{1-\delta} \equiv b_0 > 0$. □

*Proof of proposition 2.* Note that

$$v_{\pi_1}(N) = v_{\pi_1}(A) = (1-\delta)(w+b) + \delta[(1-\overline{p})v_{\pi_1}(N) + \overline{p}(\overline{w} - (1-\delta)k)]$$

Solving for $v_{\pi_1}(N)$, we get

$$v_{\pi_1}(N) = \frac{(1-\delta)(w+b-\delta\overline{p}k) + \delta\overline{pw}}{1-\delta(1-\overline{p})}$$

If $w < \overline{w}$ (i.e. if equation 3 holds), then $\pi_2$ is preferred to $\pi_0$. We have that

$$v_{\pi_1}(N) \geq v_{\pi_2}(N) \iff k \leq \frac{w+b-\overline{w}}{\overline{p}\delta} \equiv k_0$$

Yet, $k_0 \leq \overline{w} \iff \overline{w} \geq \frac{w+b}{1+\delta\overline{p}} > w + b$, which is impossible. As such $v_{\pi_1}(N) \geq v_{\pi_2}(N)$ for any $k \in [0, \overline{w}]$, so $\pi_1$ is optimal.

Suppose now that $\overline{w} < w$ (i.e. suppose that equation 4 holds). In this case, $\pi_0$ is preferred to $\pi_2$. We have that

$$v_{\pi_1}(N) \geq v_{\pi_0}(N) \iff k \leq \frac{(1-\delta)b - \overline{p}\delta(w-\overline{w})}{(1-\delta)\overline{p}\delta} \equiv k_0$$

We have that $k_0 \geq 0 \iff b \geq \frac{\overline{p}\delta(w-\overline{w})}{1-\delta} \equiv b_0$. We have that $k_0 \leq \overline{w} \iff b \leq \frac{\overline{p}\delta(w-\overline{w}\delta)}{1-\delta} \equiv b_1 > b_0$. □

*Proof of proposition 3.* Note first that if a policy $\pi^*$ such that $\pi^*(A) = 1$ is optimal, then it must be that $\pi^* = \pi_1$. Indeed, the continuation values of policy $\pi_1$ satisfy the following:

$$\begin{aligned} v_{\pi_1}(N) =& (1-\delta)(w+b) + \delta[(1-p)v_{\pi_1}(N) + p(1-q)v_{\pi_1}(A) + \overline{pw}] \\ v_{\pi_1}(A) =& (1-\delta)(w+b-c) + \delta[(1-p)v_{\pi_1}(N) + p(1-q)v_{\pi_1}(A) + \overline{pw}] \end{aligned}$$

Since $c > 0$, we have that $v_{\pi^*}(A) = v_{\pi_1}(A) < v_{\pi_1}(N)$. Furthermore, since $\pi^*(A) = 1$ is optimal, then it must be that $v_{\pi^*}(A) \geq \overline{w}$. As such, it cannot be that $\pi^*(N) = 2$, because this would imply that $v_{\pi^*}(N) = \overline{w} < v_{\pi_1}(N)$. Suppose now that $\pi^*(N) = 0$. Then $v_{\pi^*}(N) = (1-\delta)w + \delta[(1-p)v_{\pi_1}(N) + pv_{\pi_1}(A)]$. Yet, if $\pi^*(A) = 1$ is optimal, then such policy is preferred to a policy $\pi'$ such that $\pi'(A) = 0$ and $\pi'(N) = 1$. As such, we have $v_(\pi^*)(A) \geq v_{\pi'}(A) = v_{\pi^*}(N)$. Yet, we have $v_{\pi_1}(N) > v_{\pi^*}(A) \geq v_{pi^*}(N)$, a contradiction.

Note furthermore that since $w > \overline{w}$ or $\overline{w} > w$, a policy such that $\pi(N), \pi(A) \in \{0, 2\}$ and $\pi(N) \neq \pi(A)$ is suboptimal, as either $v_{\pi_0}(s) > v_\pi(s)$ or $v_{\pi_2}(s) > v_\pi(s)$ for $s \in \{A, N\}$.

As such, the remaining policies are $\pi_0$, $\pi_1$, $\pi_2$, and policies $\pi_{10}$ and $\pi_{12}$ which correspond to $\pi(N) = 1$, and $\pi(A) = 0, 2$ respectively.

Solving for $v_{\pi_1}(N), v_{\pi_1}(A)$, we get

$$\begin{aligned} v_{\pi_1}(N) =& \frac{(1-\delta)[b + w - \delta p(1-q)c] + \delta\overline{pw}}{1 - \delta(1-\overline{p})} \\ v_{\pi_1}(A) =& \frac{(1-\delta)[b + w - (1-\delta(1-p))c] + \delta\overline{pw}}{1 - \delta(1-\overline{p})} \end{aligned}$$

Continuation values for $\pi_{10}$ satisfy

$$\begin{aligned} v_{\pi_{10}}(N) =& (1-\delta)(w+b) + \delta[(1-p)v_{\pi_{10}}(N) + p(1-q)v_{\pi_{10}}(A) + \overline{pw}] \\ v_{\pi_{10}}(A) =& (1-\delta)w + \delta[(1-p)v_{\pi_{10}}(N) + pv_{\pi_{10}}(A)] \end{aligned}$$

Solving for $v_{\pi_{10}}(N), v_{\pi_{10}}(A)$, we get

$$\begin{aligned} v_{\pi_{10}}(N) =& \frac{(1-\delta)[(1-p\delta)b + (1-\overline{p}\delta)w] + \overline{p}\delta(1-p\delta)\overline{w}}{1 - \delta[1-(1-p)\overline{p}\delta]} \\ v_{\pi_{10}}(A) =& \frac{(1-\delta)[w + (1-p)\delta b] + \delta^2\overline{p}(1-p)\overline{w}}{1 - \delta[1-(1-p)\overline{p}\delta]} \end{aligned}$$

Finally, continuation values for $\pi_{12}$ satisfy

$$\begin{aligned} v_{\pi_{12}}(N) =& (1-\delta)(w+b) + \delta[(1-p)v_{\pi_{12}}(N) + p\overline{w}] \\ v_{\pi_{12}}(A) =& \overline{w} \end{aligned}$$

Solving for $v_{\pi_{12}}(N)$, we get

$$v_{\pi_{12}}(N) = \frac{(w+b)(1-\delta) + p\delta\overline{w}}{1-(1-p)\delta}$$

Suppose first that $w < \overline{w}$ (case 3). Note that in this case, $v_{\pi_2}(s) = \overline{w} > v_{\pi_1}(s) = w$ for any $s \in \{A, N\}$. Also note that since $w + b > \overline{w}$, we have that $v_{12}(N) > v_{\pi_2}(N)$. As such, three policies remain: $\pi_{12}, \pi_{10}$, and $\pi_1$.

We have $v_{\pi_1}(N) \geq v_{\pi_{12}}(N) \iff c \leq \frac{w+b-\overline{w}}{1-(1-p)\delta} \equiv c_1$, and $c_1 \leq b \iff b \leq \frac{\overline{w}-w}{\delta(1-p)} \equiv b_0$.

We have $v_{\pi_1}(N) \geq v_{\pi_{10}}(N) \iff c \leq \frac{(1-\delta)b+\overline{p}\delta(\overline{w}-w)}{1-\delta[1-(1-p)\overline{p}\delta]} \equiv c_2$, and $c_2 \leq b \iff b \geq b_0$.

So $v_{\pi_1}(N) = \max_\pi v(N) \iff c \leq \overline{c} \equiv \max\{c_1, c_2\}$. Furthermore, $v_{\pi_{12}}(N) = \max_\pi v(N) \iff c \leq \overline{c}$ and $b \leq b_0$. Conversely, $v_{\pi_{10}}(N) = \max_\pi v(N) \iff c \leq \overline{c}$ and $b \geq b_0$. We show similarly that this holds true for state $A$.

Suppose now that $w > \overline{w}$ (case 4). Note that in this case, $v_{\pi_2}(s) = \overline{w} < v_{\pi_1}(s) = w$ for any $s \in \{A, N\}$. Also note that since $w > \overline{w}$, we have that $v_{\pi_{10}}(s) > v_{\pi_{12}(s)}$ for any $s \in \{A, N\}$. As such, three policies remain: $\pi_0, \pi_{10}, \pi_1$.

We have $v_{\pi_1}(N) \geq v_{\pi_{10}}(N) \iff w \leq \frac{b(1-\delta)+\overline{p}\delta\overline{w}-c[1-\delta(1-(1-p)\overline{p}\delta)]}{\overline{p}\delta} \equiv w_2$. Also note that

$w_2 \geq \overline{w} \iff b \geq c\frac{1-\delta[1-(1-p)\overline{p}\delta]}{1-\delta} \equiv b_2$.

Furthermore, we have $v_{\pi_{10}}(N) \geq v_{\pi_0}(N) \iff w \leq \frac{b(1-\delta)+\overline{p}\delta\overline{w}}{\overline{p}\delta} \equiv w_1$. Also note that $w_1 \geq \overline{w} \iff b \geq 0$, and that $w_1 \geq w_2$.

□

# B Additional descriptive statistics

## B.1 Measuring corruption

### Sampling from audit reports

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.

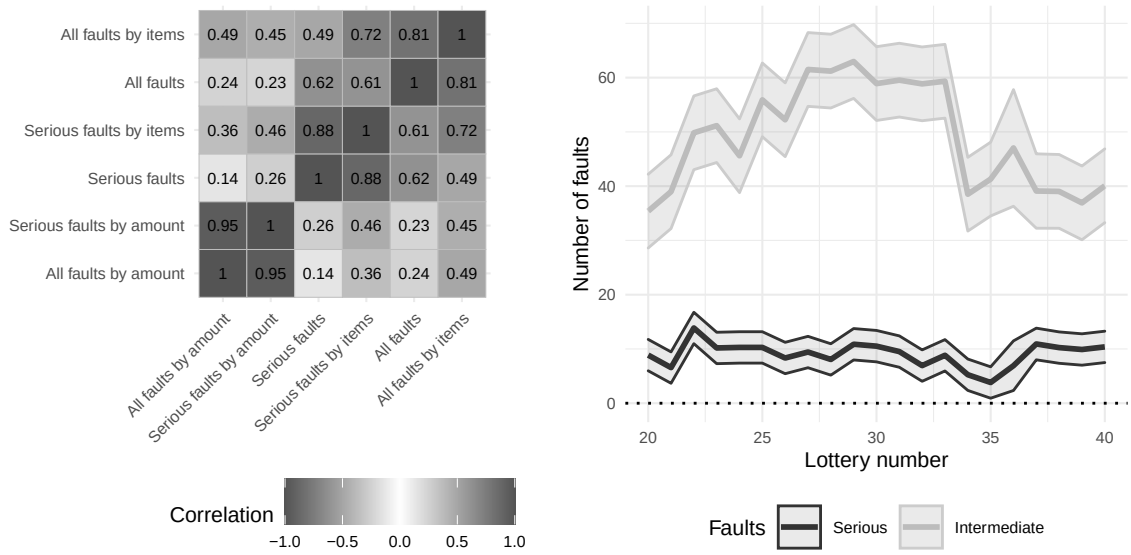
In the category *falhas medias*, we have minor infractions that are not necessarily evidence of corruption, but procedural deficiencies. For instance, we have some municipalities that fail to respect a regular meeting of the health board, or that fail to provide enough books in school. We do not believe 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.

- **Falha média:** 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.

### Constructing measures of corruption

In this section, we report the correlation among all measures of corruption, and show evidence for a time trend.




(a) Correlation among corruption metrics (b) Mean number of faults over time

Figure 12: **Constructing indicators of corruption.** Left: While many corruption metrics are highly correlated, least correlation ($< .5$) is found between the metrics that are normalized by amount audited and the other metrics. Right: Audits become more stringent from lottery 20 to 27, then less stringent from lottery 32 onwards, as evidenced by the mean number of intermediate faults picked up by an audit (shaded areas are 95 percent heteroskedastic-robust confidence intervals).

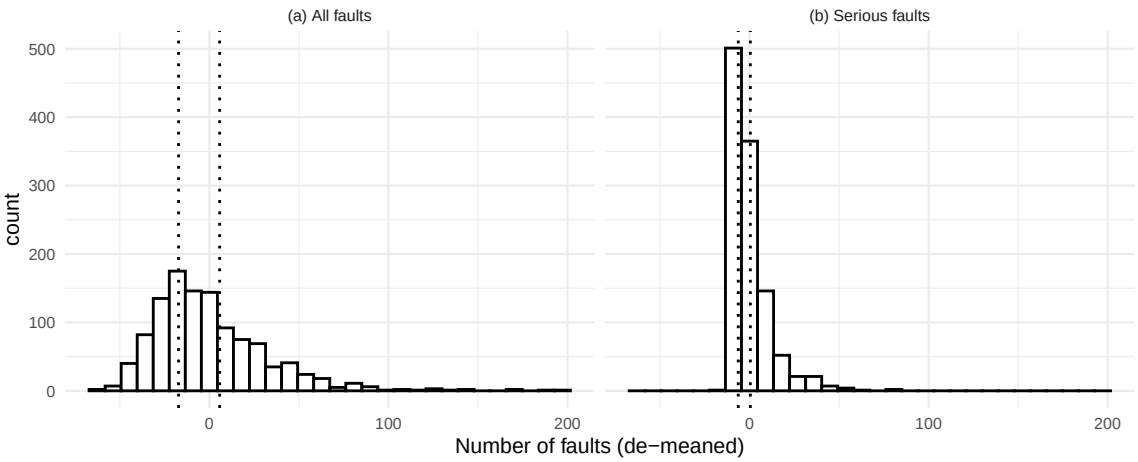



Figure 13: **Distribution of number of irregularities.** Vertical bars indicate the first and second tercile of each distribution. Most municipalities show little corruption.

**Validation: Replication of Avis, Ferraz and Finan (2018)**

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | All | | Serious | | All per Amount | | Serious per Amount | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| treat | −0.035* | −0.041** | −0.0003 | −0.017 | −0.150*** | −0.055 | −0.093* | −0.059 |
| | (0.019) | (0.019) | (0.057) | (0.057) | (0.056) | (0.036) | (0.049) | (0.046) |
| Controls | _ | ✓ | _ | ✓ | _ | ✓ | _ | ✓ |
| Avis, Ferraz and Finan (2018) | ✓ | ✓ | _ | _ | _ | _ | _ | _ |
| Observations | 1,095 | 1,095 | 1,095 | 1,095 | 1,095 | 1,095 | 1,095 | 1,095 |
| $R^2$ | 0.792 | 0.796 | 0.509 | 0.516 | 0.386 | 0.742 | 0.203 | 0.303 |
| *Note:* | | | | | | | *p<0.1; **p<0.05; ***p<0.01 | |

Table 3: **Replication of Avis, Ferraz and Finan (2018).** All models include lottery and state fixed effects and use robust standard errors. Models (1) and (2) replicate the specification in Avis, Ferraz and Finan (2018) on an extended time period. Models 1-4 control for the log number of audited items. Models 4-8 do not. Results are largely robust to alternative specifications of the dependent variable.

## B.2 Management index

### Details about construction

In this section, we outline in depth how the management index is constructed. Information is gathered from the *Pesquisa de Informações Básicas Municipais* (Munic), an annual census conducted by the Institute of Brazilian Geography and Statistics (IBGE). The questionnaire is self-reported by municipalities, gathering information on a set of administrative practices, indicating the presence or not of a certain institutional feature or practice.

To construct the index $m_{jt}$ for municipality $j$ at year $t$, we use a similar approach to Bloom and Van Reenen (2007) deploy to compare management practices among firms. We construct three dimensions of "good" management that can potentially reduce corruption. Table 4 provides a random sample of 5 practices per dimension to illustrate the types of management practices used to calculate the management index.

| Year | Practice |
|---|---|
| **Accountability** | |
| 2005 | Culture Council |
| 2009 | Urban Policy Council |
| 2012 | Council for Physical Disability Rights |
| 2013 | Health Council |
| 2013 | Environmental Council |
| **Accounting** | |
| 2004 | Property Registration |
| 2009 | Digital Property Registration |
| 2011 | Families in Housing Programs Registration |
| 2013 | Housings Programs Registration |
| 2013 | Population at Risk Registration |
| **Planning** | |
| 2008 | Transportation Planning |
| 2009 | City Planning |
| 2012 | Transportation Planning |
| 2012 | Food Safety and Nutrition Planning |
| 2014 | Food Safety and Nutrition Planning |

Table 4: Random sample of administrative practices, broken down by each respective dimension. Note that the questions can vary according to the year in which the questionnaire is administered.

Within each of these dimensions, we count the number of practices $n_{jt}$ in municipality $j$ at time $t$. Note that the index is time variant: the number of practices vary from year to year, due to modifications in the structure of the questionnaire. We then take an arithmetic mean across the three $k$ dimensions.

$$m_{jt} = \frac{1}{3} \sum_{k=1}^{3} \frac{n_{kjt}}{n_{kt}}$$

## Validation

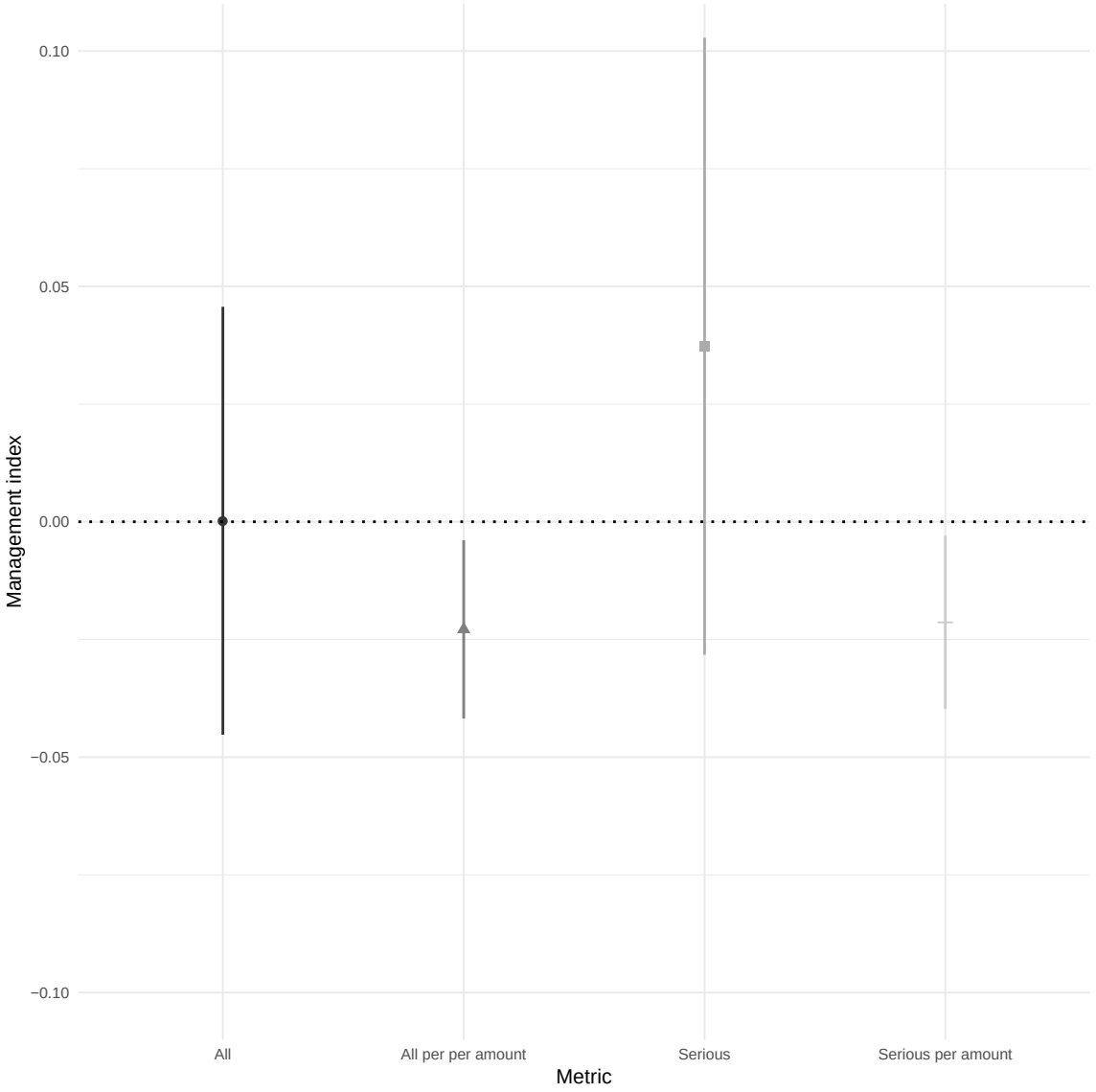



Figure 14: **Correlation management index - corruption.** We find partial support for the hypothesis that more corrupt municipalities have poorer management. This figure reports the coefficient associated with regressing the management index in municipality $j$ as measured by the Munic survey conducted in period $t$ on corruption as measured by audit conducted in period $t + 1$. State and year fixed effects, controlling for Gini coefficient, illiteracy rate, population size and total number of items audited. Standard errors are clustered at the municipal level.

## B.3 Additional descriptive statistics

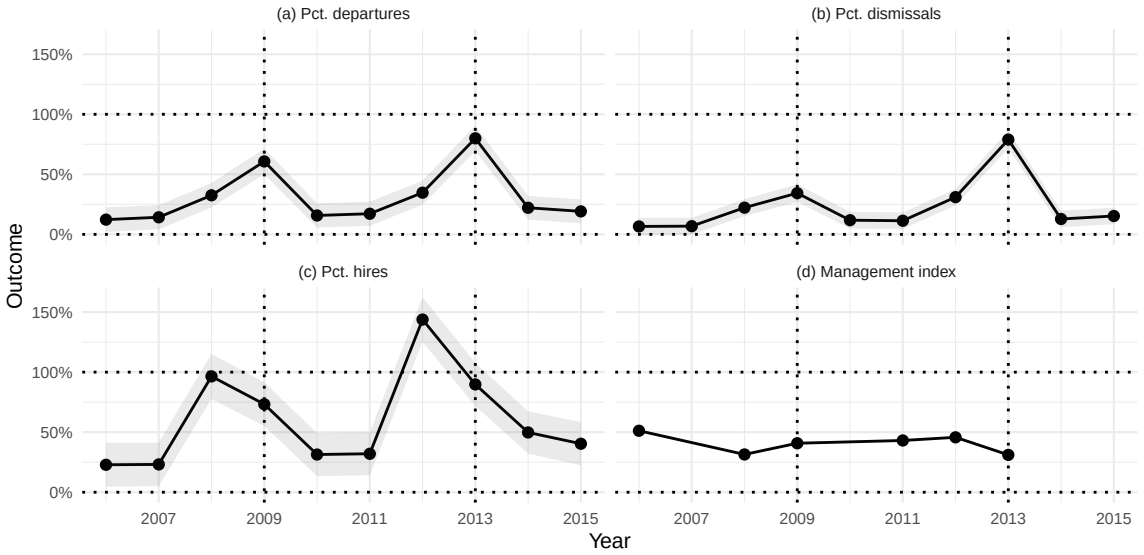



Figure 15: **Dependent variables over time.** Vertical bars denote election years. Shaded areas are heteroskedastic-robust 95 percent confidence intervals. There is seasonality in staff rotation around election years (panels a, b, c). On average, management practices remain constant over time.

# C Robustness checks

## C.1 Balance tests

We verify audits' randomization procedure by comparing the set of municipalities that were audited early to those that were audited late, defining early and late as, respectively, before and after the median audit.

| Variable | Early audit | Late audit | Diff in means (p-value) |
|---|---|---|---|
| No. of bureaucrats (2006) | 97.214 | 64.879 | 32.335 (0.118) |
| Municipal population (logged) | 9.435 | 9.312 | 0.123 (0.057)* |
| Gini coefficient | 0.226 | 0.221 | 0.005 (0.469) |
| Illiteracy rate | 0.558 | 0.561 | -0.004 (0.385) |
| Median municipal wage | 190.73 | 189.245 | 1.484 (0.816) |
| Urbanization | 0.579 | 0.58 | -0.002 (0.907) |
| Sample size | 5759 | 4397 | |

Table 5: **Covariate balance tests.** We check whether there are differences in the sample of municipalities audited early in the program (2006-9) with the later half in our sample (2009-2015). We regress each of our control variables against a dummy indicating whether the municipality was audited early, reporting the difference in means which corresponds to that coefficient. Standard errors are clustered at the municipal level. We find that none of the differences are statistically significant except for the logged municipal population, which may reflect later changes to the program which shifted priority to smaller municipalities.

## C.2 Dependent variable as percentages

This robustness check changes the dependent variable. Instead of using log counts, we follow Poulsen (2019) and use the percentage of departures, dismissals, and hires. With $n_e$, $n_h$, $n_d$, $n_f$ the numbers of employees, hires, departures, and dismissals respectively, we compute:

$$\begin{aligned}
\text{pct. hire} &= \frac{n_h}{n_e - n_d - n_f}, \\
\text{pct. departure} &= \frac{n_d}{n_e - n_h}, \\
\text{pct. dismissal} &= \frac{n_f}{n_e - n_h}.
\end{aligned}$$

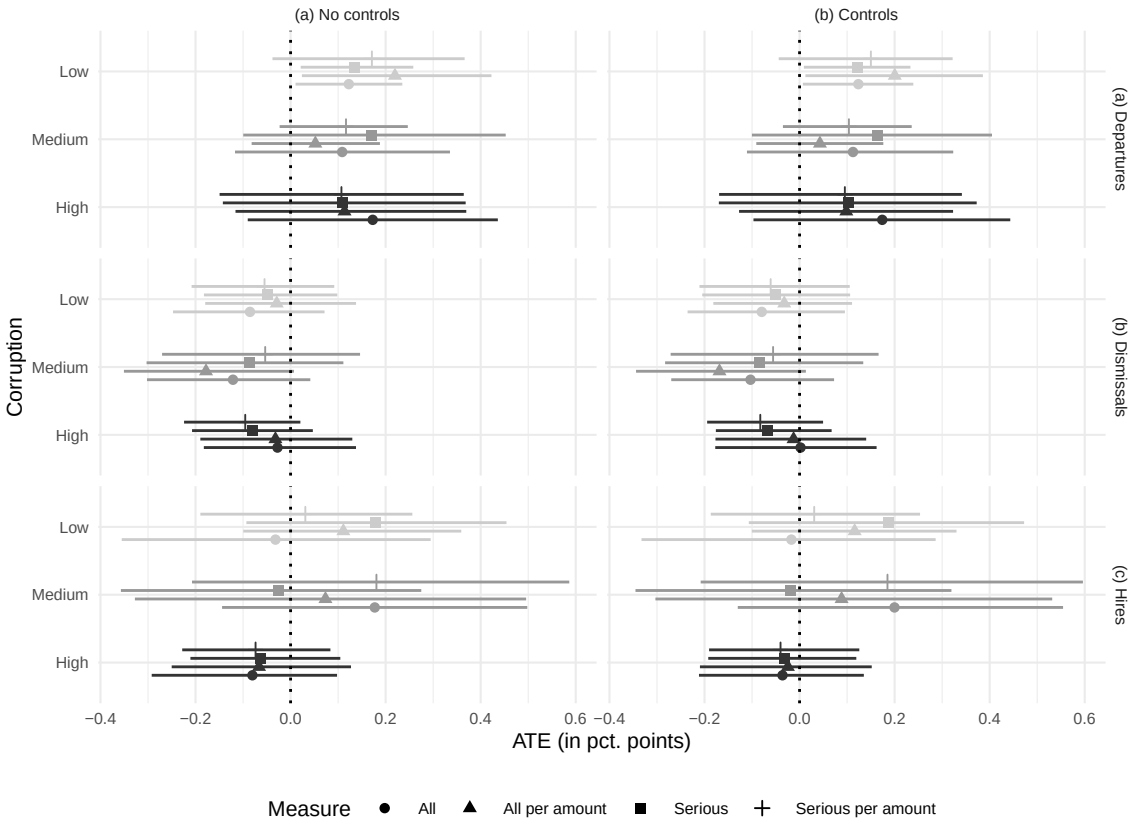



Figure 16: **Dependent variable specified as percentages.** This figure reproduces Figure 2 in the main text but specifies the dependent variables as percentages instead of log-counts. Findings are robust to this alternative specification of the dependent variable.

## C.3 Findings on management

In this section, we reproduce the bottom panel of Figure 2 but vary the number of items used to construct the management index. The measure used in the main text uses all survey items that were asked in at least one wave. Here, we make this index increasingly restrictive by using only the items that were asked in at least two, three, and more waves. Results are robust to this alternative specification. Findings are robust up to including all items that appear at least 3 times. Above this threshold, findings go away, presumably because sample sizes become prohibitively small.

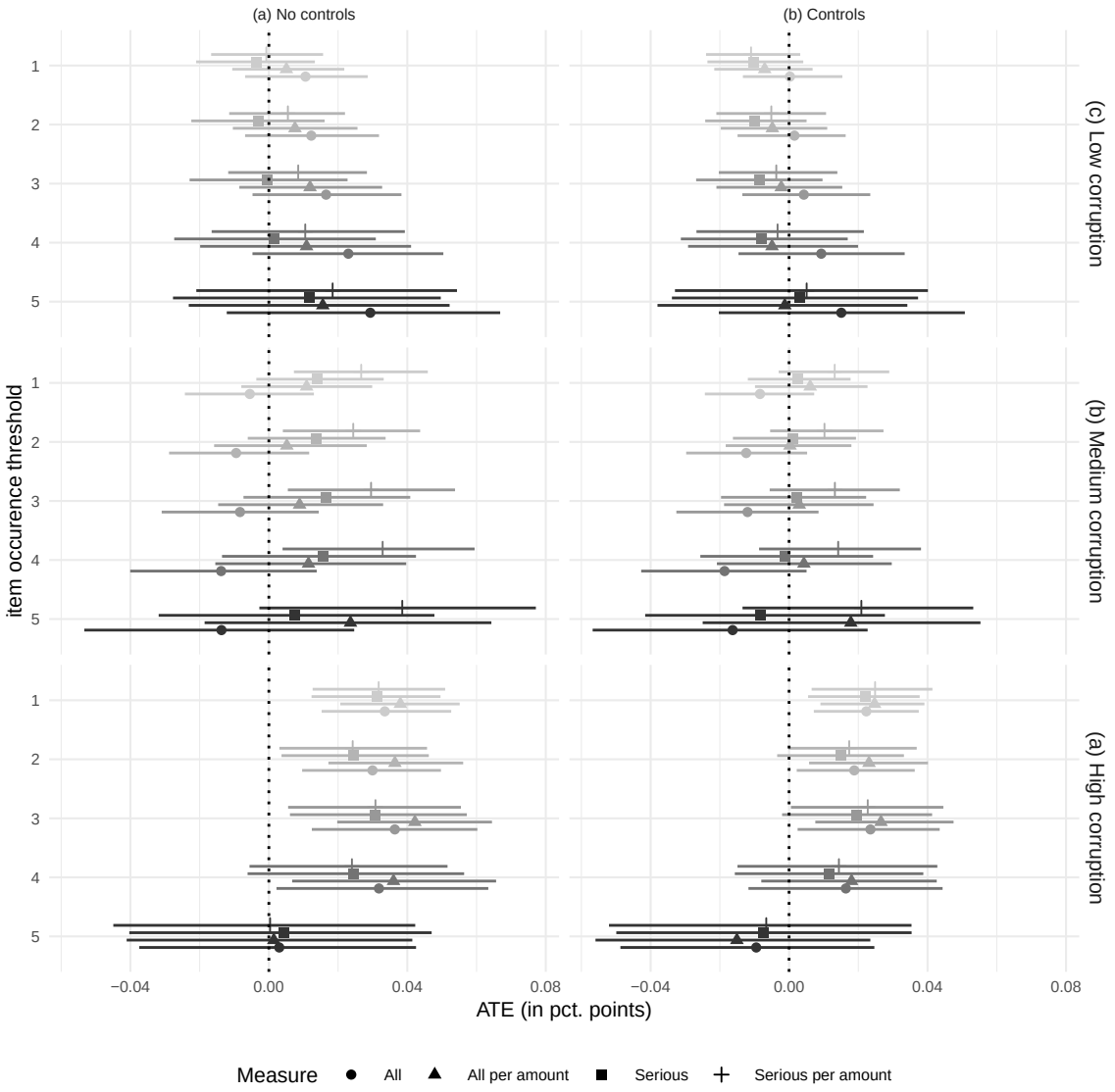



Figure 17: **Robustness of findings on management to alternative specifications of the management index.** This figure reproduces the bottom panel of Figure 2, but varies the the minimum number of occurrences necessary to include an item in the management index from 1 (threshold used in Figure 2) to 5. Findings are robust up to a threshold of 3 occurrences.

## C.4 Other categories of bureaucrats

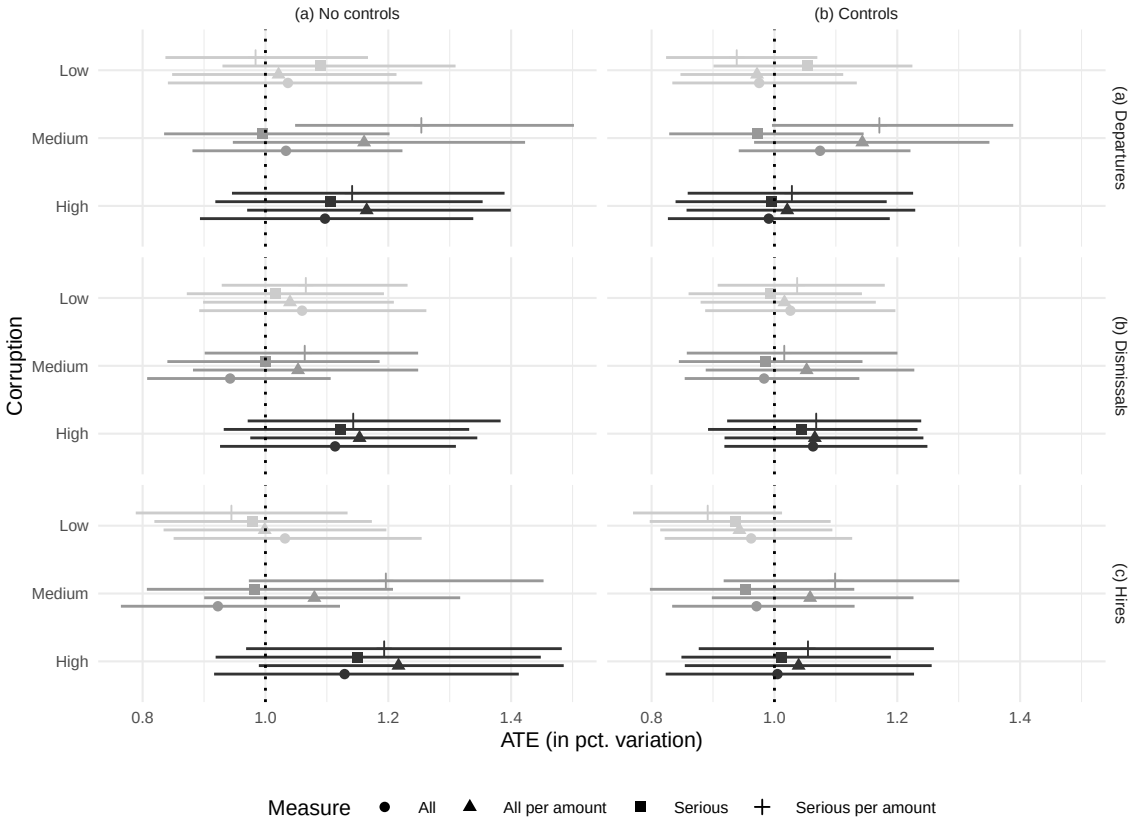



Figure 18: **Low bureaucrats.** This figure reproduces Figure 2 in the main text but considers low bureaucrats instead of high bureaucrats.

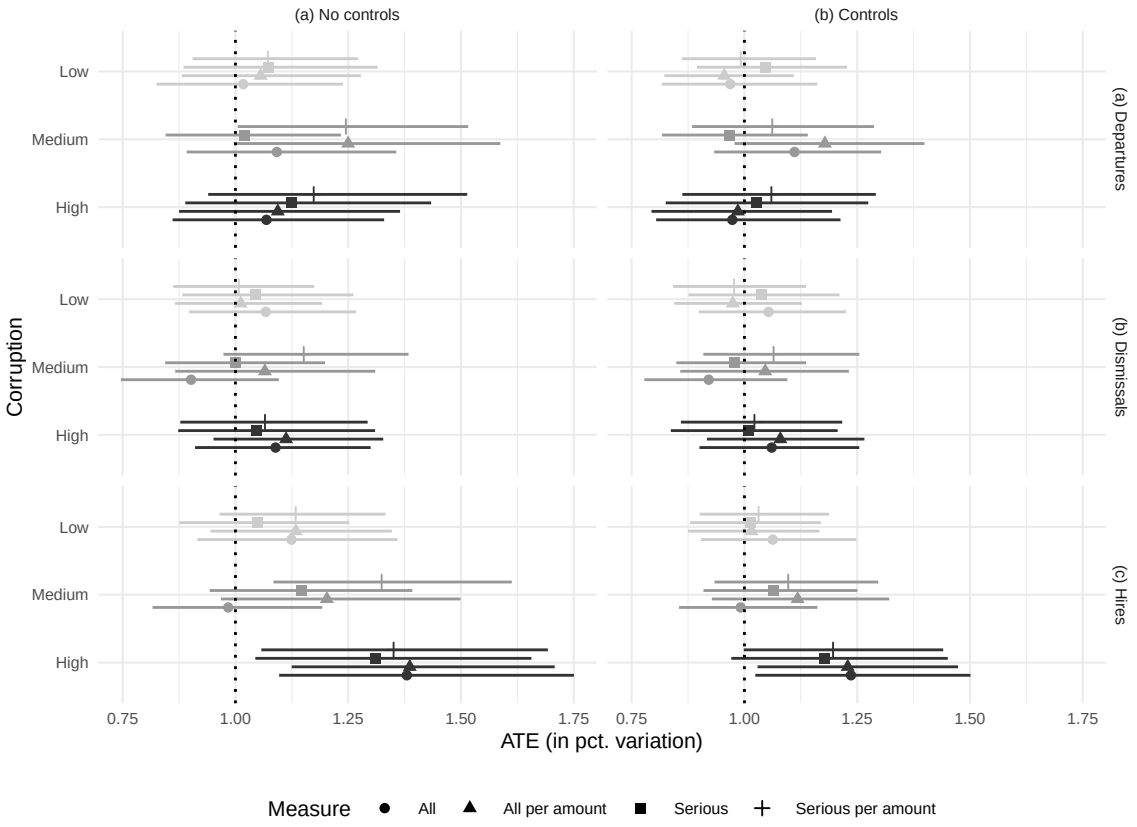



Figure 19: **High Frontline.** This figure reproduces Figure 2 in the main text but considers high frontline providers instead of high bureaucrats.

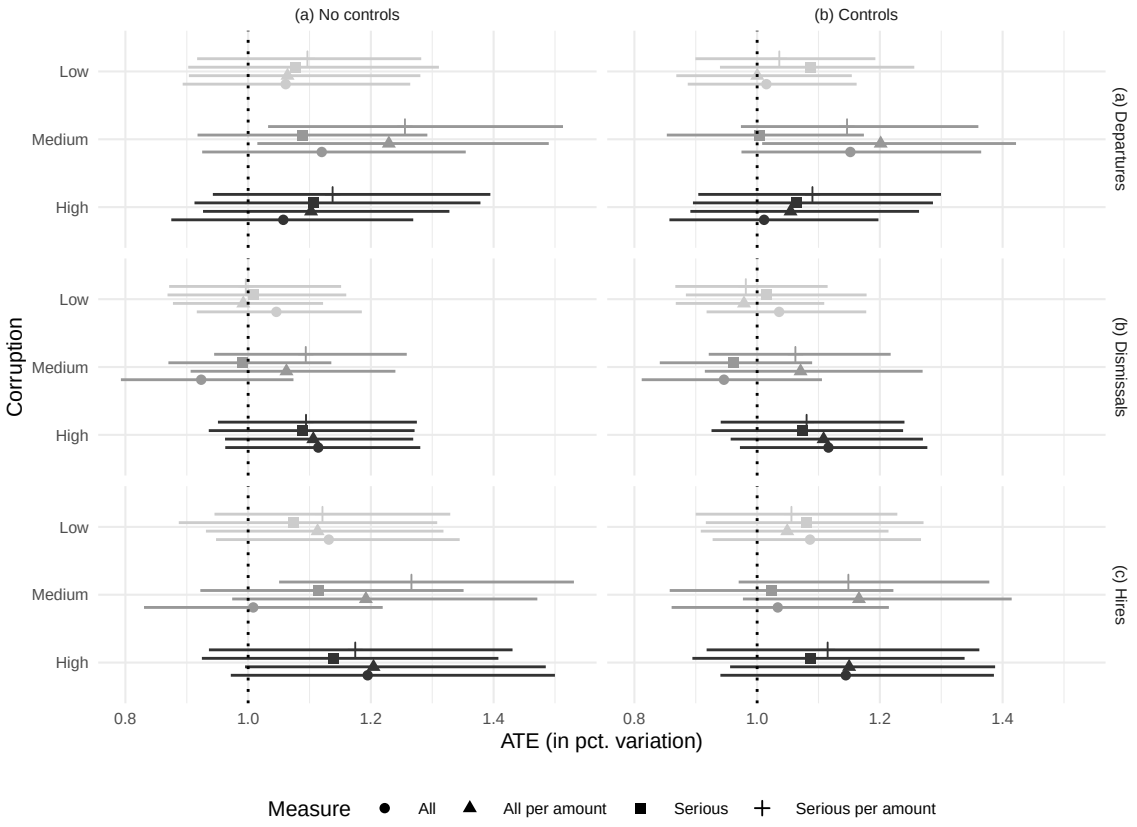



Figure 20: **Low Frontline.** This figure reproduces Figure 2 in the main text but considers high frontline providers instead of high bureaucrats.

## C.5 Subset of municipal secretaries

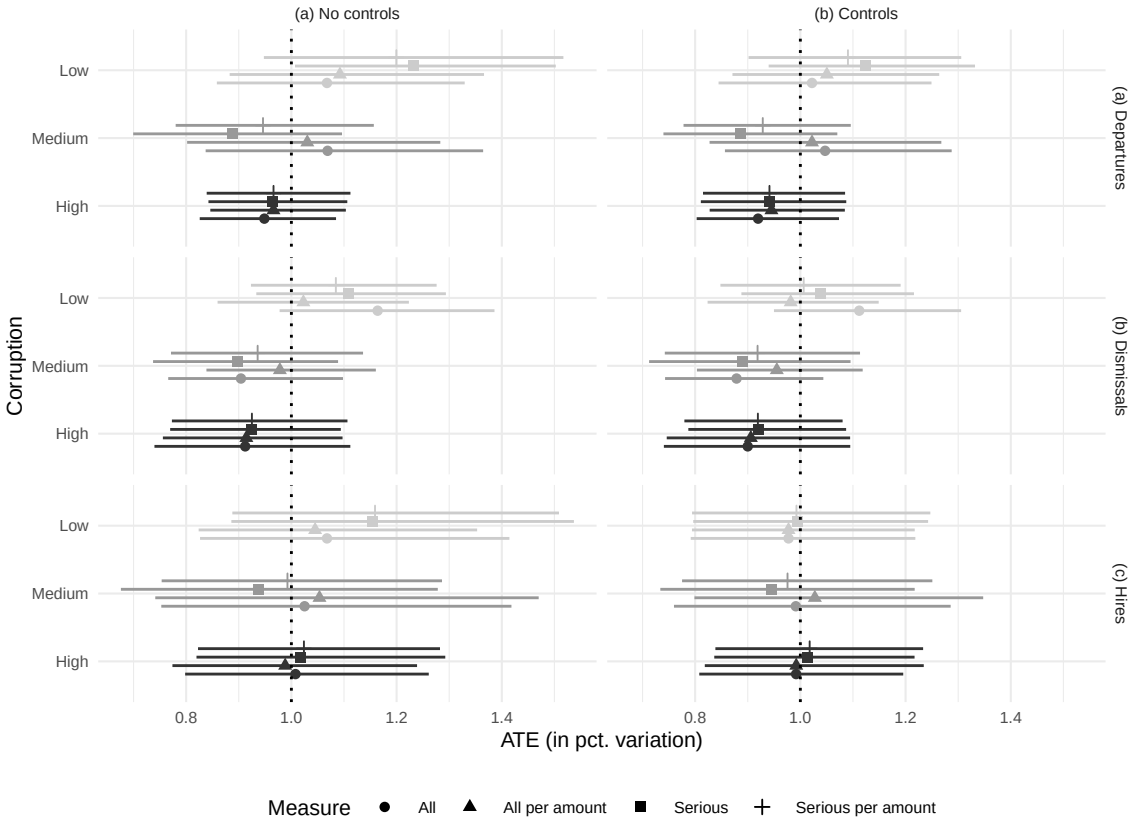



Figure 21: **Subset of municipal secretaries.** This figure reproduces Figure 2 in the main text but restricts the sample to the highest-ranking bureaucrats, namely municipal secretaries.

This robustness check focuses on the highest-ranking bureaucrats; namely, the set of municipal secretaries, who oversee municipal departments. Indeed, it might be the case that effects on personnel only affect those highest ranking employees. We find similar results. Unfortunately, this category is poorly identified by the standard classification of occupations (CBO), leaving us with municipalities that supposedly have no secretaries. We drop those from the sample.

## C.6 Subset of never audited municipalities

Some municipalities in our sample have been audited prior to 2006, the starting year of the period we consider in the study. This robustness check reestimates our models on the subset of municipalities that have been audited between 2006 and 2015, but have never been audited before, and finds similar results.

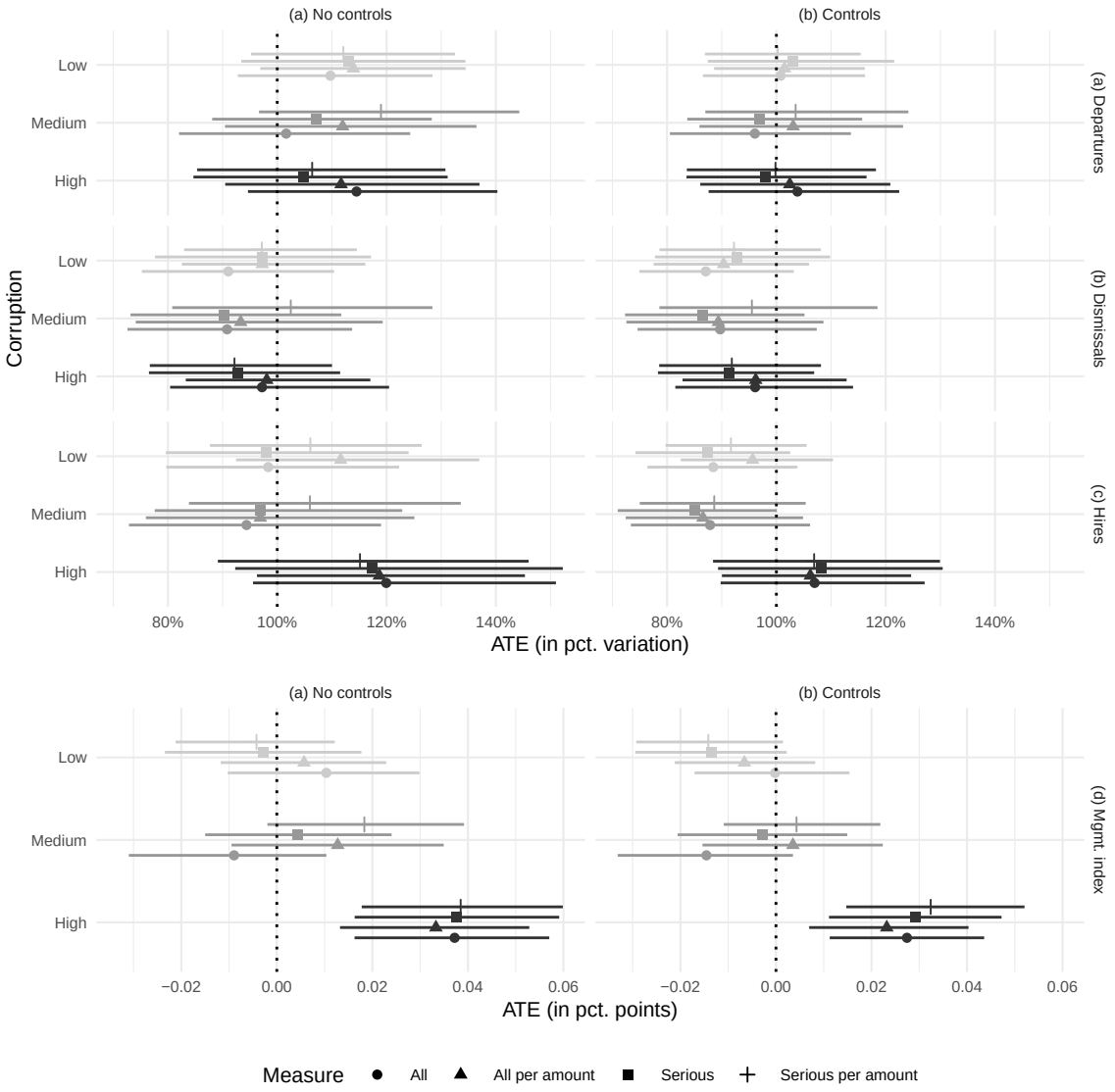



Figure 22: **Subset of never audited municipalities only.** This figure reproduces Figure 2 in the main text but considers only those municipalities that have never been audited before 2006.

## C.7 Individual-level analysis

The aggregated models we estimated in the previous section do not take full advantage of our micro-data, and separate departures from dismissals. We estimate instead a multi-outcome, discrete-time survival model in which bureaucrats may leave the bureaucracy either voluntarily or by being dismissed, and find again that audits have little effect on career interruptions.

Career interruptions may have two causes: dismissals and voluntary departures. Yet, those two events have a very different nature, since dismissals are imposed upon employees by management, while departures are voluntary, or mandated by some life event (e.g. injury or retirement). As such, they might respond differently to treatment. We inves-

tigate the possibility by considering a discrete-time multi-outcome proportional hazard model. It turns out that this model reduces to a multinomial logistic regression with year fixed effects. We take equation 1 to the individual level, defining the outcome $y_{ijst} = 0$ if bureaucrat $i$ is employed by the end of year $t$, $y_{ijst} = 1$ if $i$ departed during year $t$, and $y_{ijst} = 2$ if $i$ was dismissed during year $t$.

These models yield two sets of parameters that quantify the impact of each variable on, in turn, the (log-)odds of departure ($y_{ijst} = 1$) and dismissal ($y_{ijst} = 2$) relative to no career interruption ($y_{ijst} = 0$). Since generalized linear models are highly sensitive to model misspecification, all our specifications include controls. We add to the municipal-level controls used in our main specifications a number of individual-level controls that are either time-invariant or follow a deterministic evolution; namely, gender, education, contract type, years of work experience, and age.[18] Furthermore, since we compare bureaucrats that were affected by the audit to bureaucrats, in the same year, in municipalities that have not been audited yet, we only consider those bureaucrats that entered (and possibly left) the bureaucracy before the audit. In other words, we discard those employees that entered the bureaucracy after the audit, since this event may have been affected by treatment. Additionally, we compare within cohort by adding a cohort fixed effect.

Table 6 reports the results, and confirms that audit have no discernible effect on career interruptions. None of the coefficients of interest are consistently different from zero across corruption metrics. Furthermore, the ones that are point at a modest *chilling* effect, showing that audits lead to decreases the probability of departure and dismissal in moderate-corruption municipality (models 1 and 3), or in high-corruption municipalities (model 4).

[18]That is, log number of employees in 2006 and their median wage, municipality-level illiteracy rate, urbanization rate and gini measured in the 2001 census, and the number of audited items. Our education variable contains the levels "none," "primary school," "middle school," "high school," and "higher education." The contract type variable is a binary variable that separates tenured from untenured contracts.

| | (1) All faults | (2) Serious faults | (3) All faults normalized | (4) Serious faults normalized |
|---|---|---|---|---|
| $\beta_1$ - departure | | | | |
| treat ($\beta_{11}$) | 0.136 | 0.0777 | 0.0835 | 0.204 |
| | (0.96) | (0.57) | (0.68) | (1.53) |
| moderate corruption | 0.462* | 0.348 | 0.153 | 0.284* |
| | (2.16) | (1.84) | (0.67) | (2.05) |
| high corruption | 0.241 | 0.223 | 0.127 | 0.539* |
| | (1.02) | (0.97) | (0.68) | (2.39) |
| treat × moderate corruption ($\beta_{21}$) | -0.412* | -0.180 | -0.368 | -0.255 |
| | (-2.23) | (-0.95) | (-1.81) | (-1.48) |
| treat × high corruption ($\beta_{31}$) | -0.0119 | -0.156 | -0.206 | -0.867*** |
| | (-0.05) | (-0.69) | (-1.25) | (-3.75) |
| $\beta_2$ - dismissal | | | | |
| treat ($\beta_{12}$) | -0.0109 | 0.224 | -0.165 | -0.125 |
| | (-0.08) | (1.50) | (-1.08) | (-0.81) |
| moderate corruption | 0.221 | 0.351* | -0.0332 | 0.266 |
| | (1.42) | (2.32) | (-0.17) | (1.35) |
| high corruption | 0.104 | 0.339 | -0.414* | 0.0762 |
| | (0.42) | (1.39) | (-2.03) | (0.34) |
| treat × moderate corruption ($\beta_{22}$) | -0.138 | -0.492* | 0.0290 | 0.0779 |
| | (-0.68) | (-2.25) | (0.14) | (0.37) |
| treat × high corruption ($\beta_{32}$) | -0.187 | -0.650* | 0.273 | -0.127 |
| | (-0.81) | (-2.53) | (1.34) | (-0.52) |
| $\beta_{11} + \beta_{21}$ | −0.276** | −0.102 | −0.284** | −0.052 |
| $\beta_{11} + \beta_{31}$ | 0.124 | −0.078 | −0.122 | −0.663*** |
| $\beta_{12} + \beta_{22}$ | −0.149** | −0.268 | −0.136** | −0.047 |
| $\beta_{12} + \beta_{32}$ | −0.198 | −0.426 | 0.108 | −0.252*** |
| Observations | 448493 | 448493 | 448493 | 448493 |
| *AIC* | 357677.3 | 357656.9 | 357975.0 | 357270.2 |

$t$ statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: **Treatment effect with multiple outcomes.** Coefficients are odds ratios from multinomial logistic regression models with 95 percent confidence intervals clustered at the municipality-level. The 4 rows that add parameters report the sum of the parameters, with stars corresponding to the p-value of the associated $\chi^2$ test. All models include year, state, and cohort fixed effects and the controls discussed in this section. Audits have no effect on career interruption that is consistent across all corruption metrics. If anything, results points at a moderate chilling effect: audits reduce the probability of departure and dismissal in moderate-corruption municipality (models 1 and 3), or in high-corruption municipalities (model 4).

## C.8 Political models with other corruption metrics

In this section, we reproduce the model reported in Figure 4 of the main text using different metrics for corruption. We also report, for all such metrics, a model that tracks the effect of audits on the cohort hired by an incumbent mayor in the first year of her first term, after she has lost the election and is replaced by her challenger. For those models, the x-axis reports the electoral years of the challenger's term, while the colors refer to the year of the incumbent's term during which the audit ocurred. All specifications point towards the same conclusion that despite there being evidence of seasonality in staff rotation, with spikes in hiring and career interruptions around election years, audits do not significantly affect this pattern.

### First term

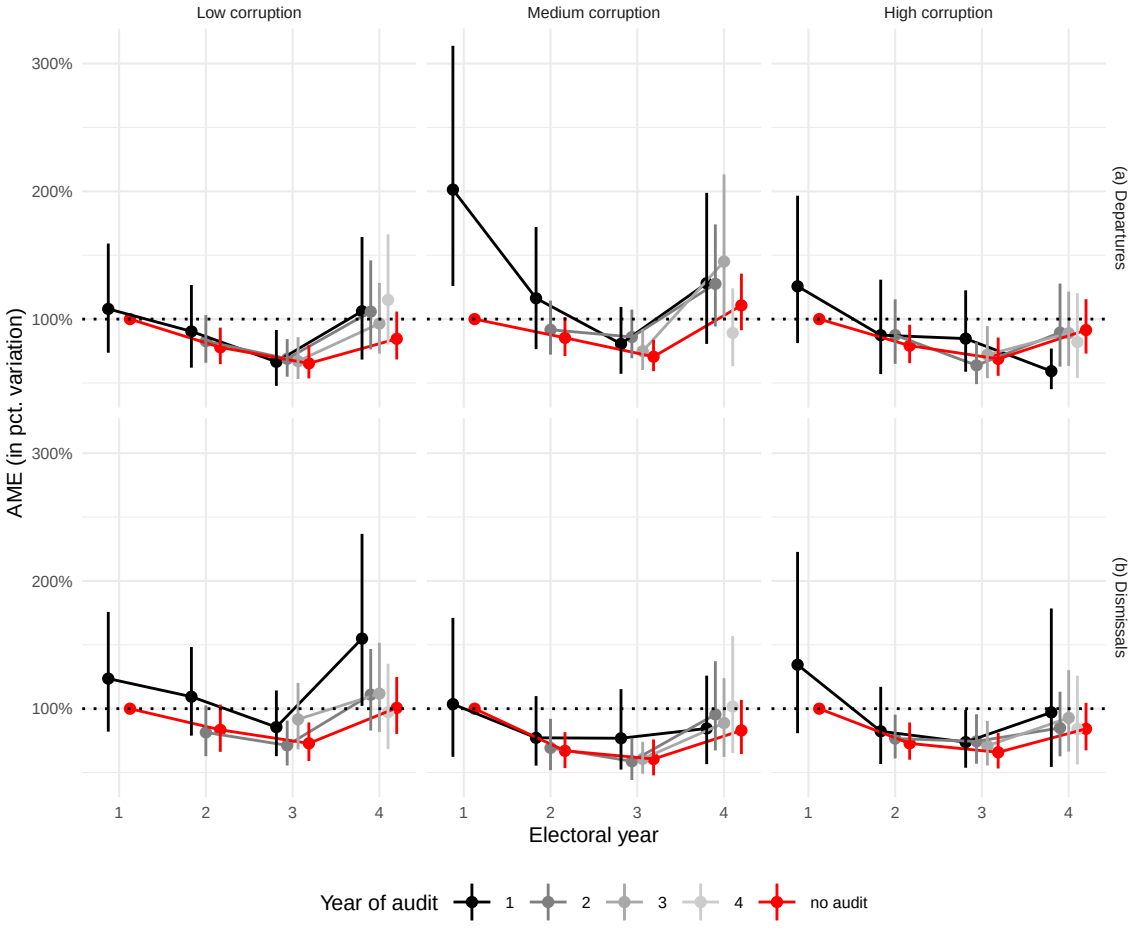



Figure 23: **Treatment effect as a function of the political cycle during the incumbent's term, corruption = # serious faults.** This figure reproduces Figure 4 in the main text but uses # serious faults as a measure of corruption.

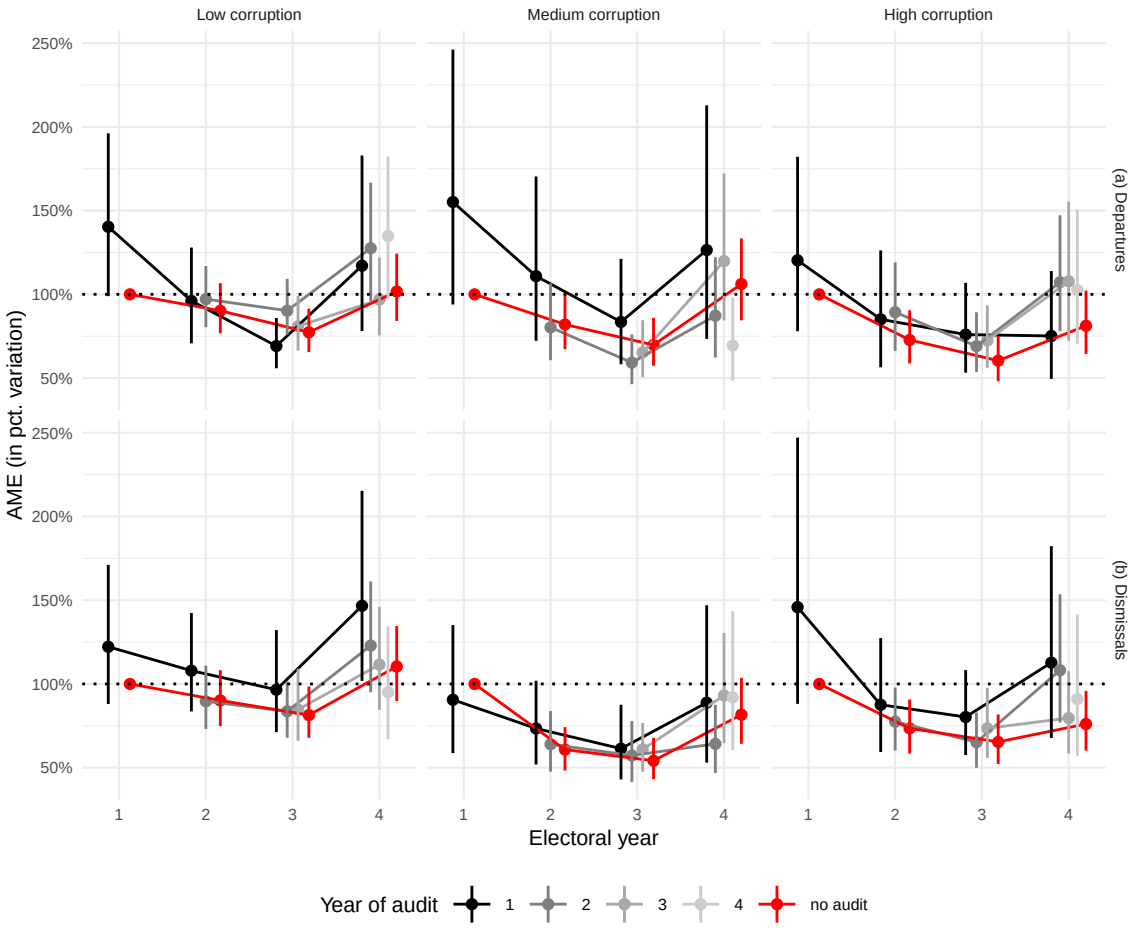



Figure 24: **Treatment effect as a function of the political cycle during the incumbent's term, corruption = total # faults per amount audited.** This figure reproduces Figure 4 in the main text but uses total # faults per amount audited as a measure of corruption.

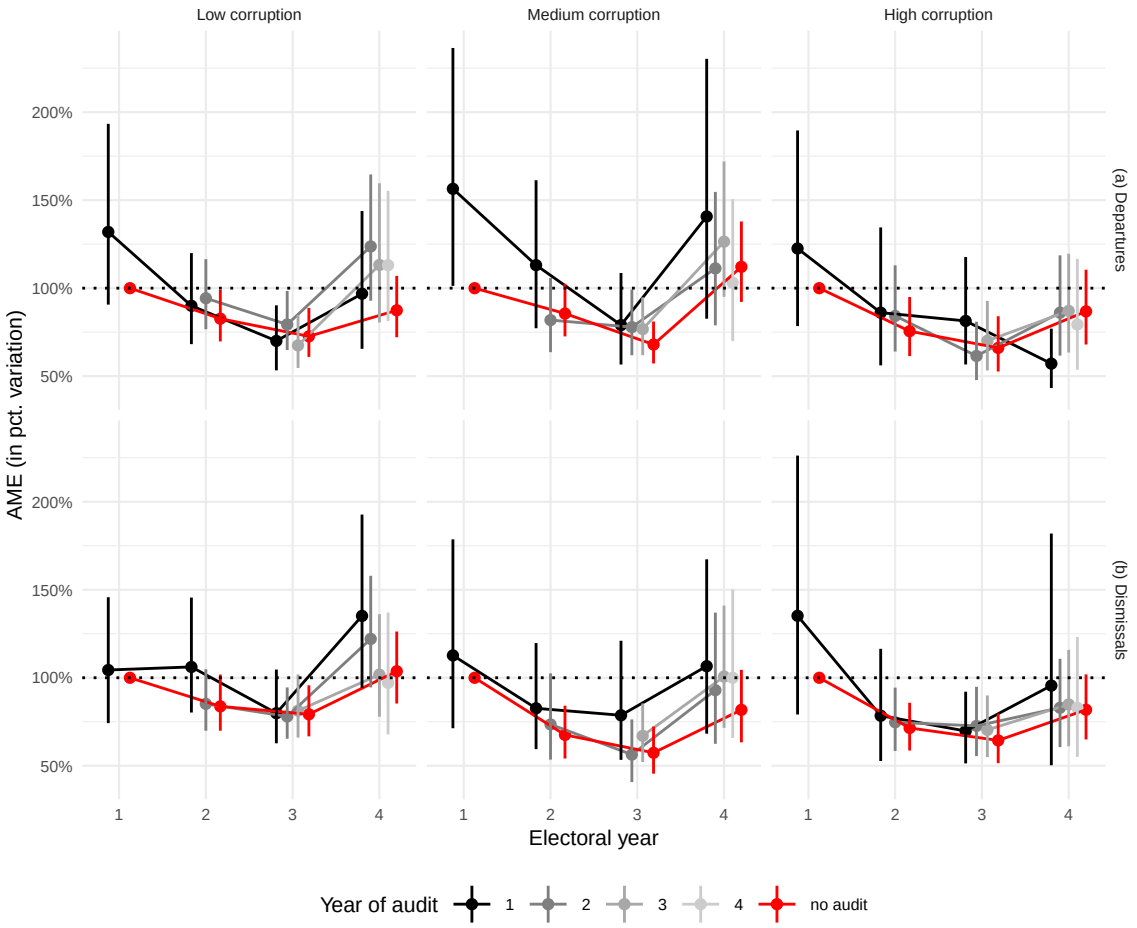



Figure 25: **Treatment effect as a function of the political cycle during the incumbent's term, corruption = total # faults per amount audited.** This figure reproduces Figure 4 in the main text but uses total # faults per amount audited as a measure of corruption.

## Second term

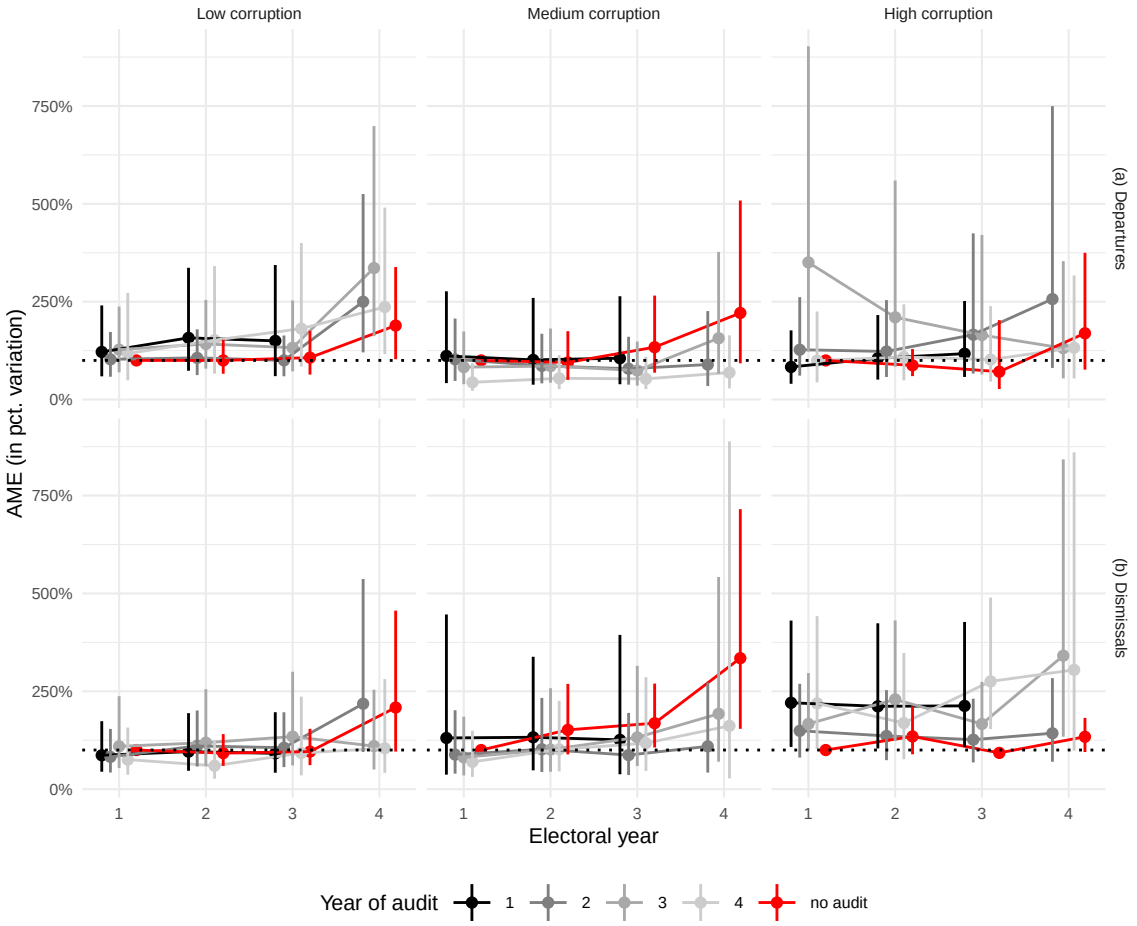



Figure 26: **Treatment effect as a function of the political cycle during the challenger's term** The y-axis represents the average marginal effect of audits the row outcome. The x-axis represents years in the political cycle, with year 1 being the first year of mandate. Colors indicate the year of the political cycle during which the audit occurred. Bars are 95 percent confidence intervals clustered at the municipality level. All models use the controls discussed in section 3.1. Again, we find no evidence that anti-corruption audits induce any changes in bureaucratic personnel.

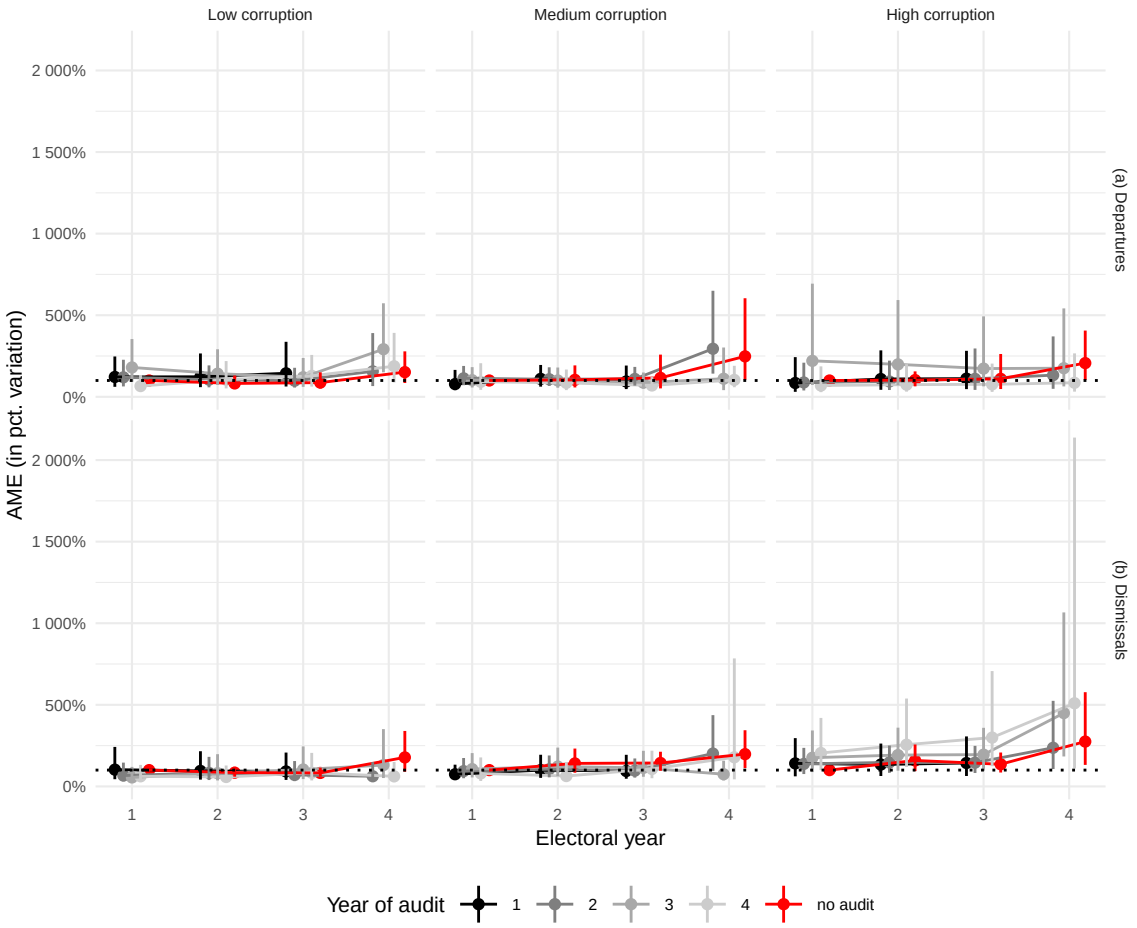



Figure 27: **Treatment effect as a function of the political cycle during the challenger's term, corruption = # serious faults.** This figure reproduces Figure 26 above but uses # serious faults as a measure of corruption.

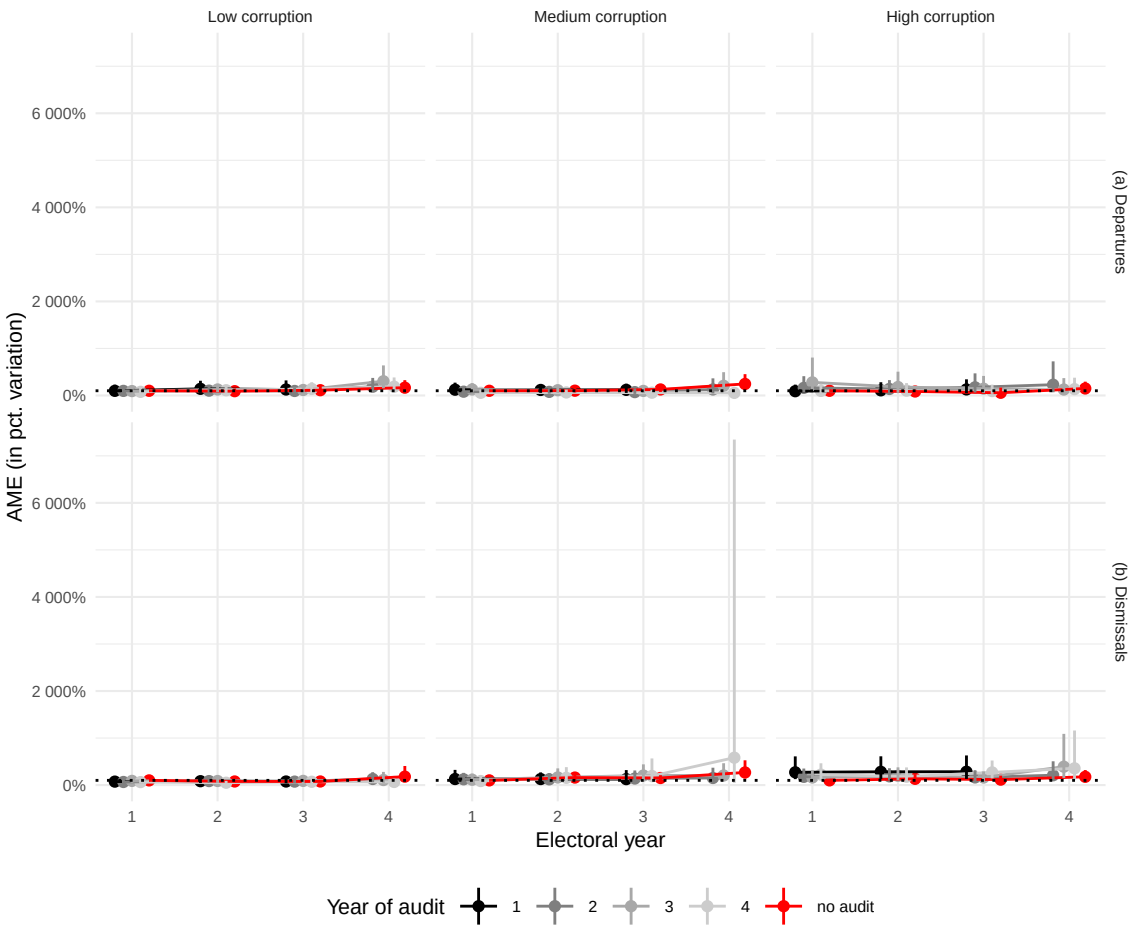



Figure 28: **Treatment effect as a function of the political cycle during the challenger's term, corruption = total # faults per amount audited.** This figure reproduces Figure 26 above but uses total # faults per amount audited as a measure of corruption.

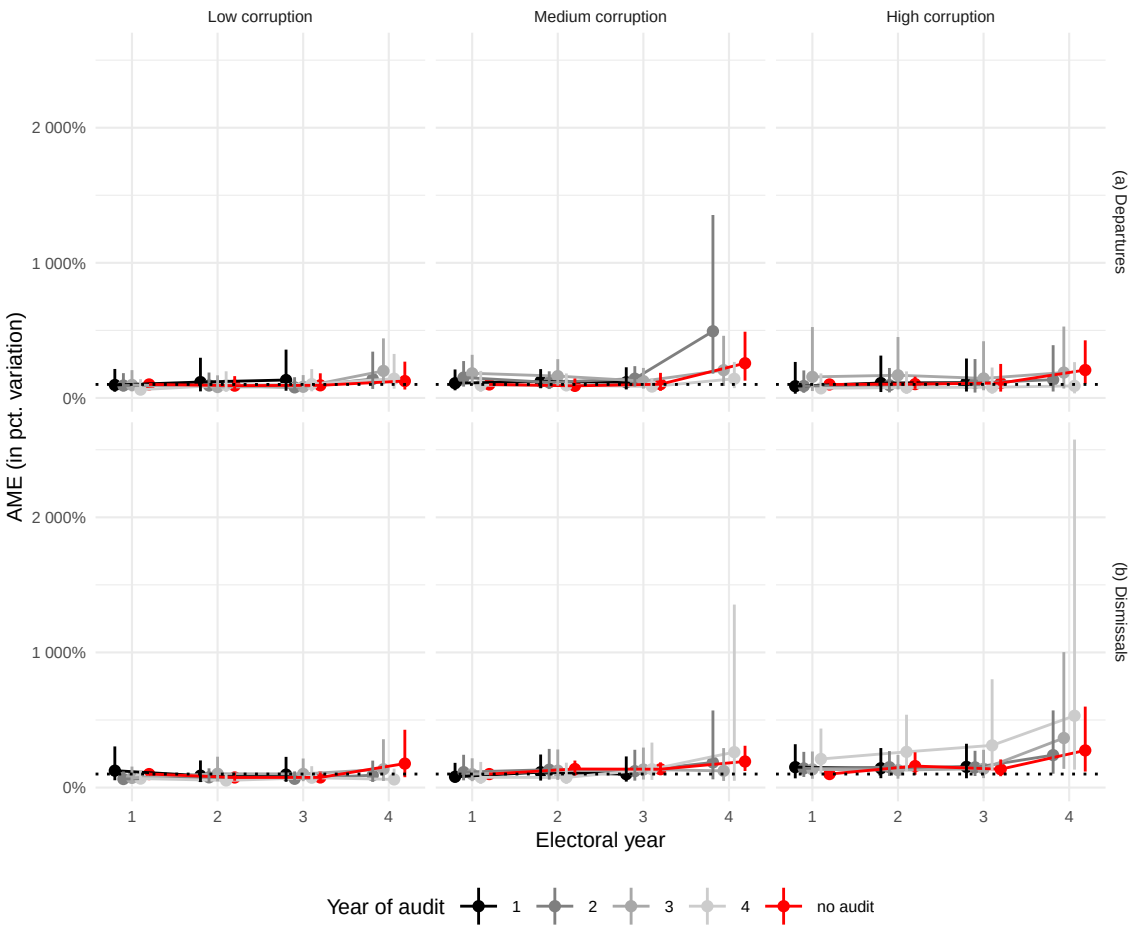



Figure 29: **Treatment effect as a function of the political cycle during the challenger's term, corruption = total # faults per amount audited.** This figure reproduces Figure 26 above but uses total # faults per amount audited as a measure of corruption.

# D Validation of the structural model

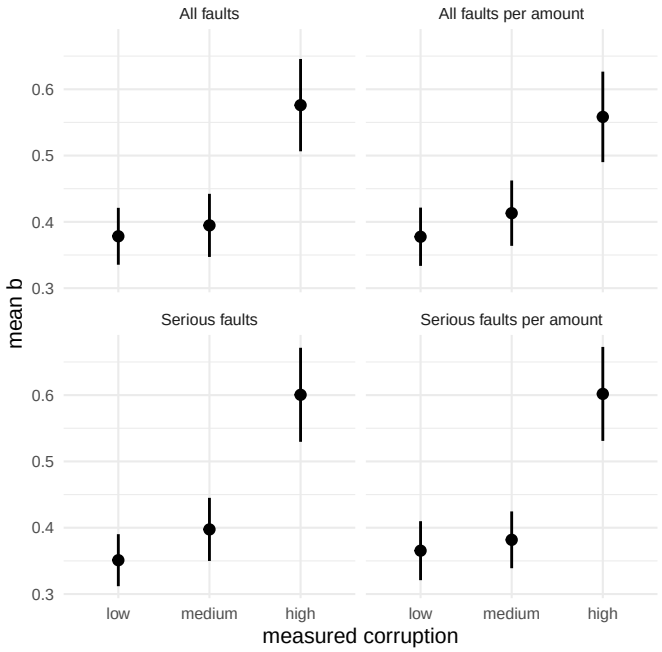



Figure 30: **Validation (1).** Municipalities that have been measured as more corrupt have significantly higher values of $b$, irrespective of the corruption metrics.

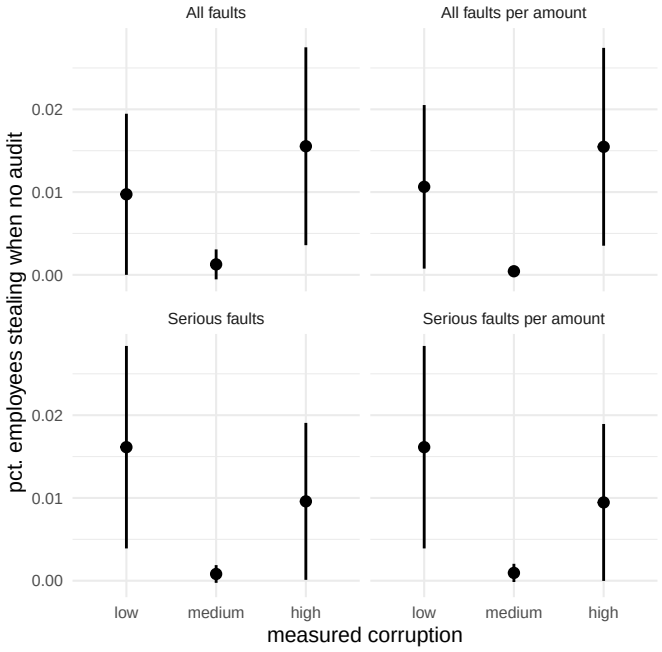



Figure 31: **Validation (2).** Although estimates are not statistically significant, highly corrupt municipalities tend to have a larger share of employees using policy "steal when no audit."