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 in 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

[1] See 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.

[2] See 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 they can potentially compute the probability of an audit for any given lottery, they do not know the likelihood of future audits occurring. 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.[3]

Consistent with this interpretation, we find that past audits also affect the corruption levels of neighboring municipalities with local media, which are the places where the mayors are the 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 15 million reais 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).[4]

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

[3] Although we model learning over the audit probability, it could also be the case that agents learn about the costs associated with audits. In the Online Appendix C, we solve for and estimate such a model. Although the structure of the learning process differs between the two models, both produce very similar results.

[4] Outside 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.

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 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 aimed 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 later dissipated in the subsequent rounds of audits. An impor-

tant 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.[5] 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 reductions in corruption over time in the U.S. were due to a combination of increased political competition, an active media uncovering corruption scandals, and an independent judiciary that successfully prosecuted corrupt officials. Also

[5]Recent 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.

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 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 of 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). Corruption tends to be more prevalent in places that receive more federal transfers, or where the local media and the judiciary are absent (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.[6] 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.[7] 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.[8] After the inspections are completed, a detailed report describing all the irregularities found is submitted to the central CGU office in Brasília. 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.

[6] This eligibility criteria has changed slightly over time.

[7] Auditors do not have discretion in auditing other projects. If 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.

[8] Ferraz 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.

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, in the 17^{th} lottery that took place in August 2005, the CGU chose to audit the sectors of 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 and 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 audited, the municipality can only be audited again after several lotteries have elapsed.[9] 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 9^{th}, this number decreased to 2 municipalities, only to then increase back to 3 after the 22^{nd} lottery. The number then changed again to 4 starting in the 34^{th} 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,

[9]This rule has changed over time going from 3 to 12 lotteries.

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 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, less than 50 mayors were convicted of irregularities in 2004, but 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 arrested next.[10]

The 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.[11] 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 15^{th}, 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 Familia.[12]

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

[11] See http://tresfronteirasam.com.br/radio/noticias.php?noticia=1003

[12] See http://www.radiopajeu.com.br/portal/pente-fino-da-cgu-no-bolsa-familia-prefeitura-de-afogados-emite-

2.3 Data

We build measures of mismanagement and corruption from a database managed by the CGU. The dataset includes the coding of all irregularities found by the auditors for each inspection order. Although all audit reports are posted online, starting with the 20^{th} lottery in March 2006, the CGU began to code the information used for the reports. 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 classify it as: 1) an act of mismanagement (e.g. documents were not properly filled out, or improper storage of food supplies and medical equipment), 2) act of moderate corruption, 3) act of severe corruption.[13]

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 20^{th} 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-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 admin-

nota/. Retrieved December 12, 2016.

[13]These 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 CGU's distinction 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). See Zamboni and Litschig (2015) for a discussion of this point.

istrative 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 match 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 information available on the CGU homepage, as well as internet searches.[14] 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 municipality targeted, 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. We then create an indicator equal to one if a municipality was subject to a crackdown in a given year and whether the mayor was involved in the irregularities and/or arrested.

Data on the convictions of mayors for misconduct in public office was obtained from the Cadastro

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

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 with 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 are 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 are few differences in the characteristics of places audited for a first time versus those that had been audited previously. 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.[15] Out of 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

[15]This 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.

suggest that the lottery used by the CGU was effective.

3 Model

To disentangle the channels through which audits reduce corruption, we consider the following model of political accountability, which builds on the career concerns model (Holmström, 1999; Persson and Tabellini, 2002). In our 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 Model Without Learning

3.1.1 The Mayor

Mayors differ along a single continuous dimension, which we label ability, that is constant throughout their tenures in office. Mayors with higher levels of ability extract more rents than those with low ability. The ability of the mayor is a function of his observable characteristics X_i (gender, education, occupation) and an unobservable characteristic ε_i.[16] Thus, we have:

$$\text{Ability}_i = X_i'\alpha + \varepsilon_i \tag{1}$$

The mayors' observable characteristics are common knowledge to both mayors and voters, and are drawn i.i.d. before the first-term mayor selects his action from a distribution with mean zero. The unobservable characteristic ε_i is drawn from a normal distribution with mean zero and variance σ_ε^2. The ε_i draw is privately observed by the first-term mayor after he chooses his action.

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). Rents in term T for mayor i are given by the sum of the mayor's rent extraction effort e_i^T and his ability:

$$r_i^T = e_i^T + X_i'\alpha + \varepsilon_i \tag{2}$$

[16] Since the environment is stationary, we omit period subscripts in this section.

In each term, after the mayor chooses his extraction effort, 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. The mayor seeks to maximize the discounted sum of rents r, net of the costs of rent extraction c over his tenure. The mayor's per-period utility in term T is given by

$$u^T(e_i^T, X_i, \varepsilon_i, a_i^T) = e_i^T + X_i'\alpha + \varepsilon_i - c(e_i^T, a_i^T) \tag{3}$$

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 describe here 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. Thus the mayor's cost function can be written as

$$c(e_i^T, a_i^T) = b_0(\gamma_0 + \gamma_1 a_i)e_i^T + b_1(\gamma_0 + \gamma_1 a_i)(e_i^T)^2 \tag{4}$$

Assuming that $b_0, b_1, \gamma_0, \gamma_1 > 0$, this function is strictly increasing and strictly convex in e_i^T.

3.1.2 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.

3.1.3 Equilibrium

The timing of the game is as follows; (1) Given his observable characteristics, the first-term incumbent chooses his effort level; (2) his unobserved 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 solve for the perfect Bayesian 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:

$$e^{S*}(q_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i)}{2b_1(\gamma_0 + \gamma_1 q_i)} \tag{5}$$

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

$$V(q_i) = \int v^{F*}(X_i, \varepsilon_i, q_i, \delta_i) + \beta \left[p(X_i, \varepsilon_i, a_i^F, q_i) \left(v^{S*}(X_i, \varepsilon_i, q_i) + \beta V(q_i) \right) + \left(1 - p(X_i, \varepsilon_i, a_i^F, q_i)\right) V(q_i) \right] \quad \mathrm{d}\mathbf{F} \tag{6}$$

where β is the discount factor, p denotes the equilibrium probability of reelection, v^{F*} and v^{S*} denote equilibrium per-period voter utilities, and $\mathbf{F}$ denotes the joint distribution of $(X_i, \varepsilon_i, \delta_i, a_i^F)$.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. The voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left((1-\beta) V(q_i) + e_i^{S*} + \tilde{\varepsilon}_i \right) \tag{7}$$

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 is[17]

$$p\left(X_i, \varepsilon_i, a_i^F, q_i\right) = F_D \left(2\mu_D + h(X_i) - \beta \left[(1-\beta) V(q_i) + e_i^{S*} + (\chi_0 + \chi_1 a_i^F)\varepsilon_i \right] \right) \tag{8}$$

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

We next solve for the first-term mayor's maximization problem. His 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:

$$\max_{e_i^F, e_i^S} \int u^F(e_i^F, X_i, \varepsilon_i, a_i^F) + \beta \mathbb{P}(R_i = 1 | e_i^F, \varepsilon_i, X_i, a_i^F, q_i) u^S(e_i^S, X_i, \varepsilon_i, a_i^S) \quad \mathrm{d}\mathbf{G} \tag{9}$$

where $\mathbb{P}(R_i = 1 | e_i^F, \varepsilon_i, X_i, a_i^F, q_i)$ is the probability the mayor is reelected,[18] and $\mathbf{G}$ denotes the joint distribution function of $(a_i^F, a_i^S, \varepsilon_i)$. Assuming an interior solution, the mayor's equilibrium first-term action is

$$e^{F*}(q_i, X_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 q_i) - \beta^2(\chi_0 + \chi_1 q_i)\sigma_D U^{S*}(q_i, X_i)}{2b_1(\gamma_0 + \gamma_1 q_i)} \tag{10}$$

where $U^{S*}(q_i, X_i)$ denotes the equilibrium expected payoff for the mayor's second term.

[17] We derive this equation in Online Appendix B.1.

[18] See Online Appendix B.1 for the derivation.

Therefore, equilibrium first-term rents, $r_i^{F*} = e_i^{F*} + X_i'\alpha + \varepsilon_i$, 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 plays a role through observable characteristics X_i and the unobservable trait ε_i.

In contrast, equilibrium second-term rents, $r_i^{S*} = e_i^{S*} + X_i'\alpha + \varepsilon_i$, 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.[19]

3.2 The Model With Learning

In the model outlined above, whether a municipality has been audited in the past has no long-term consequences on corruption. Since there is a two-term limit, an audit only affects rents in the following term through its effect on selection. Otherwise, since the expected costs of an audit do not depend on the municipality's audit history, 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.[20] 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.[21]

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

[20] For 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.

[21] Moreover, recent empirical findings suggest that in fact agents place at least some weight on their experiences when forming beliefs (Kleven et al., 2011; Malmendier and Nagel, 2011). Our learning framework is similar to the one developed in Gallagher (2014). This study finds a significant increase in insurance take-up in communities following

3.2.1 Setup

We extend our model with the following framework of Bayesian learning to rationalize why the history of audits may affect the behavior of mayors and voters (henceforth, agents). For agents in municipality i, we assume that prior beliefs over the probability of an audit in a given term are distributed $Beta(\beta_{0i}, \beta_{1i})$.[22] The mean of the prior is $\hat{q}_i := \frac{\beta_{0i}}{\beta_{0i}+\beta_{1i}}$ and the strength of the prior is captured by the sum $\beta_{0i} + \beta_{1i}$. We assume that $\beta_{0i} + \beta_{1i} < \infty$, so that there is uncertainty in the prior and thus agents will take their own (and neighboring) experiences into account when forming beliefs.

Every term, an audit is drawn in each municipality from an i.i.d. Bernoulli distribution with probability q. After each term, the agents observe their own draw and the draws of their 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 during a term in municipality i will follow a binomial distribution with sample size $n_i := |N_i| + 1$ and number of successful draws $y_i := a_i + \sum_{j \in N_i} a_j$. After observing these draws, the agents update their beliefs about the audit probability using Bayes' rule.

Although we model learning over the audit probability, it could also be the case that agents learn about the costs associated with audits. In the model, this would be the case if agents were uncertain and learned about γ_1 and χ_1 instead of q. In Online Appendix C, we explicitly solve for such a model and estimate it with the same data. Although the structure of the learning process differs between the two models, in both cases, agents are learning about the expected costs of the audit program. Thus, perhaps unsurprisingly, we find similar results when estimating either model.

Another possibility is that audits affect objective costs rather than beliefs. However, if audits only affect costs, it is difficult to rationalize why audits cause spillover effects across municipalities 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 models are consistent with the narrative evidence we presented in Section 2.

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.

[22] Prior beliefs follow a Beta distribution for mathematical convenience as this distribution is the conjugate prior for the Binomial distribution.

3.2.2 Equilibrium

In this section, we outline the Perfect Bayesian equilibrium in pure strategies when incorporating learning into the model. For the technical details, see Online Appendix B.2.

The state of municipality i is given by the vector $\omega_i := (\beta_{0i}, \beta_{1i}, n_i)$, where β_{0i} and β_{1i} parametrize the prior of the agents in the municipality, and n_i denotes the number of audit draws observed in a term. In the following period, due to Bayesian updating, the state is given by $\omega_i' = (\beta_{0i}', \beta_{1i}', n_i')$, where $\beta_{0i}' = \beta_{0i} + y_i$, $\beta_{1i}' = \beta_{1i} - y_i + n_i$, and $n_i' = n_i$. We analogously define the state in the subsequent period by ω_i''.

Then, when solving the second-term mayor's maximization problem, we must take his beliefs about the audit probability into account. Similarly, for the first-term mayor, we now consider his beliefs about the audit probability in the current and next term. Thus, the equilibrium first-term and second-term effort levels, which are now a function of the state vector ω_i, are

$$e^{F*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i) - \beta^2 \sigma_D W(X_i, \omega_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{11}$$

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{12}$$

where $W(X_i, \omega_i) := (\chi_0 + \chi_1)\hat{q}_i U^{S*}(X_i, \omega_i, a_i^F = 1) + \chi_0(1 - \hat{q}_i)U^{S*}(X_i, \omega_i, a_i^F = 0)$, and U^{S*} denotes the mayor's equilibrium expected second-term payoff, conditional on his characteristics X_i, the state ω_i and the audit draw a_i^F. These effort levels are similar to the ones we derived for the model without learning, with the key difference being that the belief about the audit probability will discipline mayors in both terms.

The equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on whether an audit is drawn and the state ω_i is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[G(\omega_i) + (\chi_0 + \chi_1)\varepsilon_i\right]\right) \tag{13}$$

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 0\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[G(\omega_i) + \chi_0\varepsilon_i\right]\right) \tag{14}$$

where $G(\omega_i) := V(\omega_i) + e_i^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i')$, and $V(\omega_i)$ denotes the value function of the voter

when a random first-term mayor is selected. Notably, this function will depend on the state ω_i:

$$V(\omega_i) = \int v^{F*}(X_i, \varepsilon_i, \delta_i, \omega_i) + \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i') \right] V(\omega_i') \right\} d\mathbf{F} \tag{15}$$

where δ_i is the popularity shock, v^{T*} denotes equilibrium per-period voter utility, and $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i')$.

In sum, 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, which are given in this case by $b_0(\gamma_0 + \gamma_1 \hat{q}_i)e_i + b_1(\gamma_0 + \gamma_1 \hat{q}_i)e_i^2$. 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 $\chi_0 + \chi_1 \hat{q}_i$. 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 this framework, audits will not only affect future corruption through a selection effect, but also a disciplining effect.

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, 14 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 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 11^{th} lottery. As expected, the number of municipalities that have been audited more than once increases over time. For instance, in the 30^{th} lottery, 19 out of 60 municipalities had been audited in the past. Given this structure of the data, we estimate the following

model for municipality m in state s, audited at date t.

$$\text{Corruption}_{mst} = \alpha + \beta \text{Past Audit}_{mst} + Z'_{ms}\gamma + f(nos)_{mst} + \nu_s + \mu_t + \varepsilon_{mst} \tag{16}$$

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, ν_s, to capture the fact that the randomization is stratified by state.[23] We also include lottery fixed effects, μ_t, which are important for two reasons. First, municipalities are more likely to become treated over time, but within a given lottery the probability a municipality had been audited in the past is the same for municipalities within a state. Second, starting in the 20^{th} 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:

$$\text{Legal}_{mt} = \alpha + \beta \text{Audited}_{mt} + \nu_m + \mu_t + \varepsilon_{mt} \tag{17}$$

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.

[23] Given 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.

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 16. 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 discussed above, there is an important distinction to be made between corruption and mismanagement. We do this 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.[24]

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

[24] We also estimate the effects of the audits on the totals acts of corruption and mismanagement, using a negative binomial regression model. We present the marginal effects in Appendix Table A.2. Overall, the findings are quite similar.

reduction in corruption. Municipalities that had experienced a previous audit committed 7.9 percent fewer acts of corruption compared to those that had not. Visually, the effects of the treatment can be seen in Figure 6. The figure plots the residuals from a regression of log corruption on state, lottery, and service order fixed effects. The figures compares the distribution of these residuals between treatment and control municipalities. From this comparison, we see that the audits reduced corruption at the upper tail of the distribution. For treated municipalities, the 99^{th} percentile of the corruption distribution corresponds to approximately the 91^{st} percentile of the corruption distribution in control municipalities. The left tails of the corruption distributions are comparable between treatment and control municipalities.

To interpret this magnitude, consider that the average municipality in our sample receives 15 million reais 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.[25]

Spillover Effects

The estimates presented in Table 2 are likely to represent a lower bound on the effects of 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 16, 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. In columns 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 un-

[25] We also test whether the effects of the audits vary according local characteristics, but find little evidence of heterogeneous effects (see Table A.3).

covered in municipalities that have been audited in the past. In columns 2-3, we test for whether the spillover effects are more pronounced in places with local media. For both AM radio (column 2) and television (column 3), we find evidence of significant spillover effects. 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 the 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.[26]

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

Effects of the Audits on Legal Actions

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 17 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

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

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, acts of mismanagement are not associated with any subsequent legal actions. Overall these findings suggest that the legal costs of engaging in corruption are substantial.

5.2 Mechanisms

Thus far, the evidence suggests that audits reduce future corruption and increase the likelihood of a legal action. In Section 3, we discussed several reasons why the audits may reduce corruption. One reason is political selection. 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 electoral discipline. 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 changed the type of mayors who run for office.

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.[27] 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 electoral or legal disciplining effects.

To further distinguish between electoral and legal disciplining, we estimate two additional specifications. We first test whether the effects of the audits vary by whether the mayor was in his first or second term. If second-term mayors, who are term limited, do not have further re-election incentives, then they should only respond to legal costs, whereas first-term mayors will respond to both types of costs. The second specification tries to relax the assumption that second-term mayors do not have further career concerns, given that they may be inclined to run for 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 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:

$$\text{Ran higher}_i = \beta_0 + \beta_1 \text{Male}_i + \beta_2 \text{campaign spending}_i + \beta_3 \text{vote share}_i + \eta_e + \theta_o + \varepsilon_i \qquad (18)$$

where Ran higher_i is an indicator for whether the mayor ran for a higher office, namely elections for state and national legislature, governor, or president. Male_i indicates whether the mayor is male, $\text{campaign spending}_i$ measures the amount of money the mayor spent is his election, vote share_i denotes the share of votes he received in his election, η_e represent a set education intercepts, θ_o represents a set of occupation dummies at the 1-digit level, and ε_i denotes the error term. The results from estimating this equation are presented in Table A.4. 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 non-audited places to municipalities in which the mayor experienced multiple audits within the same term. We

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

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. To test for the existence of the political selection channel, we compare mayors who 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 and reward non-corrupt ones, then the reelected mayors who had been audited prior to the election should be, on average, more positively selected than the reelected mayors who had not been audited prior to the election.

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. This difference in corruption levels, however, 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 concern with this interpretation is the lack of statistical precision for some of our estimates. If, for example, we used the lower bound estimate of the 95 percent confidence interval in column 3, the differential effects of the audits would imply an additional 9.8 percent reduction in corruption levels for mayors with an one standard deviation increased propensity to run for higher office. Similarly, although the results in columns 1 and 4 imply a political selection of only 2.2 percent, given our standard errors, the political selection effects could also be as large as 15.2 percent.

Another 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 columns 5 and 6, we present two specifications to test for this possibility: 1) we allow for the effects of the audits to vary flexibly by the number of terms since the last audit; 2) we control for the log of the number of years since the last audit (re-centered at the sample mean). In both specifications, we find no evidence of a differential

effect based on how much time had elapsed since the last audit.

Political Entry. A fourth channel through which audits may reduce corruption is political entry. This would be the case if the audits induced better candidates to enter 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 effects of the audits across the various subsamples. Second, large standard errors cloud some of our comparisons. 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 will allow 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

An alternative interpretation of our reduced-form findings is that audits simply teach politicians how to better hide corruption. In this case, corruption has not necessarily been reduced, but perhaps displaced. Although we cannot rule out this interpretation completely, there are at least three reasons why we do not think displacement is the primary mechanism. 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; the three most common forms of corruption.[28] In column 1-3, we find no evidence that the audits induced mayors to shift away from or into these forms of corruption. In columns 4-6, we 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 from 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. In particular, we might expect mayors who experienced an

[28] For 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.

audit to shift expenditures away from sectors that are more prone to corruption. For each audit, we compute the share of public expenditures spent in each sector during the given year. We also aggregate the share of public expenditures spent in education, health, and welfare, which are the sectors in which almost 78% of the corruption occurs during a first time audit.

The results of public expenditures are presented in Table 8. In column 1, we do not find any evidence that the audits led mayors to shift their expenditures away from high corruption sectors towards sectors that are less corruption prone. In the remaining columns, we disaggregate expenditures further, and again do not find any 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.[29]

6 Structural Estimation

We structurally estimate the model to complement our reduced-form analysis in two ways. First, an empirical challenge is that a decrease in rents in 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 corruption 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 3.2. Thus, in addition to data on corruption and elections, the structural estimation exploits data on neighboring audits and media presence.[30] Moreover, this approach allows us to recover the parameter estimates needed to analyze policy counterfactuals.

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

[30] Despite 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.

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 $\mathbf{Y}_i := (T_i, r_i, R_i, a_i^F, X_i, Z_i, \omega_i)$, 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, a_i^F denotes whether the municipality was audited in the *previous* term if the current mayor is in his second term, X_i denotes the vector of mayor characteristics, and Z_i the vector of municipal characteristics, and ω_i is the vector that determines the beliefs over the audit probability.

To compute ω_i for each observation, we use data on the past history of audits of each municipality indexing time with t. We set $t = 1$ to be the 2001-2004 mayoral term, the first which was subjected to the audit program, and let $t = 2$ denote the 2005-2008 term and $t = 3$ denote the 2009-2012 term. In the first term when the program was implemented, i.e. when $t = 1$, we assume that prior beliefs over the audit probability follow the distribution $Beta(\beta_0, \beta_1)$. We set the mean of the prior, $\frac{\beta_0}{\beta_0+\beta_1}$, equal to the objective probability of an audit in our sample. This pins down one of the two free parameters which determine the prior. 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 affected by 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. Finally, for the subsequent two time periods, we compute the prior using Bayes' rule. Hence we obtain $\omega_i = (\beta_{0i}, \beta_{1i}, n_i)$ for each observation.

The vector X_i includes mayor characteristics (gender, education and occupation), number of service orders, number of neighbors and state and lottery intercepts. We fix the popularity density shock to 2 and the annual discount factor to 10 percent, in order to identify χ_0 and χ_1. We set 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 of rents captured. 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 $\theta := (\gamma_0, \gamma_1, \chi_0, \chi_1, \mu_\delta, \sigma_\varepsilon, \alpha', \eta', \lambda')$ using Maximum Likelihood. For a municipality i where the mayor is in his first term, the likelihood function is given

by

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

where $\varepsilon_i^F = r_i - e_i^{F*}(X_i,\omega_i) - X_i'\alpha - Z_i'\lambda$ is the mayor's unobserved ability shock conditional on equilibrium play. We include the municipal characteristics Z_i additively and linearly in this term in order to control for heterogeneity across municipalities. Here, f_ε denotes the density of the shock, and p denotes the equilibrium probability of reelection, where we set a_i equal to 1 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

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

where $\varepsilon_i^S = r_i - e_i^{S*}(\omega_i) - X_i'\alpha - Z_i'\lambda$ 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, the probability of reelection is conditioned on whether an audit was realized in the previous term (a_i^F).

Let $\mathbf{Y} = (\mathbf{Y}_1',...,\mathbf{Y}_n')$ denote the data. We estimate the vector of parameters θ which maximizes the likelihood function:

$$\mathscr{L}(\theta|\mathbf{Y}) = \prod_i L(\theta|\mathbf{Y}_i)$$

We estimate the asymptotic covariance matrix of the maximum likelihood estimator by evaluating the Hessian of the likelihood function, and we use the Delta method when needed to evaluate standard errors.

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 $\mathbf{Y}$, $\mathscr{L}(\theta'|\mathbf{Y}) \neq \mathscr{L}(\theta|\mathbf{Y})$.

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 can back out the mean perceived probability $\hat{q}_i$ from the data ω_i (see our discussion on Bayesian learning and equation (12)). For instance, $\gamma_1 = 0$ would imply that second-term rents are uncorrelated with the perceived audit probability. Second, χ_0 is identified since we jointly observe rents and reelection outcomes as well as the difference in rents between first and second-term mayors (see equations (11)-(14)). This 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 $\hat{q}_i$. 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 empirical distribution of rents and μ_D follows from the distribution of reelection rates.

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 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 perceived threat of non-electoral costs, significantly affects corruption.

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.5. 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 the mayor's popularity is positively related to education, white collar occupation and male gender, but again none of these coefficients are statistically different from zero. Since we also did 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 return to this point 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 dummies 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 7.

To assess the fit of the Bayesian learning model, we regress rents on mayor and municipal characteristics, number of service orders, number of neighbors, lottery and state fixed effects. We repeat the regression with the mean belief about the audit probability as the dependent variable. Figure 8 presents the residuals of these regressions in a binned scatter plot. Recall that the mean of the pos-

[31] The 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.

terior about the audit risk increases when agents within a municipality observe a larger proportion of audits than would be predicted by their prior. 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 mean 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 mean 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 $\gamma_0 + \gamma_1 \hat{q}_i$, 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. We find that audits reduce rents through electoral discipline by 5.3 percent.

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, 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.

This decomposition excludes the possibility that audits reduce the rents of second-term mayors through electoral discipline. We consider two extensions to the model which incorporate this channel. First, we estimate the model including in the equilibrium effort equations a term for the propensity to run for a higher office and its interaction with the mean of the prior for the audit probability. We do not find significant effects on the coefficients for these terms (see Online Appendix Table A.6, column 1). Second, in Online Appendix B.3, we consider an extension to the model where the second-term mayor values the voter's belief about his type when exiting office. Again, we do not find evidence that audits reduce the corruption of second-term mayors through electoral discipline (see Online Appendix Table A.6, column 2).

Policy counterfactuals. We parameterize the model with our structural estimates and conduct a number of policy simulations. The results are presented in Table 12. We begin by simulating changes in the audit probability. Since mayors and voters are assumed to have a rational mean prior, increasing the audit probability amounts to increasing the mean of the prior distribution by the same amount. This increases the perceived audit probability 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 corruption 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 $b_0 + b_1 e_i^T$ 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, substantial 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 expected costs of corruption, 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 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 those that had not. 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 diffusive 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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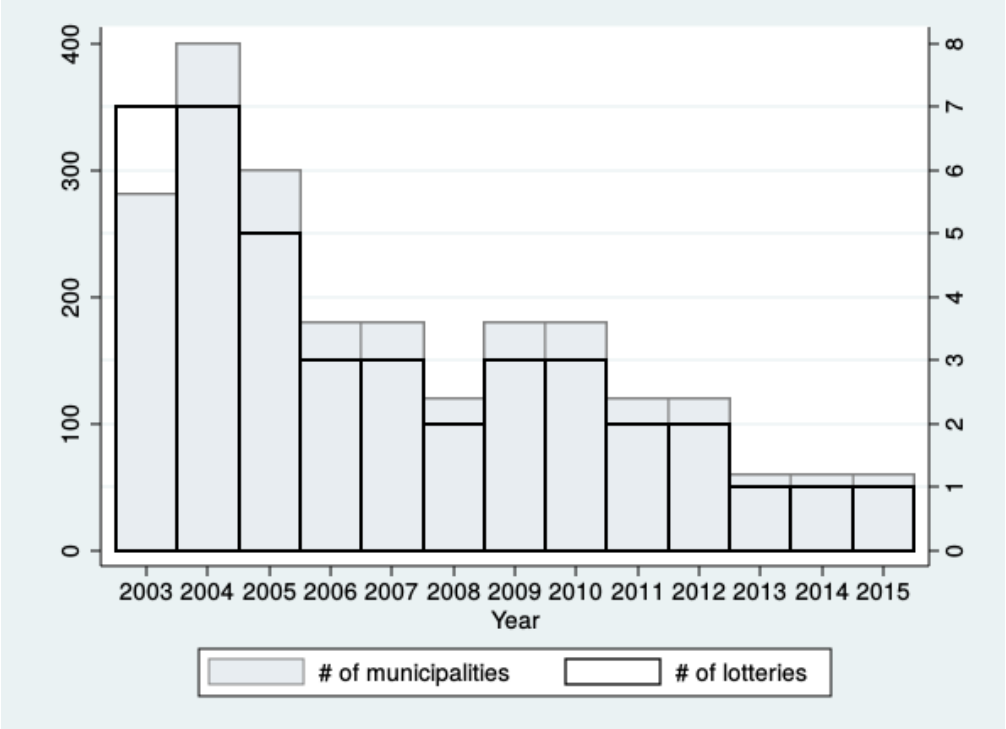
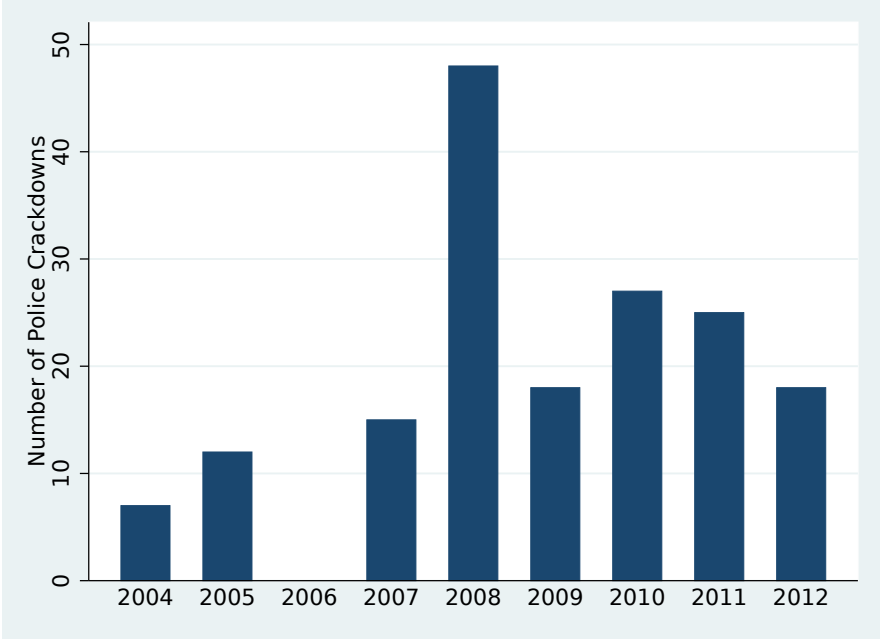
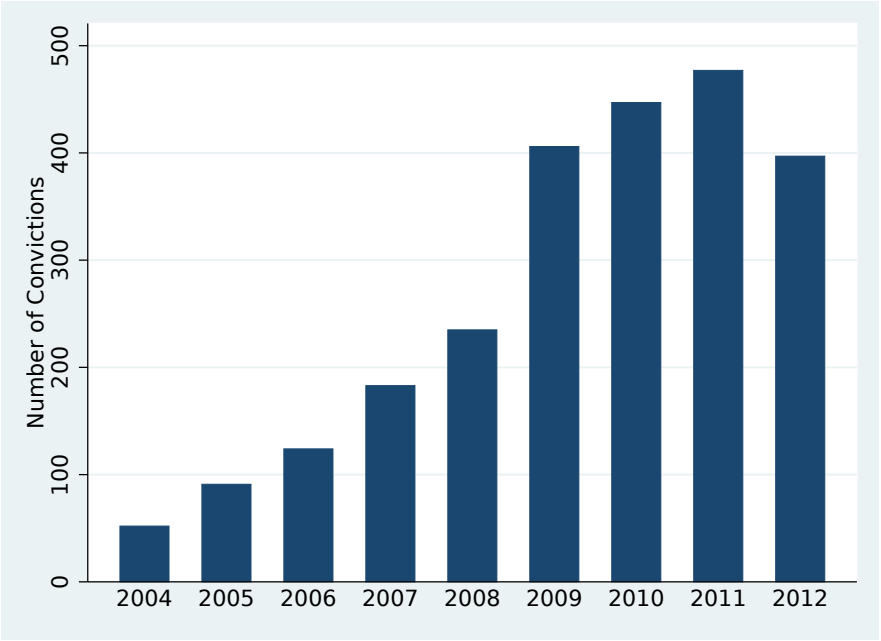


Figure 1: Number of Lotteries and Municipalities Audited Per Year

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



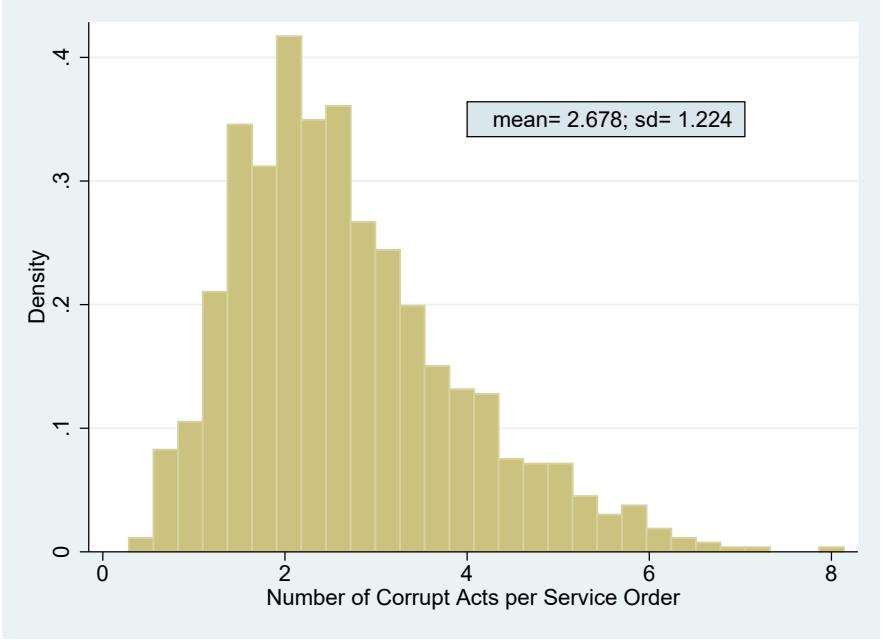
Panel A: Police Crackdowns



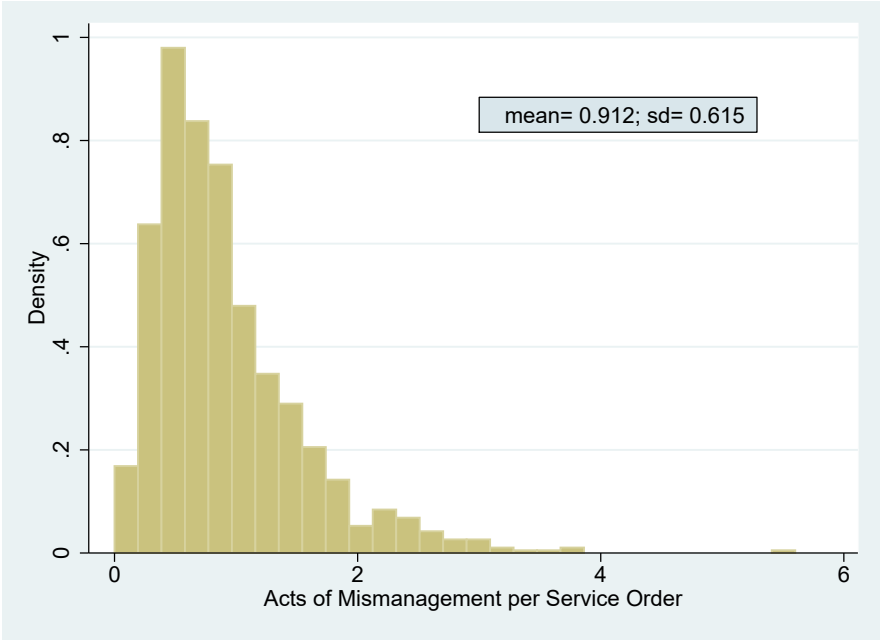
Panel B: Convictions

Figure 2: Number of Legal Actions over Time

Notes: This figures plots the number of police crackdowns and convictions 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.

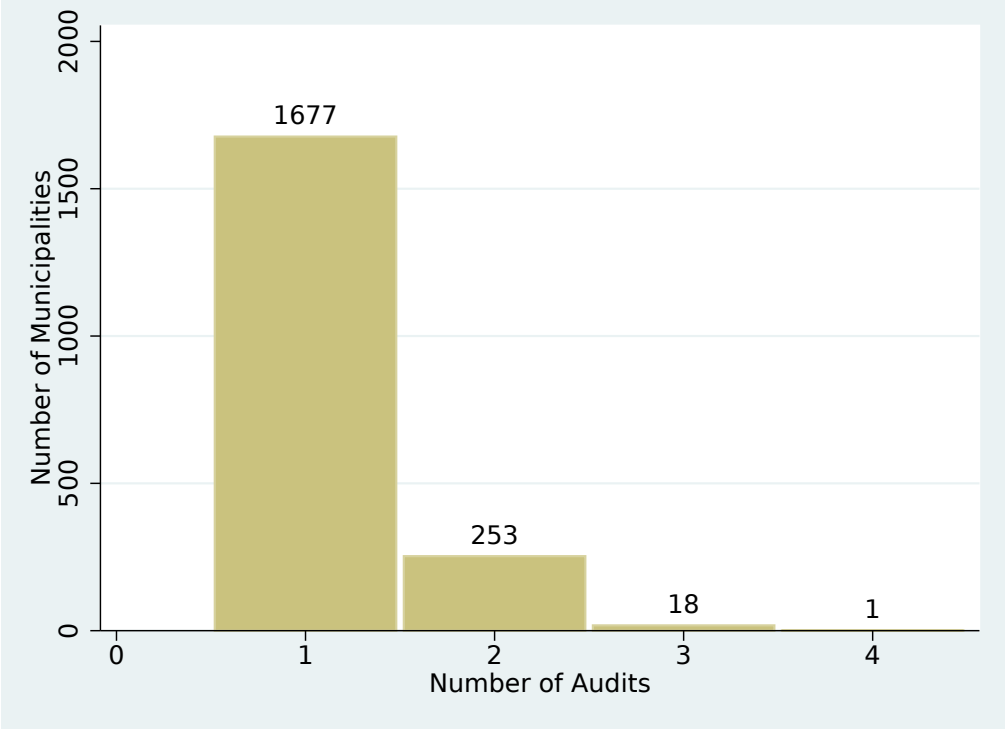


Figure 4: Distribution of Times a Municipality has been Audited

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

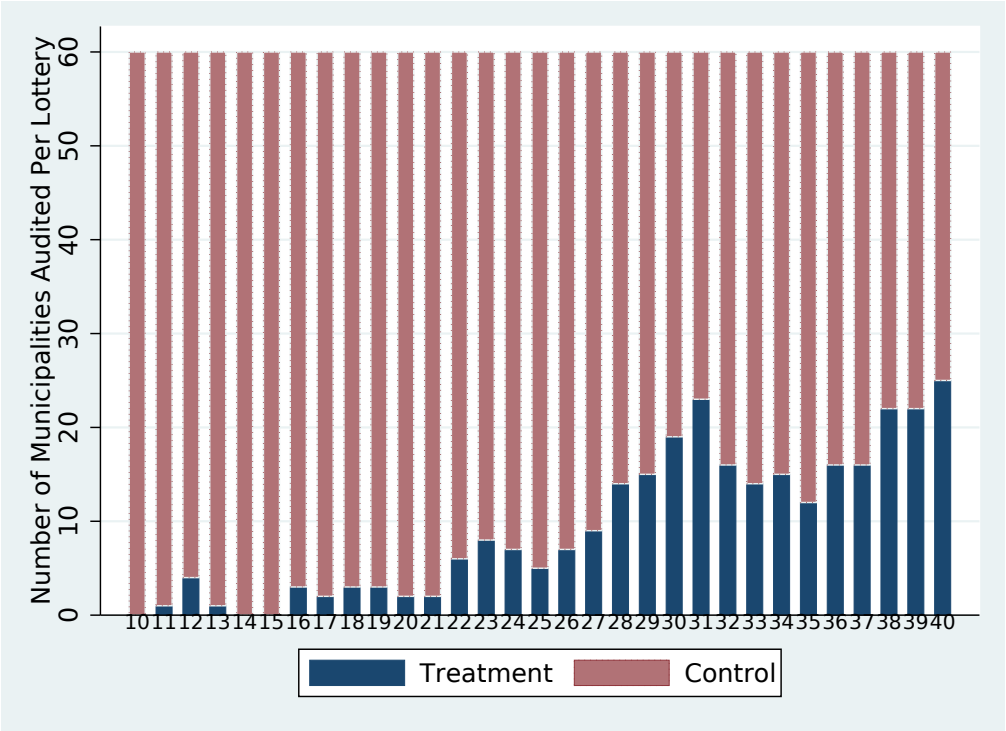


Figure 5: Distribution of Times a Municipality has been Audited

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.

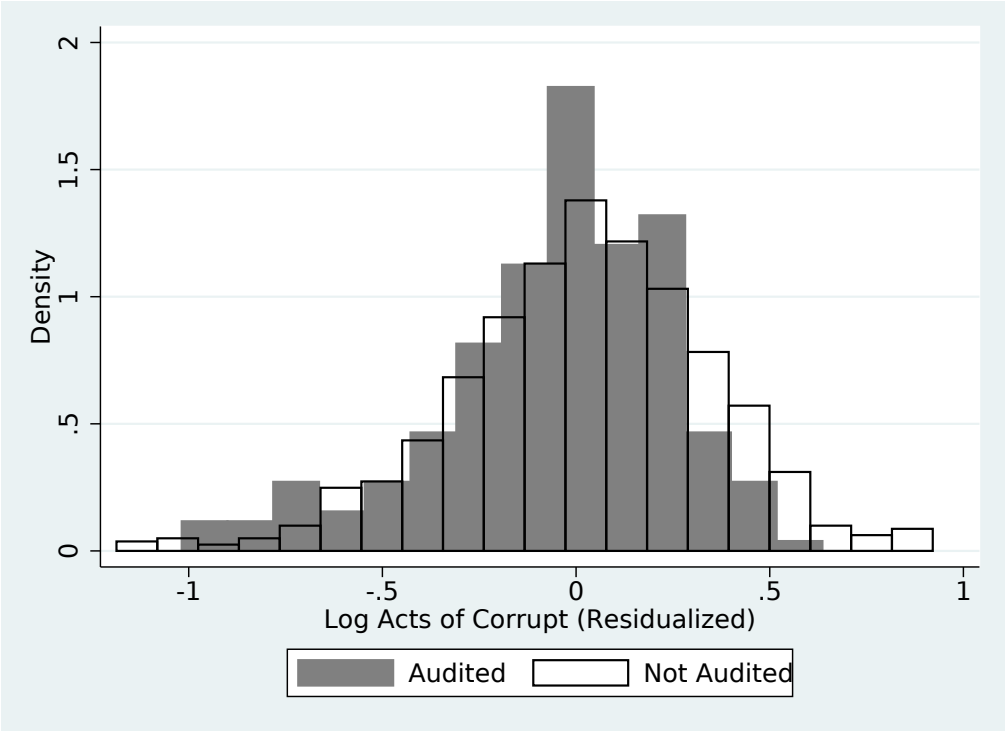


Figure 6: Effects of the Audits on Corruption

Notes: This figure compares the distribution of corruption between treatment and control municipalities. Specifically, it plots the residuals from two separate OLS regressions (one for treatment municipalities, the other for control municipalities) of log corruption on state, lottery, and service order fixed effects.

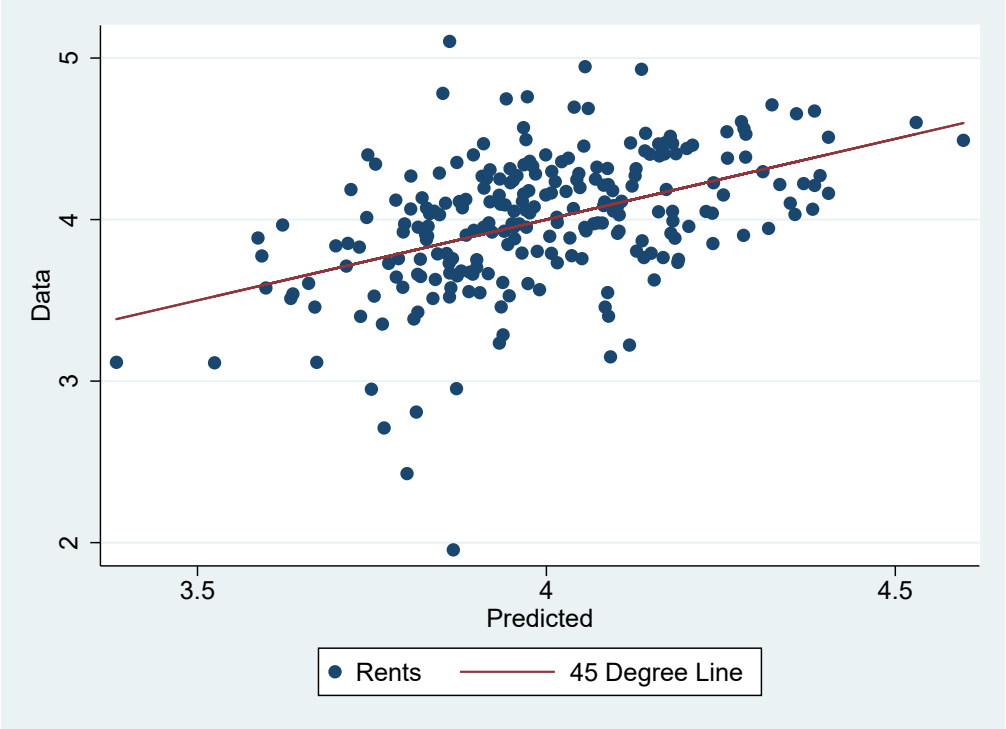


Figure 7: Out-of-Sample Fit for Rents

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.

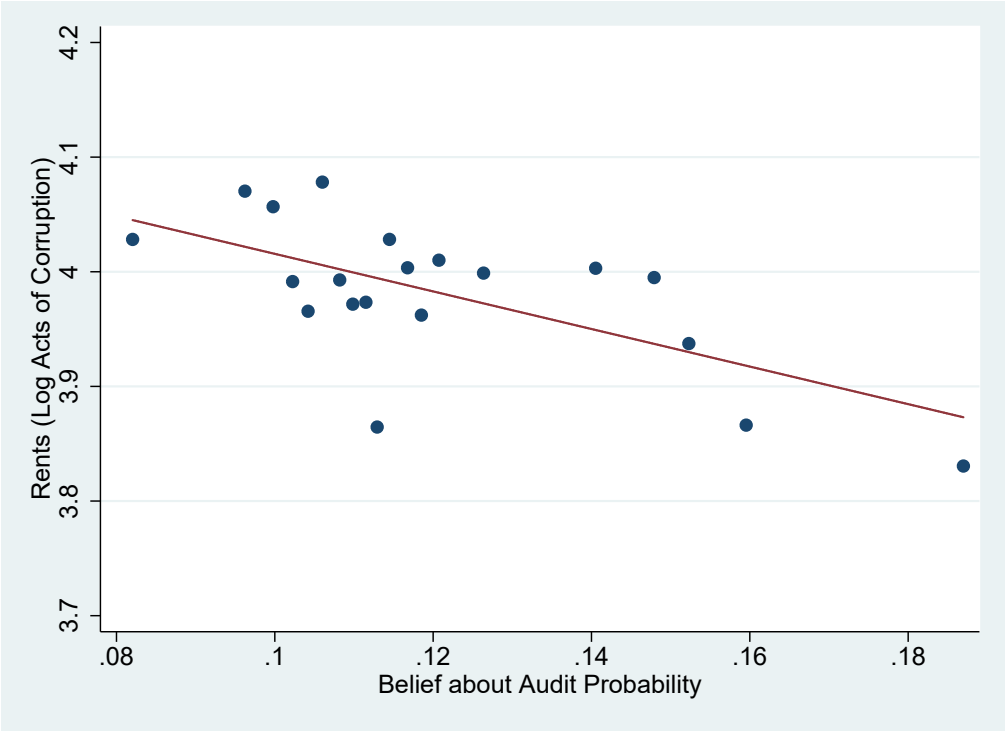


Figure 8: Rents and the Belief over the Audit Probability

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		
	Mean (1)	Std Dev. (2)	Mean (3)	Std Dev. (4)	Difference (5)
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 colllege 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	-0.045+	-0.058*	0.010	0.001	-0.023	-0.059*	-0.070*	-0.079*
	[0.021]	[0.022]	[0.021]	[0.052]	[0.048]	[0.041]	[0.024]	[0.025]	[0.027]
	(0.10)	(0.03)	(0.01)	(0.61)	(0.53)	(0.31)	(0.05)	(0.02)	(0.01)
Population (log)		0.057*	0.064*		0.047*	0.037+		0.053*	0.064*
		[0.011]	[0.012]		[0.015]	[0.021]		[0.015]	[0.018]
Income inequality (Gini)		0.337*	0.361*		0.137	0.177		0.449*	0.459*
		[0.111]	[0.112]		[0.295]	[0.249]		[0.158]	[0.177]
Income per capita (log)		-0.085	-0.102*		0.111	0.103		-0.158*	-0.176*
		[0.056]	[0.045]		[0.086]	[0.087]		[0.064]	[0.059]
Illiteracy		0.003	0.003*		0.001	0.000		0.004*	0.005*
		[0.002]	[0.001]		[0.003]	[0.002]		[0.002]	[0.002]
Share of urban population		0.123+	0.118*		-0.056	-0.068		0.190*	0.182*
		[0.067]	[0.045]		[0.091]	[0.136]		[0.086]	[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)	(7)	(8)	(9)
Audited	0.00139	-0.0000887	0.00443+	0.000195	0.00562*	0.000241			
	[0.001]	[0.001]	[0.002]	[0.003]	[0.003]	[0.003]			
Audit × Judiciary District		0.00325+		0.00933*		0.0119*			
		[0.002]		[0.004]		[0.005]			
Corruption (logs)							0.0369+	0.0601*	0.0882*
							[0.021]	[0.029]	[0.035]
Mismanagement (logs)							-0.0116	-0.00647	-0.0146
							[0.016]	[0.02]	[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 clustered at the municipality level 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	Full Sample	Open Seat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Audited in the past	-0.127*	-0.113	-0.133+	-0.149*		-0.079**	-0.122
	[0.061]	[0.070]	[0.069]	[0.060]		[0.027]	[0.076]
	(0.063)	(0.127)	(0.057)	(0.02)		(0.01)	(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)		
Number of years since last audit (logs)						-0.011	
						[0.055]	
						(0.32)	
R^2	0.47	0.47	0.47	0.50	0.48	0.48	0.53
N	821	821	821	596	983	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

				Characteristics of the Candidate Pool				Mayor Characteristics		
	Win Margin (1)	Number of Candidates (2)	Number of Parties (3)	Elementary School (4)	High School (5)	College (6)	Campaign Spending (7)	White Collar (8)	Male (9)	College (10)
Audited in the past	0.010	-0.002	0.000	-0.024	0.026	0.000	-0.018	-0.009	-0.027	0.030
	[0.020]	[0.029]	[0.028]	[0.023]	[0.028]	[0.028]	[0.068]	[0.043]	[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			
	Embezzlement (1)	Procurement (2)	Over-invoicing (3)	Embezzlement (4)	Procurement (5)	Over-invoicing (6)	Acts of Corruption (7)
Audited in the past	0.031	0.011	-0.026	-0.132	0.117	-0.050	
	[0.084]	[0.060]	[0.029]	[0.128]	[0.103]	[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

	High Corruption (1)	Education (2)	Health (3)	Administration (4)	Housing (5)	Welfare (6)	Transportation (7)	Other (8)
Audited in the past	-0.006	-0.006	-0.006	0.009	0.005	0.000	-0.004	0.001
	[0.008]	[0.006]	[0.005]	[0.008]	[0.005]	[0.003]	[0.003]	[0.004]
R^2	0.537	0.598	0.235	0.204	0.290	0.275	0.502	0.421
N	773	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	-0.053	0.018
	[0.050]	[0.077]	[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 analagous 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]

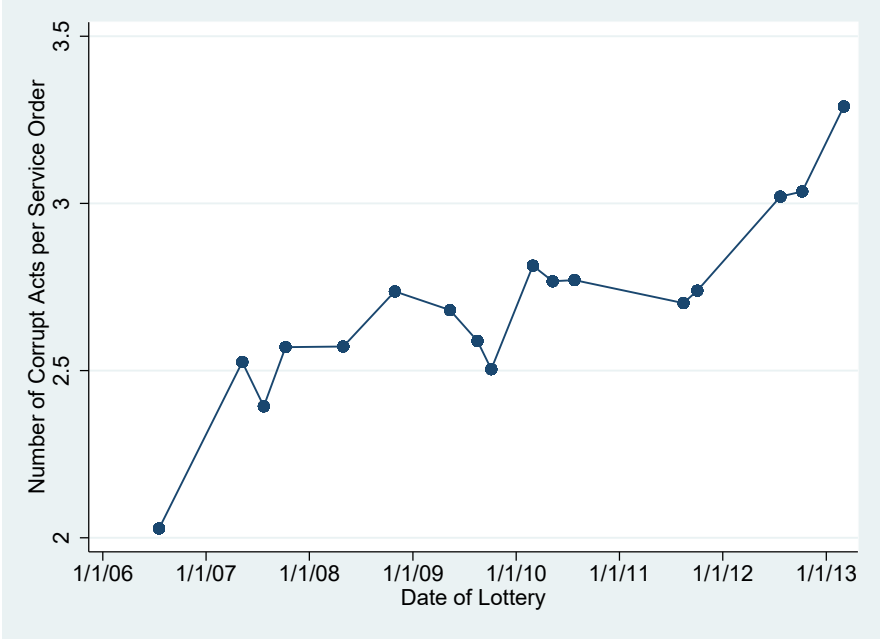
Notes: This table reports the difference in average rents between those predicted by the maximum likelihood estimates and those predicted by varying counterfactuals. Each row reports the difference for a separate counterfactual with the final row denoting the sum of the first three rows. Each counterfactual 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

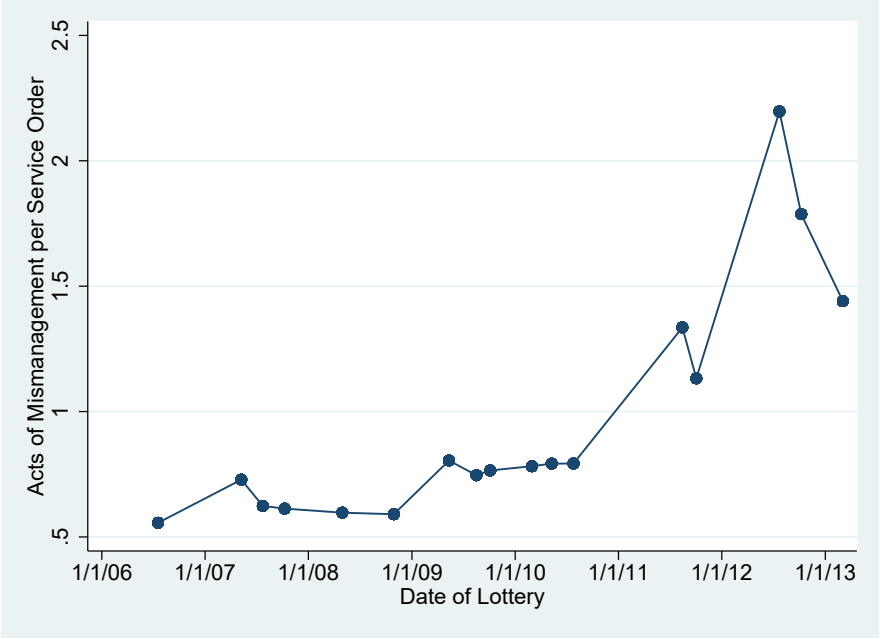
	Average Decrease in Rents (%)	
	First-term	Second-term
	(1)	(2)
Audit probability		
10 percentage point increase	14.6	9.3
20 percentage point increase	28.5	18.3
Legal cost (b_1)		
10 percentage point increase	9.8	9.7
20 percentage point increase	19.1	18.9
Local radio access to neighbors		
All municipalities 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 Addtional Figures and Tables



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: Effects of the Audits on Corruption and Mismanagement - Negative Binomial

Dependent variable:	Number of Irregularities			Acts of Mismanagement			Acts of Corruption		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audited in the past	-0.031	-0.043*	-0.057*	0.015	0.006	-0.019	-0.060*	-0.071*	-0.080*
	[0.020]	[0.019]	[0.019]	[0.033]	[0.033]	[0.033]	[0.026]	[0.025]	[0.024]
Controls	N	Y	Y	N	Y	Y	N	Y	Y
f(Service Orders)	log	log	non-parametric	log	log	non-parametric	log	log	non-parametric
R2	0.41	0.45	0.54	0.36	0.37	0.43	0.30	0.34	0.48
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, using a negative binomial regression model. The dependent variable in columns 1-3 is the total number of irregularities discovered in the audit. In columns 4-6, the dependent variable is the total acts of mismanagement, and in columns 7-9 the dependent variable is the total acts of corruption. In addition to municipal controls, 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. Robust standard errors are reported in brackets, + p<0.10, * p<0.05.

Table A.3: 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	-0.022	-0.024	-0.022	-0.029	-0.030	-0.079*	-0.080*	-0.086*	-0.081*	-0.076*	-0.079*
	[0.041]	[0.041]	[0.040]	[0.041]	[0.042]	[0.042]	[0.027]	[0.028]	[0.030]	[0.029]	[0.027]	[0.028]
Treatment Interacted with												
Radio	-0.036					-0.036	-0.058					-0.009
	[0.114]					[0.114]	[0.080]					[0.063]
% pop with a college degree		0.001				0.001		-0.017				-0.020
		[0.014]				[0.021]		[0.014]				[0.016]
Income per capita [log]			-0.009			-0.054			-0.063			0.024
			[0.064]			[0.104]			[0.053]			[0.072]
Share of urban population				0.093		0.260				-0.166		-0.109
				[0.203]		[0.272]				[0.121]		[0.125]
Judiciary District					0.016	-0.012					-0.023	0.006
					[0.084]	[0.079]					[0.047]	[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.4: 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	983

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.5: Structural Estimates for Mayor Characteristics

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

Table A.6: Estimates for Extensions Including Electoral Incentives for Term-Limited Mayors

	Parameter Estimates (1)	(2)
Panel A: Rents equation parameters		
Propensity to run for higher office	0.1079	
	[0.4264]	
Propensity to run for higher office × Audit probability prior	-0.1702	
	[3.1160]	
Education	-0.0043	-0.0040
	[0.0039]	[0.0036]
White Collar	-0.0471	-0.0486
	[0.0307]	[0.0300]
Male	0.0025	0.016
	[0.00476]	[0.00477]
Panel B: Other parameters		
Marginal utility from reputation (W)		-0.2113
		[0.1796]
Probability of legal action		
constant (γ_0)	0.0245	0.0245
	[0.0003]	[0.0003]
audit (γ_1)	0.0053	0.0055
	[0.0026]	[0.0025]
Probability of voter observing rents		
constant (χ_0)	0.0144	0.0147
	[0.0077]	[0.0077]
audit (χ_1)	0.0853	0.0851
	[0.0501]	[0.0498]
Standard deviation of ability shock (σ_ε)	0.3364	0.3364
	[0.0076]	[0.0075]
Mean of popularity shock (μ_D)	0.0028	0.0029
	[0.0114]	[0.0113]
Log likelihood	-681.67	-681.28
Observations	839	839

Notes: This table reports the maximum likelihood estimates for two exensions to the baseline structural model which account for potential electoral incentives for term-limited mayors. In the first column, the model includes in the equilibrium effort equations a term for the propensity to run for a higher office and its interaction with the mean of the belief about the audit probability. In the second column, we estimate the model presented in Online Appendix B.3, which includes electoral incentives for term-limited mayors through the parameter W.

Table A.7: Comparison of Learning Models

	Audit Cost Model (1)	Audit Probability Model (2)
Panel A: Parameter estimates		
Probability of legal action		
constant (γ_0)	0.0251	0.0245
	[0.0001]	[0.0003]
audit (γ_1)	0.0596	0.0053
	[0.0349]	[0.0025]
Probability of voter observing rents		
constant (χ_0)	0.0034	0.0147
	[0.0037]	[0.0076]
audit (χ_1)	0.0890	0.0877
	[0.0511]	[0.0496]
Standard deviation of ability shock (σ_ε)	0.3231	0.3366
	[0.0077]	[0.0075]
Mean of popularity shock (μ_D)	-0.0014	0.0028
	[0.0116]	[0.0113]
Panel B: Effects of audits on equilibrium rents, by channel (%)		
Legal discipline	82.1	71.9
	[15.0]	[18.5]
Electoral discipline	17.7	27.8
	[12.8]	[18.3]
Selection	0.3	0.4
	[5.0]	[0.2]
Log likelihood	-679.31	-682.01
Observations	839	839

Notes: This table reports the maximum likelihood estimates for the model with learning about audit costs in column 1 and the model with learning about the audit probability in column 2. Panel A reports parameter estimates of interest. Panel B reports the percent decrease in rents due to each of the three respective channels.

B Additional Material

B.1 Derivation of Equations in Section 3.1

We solved for the equilibrium reelection probability, equation (8), 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

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

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

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

We then integrate over the probability that the voter receives the signal to obtain the ex-ante probability that the voter chooses to reelect the mayor:

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

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

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

B.2 Details of Equilibrium with Learning

The timing of the game is as follows; (1) Given the state of the world ω_i, and his characteristics X_i, 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 and state ω_i', 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 in state ω_i''.

We solve for the perfect Bayesian equilibrium in pure strategies. A strategy for the mayor is a sequence of actions $e_i^T(\omega_i, X_i)$ for each term T conditional on the state ω_i and his observable characteristics X_i. A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, \omega_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i, the state, 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. The first-order condition yields the second-term mayor's equilibrium strategy as a function of his belief $\hat{q}_i$:

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i)}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)} \tag{19}$$

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

$$\begin{aligned} V(\omega_i) = \int & v^{F*}(X_i, \varepsilon_i, \delta_i, \omega_i) \\ & + \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i') \right] V(\omega_i') \right\} d\mathbf{F} \end{aligned}$$

where δ_i is the popularity shock, $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i')$, $p(X_i, \varepsilon_i, a_i^F, \omega_i')$ denotes the equilibrium probability of reelection conditional on the mayor's type and the audit draw, and v^{F*} and v^{S*} denote equilibrium per-period voter utilities. We note here

that the draws y_i and y_i' will determine the future states ω_i' and ω_i''. Furthermore, the probability of reelection will depend on the state ω_i' rather than ω_i because the voter has observed the audit draw and updated his belief about the audit probability when the election occurs.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. Conditional on the state ω_i, the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left(V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + \tilde{\varepsilon}_i \right)$$

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. The term $\mathbb{E}_{y_i|\omega_i} V(\omega_i')$ denotes the expected value for V in the following term, which depends on what the state will be (ω_i').

Following the steps analogous to those described in Online Appendix B.1, the equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on the state ω_i and an audit being drawn is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + (\chi_0 + \chi_1)\varepsilon_i\right]\right)$$

and conditional on no audit being drawn is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 0\right) = F_D\left(2\mu_D + h(X_i) - \beta\left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{y_i|\omega_i} V(\omega_i') + \chi_0 \varepsilon_i\right]\right)$$

Finally, we solve the maximization problem of the first term mayor. This problem differs from the model without learning because the mayor is not only uncertain about the audit probability in the current term, but is also uncertain (and will update his belief) about the audit probability in the following term. We solve his maximization problem by taking the first-order condition, which yields the equilibrium action as a function of the state ω_i:

$$e^{F*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1 \hat{q}_i) - \beta^2 \sigma_D \left[(\chi_0 + \chi_1)\hat{q}_i U^{S*}(X_i, \omega_i, a_i^F = 1) + \chi_0(1 - \hat{q}_i) U^{S*}(X_i, \omega_i, a_i^F = 0)\right]}{2b_1(\gamma_0 + \gamma_1 \hat{q}_i)}$$

where $U^{S*}(X_i, \omega_i, a_i^F)$ denotes the mayor's equilibrium expected second-term payoff, conditional on his known characteristics, the state in his first term, and whether he is audited in his first term. This value is computed by integrating the mayor's second-term utility over his posterior belief about the audit probability after substituting in (19) for his effort level.

Finally, we numerically solve for $V(\omega_i)$ and the equilibrium reelection probabilities.

B.3 Model Extension: Electoral Incentives for Term-Limited Mayors

In this section, we extend the model in Section 3.2 so that term-limited mayors also have electoral incentives. To keep the problem tractable, we assume that if the mayor chooses to run for higher office after his second term, the future electorate will only be informed of any signal about the mayor's type from his last term in office. Since we are focusing on the possibility of electoral incentives for second-term mayors, we assume that first-term mayors do not run for higher office if they are not reelected.

We assume that the second-term mayor's per-period utility is

$$u_i^S(e_i^S, X_i, \varepsilon_i, a_i^S) = e_i^S + X_i'\alpha + \varepsilon_i - c(e_i^S, a_i^S) - W\pi\tilde{\varepsilon}_i^S(e_i^S, a_i^S) \tag{20}$$

where $\tilde{\varepsilon}_i^S$ denotes the voter's posterior belief about the mayor's type at the end of the term, π denotes the propensity score of the mayor to run for higher office, and W denotes the marginal benefit from having a better reputation after the second term, conditional on running for higher office. The propensity score is obtained from estimating equation (18).

Given beliefs about the audit probability implied by ω_i, we maximize (20) with respect to e_i^S. This yields the second-term mayor's equilibrium term 2 action:

$$e^{S*}(X_i, \omega_i) = \frac{1 - b_0(\gamma_0 + \gamma_1\hat{q}_i) - W\pi(\chi_0 + \chi_1\hat{q}_i)}{2b_1(\gamma_0 + \gamma_1\hat{q}_i)}$$

The remaining equilibrium strategies are derived as in the original model. The first-term mayor's problem is the same as before, with the exception that we adjust the expected utility from being reelected. Similar adjustments are made for the voter's reelection problem and the value function.

We estimate this model and report the results in the second column of Table A.6. If second-term mayors place value on completing their mayoral careers with a good reputation, then we would expect W to be positive. Surprisingly, we find a negative and statistically insignificant estimate for W. The remaining parameters estimated are very close to those we obtained without considering this additional channel. Thus, our results suggest that second-term electoral incentives are not a principal channel through which audits reduce future corruption.

C Alternative Model with Learning about Costs

In this section, we modify the model so that mayors and voters learn about the costs of audits instead of the audit probability. In both models, mayors and voters learn about the *expected costs* of the audit program, but the structure of the learning process differs between the two. In the original model considered in Section 3, agents update their priors about the audit probability based on the number of observed draws which are audits. In contrast, in the model we consider below, the agent updates his priors about the audit costs based on whether, conditional on observing an audit, the costs drawn are higher or lower than the agent's priors. Although the updating process is different in this model, we find similar results when estimating it.

C.1 Setup

In each municipality, in each term, there is a fixed probability q that an audit is drawn, which is known by all players.

There is also a fixed probability that the voter observes the rent signal. If the municipality is audited, the rent signal is drawn from the $Bernoulli(\chi_T)$ distribution, and otherwise it is drawn from the $Bernoulli(\chi_C)$ distribution. Therefore, the probability that the voter in municipality observes the rent signal is

$$\chi_i = \begin{cases} \chi_C & \text{if } a_i = 0 \\ \chi_T & \text{if } a_i = 1 \end{cases} \tag{21}$$

where a_i indicates whether the municipality is audited.

The probability that the voter observes the rent signal conditional on no audit, χ_C, is common knowledge. However, the agents are uncertain about the probability of observing the rent signal conditional on an audit. We assume that the prior for χ_T in municipality i is distributed $Beta(\beta_{0i}, \beta_{1i})$.

We now consider how agents update their beliefs in some municipality i. The number of audits observed is $y_i = a_i + \sum_{j \in N_i} a_j$, where a_i is a dummy for the realization of an audit, and N_i denotes the set of neighbors that agents in municipality i observe. Similarly, we denote the number of rent signals observed by z_i. Therefore, the prior for χ_T in the following period will be distributed $Beta(\beta_{0i} + z_i, \beta_{1i} - z_i + y_i)$.

We model learning over the probability of legal action in the same manner. Agents are certain

of the probability of legal action under no audit (γ_C), but are uncertain of the probability of legal action under an audit (γ_T). Agents in municipality i have a prior distributed $Beta(\beta_{2i}, \beta_{3i})$ for γ_T. Denoting the number of legal actions observed by agents in municipality i by w_i, it follows that the prior in the next term is distributed $Beta(\beta_{0i} + w_i, \beta_{1i} - w_i + y_i)$. Note that here, the cost of a legal action is given by $b_0 e_i + b_1 e_i^2$.

C.2 Equilibrium

The derivation of the equilibrium is very similar to that for the model with learning over the audit probability. We redefine for this section only the state vector for municipality i, $\omega_i := (\beta_{0i}, \beta_{1i}, \beta_{2i}, \beta_{3i}, n_i)$. Through Bayesian updating, this state transitions in the next term to $\omega_i' = (\beta_{0i}', \beta_{1i}', \beta_{2i}', \beta_{3i}', n_i')$, where $\beta_{0i}' = \beta_{0i} + z_i$, $\beta_{1i}' = \beta_{1i} - z_i + y_i$, $\beta_{2i}' = \beta_{2i} + w_i$, $\beta_{3i}' = \beta_{3i} - w_i + y_i$, and $n_i' = n_i$. To keep the notation consistent with previous sections, we let $\gamma_0 = \gamma_C$, $\chi_0 = \chi_C$, $\gamma_1 = \gamma_T - \gamma_C$, and $\chi_1 = \chi_T - \chi_C$.

The timing of the game is as follows; (1) Given the state of the world ω_i, and his characteristics X_i, 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, the legal action 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 and state ω_i', otherwise; (5) the second-term incumbent chooses his effort level, the second-term draws are realized and second-term rents are extracted; the game continues with step (1) with a randomly drawn first-term mayor in state ω_i''.

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 actions $e_i^T(\omega_i, X_i)$ for each term T conditional on the state ω_i and his observable characteristics X_i. A strategy for the voter is the choice $R_i(\tilde{\varepsilon}_i, \delta_i, \omega_i, X_i)$ of whether to reelect the mayor conditional on his belief about the mayor's type $\tilde{\varepsilon}_i$, the popularity shock δ_i, the state, 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.

The first-order condition yields the second-term mayor's equilibrium strategy as a function of his belief $\hat{\gamma}_{1i}$:

$$e^{S*}(\omega_i) = \frac{1 - b_0(\gamma_0 + \hat{\gamma}_{1i}q)}{2b_1(\gamma_0 + \hat{\gamma}_{1i}q)} \tag{22}$$

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

$$\begin{aligned} V(\omega_i) = &\int v^{F*}(X_i, \varepsilon_i, \delta_i; \omega_i) \\ &+ \beta \left\{ p(X_i, \varepsilon_i, a_i^F, \omega_i') \left[v^{S*}(X_i, \varepsilon_i, \omega_i') + \beta V(\omega_i'') \right] + \left[1 - p(X_i, \varepsilon_i, a_i^F, \omega_i') \right] V(\omega_i') \right\} d\mathbf{F} \end{aligned} \tag{23}$$

where δ_i is the popularity shock, p denotes the equilibrium probability of reelection, v^{F*} and v^{S*} denote equilibrium per-period voter utilities, and $\mathbf{F}$ is the joint distribution function for the vector $(X_i, \varepsilon_i, \delta_i, a_i^F, y_i, y_i', w_i, w_i', z_i, z_i')$. We note here that the draws $(y_i, y_i', w_i, w_i', z_i, z_i')$ will determine the future states ω_i' and ω_i''. Furthermore, the probability of reelection will depend on the state ω_i' rather than ω_i because the voter has observed the audit draw and updated his beliefs about the audit costs when the election occurs.

Let $\tilde{\varepsilon}_i$ denote the voter's belief about the mayor's type. Conditional on the state ω_i, the voter reelects the incumbent if

$$\delta_i \geq -h(X_i) + \beta \left(V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{\mathbf{y}_i|\omega_i} V(\omega_i') + \tilde{\varepsilon}_i \right) \tag{24}$$

where $h(X_i) := X_i'\xi - \beta X_i'\alpha$, and $\mathbf{y}_i = (y_i, w_i, z_i)$. Following the steps analogous to those described in Online Appendix B.1, the equilibrium probability that a mayor of type (X_i, ε_i) is reelected conditional on an audit being drawn in the state ω_i is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 1\right) = F_D\left(2\mu_D + h(X_i) - \beta \left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{\mathbf{y}_i|\omega_i} V(\omega_i') + (\chi_0 + \hat{\chi}_1)\varepsilon_i \right]\right) \tag{25}$$

and conditional on no audit being drawn is:

$$p\left(X_i, \varepsilon_i, \omega_i, a_i^F = 0\right) = F_D\left(2\mu_D + h(X_i) - \beta \left[V(\omega_i) + e^{S*}(\omega_i) - \beta \mathbb{E}_{\mathbf{y}_i|\omega_i} V(\omega_i') + \chi_0 \varepsilon_i \right]\right) \tag{26}$$

Finally, we solve the maximization problem of the first term mayor. We solve his maximization

problem by taking the first-order condition, which yields the equilibrium action as a function of the state ω_i and characteristics X_i:

$$e^{F*}(X_i,\omega_i) = \frac{1 - b_0(\gamma_0 + \hat{\gamma}_{1i}q) - \beta^2\sigma_D\left[(\chi_0 + \hat{\chi}_{1i})qU^{S*}(X_i,\omega_i,a_i^F=1) + \chi_0(1-q)U^{S*}(X_i,\omega_i,a_i^F=0)\right]}{2b_1(\gamma_0+\hat{\gamma}_{1i}q)} \tag{27}$$

where $U^{S*}(X_i,\omega_i,a_i^F)$ denotes the mayor's equilibrium expected second-term payoff, conditional on his known characteristics, the state in his first term, and whether he is audited in his first term.

Finally, we solve for $V(\omega_i)$ and the equilibrium reelection probabilities with the approximation that $V(\omega_i) = \mathbb{E}_{\mathbf{y}_i|\omega_i}V(\omega_i')$.

C.3 Estimation

This setup introduces eight parameters which govern learning, $\beta_0,\beta_1,\beta_2,\beta_3,\chi_0,\chi_1,\gamma_0$, and γ_1, in lieu of the six parameters estimated in the original model with learning about the audit probability. Also, notably, the econometrician does not observe the realizations $\{z_{it},w_{it}\}_{t=1}^3$. We address the two issues as follows.

In order to estimate the eight parameters, we make the following assumption. We fix β_0 so that the mean of the prior for χ_T is given by χ_C, and fix β_2 so that the mean of the prior for γ_T is given by γ_C. Thus, we assume that since mayors (and voters) are uncertain about the new audit program, they start with the prior that in expectation, the costs of rent extraction are the same whether an audit occurs or not. As they observe more audits, their beliefs about the costs of audits will converge through Bayesian updating to the true costs. Therefore, we estimate only the learning parameters $\{\beta_1,\beta_3,\chi_0,\chi_1,\gamma_0,\gamma_1\}$. In this setup, β_1 and β_3 identify the strength of the priors (respectively, $\beta_0+\beta_1$ and $\beta_2+\beta_3$), whereas the remaining parameters will capture the electoral and non-electoral costs of rent extraction.

The second econometric issue is that we do not observe the realizations of rent signals and legal actions. In words, we do not know whether in municipality i in period t, voters have observed the rent signal, or a legal action has taken place against the mayor. To address this, for each observation in the data, we sum over the likelihood of the observation given each possible history of rent signals and legal actions leading up to that point.

More precisely, let $\pi(\omega_i|m_i)$ denote the probability that the state in a municipality is ω_i, conditional on the total number of observed audit draws as of the current mayor's term being m_i. This number

is given by the data, $m_i := \sum_{s=1}^{t-1} y_{is}$. Then we can write the likelihood function as:

$$\prod_i \sum_{\omega_i} L(\theta | r_i, R_i, X_i, Z_i, a_i^F, T_i, \omega_i) \pi(\omega_i | m_i) \tag{28}$$

where, given the independence of the two cost shocks,

$$\pi(\omega_i | m_i) = \pi_0(\beta_{0i}, \beta_{1i} | m_i) \pi_1(\beta_{2i}, \beta_{3i} | m_i) \tag{29}$$

and

$$\pi_0(\beta_0 + z, \beta_1 - z + m_i | m_i) = \binom{m_i}{z} (\gamma_T)^z (1 - \gamma_T)^{m_i - z} \quad \text{for } z = 0, ..., m_i \tag{30}$$

$$\pi_1(\beta_2 + w, \beta_3 - w + m_i | m_i) = \binom{m_i}{w} (\chi_T)^w (1 - \chi_T)^{m_i - w} \quad \text{for } w = 0, ..., m_i \tag{31}$$

and $\pi_0 = 0$ for any other combinations of (β_{0i}, β_{1i}), and likewise $\pi_1 = 0$ for any other combinations of (β_{2i}, β_{3i}). Here, the fact that we have use the Beta-Binomial functional form for beliefs reduces the computational burden of calculating the likelihood function. If $m_i = 0$, the we have instead $\pi(\beta_0, \beta_1, \beta_2, \beta_3, n_i) = 1$, i.e. ω_i is set to the prior described above.

C.4 Results

Table A.7 presents the results of the structural estimation. Column 1 reports the results of the present estimation, where mayors and voters learn about costs (the "cost model"). We compare these results to those obtained in the main text, which we replicate in column 2 (the "probability model"). The point estimates for the parameters are very similar for the two models. The main difference is in the estimate for γ_1, the added probability of a legal action when there is an audit, which is larger in the cost model. This difference is due to the fact that we separately estimate the strength of the prior for the legal action probability and the prior for the voter's rent signal probability in the cost model. It turns out that the estimated strength of the prior for the legal action ($\hat{\beta}_2 + \hat{\beta}_3 = 38.3$) is larger than that for the voter's rent signal ($\hat{\beta}_0 + \hat{\beta}_1 = 6.2$), which translates into a larger estimate for γ_1 to compensate for each audit causing relatively smaller shifts in the beliefs about legal actions.

Despite the minor differences in the parameter estimates, both models suggest a similar breakdown of the effects of audits between the three channels. The bulk of the reduction in rents is due to

discipline in both models, with legal discipline accounting for 82 percent of the share in the cost model compared to 72 percent in the probability model. In both frameworks, electoral discipline is the second most important in channel, accounting for respectively 18 and 28 percent of the reduction in rents. Finally, selection plays a minimal role in both models.

Unfortunately, the cost model is ill-suited to quantify the effects of a change in the audit probability on absolute levels of corruption. The setup allows for the comparison of rents between municipalities with different audit histories, but it cannot address the extent to which corruption is lower across all municipalities because of the audit program itself. Thus, we cannot compare the two modeling frameworks with respect to their predictions about the effects of counterfactual audit probabilities.

We conclude this section by comparing how the models fit the sample and out-of-sample data. First, both models fit the sample data similarly. The log-likelihood of the cost model is -679.31, compared to -682.01 for the probability model. In terms of out-of-sample fit, the two models also have similar performances. Using our estimates from the cost model, we compute the predicted rents for the additional 239 observations from the most recent audits. The mean squared deviation between predicted and observed rents is 0.139, compared to 0.140 for the probability model. The absolute squared deviation is 0.280 for both models. Hence, both models perform very similarly in fitting out-of-sample corruption.