

Do Government Audits Reduce Corruption?

Estimating the Impacts of Exposing Corrupt Politicians*

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Abstract

Political corruption is considered a major impediment to economic development, and yet it remains pervasive throughout the world. This paper examines the extent to which government audits of public resources can reduce corruption by enhancing political and judiciary accountability. We do so in the context of Brazil's anti-corruption program, which randomly audits municipalities for their use of federal funds. We find that being audited in the past reduces future corruption by 8 percent, while also increasing the likelihood of experiencing a subsequent legal action by 20 percent. We interpret these reduced-form findings through a political agency model, which we structurally estimate. Based on our estimated model, the reduction in corruption comes mostly from the audits increasing the perceived threat of the non-electoral costs of engaging in corruption.

Keywords: Corruption, Audits, Political Selection, Political Accountability, Judicial accountability

JEL: D72, D78, H41, K42, O17.

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1 Introduction

Politicians throughout the world embezzle billions of dollars each year, and in so doing induce the misallocation of resources, foster distrust in leaders, and threaten the very pillars of democracy (Rose-Ackerman, 1999). And while the adverse consequences of corruption have been long recognized, there is little consensus over how best to fight it.¹ One point of growing emphasis in the literature has been the importance of political institutions that constrain rent-seeking, and in particular the role of elections in selecting and disciplining politicians.² Another strand of the literature has instead focused on the effectiveness of a country's judicial and prosecutorial institutions: If severe enough, the legal consequences of rent extraction should also discipline politicians (Becker, 1968; Becker and Stigler, 1974).

Although a successful anti-corruption strategy is likely to include reforms to strengthen both sectors, the efficacy of these institutions ultimately depends on a government's ability to detect corruption in the first place. This has led several countries to adopt audit programs aimed at uncovering the misuse of public resources, which not only increase the probability of detecting wrongdoing, but also provide the requisite information to both voters, as well as prosecutors, to hold politicians accountable for malfeasance.

In this paper, we investigate the role government audits play in reducing political corruption among local government through the promotion of electoral and judicial accountability. We do so in the context of Brazil's anti-corruption program which began in 2003 and has since audited 1,949 municipalities at random, many of which multiple times. Consequently, for several rounds of later audits, we are able to compare the corruption levels discovered among the municipalities that are being audited for the first time (*control group*) to the corruption levels of municipalities that had also been audited in the past (*treatment group*). Because municipalities are selected at random, this simple comparison estimates the causal effects of a past audit on future corruption levels, in a setting in which both groups face the same ex-ante probability of being audited.

We find that corruption levels are approximately 8 percent lower among treated municipalities compared to control municipalities. According to most political agency models, whether a municipality has been audited in the past should not have long-term consequences on rent-seeking. If mayors have a two-term limit and are perfectly informed about the probability of an audit, the experience

¹See for example Fisman et al. (2014) for estimates of wealth accumulation of politicians in India and Pande (2008) and Olken and Pande (2012) for overviews of the economics of corruption in developing countries.

²See Besley (2007) and Ashworth (2012) for reviews of agency models and Ferraz and Finan (2008), Ferraz and Finan (2011), Bobonis et al. (2015) for evidence on how elections can discipline politicians.

of an audit should only affect corruption in the following term through its selection effects. But mayors in Brazil are not perfectly informed: Although the probability of an audit for any given lottery is well defined, the number of future audits is not. Faced with this uncertainty, it is plausible that mayors update their beliefs over the audit risk through the information they acquire from their own and others' audit experiences.

Consistent with this interpretation, we find that past audits also affect the corruption levels of neighboring municipalities with local media, which are the places most likely to learn about the outcome of another municipality's audit. For these municipalities, having an additional neighbor audited leads them to reduce their own corruption by 7.5 percent. We also find evidence of smaller spillover effects across partisan networks, which is again consistent with the mayors learning based on others' experiences. The average municipality in our sample receives R\$15,000,000 in federal transfers per year. Based on our estimates of a random sample of audit reports, 30 percent of the funds audited were found to be diverted, implying that the audits reduced corruption by R\$567,135 per year per municipality, once we account for the spillover effects.

We interpret the main findings in the context of a simple model of political accountability, which we subsequently estimate. Based on our model, there are several reasons why the audits may have led to a reduction in local corruption. First, the audits may have reduced corruption through a political selection effect. As documented in [Ferraz and Finan \(2008\)](#), in places that were audited before the election, voters were able to reward good and punish bad incumbents who were up for re-election. Second, the audits may have led to a stronger electoral disciplining effect. If an audit increases a mayor's posterior beliefs of the likelihood of an audit and he has re-election concerns, then he has less incentive to engage in corruption. Of course, an unfavorable audit can also trigger other non-electoral costs, such as legal punishment or reputation costs. So even if a mayor does not have re-election concerns, an update in the probability of being found to be corrupt can lead to what we call a legal disciplining effect. Finally, the audits may have also affected the political environment more generally by inducing a better selection of candidates (i.e. an entry effect).³

We investigate these mechanisms both in the reduced-form and structurally. Despite the different assumptions underlying these two approaches, we find consistent evidence for a legal disciplining effect, with less support for the other channels. Our model estimates suggest the disciplining effects from legal costs can explain 72 percent of the reduction in local corruption. Consistent with this

³Outside of the context of our model, there are two other possible explanations for the reduction in the corruption. One interpretation for our findings is that the audits teach mayors to better hide corruption. The other interpretation is that the federal government offered fewer transfers in response to an audit, and thus made it harder for future mayors to engage in corruption. We test for both of these channels and do not find support for these interpretations.

result, we also document that an audit can generate substantial legal costs. Using data on anti-corruption crackdowns and federal convictions, we find that having been audited increases the likelihood of incurring a legal action by 20 percent.

Given our findings, we then use our structural estimates to explore four counterfactual policies aimed at further reducing corruption. First, we simulate changes in the perceived audit probability, which would occur if the program increased the frequency of audit lotteries or the number of municipalities audited in each lottery. Second, we simulate increases in the legal costs of corruption. In practice, legal costs could be increased if, for example, the judicial system imposed harsher fines or punishment for engaging in corruption. Third, we consider policies which would affect the education or occupational backgrounds of candidates running for office. Finally, given the spillover effects we document and the importance of the media in disseminating information, we simulate a policy in which all municipalities receive access to information about neighboring audits.

Based on these simulations, policies would aim at either increasing the legal costs of corruption or increasing the probability of being audited would most reduce corruption. Based on our estimated model, a 10 percent increase in legal costs would decrease corruption by approximately 9.8 percent. Similarly, a 10 percent increase in the audit probability would decrease corruption for first term mayors by 14.6 percent and second term mayors by 9.3 percent. As we discuss later, these findings are in line with other estimates reported in the literature (e.g. [Bobonis et al. \(2015\)](#), [Olken \(2007\)](#), [Zamboni and Litschig \(2015\)](#)). In contrast to these policies, we find modest effects associated with our entry and information treatments. The latter result stems from the fact that with more access to information, mayors and voters are more likely to update their priors about the audit risk in both directions. Some mayors will acquire additional information which leads them to overestimate the probability of audits, while others will acquire information which leads to underestimation. Unless mayors have biased priors or do not update their beliefs using Bayes' rule, the effects of this policy on rents will be comparatively minor compared to the first two policy counterfactuals we considered.

Our findings are related to three broad literatures. First, our study contributes to a large literature on the determinants of political corruption and the design of policies aimed at curbing corruption. For example, [Bobonis et al. \(2015\)](#) find that audit reports in Puerto Rico released just prior to the election (compared to those released after an election) induced a significant short-term reduction in municipal corruption levels, that then later dissipated in the subsequent rounds of audits. An important distinction between our studies is that in our context the audits are conducted at random, and thus politicians are not able to anticipate them. [Di Tella and Schargrodsky \(2003\)](#) examine the

effects of an anti-corruption crackdown and found that the prices paid for homogeneous supplies by public hospitals in Buenos Aires fell by 15 percent after the government began to disseminate information on prices. [Olken \(2007\)](#) implemented a randomized experiment where prior to the start of a national wide infrastructure project, villages in Indonesia were randomly assigned into groups with different audit probabilities. The study found that 24 percent of reported funds were found to be “missing”, but when faced with a certain audit this difference was reduced by 8 percentage points. [Zamboni and Litschig \(2015\)](#) investigate the effects of a randomized experiment conducted by the Controladoria-Geral da União (CGU) designed to test whether increased audit risk reduces corruption and mismanagement. In this experiment, the CGU announced in May of 2009 to 120 municipalities that in one year time, 30 of them would be randomly selected for an audit. Based on this temporary increase in audit risk of about 20 percentage points, the authors found that the treatment reduced the proportion of local procurement processes involving waste or corruption by about 20 percent. Finally, [Lichand et al. \(2016\)](#) also examine the effects of Brazil’s audit program with a focus on corruption in health. Using a difference-in-differences strategy, the study tests whether corruption is lower in municipalities that neighbor municipalities that were audited in the past. Consistent with our spillover effects on corruption across all sectors, they find that corruption in health reduced by 5.4 percent in places that neighbor an audited municipality.

We complement these studies in various ways. First, our findings suggest that audits can be an effective policy instrument for not only promoting electoral accountability, but also enhancing judicial punishment. Second, there are several motives for reducing corruption in response to an audit. In our study, we are able to decompose the effects of these various channels, and highlight the relative importance of legal costs in disciplining the behavior of politicians. Finally, another advantage of our data is the ability to distinguish between acts of corruption versus acts of mismanagement.⁴ We do not find any evidence that the audits reduced irregularities associated with mismanagement.

Our study also contributes to a body of research documenting the importance of legal institutions for economic development, and in particular corruption ([Glaeser and Shleifer, 2002](#); [La Porta et al., 2004](#)). For example, [Glaeser and Goldin \(2006\)](#) argue that reduction in corruption over time in the U.S. was due to a combination of increasing political competition, an active media uncovering corruption scandals, and an independent judiciary that successfully prosecuted corrupt officials. Also using variation across U.S. states, [Alt and Lassen \(2008\)](#) show that corruption is much lower among states in which state supreme court judges are elected rather than appointed. Finally, [Litschig and](#)

⁴Recent studies have tried to distinguish between active and passive waste. For example, [Bandiera et al. \(2009\)](#) use data on public procurement from Italy to show that more than 80 percent of waste can be classified as passive.

Zamboni (2015) exploit variation in the presence of the judiciary across Brazil's municipalities to show that corruption is lower in municipalities with a state judiciary present. In contrast to these studies, however, we show using data on the police crackdowns and convictions of politicians that audits can be a critical instrument in promoting judicial accountability. As far as we know, this is the first paper that examines how both political and judicial accountability can affect corruption.

Finally, our study also relates to a growing empirical literature that examines the relationship between electoral accountability and politician performance. There is a growing literature showing that when voters are informed, elections can discipline corrupt politicians (e.g. Ferraz and Finan (2008), Winters and Weitz-Shapiro (2013)). Similarly, a series of papers have exploited variation in term limits to show that incumbents respond to re-election incentives. For example, Besley and Case (1995) show that re-election incentives affect the fiscal policy of U.S. governors, while List and Sturm (2006) provide evidence that term limits even influence secondary policies, such as environmental policy. In relation to the Brazilian context, Ferraz and Finan (2011) find that mayors who are in their second term, and hence do not have opportunity for re-election, engage in much more corruption relative to mayors with re-election incentives. Similarly, de Janvry et al. (2012) find that Brazil's Bolsa Escola – a conditional cash transfer program that was targeted in a decentralized manner – performed much better in places where mayors had re-election incentives.

While these studies provide convincing evidence consistent with standard political agency models, they are unable to quantify the electoral selection effects that are also central to models of political accountability. Recently, some progress has been made in this direction by taking a more structural approach. Aruoba et al. (2015) estimate a model of political accountability to quantify the discipline and selection of U.S. gubernatorial elections. Using data from 1982-2012 of U.S. governors, they find that the possibility of re-election leads to a 13 percentage point increase in the fraction of governors who exert high effort in their first term in office, as measured by voters' job approval. Although set in a different context, they too find weaker selection effects: the fraction of good governors rises by 8 percentage points from the first to second term in office. Sieg and Yoon (2016) estimate a dynamic game of electoral competition with adverse selection to compute the welfare impacts of term limits. According to their model, term limits can lead to two opposing welfare effects. On the one hand, term limits can be welfare-reducing by weakening the disciplining and selection effects that elections induce. But on the other hand, term limit can also reduce any potential entrenchment effects. Also using data from U.S. gubernatorial elections, they find that the former effects dominate, and term limits reduce voter welfare by 6 percent. Our paper complements these studies by not only disentangling selection from incentive effects, but also allowing for the possibility a legal disciplining effect.

The rest of the paper is organized as follows. Section 2 provides background on the Brazil’s anti-corruption program and presents the data used in the empirical analysis. Section 3 presents the theoretical framework. Section 4 discusses our research design and in Section 5 we present our reduced-form findings. In Section 6 we discuss the estimation of the model and present our counterfactual simulations. Section 7 concludes.

2 Background and Data

2.1 Corruption in Brazil and the Randomized Anti-Corruption Program

Brazil is one of the most decentralized countries in the world. Each year, municipalities receive millions of dollars from the federal government to provide basic public services such as primary education, health care, and sanitation. An elected mayor decides how to allocate these resources in conjunction with a locally-elected legislative body. With only minimal federal oversight accompanying these transfers, corruption at the local level has been a serious concern.

Corruption in Brazil occurs through a combination of fraud in the procurement of goods and services, diversion of funds, and over-invoicing of goods and services (Ferraz and Finan, 2011). Common irregularities include incomplete public works (paid for but unfinished) and the use of fake receipts and phantom firms (i.e., firms that only exist on paper). Places that receive more federal transfers, or where the local media and the judiciary are absent, tend to experience relatively more corruption (Brollo et al., 2013; Zamboni and Litschig, 2015).

In response to widespread corruption and a lack in the capacity to systematically detect and punish malfeasance, the federal government created in 2003 Brazil’s Controladoria Geral da União (CGU) – Office of Comptroller-General. The CGU, which is functionally autonomous and possesses the constitutional powers of a ministry, centralizes all of the Federal Government’s internal control activities, and sets government directives for combating corruption. In order to establish horizontal accountability, the CGU also forms part of a complex system of federal agencies responsible for preventing, investigating, and punishing illicit acts in the political and public spheres. To this end, the Federal Court of Accounts (TCU), the Office of the Federal Public Prosecutor (MPF), and the Federal Police are responsible for inspecting, controlling, correcting and instructing legal actions taken against public administrators and politicians in cases of corruption (Speck, 2011; Power and Taylor, 2011).

2.2 The Randomized Audits Program

Shortly after its creation, the CGU launched an anti-corruption program targeted at municipal governments. The program, named *Programa de Fiscalização por Sorteios Públicos* (Monitoring Program with Public Lotteries), consists of random audits of municipalities for their use of federal funds. It originally started with the audit of 26 randomly selected municipalities across different states, but then shortly moved towards auditing 60 municipalities per lottery. The lotteries are held publicly in conjunction with the national lottery in Brasília, and all municipalities with a population of up to 500,000 inhabitants are eligible for selection.⁵ As of February 2015, there have been 2,241 audits across 40 lotteries in 1,949 municipalities and over R\$22 billion dollars worth of federal funds audited.

Once a municipality is chosen, the CGU gathers information on all federal funds transferred to the municipal government during the previous three to four years and issues a random selection of inspection orders. Each one of these orders stipulates an audit task for a specific government project (e.g. school construction, purchase of medicine, etc.) within a specific sector.⁶ Once these inspection orders are determined, 10 to 15 auditors are sent to the municipality for one to two weeks to examine accounts and documents, to inspect for the existence and quality of public work construction, and to verify the delivery of public services. These auditors are hired based on a competitive public examination and earn highly competitive salaries, thus their incentives for corruption are lower than those of other bureaucrats in the federal level administration. Moreover, the inspections are done by a team which reduces the opportunity for corruption among individual auditors.⁷ After the inspections are completed, a detailed report describing all the irregularities found is submitted to the central CGU office in Brasilia. The central unit unifies the information and publishes a report on the internet. These reports are also sent to the Federal Courts of Accounts (TCU), the Federal Prosecutors' Office (MPF), the local judiciary, the Federal Police, and to the municipal legislative branch.

Over time the program has changed in order to improve the auditing capabilities of the CGU. Because larger municipalities receive substantially more transfers, the CGU decided in August 2005 to target a limited number of randomly selected sectors in larger municipalities. For example,

⁵This eligibility criteria has changed slightly over time.

⁶Auditors do not have discretion in auditing other projects. In case they find clear evidence of corruption in their fieldwork, they need to notify the central unit of the CGU who will then decide whether to issue a new inspection order.

⁷Ferraz and Finan (2008) find no evidence that auditors manipulate the audit reports. In a recent study of Brazil's federal government, Bersch et al. (2016) found the CGU to be one of the government's most autonomous and least politicized agencies.

in the 17th lottery that took place in August 2005, the sectors chosen were social assistance, crime prevention, and industrial policies. Municipalities smaller than 20,000 inhabitants are still subject to audits in all sectors.

Although these changes affect the areas in which municipalities can be audited, they do not affect a municipality's audit probability. Lotteries are done by state so the probability of being audited is constant for municipalities within the same state. For smaller states such as Alagoas, only 1 or 2 municipalities are typically drawn in a single lottery, whereas for a large state like Minas Gerais, with over 853 municipalities, as many as 8 municipalities have been drawn in a single lottery. Once a municipality is audited, it can only be audited again after several lotteries have elapsed.⁸ Overall, as we see from Table A.1, the implied audit probabilities in any given lottery are quite small, with the average being only 1.3 percent (s.d.= 0.005) in a given lottery. But given the frequency of the lotteries, the probability of being audited in one's political term can be quite high, ranging anywhere from 8.6 percent for the state of Minas Gerais to 26.4 percent in the case of Rio de Janeiro.

Note that even though audit probabilities are known at the time of a lottery, there are two important sources of uncertainty that can affect a mayor's perception of audit risk. First, the number of municipalities audited per state changes over time and this information is only provided right before the lottery takes place. For example, consider the state of Ceará: at the beginning the program, the CGU only selected 3 municipalities per lottery. After the 9th, this number decreased to 2 municipalities, only to then increase back to 3 after the 22nd lottery. The number then changed again to 4 starting in the 34th lottery. Similar changes have occurred in other states. Second, and most importantly, due to fluctuations in the federal budget, it is extremely difficult for mayors to anticipate how many lotteries will take place during their term in office. As we document in Figure 1, the number of lotteries held per year has varied substantially over the course of the program. In some years, the program carried out as many as 7 lotteries in given year – leading to as many as 400 municipalities being audited – while in other years the program only carried out a single audit. For these reasons, it is reasonable to assume that mayors are uncertain about future audit risk.

By various accounts, the program has served as an important weapon in Brazil's fight against political corruption. The information obtained from the CGU audits has been widely used in political campaigns and in voters' selection and sanctioning of municipal politicians (Ferraz and Finan, 2008). The federal police and federal prosecutors have also exploited the audits to better target their investigations, and to help build their cases against corrupt politicians and public servants. Consequently, since 2004 Brazil has witnessed a steady increase in the number of legal actions

⁸This rule has changed over time going from 3 to 12 lotteries.

involving political corruption, evidence of which can be seen in Figure 2.

Panel A of Figure 2 plots the number of police crackdowns, called Operações Especiais (Special Operations), aimed at uncovering municipal corruption. These crackdowns, which have increased over time and to date total 199 cases throughout Brazil, are the result of a direct collaboration between the federal police and the CGU. The number of civil court cases of individuals charged with misconduct in public office has also increased since 2004. In Panel B, we plot the number of mayors convicted of misconduct in public office who are banned for running for any public office for at least five years. As the figure depicts, while less than 50 mayors were convicted of irregularities in 2004, more than 400 were convicted in 2009. Although the CGU is not solely responsible for the increase in anti-corruption crackdowns and convictions, it has undoubtedly increased the costs of corrupt practices in Brazil, and as we will document below, its random audit program has played a significant role in this increase.

Together with the increasing number of prosecutions and anti-corruption crackdowns by the Federal Police, the local media has also contributed to the program's effectiveness. Local media is an important source of information for both politicians and voters to learn about the audits of nearby municipalities, as well as the likelihood of future legal actions. For example, on March 31, 2010, the Federal Police arrested the mayor of Satubinha, Maranhão after the CGU had discovered that he had diverted funds from over 23 procurement contracts. According to a political activist blog, when the radio announced his arrest, the mayor of São Bento, a neighboring municipality, was seen leaving on a small airplane afraid that he would be the next one to be arrested.⁹

It is also the case that radio will often report on the audit results of neighboring municipalities. For example, on September 28, 2012, Radio Três Fronteiras, located in the municipality of Campos Sales, Ceará, ran a radio program to discuss the audit results of the neighboring municipality of Arneiroz.¹⁰ The radio station Rádio Pajeú AM 1500, which covers 23 municipalities in the states of Pernambuco and Paraíba, also airs programs about municipal audits. On December 15th, they ran a show on the CGU's audit of the municipality of Afogados, to highlight the large number of irregularities found in the implementation of the Conditional Cash Transfer program Bolsa Família.¹¹

⁹See <http://isanilsondias.blogspot.com.br/2010/04/policia-federal-no-encalco-de-prefeitos.html>. Retrieved December 12, 2016.

¹⁰See <http://tresfronteirasam.com.br/radio/noticias.php?noticia=1003>

¹¹See <http://www.radiopajeu.com.br/portal/pente-fino-da-cgu-no-bolsa-familia-prefeitura-de-afogados-emite-nota/>. Retrieved December 12, 2016.

2.3 Data

We build measures of mismanagement and corruption from a database obtained from the CGU. The dataset includes the coding of all the irregularities found by the auditors for each inspection order. While all audit reports are posted online, starting with the 20th lottery in March 2006, the CGU began to code the information that was used as an input for the reports for internal use. For each inspection order, the dataset contains information on the sector and government program, the amount transferred to the municipality, and a list of findings. For each finding, the auditors describe the irregularity found and assign a code that classify irregularities into one of three categories of wrongdoing: 1) irregularities associated with mismanagement (e.g. documents were not properly filled out, or improper storage of food supplies and medical equipment), 2) moderate acts of corruption, 3) severe acts of corruption.¹²

While the CGU's distinction between acts of mismanagement and acts of corruption is clear, the difference between moderate versus severe corruption is less obvious. To illustrate this, consider for example the municipality of Chaval in Ceará, which was audited in the 20th lottery. The auditors went to the municipality with 25 inspection orders, one of which involved the financing of school buses for students attending primary schooling. They discovered two irregularities – one moderate and the other severe. For the moderate irregularity, a representative of the mayor withdrew R\$1,200 without proving how the money was spent. The severe irregularity took place during the procurement of transportation services. The contract was awarded to a firm that did not match the original proposal, and the value of the contract was for a different amount than what was offered. While the second irregularity is arguably more severe, the CGU also classified as moderate the following irregularity discovered in Urbano Santos in Maranhão: There auditors visited three schools to check whether a school lunch program had been provided. Despite the fact that the municipality had received the money to pay for the program, school lunches had not been delivered for an entire year in one school, and had gone missing for a month in the other two schools. Given these types of examples, we had decided to use as our main measure the combination of both moderate and severe irregularities.

Based on this information, we construct measures of corruption and mismanagement at the municipality-

¹²These data are similar to those used by [Zamboni and Litschig \(2015\)](#), except that our dataset spans a longer period of time. It is also worth noting that the classification used by the CGU to distinguish between moderate and severe irregularities does not map directly onto the categories used either by [Ferraz and Finan \(2008\)](#) or [Brollo et al. \(2013\)](#). Because the CGU classifies the irregularities based on potential losses accrued to the government, many of their “moderate” irregularities are typical examples of the corrupt practices used in the analysis by [Ferraz and Finan \(2008\)](#) and [Brollo et al. \(2013\)](#). [Zamboni and Litschig \(2015\)](#) for a discussion of this point.

lottery level. Our measure of corruption is the number of irregularities classified as either moderate or severe. Our measure of mismanagement is the number of irregularities associated with administrative and procedural issues. In Figure 3, we plot the distributions of irregularities associated with corruption and mismanagement per service order. The audits discovered on average 2.5 acts of corruption and 0.88 acts of mismanagement per service order, suggesting that 73.6 percent of the irregularities found during an average audit involves some act of corruption. To put these figures in perspective, [Bandiera et al. \(2009\)](#) estimate only 20 percent of waste found in Italy’s public procurement process was due to corruption. Similarly, [Olken \(2007\)](#) argues that the main reason why audited villages in Indonesia did not significantly reduce their corruption is because the audits mostly reveal acts of mismanagement as opposed to acts of malfeasance. Similar to [Bandiera et al. \(2009\)](#) we do not find any evidence that active and passive waste are positively correlated (correlation coefficient = 0.02). In Figure A.1, we plot the average number of irregularities associated with corruption and mismanagement by lottery. While our measure of corruption has been increasing steadily over time, the acts of mismanagement has varied more, particularly in recent audits. Given the changes to the auditing protocol over time, one should be cautious to interpret this temporal variation. In the regression results, we control for time trends in audit practices and exploit only within-audit variation.

Four other data sources are used in this paper. The political outcome variables such as reelection, vote shares, and mayor characteristics come from the Tribunal Superior Eleitoral (TSE), which provides results for the 2004-2012 municipal elections. These data contain vote totals for each candidate by municipality, along with various individual characteristics, such as the candidate’s gender, education, occupation, and party affiliation. With this information, we matched individuals across elections to construct measures of reelection and whether mayors are serving on a first versus second-term.

We constructed the data on the joint CGU-Federal Police crackdowns using the information available on the CGU homepage, as well as internet searches.¹³ For each year starting in 2003, the CGU lists the name of the Special Operations and a description of the target. For each crackdown, we searched for the name of each operation together with the names of the targeted municipalities and keywords such as “mayor” or “corruption”. We created a dataset comprised of the name of each municipality targeted by the special operation, a description of the findings, and whether the mayor or public servants of the targeted municipalities were involved in and/or arrested during the crackdown. Using this information we created an indicator that equals to one if a municipality was subject to a crackdown in a given year and whether the mayor was involved in the irregularities

¹³See <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/acoes-investigativas/operacoes-especiais>.

and/or arrested.

Data on the convictions of mayors for misconduct in public office was obtained from the Cadastro Nacional de Condenações Cíveis por ato de Improbidade Administrativa e Inelegibilidade. This database, administered by the National Council for Justice (CNJ), includes the names of all individuals charged of misconduct in public office. We downloaded the data in 2013 so the dataset includes all agents convicted up to that point. For each individual we observe the type of irregularity (e.g. violation of administrative principles or diversion of resources), the court where the conviction took place, and the date. These data were then matched to the electoral data based on where the individual was a mayor and the period he/she served in office. Individuals on this list are banned from running for any public office for at least five years.

Data on municipal characteristics come from the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatística (IBGE)). The 2000 population census provides several socioeconomic and demographic characteristics used as controls in our regressions. Some of these key variables include income per capita, income inequality, population density, share of the population that lives in urban areas, and share of the population that is literate.

To control for different institutional features of the municipalities, we also use information from the municipality survey, *Perfil dos Municípios Brasileiros: Gestão Pública*, which is conducted annually from 1999-2010. This municipal survey characterizes not only various aspects of the public administration, such as budgetary and planning procedures, but also more structural features such as whether the municipality has a judge. Moreover, the survey provides our key measures of media availability, namely the number of radio stations and the number of daily newspapers. The richness of this dataset allows us to comprehensively check the validity of our research design, and control for any potential confounding factors in the regressions that do not entirely rely on the randomization.

Table 1 presents summary statistics for the municipalities in our sample, by whether they were audited previously or not. For each characteristic, we also present the difference between these characteristics. As expected from the random assignment, there is little difference in characteristics between places that were audited for a first time compared to those that have previously been audited. Importantly, included among these characteristics is the number of service orders. The fact that the number of service orders is balanced between treatment and control verifies the fact that the CGU does not adjust the number of service orders based on a previous audit.¹⁴ Out of

¹⁴This is expected given the way inspection orders are issued. As we mentioned, within sectors inspection orders are issued based on a random selection of government projects from the last 3 to 4 years.

15 characteristics, only one is statistically significant at the 10 percent level. We also fail to reject the hypothesis that all the variables are jointly significant (F-test=1.17; p -value=0.30). Overall the results from Table 1 suggest that the lottery used by the CGU was effective.

3 Model

To disentangle the channels through which audits may reduce corruption, we consider the following model of political accountability, which builds on the career concerns model (Holmström, 1999; Persson and Tabellini, 2002). There are two agents, a mayor and a representative voter. The mayor faces a two-term limit whereas the voter faces an infinite time horizon. In his first term, the incumbent chooses the effort he will make to extract rents taking electoral and legal incentives into consideration. Mayors differ along a single dimension we label ability, which has an observed and an unobserved component. First-term rents are increasing in the incumbent's effort and ability. After the mayor chooses his effort, an audit may occur with a fixed probability. Audits increase the expected legal costs associated with rent-seeking and the probability that the voter observes rents. The voter, after potentially observing rents, chooses whether to reelect the incumbent. If the voter does not reelect the incumbent, the game restarts with a randomly drawn first-term incumbent. If the voter reelects the incumbent, he chooses his effort for the second term taking legal incentives into account. Second-term rents are realized and the game starts over with a randomly selected first-term incumbent.

We show that in this framework, audits reduce future corruption through a *selection effect* by altering the expected ability of reelected mayors. We then expand the framework so that observing audits causes mayors and voters to update their beliefs over the audit probability. Hence, the history of audits within a municipality will also have a *disciplining effect*: mayors who have observed relatively more audits will reduce corruption due to perceived increases in legal and electoral incentives.

3.1 The Mayor

Mayors differ along a single continuous dimension, which we label ability. Mayors with higher levels of ability extract more rents than those with low ability. The ability of the mayor i is denoted by ζ_i and is constant over his tenure in office.¹⁵ The mayor's ability is a function of his observable

¹⁵Since the environment is stationary, we omit period subscripts in this section.

characteristics X_i (gender, education, occupation) and an unobservable characteristic ε_i :

$$\zeta_i = X_i' \alpha + \varepsilon_i \quad (1)$$

The incumbent's observable characteristics are common knowledge to mayors and voters, and are i.i.d. draws from a distribution F_X with mean zero when a first-term mayor is selected. The unobservable characteristic ε_i is an i.i.d. draw from a normal distribution, which we denote by F_ε , with mean zero and variance σ_ε^2 .

Mayors face a two-term limit. Let $T \in \{F, S\}$ denote whether the mayor is in his first term (F) or second-term (S). In each term, after the mayor chooses his action, his unobserved ability is drawn, and an audit is drawn independently from a Bernoulli distribution with probability q_i . Let $a_i^T = 1$ if an audit is drawn in term T and $a_i^T = 0$ otherwise. Let $a_i \equiv (a_i^F, a_i^S)$ and let F_{a_i} denote the distribution function. The mayor seeks to maximize the discounted sum of rents r , net of the costs of rent extraction c over his tenure. Rents in term T for mayor i are given by the sum of the mayor's effort and ability:

$$r_i^T = e_i^T + \zeta_i \quad (2)$$

The mayor's per-period utility in term T is given by

$$u_i^T(e_i^T, \zeta_i, a_i^T) = e_i^T + \zeta_i - c(e_i^T, a_i^T) \quad (3)$$

Mayors incur the costs to rent extraction through two channels. First, the mayor incurs the expected cost of having a legal action taken against him, which is increasing in the effort placed into rent-seeking. We refer to this channel, captured by variation in c , as legal discipline. Second, outlined in the derivation of the voter's strategy in the following section, the mayor's expected reelection probability is decreasing in the rents he extracts. We refer to the latter as the electoral discipline channel.

We begin by describing the legal discipline channel. We assume that a legal action is taken against the mayor with probability $(\gamma_0 + \gamma_1 a_i^T) e_i^T$, where $\gamma_1 > 0$ implies that legal actions are more likely when mayors are audited. We assume that the cost of the legal action is given by $b_0 + b_1 e_i^T$, so that $b_1 > 0$ implies that punishment is increasing in the mayor's corrupt action.

The mayor chooses his action before the audit event is drawn. Therefore, expected legal costs when the mayor chooses his action are given by

$$\mathbb{E}[c_i^T | e_i^T] = b_0(\gamma_0 + \gamma_1 q_i) e_i^T + b_1(\gamma_0 + \gamma_1 q_i) (e_i^T)^2 \quad (4)$$

where q_i denotes the audit probability. Assuming that $b_0, b_1, \gamma_0, \gamma_1 > 0$, this function is strictly increasing and strictly convex in e_i^T .

Mayors discount second-term utility by β . Let $R_i \in \{0, 1\}$ denote whether the mayor is reelected or not. The first-term mayor's maximization problem is to choose, conditional on the voter's strategy, the effort levels $(e_i^F, e_i^S) \in \mathbb{R}_+^2$ which maximize his expected utility:

$$\max_{e_i^F, e_i^S} \int u_i^F(e_i^F, \zeta_i, a_i^F) + \beta \mathbb{P}(R_i = 1 | e_i^F, \varepsilon_i, X_i, a_i^F) u_i^S(e_i^S, \zeta_i, a_i^S) dF_{a_i} dF_{\varepsilon} \quad (5)$$

3.2 The Voter

We assume that there is a representative voter and adapt the standard probabilistic voting model. The voter in municipality i only chooses an action if there is a first-term mayor ($T = F$). The voter's decision, whether or not to reelect the incumbent, depends on the following factors: the mayor's observable characteristics, the voter's belief about the mayor's unobservable characteristic $\tilde{\varepsilon}_i$, and the mayor's popularity. The mayor's popularity is given by $X_i' \xi + \delta_i$, where the mayor's popularity shock δ_i is drawn independently from a uniform distribution F_D with mean μ_D and density σ_D . The voter's per-period utility when there is a first-term mayor is given by $v_i^F = -r_i^F$ with the added popularity shock $X_i' \xi + \delta_i$ if he chooses to reelect the incumbent, while the voter's per-period utility when there is a second-term mayor is $v_i^S = -r_i^S$.

The voter observes contemporaneous rents with probability $\chi_i^T \equiv \chi_0 + \chi_1 a_i^T$. Setting $\chi_1 > 0$ implies that voters are more likely to observe rents and punish mayors when an audit occurs in the same term. After observing the popularity shock and, possibly, rents, the voter chooses whether to reelect the incumbent or select a challenger who is drawn at random from the pool of candidates.

3.3 Equilibrium

In sum, the timing of the game is as follows; (1) The first-term incumbent chooses his effort level; (2) his ability draw is realized and first-term rents are extracted; (3) the audit draw, the voter's rent signal draw and the incumbent's popularity shock are realized; (4) elections are held; if the incumbent loses, the game continues with step (1) with a randomly drawn first-term mayor, otherwise; (5) the second-term incumbent chooses his effort level, the second-term audit draw is realized and second-term rents are extracted; the game continues with step (1) with a randomly drawn first-term mayor.

We utilize the perfect Bayesian equilibrium solution concept and solve for the equilibrium in pure strategies. A strategy for the mayor is a sequence of choices $e_i^T(q_i, X_i)$ for each term T conditional on the audit probability q_i and his observable characteristics X_i . A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, q_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i , the audit probability and the mayor's observable characteristics. Formally, a perfect Bayesian equilibrium is a sequence of mayor and voter strategies and voter beliefs such that: 1) the mayor's strategy is optimal given the voter's strategy, 2) the voter's strategy is optimal given the mayor's strategy, and 3) the voter's beliefs are consistent with the mayor's strategy on the equilibrium path.

We begin by considering the equilibrium strategy of the second-term mayor. The second-term mayor faces no reelection incentives and thus only maximizes his expected second-term utility (see equation (3)). We assume that $1 > b_0(\gamma_0 + \gamma_1 q_i)$ so that there is a unique interior solution. The first-order condition yields the second-term mayor's equilibrium strategy

$$e_i^{S*} = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i)}{2b_1(\gamma_0 + \gamma_1 q_i)} \quad (6)$$

We next consider the voter's equilibrium strategy. Given his belief over the mayor's type, the voter chooses whether or not to reelect the incumbent by considering which option maximizes his expected lifetime utility. In equilibrium, the voter's value function when selecting a random first-term mayor is given by

$$V(q_i) = \int v_i^{F*}(X_i, \varepsilon_i, q_i, \delta_i) + \beta \left[p(X_i, \varepsilon_i, q_i) \left(v_i^{S*}(X_i, \varepsilon_i, q_i) + \beta V(q_i) \right) + (1 - p(X_i, \varepsilon_i, q_i)) V(q_i) \right] dF_X dF_\varepsilon dF_\delta \quad (7)$$

where $p(X_i, \varepsilon_i, q_i)$ denotes the equilibrium probability of reelection conditional on observable and unobservable ability, and v_i^{F*} and v_i^{S*} denote equilibrium per-period utilities. Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. The voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left((1 - \beta)V(q_i) + e_i^{S*} + \tilde{\varepsilon}_i \right) \quad (8)$$

where $h(X_i) \equiv X_i' \xi - \beta X_i' \alpha$ denotes how much voters value the mayor's characteristics when accounting for their effects on both rents and popularity. It follows that in equilibrium, the probability

that a mayor of type (ε_i, X_i) is reelected is¹⁶

$$p(X_i, \varepsilon_i, q_i) = F_D \left(2\mu_D + h(X_i) - \beta[(1 - \beta)V(q_i) + e_i^{S*} + (\chi_0 + \chi_1 q_i)\varepsilon_i] \right) \quad (9)$$

Thus, since audits increase the probability of detection by the voter ($\chi_1 > 0$), the equilibrium reelection probability is increasing in the audit probability q_i if and only if the mayor's unobservable ability is below average ($\varepsilon_i < 0$).

We next solve the maximization problem (5). Assuming an interior solution, the mayor's equilibrium term 1 action

$$e_i^{F*} = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i) - \beta^2(\chi_0 + \chi_1 q_i)\sigma_D u_i^{S*}}{2b_1(\gamma_0 + \gamma_1 q_i)} \quad (10)$$

where u_i^{S*} denotes the equilibrium expected payoff for term 2. Therefore, equilibrium first-term rents, $r_i^{F*} = e_i^{F*} + \zeta_i$, are determined by three factors. First, they are decreasing in expected legal costs, which are captured by the legal cost parameters b_0 and b_1 , and the legal action probabilities γ_0 and γ_1 . Second, the possibility of reelection reduces the effort spent on rent extraction. The magnitude of the reduction is increasing in the probability that the voter observes rents (captured by χ_0, χ_1 , and q_i), expected term 2 utility, the density of the popularity shock, and the mayor's patience as captured by the discount factor β . Third, selection over ability ζ_i plays a role through observable characteristics X_i and the unobservable trait ε_i . In contrast, equilibrium second-term rents, $r_i^{S*} = e_i^{S*} + \zeta_i$, are only determined by legal costs and selection. Elections will improve the selection of mayors who are reelected, and will do so to a greater extent when an audit occurs prior to the election.¹⁷

Finally, we analytically solve for $V(q_i)$, the voter's equilibrium expected lifetime utility from the state where a first-term mayor is randomly selected, by simultaneously solving equations (7) and (9). This also analytically pins down the equilibrium reelection probabilities.

3.4 The Effects of Audits

In the model outlined above, whether a municipality has been audited in the past has no long-term consequences on rent extraction. The occurrence of an audit only affects rents in the following term through its effect on selection through reelection. Otherwise, since the audit probability does not

¹⁶See Appendix B for the derivation.

¹⁷To be precise, selection improves with reelection if and only if voters do not have a sufficiently strong taste for observable characteristics which increase rents.

depend on the history of audits within a municipality, mayor and voter strategies will not depend on past audits. This result is not unique to our framework as it also follows from other typical models of political agency.¹⁸ However, if mayors and voters are not perfectly informed about the ex-ante audit probability within a given electoral term, as we argued in Section 2, it is plausible that they update their beliefs over the audit risk through the information they acquire from their own and others’ audit experiences.

We propose the following simple model of Bayesian learning to rationalize why the history of audits may affect the behavior of mayors and voters. Let t denote the period (electoral term), with $t = 1$ denoting the first period in which there are audits (in our setting, the 2000-2004 electoral term). In each municipality i and period t , an audit is drawn from an i.i.d. Bernoulli distribution with probability \bar{q} . Let $a_{it} = 1$ denote the event of an audit, and $a_{it} = 0$ otherwise. After the draws at time t are made, each municipality observes its own draw and the draws of its neighbors if local media is present. Let N_i denote the set of municipalities neighboring i , where N_i is empty if local media is absent. Then the number of audit draws observed in each term will follow a binomial distribution with sample size $|N_i| + 1$ and number of successful draws $y_{it} \equiv a_{it} + \sum_{j \in N_i} a_{jt}$.

For all agents, we assume that prior beliefs over the audit probability follow the distribution $Beta(\beta_0, \beta_1)$.¹⁹ The mean of the prior distribution is given by $\frac{\beta_0}{\beta_0 + \beta_1}$, which we assume is equal to the objective probability \bar{q} . Hence agents have a rational mean prior. We also fix the sum $\beta_0 + \beta_1$, which expresses the “strength” of the prior beliefs. If agents do not take their own (and neighboring) experiences into account when forming beliefs, then we are in the case $\beta_0 + \beta_1 \rightarrow \infty$. However, recent empirical findings suggest that in fact agents place at least *some* weight on their experiences when forming beliefs (Kleven et al., 2011; Gallagher, 2014; Malmendier and Nagel, 2011).²⁰ Hence we assume that agents take their experiences into account when forming their

¹⁸For example, incorporating adverse selection in addition to moral hazard as in Besley (2007) produces the same implications. In this setup, there are two types of mayors—good and bad—and good mayors always pick the non-corrupt action whereas bad mayors choose whether to pick the corrupt or non-corrupt action (depending on a heterogeneous cost). Voters know that there is some share of mayors that are good, but don’t observe the type of the incumbent. One can incorporate audits into this setup by allowing their occurrence to reveal the rents that have been extracted. Rents are a noisy signal of the action taken by the mayor, causing the voter to update his posterior belief about the mayor’s type. This model produces a similar set of results. Audits allow voters to select a good mayor for reelection with a higher probability. However, past audits do not discipline mayors, as incumbents are only disciplined by the history-independent audit probability. These findings can also be derived in other models of political agency, such as those where selection is based on responsiveness to voters (Banks and Sundaram, 1993). See Ashworth (2012) for a review of the literature and Bobonis et al. (2015) for an enlightening discussion regarding the effects of audits in various political agency models.

¹⁹Prior beliefs follow a Beta distribution for mathematical convenience as this distribution is the conjugate prior for the Bernoulli distribution.

²⁰Our learning framework is similar to the one developed in Gallagher (2014). This study finds a significant increase

posterior beliefs, i.e. $\beta_0 + \beta_1 < \infty$.

After observing the audit draws from term 1 until term $t - 1$, mayors and voters will update their beliefs about the audit probability. The mean posterior belief \tilde{q}_{it} , conditional on the realization of the audit histories $\{y_{is}\}_{s=1,\dots,t-1}$ and $\{y_{js}\}_{s=1,\dots,t-1} \forall j \in N_i$, is given by:

$$\tilde{q}_{it} = \frac{\sum_{s=1}^{t-1} (a_{is} + \sum_{j \in N_i} a_{js}) + \beta_0}{(t-1)(1 + |N_i|) + \beta_0 + \beta_1} \quad (11)$$

There are two objects in the model which are directly affected by the belief over the audit probability. We first consider the expected legal cost faced by the mayor. In this case, following from equation (4), mayors will take expected legal costs to be $b_0(\gamma_0 + \gamma_1 \tilde{q}_{it})e_{it} + b_1(\gamma_0 + \gamma_1 \tilde{q}_{it})e_{it}^2$. Thus, in municipalities in which mayors and voters have observed larger proportions of audits, mayors will expect higher legal costs and extract less rents.

Second, the belief over the audit probability also affects the probability of voter detection. The mayor will choose his action taking the probability of detection to be $\chi_0 + \chi_1 \tilde{q}_{it}$. Thus, in municipalities in which mayors and voters have observed larger proportions of audits, first-term mayors will be disciplined by higher perceived electoral costs of corruption. Therefore, in addition to the effect of past audits on selection, this framework allows audits to also affect electoral and legal discipline.²¹

Although we only model learning over the audit probability, it could also be the case that agents learn about the expected costs associated with audits. Our framework does not rule out this possibility. Indeed, γ_1 and χ_1 could equally be interpreted as reduced-form parameters capturing learning over expected legal and electoral costs.

We prefer to model audits affecting beliefs rather than objective costs for two reasons. Most importantly, it is difficult to explain why an audit in a neighboring municipality would affect the costs associated with rent extraction *only* in the presence of local media. This seems especially unlikely for the costs associated with legal actions, as it would imply that the legal penalties for corruption are higher when neighbors have been previously audited, but only in the presence of local media. Moreover, the learning model is consistent with the narrative evidence we presented in Section 2.

in insurance take-up in communities following the experience of a flood. Similarly to the spillover effects we find, [Gallagher \(2014\)](#) also shows that insurance take-up increases when neighboring communities which share a TV media market are flooded.

²¹We note that that our solution for the perfect Bayesian equilibrium in Section 3.3 is essentially unchanged once we allow for learning over the audit probability. The distribution F_{a_i} is replaced with the posterior belief distribution when solving for the mayor's and voter's equilibrium strategies.

4 Research Design

Before structurally estimating the model, we examine whether the audits reduce future corruption in the reduced-form using the random variation induced by the lotteries. To test this hypothesis, we need to overcome the fact that we only observe corruption once a municipality has been audited. We do so by exploiting municipalities that have been audited multiple times. As we see in Figure 4, out of the 1,949 municipalities that have been audited, 25 percent of them have been audited multiple times: 253 audited twice, 18 three times, and 1 municipality 4 times. For a given round of audits, we can compare the corruption levels of municipalities that had been audited prior to this audit to those that had not (and are thus being audited for the first time).

Figure 5 shows the main variation we will exploit. The first time a municipality was audited for a second time occurred in the 11th lottery. As expected, the number of municipalities that have been audited more than once increases over time. For instance, in the 30th lottery, 19 out of 60 municipalities had been audited in the past. Note that because the lotteries are conducted at random, for a given lottery at time t the probability that a municipality has been audited in the past is the same for all municipalities within a particular state. Given this structure of the data, we estimate the following model for municipality m in state s , audited at date t .

$$\text{Corruption}_{mst} = \alpha + \beta \text{Past Audit}_{mst} + Z'_{ms} \gamma + f(\text{nos})_{mst} + v_s + \mu_t + \varepsilon_{mst} \quad (12)$$

where Corruption_{mst} is the log of the number of corrupt irregularities detected in municipality m during audit t , and Past Audit_{mst} is an indicator for whether at date t the municipality had been audited in the past. The vector Z'_{ms} consists of a set of municipal characteristics (e.g. population, income per capita, income inequality, etc.) measured in 2000. These controls allow us to account for any socio-economic differences across municipalities prior to the start of the program. The variable nos_{mst} denotes the number of service orders that auditors were sent to investigate. Because audits with more service orders tend to discover more irregularities, it is important to account for these differences in a flexible manner. In our preferred specification, the number service orders is controlled for non-parametrically. The error term, ε_{mst} , captures unobserved (to the econometrician) determinants of corruption.

Importantly, our model also adjusts for two classes of fixed-effects. We include state intercepts, v_s , to capture the fact that the randomization is stratified by state.²² We also include lottery fixed

²²Given the population density of North Brazil, when CGU draws municipalities for audit, this region, which includes the states of Acre, Amapá, Amazonas, Pará, Rondônia, Roraima and Tocantins, is treated as a single state.

effects, μ_t , which are important for two reasons. First, although municipalities are more likely to become treated over time, within a given lottery, the probability that a municipality had been audited in the past is the same for municipalities within the same state. Second, and more importantly, starting in the 20th lottery, the CGU began to audit funds in selected areas and programs. It is thus difficult to compare corruption levels over time, and hence we restrict our analysis to variation within audits. Because municipalities are audited at random, we can interpret the coefficient β as the causal effects of the audits on corruption.

In addition to estimating the effects on corruption, we also test whether an audit increases the likelihood of a federal conviction or investigation. Because we do not need to restrict the sample to only audited municipalities, we can compare audited places to non-audited places with the following specification:

$$\text{Legal}_{mt} = \alpha + \beta \text{Audited}_{mt} + v_m + \mu_t + \varepsilon_{mt} \quad (13)$$

where Legal_{mt} is an indicator for whether a legal action (e.g. crackdown involving political corruption or the mayor was convicted for corruption) occurred in municipality m in year t . Our treatment variable, Audited_{mt} , which is equal to one after the municipality has been audited for the first time, estimates the causal effect of being audited on the likelihood of incurring a subsequent legal action. The regression adjusts for municipal and year fixed effects, and the error term is clustered at the level of the municipality.

5 Results

5.1 Reduced-form Estimates

Effects of the Audits on Corruption and Mismanagement

Table 2 presents OLS regression results from estimating several variants to Equation 12. The specification in the first column estimates the effects of having been audited on the log of the total number of irregularities discovered in the audit, controlling for state and lottery intercepts, as well as the number of service orders. Column 2 extends this specification to include various socio-economic characteristics of the municipality. Our preferred specification is presented in Column 3, which modifies the specification in Column 2 to control for the number of service orders in a nonparametric manner. Our estimation sample includes all audits from lotteries 22 to 38.

The results in columns 1-3 suggest that municipalities that had been audited in the past commit significantly fewer irregularities than those that had not been previously audited. Once we control for municipal characteristics and service-order fixed-effects, we estimate a reduction of 5.8 percent. We also find that the number of irregularities correlates with several of the socio-economic characteristics that we have come to expect from the cross-country literature (e.g. [Treisman \(2000\)](#)). For example, we see strong negative associations with income per capita and literacy rates, as well as positive correlations with income inequality and population.

As we have discussed above, there is an important distinction to be made between corruption and mismanagement, which we distinguish between in columns 4-9. In columns 4-6, we replicate the previous specifications using as a dependent variable the log of total acts of mismanagement. In columns 7-9, we use the log of total acts of corruption as the dependent variable.

We do not find any evidence that audits affect mismanagement. Under our preferred specification, the point estimate is small and statistically indistinguishable from zero (coefficient = -0.023, robust standard error=0.041). In contrast, we find that having been audited in the past leads to a significant reduction in corruption. Municipalities that had experienced a previous audit committed 7.9 percent less acts of corruption compared to those that had not. In order to interpret this magnitude, consider that the average municipality in our sample receives R\$15,000,000 in federal transfers per year. Based on our estimates of a random sample of audit reports, 30 percent of the funds audited were found to be diverted, implying that audits reduced corruption by R\$355,000 per year per municipality. The municipal characteristics are also quite predictive of corruption levels: for example, a 10 percent increase in per capita income is associated with a 1.8 percent decline in corruption.²³

Spillover Effects

The estimates presented in [Table 2](#) are likely to represent a lower bound for the reduction of corruption caused by the audits. If control municipalities are learning about the audits either through the media, from an audited neighbor, or from their partisan network, then they too might refrain from corruption. We explore these possibilities in [Table 3](#). In column 1, we re-estimate [Equation 12](#), adding the number of neighboring municipalities that have experienced an audit as an additional independent variable. To account for the fact that municipalities have different numbers of neighbors, we also control non-parametrically for the number of neighbors. Moreover, because a municipality might not learn about another municipality's audit, even if neighbors, in [columns 2](#)

²³We also test whether the effects of the audits vary according local characteristics, but find little evidence of heterogeneous effects (see [Table A.2](#)).

and 3 we introduce an interaction term for whether local media is present in the municipality. Because neighboring municipalities typically share a media market, a municipality is more likely to learn about its neighbors' audits if it has local media. In Section 2, we presented anecdotal evidence in support of this claim.

In column 1, we estimate that for each additional treated neighbor, a municipality reduces its corruption by 2.0 percent, but this effect is not statistically significant. The coefficient on our main treatment variable is nearly identical to those presented in Table 2, suggesting that even when controlling for spillover effects from neighboring municipalities, fewer acts of corruption are uncovered in municipalities that have been audited in the past. In columns 2-3, we test whether the spillover effects are more pronounced when the media has a presence in the municipality. For both AM radio (column 2) and television (column 3), we find evidence of significant spillover effects. We estimate that an additional audited neighbor decreases corruption by 7.5 percent when AM radio is present, and by 10.4 percent for local television. We find no evidence of spillover effects in municipalities without the presence of the media.

In column 4, we further explore whether information about the effects of an audit is also transmitted through partisan networks. Within a state, political parties will sometimes facilitate interactions between their mayors through annual meetings and discussions with federal deputies, senators and governors. If these partisan networks are strong, then mayors might learn from the audits experienced by other mayors within their network. To test for this, we add to the specification presented in column 3 the number of times a mayor from the same party within the state had been audited in the past. To account for any differences in the strength of the partisan networks, we also included party fixed effects. The results in column 4 suggest that parties do play a relatively small but statistically significant role in information diffusion. For each additional mayor audited from their partisan network, mayors decreased their corruption levels by 0.4 percent. The spillover effects of local media also remain strong even after allowing for the effects of partisan networks. In column 5, we re-estimate the equation, allowing for the spillover effects to vary by share of the population with a college degree, income per capita, and the share of urban population. Even after allowing for differential effects along these other characteristics, the heterogeneous effects by AM radio, local television, and party remain robust.²⁴

Overall, these findings suggest that we are underestimating the audit program's true impact on corruption. Municipalities that are presumably learning about the potential effects of the audits are

²⁴We also replicate these findings when using a dummy for the presence of at least one neighboring audit, instead of the number of neighboring audits.

engaging in less corruption, even if they had not experienced an audit themselves.

Effects of the Audits on Legal Actions

After an audit, the municipality (and its neighbors) has not only updated about the likelihood of an audit, but also on the likelihood of legal consequences. As we depicted in Figure 2, both police crackdowns and convictions have increased substantially over time, in part due to the program. In Table 4, we investigate the effects of being audited on the likelihood that the municipality faces a subsequent legal action. In columns 1-6, we estimate variants of Equation 13 with three sets of dependent variables: an indicator for whether a police crackdown involving political corruption occurred (columns 1 and 2), an indicator for whether a mayor was convicted for corruption (columns 3 and 4), and an indicator for either a crackdown occurred or a mayor was convicted (columns 5 and 6). Because we are not limited to municipalities that have been audited at some point in time, we estimate these specifications for the entire sample of municipalities eligible for an audit.

Compared to non-audited municipalities, places that have experienced an audit are much more likely to face a subsequent legal action, as measured by either a police crackdown or a mayor conviction. Municipalities that have been audited in the past are 0.5 percentage points more likely to face a legal action than those that have not been audited. This effect implies that the audits led to an increase of approximately 30 legal actions from a base of 140 among control municipalities. In columns 2, 4 and 6, we find that the effects of the treatment are largely concentrated in places with a judiciary district. Among these municipalities, the treatment increased the likelihood of a legal action by 35.4 percent, relative to control municipalities with a judiciary district.

While informative, the specifications presented in columns 1-6 would ideally also condition on the level of corruption in the municipality. Because we do not observe this information in places that have not been audited, in columns 7-9 we regress our measures of legal action on log acts of mismanagement and log corruption. Not surprising, we find that corruption is strongly associated with the likelihood of a legal action. For example, a 1 percent increase in number of corruption acts is associated with a 8.8 percent increase in the likelihood of a legal action. In contrast, we do not find any evidence that acts of mismanagement are associated with any legal costs. Overall these findings suggest that the legal costs of engaging corruption are substantial.

5.2 Mechanisms

Thus far, the evidence suggests that having been audited in the past substantially reduces future corruption and increases the likelihood of a legal action in the municipality. In Section 3, we discussed several reasons why the audits may reduce corruption. One reason is a political selection effect among mayors that are reelected. If audits allow voters to punish corrupt mayors and reward good ones, then we would expect better politicians in places where the incumbent was audited prior to the election and still re-elected. Another channel is an electoral disciplining effect. If audits increase the perceived future probability of being exposed to voters, then mayors who have re-election concerns will refrain from corruption. A third is what we have termed a legal or non-electoral disciplining effect. Mayors may refrain from corruption even in the absence of re-election incentives, lest they incur reputation or legal costs. A final possibility is a political entry effect, which would occur if audits induce a change in the distribution of mayor types who choose to enter into the candidate pool.

In this section, we present reduced-form tests of these various mechanisms, and isolate their effects under the assumption that they are constant and additive. In Section 6, we relax these restrictive assumptions and instead disentangle the channels by structurally estimating the model.

Electoral and Legal Disciplining Effects. To isolate the effects from electoral and legal discipline, we consider the set of municipalities in which a mayor experiences an audit early in his term (often over funds that he did not administer), and is then effectively audited again in the same term.²⁵ In these cases, no election has occurred, which rules out the possibility of any audit-induced political selection or entry effect. Any difference in corruption levels between these municipalities and those that have not been audited (control group) can only be due to an electoral or legal disciplining effect.

To further distinguish between these two effects, we estimate two additional specifications. First, we test whether the effects of the audits vary by whether the mayor was in his first or second term. Because second-term mayors are term limited, they should only respond to legal costs, whereas first-term mayors will respond to both types of costs. The second specification allows for the possibility that second term mayors, although term limited, may still face electoral incentives through an election to a higher office. To account for this possibility, we first estimate a mayor's propensity to run again for a future office using data from all elections held during 2000 to 2012. To compute

²⁵Note that the audit may have even occurred in the subsequent term, but the funds audited referred to those administered under the previous term.

this propensity score, we estimate a Logit model based on a mayor’s gender, education, previous occupation, vote share and campaign spending in the past election. Specifically, we estimate the follow equation:

$$\text{Ran again}_i = \beta_0 + \beta_1 \text{Male}_i + \beta_2 \text{campaign spending}_i + \beta_3 \text{vote share}_i + \eta_e + \theta_o + \varepsilon_i$$

where Ran again_i is an indicator for whether the mayor ran for a higher office, Male_i indicates whether the mayor is male, $\text{campaign spending}_i$ measures the amount of money the mayor spent in his mayoral election, vote share_i denotes the share of votes he received in his mayoral election, η_e represent a series education intercepts, θ_o represents a series of occupation dummies at the 1-digit level, and ε_i denotes the error term. The results from estimating this equation are presented in the appendix table A.3. Our second specification tests whether the effects of the audits were more pronounced for mayors who were more likely to run for a future office.

We perform these comparisons in columns 1-3 of Table 5. In column 1, we compare to non-audited places, municipalities in which the mayor experienced multiple audits within the same term. Based on this comparison, we find that the audits led to 12.7 percent reduction in corruption, which can be attributed to either an electoral disciplining effect, a legal disciplining effect, or both. In column 2 and 3, however, we do not find a statistically significant differential effect based on whether the mayor is in his second term or is more likely to run for a higher office. For example, based on a one standard deviation increase in the propensity to run for a higher office, the estimates reported in column 3 suggest that the differential effects of the audits led to only an additional 0.3 percent reduction in corruption.

Political Selection. The effect of political selection on corruption stems from voters reelecting at greater rates the mayors who are less corrupt. Thus, to test for the existence of the political selection channel, we compare mayors that were audited and re-elected to mayors who were not audited but were also reelected. If, as documented in Ferraz and Finan (2008), the audits enable voters to punish corrupt politicians while rewarding non-corrupt ones, then places that were audited prior to the election and where the mayor was subsequently reelected should be, on average, positively selected, compared to those places that were not audited and the mayor was still reelected.

We present this comparison in column 4. Among municipalities where the mayor was re-elected, corruption levels were 14.9 percent lower in audited municipalities compared to non-audited municipalities. Note however, that this difference in corruption levels reflects both the change in the composition of mayor types (political selection), as well as a legal disciplining effect. But given our

previous estimates of the disciplining effects (in columns 1-3), these results suggest that political selection is actually playing a relatively minor role in how these audits are reducing corruption.

One potential concern with our comparison between discipline versus selection effects is in the timing of the audits. If the effects of the audits differ depending on how much time had elapsed since the last audit, perhaps due to recency bias, then the comparison between the effects in columns 1 and 4 would also incorporate this additional effect. In column 5, we test for this possibility. We estimate the effects of the audits based on the number terms since the last audit. We find no evidence of a differential effect based on how much time had elapsed since the last audit.²⁶

Political Entry. A fourth channel through which audits may reduce corruption is political entry. This would be the case if the audits shifted the political equilibrium, whereby better candidates entered into politics. We test for this mechanism by comparing corruption levels in places that were audited at $t - 1$ to those that were not, conditional on having a new mayor in time t due to an open seat election. By focusing on open-seat elections, we obviate the direct effects of the audits on any potential candidate, given that the audits had taken place on a term-limited mayor. As such, any effects of the audits would have to come from changes to the political environment more generally. Conditional on having a new mayor at time t , there are three effects that could be driving this difference: electoral and legal disciplining effects, and a political entry effect. With estimates of the first two effects, we can isolate the effects of audits through political entry.

In column 6, we find that compared to non-audited municipalities, corruption is 12.2 percent lower in places that were audited in the prior administration. Once we net the effects estimated in column 1, these results suggest that the political entry effect is zero, and provide additional support for the importance of legal disciplining.

As a further test for political entry, we examine whether the audits impacted the types of candidates that ran for office during open-seat elections. In Table 6, we examine whether the audits impacted the competitiveness of the elections, as well as the characteristics of the candidate pool and elected mayor. Consistent with a negligible entry effect, we find no evidence that the audits affected any of these election characteristics.

In sum, the results from Table 5 and 6 suggest that the audits' impact on corruption were driven mostly by legal disciplining effects. There are, however, two important caveats to this interpretation. First, we need to assume additive and constant treatment effects in order to compare the

²⁶We can repeat this exercise for the number of years since the last audit and the results are qualitatively similar.

effects of the audits across the various subsamples. Second, large standard errors cloud some of our comparisons. For example, although we don't find much statistical evidence of an electoral disciplining effect, we also cannot reject sizeable economic effects. Given our estimates in column 3, if we take the lower bound estimate of the 95 percent confidence interval, the differential effects of the audits would imply an additional 9.8 percent reduction in corruption level for mayors with an one standard deviation increased propensity to run for higher office. We also cannot statistically reject meaningful political selection effects. Although the results in columns 1 and 4 imply a political selection of only 2.2 percent, given our standard errors, the effects could also be as large as 15.2 percent. In light of these limitations, it is important to complement our reduced-form findings with structural analysis. This approach will relax the assumptions that the effects are additive and constant, and allows us to better disentangle the various channels contributing to corruption. Also, by employing more structure to the problem we can better exploit the variation in the data to obtain additional precision.

5.3 Alternative Mechanisms

Displacement An alternative interpretation of our reduced-form findings is that politicians from previously audited municipalities are learning how to better hide corruption. In this case, corruption has not necessarily been reduced, but perhaps displaced. There are several reasons, however, why displacement is an unlikely interpretation. First, it is unlikely that displacement would have generated the spillover effects we documented in Table 3. Second, the set of programs and sectors that are subject to an audit vary over time, making it difficult for mayors to predict which specific areas and programs will be audited in the future. Third, based on the audit reports, we can classify how the corruption occurred. If places that have been audited in the past learned how to displace corruption, then we might expect an audit to affect the type of corruption committed in subsequent audits.

We test for these explanations in Table 7. Here, we estimate the effects of having been audited in the past on the share of corrupt acts associated with embezzlement, procurement contracts, and over-invoicing; three of the most common forms of corruption.²⁷ In column 1-3, we estimate the effects for the entire sample, and find no evidence that in places that had been audited in the past, mayors are more likely to shift away from or into these types of corruption. In columns 4-6, we

²⁷For each audit report, we create these shares by first counting keywords which are associated respectively with embezzlement, procurement contracts, and over-invoicing. We then divide the counts by the number of corrupt acts and finally we normalize the measures.

restrict the sample to consider only those cases in which the mayor experienced multiple audits, which presumably is where the learning effects would be easiest to detect. But again, we find no evidence that the audits affected the nature of corruption in these places.

To further test the displacement hypothesis, we explore whether, across municipalities that are audited multiple times, less corruption is uncovered when the same sectors are audited. We estimate in column 7 the association between the amount of corruption detected during a second audit and the share of sectors investigated in both this and the municipality's previous audit. If mayors in treated municipalities are learning to better hide corruption, then presumably less corruption should be uncovered in places where the audits investigated funds in the same sectors. But as the result in column 7 indicates, the correlation, instead of negative, is positive and not statistically significant.

As a final test of displacement, we examine whether the audits affect how municipalities spend their budgets. If local governments are displacing corruption by shifting their expenditures to sectors where corruption is harder to detect, then expenditure shares should be different in municipalities that have been audited in the past. For each audit, we compute the share of public expenditures spent in each sector during the given year. As Table 8 documents, we find no evidence that the treatment affected the manner in which municipalities allocated their budgets. In light of our previous discussion that mayors cannot anticipate which sectors and projects will be audited in the future, this result is not surprising.

Another impact of the audits may have come from a reduction in the amount of block grants a municipality receives from the federal government. If this response in turn lowered the opportunity for mayors to engage in corruption, then this could explain the reduction in corruption we observe among previously audited places. As shown in Table 9, we do not find any evidence that having been audited in the past leads to a reduction in subsequent block grants.²⁸

6 Structural Estimation

We structurally estimate the model to complement our reduced-form analysis in two ways. First, by explicitly modeling the relationship between rents and reelection rates, the model allows us to distinguish the effects of audits through the electoral and legal channels. In the reduced-form estimation, we isolated the channels by exploiting instances where municipalities experienced two audits in the same term. More generally, the empirical challenge is that a decrease in rents in

²⁸We also do not find an effect when we interact the treatment with amount of corruption discovered in the audits.

treated municipalities could equally be caused, on the one hand, by legal discipline, or on the other, by the combination of electoral discipline and selection. The structural estimation directly tackles this issue without restricting the sample. Instead, we jointly estimate an equation for the responsiveness of voters to rents with equations derived for the mayor’s equilibrium strategy taking the voter’s strategy into account. At the cost of imposing some structure to the relationships, this approach allow us to estimate the parameters required to quantify the importance of each channel.

Second, the structural model embeds the learning process caused by the realization of audits which we formulated in Section 3.4. Thus, in addition to data on corruption and elections, the structural estimation exploits data on neighboring audits and media presence.²⁹ Moreover, this approach allows us to recover the parameter estimates needed to analyze policy counterfactuals.

6.1 Data and Estimation

We estimate the model for the same sample of audits used in the reduced-form estimation, except that we remove the second audit in cases where the same mayor is audited twice in the same term. Each observation i consists of the vector $(T_i, r_i, R_i, q_i, a_i, X_i', Z_i')$, where $T_i \in \{F, S\}$ indicates the mayor’s term, r_i is the log of acts of corruption in the audit report (the same measure as used in the reduced-form estimation), R_i is a dummy for whether the mayor is reelected, q_i is the mean of the posterior belief over the audit probability, a_i denotes whether the municipality was audited in the *previous* term, X_i denotes the vector of observable characteristics, and Z_i the vector of municipal characteristics.

The mean posteriors q_i are computed using Bayes’ rule with the prior Beta distribution parameterized by β_0 and β_1 . The mean of the prior, $\frac{\beta_0}{(\beta_0 + \beta_1)}$, is set to the objective probability of audit in the sample.³⁰ The vector X_i includes mayor characteristics (gender, education and occupation), number of service orders, number of neighbors and state and lottery intercepts. Finally, we fix the annual discount factor to 10 percent and the cost parameters to $b_0 = 8$, $b_1 = 4$, such that the penalty of a legal action is equal to the equivalent of two terms of rents plus four times the amount

²⁹Despite the reduced-form evidence of spillover effects within a mayor’s political network, we opted for parsimony and decided not to explicitly model this channel as it would require introducing parties. Not only would this complicate the model considerably, but given both the anecdotal and quantitative evidence, the primary source of information for both politicians and voters is local media.

³⁰In order to pin down the remaining parameter, we set the number of pseudo-observations of the prior to $\beta_0 + \beta_1 = 20$. Our main results decomposing the effects of audits into channels are robust to this assumption. However, the results to our counterfactual policies on the audit probability are closely tied to the choice of pseudo-observations. A larger number implies less uncertainty in the initial prior and hence smaller changes in beliefs due to experience, which in turn leads to the estimation of larger effects for changes in the audit probability on rent extraction.

of rents stolen. These assumptions are made to identify γ_0 and γ_1 : they do not substantially affect our results other than by scaling our estimates for these two parameters. We estimate the vector of parameters $\theta := (\gamma_0, \gamma_1, \chi_0, \chi_1, \mu_\delta, \sigma_\delta, \sigma_\varepsilon, \alpha', \eta', \lambda')$ using Maximum Likelihood.

For a municipality i where the mayor is in his first term, the likelihood function is given by

$$L(\theta|r_i, R_i, q_i, X_i, Z_i, T_i = F) = f_\varepsilon(\varepsilon_i^F|q_i, X_i, Z_i, \theta)p(X_i, \varepsilon_i^F, q_i = 1, \theta)^{1\{R_i=1\}}(1 - p(X_i, \varepsilon_i^F, q_i = 1, \theta))^{1\{R_i=0\}}$$

where $\varepsilon_i^F = r_i - e_i^{F*} - X_i'\alpha - Z_i'\lambda$ denotes the mayor's unobserved ability shock, and f_ε is the shock's density function. We include the municipal characteristics Z_i additively and linearly in this term in order to control for heterogeneity across municipalities. Recall that $p(\cdot)$ denotes the equilibrium probability of reelection, where we set q_i equal to 1 here because an audit is realized for each of these observations.

If the mayor is in his second term, then the likelihood function is given by

$$L(\theta|r_i, R_i, q_i, X_i, Z_i, a_i, T_i = S) = f_\varepsilon(\varepsilon_i^S|q_i, X_i, Z_i, \theta)p(X_i, \varepsilon_i^S, q_i = a_i, \theta)$$

where $\varepsilon_i^S = r_i - e_i^{S*} - X_i'\alpha - Z_i'\lambda$ again denotes the mayor's unobserved ability shock. Note that for second-term mayors, whether the municipality was audited in the previous term enters the likelihood function by altering the probability of reelection as a function of ability—hence creating a selection effect. Thus, in the probability of reelection $p(\cdot)$, q_i is replaced with whether an audit was realized in the previous term (a_i).

We estimate the vector of parameters θ which maximizes the likelihood function:

$$\mathcal{L}(\theta|r, R, q, X, Z, a, T) = \prod_i L(\theta|r_i, R_i, q_i, X_i, Z_i, a_i, T_i)$$

We estimate the asymptotic covariance matrix of the maximum likelihood estimator by evaluating the Hessian of the likelihood function. We evaluate standard errors by the Delta method when appropriate.

6.2 Results

Identification. Before describing the parameter estimates, we briefly discuss their identification. Formally, the parameter vector θ is identified if for any other parameter vector $\theta' \neq \theta$, for some data (T, r, R, q, X, Z, a) , $\mathcal{L}(\theta'|r, R, T, q, X, Z, a) \neq \mathcal{L}(\theta|r, R, T, q, X, Z, a)$.

First, consider the legal parameters γ_0 and γ_1 . Ignoring selection on the unobservable for now, the parameter γ_0 is identified because we observe the rents of second-term mayors, while γ_1 is identified because we also observe the mean audit probability and the history of audits, which yield the mean perceived probability q_i (see equations (6) and (11)). For instance, $\gamma_1 = 0$ would imply that second-term rents are uncorrelated with the perceived audit probability. Second, σ_D and χ_0 are identified since we jointly observe rents and reelection outcomes (equation (9)) as well as the difference in rents between first and second-term mayors (see equations (2), (6) and (10)). These parameters will determine the size of the selection effect on second-term rents that was necessary to pin down γ_0 and γ_1 . Since χ_0 is identified, the parameter χ_1 is also identified as we observe q_i . Next, the vectors (α', λ') and η are identified by variation, respectively, in rents and reelection probabilities as a function of observable characteristics. Finally, the parameter σ_ε follows from the variance of the rents distribution and μ_D follows from the mean reelection probability.

Parameter estimates. Table 10 reports maximum likelihood estimates for our parameters of interest. The first two rows present the estimates for the probability of legal action. The estimate for the constant γ_0 is 0.0245. This implies that for a mayor who extracted average rents in the data ($r = 3.9825$), the probability of legal action when no audit occurs is 9.8 percent. This estimate is close to the mean number of legal actions which occur during a mayoral term ($0.029 \times 4 = 11.6$ percent, reported in Table 4). The positive, statistically significant estimate for γ_1 of 0.0052 implies that the realization of an audit increase the probability of legal action by 2.1 percentage points for a mayor who extracted average rents. This represents a 21 percent increase from the baseline probability when no audit occurs, which is remarkably close to the 20 percent increase we estimated with the legal action data in the reduced-form section. Therefore, these results suggest that the history of audits in a municipality and its neighbors, through its effect on the belief over the probability of an audit, significantly affects the rents extracted by mayors.

The next two rows of Table 10 report estimates for the probability that the voter observes rents. The estimate for the constant χ_0 is 0.0147, which implies that, if no audit is realized, the probability that the voter observes rents is approximately 1.5 percent. The estimate for χ_1 implies that this probability increases by 8.77 percentage points if an audit is realized. This result is consistent with

the hypothesis that audits affect electoral discipline and selection. In the final two rows we report estimates for two more structural parameters. We estimate the standard deviation of the ability shock to be 0.3366. Since it is significantly larger than zero, the estimate implies that there is scope for voters to select mayors who extract less rents during elections. The final estimate reported is for the mean of the popularity shock. The estimate is positive, but not statistically significant. Thus, in the current sample, we do not find evidence for an incumbency advantage, which is consistent with the empirical literature for Brazilian municipalities (Klašnja and Titiunik, 2014).

In the rents and reelection terms within the likelihood equation, we also include the vector of mayoral characteristics. We report the coefficients for each characteristic in Table A.4. In column 1, we find that rents are uncorrelated with gender and negatively correlated with education and quality of occupation (captured by a dummy for white collar occupations). However, these estimates are statistically indistinguishable from zero. In column 2, we find that reelection rates are positively correlated with education, white collar occupation and male gender, but again none of these correlations are statistically significant. Since we also do not find any reduced-form evidence that candidate characteristics depend on the history of audits in a municipality, it is unlikely that candidate entry explains why audits reduce corruption. We test this hypothesis further in the counterfactuals section.

Equilibrium outcomes. Given the maximum likelihood estimates, we compute predicted rents (log acts of corruption) for all mayors in the sample. The average predicted rents for mayors are 3.9825, the same as average rents in the estimation sample. To assess goodness-of-fit, we perform a Likelihood Ratio Test comparing the unrestricted model to a restricted model where only γ_0 , σ_ε , μ_D and the lottery and state fixed effects are estimated. The restricted model is essentially one where a constant determines rents and a separate constant determines the reelection rate. We strongly reject the hypothesis that the restricted model is true ($\chi^2 = 159.37$, p -value $< 10^{-16}$).

To assess the out-of-sample fit of the model, we use data from the most recent audits which were not used in the structural estimation (i.e. the audits uncovering corruption from the 2012-2016 term). We test whether the structural model predicts out-of-sample corruption more accurately than an OLS model with the same set of explanatory variables. Using the parameter estimates for each model computed with the same sample of 839 observations, we compute predicted rents for the additional 239 observations from the most recent audits. We find the mean squared deviation between predicted and observed rents to be 0.140 when using the structural estimates compared to 0.161 when using the OLS estimates. Thus, the structural model outperforms the OLS model when

fitting out-of-sample data on corruption.³¹ We plot the data against rents predicted by the structural model in Figure 6.

To assess the fit of the Bayesian learning model, we regress rents and the posterior beliefs about the audit probability on mayor and municipal characteristics, number of service orders, number of neighbors, lottery and state fixed effects. Figure 7 presents the residuals of these regressions in a binned scatter plot. Recall that the posterior over the audit risk increases when agents within a municipality observe a larger proportion of audits than would be predicted by their prior belief. This plot shows that in such cases, mayors extract less rents. Likewise, in municipalities with histories where agents observe a smaller proportion of audits, the figure suggests that mayors extract more rents. Overall, the relationship between rents and the belief about the audit risk appears to be well approximated by a linear fit. Moreover, the R^2 of a linear regression (with the aforementioned controls) of rents on the posterior belief is larger than that obtained from a linear regression of rents on the number of audits observed in the municipality.

Decomposing the effects of the audits. We decompose the effect of the audits on rents through legal discipline, electoral discipline and selection, and report the results in Table 11. The effect of legal discipline is computed by setting $\gamma_1 = 0$ for all observations and computing predicted rents under this condition. The condition implies that mayors are choosing their actions as if the probability of legal action were only γ_0 instead of $\gamma_0 + \gamma_1 q_i$, that is, as if the agents were in a counterfactual setting where audits do not affect the probability of legal action. We then compare mean predicted rents in this counterfactual setting to those derived using our estimated parameters. We find that rents are on average 13.8 percent lower due to the effect of audits on legal discipline.

We quantify the effect of audits on the electoral discipline and selection channels using a similar methodology. We eliminate both channels by setting $\chi_1 = 0$. To back out electoral discipline, we then compare the counterfactual first-term rents under this condition to those predicted by our maximum likelihood estimates. We do not compare second-term rents as our model restricts electoral discipline to first-term mayors.³² We find that audits reduce rents through electoral discipline by 5.3 percent.

³¹The structural model also outperforms the restricted model described in the previous paragraph, which yields a mean squared deviation of 0.179. Furthermore, we find a similar result when using absolute deviations instead of squared deviations. The mean absolute deviation between predicted and observed rents is 0.280 when using the structural estimates compared to 0.291 for the OLS model and 0.317 for the restricted model.

³²We justify this assumption with our reduced-form evidence presented in Table 5 and discussed in Section 5, where we did not find evidence for electoral discipline driven by future political career concerns. We also estimated the structural model adding a reduced-form term to each mayor's effort level that is a linear function of the predicted probability of running in the future. We do not find significant effects on the coefficients for the predicted probability of running (the results are available upon request).

Next, we measure selection by comparing second-term rents when $\chi_1 = 0$ to those predicted by our estimates. This channel captures the effect of audits on the distribution of the ability of second-term mayors. The comparison shows that selection plays a negligible role: rents are on average less than 0.1 percent lower due to this channel. While the negligible selection effect may appear surprising at first, it can be explained by the fact that few municipalities in our sample are affected by the selection effect of audits, whereas all are affected by its disciplining effect. This is because only 30 percent of our sample are second-term mayors, of which only 10 percent were audited in the previous term. If we restrict our analysis to this subsample of affected mayors, we find that audits reduce rents by 2.4 percent due to selection over unobserved ability. Thus while audits do affect selection, very few municipalities are subject to this effect.

Overall, the above results suggest that in our sample approximately 72 percent of the reduction in rents caused by audits is due to legal discipline, 28 percent is due to electoral discipline and less than 1 percent is due to selection. The importance of the legal discipline channel in reducing rents is consistent with our reduced-form findings.

Policy counterfactuals. We parameterize the model with the estimates reported in Tables 10 and A.4 and conduct a number of policy simulations. The results are reported in Table 12.

We begin by simulating changes in the audit probability. Since agents in the model are assumed to have a rational mean prior, increasing the audit probability amounts to increasing the mean of the prior distribution by exactly the same amount. This increases the posterior beliefs for the probability of audit for all mayors in the sample. We find that a 10 percentage point increase in the audit probability—roughly equivalent to doubling the audit probability—reduces rents by an average of 14.6 percent for first-term mayors and by 9.3 percent for second-term mayors. The slightly larger effect for first-term mayors stems from electoral discipline reducing first-term rents more rapidly than the selection effect reduces second-term rents. Assuming that the effect of the audit probability on rent-seeking is linear, our results are similar to those found in the literature. For instance, [Zamboni and Litschig \(2015\)](#) find that a 20 percentage points increase in the objective audit probability for a group of Brazilian municipalities decreased corruption by approximately 20 percent. Moreover, [Bobonis et al. \(2015\)](#) find that in the context of a long-standing audit program of municipalities in Puerto Rico, releasing audit reports just prior to an election induces a reduction in corruption by 67 percent. [Olken \(2007\)](#) finds that an increase in the audit probability from 4 to 100 percent for construction projects in Indonesian villages led to a reduction in missing expenditures by 30 percent. Although we find similar results, we caution that our estimates are sensitive to the assumptions in the learning framework we have used to model the effects of audits, and in

particular, the parametrization of the prior distribution.

We next study the extent to which mayors can be disciplined by increasing the legal penalties associated with corruption. Recall that legal costs are assumed to have the linear functional form $b_0 + b_1 e_{iT}$ and that expected legal costs are given by the product of the legal costs and the probability of legal action. We simulate percent increases in the parameter b_1 , which in practice would map to increases in the percentage of resources stolen which must be paid when one is caught. We find similar effects for mayors in both terms: increasing the legal cost on rents extracted by 10 percentage points reduces average rents by 9.8 percent for first-term mayors and by 9.7 percent for second-term mayors.

Given the importance of the media in disseminating information and the large spillover effects we document in Section 5, a third policy prescription we study is a change in access to information about neighboring audits. We simulate the model under the assumption that every municipality has access to information from its neighbors—equivalently, we simulate the model under the assumption that every municipality has access to local radio which reports on neighboring audits. We find that on average, first-term rents are 2.39 percent lower and second-term rents are 1.31 percent lower under this counterfactual setting. The comparatively modest effects stem from the fact that with more access to information, mayors and voters are more likely to update their priors about the audit risk in both directions. Thus, some agents will acquire additional information which leads them to overestimate the probability of audits, while others will acquire information which leads to underestimation. Unless agents have biased priors or do not update their beliefs using Bayes' rule, the effects of this policy on rents will be comparatively minor compared to the first two policy counterfactuals we considered.

Another mechanism which has garnered much attention is political entry. We consider whether significant gains could be made in curbing corruption by instituting formal requirements to run for office. The following counterfactuals are at best suggestive as mayor characteristics may capture unobserved heterogeneity in the estimation, in which case our results are likely upper bounds for the true effect sizes. We find modest effects however. Requiring mayors to have a college degree only decreases average rents by 1 percent, whereas requiring mayors to have previously been employed in a white collar occupation reduces average rents by about 3 percent.

7 Conclusions

This paper shows that the use of anti-corruption audits can be an effective policy in the fight against corruption. We find that in the case of Brazil’s municipalities, corruption is 8 percent lower in places that have been audited in the past compared to municipalities that have not been audited. Naturally, this estimated impact captures only partial, short-term equilibrium effects. In the presence of spillovers, our estimates are likely to represent underestimates of the true impact, and we provide some evidence of this by showing that corruption is lower in municipalities where a neighbor was audited and local media is present to diffuse the information. We also show that audits increase the legal actions taken against corrupt mayors by increasing the chances of a police crackdown or a conviction in court.

By highlighting how audits can help spur legal sanctions, our findings offer important policy implications. While the existing literature has shown that information obtained through audits can help promote electoral accountability, this channel alone might not be sufficient to reduce corruption in the long run, especially if in response, public officials are able to adjust their electoral strategies or find alternative forms of corruption (Bobonis et al. (2015), Olken and Pande (2012)). A sustainable reduction in corruption may instead require policies aimed at improving the state’s capacity to detect and prosecute corrupt politicians (e.g. Besley and Persson (2011)). Our results suggest that channeling resources to anti-corruption agencies who can implement well-executed random audits may be an important step towards this direction.

Although we have emphasized the importance of legal accountability for reducing political corruption, our understanding of how best to improve a country’s legal system remains limited, particularly in a context where corruption is endemic. More research is needed to better understand how we can improve the selection of public prosecutors and judges, and the incentives they face to punish corrupt politicians.

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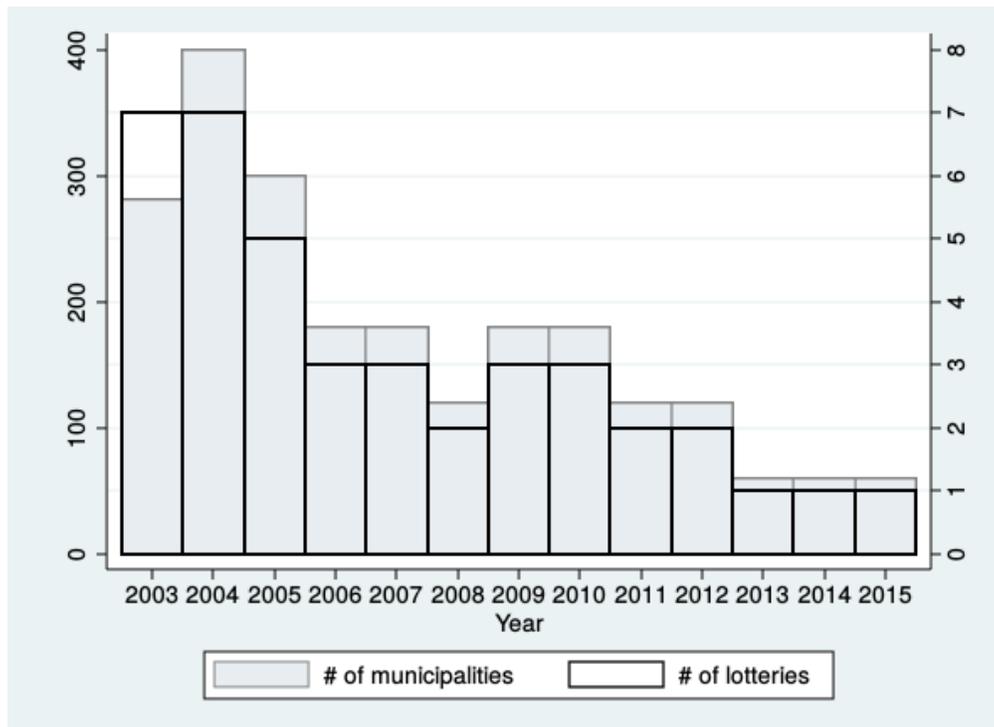
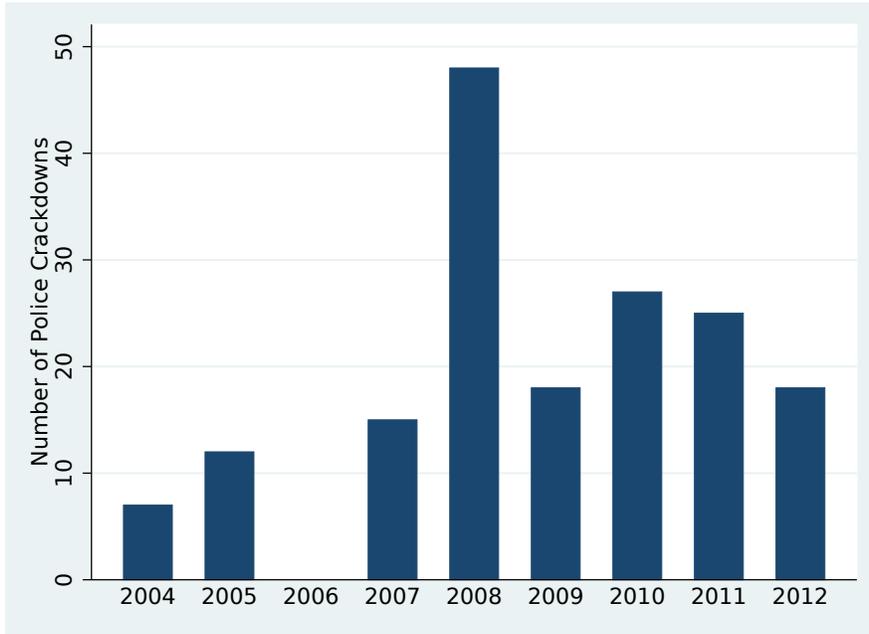
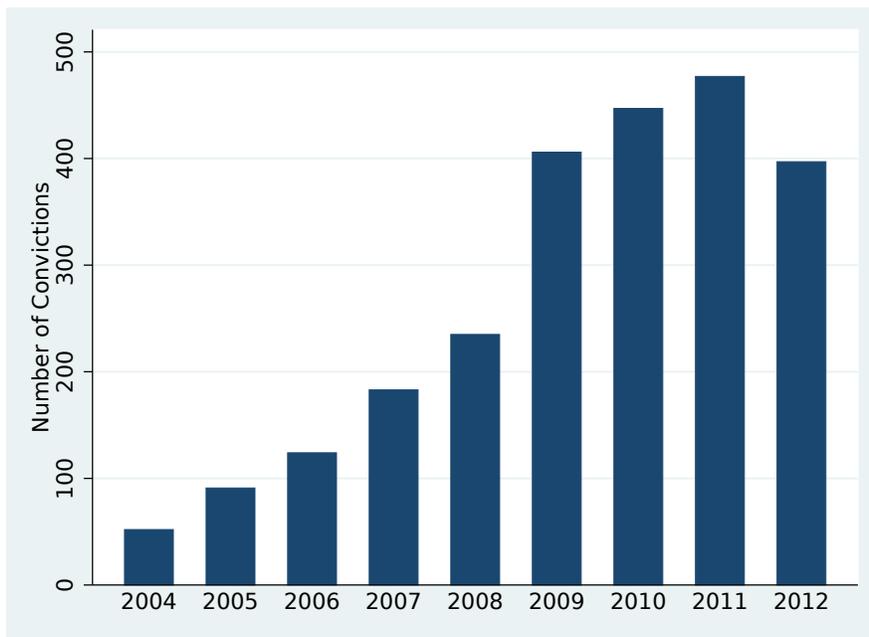


Figure 1: Number of Lotteries and Municipalities Audited Per Year

Notes: This figures plots the number of lotteries and the number of municipalities that have been audited during the duration of the program.



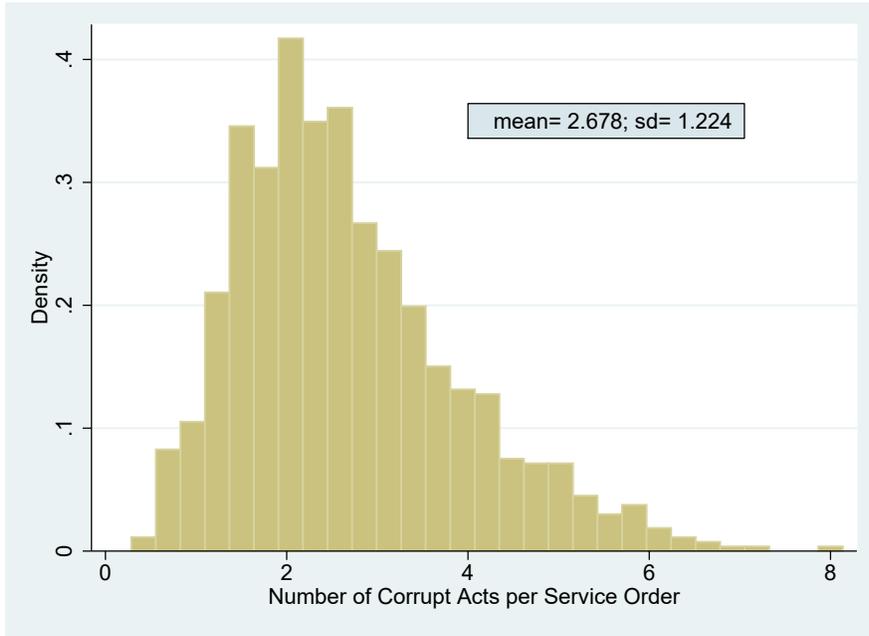
Panel A: Police Crackdowns



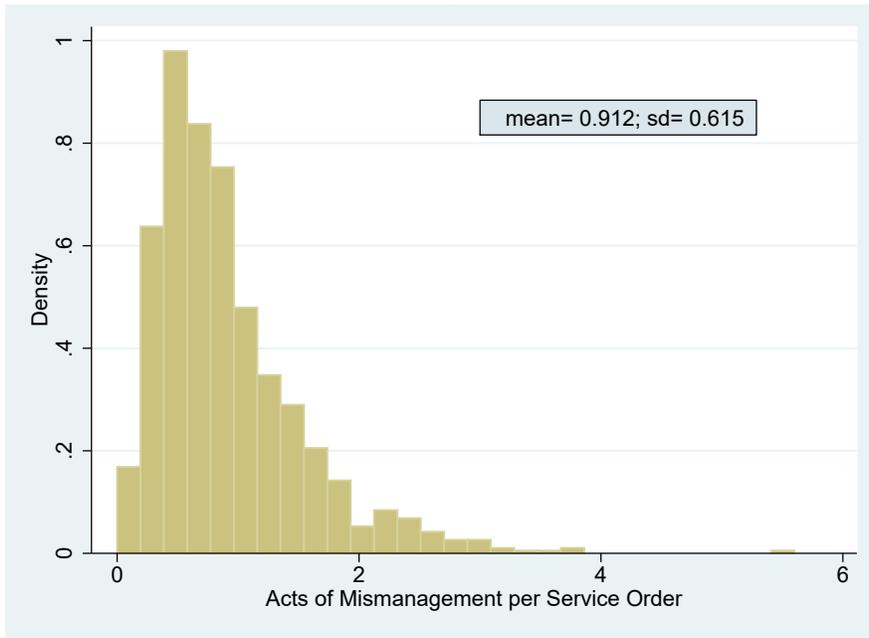
Panel B: Convictions

Figure 2: Number of Legal Actions over Time

Notes: This figures plots the number of convictions and police crackdowns involving political corruption during the period 2004 to 2012.



Corruption



Mismanagement

Figure 3: Distribution of Irregularities Associated with Corruption and Mismanagement

Notes: This figure displays the distribution of irregularities per service order associated with corruption and mismanagement. These data are based on the audits conducted from lotteries 22 to 38.

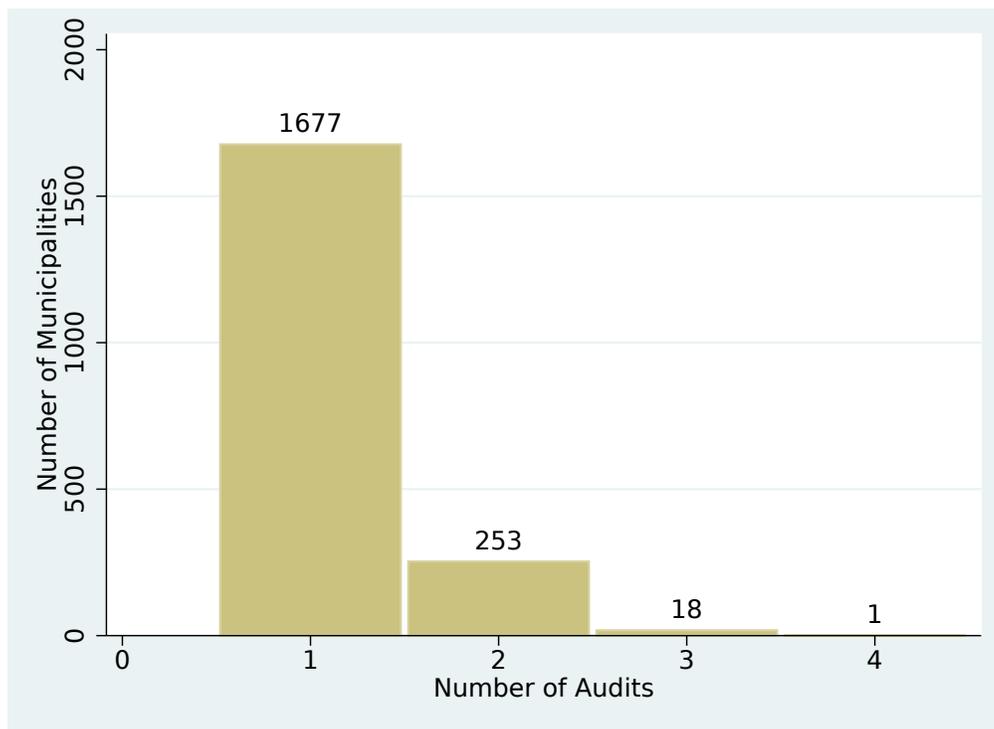


Figure 4: Distribution of Times a Municipality has been Audited

Notes: This figures plots the distribution of the number of times a municipality has been audited during the duration of the program.

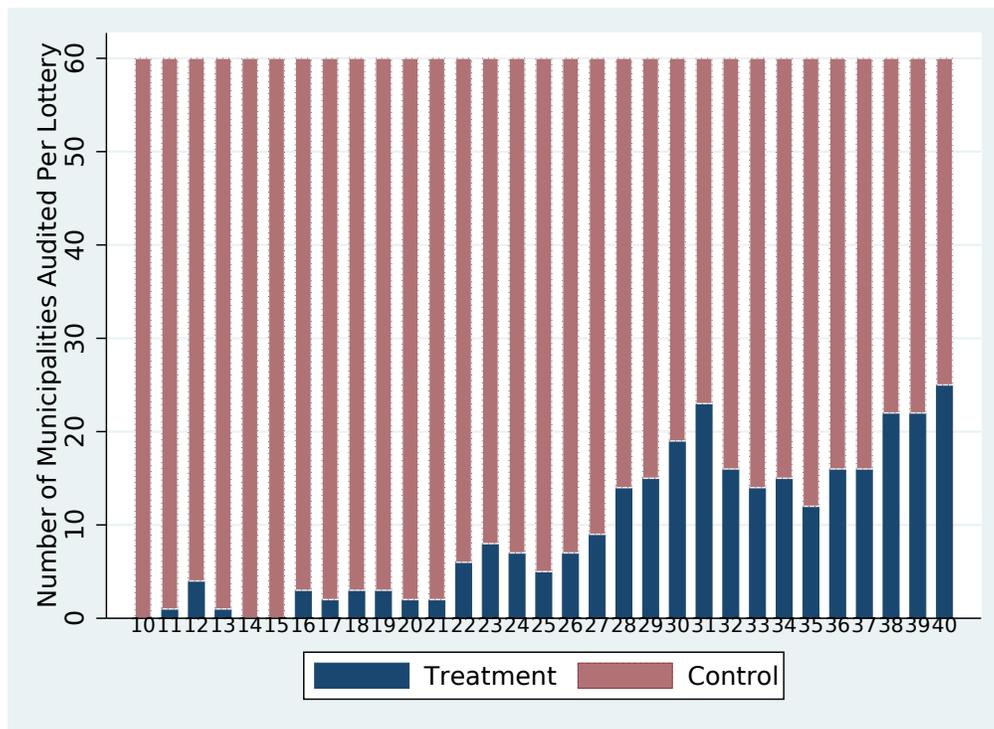


Figure 5: Distribution of Times a Municipality has been Audited

Notes: This figure plots the number of municipalities that had been audited in the past for a given lottery. The solid blue bars denote the number of treated municipalities (i.e. previously audited). The solid red bars denote the number of control municipalities.

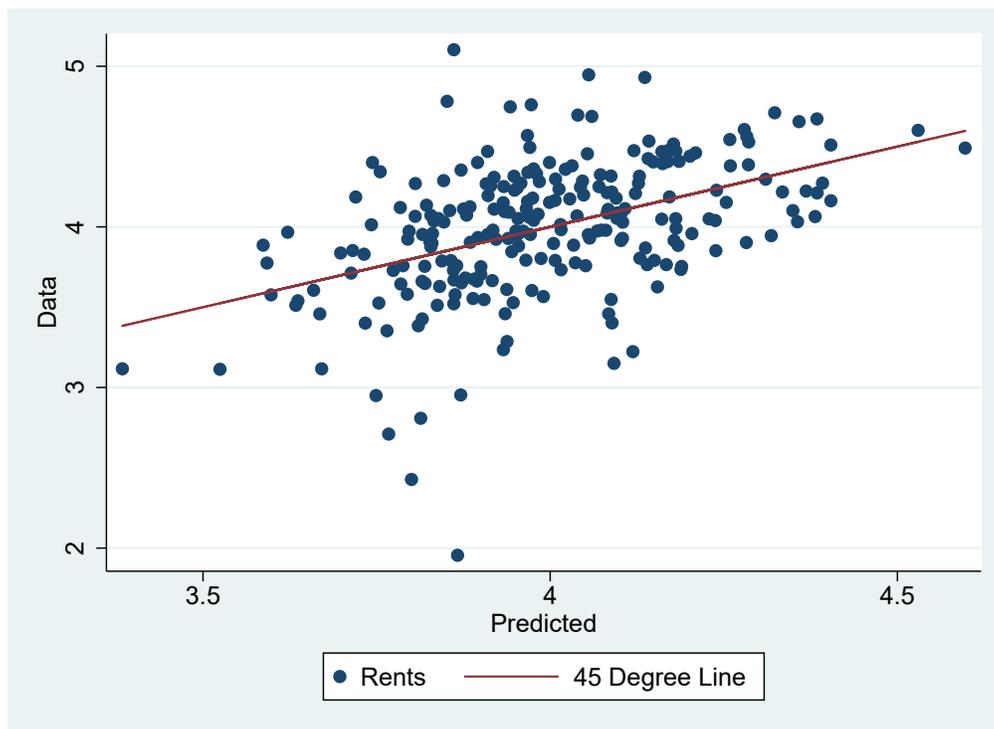


Figure 6: Out-of-Sample Fit for Rents

Notes: This figure displays predicted and actual rents for 239 audits which occurred in the period following those used in our estimation sample. Predicted rents are computed using the maximum likelihood estimates.

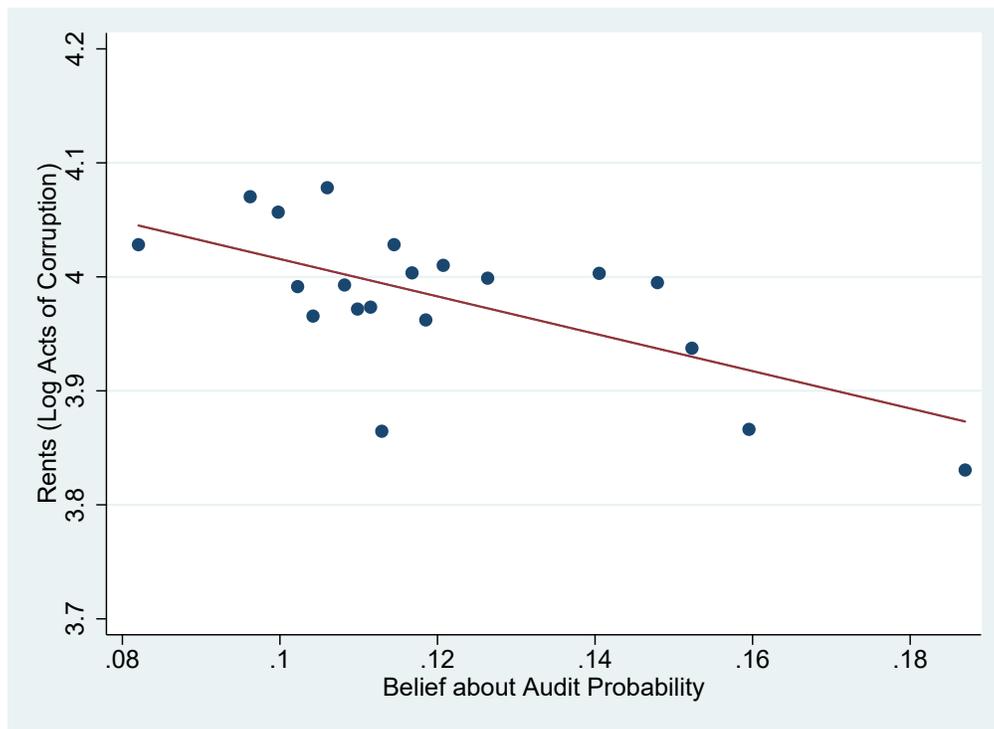


Figure 7: Rents and the Belief over the Audit Probability

Notes: This figure displays a residuals binned scatter plot of rents on the mean belief over the audit probability. The regressions control for mayor and municipal characteristics, number of service orders, number of neighbors, lottery and state fixed effects.

Table 1: Mean Comparisons Between Audited and Non-audited

	Control		Treatment		Difference (5)
	Mean (1)	Std Dev. (2)	Mean (3)	Std Dev. (4)	
Population	22992.720	45069.940	26000.850	43799.660	436.700 [2553.579]
Share female	0.495	0.015	0.496	0.014	0.000 [0.001]
Share urban	0.574	0.235	0.576	0.234	0.008 [0.014]
Human Development Index	0.507	0.105	0.492	0.101	-0.002 [0.004]
Income inequality (Gini)	0.550	0.068	0.563	0.069	0.003 [0.005]
Income per capita (log)	5.575	0.580	5.499	0.582	-0.001 [0.026]
% Poor	0.445	0.229	0.486	0.215	0.502 [0.821]
Share illiterate	0.247	0.136	0.268	0.134	0.303 [0.494]
% bureaucracy with a college degree	0.192	0.123	0.180	0.118	-0.007 [0.006]
% population with a college degree	0.207	0.212	0.204	0.229	0.009 [0.011]
Has AM Radio	0.211	0.408	0.243	0.430	0.017 [0.032]
Has a Judiciary District	0.447	0.497	0.523	0.501	0.002 [0.038]
Effective Number Candidates for Mayor	2.150	0.550	2.204	0.648	0.044 [0.038]
Reelection rates for Mayors	0.405	0.491	0.437	0.497	0.026 [0.048]
Mayor's Years of Education	12.009	4.194	11.868	4.355	-0.229 [0.387]
Share of Votes Mayor received	0.561	0.125	0.564	0.133	0.006 [0.010]
Number of Service Orders	25.205	9.264	24.802	9.983	-0.169 [0.618]
N	881		222		

Notes: This table shows means and standard deviations of various municipal characteristics by places that have been audited in the past (treatment) and places that have not been audited in the past (control). The difference and corresponding standard error (in brackets) are computed based on a regression that controls for both state and lottery fixed effects. All of these characteristics are based on information collected in 2000, except for the share of the bureaucracy with a college degree, which is based on a 2005 survey.

Table 2: The Effects of the Audits on Corruption and Mismanagement

	Number of Irregularities			Acts of Mismanagement			Acts of Corruptions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audited in the past	-0.034 [0.021] (0.10)	-0.045+ [0.022] (0.03)	-0.058* [0.021] (0.01)	0.010 [0.052] (0.61)	0.001 [0.048] (0.53)	-0.023 [0.041] (0.31)	-0.059* [0.024] (0.05)	-0.070* [0.025] (0.02)	-0.079* [0.027] (0.01)
Population (log)		0.057* [0.011]	0.064* [0.012]		0.047* [0.015]	0.037+ [0.021]		0.053* [0.015]	0.064* [0.018]
Income inequality (Gini)		0.337* [0.111]	0.361* [0.112]		0.137 [0.295]	0.177 [0.249]		0.449* [0.158]	0.459* [0.177]
Income per capita (log)		-0.085 [0.056]	-0.102* [0.045]		0.111 [0.086]	0.103 [0.087]		-0.158* [0.064]	-0.176* [0.059]
Illiteracy		0.003 [0.002]	0.003* [0.001]		0.001 [0.003]	0.000 [0.002]		0.004* [0.002]	0.005* [0.002]
Share of urban population		0.123+ [0.067]	0.118* [0.045]		-0.056 [0.091]	-0.068 [0.136]		0.190* [0.086]	0.182* [0.058]
Controls	N	Y	Y	N	Y	Y	N	Y	Y
f(Service Orders)	log	log	nonpar	log	log	nonpar	log	log	nonpar
R ²	0.414	0.449	0.539	0.364	0.374	0.434	0.303	0.337	0.483
N	983	983	983	983	983	983	983	983	983

Notes: This table reports the effects of being audited in the past on corruption and mismanagement. The dependent variable in columns 1-3 is the log of the total number of irregularities discovered in the audit. In columns 4-6, the dependent variable is the log of total acts of mismanagement, and in columns 7-9 the dependent variable is the log of total acts of corruption. In addition to the controls presented in the table, each regression controls for state and lottery fixed effects. In columns 3, 6, 9 the number of service items audited is controlled for in a fully nonparametric fashion. In the other columns, we control for the log of the number of service items audited. P-values based on randomization inference reported in the parentheses. The p-values were computed based on 1,000 random draws. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 3: Spillover Effects of Neighboring Audits on Acts of Corruption

	Acts of Corruption				
	(1)	(2)	(3)	(4)	(5)
Audited in the past	-0.078*	-0.081*	-0.086*	-0.093*	-0.094*
	[0.028]	[0.028]	[0.028]	[0.028]	[0.028]
Neighbors Audited	-0.020	0.003	0.010	0.006	0.098
	[0.015]	[0.016]	[0.016]	[0.016]	[0.162]
Radio AM		0.065	0.050	0.044	0.065
		[0.046]	[0.046]	[0.046]	[0.046]
Neighbors Audited × Radio AM		-0.075*	-0.050+	-0.052+	-0.073*
		[0.028]	[0.030]	[0.030]	[0.034]
TV			0.012	0.013	0.032
			[0.054]	[0.055]	[0.055]
Neighbors Audited × TV			-0.083*	-0.081*	-0.094*
			[0.036]	[0.036]	[0.038]
Same Party Audited				-0.005*	-0.005*
				[0.002]	[0.002]
Full Set of Interactions	N	N	N	N	Y
N	983	983	983	983	983
R ²	0.65	0.65	0.65	0.67	0.67

Notes: This table reports the indirect effects on corruption of one's neighbor or one's political party being audited. The dependent variable is the log of the total acts of corruption discovered in the audit. The independent variable "Same Party Audited" is the number of times in a given term a mayor from the same party and from within the same state was audited. In addition to the municipal controls presented in Table 2, each regression controls the following set of fixed effects: state, lottery, service order, number of neighbors, and political party (for columns 4 and 5). In column 5, we interact Neighbors Audited with the full set of municipal controls. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 4: The Effects of the Audits on Legal Actions

	Crackdowns		Convictions		Legal Action		Crackdowns	Convictions	Legal Action
	(1)	(2)	(3)	(4)	(5)	(6)			
Audited	0.00139 [0.001]	-0.0000887 [0.001]	0.00443+ [0.002]	0.000195 [0.003]	0.00562* [0.003]	0.000241 [0.003]			
Audit × Judiciary District		0.00325+ [0.002]		0.00933* [0.004]		0.0119* [0.005]			
Corruption (logs)							0.0369+ [0.021]	0.0601* [0.029]	0.0882* [0.035]
Mismanagement (logs)							-0.0116 [0.016]	-0.00647 [0.02]	-0.0146 [0.024]
Control group mean	0.003	0.003	0.025	0.025	0.029	0.029	0.047	0.202	0.240
N	70,902	70,902	70,902	70,902	70,902	70,902	982	982	982

Notes: This table investigate the effects of the audits on the occurrence of a legal action. In columns 1, 2 and 7, the dependent variable is whether a police crackdown on political corruption was conducted in the municipality in a given year. In columns 3, 4, and 8, the dependent variable is whether a mayor was prosecuted for corruption in a given year. In columns 5, 6 and 9, the dependent variable is whether a police investigation or a conviction occurred. Each regression controls for our set of municipal controls. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 5: The Effects of the Audits on Corruption

	Acts of Corruption					
		Same Term		Reelected	Full Sample	Open Seat
	(1)	(2)	(3)	(4)	(5)	(6)
Audited in the past	-0.127*	-0.113	-0.133+	-0.149*		-0.122
	[0.061]	[0.070]	[0.069]	[0.060]		[0.076]
	(0.063)	(0.127)	(0.057)	(0.02)		(0.22)
Second-term mayor		-0.032				
		[0.029]				
Audited in the past × Second-term mayor		-0.050				
		[0.098]				
Audited in the past × Propensity to seek higher office			-0.025			
			[0.366]			
Propensity to seek higher office			-0.066			
			[0.106]			
Audited one term ago					-0.078*	
					[0.028]	
					(0.056)	
Audited two or more terms ago					-0.074+	
					[0.038]	
					(0.078)	
R^2	0.47	0.47	0.47	0.50	0.48	0.53
N	821	821	821	596	983	665

Notes: This table reports the effects of being audited in the past on corruption. The dependent variable is the log of the total acts of corruption discovered in the audit. In addition to the municipal controls presented in 2, each regression controls for state, lottery, service order fixed effects. P-values based on randomization inference reported in the parentheses. The p-values were computed based on 1,000 random draws. Robust standard errors are reported in brackets, + $p < 0.10$, * $p < 0.05$.

Table 6: The Effects of the Audits on Entry

	Win Margin (1)	Number of Candidates (2)	Number of Parties (3)	Characteristics of the Candidate Pool			Mayor Characteristics			
				Elementary School (4)	High School (5)	College (6)	Campaign Spending (7)	White Collar (8)	Male (9)	College (10)
Audited in the past	0.010 [0.020]	-0.002 [0.029]	0.000 [0.028]	-0.024 [0.023]	0.026 [0.028]	0.000 [0.028]	-0.018 [0.068]	-0.009 [0.043]	-0.027 [0.030]	0.030 [0.047]
R^2	0.07	0.30	0.31	0.15	0.23	0.21	0.65	0.24	0.11	0.12
N	665	684	684	684	684	684	672	662	679	685

Notes: This table reports the effects of being audited on the candidate pool. The dependent variable is specified at the top of each column. The number of candidates, number of parties, and campaign spending are measured in logs. In addition to the municipal controls presented in 2, each regression controls election and state fixed effects. The sample is restricted to open-seat elections. Robust standard errors clustered at the municipality level are reported in brackets, + p<0.10, * p<0.05.

Table 7: The Effects of the Audits on Displacement

	Full Sample			Same Term			Acts of Corruption (7)
	Embezzlement (1)	Procurement (2)	Over-invoicing (3)	Embezzlement (4)	Procurement (5)	Over-invoicing (6)	
Audited in the past	0.031 [0.084]	0.011 [0.060]	-0.026 [0.029]	-0.132 [0.128]	0.117 [0.103]	-0.050 [0.061]	
Share of same sectors audited							0.200 [0.266]
R^2	0.13	0.21	0.03	0.14	0.21	0.04	0.69
N	983	983	983	821	821	821	217

Notes: This table reports the effects of being audited in the past on type of corruption detected. Embezzlement, Procurement, Over-invoicing correspond to the number of acts of corruption involving these procedures as a share of the total number of corrupt violations. In columns 1-3, the regressions are estimated for the entire sample. In columns 4-6, the treatment is restricted to those mayors that were audited twice in a single term. In addition to the municipal controls presented in 2, each regression controls for state, lottery, service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 8: The Effects of Audits on Public Spending

	Education (1)	Health (2)	Administration (3)	Housing (4)	Welfare (5)	Transportation (6)	Other (7)
Audited in the past	-0.006 [0.006]	-0.006 [0.005]	0.009 [0.008]	0.005 [0.005]	0.000 [0.003]	-0.004 [0.003]	0.001 [0.004]
R^2	0.598	0.235	0.204	0.290	0.275	0.502	0.421
N	773	773	773	773	773	773	773

Notes: This table reports the effects of being audited in the past on public spending. Public spending data are obtained from the IPEA. The dependent variable is the share of public spending on one of seven mutually exclusive categories: education, health, administration, housing, welfare, transportation, and other spending. In addition to the municipal controls presented in 2, each regression controls for state, lottery and service order fixed effects. The sample size is less than 983 due to missing data on public spending, in particular for audits which occurred in 2012 as the IPEA data ends in 2011. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 9: The Effects of the Audits on Federal Block Grants

	Number of Block Grants (1)	Amount of Block Grants (2)	Share of Funds Disbursed (3)
Audited in the past	-0.027 [0.050]	-0.053 [0.077]	0.018 [0.018]
R^2	0.55	0.42	0.34
N	794	794	793

Notes: This table reports the effects of being audited in the past on the amount of blocks grants the municipality received in the subsequent years of the administration. The dependent variables in columns 1 and 2 are expressed in logs. In addition to the municipal controls presented in 2, each regression controls for state, lottery, service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table 10: Structural Estimates of Interest

	Parameter Estimate (1)
Probability of legal action	
constant (γ_0)	0.0245 [0.0003]
audit (γ_1)	0.0053 [0.0025]
Probability of voter observing rents	
constant (χ_0)	0.0147 [0.0076]
audit (χ_1)	0.0877 [0.0496]
Standard deviation of ability shock (σ_ε)	0.3366 [0.0075]
Mean of popularity shock (μ_D)	0.0028 [0.0113]

Notes: This table reports the maximum likelihood estimates. The first two rows report parameter estimates for the probability of legal action. The constant denotes the probability of legal action conditional on the realization of no audit. The audit coefficient denotes the increase in the probability of legal action when an audit is realized. Rows 3 and 4 report analogous parameter estimates for the probability of the voter observing the rent signal. The last two rows report estimates of other parameters of interest. Number of observations 839. Log likelihood -682.01.

Table 11: Reduction in Rents Due to Audits by Channel

	Average Difference in Rents (1)
Due to:	
Legal discipline	0.138 [0.067]
Electoral discipline	0.053 [0.030]
Selection	0.0007 [0.0004]
Total	0.192 [0.057]

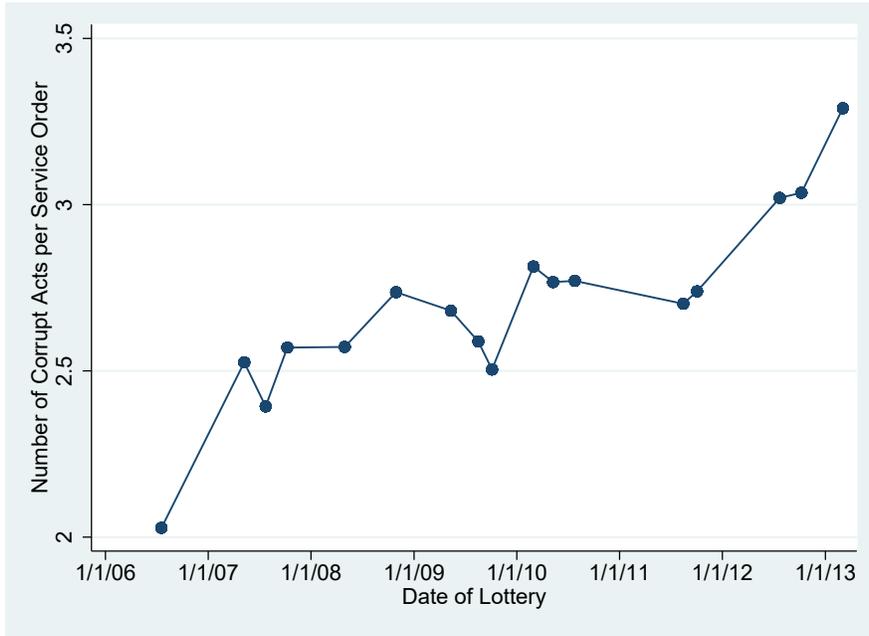
Notes: This table reports the difference in average rents between those predicted by the maximum likelihood estimates and those predicted by varying counterfactuals. Each row reports the difference for a separate counterfactual with the final row denoting the sum of the first three rows. Each counterfactual represents a setting where audits do not affect the respective channel; the legal discipline counterfactual rents are those under the assumption $\gamma_1 = 0$ and the electoral discipline and selection counterfactual rents are first and second term rents respectively under the assumption $\chi_1 = 0$. Standard errors are computed using the Delta method.

Table 12: Counterfactuals

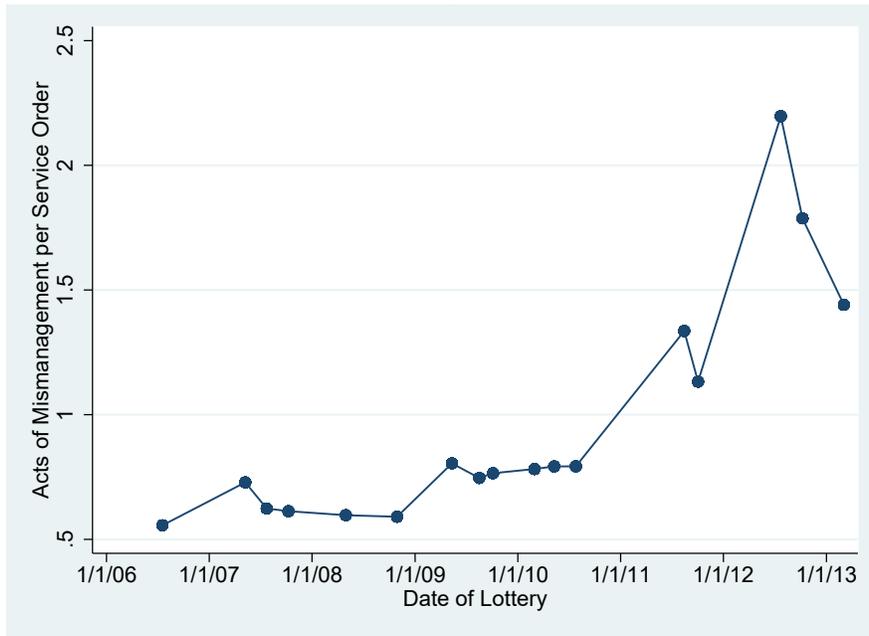
	Average Decrease in Rents (%)	
	First-term (1)	Second-term (2)
Audit probability		
10 percentage point increase	14.6	9.3
20 percentage point increase	28.5	18.3
Legal cost (b_1)		
10 percentage point increase	9.8	9.7
20 percentage point increase	19.1	18.9
Local radio access to neighbors		
All municipalities have radio	2.39	1.31
Mayor characteristics		
All mayors college educated	1.08	1.09
All mayors white collar	3.48	3.25

Notes: This table reports the difference in average predicted rents between the maximum likelihood estimates and the following set of policy counterfactuals. The first set increases the audit probability of all municipalities. The second increases the legal cost parameter b_1 associated with rent extraction. The third sets all municipalities to have access to information about neighboring audits. The fourth alters the characteristics of incumbent mayors.

A Appendix A



Corruption



Mismanagement

Figure A.1: Average Number of Irregularities By Lottery

Notes: This figure displays by lottery the average number of irregularities per service order associated with corruption or mismanagement. These data are based on the audits conducted in lotteries 22-38.

Table A.1: Probability of Being Audited

	Lottery	Year	Term
Alagoas	1.4	4.9	14.7
Bahia	1.1	4.3	12.5
Ceará	1.6	5.5	16.3
Espírito Santo	1.3	5.3	14.7
Goiás	1.1	4.5	11.8
Maranhão	1.1	4.0	12.0
Minas Gerais	0.8	3.1	8.6
Mato Grosso do Sul	1.6	6.4	17.2
Mato Grosso	1.3	5.2	13.6
North	1.7	6.3	16.3
Paraíba	1.1	3.9	11.6
Pernambuco	1.4	4.7	14.6
Piauí	1.1	4.1	11.8
Paraná	0.8	3.4	9.2
Rio de Janeiro	2.3	11.5	26.4
Rio Grande do Norte	1.5	5.2	16.1
Rio Grande do Sul	0.9	3.5	9.7
Santa Catarina	0.8	3.7	9.6
Sergipe	1.8	5.7	17.2
São Paulo	0.8	3.2	9.1

Notes: This table shows the probability of being audited by state for a given time period. Column 1 is the probability of being audited in a given lottery. Column 2 is the probability of being audited in a given year. Column 3 is the probability of being audited in a given term.

Table A.2: The Effects of the Audits on Corruption and Mismanagement By Local Characteristics

	Acts of Mismanagement (logs)						Acts of Corruption (logs)					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Audited in the past	-0.023 [0.041]	-0.022 [0.041]	-0.024 [0.040]	-0.022 [0.041]	-0.029 [0.042]	-0.030 [0.042]	-0.079* [0.027]	-0.080* [0.028]	-0.086* [0.030]	-0.081* [0.029]	-0.076* [0.027]	-0.079* [0.028]
Treatment Interacted with Radio	-0.036 [0.114]					-0.036 [0.114]	-0.058 [0.080]					-0.009 [0.063]
% pop with a college degree		0.001 [0.014]				0.001 [0.021]		-0.017 [0.014]				-0.020 [0.016]
Income per capita [log]			-0.009 [0.064]			-0.054 [0.104]			-0.063 [0.053]			0.024 [0.072]
Share of urban population				0.093 [0.203]		0.260 [0.272]				-0.166 [0.121]		-0.109 [0.125]
Judiciary District					0.016 [0.084]	-0.012 [0.079]					-0.023 [0.047]	0.006 [0.042]
P-value (Joint test)						0.87						0.46
R2	0.43	0.43	0.43	0.43	0.43	0.44	0.48	0.49	0.48	0.48	0.48	0.49
N	983	983	983	983	983	983	983	983	983	983	983	983

Notes: In columns 1-6, the dependent variable is the log of total acts of mismanagement, and in columns 7-12 the dependent variable is the log of total acts of corruption. In addition to the interaction terms presented in the table, each regression controls for the direct effect, the controls presented in 2, as well as state, lottery, service order fixed effects. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table A.3: Probability of Running for a Higher Office

Dependent variable:	Ran for higher office (1)
Some primary school	1.489* [0.694]
Primary School	2.247* [0.695]
High School	1.980* [0.711]
Some college	2.278* [0.683]
College	2.714* [0.707]
More than College	3.061* [0.688]
Male	-0.055 [0.100]
Vote Share in previous election	-1.205* [0.385]
Campaign Spending in last election	0.042* [0.008]
Occupation dummies	Y
N	938

Notes: The dependent variable equals one if the mayor ran again for a higher office and zero otherwise. The regression also controls for 1-digit occupation codes. Campaign spending is measured in R\$100,000s. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table A.4: Structural Estimates for Mayor Characteristics

	Rents Equation (1)	Reelection Equation (2)
Education	-0.0032 [0.0036]	0.0009 [0.0027]
White Collar	-0.0520 [0.0300]	0.0093 [0.0228]
Male	0.0005 [0.0477]	0.0246 [0.0316]

Notes: This table reports the maximum likelihood estimates on the mayor characteristics for the rents and reelection equations. Education is measured in years of schooling. Both equations also include controls for state, lottery, number of neighbors and number of service orders. Column 1 also controls for municipal characteristics (population, Gini coefficient, GDP per capita, share college educated, share urban).

B Appendix B

We solved for the equilibrium reelection probability as follows.

In the main text, we showed that the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left((1 - \beta)V(q_i) + e_i^{S^*} + \tilde{\varepsilon}_i \right)$$

where $h(X_i) \equiv X_i' \xi - \beta X_i' \alpha$ denotes how much voters value the mayor's characteristics when accounting for their effects on both rents and popularity.

Let $s_i^T \in \{0, 1\}$ denote whether the voter observes the rent signal in term T . Suppose $s_i^F = 1$. Then the voter's posterior belief about the mayor's type is $\tilde{\varepsilon}_i = \varepsilon_i + e_i^F - e_i^{F^*}$ by equations (1) and (2). The probability that the voter reelects an incumbent conditional on the mayor's type and his action e_i^F is given by

$$\mathbb{P}(R_i = 1 | s_i^F = 1, X_i, \varepsilon_i, e_i^F, q_i) = F_D \left(2\mu_D + h(X_i) - \beta[(1 - \beta)V(q_i) + e_i^{S^*} + \varepsilon_i + e_i^F - e_i^{F^*}] \right)$$

Now consider the case $s_i^F = 0$, where the voter does not observe the rent signal. In this case, the voter reelects the mayor with probability

$$\mathbb{P}(R_i = 1 | s_i^F = 0, X_i, \varepsilon_i, e_i^F, q_i) = F_D \left(2\mu_D + h(X_i) - \beta[(1 - \beta)V(q_i) + e_i^{S^*}] \right)$$

We then integrate over the probability that the voter receives the signal to obtain the ex-ante (before the popularity shock, the audit, and the rent signal draws are realized) probability that the voter chooses to reelect the mayor:

$$\mathbb{P}(R_i = 1 | X_i, \varepsilon_i, e_i^F) = F_D \left(2\mu_D + h(X_i) - \beta[(1 - \beta)V(q_i) + e_i^{S^*} + (\chi_0 + \chi_1 q_i)(\varepsilon_i + e_i^F - e_i^{F^*})] \right)$$

Hence the equilibrium reelection probability follows immediately by setting $e_i^F = e_i^{F^*}$:

$$p(X_i, \varepsilon_i, q_i) = F_D \left(2\mu_D + h(X_i) - \beta[(1 - \beta)V(q_i) + e_i^{S^*} + (\chi_0 + \chi_1 q_i)\varepsilon_i] \right)$$