

Contents

1	Education Patronage	4
1.1	Introduction	1
1.2	Related literature	3
1.3	Institutional context and data	6
1.3.1	Municipal education in Brazil	6
1.3.2	Local governance and mayoral coalitions	8
1.3.3	Data	9
1.4	Theory	12
1.5	Empirical results	19
1.6	Conclusion	28
1.6.1	Covariate balance	30
1.6.2	Accountability of mayors	31
1.6.3	Regression Discontinuity Design	32
2	Corruption Audit	33
2.1	Introduction	35
2.2	Context and Data	39
2.2.1	Municipal bureaucracies and management	39
2.2.2	Federal transfers and anti-corruption audits	43
2.2.3	Additional data	45
2.3	Reduced-form estimation	45
2.3.1	Approach	46
2.3.2	Results	49
2.3.3	Discussion	53

2.4	Theory	54
2.4.1	Setting	56
2.4.2	A baseline model	58
2.4.3	Introducing a wage penalty	59
2.4.4	Introducing a clean-up effect	61
2.4.5	Discussion	62
2.5	Structural estimation	64
2.5.1	Approach	65
2.5.2	Estimates, validation and counterfactual experiments	67
2.6	Conclusion	73
2.7	Appendix	76
2.7.1	Proofs	83
2.7.2	Additional descriptive statistics	86
2.7.3	Measuring corruption	86
2.7.4	Management index	91
2.7.5	Additional descriptive statistics	94
2.8	Robustness checks	95
2.8.1	Other categories of bureaucrats	95
2.8.2	Subset of municipal secretaries	98
2.8.3	Subset by Tenure	99
2.8.4	Hiring Practices	101
2.8.5	Balance tests	103
2.8.6	Dependent variable as percentages	103
2.8.7	Findings on management	105
2.8.8	Subset of never audited municipalities	107
2.8.9	Individual-level analysis	108
2.8.10	Political models with other corruption metrics	112
2.9	Estimation and validation of the DDC model	119
3	Patronage	122
3.1	Introduction	124
3.2	Context and Data	126

3.2.1	Brazilian municipal government	126
3.2.2	Municipal employment and patronage in Brazil	127
3.2.3	Partisan affiliation in Brazil	128
3.3	Data	129
3.3.1	Municipal and Private Sector Employment	129
3.3.2	Partisan affiliation	130
3.3.3	Joining Public Sector Employment to Partisanship Data	133
3.3.4	Who benefits from patronage?	135
3.3.5	Islands of Patronage	140
3.4	Campaign Finance and Wealthy Patrons	145
3.5	Conclusion	145

Chapter 1

Education Patronage

Governing through patronage: the bargain for
education in decentralized Brazil

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Abstract

How can governments improve the quality of education? I argue that local politicians govern through patronage, undermining education at the expense of voters. Building on qualitative evidence in Brazil, I leverage large-scale administrative data to show that public education is captured by local political elites. Mayors buy off local legislators to enact policy agendas by offering them positions into the educational sector. The degree to which patronage occurs varies: when mayors have a stronger ally base in the city council, they face less pressure to bargain. Patronage induces turnover in educational staff, with negative downstream effects on student learning. Weak electoral backlash suggest that patronage is primarily a political elite game, with limited accountability to the electorate. These findings point to the dangers of elite capture of public services and its downstream consequences for social welfare.

1.1 Introduction

Across the world, vulnerable populations rely on governments for access to basic public services such as education. In the developing world, the quality of public education remains woefully low: functional illiteracy remains widespread, basic mathematical operations.¹ Clearly, politicians play an important role in delivering these services (??), but what are the incentives they face and what are their implications for bureaucratic institutions responsible for delivering them. (??). How can local governments improve the quality the of public services they deliver? In particular, how do political actors reshape bureaucratic institutions, and what are their downstream consequences for student learning?

The quality of public education in the across the world, and particularly in the developing world, remains low: functional illiteracy is widespread, children cannot perform arithmetic operations. How can public education be improved?

Previous studies have found that certain incentives, whether these be electoral or performance-related, can help raise the quality of public services (??). Often relying on individualized interventions, these studies provide compelling evidence that political and bureaucratic actors respond to policy changes.(?) But how do these actors relate to one another in local institutions? Who holds power over public education, and whose interests is it meant to serve?

To answer these questions, over two years I conducted fieldwork research in municipalities across Brazil. In interviews with local politicians, school principals and teachers, I discovered what many scholars working on the developing world already know: clientelism is pervasive, and it extends to public education with nefarious consequences (?). However, the type of clientelism I observed was not embedded in electoral politics (?) or partisan networks (??), but the elite politics of government (?). In Brazil, the exchange of public sector appointments for political favors – a canonical example of patronage – grants the mayor a carte blanche to enact her policy agenda.

¹The World Bank calls this “learning poverty”. See <https://www.worldbank.org/en/topic/education/brief/learning-poverty>

In this paper, I argue that public education in municipalities across Brazil have been captured by political elites, with personnel appointments into public schools and management dominated by bargains between executive and legislative branches. Similar to coalition building in presidential contexts, mayors bargain for legislative support for their policy agenda through the allocation of public sector positions to city councilors and their constituencies (??). This effectively crowds out electoral accountability, as mayors prioritize building support by other political elites over voter welfare (?). By catering to the city council through patronage appointments, mayors shuffle the local educational bureaucracy, with negative downstream consequences for public school students.

The estimation proceeds in two parts. First, I show that political alignment between mayors and city councilors has a direct effect on patronage. Building on a canonical model of legislative vote-buying, I propose a theoretical model to analyze patronage under an institutionalized separation of powers, deriving my main empirical test: mayors who face less opposition in the city council engage in less patronage. To verify this hypothesis, I build a set of indicators to track educational staff turnover, leveraging administrative data of over 2 million school principals and teachers employed by municipalities. In line with qualitative accounts and theoretical expectations, mayors who have a stronger ally base in the legislature engage in less patronage. This results holds across a set of specifications and different measures of turnover.

I then show that this politically induced turnover has a negative impact on student learning. To estimate the effect of patronage on quality of education, I combine qualitative and quantitative evidence. Interviews conducted with educational bureaucrats and politicians suggested firm that turnover had a negative impact on teachers' ability to educate students, as time horizons were compressed and bonds between educators and learners were broken. To validate these accounts I combine administrative data on education from two separate student learning measures: *Prova Brasil* and the *SPAECE*. I construct multiple datasets to test these claims: the main specification contains over 1 million classrooms spread across the national territory. A set of es-

timations, combining multi-level modeling and fixed effects, provide strong evidence that teacher turnover has a negative effect on student learning.

This study contributes to an emerging literature on the politics of personnel and public services (??). I analyze how incentives shape political decision on how to manage local bureaucracies (??), but I highlight the non-electoral policy goals of executive leaders. I also contribute to a growing research agenda on the consequences of political competition, demonstrating that increased political fragmentation between executive and legislative branches can lead patronage and decrease in the quality of public services. (??). I bring to focus the end-to-end provision of public services, echoing a long-standing literature on state capacity (??).

The paper is structured as follows. Section 1.2 provides an overview of the scholarly debate over public goods provision and personnel, as well as more specific treatments of the subject in Brazil. Section 3.2 presents the context and data. In section 2.4, I present the main argument, with a formal treatment of the subject using a canonical vote-buying model. Section 1.5 presents the research design and main results. Section 5 concludes.

1.2 Related literature

In this section I review extant literature on public goods provision and the politics underlying it, focusing on more recent studies of bureaucratic personnel and political leaders reshape these institutions. I also highlight how my research incorporates multiplicity in political actors and how this affects bargaining over public sector jobs. I address this gap by adapting previous analyses of executive-legislative bargaining to bureaucratic control at the local level.

Bureaucratic personnel and public goods provision

Bureaucracies have a clear impact on the delivery of public services. A long-standing literature on state capacity provides a theoretical and substantive

foundation to analyze bureaucratic institutions (??). A first generation of scholars, focusing on the successful developmental cases of East Asia, highlighted the need for a technocratic and autonomous bureaucracy (??). A Weberian wall separating bureaucrats from elected officials was considered indispensable for the successful provision of economic growth (?).

Recent studies have added nuance to these claims, finding that high bureaucratic performance can coexist with political interference. Toral 2019 finds in Brazil that school principals appointed by mayors tend to perform better than their non-appointed counterparts in standardized test scores. ? show that local politicians who are able to internalize electoral benefits make bureaucrats exert more effort, increasing local employment. ?, on the other hand, highlight the pitfalls of political capture, showing that party turnover can lead to the replacement of school principals, with detrimental effects for student learning.

This recent wave of studies shed light on the intersection between politicians and bureaucracies. However, few of these studies explicitly model the multiple actors involved in managing bureaucracies. Understanding their diverse goals and action space provides a firm theoretical foundation to how different politicians can reshape bureaucracies. To do so I turn to the well-established literature on executive-legislative bargaining, applying its insights to the analysis of local government and administration.

Presidential coalitionism and patronage

For every elected mayor in Brazil, a group of legislators are also elected into office. These political actors have competing claims over the local bureaucracy, with important implications for public service delivery. This structure parallels other settings with an institutionalized separation of powers and a bureaucratic pie to be split among the actors (??). Divergent political interests can lead to strategic interaction between executive and legislative actors. A rich literature in Brazil explores these relations, with important insights to how executive and legislators bargain over bureaucracy. ??.

In the Brazilian federal context, executive-legislative relations are analyzed under the prism of presidential coalitionism. Executive leaders garner legislative support from the National Congress by exchanging key positions in the federal bureaucracy, appointing members of their legislative coalition into cabinet positions. (?). In a setting characterized by weak party cohesion and programmatic commitments ??, bureaucratic positions for members of the coalition provide material incentives for legislators to support the executive agenda ???.

In municipalities, mayors have to garner legislative support from city councilors to secure budgetary approval and implement desired policies. Due to weak programmatic commitments at the local level, public sector jobs are used to legislative support.² Mayors enjoy full discretion into how to appoint workers into the public sector, and use patronage to coopt legislative support from members of the coalition, a practice known locally as *empregoismo*. As noted by a former mayor of the municipality of Sobral, "city councilors knocked on my door with a list of names for people they wanted me to hire."³ These hires induce changes in personnel, with important consequences for bureaucracies and educational services.

Bureaucratic turnover and inexperienced education

Bureaucracies exposed to turnover experience productivity shocks, often with detrimental effects. As new staff enters the bureaucracy, they must learn and acquire skills to deliver services to the population ?. Focusing on education, studies show that students taught by inexperienced teachers perform worse than those attending class with an experienced teacher ?. ? finds that students attending a school with a recently appointed school principal perform worse in standardized test scores. When bureaucratic turnover is driven by patronage, political concerns take precedence over meritocratic ones ?.

"I am aware that the position is temporary. Especially because it

²Interview with C, August and September 2019.

³Interview with C, August 2019.

is a political position, decided by the administration. If the current administration is out of power, we are automatically dismissed.” - Interview with school principal A, August 2019.

Negotiations between executive and the legislative thus have a knock-on effect on the quality of educational services, as political considerations lead to bureaucratic turnover at the school and administrative level. In this study, the primary focus is on bureaucrats working within the boundaries of a school: school principals and teachers. In the following section I describe the institutional context for public education in Brazil, as well as the data employed for the the estimation.

1.3 Institutional context and data

1.3.1 Municipal education in Brazil

In Brazil, the responsibility to provide public, primary education is primarily delegated to local governments ?. In this critical learning period, students acquire skills such as reading/writing, as well as foundational concepts in math such as addition and subtraction. The municipal educational system has increased in relevance over the past few decades, and currently over 60 percent of lower school students attend a public school. Figure 1.1 plots the total number of students in primary education per government level, including private schools. As of 2016, over 25 million students were enrolled in over 115 thousand municipal schools.

The local executive branch has sole jurisdiction over the hiring and firing of teachers and school principals. School principals are considered positions of trust (*cargos de confiança*) and usually appointed by the mayor or secretary of education ?. Teachers usually enter the public service through a public exam and are eligible for tenure after two years ?. However municipalities increasingly resort to temporary contracts to hire new teachers as budgetary pressures amount. Municipal teachers and school principals are overseen by a local department of education (?).

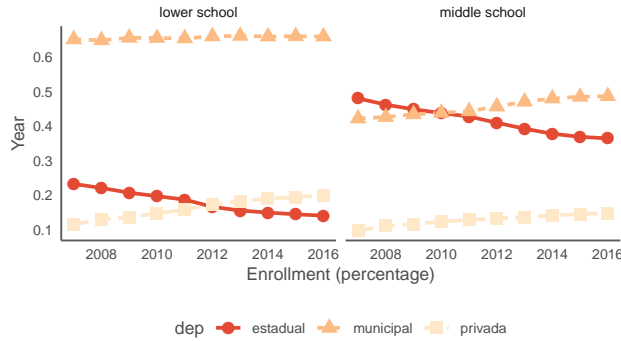
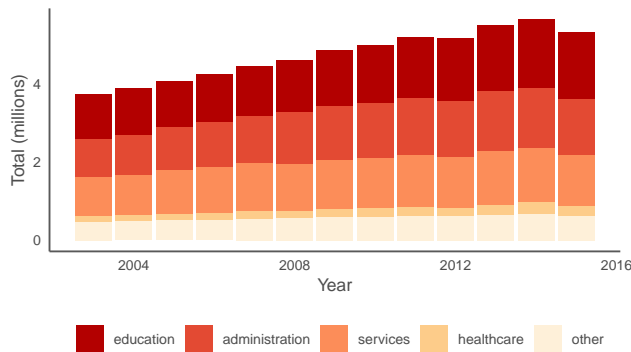


Figure 1.1: **Student enrollment by administration.**

Positions in the educational sector are politically valuable. As a department, it comprises over 25 percent of local public sector jobs. Due to their relatively high compensation and social status compared to other positions in the local bureaucracy, educational staff positions are particularly valuable for constituencies seeking employment. Additionally, qualitative evidence collected during fieldwork suggests that teachers play an important role in local party networks, leading electoral campaigns and brokering votes, similar to dynamics found by ? in Argentina’s campaigning teachers.



Funding for municipal education relies primarily on federal transfers, the Fundeb, derived from 10 percent of tax revenues for each level of government to education. At the municipal level, 25 percent of the local budget must be allocated to education, and compensatory federal transfers are institution-

alized by law to those municipalities which do not reach the target.⁴ While municipalities may be audited to verify whether funds are being properly used, personnel decisions and daily operations are fully under municipal discretion ?.

1.3.2 Local governance and mayoral coalitions

Brazil's local level government is composed by over 5 thousand municipalities, each with their own mayor hall (*prefeitura*) and city council (*câmara dos vereadores*). Mayors and city councillors (*vereadores*) are democratically elected every four years, with the possibility of reelection for both.⁵ The executive is responsible for the management of public services such as education, with exclusive rights over personnel appointment. The city council, on the other hand, oversees legislation and approves the budget for the fiscal year. As noted by ?, decisions over whom to appoint into the educational sector are primarily in the hands of the mayor and her secretary of education.

In order to win elections and garner support for their campaign, mayors form electoral alliances with local party labels ?. These mayoral coalitions, once in government, are an important basis for approving budgets and, more generally, executive control over the legislative chamber. Interviews with city councilors indicate that the legislature is divided into a pro-government (*governo*) and opposition (*oposição*) groups (*bancada*). While these electoral coalitions do not necessarily remain intact once governments are formed, fragmentation is generally on the margins, with mayors swapping defecting legislators for new ones.⁶

Interviews with secretaries of education and mayors provide evidence that city councilors play an important role in staffing decisions. While mayors hold jurisdiction over personnel decisions, there are extensive consultations between mayors and city councilors to decide who becomes the principal of a school, or which teacher remains in a school or leaves. These executive-legislative con-

⁴For more details on the Fundeb, see [here](#)

⁵The last municipal elections were in 2016. These take place every 4 years.

⁶Interview with C, former chief of staff of municipality J. September 2018.

sultations ultimately determine the allocation (*alotação*) of educational staff, serving as the primary tool through which mayors reward or coopt legislators into supporting them in the city council. In the words of a set of school administrators in the municipality of I:

School principal: Here, we are invited to work at the school by the department of education, with the [political] candidate, the city councilor...deciding which are the positions they are searching for and appointing people they think have the necessary qualifications.

Educational counselor: I was also invited to work here, by the city councilor and the department of education.⁷

These accounts, along with previous case studies of municipalities in Brazil, suggest that city councilors play an important role in nominating staff. To verify these claims in a broader set of municipalities, as well as outlining the research design, I apply a theory of legislative vote-buying that illustrates the employ extensive administrative and electoral data collected by the Brazilian federal government. In the next section I describe the data, where it is publicly available, as well as the preparation necessary for the set of estimations presented in the study.

1.3.3 Data

Brazil collects fine-grained data on education and makes it publicly available for research. For this study, data on education are collected from two main sources: the SAEB and School Census.⁸ The SAEB (National System for Educational Assessment) is a biannual standardized exam administered by the INEP (National Institute for Educational Studies and Research) to all public schools and a sample of private schools. In 2017, over 5 million students, in 5th and 9th grade, took the exam, testing their proficiency in both mathematics and Portuguese. In this study, test scores are the primary metric for assessing the quality of education received by students.

⁷Interview with the administrative board of school A., municipality J. August 2019.

⁸These can be accessed [here](#).

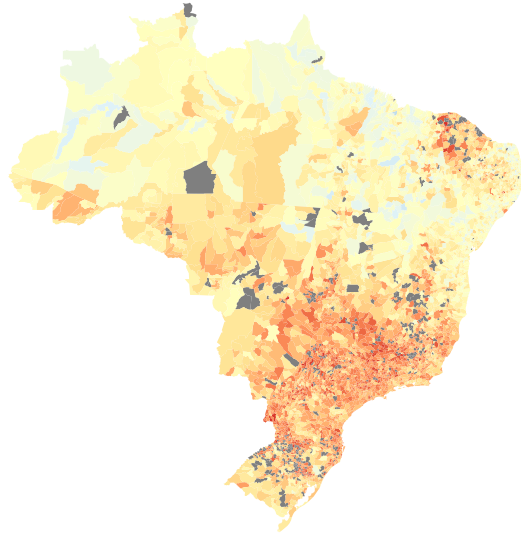


Figure 1.2: Uneven quality of municipal education. Polygons represent municipalities, warmer colors indicate higher municipal average in standardized test scores. Averages are weighted by student participation rate.

Figure 1.2 presents a map of Brazil with the municipal averages for the SAEB of 2015. Warmer colors such as red and orange indicate higher average scores, with the converse denoted by colder, blue shades. There is clear variation in average test scores, with the Southeast and Midwest outperforming poorer regions in the North and Northeast. Even within regions, however, there is wide heterogeneity. In particular, note that in the northern part of Brazil, high-performing municipalities neighbor low-performing ones. This indicates that despite spatial clustering, municipal factors can lead to variation in the quality of education.

Employment data on educational staff, including teachers and school principals, are extracted from the RAIS, SAEB and the School Census.⁹ The *Relatório Anual de Informações Sociais* (RAIS) is an annual census administered by the now Ministry of Finance to all formal employers in Brazil. Employers are mandated to accurately report data on employees, subject to fines for mis-

⁹RAIS data, along with additional Brazilian employment data, can be accessed [here](#).

reporting. Subnational governments, including municipalities, also report to the RAIS. The dataset therefore contains micro-level information on all municipal personnel, including when they were hired/fired, as well as wages, type of contract and education levels.

Figure ?? provides descriptive statistics on bureaucratic personnel in Brazil, segmented by department. Educational staff, in this case school principals and teachers, comprise approximately a third of municipal public sector jobs, totaling around 2 million in 2015. While comparable to administrative staff, this total significantly exceeds that for healthcare services. Focusing on turnover, we note that new hiring and dismissals in education staff is relatively high. While lower than that for administrative staff, new hires can represent over 10 percent of extant staff. The past two decades was a moment of rapid expansion of municipal staff, and hiring has far exceeded dismissals in that time period.

Figure 1.3: Proportion of public sector jobs by department. Note that I only keep the top 5 categorie

I propose three different specifications for measuring bureaucratic turnover. From the RAIS I obtain the percentage of teachers and principals who are dismissed and hired at any given year. Using school census data, it is possible to track individual teachers across time and schools. With that data I calculate a turnover index for school s , in municipality j at year t , based on an index proposed by ?. The number of teachers leaving and entering the school at a given year are summed and divided by the total number of teachers in the current and previous period.

$$\text{turnover}_{sjt} = \frac{\text{exit}_{sjt} + \text{entry}_{sjt}}{N_{sjt} + N_{sjt-1}}$$

[Insert more information and descriptive paragraph on turnover index here](#)

Information on mayor, city councilors and mayoral coalitions are made available by the Supreme Electoral Court (TSE), the national authority responsible for overseeing elections.¹⁰ In order to calculate the share of leg-

¹⁰Available [here](#).

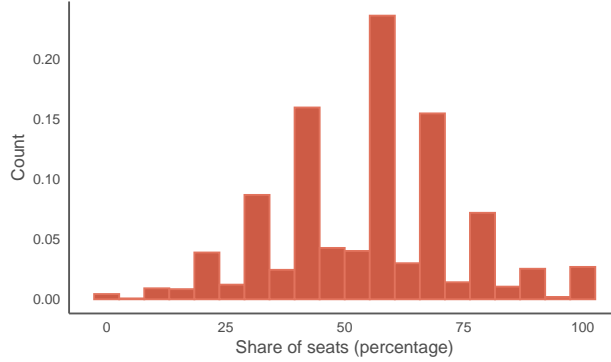


Figure 1.4: Distribution of share of legislative seats allied with the mayor during the electoral campaign for the electoral period of 2002-2016.

islatve seats held by the mayoral coalition, I match the partisanship of each city councilor to the mayoral electoral coalition. The distribution of share of coalition seats is depicted in figure 1.4. Note that although most city councils are controlled by the mayor, in over 40 percent of municipalities the mayoral coalition is a minority government.

Additional data has been collected to supplement the estimation exercise. Sociodemographic data comes from the National Institute for Statistics and Geography (IBGE), budgetary data from (FINBRA) and student test scores from Ceará (SPAEEC) are graciously provided by the Department of Education of Ceará.¹¹

1.4 Theory

The Setting

The government G and opposition O compete over legislative votes to enact their preferred policies.¹² There are two possible outcomes: a policy x favored by the government, and the status quo, denote as y , preferred by the opposition. In order to implement her policy the mayor must pass a simple majority

¹¹Respectively, data can be found here: (1)[IBGE](#), (2)[Finbra](#), (3)[Spaece](#).

¹²Note that for this model, I use the terms *mayor* and *government* interchangeably.

vote in the city council, comprised of an odd number N of legislators. The total amount of political resources available is W_G and W_O , which for the mayor includes public sector appointments.

Each city councilor is characterized by a publicly observed policy preference v_i for all $i \in N$, where $v_i > 0$ denotes that the mayor's proposal x is preferred by legislator i . Let $\mathbf{v} = (v_1, \dots, v_n)$ describe the preference profile for the city councilors. In this setting, v_i measures the degree to which an individual city councilor supports the mayor, with higher values of v denoting stronger support for the mayor and vice versa. Payoff are realized when city councilor i votes, independent of the outcome of the voting procedure. This sincere voting preference closes the possibility of general equilibria in which i 's voting behavior affect j .

We solve the game through backward induction. The timing of the game is as follows:

1. Government G offers a bribe schedule $a \in (a_1, \dots, a_n) \in \mathbb{R}_+^n$.
2. Opposition O observes the bribe schedule m and makes a counter-offer $b \in (b_1, \dots, b_n) \in \mathbb{R}_+^n$.
3. City councilors cast their votes at the end of bribing period.
4. Nature sums legislative votes, legislative outcome is decided and payoffs are realized.

Given a bribe schedule (a, b) , councilor i prefers to vote for the mayor's proposal x if $a_i + v_i > b_i$ and the status quo y otherwise. Since indifferent councilors vote for the status quo, the opposition needs to only match bribes from M , adjusting for individual preferences, i.e. $b_i = a_i + v_i$. For the mayor, she needs to construct the cheapest winning coalition in order to defeat the opposition.

Following Groseclose and Snyder (1996) and Banks (2000) we focus our analysis on the set of equilibria in which the mayor wins.¹³ In this context,

¹³Since strategies for both players are symmetrical, any set of equilibria in which the mayor loses can be modeled as cases in which the the opposition loses.

the amount of patronage resources W_G is sufficiently large relative to W_O and \mathbf{v} that the mayor's preferred policy x is implemented over y . Let $U(\mathbf{v}, W_O)$ denote the set of unbeatable patronage schedules for the mayor, and for any patronage schedule let $S(a) = \sum_{i=1}^n a_i$ be the total amount of patronage disbursed. The mayor then solves

$$\min_a \{S(a) : a \in U(\mathbf{v}, W_B)\} \quad (1.1)$$

Note that for any equilibrium strategy, it must be the case that mayor M uses a leveling schedule: every city councilor in her coalition C is equally expensive for the opposition O to bribe. More formally, for any $a \in \mathbb{R}_+^n$, let $C(a) = \{i \in N : a_i > 0\}$ denote the set of individuals who receive a bribe from from the government G . One can show that there is a bribe schedule a' such that for any $i, j \in C(a)$, $a'_i + v_i = a'_j + v_j$. The intuition is that the mayor has no incentive to make voters differentially expensive, because the opposition O will simply ignore the more expensive voters and target the least favorable members of the coalition. We refer to these strategies as leveling schedules.

We can characterize the set of equilibria in the game by introducing additional notation. Let $U^l(\mathbf{v}, W_O) \subseteq U(\mathbf{v}, W_O)$ denote the set of unbeatable leveling schedules. These are bribe schedules such that $a_i + v_i = a_j + v_j \equiv t(a)$. The bribe $a_i = t(a) - v_i$ is the sum of two terms. The first is the common "transfer" among all voters in $C(a)$, the second $(-v_i)$ is individual specific. The latter term makes voters indifferent between x and y absent any bribe from B ; the former represents the per capita amount necessary to make $C(a)$, together with any unbribed voters, unaffordable for B .

I impose the following two assumptions:

$$\begin{aligned} A1 : v_{(n+1)/2} &< 0 \\ A2 : v_1 &< 2W_B/(n+1) \end{aligned}$$

$A1$ implies that absent any bribes by A , y will defeat x . Therefore A must bribe at least one voter. $A2$ implies that A must bribe at least a majority of voters, otherwise B will have sufficient resources to bribe $(n+1)/2$ voters and win. $A2$

also implies that for all $a \in U^l(\mathbf{v}, W_B)$, it must be that $t(a) \geq 2W_B/(n+1)$, otherwise B can bribe a majority from $C(a)$ itself and win the vote.

These assumptions allow us to restrict our analysis to unbeatable monotonic leveling schedules, which we denote as U_m^l .¹⁴ We can simplify the total expenditure on patronage by the government, $S(a)$, as

$$S(a) = \sum_{i \in C(a)} a_i = k(a) \cdot t(a) - \sum_{i \leq k(a)} v_i$$

Note that the choice of $k(a)$ and $t(a)$ fully characterize any schedule $a \in U_m^l(\mathbf{v}, W_B)$. We can thus fully characterize the optimization problem of A in equation 1.1 as

$$\min_{k,t} k \cdot t - \sum_{i \leq k} v_i$$

subject to the constraint that the induced schedule $a \in U_m^l$. We can reformulate this as an unconstrained problem by noting the following. First, if $a(k, t, \mathbf{v})$ is unbeatable, it must be that $k \geq (n+1)/2$, so by A1 it must be that if $a_i(k, t, \mathbf{v}) = 0$, then $v_i < 0$. Therefore, B receives all non-bribed voters for free. For $a(k, t, \mathbf{v})$ to be unbeatable, then, it must be that B cannot afford the remaining $(n+1)/2 - (n-k) = k - (n-1)/2$ voters, or

$$t \cdot (k - (n-1)/2) \geq W_B$$

Solving this for equality yields the optimal transfer from A to members of $C(A) = \{1, \dots, k\}$, conditional on k :

$$t(k, W_B) = \frac{W_B}{k - (n-1)/2} \tag{1.2}$$

Defining minimal winning expenditures as

¹⁴A detailed explanation can be found in the appendix.

$$E(k, \mathbf{v}, W_B) = k \cdot t(k, W_B) - \sum_{i \leq k} v_i \quad (1.3)$$

we can state A 's problem as

$$\min_k \{E(k, \mathbf{v}, W_B) : k \in (n + 1/2), \dots, n\} \quad (1.4)$$

Denote the solution to expression 1.4 as $k^*(\mathbf{v}, W_B)$. This solution implicitly generates a solution to expression 1.1 through expression 1.3 and the induced bribe schedule above. Therefore, characterizing the optimal k^* is sufficient to fully characterize the optimal behavior of the mayor.

Results

First, characterize a solution for k^* . Because k is finite, calculus cannot be employed. Instead, we deploy a discrete version of these techniques. First let's define $\Delta(k) = E(k + 1) - E(k)$ as the difference in expenditure from adding another coalition member. Note that if $\Delta(k) \geq 0$ then A does not want to add another member to the coalition. Conversely, if $\Delta(k) < 0$, then A is strictly better off by adding the $k + 1$ th member of the coalition.

Next, suppose that $\Delta(k)$ is increasing in k . The following algorithm can then be used to identify k^* : if $\Delta((n + 1)/2) \geq 0$, then we know from $\Delta(k)$ increasing that A is better off by setting k^* to $(n + 1)/2$. If $\Delta((n + 1)/2) < 0$, then we know that k^* must be greater than $(n + 1)/2$, so we next solve for $\Delta((n + 3)/2)$, and so on.

We can therefore search for the optimal k^* with the following algorithm:

$$k^* = \begin{cases} (n + 1)/2 & \text{if } \Delta((n + 1)/2) \geq 0 \\ \max\{k : \Delta(k - 1) < 0\} & \text{otherwise} \end{cases} \quad (1.5)$$

We can also further characterize the change in minimum winning expenditures in equation 1.3 as

$$\Delta(k) = \left[\frac{(k+1)W_B}{k+1 - (n-1)/2} - \sum_{i \leq k+1} v_i \right] \quad (1.6)$$

$$= \frac{-W_B(n-1)}{2(k+1 - (n-1)/2)(k - (n-1)/2)} - v_{k+1} \quad (1.7)$$

$$\equiv T(k, W_B) - v_{k+1} \quad (1.8)$$

Using equation 1.5 and substituting in equation 1.7 we have the following.

Proposition 1. (a) $k^*(\mathbf{v}, W_B) = (n+1)/2$ if and only if $v_{(n+3)/2} \leq -W_B(n-1)/4$; (b) $k^*(\mathbf{v}, W_B) = n$ if and only if $v_n > -2W_B/(n+1)$.

Banks also identifies how the optimal coalition k^* respond to marginal changes in voter preference intensity. Given an arbitrary amount W_B and preference profile \mathbf{v}' , let $k' = k^*(\mathbf{v}', W_B)$. If $k' = (n+1)/2$, then we know that $k' \leq k^*(\mathbf{v}, W_B)$ for all \mathbf{v} , so suppose $k' > (n+1)/2$.

From equation 1.5 we infer that $\Delta(k' - 1, \mathbf{v}', W_B) < 0$, which from equations 1.7 and 1.8 is equivalent to $v'_k > T(k' - 1, W_B)$. Now suppose that the preference profile changes from \mathbf{v}' to \mathbf{v} , and $v_{k'}$ is such that $v_{k'} \geq v'_k$. Then, $v_{k'} > T(k' - 1, W_B)$, and hence $\Delta(k' - 1, \mathbf{v}, W_B) < 0$. But then from equation 1.5 it must be the case that $k^*(\mathbf{v}, W_B) \geq k'$. Therefore, the following holds:

Proposition 2. For all W_B , if \mathbf{v} and \mathbf{v}' are such that $v_{k'} \geq v'_{k'}$, where $k' = k^*(\mathbf{v}', W_B)$, then $k^*(\mathbf{v}, W_B) \geq k^*(\mathbf{v}', W_B)$

In words, if the preference intensity of the marginal bribed voter weakly increases, then the optimal coalition size also weakly increases. Substantively, the number of voters bribed by A weakly increases as the voter who receives the largest bribe finds A 's preferred alternative, x , more attractive. Similarly

Proposition 3. For all W_B , if \mathbf{v} and \mathbf{v}' are such that $v_{k'+1} \leq v'_{k'+1}$, where $k' = k^*(\mathbf{v}', W_B)$, then $k^*(\mathbf{v}, W_B) \leq k^*(\mathbf{v}', W_B)$

The “convexity” of E guarantees that local information is sufficient to generate comparative statistics regarding changes in preferences $\mathbf{v}' \rightarrow \mathbf{v}$. We can characterize the change in total expenditures as a result of a shift in voter preferences. Given two preferences \mathbf{v} and \mathbf{v}' , write \mathbf{v} and \mathbf{v}' if $v_i \geq v'_i$ for all $i \in N$. From equation 1.3 we have

$$\begin{aligned} E(k, \mathbf{v}, W_B) - E(k, \mathbf{v}', W_B) &= \\ &= k \cdot t(k, W_B) - \sum_{i \leq k} v_i - \left[k \cdot t(k, W_B) - \sum_{i \leq k} v'_i \right] \\ &= \sum_{i \leq k} (v'_i - v_i) \end{aligned}$$

Since $v'_i - v_i \leq 0$, the difference in expenditure between moving from a favorable to a less favorable legislature is always non-positive, i.e. the government has to spend less resources to pass her preferred policy. This holds despite the fact that when these preferences shift there is an increase in the overall size of the coalition. This result has a similar flavor to ?, who motivate their model by stating that it may be optimal to increase the size of the coalition (instead of buying a simple majority) because doing so overall can lead to a reduction in the amount of expenditures by the vote-buyer.

Discussion

Enacting policy requires the exchange of political currency for votes. Whether it be in presidential coalitionism, or in the local city council politics, mayors who wish to govern have to engage in transactions with the legislature. What I showed in this section was that political misalignment between the government and the legislature can in fact be counterproductive: more patronage occurs, leading to worse public service outcomes.

The model also highlights a key aspect of clientelism that is often neglected electoral accountability models: voters have a limited voice. Ultimately, the exchanges which occur between the legislature and the mayor have little to

do with the voter at the end of the pipeline, and more to do with the city councilors. The first order requirement for the government is to ensure that it has enough legislative votes in order to enact the very policies that the voter may or may not desire. This transactional cost is not illegal: rather, it is necessary for democratic relations between different branches of government.

In the next section, I test whether the empirical implication of the model is correct: does more patronage occur in municipalities with greater political misalignment between the mayors and city councilors. I test additional implications of the model, including whether shifts in the resources controlled by the opposition can affect the government’s patronage strategy.

1.5 Empirical results

Model specification

This study presents two sets of estimations. The first one identifies the effect of bureaucratic turnover on student learning, verifying the hypothesis that teacher and school principal turnover has a negative effect on the quality of municipal education. To do so, I leverage the test scores made available by *SAEB* and *SPAECE* to estimate the effect of turnover on student test scores. To avoid interference, individual test scores are aggregated at the classroom level, for each 5th and 9th grade of each school contained in the sample. The outcome of interest is thus the average test scores for all students in the same classroom (*SAEB*) or same grade (*SPAECE*), for a given school.

For the first estimation, I test the hypothesis that mayors who control less seats in the city council engage in more patronage. To do so I leverage employment data from *RAIS* and school census data to measure teacher turnover rates. I deploy three sets of models. For the second and third estimation I turn to the micro, bureaucrat-level and then to aggregated turnover at the municipal level. The first model is a logistic regression where the outcome of interest is whether a particular teacher or school principal is hired/fired for any given year. The second model is a linear model with fixed effects, where

the outcome of interest is the proportion of educational staff hired/fired for a municipality at any given year. For the second set of estimations, the main specification is:

$$\text{turnover}_{jt} = \gamma \text{coalition seats}_{jst} + \mu P_{jt} + \zeta W_{jt} + \alpha_k + \delta_t + \epsilon_{jt}$$

The share of coalition seats held by the mayoral coalition is the treatment in this set-up. We are interested in γ , the change in bureaucratic turnover associated with variation in the share of legislative seats held by the mayoral coalition. Municipal characteristics W_{jt} are similar to the ones used for the first set of estimation. I add political variables to the estimation in order to control for differences in mayor partisanship, incumbency status, and individual characteristics of the mayor: professional background, education, and age.

To estimate downstream effects of turnover on education, I employ a hierarchical linear model to estimate the effect of teacher turnover on average test scores for grade i at school s , in municipality j and year t . This estimation strategy has been used in multiple studies to analyze educational outcomes, due to its natural multi-level setting (classroom, school and municipality) as well as ability to incorporate covariates at each level of the estimation (??) Let grade_{isjt} be a dummy variable equal to 1 if the classroom is in grade 9 and 0 otherwise. The main specification is as follows:

$$\begin{aligned} \text{test score}_{isjt} = & \beta_1 \text{turnover}_{isjt} + \beta_2 \text{grade}_{isjt} + \beta_3 \text{turnover}_{isjt} \times \text{grade}_{isjt} + \\ & \psi X_{isjt} + \phi V_{sjt} + \zeta W_{jt} + \alpha_k + \delta_t + \epsilon_{isjt} \end{aligned}$$

We are interested in β_1 and β_3 , the change in test scores associated with teacher or school principal turnover. In this set-up, β_3 is the additional effect of staff turnover on test scores if students are in 9th grade. The first level is the grade, with associated characteristics X_{isjt} at the grade level: among others, percentage of students who have not graduated last year and share of children with a fridge in their house. The second is the school s , with characteristics

V_{sjt} , such as access to internet or the presence of a cafeteria for students. The third level is the municipality j , including municipal sociodemographic characteristics (W_{jt}) such as population size and median wages, as well as per pupil budgetary expenditures on education. Finally, I include state (α_{kj}) and year (δ_t) fixed effects to account for unobserved time-invariant state characteristics and annual seasonality.

Results

I first turn to testing the main proposition of this paper: that turnover in educational staff responds to the degree of political alignment between the mayor and politician. In line with theory, there is strong evidence that mayors who are electoral allies with more (less) seats in the city council resort to less (more) patronage. This is robust to a set of specifications, including state-year fixed effects and alternative measures of turnover in educational staff.

Table 1.1 presents the results of the estimation. Model 1 regresses turnover index on coalition share, where the unit of analysis is a grade-level per school. Models 2 and 3 estimate the effect of the share of legislative seats on hiring of new bureaucrats aggregated at the municipal level. The third set of models (5 and 6) estimate the change probability of new bureaucratic hires at the individual level. All models suggest that an increase in the executive share of legislative seats decrease turnover.

	Outcome					
	Turnover index (municipal)		Hires (municipal)		Hires (individual)	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of legislative seats	−0.026*** (0.008)	−0.053*** (0.007)	−0.028*** (0.006)	−0.030*** (0.006)	−0.040*** (0.002)	−0.047*** (0.003)
School principal			−0.252*** (0.015)	−0.245*** (0.017)	−0.222*** (0.013)	−0.386*** (0.015)
Executive share of seats X School principal			−0.014 (0.011)	−0.022** (0.010)	−0.016 (0.013)	−0.128*** (0.014)
Controls		✓		✓		✓
State and year FE	✓	✓	✓	✓	✓	✓
Observations	2,591,629	1,632,748	61,983	61,983	1,303,399	1,303,399
R ²	0.027	0.045	0.184	0.296		

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 1.1: **Executive coalitions and staff turnover.** An increase in the share of legislative seats held by the mayoral coalition decrease the amount of turnover for teachers and school principals, including hires or dismissals. Models 1 and 2 present results on the turnover index at the school level. Models 2 and 3 are aggregate hiring rates at the municipal level. Models 5 and 6 are logistic regressions at the individual, bureaucrat level. where the outcome of interest is the proportion of staff either hired or dismissed at a given year. Models 1, 3, and 5 include year and state fixed effects.

For visual intuition, I present the estimated coefficients for the models with controls – models 2, 4, and 6 – in context with other coefficients for demographic/contextual variables. Note that while the share of allied seats is a precise predictor of the degree of turnover in educational staff, other important factors such as the level of economic development – municipal median wage – are less informative. Literacy rate, although unprecise, is negatively associated with staff turnover, suggesting that a more educated electorate may exert some pressure to retain teachers and school principals.



Figure 1.5: **Political alignment and staff turnover.** A visual representation of results presented in table 1.1. All models include year and state fixed effects.

For visual intuition, I present the predicted values for models 4 and 5, respectively the share of hired and dismissed bureaucrats against the share of executive seats in the city council. The reduction in new hires is more pronounced for school principals than teachers, suggesting that exposure to patronage is more concentrated in the leadership positions at the school level. Overall, these results suggest that weaker executive control over the legislature increases patronage, with potentially negative effects for public service delivery in municipalities in Brazil.

Finally, I show that the executive-legislative bargain extends to second-term mayors. I subset the data used for the above analysis to only mayors who are reelected, and re-run the previous analyses with the same specification. I find that the results are similar in magnitude and precision to those presented above. This finding suggests that patronage is not motivated solely

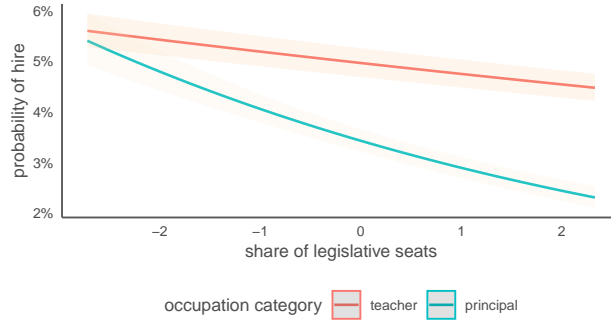


Figure 1.6: **Differential reductions in patronage in teachers and school principals.** Predicted values for bureaucratic turnover plotted against the proportion of educational staff either hired or fired for any given year against the share of seats controlled by the executive.

by reelection concerns. Rather, patronage achieves an important policy goal for incumbent mayors who seek to implement their preferred policy, regardless of whether they are just initiating their mandate or concluding it.

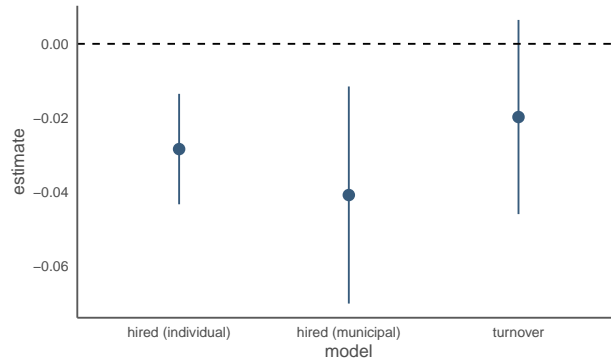


Figure 1.7: **Patronage continues into the second term.** There is strong evidence that executive-legislative bargains continue into the mayor's second term. All models include a full set of controls, as well as year and state fixed effects.

Moving on to characterizing the downstream effects of patronage, I find that teacher and school principal turnover have significant, negative effects on average test scores. These results are robust to alternative specifications of turnover, as well as the use of different quality metrics (SAEB, SPAECE) in

our estimations. Table [1.2](#) presents the results of the estimation on SAEB and SPAECE test scores. We present two specifications for turnover. Turnover index measures the amount of turnover in teachers at the school level. Work experience for teachers and school principals serve as an alternative measure of turnover.

	Student learning					
	SAEB test score					
	(1)	(2)	(3)	(4)	(5)	(6)
Turnover index	−0.013*** (0.001)	−0.010*** (0.002)			−0.001 (0.001)	−0.008*** (0.002)
Teacher experience (2-10 years)			−0.126*** (0.007)	−0.048*** (0.012)		
Teacher experience (2 years)			−0.208*** (0.007)	−0.134*** (0.012)		
School principal experience (2-10 years)			−0.013*** (0.003)	0.019*** (0.006)		
School principal experience (2 years)			−0.132*** (0.003)	−0.064*** (0.007)		
School principal experience (2-10 years) × 9th grade			−0.022 (0.014)	−0.046* (0.024)		
School principal experience (2 years) × 9th grade			−0.029** (0.014)	−0.057** (0.024)		
Teacher experience (2-10 years) × 9th grade			0.016** (0.007)	0.021* (0.012)		
Teacher experience (2 years) × 9th grade			0.042*** (0.007)	0.041*** (0.012)		
Controls	−	✓	−	✓	−	✓
Observations	602,836	146,453	811,815	244,945	224,794	142,565

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 1.2: **Bureaucratic turnover and student learning** Teacher and school principal turnover have a negative effect on student learning. Models 1 and 2 present results for teacher turnover index constructed at the school level. Models 3 and 4 estimate the effect of new teachers and school principals entering the school (less than two years). All models include year and state fixed effects.

I also present the coefficients for the regression exercise above in figure 1.8. Note that the results are consistent across the board, providing strong evidence that staff turnover has negative downstream consequences for student learning.

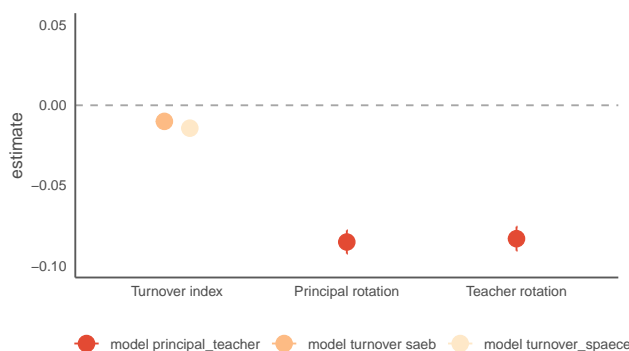


Figure 1.8: **Bureaucratic turnover and student learning** Teacher and school principal turnover have a negative effect on student learning, across different sets of exams (*SAEB* and *SPAECE*). All models include a full set of controls, as well as year and state fixed effects.

Finally, I present some evidence of differential returns to patronage, in particular for city councilors. For each position (mayor, city councilor) I estimate the probability of being reelected conditional on the amount of patronage occurred in the first term manda While mayors themselves do not directly benefit from greater patronage, city councilors who are not members of the electoral base of the mayor seem to be negatively affected by patronage appointments into education. The negative consequences of patronage on student learning, however, do not bite, as nayors who deliver a worse quality of education are no less likely to be reelected in the next term.¹⁵

As a final note, this set of estimations are based on observational data and therefore suffer from well-known concerns of endogeneity. It is in these circumstances that validating causal mechanisms through fieldwork and careful data analysis can increase the validity of a causal claim. In-depth interviews with local actors involved in managing education, as well as the use of dif-

¹⁵See appendix 1.6.2.

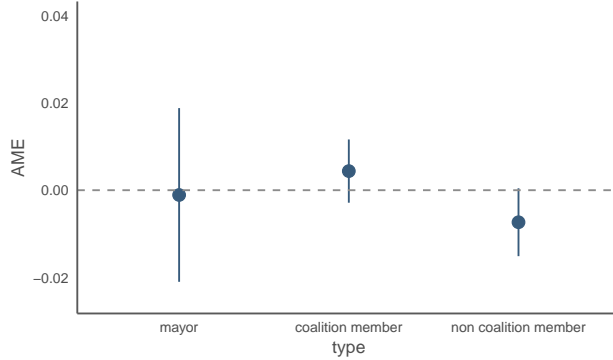


Figure 1.9: **Differential returns to patronage.** Mayors who engage in patronage do not directly benefit from it. Instead, city councilors in the mayoral ally base seem to be benefiting from it at the expense of non-allied city councilors.

ferent measurements for both educational staff turnover and student learning, provides strong evidence that staff turnover has a negative impact on student learning. Bureaucratic turnover stems from political considerations.

1.6 Conclusion

Improving the quality of education received by children across the world remains a challenge. This paper proposes an analytical framework and estimation strategy to understand the decision-making process behind the administration of educational services in Brazil. In a decentralized context, sub-national political actors have a direct say on how educational services are managed, with profound implications for the quality of public services delivered to citizens. These actors interact with bureaucracies and other local elites in complex ways that are only beginning to be mapped.

In this study I theorize and demonstrate that staff turnover stems from the executive's need to garner support from legislators in the city council. This process of coalition building is consolidated through employment offers to city councilors and their constituencies. As the share of seats held by the executive coalition decreases, the costlier it becomes to coopt legislators to support the executive. As a result, the amount of patronage we observe should increase.

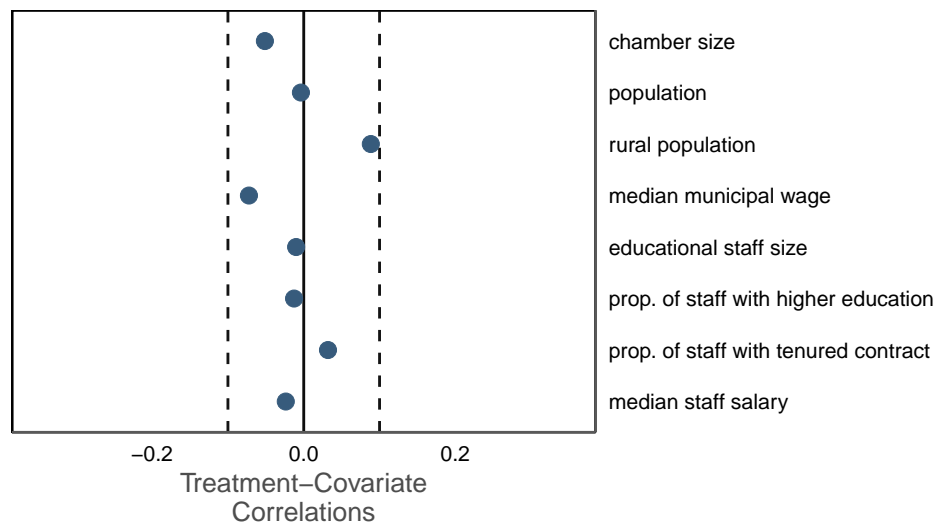
That is precisely what the data indicates, with teacher turnover increasing in schools, as well as increased hiring and dismissals of teachers and school principals.

This bureaucratic turnover has important consequences for the quality of education delivered in municipalities. I find that turnover has a negative effect on student learning, across a set of specifications for turnover and different evaluation metrics for student learning. The evidence therefore seems to point out that mayors with a weaker hold on the city council resort to greater patronage, with negative consequences for student learning. This set of findings contribute to an emergent literature on the ambiguous consequences of stronger competition in weakly institutionalized contexts (?).

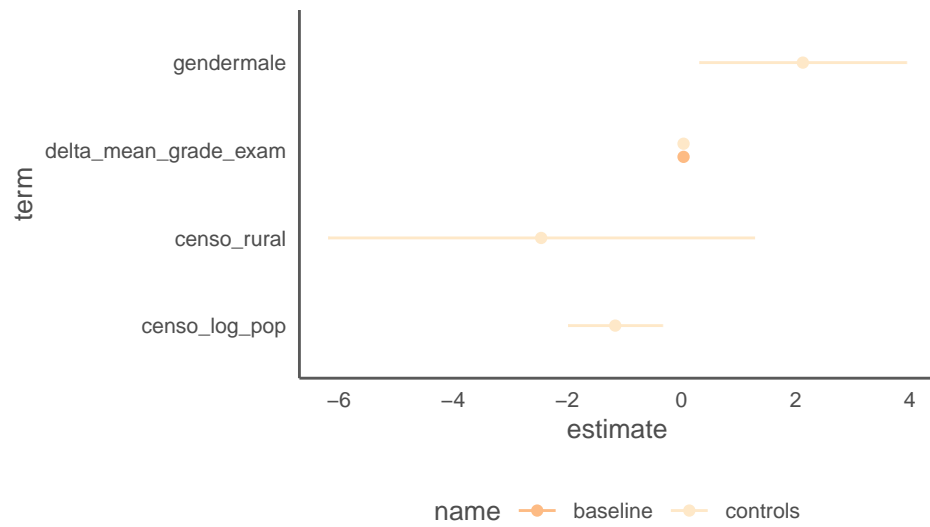
Future research on patronage and public service delivery would benefit from a clearer treatment of the institutional context in which local political actors operate. Important insights have been derived on executive leaders, but these actors seldom govern alone. Incorporating other local elites paints a more complex and accurate understanding of the strategic considerations taking place in the political management of education, and public services more broadly.

Appendix

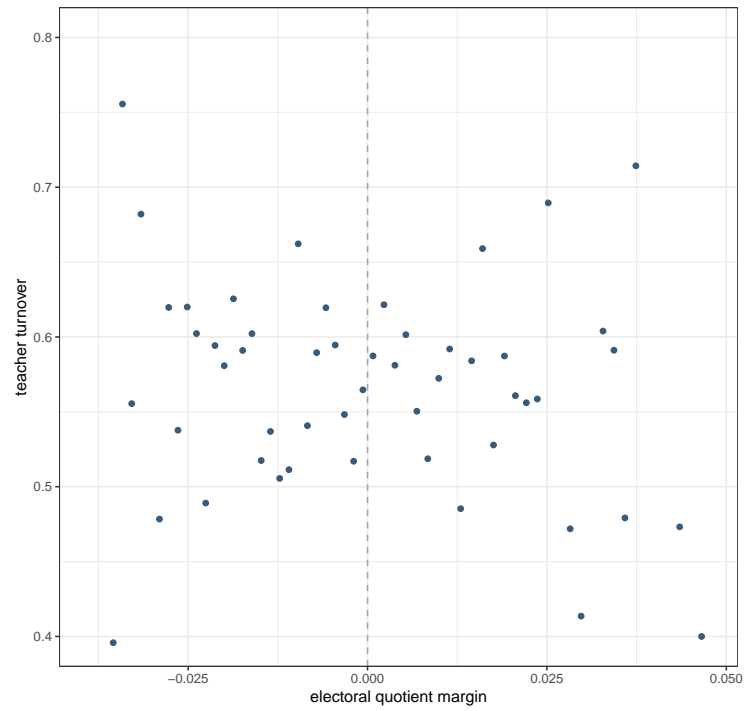
1.6.1 Covariate balance



1.6.2 Accountability of mayors



1.6.3 Regression Discontinuity Design



Chapter 2

Corruption Audit

Under which conditions do anti-corruption policies effectively reduce bureaucratic corruption? Previous studies find that anti-corruption audits are effective in disciplining politicians, but their impact on bureaucrats is unclear. We leverage 10 years of randomized audits and the careers of 275 thousand Brazilian municipal officials. We find that even when strong evidence of corruption is found, audits do not have observable implications for bureaucratic careers, such as dismissals or departures. To investigate whether audits trigger unobservable reductions in corruption or have long-term disciplining effects, we propose a model of corruption with career concerns that we estimate structurally. We rule out that audits have unobservable reductions in corruption, and our results are consistent with either large disciplining effects, or limited effectiveness. We identify strong complementarities among the program's components, suggesting that multi-pronged approaches combining increases in the frequency of audits with tougher sanctions are most effective at reducing corruption.

2.1 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. ???). In response, states have adopted policies designed to detect and punish corruption by public officials. (?). Audits have grown increasingly popular, with previous studies documenting their effectiveness in reducing corruption and sanctioning politicians (???). 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 that 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 (??). Bureaucrats, on the other hand, do not face electoral sanctions, and have long-term careers that are open to transitions to the private 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.

This paper asks the following question: under which conditions do audits effectively reduce bureaucratic corruption? Our empirical strategy focuses on Brazil, a democracy in the developing world with median levels of corruptions.¹ We combine two main sources of administrative data, building a long panel: 10 years, from 2006 to 2015. First, detailed corruption data from a well-known municipal audits program (studied by e.g. ???) that is fully randomized and has been widely held to be reliable.² Second, a panel capturing the universe

¹Brazil ranks 106 out of 180 countries in the Transparency International Corruption Perceptions Index.

²Auditors are widely considered as professional and not subject to political pressures by either mayors or the federal government. They 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 (??)

of formal-sector workers, out of which we focus on 275 thousand municipal bureaucrats. This data allows us to track the effectiveness of audits and their impacts on the careers of bureaucrats, allowing for potential heterogeneous effects across municipalities.

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.³

These initial results thus 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 an audit. It may also be that ex-post effects are unobservable, as bureaucrats could 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 that audits may potentially have. In the model, a forward-looking bureaucrat may choose to do her job, engage in corruption, or permanently depart to the private sector. Engaging in corruption implies a tradeoff between increasing one's salary through illegal rents from corruption, and the risk of getting dismissed in the event of an audit. This model has the additional benefit of breaking down this complex policy into a set of mechanisms: (1) audit *frequency* (2) *monitoring*, the auditors' capacity to detect and dismiss corrupt bureaucrats, and (3) *clean-up* of the bureaucracy that temporarily reduce the size of the rents and could stem from those improve-

³The CGU has conducted a set of crackdown operations – *operações especiais* – which has led to the arrest of bureaucrats found to be engaging in corruption, see articles.

ments in managerial practices we identified empirically. 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.

Acknowledging that bureaucrats are forward-looking, we finally estimate a dynamic discrete choice (DDC) model that precises the ways in which bureaucrats' response to anti-corruption audits differ from those of politicians. The model reduces our theoretical model by collapsing the unobserved actions of stealing and not stealing into the observed action of remaining in the bureaucracy. The model uses observed audit results as a proxy for the total amount stolen in the municipality. It decomposes one's payoff from remaining in the bureaucracy into (1) an average ex-post effect of audits, (2) an average, time-invariant payoff from corruption that varies by a municipality's observed levels of corruption, and (3) an intercept that captures an average time- and municipality-invariant payoff from public employment, thus capturing either a form of public sector motivation (?) or municipality-invariant benefits from corruption. The estimates show that bureaucrats decision to remain in the bureaucracy is largely unaffected by audit events and municipal variation in levels of corruption, and owes instead to this large time- and municipality-invariant payoff. Being unable to further decompose this intercept into illegal rents and public sector motivation, we are unable to say whether the program is ineffective (i.e. the intercept mostly captures illegal rents, and observed low probabilities of dismissal indicate weak punishment), or whether it is very effective (i.e. the intercept mostly captures public sector motivation, and observed low probabilities of dismissal indicate low corruption).

We finally use our estimates from the DDC model to calibrate a computational exercise that investigates how to redesign audits to improve their effectiveness, assuming that the program was not effective. We manipulate each channel and compare them to a baseline counterfactual in which audits never took place. In isolation, increasing the strength of the monitoring associated with audits is the most effective deterrent to corruption, consistent with earlier findings (???). However, pulling all the levers simultaneously provides the greatest effect: a 25% increase in all channels reduces corruption more than

meeting corruption with certain dismissal. 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 (??). While previous studies have focused on how politicians engage in corruption (???), we focus on bureaucrats’ decision, showing that the same policy may have different outcomes on politicians and bureaucrats. While ? show that politicians’ time in office is strongly conditioned by whether audits reveal corruption to voters, time in office, we find much more limited effects on bureaucrats’ careers and corrupt behavior: audits have minuscule ex-post effects, and their consequences vary little according to the results of the audit. As such, bureaucrats’ propensity to stay in office above and beyond what can be explained by the public-private wage differential owes either to public sector motivation or to time- and municipality-invariant rents from corruption.

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 (??), but often failed to break down these complex policy bundles into their constitutive components (?). At the micro-level, previous studies have shown, using experimental or quasi-experimental settings, that focalized policy interventions can improve bureaucratic quality (e.g. ??) but focused on improving a single component of a complex reform. Our study highlights the difficulties of evaluating complex, national-level policies through experimental methods when those feature long-term effects. It also breaks down one such policies into a set of simpler components using computational approaches, highlighting how we can exploit their complementarity to enhance the policy’s effectiveness.

The paper is structured as follows. Section 3.3 provides the institutional context and descriptive summary of the data for municipal governments, bureaucracies and anti-corruption audits in Brazil. Section 2.3 discusses our empirical strategy and reduced-form results, highlighting the short-term, observable effects of audits. Section 2.4 outlines the theoretical model that guides

our analysis of corruption in bureaucratic careers, while section 2.5 presents the results of our DDC model and counterfactuals. Section 3.5 concludes by discussing policy implications, and how our findings may generalize to other settings.

2.2 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 2.1.

2.2.1 Municipal bureaucracies and management

Context

Brazil is a decentralized democracy, composed of over 5 thousand municipalities. Each municipality is governed by an executive (mayor) and a legislative (city council) branch, both elected simultaneously at four year intervals. With democratization in 1988, much of the social policy responsibilities were delegated to municipalities (?). As a result, the 1990s and 2000s saw a rapid expansion of local bureaucracies to manage and deliver these public goods and services services (?). Along with these responsibilities, there was an increase in the amount of public resources allocated to the local level (?), generating new opportunities for public officials to engage in corruption.

Decisions over appointment and dismissal of bureaucrats is under the exclusive jurisdiction of the municipal government. Currently, over half of Brazilian bureaucrats are hired and paid by municipalities, but there is no established civil service system that governs these bureaucratic careers. Hiring practices are not always meritocratic, as mayors enjoy wide discretion that they can use for patronage appointments and spoils distribution (??). Due to the absence of a career service system, the boundaries between the private and public sectors are porous and every year around 20 percent of high-level bureaucrats leave

for the private sector.⁴ In a similar fashion, managerial practices are under municipal jurisdiction, leading to a wide variation in the extent and types of administrative practices implemented locally.

Data

Employment data on municipal bureaucrats is gathered by the *Relação Anual de Informações 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 education⁵ 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 three outcomes. Directly related to the theory (section 2.4), we consider departures and dismissals from the bureaucracy. We also consider another related concept: the quality of management practices, which may impact the size of rents from corruption and is an important theoretical mechanism in our model. Our analysis focuses on high-level bureaucrats, who are responsible for the top-level decisions in the management of public resources, and enjoy a direct connection to politicians.⁶ 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 2.7.4 for details). Following ??, the index is a simple count of good management practices implemented in the municipality,

⁴More details on our classification of high-level bureaucrats below.

⁵This dataset has been widely used in other studies (e.g. ??).

⁶As 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.

	Value
Dependent variable	
Pct. departures	30%
Pct. dismissals	22%
Management index	0.405
Corruption	
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 2.1: **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.

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;⁷ second, the set of practices measured by the survey changes from year to year; and third, the survey uses self-reported data, open-

⁷Data is not available for the years 2008, 2010 and 2014-5.

ing the way for misreporting. We address these limitations by verifying that (1) good management correlates with low corruption (Appendix 2.7.4), and (2) that our results are robust to including only those items that appear most frequently in the surveys (Appendix 2.8.7).

Table 2.1 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 2.7.5 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 2.5.1 for a discussion). Figure 2.1 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.

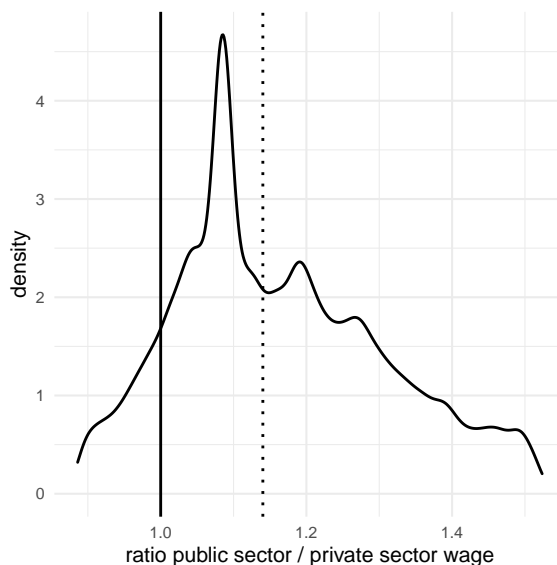


Figure 2.1: **Distribution of the ratio public/private sector wage.** The dotted bar represents the median.

2.2.2 Federal transfers and anti-corruption audits

Context

Brazilian municipalities rely to a large extent on federal transfers to fund their operations and payroll (?). For some, these constitute over 90 percent of their local budget (?). To reduce misuse of these public resources, 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.⁸ Each year, a state-level lottery selects at random two municipalities per state. As such, a municipality has a 2% chance of being audited on any given year and may be audited more than once over our period. Shortly after the lottery, teams of ten to fifteen auditors are sent to municipalities with a 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 2.1).

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 auditing of local budgets. However, audits may have direct 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. These special ops (*operações especiais*) have led to multiple arrests of public servants found to be engaging in corruption.⁹ Similarly, the local city council may use these findings as a basis to impeach

⁸For a description of the program, see <https://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos>.

⁹See <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/acoes-investigativas/operacoes-especiais>, for a journalistic coverage, see <https://g1.globo.com/pb/paraiba/noticia/2020/09/02/pf-deflagra-4a-fase-da-operacao-famintos-que-investiga-fraudes-na-merenda-em-campina-grande.ghtml>.

mayors.¹⁰

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 2.1). We define corruption as the *intentional* abuse of public office for private gain. While our reading of reports suggest that serious faults tend to pick up corruption, we also found, in line with ? that the difference between intermediate and serious faults is quite blurry, as some intermediate faults also feature cases in which intentionality seems apparent, such as instances over-invoicing, shadow employees, and rigged public procurement (see Appendix 2.7.3 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 ?, 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, for a total of 6 metrics. While many of those metrics are highly correlated, the metrics that are normalized by amount stand out (Figure 2.11a in Appendix 2.7.3). 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, we de-mean irregularity counts by lottery to account for potential variation in

¹⁰See <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.ghml>.

auditing standards over time, and classify each municipality’s corruption level by tercile (for more details, see Appendix 2.7.3).

2.2.3 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).

2.3 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.

We find that audits have no observable impact on career outcomes; in other words, they fail to trigger waves of departures/dismissals. This finding is surprising, given that this program has been shown to effectively remove corrupt politicians from office (??), and deter them from engaging in corruption. As such, we then focus on those specific instances, and investigate whether audits trigger career interruptions for those municipalities in which we know audits have a high chance of removing the mayor from office. We fail to find evidence of such effects. 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.

2.3.1 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.¹¹ For our estimation, we construct a trichotomous variable c_j 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 τ_{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 $t_j \in \{\underline{t}, \dots, \bar{t}\}$. For every municipality j , we observe a sequence $(\tau_{j\underline{t}}, \dots, \tau_{j\bar{t}})$ such that $\tau_{jt} = 0$ for

¹¹If municipality j has been audited twice, we construct that variable using the results of the first audit.

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:

$$y_{jst} = \alpha_t + \alpha_s + \beta_2 \tau_{jt} + \sum_{k=1}^2 \beta_{1k} 1\{c_j = k\} + \beta_{3k} \tau_{jt} 1\{c_j = k\} + \beta_4 x'_j + \epsilon_{jst}, \quad (2.1)$$

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, ϵ_{jst} is an error term.

The model in equation 2.1 identifies the effect of an audit on the municipality-level outcome y_{jst} . Parameter β_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 α_s and make within-year comparisons using a year fixed effect α_t . Additionally, we cluster standard errors at the municipality level.

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 2.1 and parametrize the treatment effect flexibly. We turn our treatment indicator τ_{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 his first term, which largely correspond to patronage appointments. If a mayor in his first term – who therefore may run for reelection – is found corrupt, he may have an incentive to dismiss his clients in order to wither down future electoral sanctions. Should he lose the elections, his 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 2.8). 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 2.8.1), tenured and untenured bureaucrats (Appendix 2.8.3), as well as to the most important high bureaucrats (i.e. municipal secretaries, Appendix 2.8.2). 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. All other robustness checks also consider hires. We invite the reader to consult the relevant Appendix [2.8.4](#).

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 municipalities that were audited early match municipalities that were audited later on (Appendix [2.8.5](#)). We show that our results are robust to the four corruption metrics outlined in section [2.2.2](#). We show that results are robust to using a measure of personnel turnover that uses percentages instead of log counts (Appendix [2.8.6](#)), and measures of management that use only the items that occur most frequently (Appendix [2.8.7](#)). 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 [2.8.8](#)). Finally, we consider individual-level outcomes instead of municipal-level aggregates and show robustness to such disaggregation (Appendix [2.8.9](#)).

2.3.2 Results

Table [2.2](#) 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, with audits increasing the quality of management by 2.2 percentage points; that is, a 5% increase relative to the sample mean. Figure [2.2](#) shows that results extend to all the corruption metrics we consider.

	<i>Dependent variable:</i>					
	No. of departures (log)		No. of dismissals (log)		Management index	
	(1)	(2)	(3)	(4)	(5)	(6)
Audited (β_2)	0.134*	0.068	-0.075	-0.111	0.011	0.0003
	(0.078)	(0.068)	(0.087)	(0.073)	(0.009)	(0.007)
Moderate corruption	0.152	0.123	0.076	0.090	0.006	0.004
	(0.107)	(0.088)	(0.111)	(0.090)	(0.011)	(0.009)
High corruption	0.125	0.039	0.053	0.070	-0.008	-0.020*
	(0.113)	(0.104)	(0.117)	(0.108)	(0.012)	(0.011)
Audited \times Moderate corruption (β_{31})	-0.134	-0.104	-0.001	0.039	-0.016	-0.009
	(0.108)	(0.095)	(0.121)	(0.102)	(0.012)	(0.010)
Audited \times High corruption (β_{32})	0.008	-0.020	0.103	0.115	0.023*	0.022**
	(0.109)	(0.094)	(0.109)	(0.095)	(0.012)	(0.010)
Controls	-	✓	-	✓	-	✓
$\beta_2 + \beta_{31}$	0	-0.036	-0.076	-0.072	-0.006	-0.008
$\beta_2 + \beta_{32}$	0.142	0.048	0.028	0.004	0.033***	0.022***
Observations	5,053	5,053	5,053	5,053	5,053	5,053
R ²	0.148	0.300	0.132	0.269	0.316	0.441

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 2.2: **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 $\beta_2 + \beta = 0$. All models include year and state fixed effects, and measure corruption using all faults. Standard errors clustered at the municipality level. See section 2.3.1 for details about controls.

Analyzing the effects of audits over time (Figure 2.3) confirms that audits have no discernible effects on career interruptions (top two panels): for all three types of municipalities, departure and dismissal 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.

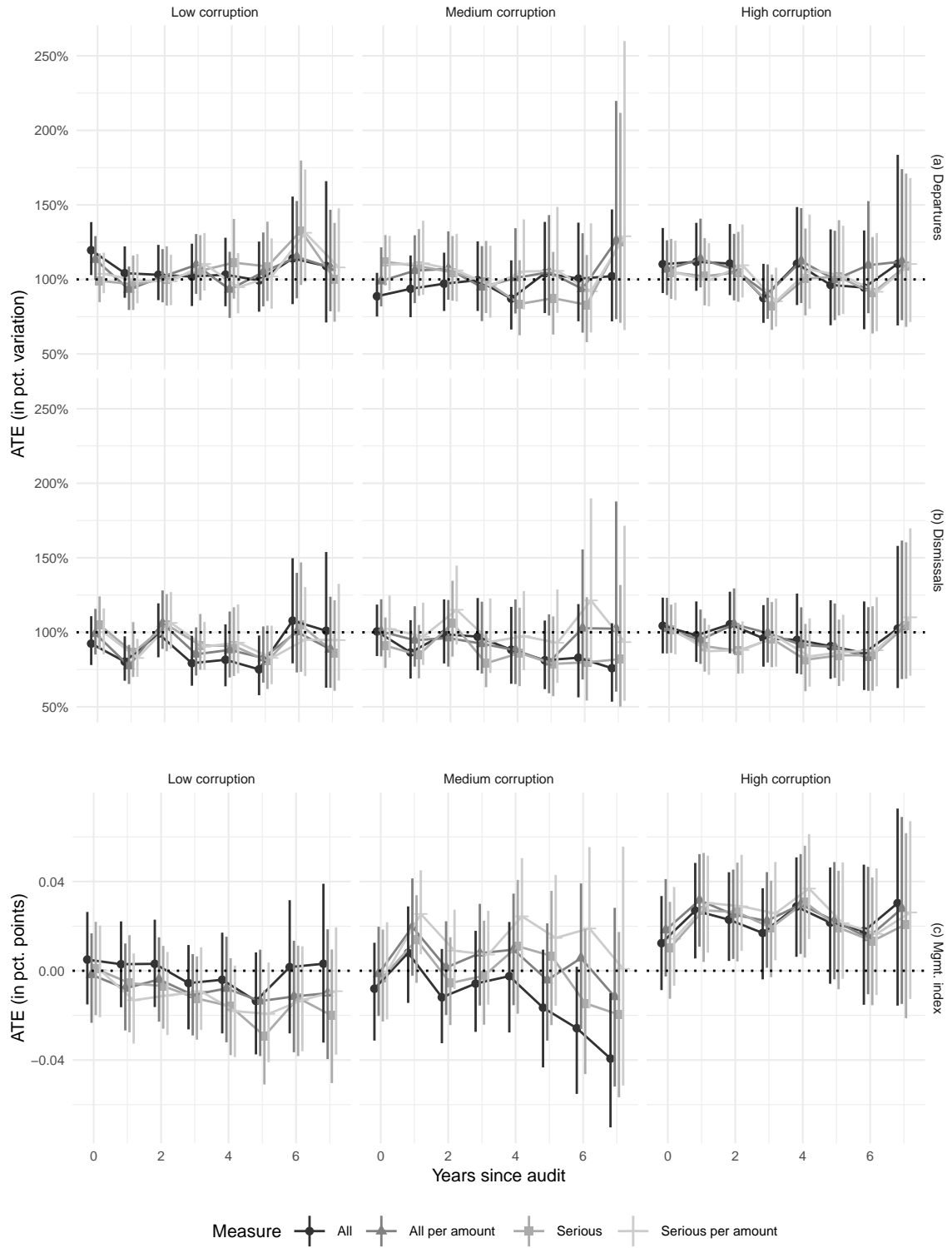


Figure 2.3: **Treatment effect over time.** The y-axis represents the average marginal effect of some number of years⁵² after the audit on the row outcome. It is measured in percentage points for the top and bottom panels, and in percentage of variation for the middle panel. Bars are 95 percent confidence intervals clustered at the municipality level. All specifications include the controls discussed in section 2.3.1. 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 statistically significant at the 5 percent level.

We finally show that electoral accountability mechanisms do not trickle down to bureaucrats. Figure 2.4 reports the effects of audits during a mayor’s first term, using all faults as a measure of corruption. The figure uses the first year 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 2.8.10, we report similar effects for the subsequent mayor, and show that the findings extend to the remaining three corruption metrics.

2.3.3 Discussion

Overall, we find that audits not trigger waves of departures or dismissals, but lead to small, temporary improvements in managerial practices. At first glance, these initial results might lead us to conclude that audits have no effect on bureaucratic careers and limited effectiveness on corruption. However, two alternative explanations are consistent with these findings and reductions in corruption. First, 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.¹² 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 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.

2.4 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. This model aims at exhausting all potential channels through which audits may reduce corruption, in order

¹²This approach has been explored for this program, with ? leveraging a one-time experiment in the randomization. Results suggest that these audits have indeed no significant disciplining effects, but are severely constrained by lack of power.

to ascertain that we are not missing any.

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 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 moderate 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 deterring well-paid bureaucrats from stealing, or triggering preemptive departures from the bureaucracy.¹³

¹³This latter result, although not apparent in the simple cases we analyze in this section,

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 2.7.1. 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.

2.4.1 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 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 .¹⁴ Additionally, if the agent is audited and stole in the previous time period,

becomes apparent when introducing a baseline probability of getting dismissed $q_0 \neq 0$.

¹⁴Although 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.

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 $\bar{w} > 0$. However, when the agent is punished, she undergoes private-sector *wage penalty* $k \in [0, \bar{w}]$. Normalizing the private-sector payoff to 0, the agent's payoff at period t ; that is, $u : \mathcal{A} \times \mathcal{S} \rightarrow \mathbb{R}$, writes

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

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 $\pi : \mathcal{S} \rightarrow \mathcal{A}$, which maps states s_t to actions a_t .¹⁵ With Π denoting the set of possible policies, our agent solves the following dynamic programming problem

¹⁵Since payoffs u are bounded and stationary, and transition probabilities are also stationary, and \mathcal{S} and \mathcal{A} are finite, a stationary policy $\pi : \mathcal{S} \rightarrow \mathcal{A}$ is optimal.

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

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.¹⁶ 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:

$$w < w + b < \bar{w} \tag{2.2}$$

$$w < \bar{w} < w + b \tag{2.3}$$

$$\bar{w} < w < w + b \tag{2.4}$$

Case 2.2 is not interesting in the context of this model, because departing to the private sector dominates both honest and corrupt behavior. In case 2.3, bureaucrats are underpaid relative to the private sector, but corruption is more profitable than private-sector employment. In case 2.4 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 $k = 0$, $c > 0$.

2.4.2 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 ($\pi(N) = \pi(A) = 2$).

¹⁶Specifically, the structural model makes no additional assumptions on q_0, k, c .

When $w < \bar{w} < w + b$, 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 $\bar{w} < w < w + b$, 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 2.5 for an illustration). Formally:

Proposition 4. *If $k = c = 0$, then $\pi^*(N) = \pi^*(A) = 1$ is optimal if equation 2.3 holds. If equation 2.4 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.*

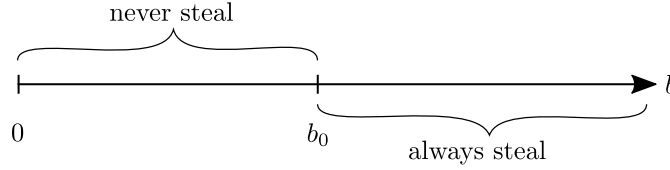


Figure 2.5: **Graphical illustration of proposition 4 in the case where $\bar{w} < w < w + b$.** The strategies “always steal” and “never steal” correspond, respectively, to $\pi(N) = \pi(A) = 1$ and $\pi(N) = \pi(A) = 0$.

2.4.3 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 $w < \bar{w} < w + b$, 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 $\bar{w} \gg w + b$.

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

Proposition 5. *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 2.3 holds, then $k_0 > \bar{w}$. If equation 2.4 holds, then there are b_0, b_1 with $0 < b_0 < b_1$ such that $k_0 < 0$ if $b < b_0$, $k_0 \in [0, \bar{w}]$ if $b \in [b_0, b_1]$, and $k_0 > \bar{w}$ otherwise. Other stationary policies are not optimal.*

Proposition 5, illustrated graphically in Figure 2.6, tells us that if the wage penalty is sufficiently high, it has a deterrence effect, pushing agents to never steal. Conversely, if it is not high enough, then the agent engages in corruption until she gets detected and punished. Again, since $w < \bar{w} < w + b$, never stealing is not attractive.

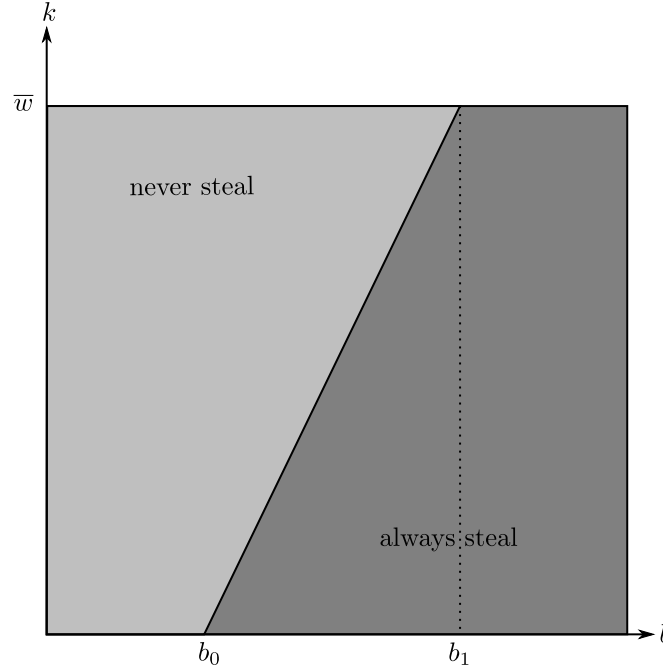


Figure 2.6: **Graphical illustration of proposition 5** in the case where $\bar{w} < w < w + b$. The strategies “always steal” and “never steal” correspond, respectively, to $\pi(N) = \pi(A) = 1$ and $\pi(N) = \pi(A) = 0$.

2.4.4 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.

When $w < \bar{w} < w + b$ (Figure 2.7, left panel), always stealing is optimal if the clean-up is sufficiently small. When such clean-up effect increases and makes rents too small, other policies become optimal. Which policy is optimal depends 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. $\pi(A) = 2$).

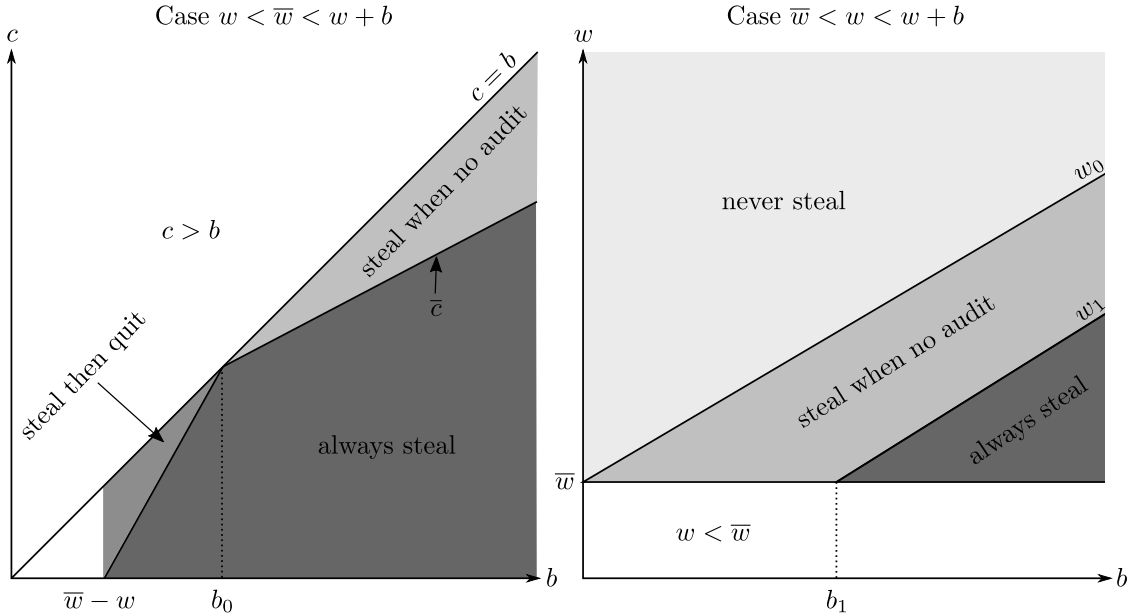


Figure 2.7: **Graphical illustration of proposition 6.** The policies “never steal,” “always steal,” “steal when no audit,” and “steal then quit” correspond, respectively, to $\pi(A) = \pi(N) = 0$; $\pi(A) = \pi(N) = 1$; $\pi(A) = 0, \pi(N) = 1$; and $\pi(A) = 2, \pi(N) = 1$.

When $\bar{w} < w < w + b$ (Figure 2.7, 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 6. *Suppose $k = 0$ and $c > 0$. If equation 2.3 holds, then there is $\bar{c}(b) \leq b$ such that $\pi^*(N) = \pi^*(A) = 1$ is optimal whenever $c \leq \bar{c}$. The policy $\pi^*(N) = 1, \pi^*(A) = 2$ is optimal whenever $c \geq \bar{c}$ and $b \leq b_0$ such that $w + b_0 \geq \bar{w}$. Finally, the policy $\pi^*(N) = 1, \pi^*(A) = 0$ is optimal whenever $c \geq \bar{c}$ and $b \geq b_0$. If equation 2.4 holds, then the policy $\pi^*(N) = \pi^*(A) = 0$ is optimal whenever $w \geq w_0 \geq \bar{w}$. The policy $\pi^*(N) = 1, \pi^*(A) = 0$ is optimal whenever $w \in [w_0, w_1]$, with $w_1 \geq \bar{w} \iff b \geq b_1 > 0$. 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.*

2.4.5 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 (see e.g. ?). 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 ? for a discussion of when either effect could materialize). Similarly, the political principal may engage in corruption herself, hence adopting lenient responses, and setting c and q to low values, or instead be tough on corruption, hence setting c and q to high values (see e.g. ? 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.

2.5 Structural estimation

Having devised a model that shows all the ways in which audits may affect bureaucratic careers and corruption, we use it to guide a more informed exploration of the data. Full structural estimation of the model is challenging, for two reasons: first, we do not observe whether agents engage in corruption or not (i.e., whether $a_{it} = 0$ or 1) and second, the model attributes all the part of the decision to stay that cannot be attributed to the wage differential $w - \bar{w}$ to rents from corruption b , ignoring the well-known fact that bureaucrats may derive non-monetary benefits from staying in office, which we broadly refer to as public sector motivation. These two points make identifying a structural model challenging, because it is unclear whether the low dismissal and departure rates observed in the data owe to high corruption, high rents and low probability of sanction, or lack of corruption because of a large threat of punishment.

Given these difficulties, we chose an intermediate approach and estimate a dynamic discrete choice (DDC) model. This model moves closer to the theory by introducing forward-looking agents and payoff functions that resemble the theory. It differs from the theory by lumping the decision of engaging in cor-

ruption or not into a decision of staying in the bureaucracy. Using the results from audits as proxies for municipal-level corruption, parameter estimates average over agents that steal and agents that do not. They separate for agents with observed characteristics x_i (1) an average level of rents that varies systematically according to observed municipal levels of corruption – presumably capturing illegal rents, (2) an average short-term effect of those audits on such rents, and (3) a time- and municipality-invariant rent that may owe to public sector motivation or unobserved rents.

In what follows, we first describe the DDC model and present our results. The results rule out unobservable ex-post effects and any systematic variation in the size of rents according to observed levels of municipal corruption. The results show that bureaucrats remain in office much more than what would be expected given their public/private-sector wage differential, leaving us with two opposite conclusions as to the effectiveness of the program: either the program has a strong disciplining effect and bureaucrats’ stickiness owes to strong public sector motivation, or it has a weak disciplining effect and bureaucrats stay in office because they pocket large rents from corruption. We finally use those estimates to calibrate a series of counterfactual experiments that investigate ways of making the program more effective.

2.5.1 Approach

In this model, bureaucrat i in municipality j makes at each period t the career decision $y_{ijt} \in \{0, 1\}$, with 0 corresponding to staying in the public sector, and 1 to departing to the private sector. The crucial differences between this model and the theoretical model is that now, the agent gets payoff b_{ij} from engaging in corruption that does not depend upon her actions, and enjoys (non-monetary) public sector motivation m_i . Additionally, at each time period, and for every potential choice, the agent enjoys taste shock ϵ_{ijt}^y . Normalizing to 0 her payoff

in state P , the agent's flow payoff writes:

$$\begin{aligned}
u_{ijt}(0, N) &= b_{ij} + m_i + w_i - \bar{w}_i + \epsilon_{ijt}^0 \\
u_{ijt}(0, A) &= b_{ij} - c_i + m_i + w_i - \bar{w}_i + \epsilon_{ijt}^0 \\
u_{ijt}(1, N) &= u_{ijt}(1, A) = u_{ijt}(y_{it}, P) = 0 + \epsilon_{ijt}^y \\
u_{ijt}(y_{it}, P') &= -k_i + \epsilon_{ijt}^y
\end{aligned}$$

If the agent choses to stay in the public sector, she gets dismissed with probability q_j^A and q_j^N in the respective events that the bureaucracy gets audited or does not. The transition matrix for $y_{ijt} = 1$ has $\Pr(s_{ijt+1} = P | y_{ijt} = 1, s_{ijt}) = 1$ for any s_{ijt} . The transition matrix for $y_{ijt} = 0$ writes

$$\begin{array}{c}
\begin{array}{cccc}
& N & A & P' & P \\
\begin{array}{c} N \\ A \\ P' \\ P \end{array} & \left[\begin{array}{cccc}
(1-p)(1-q_j^N) & p(1-q_j^A) & (1-p)q_j^N + pq_j^A & 0 \\
(1-p)(1-q_j^N) & p(1-q_j^A) & (1-p)q_j^N + pq_j^A & 0 \\
0 & 0 & 0 & 1 \\
0 & 0 & 0 & 1
\end{array} \right]
\end{array}
\end{array}$$

We assume that b_{ij} is a function of observed, municipal level corruption \bar{b}_j , a vector of individual-level time-invariant characteristics x_i ; i.e. $b_{ij} = f(\bar{b}_j, x_i)$. We assume that m_i and c_i are also functions of x_i ; i.e. $m_i = g(x_i)$ and $c_i = h(x_i)$. Let's assume, finally, that f , g and h are linear functions: $f(\bar{b}_j, x_i) = \alpha_b + \bar{b}_j \beta_b + x_i' \bar{b}_j \gamma_b$, $g(x_i) = \alpha_m + x_i' \beta_m$ and $h(x_i) = \alpha_c + x_i' \beta_c$. The agent's flow payoff from action 0 in states A and N writes

$$u_{ijt}(0, s \in \{A, N\}) = \alpha_m + \alpha_b + x_i' \beta_m + \bar{b}_j \beta_b + x_i' \bar{b}_j \gamma_b - 1\{s = A\} [\alpha_c + x_i' \beta_c] + w_i - \bar{w}_i + \epsilon_{ijt}^0$$

The agent makes decisions that maximize its long-run discounted payoff

$$U(y_{ij0}, y_{ij1}, \dots) = \mathbb{E} \left\{ \sum_{t=0}^{\infty} \delta_i^t u_{ijt}(y_{ijt}, s_{jt}) \right\}$$

We estimate this model, up to parameters α_m and α_b , using the two-step

conditional choice probability partial likelihood estimator (?) and a series of auxiliary models. We observe the states, career decisions, as well as characteristics x_i that include gender, level of education and contract type. We estimate w_i , \bar{w}_i , δ_i , k_i using auxiliary models that we detail in Appendix 2.9. We estimate state transition probabilities q^A , q^N non-parametrically. We obtain first-stage estimates of conditional choice probabilities using a logistic regression with municipal-level fixed effects, controlling for x_i and the state. We then use those state transition probabilities and conditional choice probabilities to derive estimates of the conditional value function and finally estimate parameters $\beta_m, \beta_b, \gamma_b, \alpha_c, \beta_c$ and parameter $\theta = \alpha_m + \alpha_b$ in a second-stage logistic regression.

Note that the model is fully identified under the assumption that $f(0) = 0$; that is, assuming that in municipalities in which audits reveal no corruption have indeed no corruption. Indeed, the assumption implies that $\alpha_b = 0$, and therefore that $\theta = \alpha_m$. In that case, we get $\theta + x'_i \beta_m = m_i$, $\bar{b}_j \beta_b + x'_i \bar{b}_j \gamma_b = b_i$ and $c_i = \alpha_c + x'_i \beta_c$.

We shy away from making this arguably strong assumption. Our estimates therefore recover the short-term change in such rents following audits c_i , the part of rents from corruption that varies with municipal corruption and individual characteristics, i.e. $b_{ij} - \alpha_b$, and the part of public sector motivation that varies with individual-level characteristics, i.e. $m_i - \alpha_m$. The intercept θ conflates a baseline level of public sector motivation and a baseline level of rents from corruption.

2.5.2 Estimates, validation and counterfactual experiments

Table 2.3 reports standardized parameter estimates from our DDC model. We see that the intercept $\theta = \alpha_m + \alpha_b$ is large, while the part of rents from corruption that varies with municipal corruption and individual-level characteristics (i.e. parameter β_b) is small and not significantly different from zero, and so is the parameter β_c on the clean-up effect c_i .

Figure 2.8 reports the unstandardized, median in-sample value of quantities of interest under the assumption that $\alpha_b = 0$ (i.e. that the intercept only captures public sector motivation). We find that public sector motivation m_i is large, amounting to about 90% of median wage w_i . Rents from corruption b_{ij} and the clean-up effect c_i are small.

Together these results show that (1) departure rates show systematic little variation across municipalities as a function of the number of irregularities reported by audits; (2) they also show little variation depending on whether the municipality is audited or not; and (3) that the public/private-sector wage differential $w_i - \bar{w}_i$ is insufficient to account for the low departure rates observed in the data. While results (1) and (2) echo our reduced-form results on the number of departures, result (3) adds an important detail to the picture: bureaucrats' stickiness owes to a large unobserved component θ . Whether that component captures an invariant payoff from public sector motivation α_m or invariant rents from corruption α_b is an open question.

These results are consistent with two opposite conclusions as to the effectiveness of the audits program; namely that the program either has a large or a small disciplining effect. Our reduced-form estimates showed that the ex-post, observable effect of the program is small. Structural estimates established that the ex-post, unobservable effect of the program is also small. Whether the program has a disciplining effect is an open question. If θ mostly captures public sector motivation, then the program has a large disciplining effect. Under this hypothesis, rents from corruption are negligible while public sector motivation is large. The low dismissal and departure rates observed in the data indicate that bureaucrats stay in office because they derive large public sector motivation, as the program has successfully eliminated corrupt rents. Conversely, if θ mostly captures rents from corruption, then the program also has a negligible disciplining effect. Under this hypothesis, rents from corruption are large while public sector motivation is negligible. The low dismissal and departure rates observed in the data indicate that bureaucrats stay in office because they derive large rents from corruption, and that the program is unable to remove corrupt bureaucrats from office.

We validate these estimates by checking that they match patterns observed in the data. We check whether the career paths implied by estimated parameter values match the career paths observed in the data (Appendix 2.9, Figure). To do so, we compare year to year variation in the size of the bureaucracy as observed in the data to predicted variation under parameter values. We find that our predictions match the data remarkably well, except for years 2008 and 2012, where we fail to predict the large waves of departures and dismissals. As discussed in section 2.3, these years are pre-electoral years, during which turnover is large, a feature that we did not include in the model.

We finally evaluate the impact of the program by comparing the total amount of rents extracted through corruption to a series of counterfactual benchmarks under the theoretical model analyzed in section 2.4, in which we augment flow payoffs with random extreme value type I shocks. This computational exercise is calibrated using parameter estimates from the DDC model. Doing so amounts to ascribing the average effects identified by the DDC model to structural parameter values. The assumption would be verified under the admittedly unrealistic case in which all agents always engage in corruption. Should some agents not engage in corruption, then the assumption understates gains from corruption b , the clean-up effect c , and the probability of dismissal q . Furthermore, since, as argued above, it is unclear whether the intercept θ captures public sector motivation or invariant rents from corruption, we consider the following three scenarios: (1) $\theta = \alpha_m$, (2) $\theta = \frac{\alpha_m}{2} + \frac{\alpha_b}{2}$, and (3) $\theta = \alpha_b$. In other words, we assume that the intercept either fully captures public sector motivation (case 1), fully captures invariant rents from corruption (case 3), or is composed in equal parts of public sector motivation and rents from corruption (case 2).

We consider a *baseline*, which corresponds to predictions under the estimated parameters. We compare this baseline against two benchmarks. First, a *negative benchmark* in which we set both c and q to zero. This allows evaluating how much corruption has been deterred by the program, compared to a counterfactual in which the program was never enacted. We then compare our baseline to a *positive benchmark*, in which we set again both c and q to

zero, but reduce the size of rents to the median for all those municipalities for which b is above the median. This positive benchmark corresponds to a counterfactual world in which audits never took place, but the size of rents is halved. All scenarios consider the distribution of states as they occurred in the data (i.e. we use the realizations of audits that occurred in the data). They consider each sample bureaucrat on the year they enter the sample and estimate their optimal policy given parameter values. This allows deriving their probability of departure/dismissal, 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.

Comparing the baseline to the negative benchmark in which we remove audits (Figure 2.9), we find that the program had little impact on corruption: irrespective of the assumption on θ , the program reduced corruption by less than 2 percentage points. Comparing the baseline to the positive benchmark confirms the finding. Halving the size of rents reduces total corruption by 75, 17, or 9 percentage points depending on whether θ fully captures public sector motivation, a mix of public sector motivation and corruption, or fully captures corruption respectively.

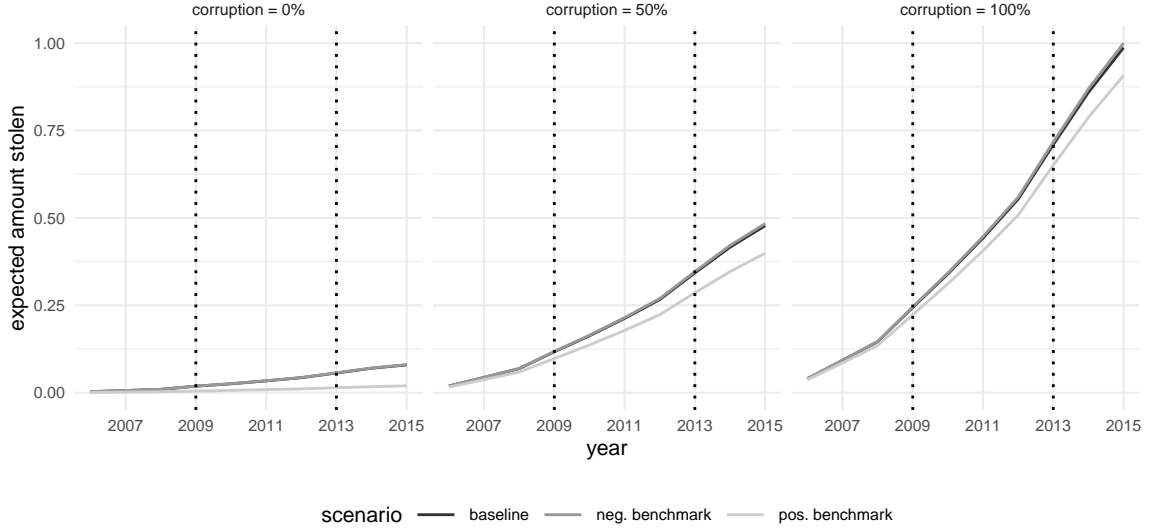


Figure 2.9: Impact of the program on corruption. The program occasioned less than a 2 percentage points decrease in overall corruption comparing to a benchmark in which audits never occurred. This is a far cry from halving the size of the rents (positive benchmark), which reduces the amount of corruption by about 75, 17, or 9 percentage points (left to right panel).

We finally investigate ways to make the program more effective. To do so, we construct a series of counterfactuals in which we progressively increase the value of each of the parameters p, c, q from 0 to 1 for all those municipalities which fall below the target value, while keeping other parameters constant. We finally consider a scenario which increase all three parameters jointly. We examine, for each of these scenarios, the total expected amount stolen by the end of the period. Our key comparison is how much reduction in corruption those scenarios achieve compared to the baseline, and our positive benchmark.

The results, reported in Figure 2.10, show that, considering each component of the program in isolation, increasing the strength of monitoring associated with audits (i.e. parameter q) is the most effective way of reducing corruption. If corruption was met with certain dismissal (recall that the average $q = 0\%$), corruption would decrease by 25, 15, and 12 percentage points depending on whether θ fully captures public sector motivation, a mix of public sector motivation and corruption, or fully captures corruption respec-

tively. This, however, only matches our positive benchmark in the latter two cases. The most effective way to reduce corruption is to increase all parameters jointly, suggesting that program components display strong complementarities. Irrespective of the hypothesis on θ , increasing all parameters to 25% is more effective than punishing corruption with certain dismissal.

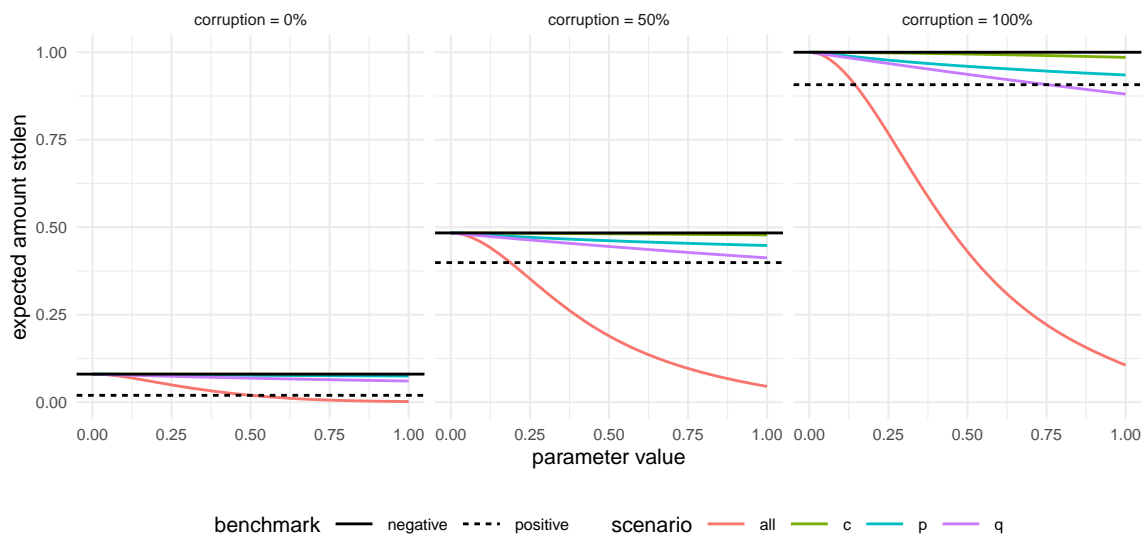


Figure 2.10: Impact of increasing parameter values on total expected corruption. When considering each component of the program in isolation, increasing the strength of monitoring associated with audits q is the most effective approach. Program components display strong complementarities: increasing all parameters jointly is by far more effective than increasing any parameter in isolation.

Overall, our results paint a nuanced picture of the effect of the audits program on bureaucratic careers and corruption. Both structural and reduced-form results show that audits have caused small ex-post reductions in corruption. Furthermore, while bureaucrats stay in office much more than what is to be expected given their private/public-sector wage differential, it is unclear whether this owes to strong public-sector motivation, or pervasive rents from corruption. Finally, The program could be made more effective by making the monitoring component of audits more effective and, mostly, by leveraging strong complementarities among program components: a multi-pronged

approach of making small improvements to the frequency, monitoring, and clean-up effects of audits is far more effective than making large improvements in a single component of the program.

2.6 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. We considered a program that has been shown to be effective at disciplining politicians, and considered its effect on bureaucrats. We devised a theoretical model that takes into account how the specifics of bureaucratic careers – as opposed to politicians’ – feature in their decision to engage in corruption. Specifically, when deciding whether to engage in corruption, bureaucrats weigh their exit options against an extended career time-horizon, leading to a wide range of potential ex-post, or disciplining effects.

Taking the model to the data showed that audits have little ex-post effects, and that bureaucrats remain in office much more than what would be expected given their public/private-sector wage differential: bureaucrats receive a time- and large municipality-invariant payoff that amounts to 90% of the median public-sector wage. We are, however, unable to tell whether this payoff stems from high public sector motivation or from pervasive rents from corruption. In other words, results suggest that the program either has a large disciplining effect or no effect at all.

Our results have important implications for the design of effective anti-corruption policies. Investigating ways to make the program more effective, we found that the three components of the program; namely, the frequency of audits, the quality of monitoring, and the size of bureaucratic clean-ups show strong complementarities. Out of all three components, increasing the strength of the monitoring associated with audits is most effective at curbing corruption, as suggested by previous studies (???). However, improving all three components jointly is far more effective than working in isolation: improving all components to 25% is more effective than perfectly punishing corruption.

This suggests that multi-pronged approaches aiming at reinforcing jointly all components of a policy are more effective than narrower, single-pronged approaches.

Our paper proposes an alternative approach to policy evaluation, that usefully complements randomized controlled trials (RCTs), a practice that has become increasingly common in the development community (?). Using structural estimation, our approach features a tight linkage between theory and empirics. The first benefit is breaking down a complex, nation-wide policy into simple constitutive components, and manipulating each of those separately. This allows us to assess the effectiveness of the program by comparing to a counterfactual in which it never took place – which RCTs already do – while also considering counterfactuals in which we manipulate each of its components individually or jointly. A second benefit is that we are able to identify, through theory, unobservable and long-term disciplining effects. In our particular setting, a reduced-form approach would have missed such long-term effects. These benefits, however, come at a cost: our estimation relies on stronger assumptions which RCTs, by design, do not, and we are unable to ascribe such long-term effects to large public sector motivation or pervasive rents from corruption. Nevertheless, designing similar policy evaluations using RCTs requires complex interventions that may not be feasible in practice. Overall, we advocate for complementary approaches to policy evaluation which leverage the comparative advantages of each.

Finally, an important line of inquiry is to investigate the generalizability of our findings. While we provide a theoretical framework that could, *a priori*, extend to any other audit program, several points may not generalize as easily. First, while a large number of countries introduce some randomness in their audits, full randomization is not a common feature, as countries tend to prioritize those places that are more likely to be corrupt. Our model could accommodate this by making the probability of an audit depend on the results of the previous audit. Second, our particular program showed little ex-post effect because audits fail to trigger large waves of dismissals or departures in corrupt municipalities. Investigating the reasons behind this in our context may help

predict whether a similar policy will show comparable effects in other settings. We surmise that this owes to the facts that (1) audits occur infrequently because they are costly, and (2) that unionization introduces additional frictions that shelter corrupt bureaucrats from punishment. Lastly, we showed that the program exhibits strong complementarities. Further research could explore whether similar complementarities apply in other settings.

2.7 Appendix

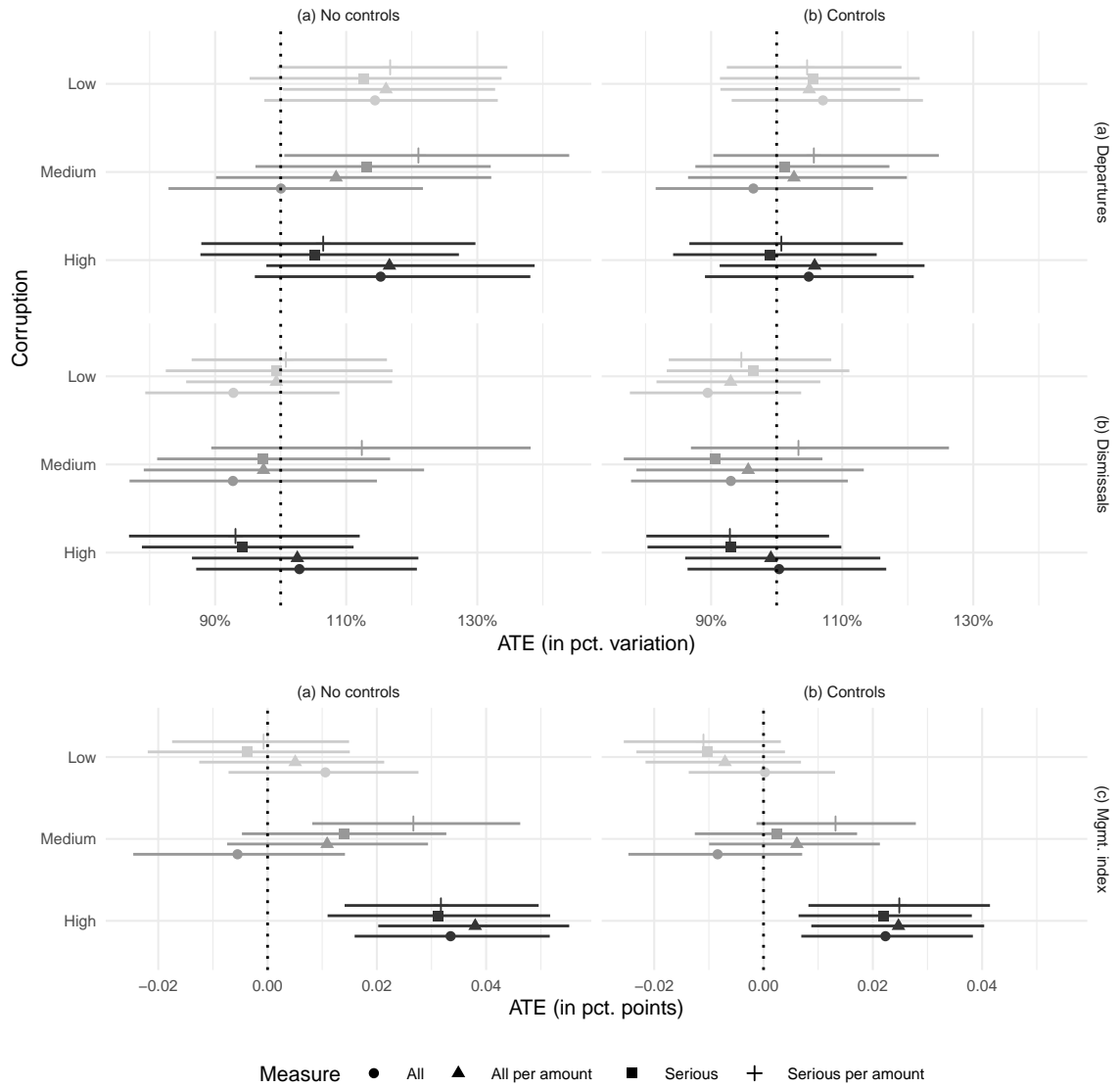


Figure 2.2: **Main result, different corruption metrics.** This table reestimates the specifications in Table 2.2 using all four corruption metrics. For each specification, we report the parameters β_2 , $\beta_2 + \beta_{2,1}$, and $\beta_2 + \beta_{3,1}$. The top panel exponentiates these parameters 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.

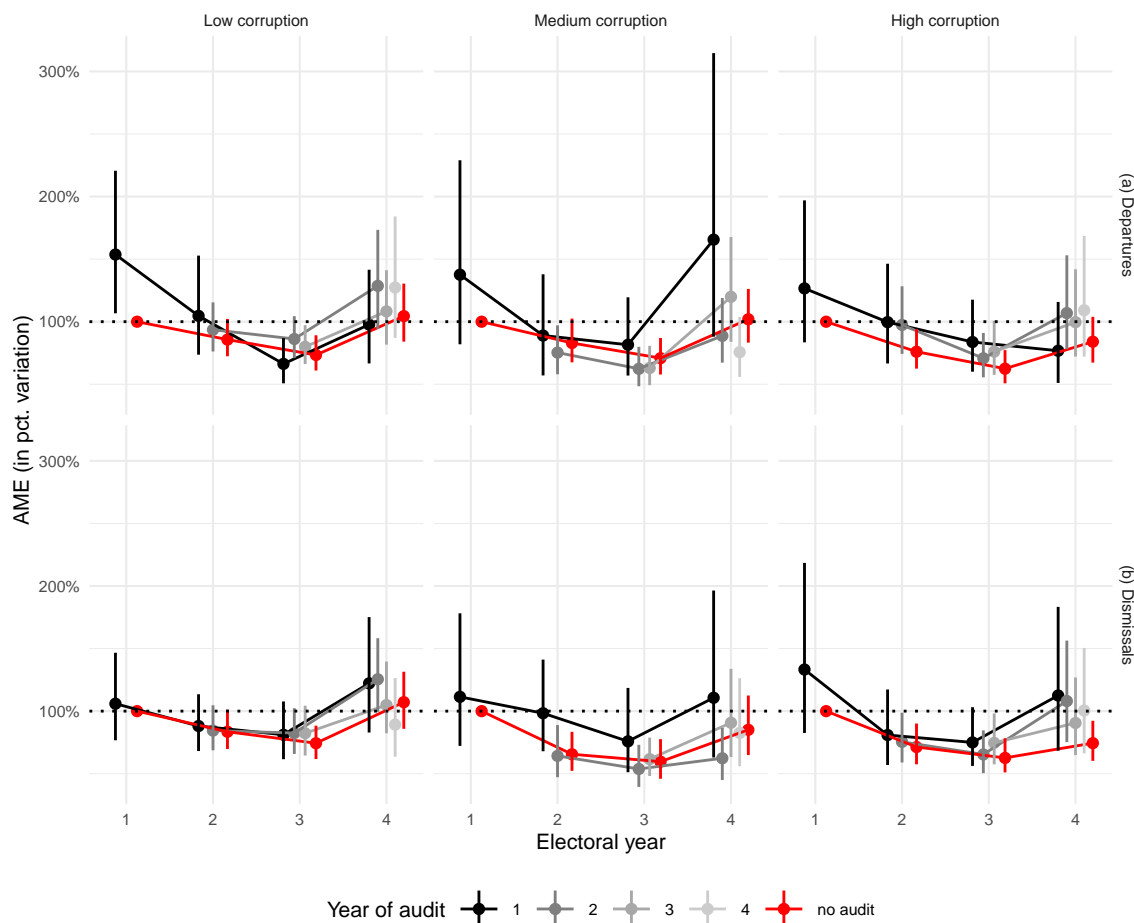


Figure 2.4: **Treatment effect as a function of the political cycle during the first term.** The y-axis represents the average marginal effect of audits the row outcome. The x-axis represents years in the political cycle, with year 1 being the first year of mandate. Colors indicate the year of the political 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 2.3.1. There is no evidence that audits lead to greater dismissals or departures of high-level bureaucrats.

Constant ($\theta = \alpha_m + \alpha_b$)	1.985*** (0.297)
education - 2ary (β_{m1})	-0.074 (0.196)
education - higher (β_{m2})	-0.742*** (0.174)
male (β_{m3})	-0.209*** (0.074)
tenure (β_{m4})	0.363 (0.272)
\bar{b}_j (β_b)	0.020 (0.020)
$\bar{b}_j \times$ education - 2ary (γ_{b1})	0.009 (0.016)
$\bar{b}_j \times$ education - higher (γ_{b2})	0.009 (0.012)
$\bar{b}_j \times$ male (γ_{b3})	-0.006 (0.005)
$\bar{b}_j \times$ tenure (γ_{b4})	-0.007 (0.019)
audit \times education - 2ary (β_{c1})	0.107 (0.219)
audit (α_c)	-0.006 (0.128)
audit \times education - higher (β_{c2})	0.159 (0.168)
audit \times male (β_{c3})	0.039 (0.062)
audit \times tenure (β_{c4})	-0.220 (0.254)
Observations	671,831
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01	

Table 2.3: **Standardized DDC estimates.** The intercept $\theta = \alpha_m + \alpha_b$ is large, while the part of rents from corruption that varies with municipal corruption and individual-level characteristics ($b_{ij} - \alpha_b$) is small and not significantly different from zero, and so is the clean-up effect c_i . Standard errors are clustered at the municipal level and obtained through a non-parametric bootstrap procedure with 10,000 replicates.

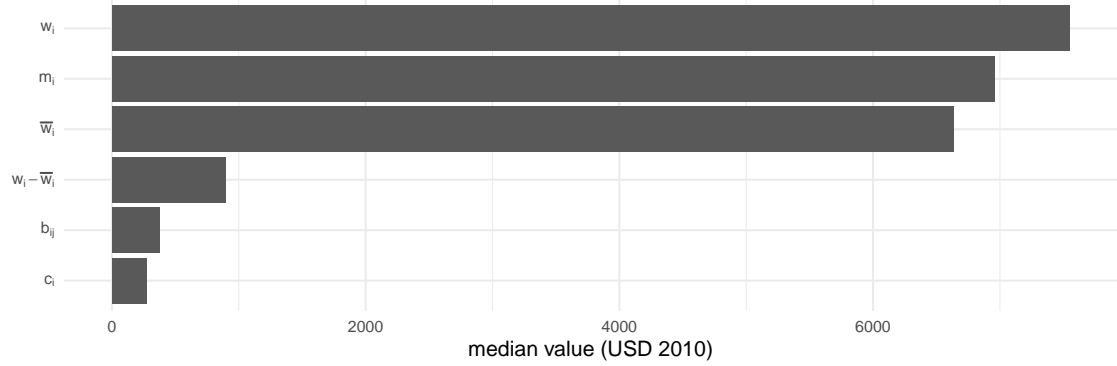


Figure 2.8: **Median values of quantities of interest.** This figure reports the median in-sample value of quantities of interest under the assumption that $\alpha_b = 0$. We see that public sector motivation m_i is large, amounting to about 90% of median wage w_i . Rents from corruption b_{ij} and the clean-up effect c_i are small.

Appendices

2.7.1	Proofs	83
2.7.2	Additional descriptive statistics	86
2.7.3	Measuring corruption	86
2.7.4	Management index	91
2.7.5	Additional descriptive statistics	94
2.8	Robustness checks	95
2.8.1	Other categories of bureaucrats	95
2.8.2	Subset of municipal secretaries	98
2.8.3	Subset by Tenure	99
2.8.4	Hiring Practices	101
2.8.5	Balance tests	103
2.8.6	Dependent variable as percentages	103
2.8.7	Findings on management	105
2.8.8	Subset of never audited municipalities	107
2.8.9	Individual-level analysis	108
2.8.10	Political models with other corruption metrics	112
2.9	Estimation and validation of the DDC model	119
3.1	Introduction	124
3.2	Context and Data	126
3.2.1	Brazilian municipal government	126
3.2.2	Municipal employment and patronage in Brazil	127
3.2.3	Partisan affiliation in Brazil	128
3.3	Data	129
3.3.1	Municipal and Private Sector Employment	129
3.3.2	Partisan affiliation	130
3.3.3	Joining Public Sector Employment to Partisanship Data	133
3.3.4	Who benefits from patronage?	135
3.3.5	Islands of Patronage	140
3.4	Campaign Finance and Wealthy Patrons	145
3.5	Conclusion	145

2.7.1 Proofs

In this section, we denote by $v_\pi(s) = \mathbb{E}_\pi[(1 - \delta) \sum_{t=0}^{\infty} \delta^t u(\pi(s_t), s_t) | s_0 = s]$ the continuation value implied by a strategy in state s . We denote by π_0, π_1, π_2 the policies that consist in playing 0, 1, 2 respectively in both states A and N . Note that $v_{\pi_0} = w$ and $v_{\pi_2} = \bar{w}$. For concision, we also define $\bar{p} \equiv pq$. 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 4. Note that

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

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

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

Note that $v_{\pi^*}(N) > \bar{w} \iff w + b > \bar{w}$, which is true by assumption. So π^* is always preferred to π_2 . Also note that $v_{\pi^*}(N) > w \iff b(1 - \delta) + \bar{p}(\bar{w} - w)\delta > 0$. As such, if $\bar{w} > w$, then π^* is always preferred to π_0 . If $\bar{w} < w$, π^* is preferred to π_0 iff $b \geq \frac{\bar{p}(w - \bar{w})\delta}{1 - \delta} \equiv b_0 > 0$. \square

Proof of proposition 5. Note that

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

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

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

If $w < \bar{w}$ (i.e. if equation 2.3 holds), then π_2 is preferred to π_0 . We have that

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

Yet, $k_0 \leq \bar{w} \iff \bar{w} \geq \frac{w+b}{1+\delta p} > w+b$, which is impossible. As such $v_{\pi_1}(N) \geq v_{\pi_2}(N)$ for any $k \in [0, \bar{w}]$, so π_1 is optimal.

Suppose now that $\bar{w} < w$ (i.e. suppose that equation 2.4 holds). In this case, π_0 is preferred to π_2 . We have that

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

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

Proof of proposition 6. Note first that if a policy π^* such that $\pi^*(A) = 1$ is optimal, then it must be that $\pi^* = \pi_1$. Indeed, the continuation values of policy π_1 satisfy the following:

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

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 $v_{\pi^*}(A) \geq \bar{w}$. As such, it cannot be that $\pi^*(N) = 2$, because this would imply that $v_{\pi^*}(N) = \bar{w} < v_{\pi_1}(N)$. 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 π' 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 $w > \bar{w}$ or $\bar{w} > w$, 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 π_0 , π_1 , π_2 , and policies π_{10} and π_{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

$$v_{\pi_1}(N) = \frac{(1-\delta)[b+w-\delta p(1-q)c] + \delta \bar{p}\bar{w}}{1-\delta(1-\bar{p})}$$

$$v_{\pi_1}(A) = \frac{(1-\delta)[b+w-(1-\delta(1-p))c] + \delta \bar{p}\bar{w}}{1-\delta(1-\bar{p})}$$

Continuation values for π_{10} satisfy

$$v_{\pi_{10}}(N) = (1-\delta)(w+b) + \delta[(1-p)v_{\pi_{10}}(N) + p(1-q)v_{\pi_{10}}(A) + \bar{p}\bar{w}]$$

$$v_{\pi_{10}}(A) = (1-\delta)w + \delta[(1-p)v_{\pi_{10}}(N) + pv_{\pi_{10}}(A)]$$

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

$$v_{\pi_{10}}(N) = \frac{(1-\delta)[(1-p\delta)b + (1-\bar{p}\delta)w] + \bar{p}\delta(1-p\delta)\bar{w}}{1-\delta[1-(1-p)\bar{p}\delta]}$$

$$v_{\pi_{10}}(A) = \frac{(1-\delta)[w + (1-p)\delta b] + \delta^2\bar{p}(1-p)\bar{w}}{1-\delta[1-(1-p)\bar{p}\delta]}$$

Finally, continuation values for π_{12} satisfy

$$v_{\pi_{12}}(N) = (1-\delta)(w+b) + \delta[(1-p)v_{\pi_{12}}(N) + \bar{p}\bar{w}]$$

$$v_{\pi_{12}}(A) = \bar{w}$$

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

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

Suppose first that $w < \bar{w}$ (case 2.3). Note that in this case, $v_{\pi_2}(s) = \bar{w} > v_{\pi_1}(s) = w$ for any $s \in \{A, N\}$. Also note that since $w+b > \bar{w}$, we have that $v_{12}(N) > v_{\pi_2}(N)$. As such, three policies remain: π_{12}, π_{10} , and π_1 .

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

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

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

Suppose now that $w > \bar{w}$ (case 2.4). Note that in this case, $v_{\pi_2}(s) = \bar{w} < v_{\pi_1}(s) = w$ for any $s \in \{A, N\}$. Also note that since $w > \bar{w}$, we have that $v_{\pi_{10}}(s) > v_{\pi_{12}}(s)$ for any $s \in \{A, N\}$. As such, three policies remain: π_0, π_{10}, π_1 .

We have $v_{\pi_1}(N) \geq v_{\pi_{10}}(N) \iff w \leq \frac{b(1-\delta) + \bar{p}\delta\bar{w} - c[1-\delta(1-(1-p)\bar{p}\delta)]}{\bar{p}\delta} \equiv w_2$. Also note that $w_2 \geq \bar{w} \iff b \geq c \frac{1-\delta[1-(1-p)\bar{p}\delta]}{1-\delta} \equiv b_2$.

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

□

2.7.2 Additional descriptive statistics

2.7.3 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.

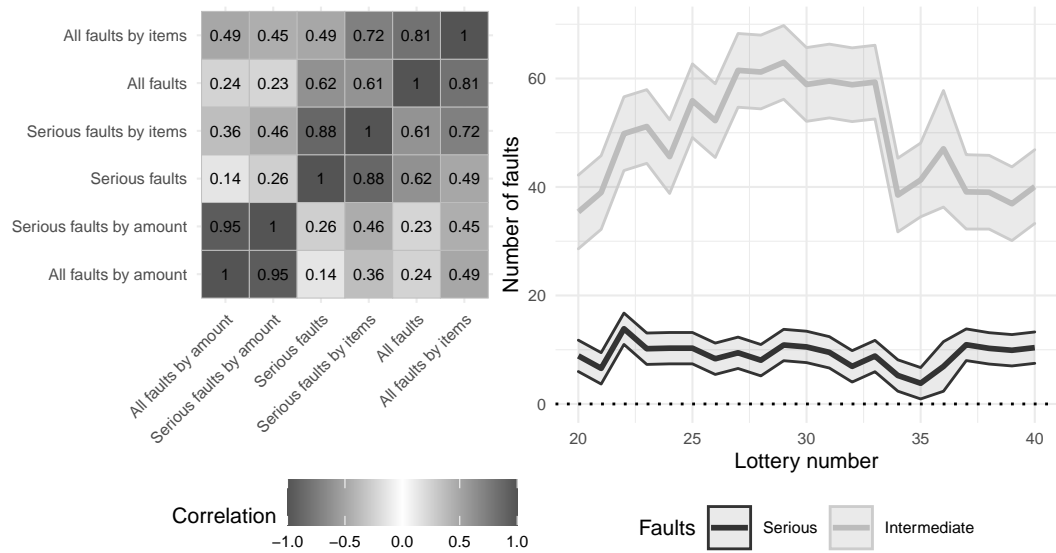
- **Falha média:** Minor irregularities in the execution of programs. Mostly

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. 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, and validate our four measures by verifying that they reproduce the main finding of ?; 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.



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

Figure 2.11: **Constructing indicators of corruption.** Left: While 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).

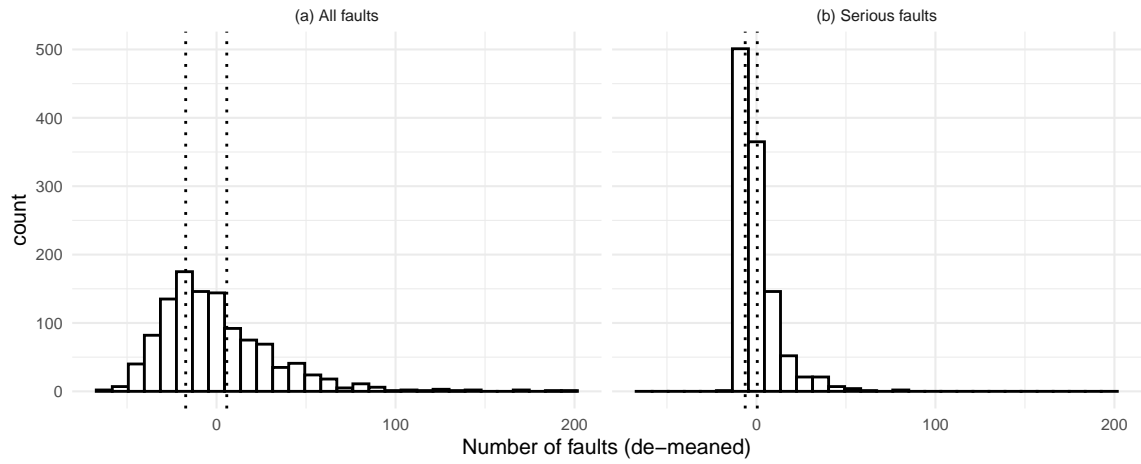


Figure 2.12: **Distribution of number of irregularities.** Vertical bars indicate the first and second tercile of each distribution. Most municipalities show little corruption.

Validation: Replication of ?

	<i>Dependent variable:</i>							
	All		Serious		All per Amount		Serious per Amount	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treat	−0.035* (0.019)	−0.041** (0.019)	−0.0003 (0.057)	−0.017 (0.057)	−0.150*** (0.056)	−0.055 (0.036)	−0.093* (0.049)	−0.059 (0.046)
Controls	−	✓	−	✓	−	✓	−	✓
?	✓	✓	−	−	−	−	−	−
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 2.4: **Replication of ?**. All models include lottery and state fixed effects and use robust standard errors. Models (1) and (2) replicate the specification in ? 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.

2.7.4 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 ? deploy to compare management practices among firms. We construct three dimensions of "good" management that can potentially reduce corruption. Table 2.5 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 2.5: 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

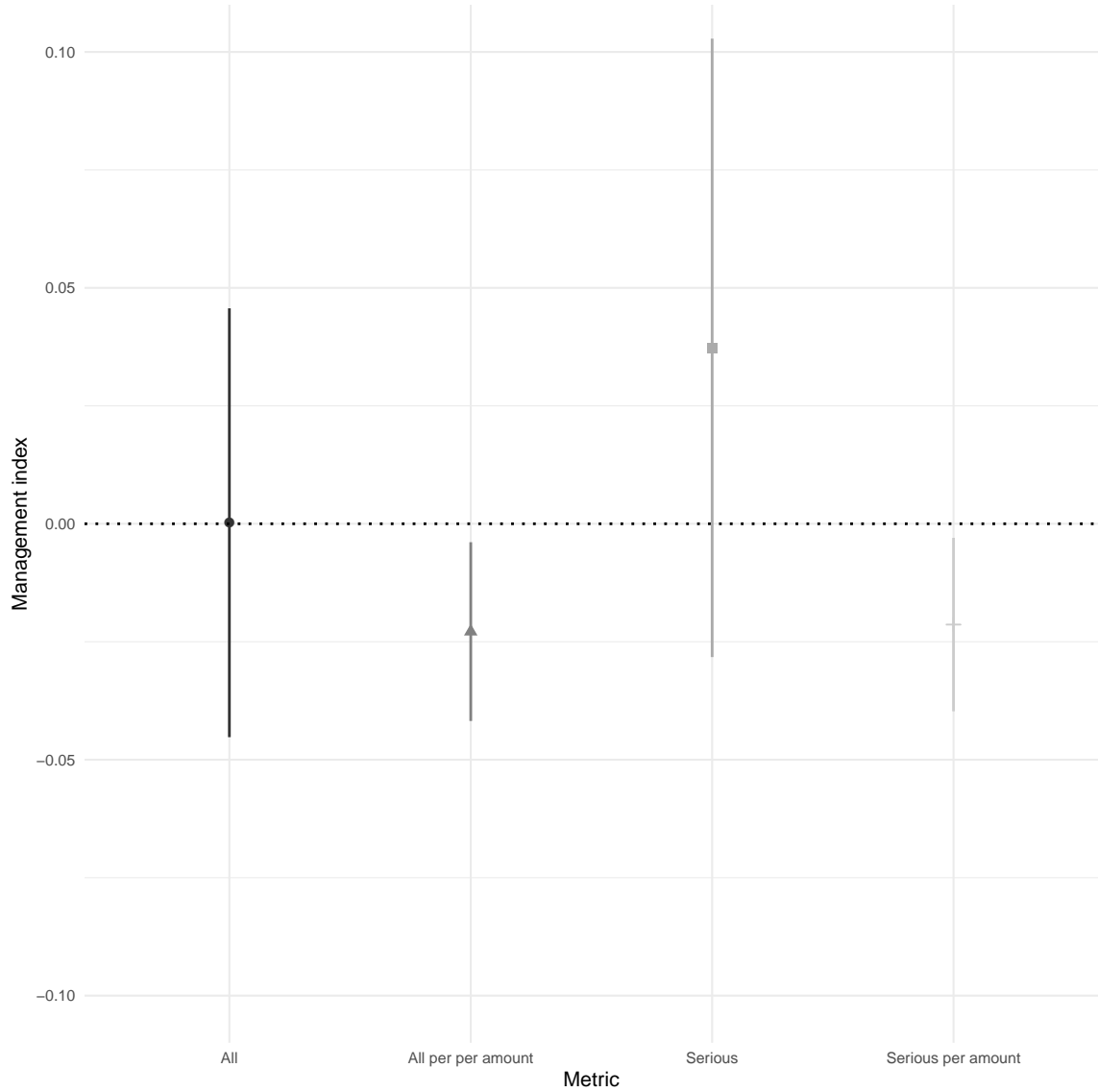


Figure 2.13: **Correlation management index - corruption.** We find partial support for the hypothesis that more corrupt municipalities have poorer management. This figure reports the coefficient associated with regressing the management index in municipality j as measured by the Munic survey conducted 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.

2.7.5 Additional descriptive statistics

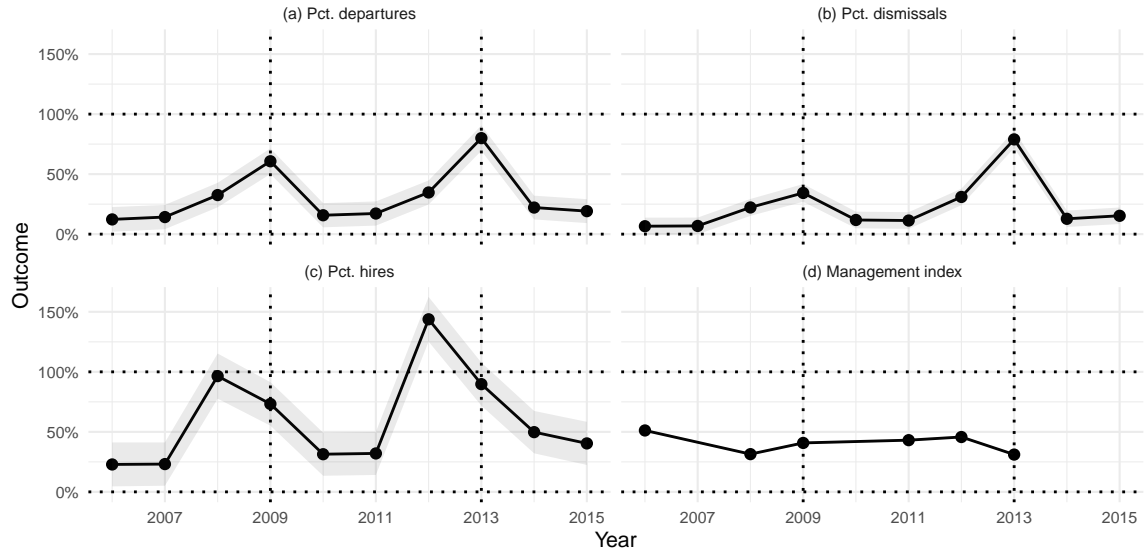


Figure 2.14: **Dependent variables over time.** Vertical bars denote election years. Shaded areas are heteroskedastic-robust 95 percent confidence intervals. There is seasonality in staff rotation around election years (panels a, b, c). On average, management practices remain constant over time.

2.8 Robustness checks

2.8.1 Other categories of bureaucrats

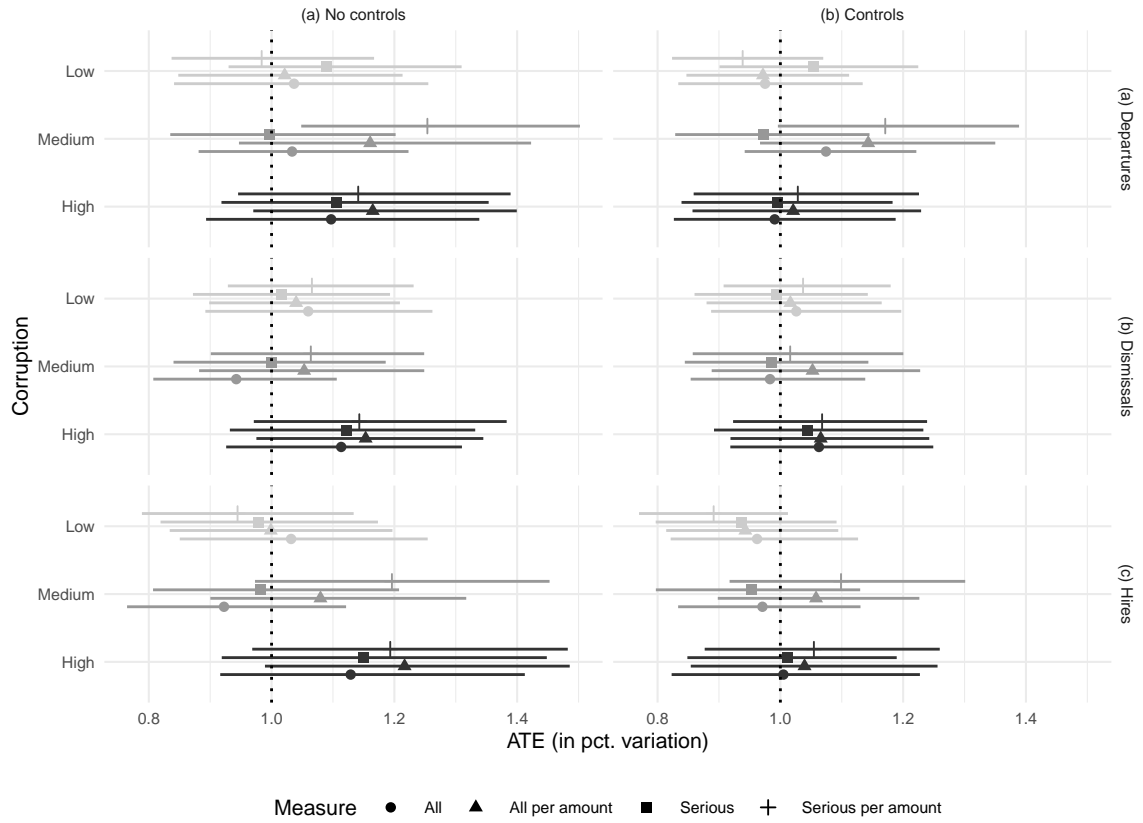


Figure 2.15: **Low bureaucrats.** This figure reproduces Figure 2.2 in the main text but considers low bureaucrats instead of high bureaucrats.

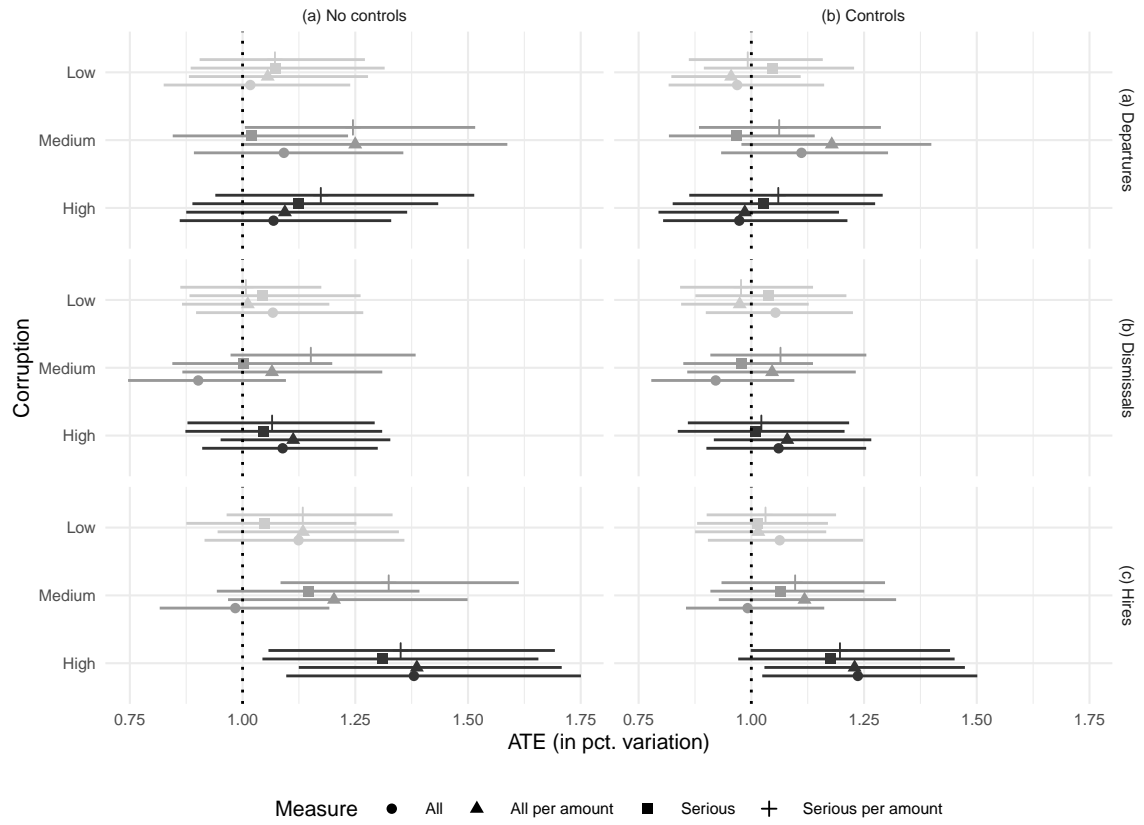


Figure 2.16: **High Frontline.** This figure reproduces Figure 2.2 in the main text but considers high frontline providers instead of high bureaucrats.

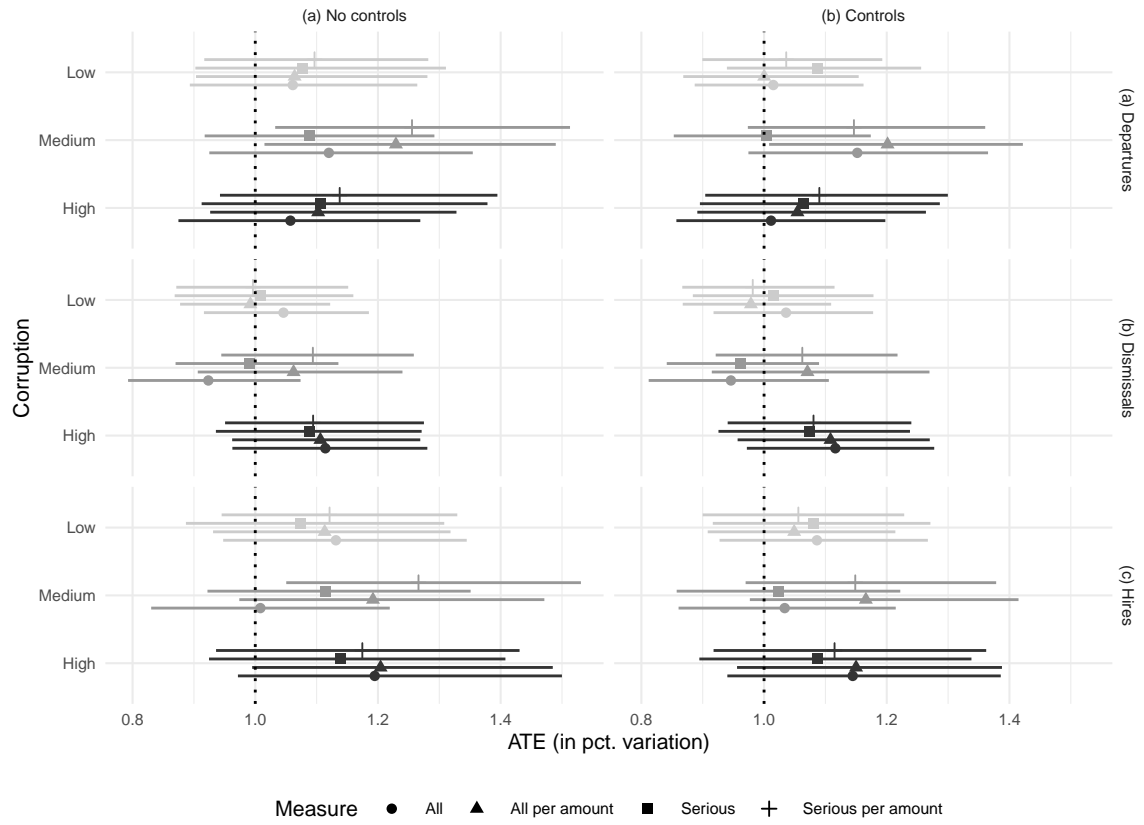


Figure 2.17: **Low Frontline.** This figure reproduces Figure 2.2 in the main text but considers high frontline providers instead of high bureaucrats.

2.8.2 Subset of municipal secretaries

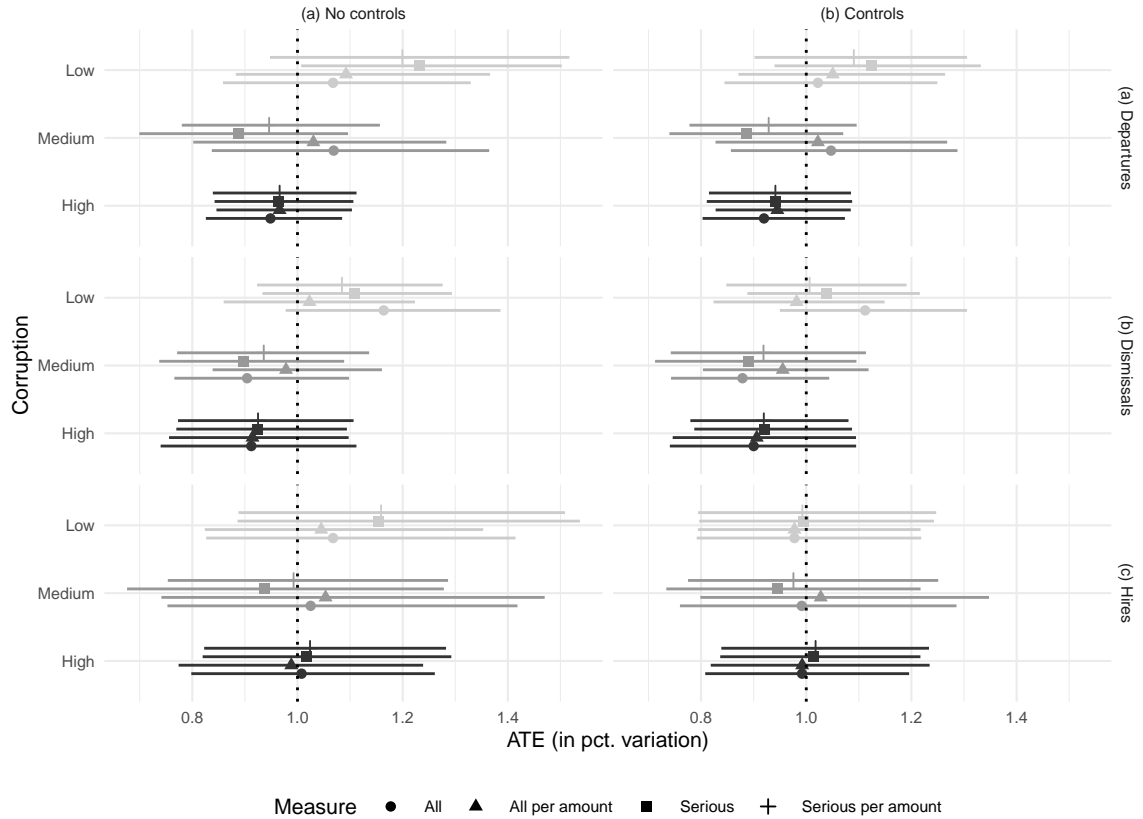


Figure 2.18: **Subset of municipal secretaries.** This figure reproduces Figure 2.2 in the main text but restricts the sample to the highest-ranking bureaucrats, namely municipal secretaries.

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.

2.8.3 Subset by Tenure

This robustness check verifies whether there are heterogeneities according to the type of contract that the high-level bureaucrat holds: tenured or un-tenured. We find similar results to our original findings, with no evidence of heterogeneity in outcomes.

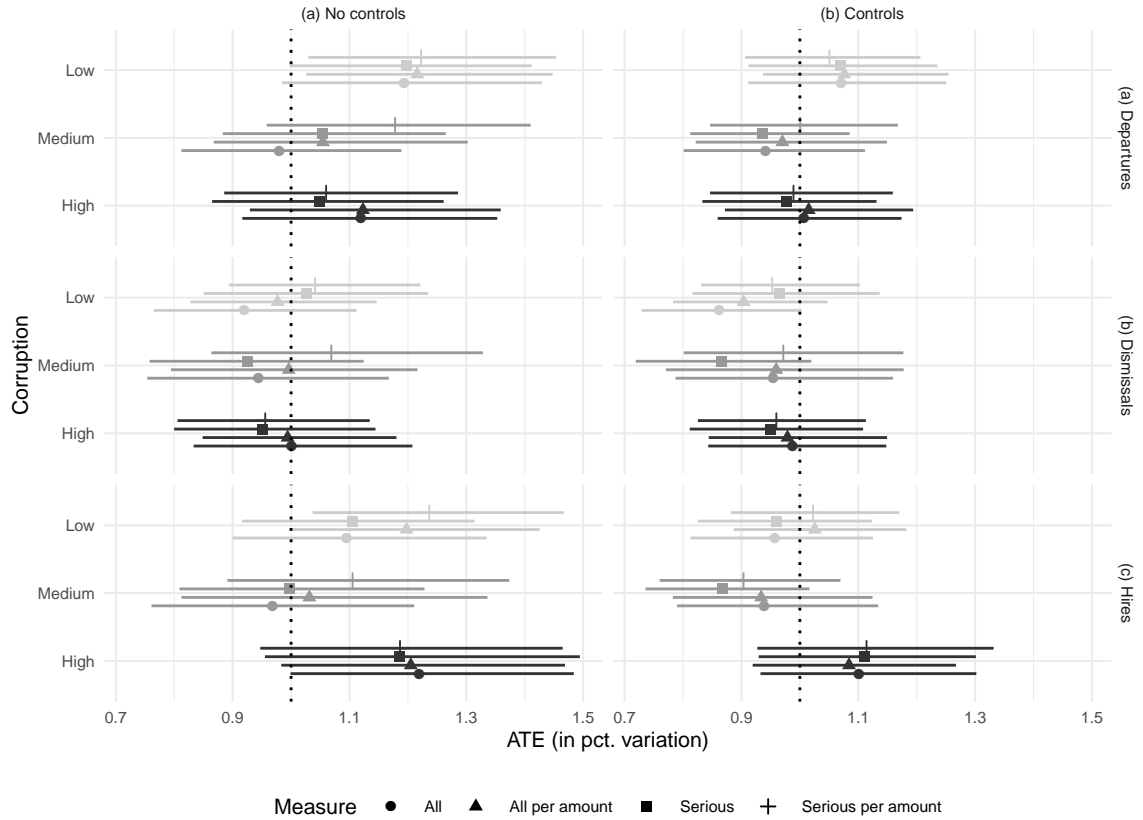


Figure 2.19: **Subset of tenured bureaucrats.** This figure reproduces Figure 2.2 in the main text but splits the sample of highest-ranking bureaucrats into tenured bureaucrats.

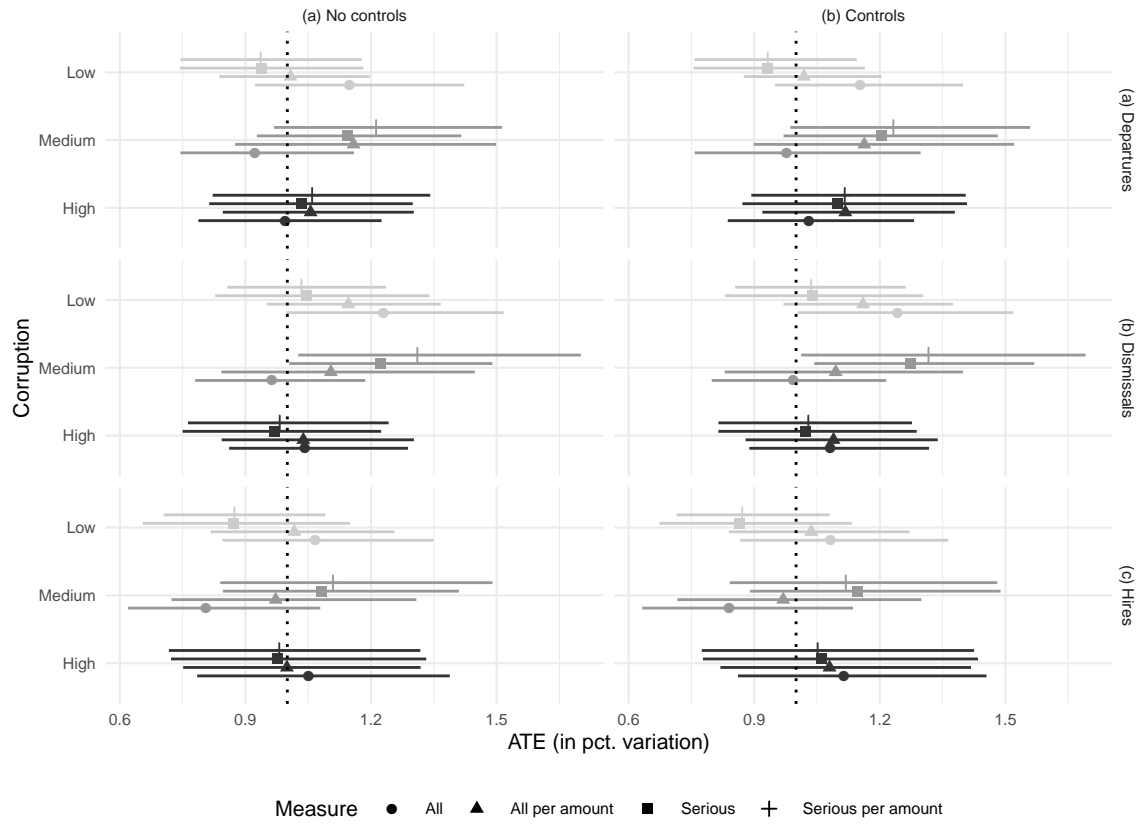


Figure 2.20: **Subset of untenured bureaucrats.** This figure reproduces Figure 2.2 in the main text but splits the sample of highest-ranking bureaucrats into untenured bureaucrats.

2.8.4 Hiring Practices

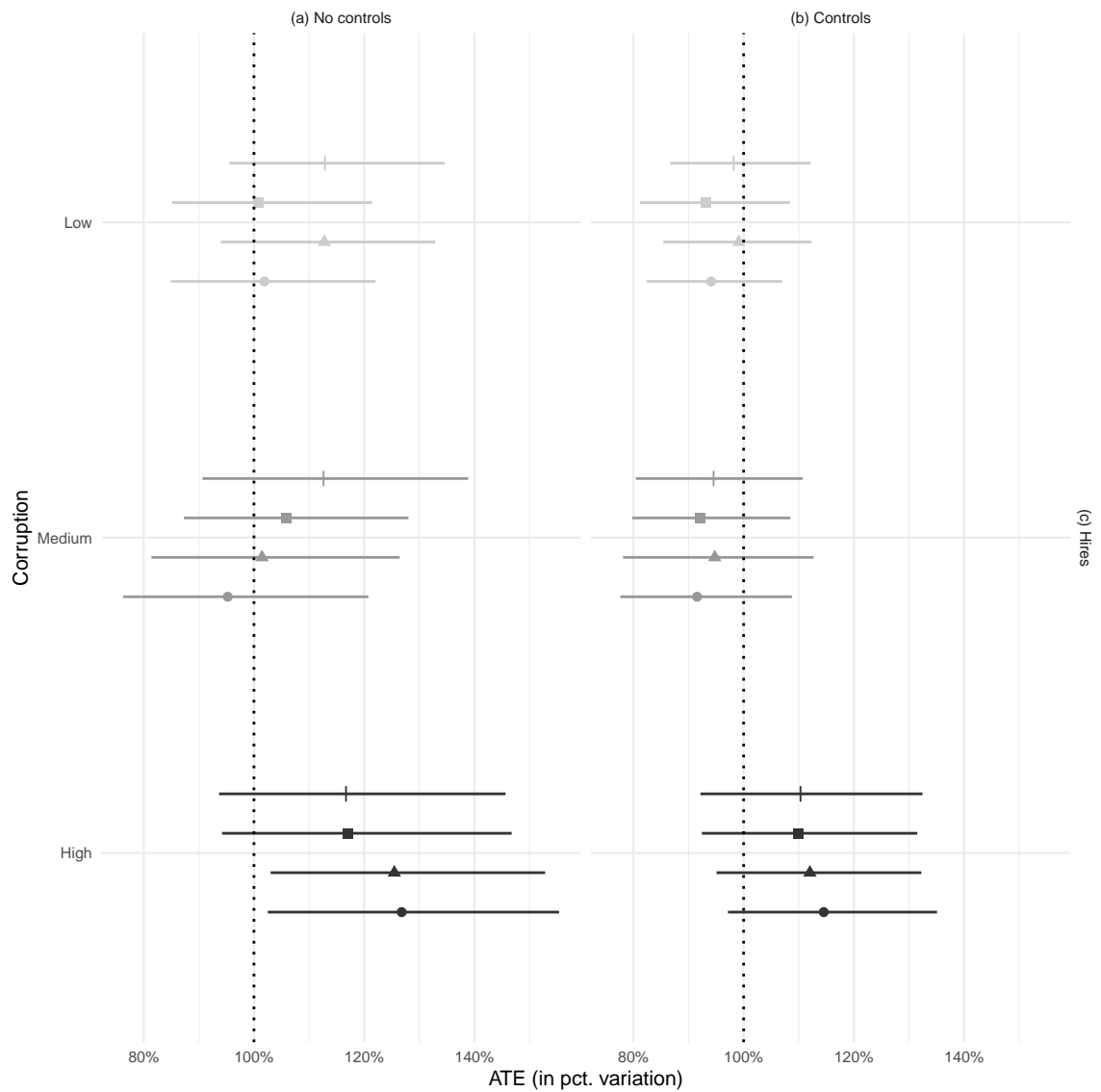


Figure 2.21: **Low bureaucrats.** This figure reproduces Figure 2.2 in the main text but considers only hires.

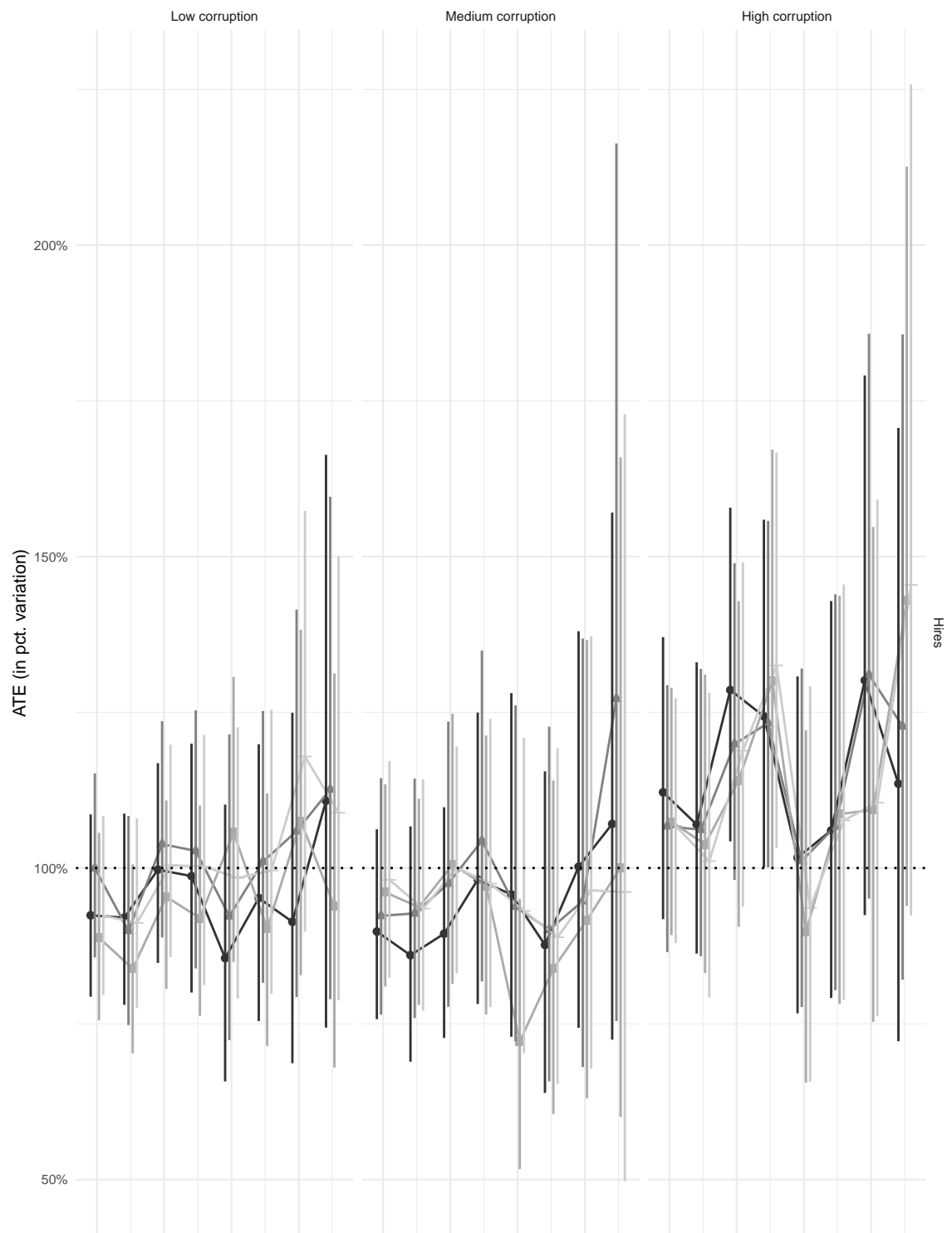


Figure 2.22: **High Frontline.** This figure reproduces Figure 2.3 in the main text but considers only hires.

2.8.5 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 2.6: **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.

2.8.6 Dependent variable as percentages

This robustness check changes the dependent variable. Instead of using log counts, we follow ? 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:

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

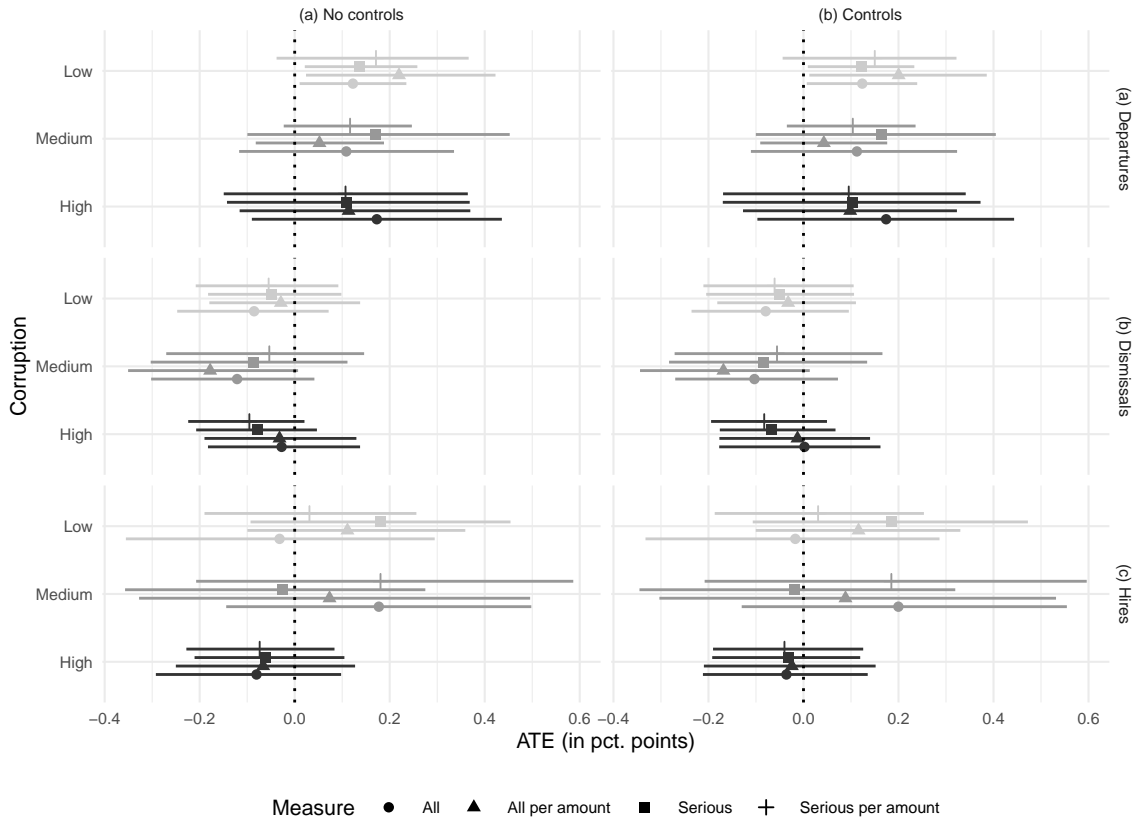


Figure 2.23: **Dependent variable specified as percentages.** This figure reproduces Figure 2.2 in the main text but specifies the dependent variables as percentages instead of log-counts. Findings are robust to this alternative specification of the dependent variable.

2.8.7 Findings on management

In this section, we reproduce the bottom panel of Figure [2.2](#) 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.

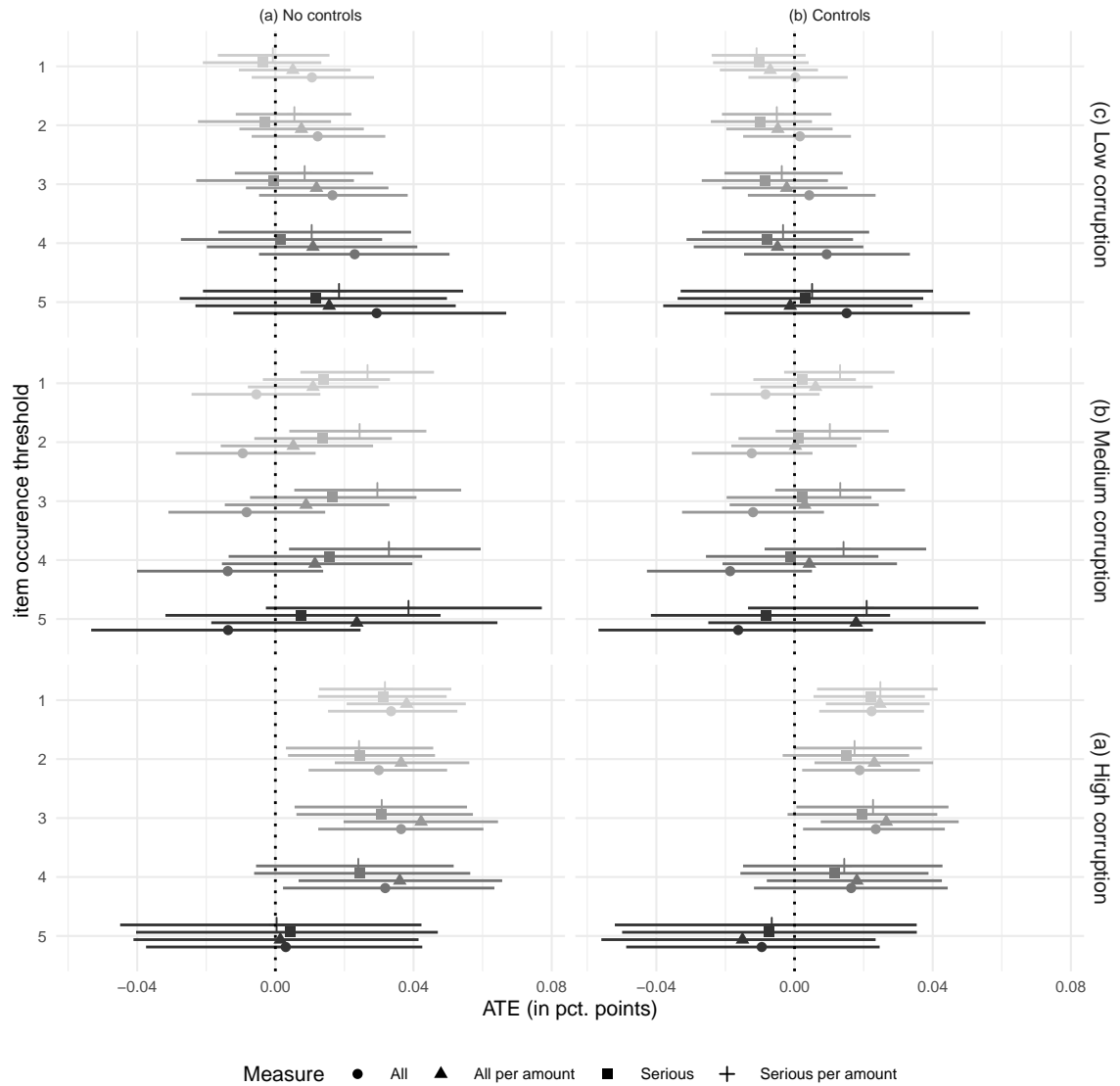


Figure 2.24: **Robustness of findings on management to alternative specifications of the management index.** This figure reproduces the bottom panel of Figure 2.2, but varies the the minimum number of occurrences necessary to include an item in the management index from 1 (threshold used in Figure 2.2) to 5. Findings are robust up to a threshold of 3 occurrences.

2.8.8 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.

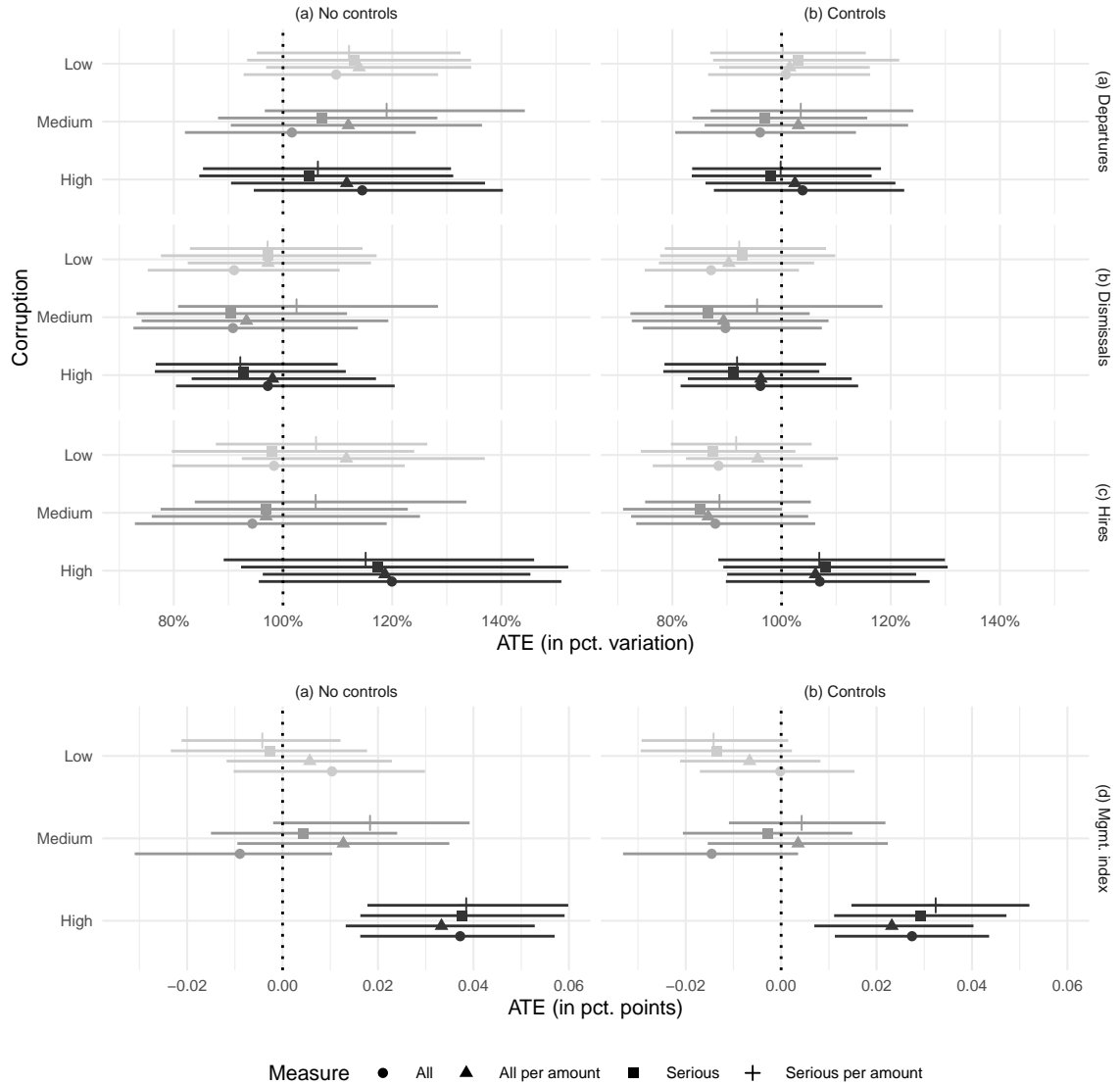


Figure 2.25: **Subset of never audited municipalities only.** This figure reproduces Figure 2.2 in the main text but considers only those municipalities that have never been audited before 2006.

2.8.9 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 investigate 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 2.1 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.¹⁷ 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 2.7 reports the results, and confirms that audit have no discernible ef-

¹⁷That 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.

fect 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).

	(1)	(2)	(3)	(4)
	All faults	Serious faults	All faults normalized	Serious faults normalized
β_1 - departure				
treat (β_{11})	0.136 (0.96)	0.0777 (0.57)	0.0835 (0.68)	0.204 (1.53)
moderate corruption	0.462* (2.16)	0.348 (1.84)	0.153 (0.67)	0.284* (2.05)
high corruption	0.241 (1.02)	0.223 (0.97)	0.127 (0.68)	0.539* (2.39)
treat \times moderate corruption (β_{21})	-0.412* (-2.23)	-0.180 (-0.95)	-0.368 (-1.81)	-0.255 (-1.48)
treat \times high corruption (β_{31})	-0.0119 (-0.05)	-0.156 (-0.69)	-0.206 (-1.25)	-0.867*** (-3.75)
β_2 - dismissal				
treat (β_{12})	-0.0109 (-0.08)	0.224 (1.50)	-0.165 (-1.08)	-0.125 (-0.81)
moderate corruption	0.221 (1.42)	0.351* (2.32)	-0.0332 (-0.17)	0.266 (1.35)
high corruption	0.104 (0.42)	0.339 (1.39)	-0.414* (-2.03)	0.0762 (0.34)
treat \times moderate corruption (β_{22})	-0.138 (-0.68)	-0.492* (-2.25)	0.0290 (0.14)	0.0779 (0.37)
treat \times high corruption (β_{32})	-0.187 (-0.81)	-0.650* (-2.53)	0.273 (1.34)	-0.127 (-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 2.7: **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 χ^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).

2.8.10 Political models with other corruption metrics

In this section, we reproduce the model reported in Figure 2.4 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 occurred. 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

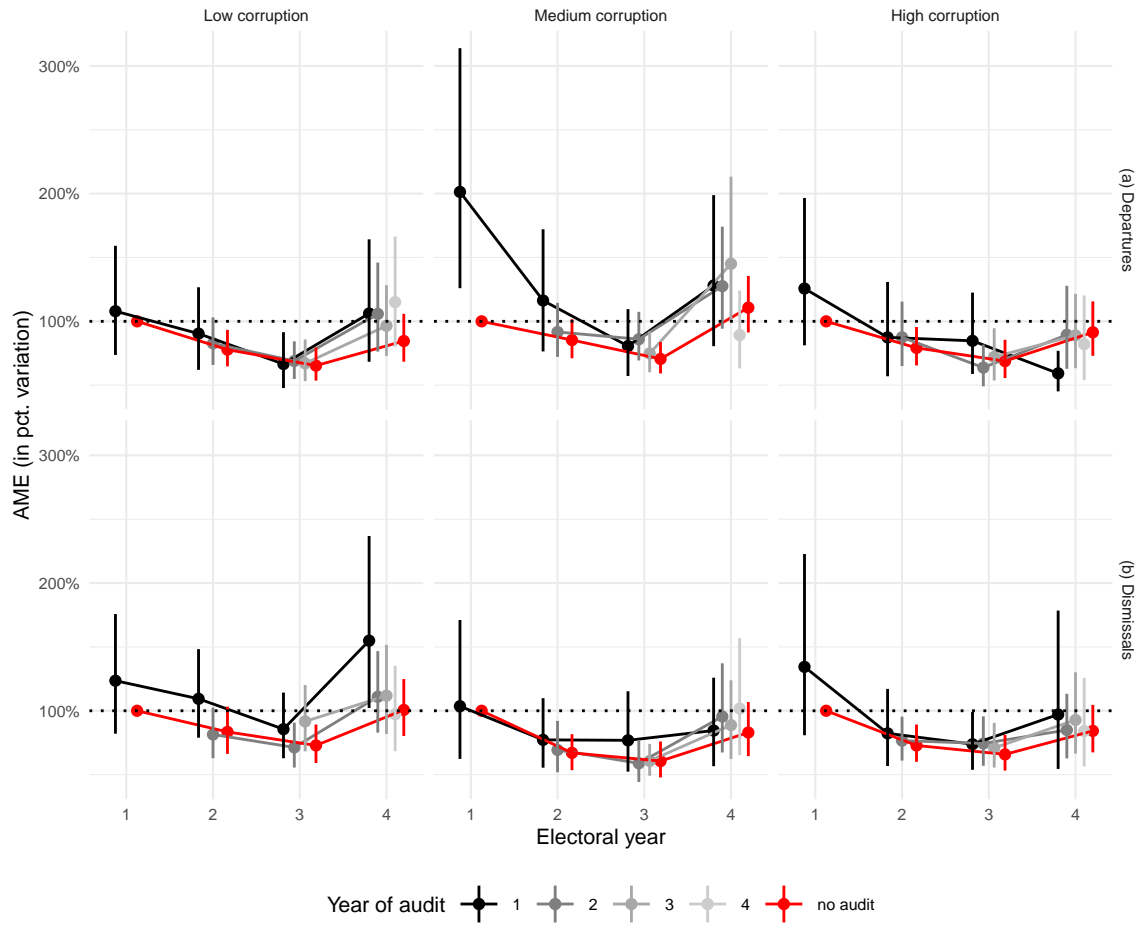


Figure 2.26: Treatment effect as a function of the political cycle during the incumbent's term, corruption = # serious faults. This figure reproduces Figure 2.4 in the main text but uses # serious faults as a measure of corruption.

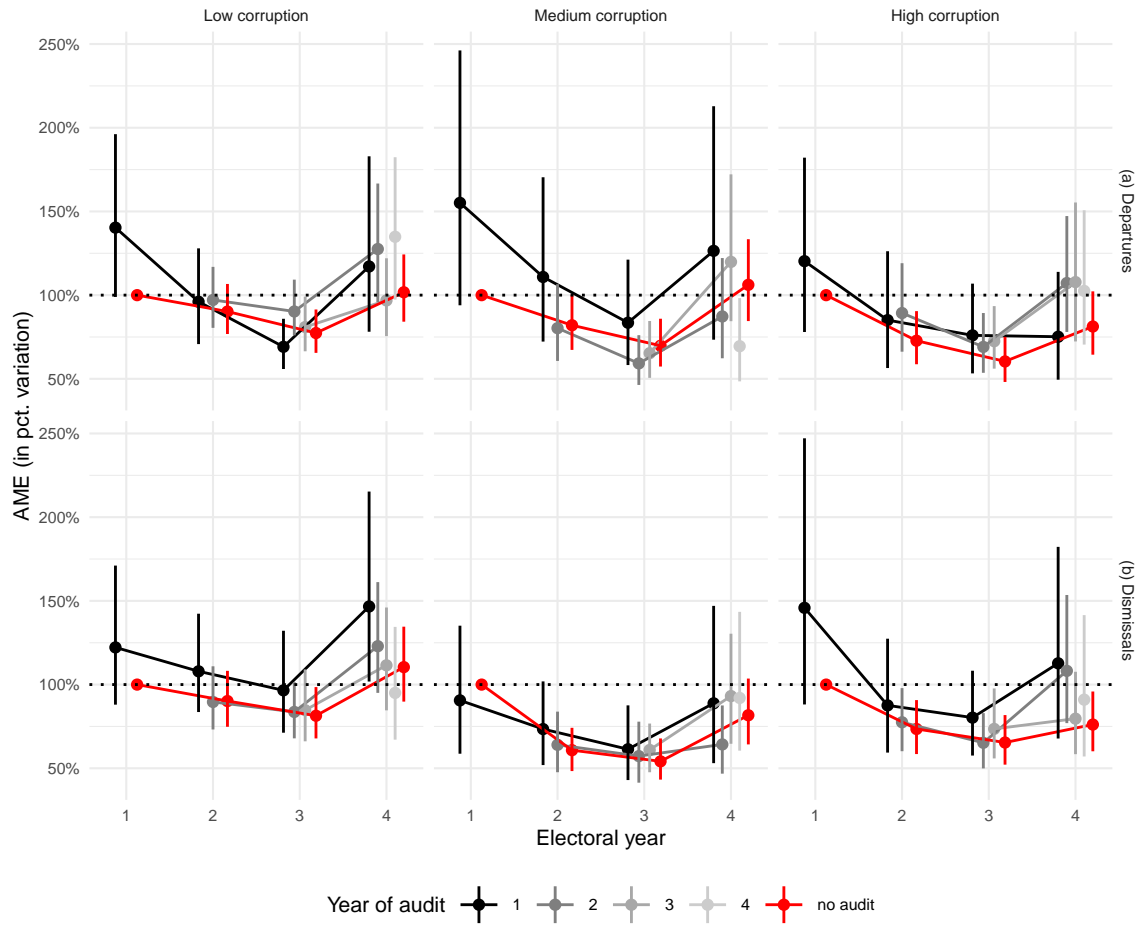


Figure 2.27: **Treatment effect as a function of the political cycle during the incumbent's term, corruption = total # faults per amount audited.** This figure reproduces Figure 2.4 in the main text but uses total # faults per amount audited as a measure of corruption.

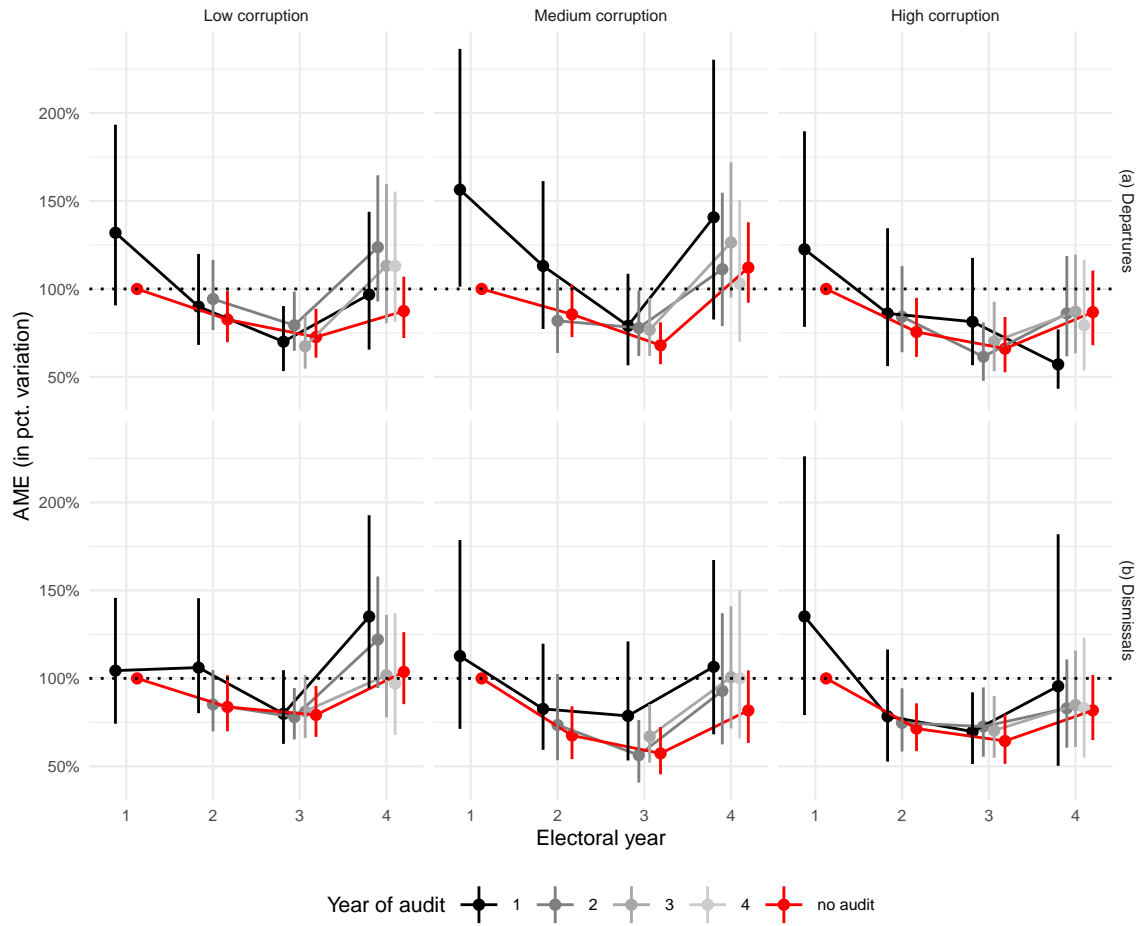


Figure 2.28: **Treatment effect as a function of the political cycle during the incumbent's term, corruption = total # faults per amount audited.** This figure reproduces Figure 2.4 in the main text but uses total # faults per amount audited as a measure of corruption.

Second term

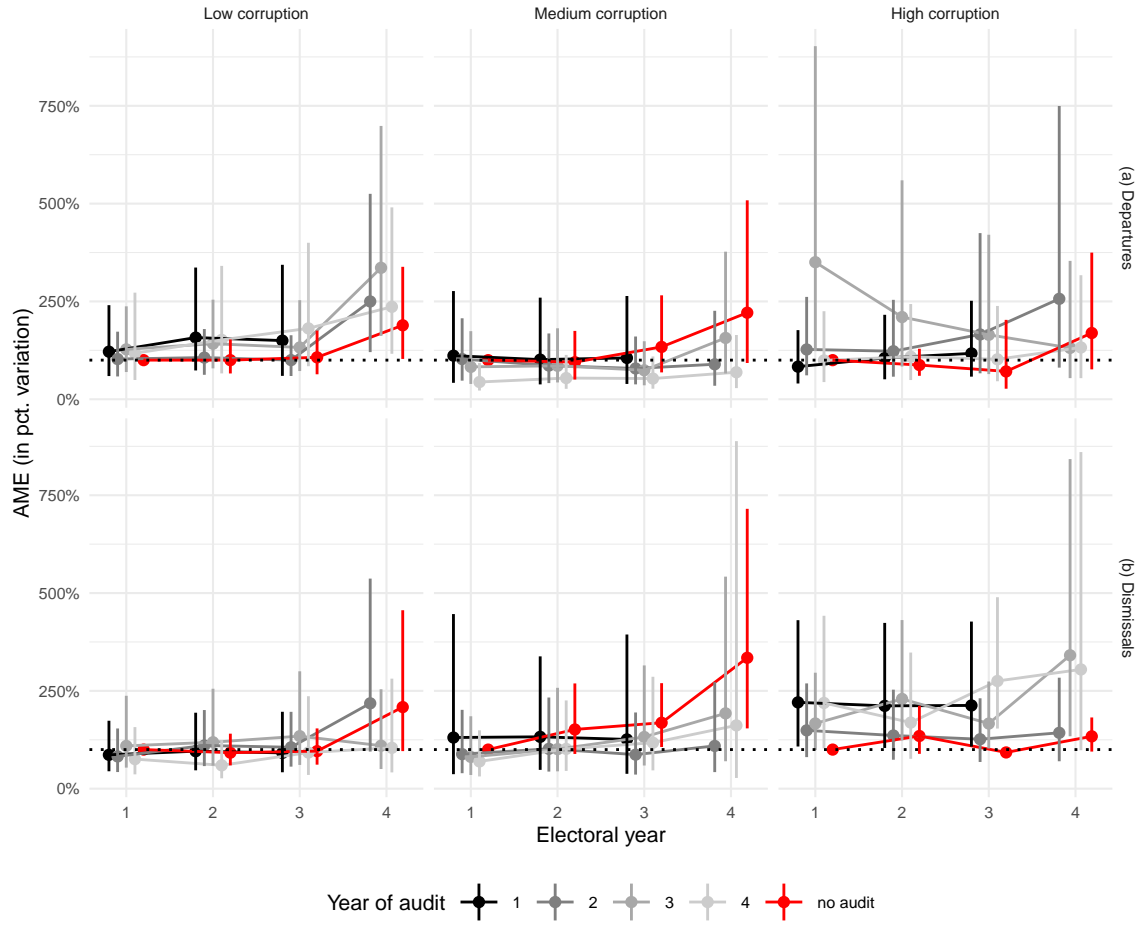


Figure 2.29: **Treatment effect as a function of the political cycle during the challenger's term** The y-axis represents the average marginal effect of audits the row outcome. The x-axis represents years in the political cycle, with year 1 being the first year of mandate. Colors indicate the year of 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 2.3.1. Again, we find no evidence that anti-corruption audits induce any changes in bureaucratic personnel.

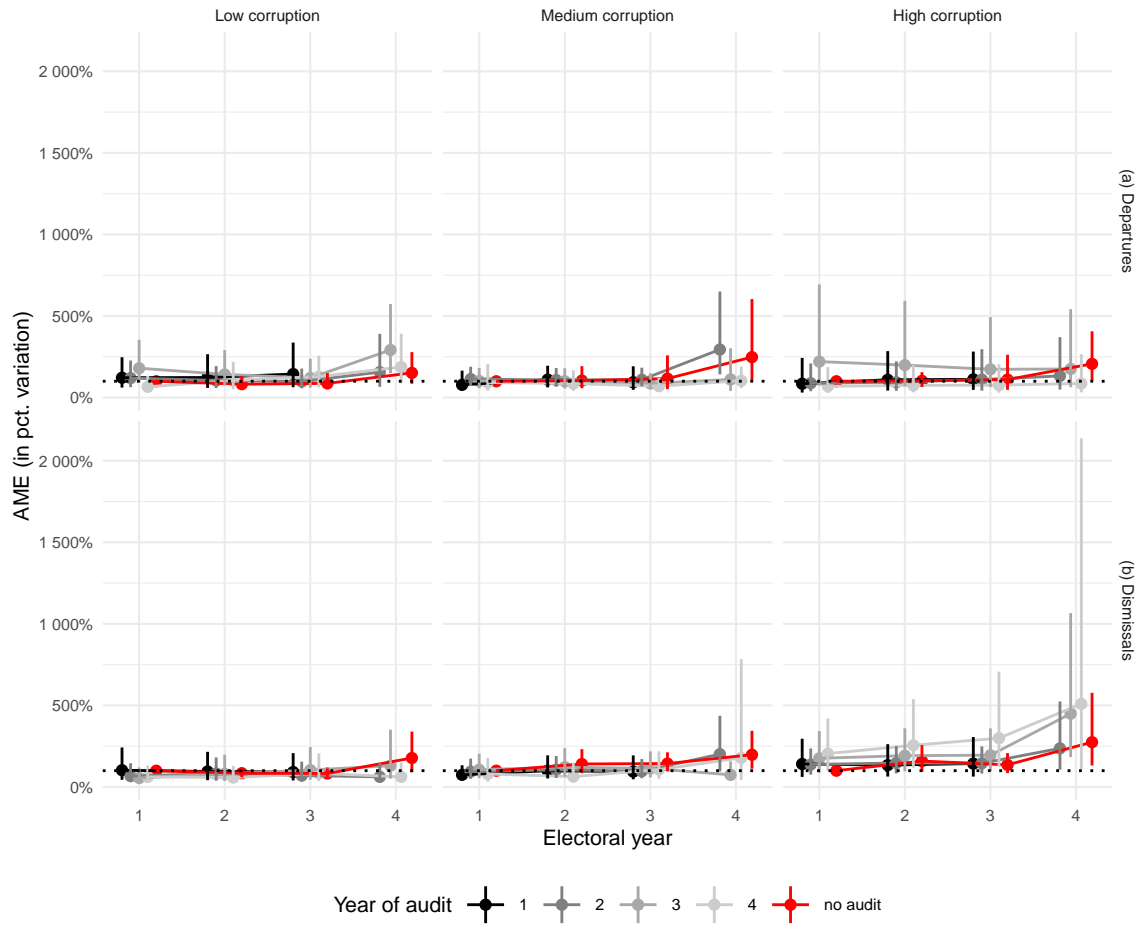


Figure 2.30: **Treatment effect as a function of the political cycle during the challenger's term, corruption = # serious faults.** This figure reproduces Figure 2.29 above but uses # serious faults as a measure of corruption.

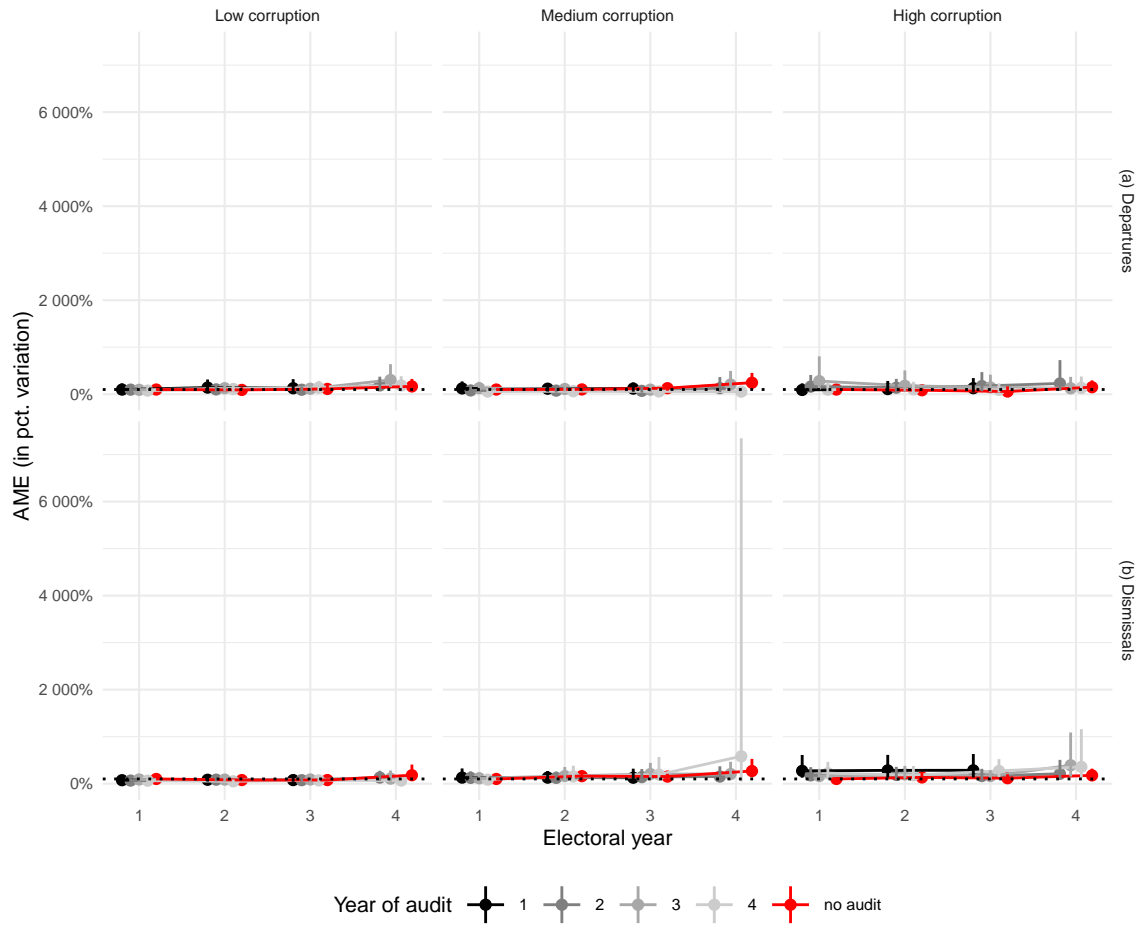


Figure 2.31: **Treatment effect as a function of the political cycle during the challenger's term, corruption = total # faults per amount audited.** This figure reproduces Figure 2.29 above but uses total # faults per amount audited as a measure of corruption.

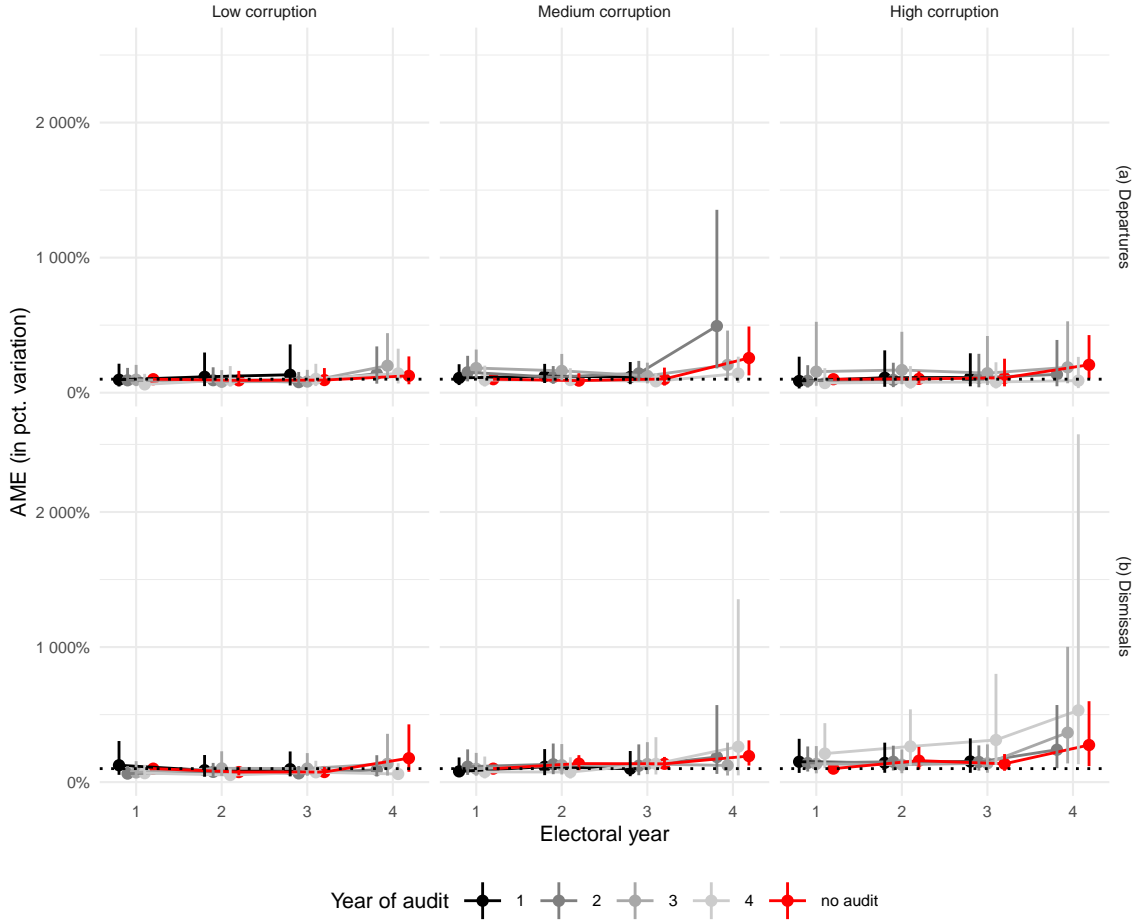


Figure 2.32: **Treatment effect as a function of the political cycle during the challenger's term, corruption = total # faults per amount audited.** This figure reproduces Figure 2.29 above but uses total # faults per amount audited as a measure of corruption.

2.9 Estimation and validation of the DDC model

In this section, we first report the auxiliary models used in estimating the DDC model reported in Section 2.5, and show results from a validation exercise (Figure 2.33).

Recall that the vector of individual-level parameters is $\theta_i = (\delta_i, p_i, w_i, \bar{w}_i, k_i)$. Estimating parameter p is straightforward: since p is the probability of an au-

dit 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 2.3), state-level fixed effects, as well as deterministic time-varying variables such as age and work experience.

We estimate the discount factor δ by predicting the probability of individual i retiring from the labor market using a logistic regression. We estimate public and private-sector wages w_i and \bar{w}_i using the Blinder-Oaxaca procedure (?); i.e. we regress the (log) wages of public sector employees over predictors x_i , and estimate a second model for private sector employees.¹⁸ 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 \bar{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 \bar{w}_1 that follows a voluntary departure to the private sector wage \bar{w}_2 that follows a dismissal and obtain k_i by averaging those differences of the individual's lifecycle from the first year we observe her until retirement. That is, we have $k_i = \frac{1}{t_i} \sum_{t=1}^{t_i} \widehat{\bar{w}_2(i, t)} - \widehat{\bar{w}_1(i, t)}$, with t_i the number of years until i retires.

¹⁸Predictors include education, age, gender, work experience.

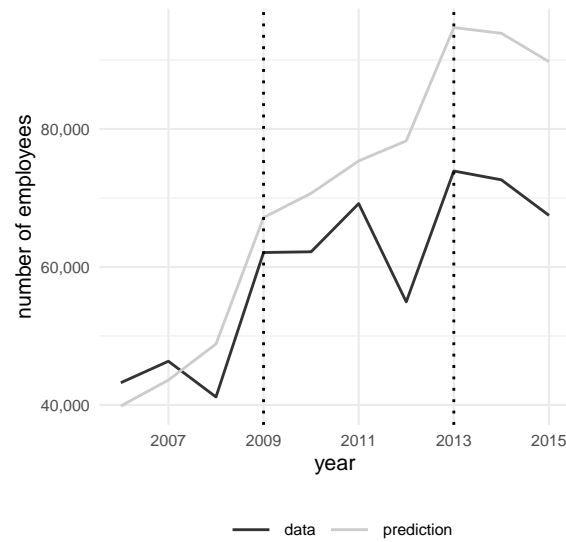


Figure 2.33: **Validation.** Comparing variation in the size of the bureaucracy as observed in the data and as predicted by our estimates, we find that predictions match reality remarkably well, except in years 2008 and 2012, which are pre-electoral years during which large waves of dismissals and departures occur.

Chapter 3

Patronage

How extensive is patronage in Brazil? What are the differences in observables between partisan affiliates and non-partisan members? Leveraging a novel dataset of partisan affiliation and employment data on every municipal bureaucrat in Brazil, I find that party members are more likely to be overcompensated than their peers, concentrating in areas of executive leadership, while being less educated than their peers. In addition, their employment spells tend to be more durable over time, leading to the build-up of party members in the bureaucracy over time. These findings provide raise important questions regarding the nature of patronage, party building and its consequences for local bureaucracies.

3.1 Introduction

Who benefits from patronage in local bureaucracies? Are there systematic differences between patronage beneficiaries and their non-partisan counterparts? And what are its implications on local public sector employment? Patronage in developing countries is a wide-spread and well-documented phenomenon (??). Previous research on the topic has shown that the prevalence of patronage appointments in the bureaucracy can lead to lower economic development (??), as well as instability in the execution of policy and the primacy of short-term political gains over long-term provision of public goods (?).

Empirical research on allocation of public sector jobs has consistently found that political motivations are an important determinant of public employment in the developed and developing world (?). In the United States, appointments to the federal bureaucracy involve considerations of party loyalty and ideological alignment with the president (??). In Brazil, whether it be at the presidential (?) or at the municipal level (??), politicians have discretion over bureaucratic appointments and frequently use these to further their political goals.

A set of explanations have been proposed to explain the determinants of this allocation, whether it be to reduce frictions in policy implementation due to ideological divergence (?), exploit the benefits of strong ties (?) or to reward followers for campaign contributions (?). These findings have provided important insights into the drivers of patronage appointments into employment in the public sector. Yet the literature has been relatively silent about the characteristics of those entering the bureaucracy (?). Who becomes a bureaucrat? Are political appointees systematically different from their non-partisan counterparts? And where, within the bureaucracy, do these patronage appointees go?

In this paper, I focus on party membership as a determinant of not only whether or not individuals enter into public sector employment, but the type of employment they receive. In particular, using a rich panel data set of all public sector workers, I identify the party membership of all municipal bureaucrats

in Brazil to provide a novel set of empirical findings: 1) the pre-bureaucracy characteristics of public sector workers, 2) employment trajectories within the bureaucracy and 3) post-public sector employment of party members. This complete revolving-door of party and non-party members provide a unique frame-by-frame evolution of the employment trajectories of pre- and post-bureaucrats in a developing world context.

The main finding is that patronage primarily benefits a local economic elite, accruing to the richest formal sector workers and allocating them into the best-paying jobs in the municipal bureaucracy. These higher compensation structures are not commensurate to skills on a set of observable qualifications, such as education level and work experience, suggesting that these benefits accrue from channels other than individual skills. Additionally, party members accumulate jobs at higher levels of government such as executive leadership and administration, which are better compensated, as well as benefiting from longer income streams due to privileged access to tenured contracts.

These findings suggest that, overall, patronage does not necessarily accrue to poor voters, contrary to theoretical and empirical findings in studies of clientelism in the developing world (?). A different logic seems to be at play. Patronage to party loyalists can be used strategically as a mean of securing control over the bureaucracy – through tenured contracts – as well as securing buy-in from wealthy patrons or notables in the local economy, consistent with empirical findings by ?. Patronage therefore can serve a crucial role in securing access to economic resources through wealthy patrons, which in turn can be allocated to finance campaigns and other exercises in party building.

This paper contributes to literature on clientelism that outlines the political logic of patronage allocations. The contribution is twofold: 1) first, I find that patronage is a patron-elite game, generating a form of patronage that is distinct from the politician-voter nexus that has been traditionally the focus of extant literature on clientelism ?? and 2) I find that the benefits of patronage go beyond simply an electoral pay-off. Instead, what patronage accomplishes is securing access to economic resources that can be tapped into once employment is offered to wealthy patrons, similar in spirit to the theoretical findings by ?.

Patronage binds parties and patrons, in effect capturing the benefits of public sector employment to finance efforts towards party-building.

The paper is structured as follows. Section 3.2 provides context for the data and the hiring process for local bureaucracies, while section ?? provides some descriptive statistics outlining the differences between partisan and non-partisan members. Section ?? outlines the empirical strategy. Section 3.5 concludes.

3.2 Context and Data

3.2.1 Brazilian municipal government

Brazil is a federal republic comprised of 26 states and over 5500 municipalities. Each municipality is headed by a mayor and city councilors (*vereadores*). Local officials are elected for a four-year term, with reelection. Each local election takes place at the same time, in October, with the new administration taking office in January of the following year. With decentralization embedded in the enactment of a new constitution in 1988, local municipalities were given large autonomy with respect to public services such as education and healthcare, as well as building an administrative infrastructure to oversee its daily operations (?).

Municipal budgets are financed with a mix of federal transfers and local revenues. Smaller municipalities tend to rely on federal transfers, which are subject to oversight by higher levels of government, either federal or state, but in practice are under large discretion by municipal governments. The past two decades have seen a growing share of expenditures social service concentrated by municipalities, which has led to expansion in access to and the quality of local services (?). Additionally, laws and regulations concerning local economies and society are largely autonomous and instituted by the mayor's office, subject to revision and approval by the local city council (?).

Finally, it municipalities are in their vast majority quite small, with approximately 90 percent of municipalities having a population of less than 50

thousand people. In contrast, the 27 state capitals (including Brasília) concentrate over 23 percent of the Brazilian population.¹ This largely uneven concentration of the Brazilian population across its municipal governments, as well as the prevalence of small, poorer municipalities, has led observants to conclude that municipal politics is often characterized by clientelism, with local political elites concentrating power through the strategic use of public resources (?).

3.2.2 Municipal employment and patronage in Brazil

Municipal employment in Brazil is under local jurisdiction, with personnel appointments under the exclusive authority of the executive branch. Salaries, contract modalities and terminations are also under municipal jurisdiction. There are multiple forms of contract available, a permanent contract (*estatutário*), a regular contract (*CLT*) and temporary hires. Personnel expenditures cannot exceed a ceiling of 60 percent of the local budget, but as long as the ceiling is not exceeded, other levels of government cannot interfere with local personnel decisions.

There are no civil service laws regulating municipal employment. With the exception of permanent contracts, municipal employees are subject to the same labor laws as private sector workers. The lack of a civil service system at the local level has been the subject of extensive research (?), and the high turnover that are associated with municipal employment have been widely documented (?). Labor unions do exist, in particular in the educational sector, but these are regional in focus and concentrated in metropolitan areas. According to the latest education census (*Censo do Magistério*), approximately 11 percent of educational staff was unionized.

Municipal discretion leaves ample room for patronage, whether it be to support mayoral coalitions as outlined in the first paper of this dissertation or to reward contributors and party loyalists (??). These politically motivated appointments are particularly prominent in high-level positions, the so

¹See coverage [here](#).

called *cargos de confiança*, local ministerial positions that are both high in compensation and provide access to decisions over key public services such as transportation and health. Regarding the nexus between party membership and public sector employment, ? finds that party members who are politically aligned with the winning mayor are 30 percent more likely to receive public sector employment than their runner-up counterparts.

These empirical findings suggest a clear political nexus between employment into local bureaucracies and political motives in Brazilian municipalities. This paper aims to provide an extensive treatment of the differences in the observable characteristics of party members - as opposed to their non-party members.

3.2.3 Partisan affiliation in Brazil

Partisanship in Brazil is voluntary and widespread, with over 11 percent of registered voters affiliated to a party (?)². Registration occurs in the following sequence: a voter reports to a municipal party office, and the party officials then register the voter officially through the *Tribunal Regional Eleitoral* (Regional Electoral Office). This registration is then collected and centralized by the *Tribunal Superior Eleitoral* (Supreme Electoral Office) and updated accordingly. If there are overlapping registrations, former ones become annulled and are reported as irregular to local party officials. Each voter is therefore only allowed to register for a single party, without any ceilings or floors regarding the duration of this affiliation.

There is an ongoing debate on the strength of partisan ties in Brazil. On the one hand, scholars have noted that partisanship in Brazil is weak, meaning that politicians and voters do not have strong party loyalty and often “switch” to other parties (??). On the other, some scholars have noted that party ties have grown in strength over time, in particular for leftist programmatic parties such as the PT (??). Part of this debate owes to disagreements on how to measure party strength, whether it be testing voters’ prior knowledge

²For context, in most OECD countries party registration does not exceed 5 percent of the electorate. See ?.

of party’s ideological positions or if instead, it should be measured by testing whether voters issue ballots for individual candidates or their party labels – with each one of these measures painting an opposite picture of the relative strength of party ties.

One thing is clear: party ties at the electoral level are durable, with many voters remaining affiliated to a single party for their entire life, as highlighted by figure *FIGURE*. Noting that party affiliations are registered at the municipal level, this empirical fact aligns with qualitative evidence provided by ?, who notes that parties at the local level constitute political factions (*grupos políticos*) with well-defined boundaries and power disputes. This relative stability of party ties at the electorate level for the minority of voters who are registered with a parties suggests a distinct dynamic tying an elite group of party members to the city hall.

3.3 Data

3.3.1 Municipal and Private Sector Employment

Data on formal sector employment is collected by the Department of Labor in Brazil through a census instrument, *RAIS*. This instrument is completed electronically by all formally registered companies regardless of workforce size or capital, subject to sanctions if evidence of misreporting is found.³ In total, over 97 percent of companies in the formal sector are included in the *RAIS*, as well as all public sector employment, including federal, state and municipal level bureaucrats. The quality of the data is subject to constant review by the Department of Labor, which relies on accurate information to calculate taxes and retirements benefits for the Brazilian workforce.

The *RAIS* data is structured as follows. Each row corresponds to a job associated to a worker, which may or may not appear more than once in the dataset – since workers can hold multiple jobs at the same time. Each worker is tagged by a unique national id, the *CPF*, which allows for following work-

³See coverage and description [here](#).

ers across sectors and over time. Additionally, the identified *RAIS* contains the names and dates of birth of workers.⁴ These are the unique primary keys that make possible join this dataset to other sources of data. Other studies have leveraged similar join approaches to infer employment benefits of campaign contributors (?), as well as whether party members receive public sector employment if they belong to the same party as the winning mayor (?).

In contrast to these previous studies, I leverage the rich dataset of covariates that includes, *inter alia*, wage compensation, education, work experience and type of contract, to estimate differential returns to compensation, outlined in greater detail in Section ???. The richness of *RAIS* allows for a complete radiography of the public sector, revealing how compensation structures are altered by their intersection with patronage dynamics. In particular, because of the panel nature of its structure, researchers can follow bureaucrats before and after their public sector employment spell, providing an unprecedentedly accurate depiction of the municipal revolving door.

3.3.2 Partisan affiliation

Data on partisan affiliation in Brazil is rich. The *TSE*, the national electoral authority, collects data on all party members in the national territory, including commencement and termination dates, affiliated party, municipality and name of the registered party member, as well as their electoral title.⁵ Parties are mandated by electoral law to report all party members as well as providing updated registry information throughout the year. Parties have municipal offices in which citizens may update their registry, and these party offices in turn report to the state electoral authority, which relays this information to national electoral authorities. If irregularities are found parties may be sanctioned by the *TSE*.

The party affiliation data is structured as a rolling registry (*fita espelho*), in which existing and expired party memberships are preserved. Each row

⁴Access to the identified *RAIS* was generously provided by the Department of Economics at Princeton University, hosted at the Industrial Relations Office.

⁵The data is available [here](#).

therefore corresponds to a particular party member entry, containing the state, municipality, electoral zone and date of the registration. Additionally, the registry indicates the status of the membership, whether it is active (*regular*) or canceled (*cancelado/desfiliado*), as well as the date of the change in the membership status. Individual identifiers are provided, including the name and a nationally unique electoral title (*título eleitoral*) that allows researchers to join names contained in the *RAIS* and party affiliation data.

Note that party affiliation in Brazil is relatively high, with over 10 percent of nationally registered voters affiliated to a political party, as compared with other advanced democracies that hover around 5 percent (?). Partisan ties at the voter level are stable, in contrast with more strategic party switching occurs at the political candidate level (?). As of 2019, the year for which party affiliation data was extracted, there were over 11 million active party affiliates, with around 5 million registries either canceled or terminated by party members.

It is important to note that the affiliation of party members has been increasing over time. Figure 3.1 illustrates this pattern. The growth in the number of partisan affiliates has been rather rapid over time. While in the year 1997 there were around 200 thousand active party members, by the year 2018 this has increased to 10 million. While the causes of this rapid increase are not clear, it is most likely due to both increased registration and better quality data on partisan affiliation as the collection of information by the national electoral authority.

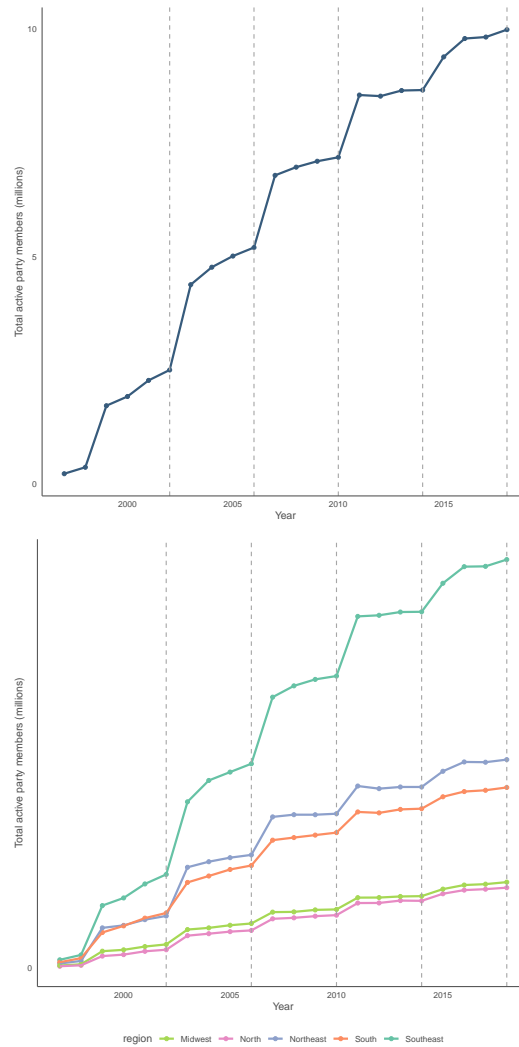


Figure 3.1: **Number of active party members by year.** Pooled (left) and disaggregated by region (right). There has been a rapid growth in the number of party members in the period between 1997 and 2020.

Another interesting feature to note in the data is that there are discontinuities in the number of party members. The dashed lines indicate the year of a local election in Brazil, such as 2002 or 2018. The jumps in party affiliation are expressive, in particular for earlier electoral cycles. For instance, in the electoral year of 2002, there were around 2 million new party members registered as a result of that particular local election. While these jumps have

decreased in more recent elections, it is an empirical regularity that highlights the electoral motivation driving party affiliation in Brazil.

Disaggregating the data further into regions, we find significant heterogeneities with respect to regional trends. The region that concentrates the most rapid growth and the largest amount of party members is the Southeast, a densely populated and most economically developed region in Brazil. It is closely followed by the Northeast, a region that although relatively poor, has been characterized by deep partisan roots that underlie patronage exchanges (?). The South, an affluent but predominantly rural region has similar levels of partisanship to the Northeast. In contrast, party membership in the Northern and Midwestern regions are low.

Finally, we present data on party membership spells in Figure 3.2. A few patterns emerge. First, the distribution of spells is uniform, with spikes every four years that mirror the jumps in partisanship in Figure 3.1. There are no clear patterns with respect to skewedness, suggesting that partisan affiliation duration is distributed in a wave-like pattern, with discontinuities that reflect the occurrence of municipal elections rather than other, long-term dynamics. Overall, the evidence on party membership spells suggests that a constant stream of party members is interspersed with local elections. However, once these membership ties are formed, they are stable and long-lasting.

3.3.3 Joining Public Sector Employment to Partisanship Data

One of the main contributions of this paper is performing a join of public sector employment data to partisanship data, to allow us to identify and follow partisan affiliates in their entire career. The key challenge in joining is the low coverage of keys that allow for a unique match across these databases. The problem is as follows. Only a subset of party members have the unique national id, *CPF*, associated with their registration, which is uniquely identified by the national electoral title (*título eleitoral*).

To maximize coverage, the party membership data is first joined to the

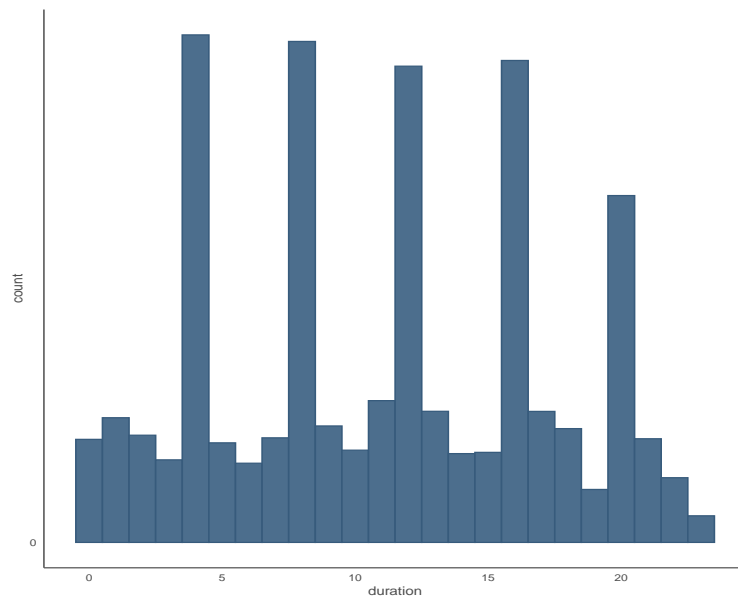


Figure 3.2: **Party membership spells.** Data includes all party memberships that start in 1985 - the Brazilian democratic transition - and 2019. Note that there are no significant skewedness in the data either to the left or to the right, indicating that there is a constant stream of party members with the exception of local election spikes.

candidacy data generated by the *TSE* for every electoral cycle using the electoral title as the key. This allows us to recover over 300 thousand national id's. Additionally, we use the complete names of both the party members and the *RAIS* employees to perform an exact name match for every municipality in the dataset for active employees and party members, by year. To prevent many to many merges, complete names are deduplicated by municipality, our smallest geographical unit. Deduplication leads only to minimal data loss: for the *RAIS* data we exclude around 6 percent of the names database per year, while for the party membership data we exclude around 5 percent.

In total, we achieve the match of 18.5 million unique matches between the the *RAIS* dataset and the party membership data. To give a sense of the scale of the data ETL (extraction, transformation and loading): in the year of 2015 there were in total 54 million employees in the *RAIS* data, while in the party membership there were 9.2 million active membership ties. Partitioning the data into manageable chunks requires the development of both a semantic layer and data pipelines that are often beyond the scope of political science and enter the domain of data science and functional programming that fall under the domain of computer science.⁶ The entire data pipeline is documented [here](#).

3.3.4 Who benefits from patronage?

Why do politicians engage in patronage? And who should politicians buy off? There are competing views on who benefits from patronage, and what is motivating it in the first place. On the one hand, it can be cheaper to buy voters who are poor, insofar as these voters need and value more monetary incentives ?. Other scholars argue that in order to maximize electoral returns, politicians target swing voters, those who are indifferent to either party and therefore, again, require the least resources to buy off ?.

The relative cost of voters is not, however, the only consideration that politicians take into place. Patronage can be used by politicians to reward political loyalists for resources they have contributed for an electoral cam-

⁶For great resources on the topic, see the following [R for Data Science](#) resource.

paign, a practice generally denoted as *prebendism*(?). In Brazil, studies have found that campaign contributors are more likely to be employed in the local bureaucracy, in the magnitude of a 30 percent increase in the probability of being hired (?). Additionally, there is strong evidence that employment in the bureaucracy is used as a spoil to reward party loyalists, again with evidence from Brazilian local bureaucracies (?).

The timing of clientelistic transfers is key. There is an important distinction to be made between different forms of clientelism such as disbursement of campaign materials (??) and another form of clientelism which can only occur after a politician takes office. In particular, a politician cannot offer job to a voter prior to holding office. Moreover, public sector jobs are coarse goods in the sense that they cannot be infinitely divided among voters. There are tight budget and administrative constraints that distinguishes a public sector jobs from other forms of resources available to politicians.

If public sector jobs are scarce, and there are only so many spots that can be filled post-election, a different form of consideration takes place from the standpoint of the politician. Differently from an electoral race in which each voter theoretically contributes the same exact amount to the race, a single vote, members of the constituency can vary widely with respect to the resources that they bring in to the table. A well established literature on the influence of money on politics has documented how campaign contributions play a crucial role in electoral outcomes in both the developed and developing world (?). What is underemphasized is the fact that politicians can use patronage to lock-in wealthy patrons into the bureaucracy and incentivize greater campaign contributions in the future, effectively securing important revenue streams for both the politician and her party (?).

While proving the second point is beyond the scope of this paper, there is strong evidence that the first dynamic, that patronage disproportionately targets wealthy patrons, is true. First, on average party members who enter the bureaucracy are wealthier than their non-partisan counterparts, in particular at the moment that the mayor assumes office. With the RAIS data, it is possible to identify for each and every employee the last wage in the non-municipal

sector prior to public sector employment. Figure 3.3 plots the median of the last non-municipal sector wage by partisanship status. The gap between partisan and non-partisan wages is the highest for the beginning of the mayoral term, which is often associated with more intense patronage activity.

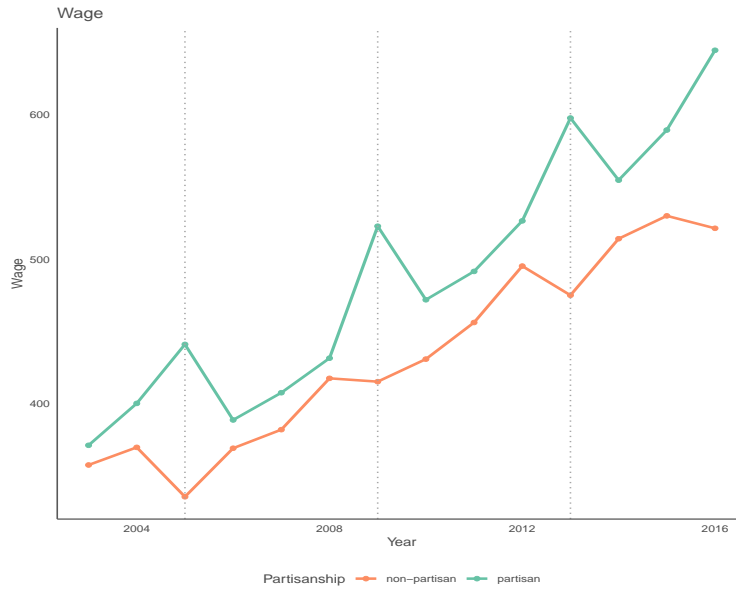


Figure 3.3: **Wage Gap between Partisans and Non-Partisans.** The graph tracks the time trend in the median wage of employees prior to entering the municipal bureaucracy.

Additionally, these same workers tend to have a longer experience working in the formal sector prior to employment. Combined with the higher compensations, the employment evidence suggests that that party members who first enter the bureaucracy in the initial years of the mandate belong to a wealthier strata than their non-partisan counterparts. They have experienced both longer bouts of employment in the formal sector, as well as higher compensations in their tenure. While it is clear that these hires are a reward for previous contributions, it is not clear whether a party member who is hired into the bureaucracy is more likely to donate to their party. In order to determine this, it would be necessary to compare party members just before and after joining the local public sector, and see whether or not they are more likely to give

campaign contributions.

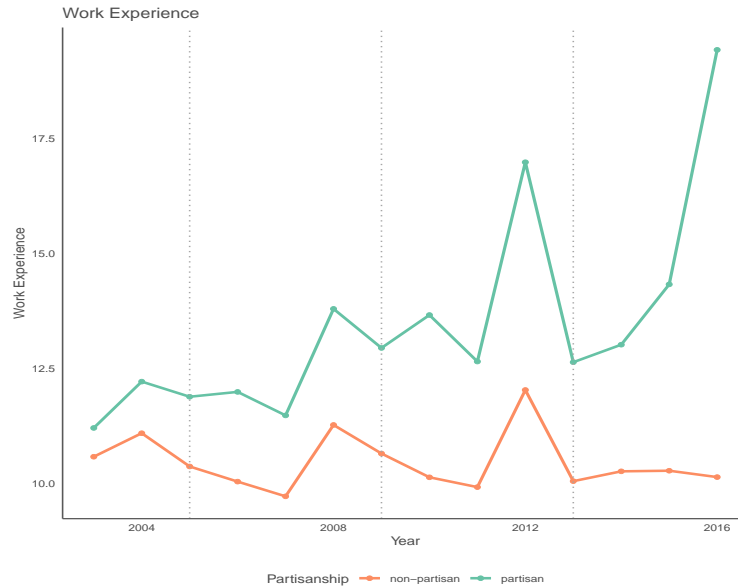


Figure 3.4: **Work Experience Differential between Partisans and Non-Partisans.** Partisan bureaucrats tend to arrive from longer work tenures in the non-municipal sectors.

In sum, employment evidence seems to suggest that party members who enter the bureaucracy in the first years of mandate come predominantly from wealthier strata of the population. Additionally, they have been working for longer time periods in the formal sector. However, on average, party members who enter the bureaucracy are less educated and older than their non-partisan counterparts, as evident in figure 3.5. Finally, while it is not clear what



Figure 3.5: **Heterogeneity in Education and Age.** Party members tend to be less educated and older than their non-partisan counterparts.

This empirical record suggests important dynamics occurring in the selection phase of the bureaucracy: who enters the public sector. Another important question is once these party members enter the bureaucracy, where do they go to. The next section explores empirical regularities with respect to

the concentration of party members once they are in the local bureaucracies, or islands of patronage.

3.3.5 Islands of Patronage

The distribution of partisan affiliation varies significantly over the national territory, with its predominance in the Northeast, as well as in the Northern regions of the country. The Midwest and Southeast appear to have the lowest prevalence of partisan affiliation. Second, that there is wide variation with respect to the prevalence of partisanship. In particular, some municipalities are almost entirely occupied by party members, with over 60 percent of its workforce affiliated to a party. Others are largely autonomous from party members, with less than 10 percent of municipal workers affiliated to a party.

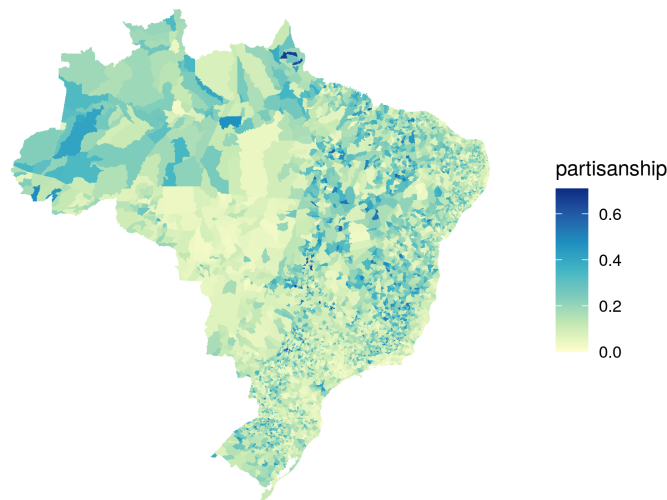


Figure 3.6: **Proportion of party-affiliated members by municipality (2015).** Darker colors indicate a larger degree of partisan affiliation. Sample includes all bureaucrats, from high-level managers to service staff.

Alongside these spatial differences, there are significant differences with re-

spect to the types of positions occupied by party members. Figure ?? breaks down the distribution of party members across different levels of the bureaucracy, using CBO (occupation) classifications provided by the RAIS data. Bureaucrat high corresponds to high-level management positions, such as directors and managers, bureaucrat low are low-level management such as supervisors and clerks. Frontline providers includes all types of service providers, where frontline high includes doctors and teachers, while low are other service providers such as janitors or security guards.

Party members are relatively concentrated in high level bureaucrat positions, such as managers and directors, as illustrated by figure 3.7. While there is wide variation with respect to the proportion of partisans, the median proportion of party members in top-level bureaucrat positions is 28 percent in the time period between These are also the best paying positions and the ones with the highest proportion of career protection in the form of contract tenure (*estatut'ario*). Moving down the hierarchy, it is clear that party members predominantly occupy high level positions, with a much larger prevalence of party members in bureaucrat low, frontline high and then frontline low, as compared to other positions in the bureaucracy.

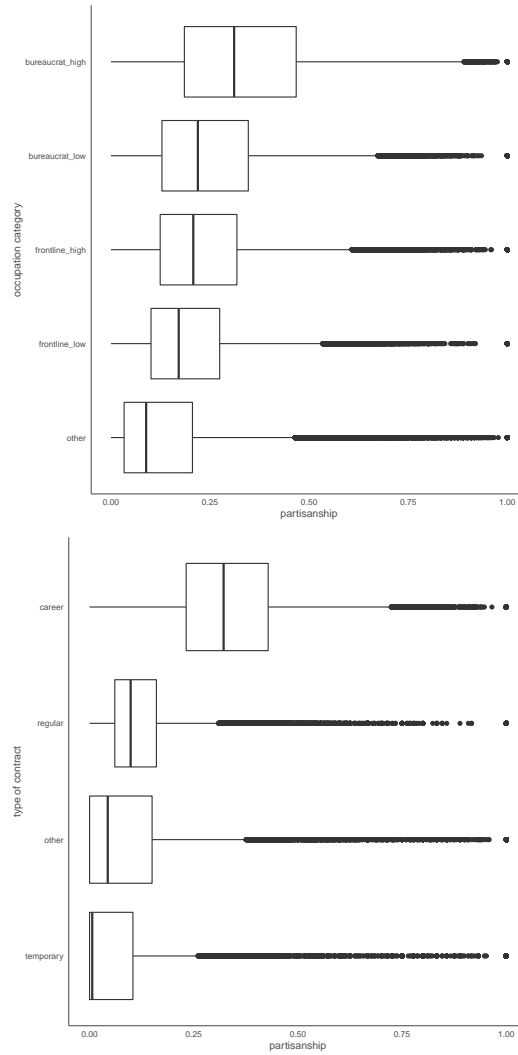


Figure 3.7: **Proportion of party-affiliated members.** On the left, breakdown of party membership by types of occupation. On the right, by type of contract.

Additionally, party members are predominantly concentrated in career positions, which are protected from dismissals and departures. In contrast, temporary and other regular types of contract subject to turnover are relatively more protected from partisan influence. This empirical finding has substantive implications with respect to the nature of patronage in Brazil and developing countries more broadly. A large body of literature around civil service systems has proposed that establishing a career track with autonomy from political pro-

cesses is a key development in protecting bureaucracies from political influence (??).

However, just because there are well established forms of career service does not mean that partisan affiliation is necessarily filtered out in the selection process. In particular, career services, with the exception of a formal requirement to do some form of public examination, does not necessarily prevent bureaucrats from joining a political party - and seldom do so. In fact, career services only structure the progression or retention of bureaucrats once they are in civil service, meaning that even if these positions are in theory autonomous from politically motivated turnover, these mechanisms do not prevent the selection of individuals who are already affiliated to a party from joining the ranks of career bureaucrats.

Finally, party members once in the bureaucracy both earn more and stay for longer than their non-partisan counterparts. Figure 3.8 illustrates these trends. Between 2003 and 2017, party members earned an average premium of around 22 percent over their non-partisan counterparts. Additionally, they have worked for longer in the bureaucracy as well, with an average 16 percent higher tenure in the municipal government. Note that while we aggregate these different figures across the years, there is widespread variation across municipalities in these different premiums. Unpacking these average values is an important avenue for future research.

In sum, the evidence suggests that party members concentrate in the upper echelons of the local bureaucracy. This reinforces priors regarding the control of bureaucratic management by politically appointed bureaucrats in a quantifiable way and in the context of a large, developing country with decentralized structure of local governance (?). Connecting this empirical evidence with the record that the majority of party members are coming from the upper strata of the formal economy, it is clear that party members are in fact transiting between the upper echelons of the private and public economies, accruing important financial gains in the process and over the long run.

This raises the question of what are the broader implications of local party organizations on the compensation and incentives for its members to join. Is

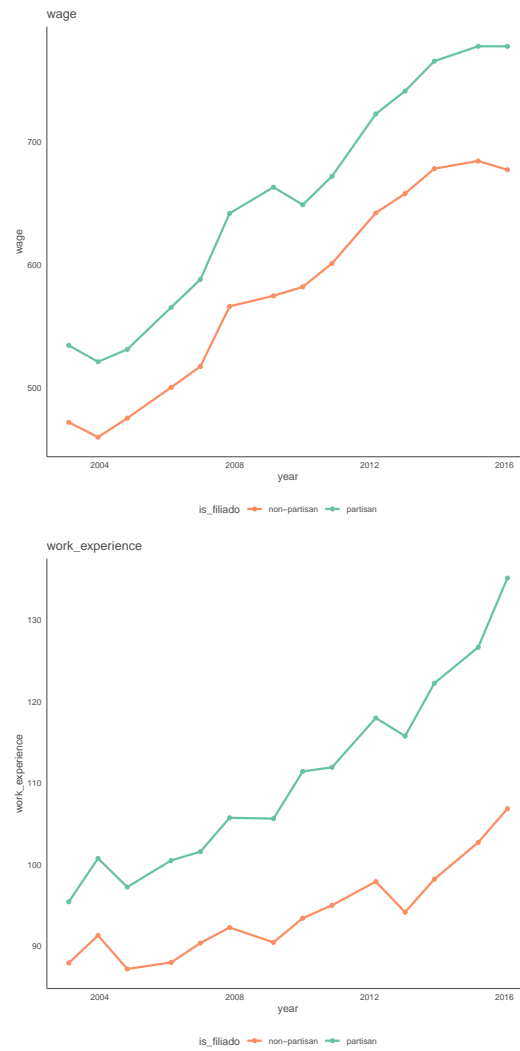


Figure 3.8: **Partisan trajectories in the bureaucracy.** Party members earn more while working in the bureaucracy (left) and also tend to work for longer (right).

this simply a club good that provides access to public sector employment? And why are only members of the wealthier classes being recruited? In the next section, I explore the incentives for parties to recruit the local elites for running campaign elections. Brazil contains detailed information on party contributions that I leverage to explore the motivation for recruitment of local elites.

3.4 Campaign Finance and Wealthy Patrons

Municipal elections are expensive to finance. In 2016, municipal races commanded an average amount of X amounts of dollars. In order to pay for these electoral races, politicians rely on a variety of resources, and at the local level, these are primarily party-member donations. In particular, while in the larger municipalities there is a greater reliance on corporate donations, for the majority of smaller municipalities, the contribution are in large part individual-level contributions. Securing access to wealthy patrons willing to donate is key

Figure x illustrates this pattern. It shows for the year 2008, the only year in which there is data on the electoral title available, who donates to politicians.

3.5 Conclusion

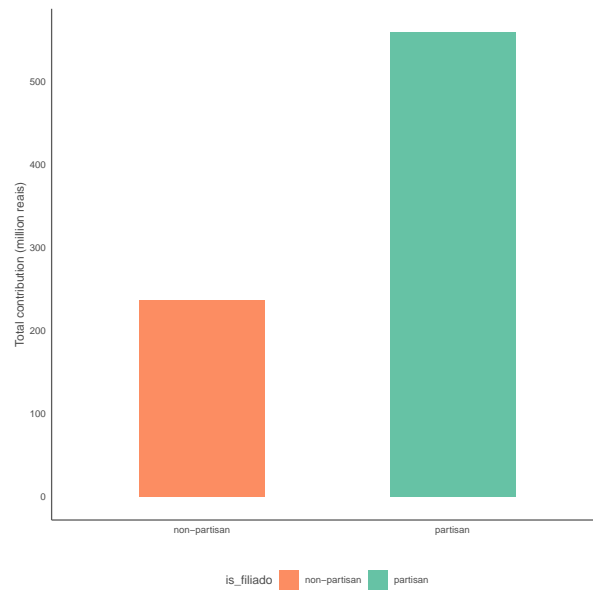


Figure 3.9: **Breakdown of Individual Donation by Party Membership.** We