Exposing Corrupt Politicians:

The Effect of Brazil's Publicly Released Audits on Electoral Outcomes*

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Abstract

This paper examines the extent to which access to information enhances political accountability. Based upon the results of Brazil's recent anti-corruption program that randomly audits municipal expenditures of federally-transferred funds, it estimates the effect of the disclosure of local government corruption practices upon the re-election success of incumbent mayors in municipal elections. Comparing municipalities which were randomly audited before the elections with those audited after, the analysis shows that the disclosure of audit results had a significant impact on the re-election rates of mayors found to be corrupt. For every additional corruption violation reported, the audit policy reduced the incumbent's likelihood of re-election by approximately 20 percent. This effect is more pronounced in municipalities where radio stations are present and higher levels of corruption are identified. These findings highlight the value of information and the role of the media in reducing informational asymmetries in the political process, thus enabling voters to not only hold corrupt politicians accountable but also to reward non-corrupt politicians.

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1 Introduction

The asymmetry of information between voters and policymakers is a central feature of political agency models. Voters typically do not observe the politician's actions or may be uninformed about his preferences.[1] Because the interests of voters and politicians are not necessarily aligned, politicians have an incentive to exploit this informational advantage to behave opportunistically. Among the potential governance problems that information asymmetries create, corruption stands at the forefront.

Consequently, one of the most powerful ways of monitoring politicians and preventing corruption is to increase transparency by expanding the right to information disclosure (Sen 1999).[2] With better information, voters can identify high quality politicians and hold corrupt politicians accountable at the polls, thus reducing agency problems (Besley (2005)).

While transparency has become the hallmark of good governance, an understanding of how the availability of corruption information affects political accountability is rather limited. The provisioning of information is thought to improve accountability, but theoretically this need not be the case when voters are faced with both a moral hazard and an adverse selection problem (Besley and Pratt 2004).[3] Empirically, the lack of objective corruption measures and the identification problems associated with the non-random nature of information disclosure continue to mount serious methodological obstacles.[4]

In this paper, we study the effect of the disclosure of local governmental corruption practices on the incumbent's electoral outcomes in Brazil's municipal elections. We overcome previous data limitations and identification concerns by using an experimental design that generates exogenous variation in the exposure of corrupt politicians to the public. Our analysis utilizes Brazil's recent anti-corruption program initiated in April of 2003, when the federal government began to audit

[1] See for example Alesina and Cukierman (1990), Barro (1970), Ferejohn (1986), Persson, Roland, and Tabellini (1997). Alternatively, information asymmetries may stem from when information is costly and policymakers have better access to information on the consequences of alternative policies. This type of asymmetry is at the heart of the models pioneered by Baron (1994) and Grossman and Helpman (1996).

[2] The World Bank, for example, asserts that "Transparency via public scrutiny has proven to be one of the most powerful forms of monitoring public officials...". This reasoning has led several countries to experiment with transparency and information dissemination programs aimed at reducing corruption. For example, Argentina began a program in 1999 called Cristal. This program disseminates online all information concerning the use of public funds in Argentina. In 1998, the municipal government of Seoul, South Korea, started the program OPEN, which publishes a variety of information related to the services, permits and licenses issued by the local government. In addition to these programs, others exist in various states of India, as well as the in Philippines, Sri Lanka, and Colombia.

[3] Recent work on political agency models that incorporate both moral hazard and adverse selection show that while greater access to information allows voters to better screen politicians, it might also encourage corrupt politicians to disguise their behavior thus decreasing the likelihood of detection. In addition to these issues of asymmetrical information, as is demonstrated theoretically, the effect of information disclosure on political accountability will also depend on the prior beliefs voters have about the incumbent's corruption activities.

[4] The evidence supporting the linkage between transparency and accountability is sparse. The existing studies examining how information about corruption affects electoral outcomes are mostly based on accusations of corruption rather than actual measures of corruption. See for example Peters and Welch (1980) for the United States and Chang (2005) and Chang and Golden (2004) for the case of Italy.

municipal governments for their application of federal funds. Each month, approximately sixty municipalities are chosen at random, and then inspected by a team of auditors for the misappropriation of federal funds, and any other irregularity associated with a federally-sponsored project or public work. To promote transparency, the results of these audits are disseminated publicly to both the municipality and general media. The randomized design and public dissemination allow us to compare the electoral outcomes of incumbent mayors in municipalities where public audits occurred to the electoral outcomes in municipalities where audits did not occur.

Although this simple comparison helps identify the average impact of the program on electoral outcomes, it does not capture the differential policy effects in terms of two important aspects: the influence of voters' prior belief and the presence of the media. In the first case, as we show theoretically, for the audits to negatively (or positively) affect re-election outcomes on average, voters would have had to systematically underestimate (or overestimate) the incumbent's corruption level. However, how the information from the audits compares to the prior beliefs is likely to depend on the information disclosed. An audit that does not report any corruption would expectedly have a different effect on electoral outcomes than one that reports severe corruption. The comparison of municipalities that were audited to those that were not fails to account for these differential effects, which on average may cancel each other out. Secondly, because the media is used as a vehicle for disseminating these audit findings, one would expect the audit policy to have a differential effect in regions where local media is present to help diffuse the audit reports.

To address these issues, we use the audit reports to construct an objective measure of corruption for each audited municipality. Although we only know the extent of corruption for municipalities that have been audited, the timing of the 2004 election and the release of the audit reports allow us to measure corruption levels for a group of municipalities that were audited but had their corruption reports released only after the election. Because the order in which municipalities are selected is random, this group of municipalities constitute a valid counterfactual for the effect of information disclosure. Thus, we measure the program's effect by comparing, among the audited municipalities, those whose reports were released prior to election to those whose reports were released after the election, allowing the effect to vary by the degree of corruption and the availability of local media.

Our main findings are as follows. We begin by showing that the electoral performance of incumbent mayors that were audited, while slightly worse, was not significantly different from the electoral outcomes of mayors that were audited after the election. However, when we account for the level of corruption that was revealed in the audit, we find that the effect of the policy was considerable. For every additional corrupt violation reported, the audit policy reduced the incumbent's likelihood of re-election by approximately 20 percent. The effect of the policy was similar for other measures of electoral performance, such as the change in vote share and margin of victory. These results suggest that voters not only care about corruption, but once empowered

with the information, update their prior beliefs and punish corrupt politicians.[5]

Furthermore, in those municipalities with local radio stations, the effect of disclosing corruption on the incumbent's likelihood of re-election was more severe. Among municipalities with local radio, for an additional corruption violation, the release of the audits decreased the probability of re-election by approximately 15.7 percentage points, which represents a decline of 40 percent from baseline. Although radio exacerbates the audit effect when corruption is revealed, it also promotes non-corrupt incumbents. When corruption was not found in a municipality with local radio, the audit actually increased the likelihood that the mayor was re-elected by as much as 20 percentage points.

In effect, these results indicate not only that the disclosure of information enhances political accountability but that the interpretation of this information is ultimately influenced by the prior beliefs of voters. On average voters do share the initial belief that politicians are corrupt, and only punish those incumbents who were discovered to have "surpassed" the average level of corruption. When no corruption is revealed and voters had overestimated the incumbent's corruption level, the incumbent is rewarded at the polls. Interestingly, in areas without local radio, voters tend to systematically underestimate the corruption level of the incumbent, indicating that local radio also influences prior beliefs. Together these results highlight how media can profoundly affect political outcomes both in the screening of politicians and in conditioning individual beliefs. The analysis suggests that the media influences the selection of good politicians both by exposing corrupt politicians and promoting good ones.

Our findings contribute to a growing literature that examines the effectiveness of anti-corruption programs. Di Tella and Schargrodsky (2003) study the impact of a corruption crack-down in the city of Buenos Aires. Using an auditing process induced by a change in government, they find that increasing monitoring decreases the price public hospitals pay for inputs. Yang (2005) shows that the adoption of pre-shipment inspection programs on imports is associated with a 6-8 percentage point annual increase in the growth rate of import duties. Because these programs are also associated with both increases in imports and declines in the misreporting of goods classification, he concludes that the increase in imports duties is likely a result of a reduction in customs corruption. Reinikka and Svensson (2004) analyze the effects of an information campaign designed to reduce the diversion of public funds transferred to schools in Uganda. Using a difference-in-difference approach, they compare the capture of public funds in schools with access to newspapers to schools without access to newspapers before and after a large anti-corruption campaign. They find that schools with newspaper access received, on average, 13 percent more of their entitlement. They

[5]Whether voters care about corruption has been a relatively unexplored empirical question due to the difficulty in obtaining objective corruption measures. Previous work focuses on studying how charges of corruption affect the vote totals of candidates running for election for the U.S. House of Representatives. Peters and Welch (1980) compare election outcomes of candidates accused of corruption to candidates who were not accused during elections held from 1968 to 1978 and find that candidates accused of corruption suffer an eight percent loss in their expected vote. Welch and Hibbing (1997) provide a follow-up study that confirm and update the previous results.

conclude that information allowed parents and teachers to exert pressure on local politicians and decrease corruption. Our paper, although related through the information dissemination mechanism, uses an identification strategy based on a randomized quasi-experiment which controls for any potential confounds associated with the endogenous acquisition of information. In a related paper Olken (2004) conducts a randomized field experiment in 608 Indonesian villages to analyze how different monitoring mechanisms might reduce corruption in infrastructure projects. His findings suggest that central auditing mechanisms are more effective to control corruption when compared to grassroots participation monitoring. We see our paper as complementary to these studies by providing evidence that information disclosure about corruption helps to reduce capture of public resources through an alternative mechanism: reducing asymmetrical information in the political process to enable voters to select better politicians.[6]

This paper is also related to the literature that explores the role of media in shaping public policy and influencing the political process. Several papers use cross-country data and show a positive association between a free, well-developed media and good governance (Brunetti and Weder (2003); Ahrend (2002)). Nonetheless these studies do not provide evidence on the specific ways in which media availability promotes good governance.[7] Recent contributions have started to fill this gap. Besley and Burgess (2002) focus on the role of the media in mitigating political agency problems by providing information to voters. They analyze the relief of shocks in India and show that in places with newspapers, governments are more responsive. In a related paper, Stromberg (2004) suggests that U.S. counties with more radio listeners received more relief funds from the New Deal program. His results are consistent with a theory in which politicians target resources to voters who are better informed.[8] Our results contribute to this literature by demonstrating the specific impacts of the media upon political accountability.

The remainder of the paper is organized as follows. Section 2 presents a simple theoretical framework to understand how information might affect political accountability. Section 3 then provides a brief background on Brazil's anti-corruption program, and a description of the data used in the analysis. The paper's main empirical findings are presented in section 5 and section 6 concludes the paper.

2 Theoretical Framework

In this section, we present a simple theoretical framework to guide the interpretation of our empirical findings. Our model, which is derived from Majumdar, Mani, and Mukand (2005) and similar to those presented in Persson and Tabellini (2000) and Alesina and Tabellini (2004), is a simple

[6] See Besley (2005) Besley, Pande, and Rao (2004) for a discussion on political selection.

[7] Besley, Burgess, and Pratt (2002) state that there is comparatively little work in the political economy literature that scrutinizes the role and effectiveness of the media in fulfilling this function.

[8] Similar to the case of rural areas in the U.S. in the early 20th century, radio in small municipalities in Brazil plays a crucial role as a media source and influences citizens opinions and perceptions.

adaptation of the standard political agency models. This class of models, where voters have limited information about the incumbent's type or behavior, provides a natural environment to examine the value of information in promoting political accountability.

2.1 A Simple Model

Consider a simple model where society is comprised of informed and uninformed voters. Voters have linear preferences over a single policy g, which is affected by both the incumbent politician's effort e and his type τ. The random variable τ measures the politician's propensity to be good (or non-corrupt) and, like the politician's effort, is unobserved to the voters. Voters do however have prior beliefs about the politician's type based on the knowledge that τ is distributed normally with mean $\bar{\tau}$ and variance σ_τ. The policy outcome g is thus defined as,

$$g = e + \tau + \nu$$

where ν is a productivity shock, which is distributed $N(0, \sigma_\nu)$ and is uncorrelated with τ. While all voters observe policy outcome g, only informed voters also observe ν. In this additive setting, all else held constant, voters prefer less corrupt politicians (higher τ), since this would imply a higher policy outcome g.

Besides preferences over government policy, voters possess an ideological preference δ in favor of the incumbent mayor. We further assume that the parameter δ is distributed uniformly over the interval $[-\frac{1}{2\xi}, \frac{1}{2\xi}]$. Thus in deciding to vote after observing his utility, the citizen compares his estimate of the incumbent's type and ideological preferences to his prior beliefs,

$$E[\tau|\Omega] + \delta > \bar{\tau}$$

where $E[\tau|\Omega]$ is the voter's posterior belief of the incumbent's type given his information set, Ω. Thus for given realizations of g and ν and the distribution of δ, the incumbent's vote share is given by,

$$\pi = \frac{1}{2} + \xi[\alpha(E[\tau|g,\nu] - \bar{\tau}) + (1-\alpha)(E[\tau|g] - \bar{\tau})] \tag{1}$$

where α is the proportion of that population that is informed.

Given this simple setup, the timing of the events is as follows. In beginning of the period, the incumbent politician, whose simple objective is to maximize the probability of re-election, chooses the level of effort, e, before knowing his type τ.[9] Nature then reveals the value of τ and ν, determining the outcome policy, g. Observing only their own utility, voters are forced to

[9]The assumption that the incumbent does not know his own type is admittedly strong. This simplifying assumption allows us to avoid issues associated with politicians using their effort level to signal their type. See Persson and Tabellini (2000) and Besley (2004) for a discussion of political agency models that assumes both moral hazard and adverse selection.

make inference on the incumbent's type. Elections are held. If the incumbent wins, his type τ is maintained. Otherwise, the challenger enters office with a competency level randomly drawn from the same normal distribution.

To compute the posterior beliefs of the voters, we assume that voters update their beliefs according to Bayes' rule.[10] Given our distribution assumptions, the expected mean of an uninformed voter's posterior beliefs is

$$E[\tau|g] = \frac{h_\tau \bar{\tau} + h_\nu(g - e^*)}{h_\tau + h_\nu} \tag{2}$$

where $h_\tau = \frac{1}{\sigma_\tau}$ and $h_\tau = \frac{1}{\sigma_\nu}$, and e^* denotes the voters' expectation of the incumbent's effort which is realized in equilibrium. For informed voters, who observe ν, their posterior beliefs can be expressed as follows,

$$E[\tau|g, v] = g - e^* - v. \tag{3}$$

Inserting equations 2 and 3 into 1, an incumbent of type τ wins the election if he receives a majority of the votes, i.e.

$$\xi\left(\alpha(\tau - \bar{\tau}) + (1 - \alpha)\left(\frac{h_\tau \bar{\tau} + h_\nu(\tau + \nu)}{h_\tau + h_\nu} - \bar{\tau}\right)\right) > 0.$$

Therefore, the probability that this incumbent of type τ wins is given by

$$1 - \Phi\left(-\frac{\tau - \bar{\tau}}{(1 - \alpha)\sqrt{h_\nu}}(\alpha(h_\tau + h_\nu) + (1 - \alpha)h_\nu)\right) \tag{4}$$

where Φ is the cdf of the standard normal distribution. The probability of re-election is thus increasing in the politician's type, τ, and exceeds 0.5 when $\tau - \bar{\tau} > 0$.

Equation 4 implies that the effect of more information on political accountability will depend on voters' initial priors. Providing information to the voters (i.e. increasing proportion of informed voters, $\uparrow \alpha$) will increase the probability of re-election if the politician's expected type exceeds the voters' beliefs ($\tau - \bar{\tau} > 0$) but decrease the probability of re-election if voters' had overestimated the politician's type ($\tau - \bar{\tau} < 0$). Moreover, whether voters over or underestimate the corruption level of the politician is likely to depend on the type of information that was revealed, and thus accounting for the level of reported corruption is a critical feature of our empirical strategy.

In effect, this simple model provides clear empirical predictions. Unless voters systematically over or under-estimate the incumbent's corruption level, the simple average treatment effect of the audits may be undetermined, and will expectedly vary according to the level of reported corruption. The average treatment effect will likely be negative at higher levels of reported corruption, and

[10] While Bayesian updating is a standard assumption in economic models, it should not be taken lightly. There is a large and growing body of literature suggesting that individuals are instead selective in the manner in which they gather and process information. Beliefs, once formed, are slow to change as individuals interpret new information to confirm previously held convictions (Rabin 1998).

presumably positive at zero levels of reported corruption.

3 Background and Data

3.1 Brazil's anti-corruption program

In May 2003 the government of Luiz Inácio Lula da Silva started an unprecedented anti-corruption program based on the random auditing of municipal government's expenditures.[11] The program, which is implemented through the Controladoria Geral da União (CGU), aims at discouraging misuse of public funds among public administrators and fostering civil society participation in the control of public expenditures. To help meet these objectives, the findings from each audit are posted on the internet and released to the media. The following quote from President Lula, extracted from the program's inaugural speech, emphasizes this: "I think the Brazilian society needs to understand once and for all, that we are only going to be able to truly fight corruption when the civil society, with the instruments made available, can act as a watch dog."[12]

The program started with the audit of 26 randomly selected municipalities, one in each state of Brazil. The program has since expanded to auditing 50 and later 60 municipalities per lottery, from a sample of all Brazilian municipalities with less than 450,000 inhabitants.[13] The lotteries, which are held on a monthly basis at the Caixa Econômica Federal in Brasilia, are drawn in conjunction with the national lotteries. To assure a fair and transparent process, representatives of the press, political parties, and members of the civil society are all invited to witness the lottery.

Once a municipality is chosen, the CGU gathers information on all federal funds transferred to the municipal government from 2001 onwards. Approximately 10 to 15 CGU auditors are then sent to the municipality to examine accounts and documents, to inspect for the existence and quality of public work construction, and delivery of public services. Auditors also meet members of the local community, as well as municipal councils in order to get direct complaints about any malfeasance.[14] After approximately one week of inspections, a detailed report describing all the irregularities found is submitted to the central CGU office in Brasilia. The reports are then sent to the Tribunal de Contas da União (TCU), to public prosecutors and to the legislative branch of the municipality. For each municipality audited, a summary of the main findings is posted on the internet and disclosed to main media sources. It is from these reports that we construct an objective measure of corruption.

[11] In Portuguese the program is called *Programa de Fiscalização a partir de Sorteios Públicos*.

[12] The speech is available at www.presidencia.gov.be/cgu.

[13] This excludes approximately 8 percent of Brazil's 5500 municipalities, comprising mostly of the state capitals and coastal cities.

[14] These auditors are hired based on a public examination, and prior to visiting the municipality receive extensive training on the specificities of the sampled municipality. Also, there is a supervisor for each team of auditors.

3.2 Measuring Corruption from the Audit Reports

In this section we describe how we use the audit reports to construct an objective measure of corruption. As of July 2005, reports are available for the 669 municipalities that were randomly selected across the first 13 lotteries.[15] Each report contains the total amount of federal funds transferred to the current administration and consequently audited, as well as, an itemized list describing each irregularity, in what sector it occurred (e.g. health, education, etc.), and in most cases the amount of funds involved.

Based on our readings of the reports, we codified the irregularities listed into several categories; some indicating corruption while others simply exposing poor administration.[16] We define political corruption to be any irregularity associated with fraud in procurements, diversion of public funds, and over-invoicing.[17] Although local corruption in Brazil assumes a variety of forms, illegal procurement practices, diversion of funds, and the over-invoicing of goods and services are among the most common violations found in the audit reports.[18] Illegal procurement practices typically consist of benefiting friendly or family firms with insider information on the value of the project, or imposing certain restrictions to limit the number of potential bidders. This was the situation in *Cacule*, Bahia, where the call for bids on the construction of a sports complex specified as a minimum requirement for participation that all firms needed to have at least R$100,000 in capital and a specific quality control certification. Only one firm called Geo-Technik Ltda., which was discovered to have provided kickbacks to the mayor, met this qualification. While some procurement practices manipulate the process in favor of firms that offer potential kickbacks, other strategies are more blatant forms of fund diversions. In *São Francisco do Conde*, Bahia, for example, a health contract of R$308,000 was awarded to a phantom firm: a firm that only exists on paper.

Other dominant forms of corruption include mayors diverting funds intended for education and health projects towards private goods, (e.g. the purchase of cars, fuel, apartments, or payment of their friends' salaries) or simply over-invoicing goods and services. For example, in *Paranhos*, Mato Grosso do Sul, R$189,000 was paid to implement a rural electrification project. As it turns out, the mayor owned one of the farms benefitted by the project. Over-invoicing is typically found in the provision of medical supplies and the construction of public works.

These types of practices have not only been shown to be the most common forms local politicians

[15] Audit reports are only available for 669 municipality, instead of 676 municipalities, because 7 municipalities audited were randomly selected twice.

[16] We also used an independent research assistant to code the reports in order to provide a check on our coding. Also, see Ferraz and Finan (2005) for more details on how we coded the audit reports.

[17] Specifically, we define a *procurement to be irregular* if: i) a required procurement was not executed; ii) the minimum number of bids was not attained; iii) there was evidence of fraud in the procurement process (e.g. use of bids from non-existing firms). We categorize *diversion of public funds* as any expenditure without proof of purchase or provision and/or direct evidence of diversion provided by the CGU. Finally, we define *over-invoicing* as any evidence that public goods and services were bought for a value above the market price.

[18] These forms of corruption are also frequently discussed in the Brazilian literature on corruption. See for example Trevisan, Antoninho M. et al. (2003); Fleischer (2002); Geddes and Neto (1999).

find to appropriate resources, but in many instances they are not necessarily mutually exclusive (see Trevisan, Antoninho M. et al. (2003)). In effect, over-invoicing and illegal procurement practices often serve as vehicles for funds diversion. As such, we combined indicators of these three categories to best capture the municipality's corruption level. Specifically, we sum up for each municipality all the irregularities associated with each of these three categories and define this as our measure of corruption.[19]

3.3 Complementary Data Sources

Three other data sources are used in this paper. The political outcome variables and mayor characteristics come from the Tribunal Superior Eleitoral (TSE), which provides results for 2000 and 2004 municipal elections. These data contain vote totals for each candidate by municipality, along with various individual characteristics, such as the candidate's gender, education, occupation, and party affiliation. With this information, we matched individuals across elections to construct our main dependent variable - whether the incumbent mayor was re-elected - as well as other measures of electoral performance such as vote shares and win margin.

To capture underlying differences in municipal characteristics, we relied on two surveys from the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatística (IBGE)). First, the 2000 population census provides several socioeconomic and demographic characteristics used as controls in our regressions. Some of these key variables include per capita income, income inequality, population density, share of the population that is urban, and share of the population that is literate. Second, to control for different institutional features of the municipality, we benefited from a 1999 municipality survey, *Perfil dos Municípios Brasileiros: Gestão Pública*. 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 the percentage of roads that are paved, whether the municipality has a judge, among others. Moreover, the survey provides our key measures of the availability of media, 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.

Basic descriptive statistics of our corruption measure, electoral outcomes, and municipal characteristics are presented in Table 1. Besides providing background on the average municipality's socio-economic and political characteristics, the table also reports, as a check of the randomization, whether any systematic differences exist between municipalities audited before and after the elections. Column 1 presents the mean for the municipalities that were audited after the election (control group), while Column 2 presents the mean for the municipalities that were audited prior

[19]To give a better sense of the irregularities found and the procedure used to code corruption, we present in the appendix some specific examples from the audit reports.

to the election (treatment group). The difference in the group means are reported in Column 3, and the standard errors of these differences are presented in Column 4.

Panel A and B document the political outcomes and characteristics of the mayors in our sample. Re-election rates for the past two elections have been roughly 40 percent among the incumbent mayors that are eligible for re-election.[20] While it might appear that Brazilian mayors do not enjoy the same incumbent advantage that is reputed in other countries, re-election rates do increase to 59 percent (see column 1), when conditioned on the mayors that ran for re-election (approximately 70 percent of all eligible mayors). Re-election in Brazil requires only a relative majority, and yet on average elected mayors win with over 50 percent of the votes. Even though 18 political parties are represented in our sample, over 70 percent of the elected mayors belong to one of the 6 parties presented in Panel B, and on average only 3 political parties compete within a particular municipality.

The municipalities in our sample tend to be sparsely populated and relatively poor (see Panel C). The average per capita monthly income in our sample is only R$204 (US$81), which is slightly less than the country's minimum wage of R$240 per month. Approximately 38 percent of the population of these municipalities live in rural areas, and only 21 percent of the adult population is illiterate. Local AM radio stations exist in only 27 percent of the municipalities; among those municipalities with an AM radio station the average number of radio stations is 1.32.

The characteristics summarized in panels A-C are well-balanced across the two groups of municipalities. There are no significant differences across groups for any of the characteristics presented in the table, at a 5 percent level of significance.[21] In fact, out of 90 characteristics, only three variables - the number of museums, whether the municipality has a local constitution, whether the municipality has an environmental council - were significantly different between the two groups of municipalities; including these three characteristics in the regressions do not affect the estimated coefficients.

The last couple rows of Table 1 present the constructed corruption measure, and the average amount of federal funds audited. The program audited approximately 5.5 million reais per year, and found that in Brazil municipal corruption is widespread. At least 73 percent of the municipalities in our sample had some incidence of corruption reported, and the average number of corrupt irregularities found was 1.74. Municipalities that were audited after the elections tend to be slightly more corrupt than those audited before the election, but this difference is small and statistically indistinguishable from zero.

For a better sense of the corruption measure, Figure 1 presents the distributions of reported corruption for municipalities that were audited before and after the elections. As this figure depicts,

[20] In the 2000 election, every mayor was eligible for re-election. It was the first municipal election in which incumbents could seek a consecutive term. Ferraz and Finan (2005) exploit the introduction of the two term limit to investigate how re-election incentives affect corruption levels.

[21] Whether the mayor belongs to PMB is significantly different between the groups at the 10 percent level. As demonstrated in the results section, controlling for this variable does not affect the estimation results.

the mass of the distribution falls between 0 and 4 corrupt violations, with less than 6 percent of the sample having more than 4 corrupt violations. As with the comparison in means, the distributions of corruption between the two groups are also fairly well balanced. At each level of corruption, none of the differences in distributions are statistically significant at a 10 percent level. This comparison further validates not only the program's randomized auditing, but also the integrity of the audit process.[22]

4 Estimation Strategy

Based on the theoretical model presented above, we are interested in testing whether the release of information about the extent of municipal government corruption affects the electoral outcomes of incumbent mayors. The ideal experiment to test this would consist of auditing municipalities to record their corruption levels and then releasing this information to voters in a random subset of municipalities. For any given level of corruption, the simple comparison of the electoral outcomes in municipalities where information was released to those where no information was released estimates the causal effect of disclosing information about corruption on voting patterns. In practice however, this experiment is both unethical and politically infeasible. Our research design, which exploits the random auditing of the anti-corruption program and the timing of the municipal elections, is perhaps the closest approximation to such an experiment.

Figure 2 depicts the timing of the release of the corruption reports. Prior to the October elections, the Federal government had audited and released information on the corruption levels of 376 municipalities randomly selected across 8 lotteries. After the municipal elections, audit reports for 300 municipalities were released, providing us information on corruption levels for two groups of municipalities: those where corruption levels were released prior to the elections - potentially affecting voters' perceptions on mayor's performance - and those that were audited and had their results released only after the elections. Since municipalities were selected at random, the set of municipalities where audit reports were only made available after the elections represent a valid control group. Furthermore, by restricting our analysis to only municipalities that were audited, we are able to construct a measure of corruption for each municipality and thus account for the opposite effects that positive and negative corruption reports might induce.[23]

In this section we proceed as follows. Under the assumption that the audits of municipalities before the elections did not affect the electoral outcomes of municipalities audited after the elections, we compare electoral outcomes in municipalities that were audited after the elections to estimate the average effect of the audit policy. We then investigate how the effect of the program varies by

[22]Had corruption levels differed significantly between municipalities audited before and after the elections, one might have been worried that the audit process itself was corrupt. Nevertheless, we provide additional evidence against this hypothesis in the analysis below.

[23]To estimate the average impact of the audit policy, we can also compare municipalities that were audited to those that were not audited. To do so produces similar results.

the information revealed and availability of media. Given the use of media to disseminate the audit results, one might expect the program to have a more significant impact on electoral outcomes in areas where more corruption is revealed and local media is present to diffuse such information.

Basic Models

To estimate the average effect of the audit policy on electoral outcomes, we begin with the following reduced-form model

$$E_{ms} = \alpha + \beta A_{ms} + X_{ms}\gamma + \nu_s + \varepsilon_{ms}, \tag{5}$$

where E_{ms} denotes the 2004 electoral performance of an incumbent mayor eligible for re-election in municipality m and state s, A_{ms} is an indicator for whether or not the municipality was audited prior to the October 2004 elections, X_{mj} is a vector of municipality and mayor characteristics that determine electoral outcomes, ν_s is a state fixed effect, and ε_{ms} is a random error term for the municipality.[24] Because of the randomized auditing, the coefficient β, provides an unbiased estimate of the average effect of the program on the electoral outcomes of the incumbent politician: capturing both the effect of being audited and the public release of this information.

Although estimation of equation 5 relies only on the 2004 election results, for some electoral outcomes, such as the mayor's vote share and margin of victory, we can also measure the incumbent's performance in the previous 2000 election. Thus, we exploit this additional information to compare changes in electoral outcomes between municipalities that were audited and those that were not, using the following specification:

$$\Delta E_{ms} = E_{mst} - E_{mst-1} = \alpha + \beta A_{ms} + X_{ms}\gamma + \varepsilon_{ms}, \tag{6}$$

where ΔE_{ms} denotes the difference in the mayor's electoral performance between the 2000 and 2004 elections. Consequently, β measures the effect of the audit on the change in the mayor's electoral outcome, ΔE_{ms}. Note that while the identification of β still stems from the randomized policy, by estimating a change in outcomes, we are implicitly removing any time invariant determinants of voter behavior across municipalities. Moreover, with state intercepts, this specification controls for any unobserved state-specific shocks affecting the change in vote shares.

Differential Effects by Corruption and Media

The specifications presented in equations 5 and 6, while providing estimates of the average effect of the audits, ignore that the effect of a publicly released audit may depend on the information disclosed. An audit revealing extensive corruption should have a markedly different effect on an

[24] Because each state had its own team of auditors, state intercepts absorb any differences in how teams from different states audited their municipalities. Also, the northern region of Brazil (i.e. the Amazon region consisting of Amapa, Amazonas, Roraima, Rondonia, Acre and Tocantins), was considered a single state for lottery purposes due to its low population density. We use a unique indicator for this region in the state fixed effect specifications.

incumbent's re-election chances than one that reports no corruption. To test for this differential effect, we estimate a model that includes an interaction of whether the municipality was audited prior to the elections with the level of corruption discovered in the audit:

$$E_{ms} = \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 (A_{ms} \times C_{ms}) + X_{ms}\gamma + \nu_m + \varepsilon_{ms}, \quad (7)$$

where C_{ms} is the number of corrupt irregularities found in the municipality. In this model, the parameter β_2 estimates the causal impact of the policy, conditional on the municipality's level of corruption.

Another potentially important source of variation is the availability of media in the municipality. A critical design feature of the policy is the use of mass media to divulge the results of the audit. So even though the audit report is sent to the local legislative branch, if the government audits and media are complements then we would expect a more pronounced effect in areas with a local media. On the other hand, if in areas with media the public is already informed about the extent of the mayor's corruption - perhaps due to better investigative journalism - then the audits and media might instead function as substitutes. In this situation, we might expect the audits to have had a more significant impact in areas without media. To test these two competing hypotheses, we augment the specification in equation 7 with a set of terms to capture the triple interaction between whether the municipality was audited, its corruption level, and its availability of local media:

$$\begin{aligned} E_{ms} &= \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 M_{ms} \\ &+ \beta_3 (A_{ms} \times M_{ms}) + \beta_4 (A_{ms} \times C_{ms}) + \beta_5 (M_{ms} \times C_{ms}) \\ &+ \beta_6 (A_{ms} \times C_{ms} \times M_{ms}) + X_{ms}\gamma + \nu_m + \varepsilon_{ms}. \end{aligned} \quad (8)$$

Our measure of media, M_{ms}, in this specification is the number of AM radio stations that exist in the municipality. For the majority of municipalities in Brazil, radio is often the most important source of information on both local politics and federal news (Dunn 2001). We also experiment with the number of newspapers in the municipality as an alternative measure of media, even though in Brazil the printed press is not a widely used source of local information. With this model, the main parameter of interest β_6 captures the differential effect of audits by the level of corruption reported and the number of radio stations in the municipality.

5 Results

5.1 Measuring the Average Effect of the Audits on Electoral Outcomes

We begin this section by presenting estimates of the average effect of the audit policy on various electoral outcomes. Panel A of Table 2 presents regression results from estimating several vari-

ants to equation 5, where the dependent variable is an indicator for whether the incumbent was re-elected in the 2004 elections. The results presented in Columns (1) and (2) of panel A are estimated for the sample of incumbent mayors who were eligible for re-election in 2004, whereas Columns (3) and (4) of panel A consider only those mayors who actually ran for re-election.[25] The first specification (Column (1)) controls for state fixed-effects but excludes other control variables. Column (2) presents an extended specification that includes various municipal and mayor characteristics. Columns (3) and (4) simply replicate the specifications presented in Columns (1) and (2) for the other sample.[26]

The results in panel A suggest that the audits and the associated release of information did not have a significant effect on the re-election probability of incumbent mayors. While re-election rates are 3.6 percentage points lower in municipalities that were audited prior to the elections (column 1 of panel A), we cannot reject that this effect is not statistically different from zero (standard error is 0.053). The inclusion of municipal and mayor characteristics (Columns (2) of panel A), which should absorb some of the variation in the error term, does not alter the estimated effect or the estimated precision. Restricting the sample to include only mayors that ran for re-election provides similar results (Columns (3) and (4) of panel A).

Even though the audits do not appear to have significantly affected re-election probabilities, winning the election is a discontinuous outcome. The program might have impacted other measures of electoral performance: such as, vote shares and electoral competition, without ultimately affecting the election outcome. Panel B displays the results of estimating equation 5 using vote share and win margin as dependent variables.[27] As in the case of re-election rates, we do not find any evidence that vote share (see columns (1) and (2) of panel B) or margin of victory (see columns (4)-(6) of panel B) differed systematically between municipalities audited before and after the elections.[28]

In panel C, we present estimates of the regression model presented in equation 6. This model exploits additional information on electoral outcomes from the 2000 elections to control for any unobserved time invariant characteristics of the mayor and municipality, as well as any state-specific shock to the change in voting sentiment between 2000 and 2004. Even with this alternate model, we find only minimal evidence that the audit policy affected the change in vote share or margin of victory from the 2000 to 2004 elections. The change in vote share is 3.2 percentage points lower in municipalities audited prior to the elections (column 2 of Panel C), and statistically

[25] Only 60 percent of the all Brazil mayors were eligible for re-election in 2004. The remaining 40 percent, which had been elected to a second term in 2000, were not eligible for re-election under the Brazilian constitution which limits member of the executive branch to two consecutive terms. See Ferraz and Finan (2005) for a description of term limits in Brazil and its potential effects on corruption.

[26] Also note that the sample has been restricted to the non-missing observations of the various control variables, so as to keep its size constant across specifications.

[27] If defeated in the 2004 elections, the margin of victory for the incumbent is negative.

[28] This finding is robust to the use of alternative measures of political competition such as the effective number of candidates, and different sample specifications.

significant at 90 percent confidence. Even though this estimate implies a 52 percent decline from a baseline of -.057, overall the results are far from convincing. In effect, the results presented in table 2 show that the audits and its release of information on corruption levels had a minimal effect on subsequent electoral outcomes.

The lack of evidence documenting an average effect of the anti-corruption policy on electoral outcomes is to some extent expected. According to the theory, for the audits to negatively (or positively) affect re-election outcomes voters would have had to systematically underestimate (or overestimate) the incumbent's corruption level. But how the information from the audits compares to voters' priors is likely to depend on the information disclosed. Audits that do not reveal corruption may produce positive effects on electoral outcomes that on average cancel out the negative effects induced by audits revealing extreme corruption. The simple comparison between municipalities audited pre and post election ignores not only this possibility, but also the potential effect media has in disseminating the information. Because the program was designed to release the audit results to the public using the media, the release of information on corruption may not have reached as many voters in municipalities where local media sources are not available. To account for these program characteristics, we can exploit the fact that we observe the corruption level of audited municipalities to test for a differential effect of the program that captures both the role of media and the level of corruption revealed. Because of the random release of the audit reports, causal inference can still be made conditional on the municipality's corruption level.

5.2 Testing for Differential Effects by Corruption Levels

This section investigates whether the policy's effect varies according to the extent of corruption found. To do so, we compare the electoral performance of incumbent mayors of municipalities that were audited prior to the election to those audited after the election. Because each municipality has been audited, we can construct objective measures of corruption for each of these municipalities, and then exploit the random timing of the audits to estimate how the effect of the policy varies according to the level of corruption revealed. Based on both graphical and regression analysis, we find that the disclosure of a report with corruption did have a significant negative impact on the incumbent's performance in the 2004 mayor elections.

Graphical Analysis

To get an understanding for how the dissemination of information about corruption might affect an incumbent's electoral performance, Figure 3 illustrates the relationship between corruption and re-election rates. The figure plots the proportion of eligible mayors re-elected in the 2004 elections against the level of corruption discovered in the audit, distinguishing between municipalities that were audited prior to the election (represented by a triangle) and municipalities that were audited

after the election (represented by a circle).[29]

Municipalities that were audited and had their findings disseminated prior to the municipal elections exhibit a striking downward yet nonlinear relationship between re-election rates and corruption. Among the municipalities where not a single violation of corruption was discovered, 53.2 percent of the incumbents eligible for re-election were re-elected. Re-election rates decrease sharply as the number of corrupt irregularities discovered approaches three, which is almost double the sample average of corrupt violations found. In contrast to the municipalities where corruption was not discovered, re-election rates were about 20 percent among municipalities where auditors reported three corrupt violations. For municipalities with four or more violations, re-election rates increase slightly, but still remain low at less than 30 percent (10 percentage point below the sample average). In general, the relationship suggests that voters do care about corruption, and hold corrupt politicians accountable.

The sharply negative association between re-election rates and corruption among municipalities that experienced a pre-election audit lies in stark contrast to the relationship depicted for municipalities that underwent a post-election audit. With the exception of a couple of points, re-election rates remain steady across corruption levels at close to the population average of 40 percent. The comparison of these two relationships provides interesting insights into both the effects of the policy and also voters' initial priors. At corruption levels of less than one (which is the sample median), voters' prior beliefs appear to have overestimated the incumbent's corruption level, as the audits may have increased an incumbent's likelihood of re-election. Beyond this crossover point, politicians are punished as voters have systematically underestimated their corruption levels. This graph provides a first indication that the audit policy may have not only impacted the incumbent's likelihood of re-election but that this impact depends on the severity of the corruption reported.

Although not reported, we also repeat the graphical analysis for other measures of electoral performance[30]: re-election rates for the sample of incumbent mayors that reran for re-election, the change in the incumbent vote shares from 2000 to 2004, and the mayor's change in win margins from the 2000 to 2004 elections. Each figure demonstrates relationships consistent with those depicted in Figure 3. Independent of the measure of electoral performance, municipalities audited prior to the elections generally display a negatively sloped association between electoral performance and corruption that is not present among the municipalities that were randomly audited post-election.

In effect, Figure 3 presents suggestive evidence that the dissemination of information about corruption did lead to political accountability. The average voter appeared to have had the prior belief that the incumbent committed approximately one corrupt violation. Consequently, incumbents with less than one corruption violation were rewarded, and those with more were punished. The

[29]Because municipalities with 5 or more incidences of corruption represent only 3 percent of the sample, we group together, for the sake of clarity, municipalities where at least four incidences of corruption were uncovered. With this regrouping, each level of corruption contains approximately 20 percent of the sample.

[30]Available upon request.

figure also demonstrates two important patterns that deserve econometric considerations. First, the effect of the policy is a function of the information revealed, and second, voters may not necessarily respond to the announced corruption in a linear fashion.

Regression Analysis

Table 3 provides a basic quantification of the relationship depicted in Figure 3. The estimation results are from a series of models based on equation 7, where the dependent variable is an indicator for whether the incumbent was re-elected in the 2004 elections. As in the previous table, the specifications presented in odd-numbered columns control for state fixed-effects, but exclude any other control variables; whereas, the specifications in the even-numbered columns control for an additional 20 municipal and mayor characteristics.

The models in Columns 1 and 2 assume a linear relationship between re-election rates and corruption, but allow this relationship to differ between municipalities audited before and after the elections. In these specifications, the point estimates suggest that the audits had a differential impact of -3.6 percentage points. However, despite the fact that these estimates represent a 9 percent decline in re-election rates, they are not statistically significant at conventional levels. While it is possible that the audit policy did not illicit electoral retribution, the patterns presented in Figure 3 suggest that a linear regression model might be misspecified.

The models in Columns 3 and 4 present alternative specifications that include quadratic terms for corruption and the interaction terms. As opposed to the linear specifications, these models assume a quadratic relationship between the probability of re-election and corruption, and in so doing allow for the up-tick in re-election rates at the higher levels of corruption.[31] The estimates reported in these columns suggest that the quadratic terms do have some predictive power (F-test= 2.58; P-value=0.08 on the quadratic terms) and improve the models' overall fit. In these specifications, the dissemination of the audit reports revealing extensive corruption had a negative and statistically significant impact on the incumbent's likelihood of re-election. Among the municipalities where only one corruption violation was discovered, which is approximately the intersection point in Figure 3, the dissemination of this information reduced re-election rates by only 4.6 percentage points (F(1,348)=0.57; P-value=0.45). In contrast, the audit policy reduced re-election rates by 17.7 percentage points (F(1,348)=4.93; P-value=0.03) in municipalities where 3 corrupt violations were reported. At sample means, a one standard deviation increase in reported corruption, the likelihood of re-election is reduced by 12.2 percentage, or 30.2 percent.

Is the relationship between re-election rates and corruption levels u-shaped or does this just reflect noise in the data? In columns 5 and 6, we fit the linear model presented in the first two

[31] Ideally, we would like estimate a nonparametric model, however our sample size does permit us to identify the impact of the audit policy at each level of corruption. The estimated impacts at each corruption level (and their standard errors) are the following: 0.089 [0.154] at $corruption = 0$; -0.142 [0.152] at $corruption = 2$; -0.207 [0.185] at $corruption = 3$; -0.09 [0.172] at $corruption \geq 4$.

columns to the subset of municipalities that had no more than 5 corrupt violations, thus excluding 11 observations (5 from treatment and 6 from control). These observations represent not only less than 3 percent of the sample, but corruption levels that are almost 3 standard deviations away from the mean. With the removal of these outliers, the point estimates increase substantially to almost double the original estimates, and become statistically significant at the 10 percent level. The estimate on the interaction term is -0.071 (standard error 0.041; see column 5), implying that for every additional corrupt violation reported, the release of the audits reduced the incumbent's likelihood of re-election by 17 percent. If we restrict the sample further, excluding municipalities with more than 5 corrupt violations - less than 6 percent of sample - the point estimate on the interaction increases even more to -0.090 (standard error = 0.042; see column 7).

The remaining rows of columns 5-8 contain the estimated counterfactual relationship between re-election rates and corruption. The estimates, which are close to zero and statistically insignificant, expectedly reflect the fact that voters are uninformed about their mayor's corruption activities before voting at the polls. Moreover, comparing the estimates in column 1 to those in column 5, we see that including these 6 highly corrupt mayors in the sample creates a negative relationship between re-election rates and corruption in control municipalities. In sum, with such few observations and the absence of a well-defined relationship in the control municipalities, it appears that the lack of a statistically significant effect reported in columns 1 and 2 is mostly due to noise.

The Effect of the Audit Policy on Other Measures of Electoral Outcomes

Table 4 presents a series of the models that are similar to those reported in Table 3, but estimate the effects of the policy on other measures of electoral performance. These other electoral outcomes by construction limit the analysis - and thus inference - to the select group of mayors that ran for re-election.[32]

Columns (1)-(3) of Panel A present three different models estimating the impact of the policy on the probability of re-election for this selected sample of politicians. The linear model estimated with a full set of controls is shown in Column 1, the quadratic model is displayed in Column 2, and Column 3 reports the linear model but estimated among the municipalities with no more than 5 corrupt violations. Columns (4)-(6) of Panel A re-estimate these models using the incumbent's margin of victory as the dependent variable. Panel B, which is formatted similarly, presents the effects of the policy on the incumbent's vote share (columns (1)-(3)) and change in vote share (columns (4)-(6)).

Overall the coefficients of the key variables reported in Table 4 tell a similar story. Disclosing corruption had a negative effect on re-election rates, even among this select group of incumbents. For an additional corrupt violation, the audits reduced the likelihood of re-election by 14 percent

[32]Interestingly, we find no evidence that the audit policy reduced the probability that the mayor would run for re-election. See the table in the Appendix.

(see columns (1)-(3) of Panel A) from the baseline means. When we estimate the effects of the audit policy on the other measures of electoral performance, the results remain consistent and statistically significant.[33] For example, the estimates in Column 4 of Panel A imply that reporting an additional corrupt violation reduced the incumbent's margin of victory by 3.4 percentage points among municipalities that were audited prior to the elections relative to those that were audited afterwards.

Additional Specification Checks

Political manipulations

The credibility of our research design stems from the fact that municipalities were audited randomly together with the timing of the municipal elections. Even though it is unlikely that the selection of municipalities was manipulated, one potential concern could lie in the actual audit process itself.[34] If the audits conducted before the elections differed systematically to those done after the elections, then our research design would be comprised.

The most obvious concern is if the auditors themselves were corrupt. This would potentially cause systematic differences across the two groups because relative to mayors audited after the elections, those audited before the elections would have a higher incentive to bribe auditors for a more favorable report.[35] There are at least four reasons why this is unlikely to be the case. First, auditors are hired based on a highly competitive public examination, and are well-paid public employees. Moreover, each team of auditors - and there is typically one team per state - reports to a regional supervisor. Second, according to program officials there has never been incidences in which auditors have even been offered bribed.[36] Third, had there been any manipulations of the audit findings, it is unlikely that the corruption levels would have been balanced. But, as shown in Figure 1, the levels of corruption across the two groups were well-balanced not only on average but at each point of the distribution. Finally, the effects of the audit are identified using within-state variation. Given that there is typically one team per state, we control for any potential differences in the audit process across states.

If however the audits could be manipulated, then we might expect mayors that were politically affiliated with either the federal or state governments to receive more favorable audit reports, and particularly if audited before the elections. To test for this possibility, Column 1 of Table 5 reports a model that regresses the number of corrupt violations on whether or not the municipality was audited prior to the elections, whether the mayor is member of the governor's political party, party dummies, and a full set of interaction terms. From the results presented in Column 1, we

[33] Although not reported, the effects are also consistent when using the change in the margin of victory.

[34] Each random drawing was done jointly with the national lottery and witnessed by members of the media and government officials. As we also pointed out, the comparison of municipal and mayor characteristics does support the randomized selection.

[35] This argument of course assumes that mayors audited after the elections do not have further re-election incentives.

[36] Based on the interviews conducted by the authors with program officials in Brasilia.

do not find any evidence that mayors from the same political party as the state governor or the federal government received a differential audit (point estimate = -0.155, standard error = 0.256).[37] Moreover, there are no differential effects for any of the six major parties (P-value = 0.97).

Another possibility is if incumbents who won by narrow victories in previous election and thus have greater incentives to bribe the auditors, received differential reports. To test for this hypothesis, we extend the model presented in column (1) to control for the incumbent's margin of victory in the 2000 election and its interaction with whether the municipalities was audited prior to the elections. Again, we do not find any evidence that a mayor's level of political support influenced the audit process and in fact the point estimate,-0.638 (standard error =0.865), is the opposite sign.

The remaining columns of Table 5 provide further evidence of the robustness of our results. Columns 3-8 report the same set of models presented in Table 3, except that the models control for the various political variables and interaction terms seen in Columns 1 and 2. These specifications allow us to examine whether these differences in corruption levels- even if statistically insignificant - affect the estimated impact of the audit policy. However, as seen in the table, the estimates of the effects of the program by corruption are very similar to those presented in Table 3.

Placebo analysis

Table 6 presents another specification check of the research design. If the audit policy had an effect on the 2000 electoral outcomes, then it would suggest that unobserved characteristics of the municipality that determine the association between re-election rates and corruption are driving the results presented in Table 3. Columns (1)-(4) report the differential effect of the audit policy on the incumbent's vote share in the 2000 elections, and columns (5)-(8) report the differential effect on the incumbent's win margin in the 2000 elections.[38] The estimates presented in Table 6 suggest that the policy did not affect electoral outcomes in 2000. In each specification, the point estimates are close to zero and in some cases even slightly positive.

To summarize, re-election rates are lower among municipalities that were reported to have had higher level of corruption and were audited before the elections compared to municipalities with similar corruption levels but were audited only after the elections. These findings are robust to the inclusion of various municipal controls and are consistent using different measures of electoral performance. We also do not find any evidence that the audit process was manipulated, and particular in a political manner. Overall, these findings suggest that the disclosure of corruption practices as produced by the audit policy had a significant negative impact on the electoral performance of incumbent mayors found to be corrupt.

[37]The interaction between the Worker's party (PT) and pre-election audit controls for whether the mayor is in the same political party as the federal government.

[38]Note that we cannot use re-election rates since 2000-2004 represents their first term. Ideally, we would also like to regress electoral outcomes in 2000 against 1996-2000 corruption levels. For our test to have much power, we are implicitly assuming that there is a monotonic relationship between corruption levels in 1996 and 2000.

5.3 Testing for Differential Effects of the Audits by Corruption Levels and Media Availability

Thus far, we have demonstrated that the audit policy had a negative effect on the re-election success of the mayors that were found to be corrupt. This reduced-form effect of policy, while well identified from a randomized design, does not reveal the underlying mechanisms through which the policy operated. The audits may have induced political parties to change their candidates, or forced the incumbent mayor to run a cleaner or less clientelistic campaign. While it is impossible to definitively reject these competing hypotheses, in this section, we provide strong evidence consistent with our simple story about information. The audit policy provided new information about the mayor's corruption practices that for many voters differed from their initial priors about the their mayor's corruptness. Mayors that were found to be extremely corrupt were punished at the polls.

5.3.1 The Role of Media

Table 7 presents the estimation results from a variety of specifications based on the regression model defined in equation 8. These specifications test whether the audit policy had a differential effect by both the level of corruption reported and the presence of local media, where our measure of local media is the number of AM radio stations in the municipality. Radio is the single most important source of information for local politics, and local programming is only broadcasted on AM stations. With the exception of the additional interaction terms, the columns correspond to the same set of specifications presented in Table 3.

The first set of rows shows how the effects of the audits varies by both the level of corruption reported in the audit and the number of radio stations in the municipality. The estimated effect is significant at conventional levels and suggests that the effects of audits were much more pronounced in a municipalities that have both higher levels of reported corruption and more radio stations.[39]

From the specification in Column 1, which excludes additional control variables but does include state intercepts, the audit policy decreased the likelihood of re-election by -17.8 percentage points (F(1, 345)= 3.72, P-value=0.05), among municipalities with a radio station and where the audits reported 3 corrupt violations. Although radio exacerbates the audit effect when corruption is revealed, it helps to promote non-corrupt incumbents. When corruption was not found in a municipality with local radio, the audit actually increased the likelihood that the mayor was re-elected by 18 percentage points (column 1). Column 2 demonstrates that our estimates are robust to the inclusion of several municipal and mayor characteristics.[40]

[39] We find similar results when we use other measures of electoral performance and restrict the sample to mayors that ran for re-election.

[40] Although not reported, we investigate whether in addition to radio, local newspapers increased voters' awareness of the audit findings. The policy did not have a differential effect by the number of local newspapers in the municipality. Given Brazil's generally low circulation rates and low literacy (particularly in the interior municipalities), these results are not too surprising. Moreover, it emphasizes the importance of radio in conveying information to the

Columns (3)-(8) provide further evidence that radio played a significant and complementary role. The estimated effects of the audit are insensitive to both functional form and the exclusions of the corruption outliers. In column (3), the OLS estimates imply that at sample means, the audits reduced re-election rates by 52 percent among municipalities with a radio station. When the sample is restricted to municipalities with no more than 5 corrupt violations, the point estimate on the triple interaction term increases to -0.14 (standard error=0.064).

To get an even better sense for these estimates, Figure 4 plots the 2004 re-election rates among eligible mayors against the number of corrupt violations found in the audit, distinguishing the relationship for four groups of municipalities: those with and without local radio that were audited before and after the elections. For municipalities that were audited prior to the election but are without a local radio station (depicted by a circle), there is slight negative association between re-election rates and corruption, consistent with the effects of the audit. However, when compared to municipalities audited prior to the election and with local radio, we see clearly the significant role radio played in disseminating the audit information. Among these municipalities (depicted by a triangle), re-election rates fall drastically as the number of corruption violations increase. In fact, there exists a 47 percentage point difference in re-election rates between non-corrupt mayors and mayors with at least 4 corruption violations. In comparing these two relationships, we also observe the electoral advantage non-corrupt mayors of municipalities with local radio receive with an audit, as there exists a 24 percentage point difference in re-election rates between municipalities with and without local radio.

For municipalities audited post-election, there is little distinction by radio. Among these municipalities, the relationship between re-election rates and corruption is relatively flat, independent of the existence of radio. Only a level difference, consistent with an expected positive association between media and electoral competition, distinguishes these two groups of municipalities, as re-election rates tend to be higher in the municipalities audited post-election but without local radio.

Figure 4 also illustrates how the existence of radio influences voters' initial priors. Among municipalities with local radio, voters' exhibit the prior belief that incumbents on average commit one corrupt violation (as depicted in figure 3). As radio serves to disseminate the findings of the audit more broadly, non-corrupt politician are rewarded heavily by voters' overestimation of their corruption level. Conversely, beyond one corrupt violation, politicians are severely punished. For areas without radio, the crossover point is even lower, intersecting at zero violations. Thus, not only does the audit reduce the incumbent's likelihood of re-election independent of his corruption level, it also suggest that the public are more systematically wrong in their estimation of corruption when there exists less media.

Together these results illustrate how media can influence the selection of politicians, by exposing

general public in Brazilian society.

corrupt politicians and promoting good ones. This ability of media to help screen politicians is one of the main channels by which it can influence public policy (Besley, Burgess, and Pratt 2002).

Testing for compositional confounds

We have argued that the presence of local radio enable voters to further punish corrupt politicians once the anti-corruption program reveals the true extent of their corruption. Unfortunately however our experiment, while randomized over which municipalities were audited, was not randomized on the availability of media. As such, our measure of media could be serving as a proxy for other characteristics of the municipality that induce a differential effect of the audit reports on re-election outcomes. For example, if more available media is positively correlated with the literacy rate of the municipality, then our results may be capturing a differential effect by education levels rather than media per se. It would then cast doubt on our claim that local media has led to a more pronounced effect of this anti-corruption program.

To test for these potential confounds, we include in the estimation of equation 8 a series of triple interaction terms on a host of other characteristics that might be correlated with the number of radio stations in the municipality.[41] Table 8 presents the results from these specifications. For easy of comparison, column (1) reproduces the base specification of table 7. Columns (2)-(9) present a series of specifications that sequentially include different triple interactions of municipal characteristics. Our most general model includes, in addition to all the triple interaction characteristics, a set of municipal controls (column (9)). Across each specification, our estimate of the triple interaction between radio, corruption, and pre-election audit remains remarkably stable and statistically significant. This result holds with the inclusion of such measures as literacy rates, electoral competition, income and income inequality, as well as various other potential correlates of media availability. In effect, this table provide suggestive evidence that local radio is an important channel through which this anti-corruption program has increased political accountability.

Other Potential Mechanisms

The results thus far support a simple model. The audit policy provided new information to voters about their mayor's corruption practices. Voters used this information to update their priors and punish politicians that were found be more corrupt than average. The audit effects were in turn more pronounced in areas where the local media could disseminate these findings more widely.

However as mentioned above, the effects of the audit on re-election rates may have come about through channels other than information. For example, the audits may have also led the incumbent to alter his campaign strategies, or induced the opposition parties to run a cleaner candidate.

[41] For each triple interaction, we also include variable itself, the variable interacted with being audited prior to the elections, and the variable interacted with corruption.

Another possibility is that mayors that were revealed to be corrupt received less campaign contributions, which lowered their likelihood of re-election.[42] The most pertinent evidence against these interpretations is presented in Table 9, which explores how the timing of the audits affected re-election rates.

Panel A of Table 9 reports the estimated effects of the audit policy on re-election rates based on our quadratic specification. For easy of comparison, the model presented in Column 1 corresponds to the one presented in Column 4 of Table 3, which is estimated for the entire sample and with a the full set of mayor and municipal controls. Column 1 reports the estimates of same model but for a sample that excludes the first lottery. In each subsequent column, we continue to drop earlier lotteries until Column 7, where the treatment municipalities are those that were selected from the last 2 lotteries prior to the election. Panel B reports the same exercise using a linear specification and excluding municipalities that had more than 5 corrupt violations.

The estimates reported in Table 9 suggest that the audit policy did not have a differential effect based on when the municipality was audited. The effect of the policy on the municipalities that were audited just prior to the election (Column 7) was not statistically different from the average effect. Since political parties decide upon their candidate and receive campaign funds several months (if not years) before the elections, it appears unlikely that the audits induced such changes.

6 Conclusions

It is widely believed that improved voter information enhances the accountability of politicians. In this paper, we exploit a natural experiment provided by Brazil's audit program to provide empirical evidence on the importance of information in affecting electoral outcomes. In April of 2003, the Brazilian government began an ambitious anti-corruption program designed to audit a municipality's expenditure of federally-transferred funds. The federal government randomly audits 60 municipalities approximately every month, and then discloses the findings of the report to the municipality and the media. Using these reports to construct an objective measure of corruption, we exploit the program's random design to first, test whether given the level of reported corruption, the release of information about government corruption affected the incumbent's performance in the 2004 elections; and second, test if this differential effect is more pronounced in municipalities with local media.

We find that the dissemination of information on corruption, which is facilitated by media, does have a detrimental impact on the incumbent's electoral performance. In particular, for a one standard deviation increase in reported corruption at the sample median, the audit policy reduced the incumbent's likelihood of re-election by 25 percent. The effect of the program was much more

[42] Although not reported, we have estimated the effects of the audits on campaign contributions and do not find any effects. We are however, reluctant to put much faith on these particular results because the data are of poor quality and information is missing for over half of our sample.

pronounced in areas where local radio is available, indicating that media played a complementary role to the program. For a marginal increase in corruption and local media, the release of the audits decreased the probability of re-election by 49 percent. Moreover, while radio exacerbates the audit effects when corruption is revealed, it also helps to promote non-corrupt incumbents by drastically increasing the likelihood of their re-election.

Overall, our findings lend strong support for the value of information in promoting political accountability. How this information is consequently interpreted however, depends on voters' prior beliefs. Politicians that were revealed to be extremely corrupt were punished, while non-corrupt politicians were rewarded. These results also highlight the influence media have on political outcomes, and particularly in helping to screen out bad politicians and promoting good politicians. This finding is consistent with a growing literature that emphasizes the role of media in inducing government to be more accountable and responsive to voters.

Finally, while the program has had a significant impact on political accountability, whether it has led to the imprisonments of corrupt politicians or reduced municipal corruption is still unknown. Analyzing how the public dissemination of the audits affects these other outcomes is an area of ongoing research.

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A Coding the Audit Reports

This appendix explains how we used the audit reports to code the corruption and poor administration variables. We provide the definition used for each type of irregularity and include an illustrative example drawn from the reports.[43]

A.1 Examples of Political Corruption

Health related purchases without procurement using false receipts: The ministry of Health transferred to the municipality R$ 321,700 for the Programa de Atenção Bsica. The municipal government used fake receipts valued at R$ 166,000 to provide proof of purchase. Furthermore, there is no proof that the good were purchased since there were no registered entries of the merchandize in the stock. Also, in 2003 the municipality bought medicines valued at R$253,300 without procurement. In 2004, the value was R$113,700, also without procurement. We classified this violation as an incidence of irregular procurement and diversion of public funds in the area of health. We valued this irregularity as a diversion of R$166,000. This irregularity occurred in Capelinha, Minas Gerais, drawn by lottery number 9.

Evidence of irregularities in well construction: The Ministry of National Integration transferred R$117,037 to the municipality for the maintenance of water infra-structure. The working plan specified the maintenance of ten wells and four dams. None of these repairs were made. Instead, the dam Henrique Dantas, located inside a private farm was repaired. We classified this violation as an incidence of diversion of public funds in the area of water and sanitation. We valued this irregularity as a diversion of R$117,037. This irregularity occurred in Santa Cruz, Rio Grande do Norte, drawn by lottery number 9.

Over-invoice of more than R$3 million in road construction: The firm Mazda was hired, without procurement, to build approximatly nine kilometers of a road. The cost of the construction was estimated at R$ 1 million based on similar constructions. The receipts presented by Mazda and paid by the government totalled R$ 5 million. No further documentation was shown by the municipal government proving the need for the additional amount of resources. The auditors found that the firm Mazda, did not have any experience with construction and had sub-contracted the firm CTE for R$ 1.8 million to do the construction. Hence, the project was over paid by more than R$ 3 million. As evidence of corruption, it was late found that the firm Mazda gave an apartment for the mayor and his family valued at R$600,000 suggesting a kickback. We classified this violation as an incidence of over-invoice in the area of infrastructure. We valued this irregularity as a diversion of R$3.2 million. This irregularity occurred in São Francisco do Conde, Bahia, drawn by lottery number 6.

[43] For access to the summary of the audit reports, see www.presidencia.gov.br/cgu.

A.2 Examples of Bad Administration

Unfinished construction of sanitation system: The ministry of health transferred to the municipality R$ 2 million, for the construction of a sanitation system. The auditors analyzed the documentation and visited the construction site. They saw that the construction was stopped and abandoned, although the construction did not reach even 30% of the original project. This irregularity occurred in Rorainópolis, Roraima, drawn in lottery 7.

Municipal councils do not execute their activities: the municipal councils of Fundef and School Lunches (Conselho de Alimentao Escolar- CAE) do not execute their activities. This implies that the application of the federal resources to both programs are not accompanied and inspected by the respective municipal council. This irregularity occurred in Malhada das Pedras, Bahia, drawn in lottery 5.

The family health program is not functioning properly: According to interviews with patients, the auditors found that the medical team was not visiting families regularly. Based on the information from the interviews, the auditors concluded that the number of visits is not compatible with what was registered in the program's database (Sistema de Identificao de Ateno Bsica (SIAB). The auditors emphasized that the municipality only provided one doctor to attend the entire population. This irregularity occurred Viosa, RN, drawn in lottery 4.

	Post-election audit (1)	Pre-election audit (2)	Difference (3)	Standard error (4)
Panel A: Political characteristics				
Re-election rates for the 2004 elections	0.413	0.395	0.018	0.045
Re-election rates for the 2000 elections	0.423	0.443	-0.020	0.040
2004 re-election rates, among those that ran	0.585	0.559	0.026	0.044
Ran for re-election in 2004	0.707	0.707	-0.001	0.060
Number of parties in 2000	2.881	2.933	-0.052	0.140
Margin of victory in 2000	0.142	0.131	0.012	0.019
Mayor's vote share in 2000	0.529	0.525	0.004	0.013
Panel B:Mayor characteristics:				
Age	47.5	48.0	-0.5	0.9
Years of education	12.2	12.0	0.3	0.3
Male	0.96	0.94	0.02	0.03
Member of PSB	0.083	0.072	0.011	0.044
Member of PT	0.030	0.048	-0.018	0.023
Member of PMB	0.254	0.172	0.082	0.047
Member of PFL	0.178	0.163	0.015	0.052
Member of PPB	0.030	0.038	-0.009	0.017
Member of PSDB	0.130	0.167	-0.037	0.043
Panel C: Municipal characteristics:				
Population density (Persons/km)	0.57	0.73	-0.16	0.33
Literacy rate (%)	0.81	0.80	0.01	0.03
Urban (%)	0.62	0.62	0.00	0.05
Log per capita income	4.72	4.66	0.06	0.15
Income inequality	0.55	0.54	0.00	0.01
Zoning laws	0.29	0.21	0.08	0.07
Economic Incentives	0.66	0.58	0.07	0.06
Paved roads	58.99	58.30	0.69	7.74
Size of public employment	42.45	42.76	-0.32	1.53
Municipal guards	0.20	0.21	-0.01	0.07
Small claims court	0.38	0.34	0.04	0.08
Judiciary district	0.59	0.56	0.03	0.07
Number of Newspapers	3.58	2.21	1.37	0.79
Municipalities with a radio stations	0.31	0.24	0.07	0.06
Number of radio stations, conditional on having one	1.37	1.29	0.08	0.11
Number of corrupt violations	1.952	1.584	0.369	0.357
Total resources audited ($R)	5,770,189	5,270,001	500,188	1,361,431

Notes: Re-election rates for the 2000 elections include by definition mayors that are in their second and final term The averages of the remaining characteristics are computed for the 372 mayors that were eligible for re-election in the 2004 elections and were audited.

Table 1: Differences in mayor and municipal characteristics between pre- and post-election audits

	(1)	(2)	(3)	(4)
Panel A.	Dependent variable: *Pr(reelection)*			
	All incumbent mayors		Only incumbent mayors that ran for reelection	
Preelection Audit (1/0)	-0.036	-0.036	-0.058	-0.059
	[0.053]	[0.052]	[0.066]	[0.065]
Observations	372	372	263	263
R-squared	0.05	0.17	0.07	0.22
Panel B.	Dependent Variable: Vote share		Dependent Variable: Win margin	
Preelection Audit (1/0)	-0.043	-0.055	-0.008	-0.02
	[0.072]	[0.072]	[0.026]	[0.027]
Observations	262	262	262	262
R-squared	0.04	0.16	0.07	0.22
Panel C.	Dependent Variable: Change in vote share		Dependent Variable: Change in win margin	
Preelection Audit (1/0)	-0.023	-0.032	-0.02	-0.028
	[0.018]	[0.018]+	[0.027]	[0.027]
Observations	262	262	262	262
R-squared	0.31	0.39	0.21	0.31
State fixed effects	Yes	Yes	Yes	Yes
Municipal characteristics	No	Yes	No	Yes
Mayor characteristics	No	Yes	No	Yes

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 2: Effects of the audits on electoral outcomes

Dependent variable: *Pr(re-election)*	Full sample				Corruption ≤ 5		Corruption ≤ 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preelection audit	0.029	0.030	0.128	0.126	0.067	0.068	0.083	0.086
	[0.083]	[0.082]	[0.100]	[0.101]	[0.087]	[0.087]	[0.088]	[0.088]
Preelection audit × Number of corrupt violations	-0.038	-0.038	-0.211	-0.200	-0.071	-0.070	-0.090	-0.088
	[0.035]	[0.035]	[0.091]*	[0.090]*	[0.041]+	[0.041]+	[0.042]*	[0.043]*
Preelection audit × Number of corrupt violations²			0.036	0.034				
			[0.017]*	[0.017]*				
Number of corrupt violations	-0.013	-0.012	0.035	0.037	0.008	0.012	0.000	0.003
	[0.026]	[0.027]	[0.066]	[0.066]	[0.031]	[0.033]	[0.035]	[0.036]
Number of corrupt violations²			-0.009	-0.009				
			[0.011]	[0.011]				
Observations	373	373	373	373	362	362	351	351
R-squared	0.05	0.18	0.07	0.19	0.06	0.19	0.08	0.2
F-test (p-values)			0.068	0.089				
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Mayor characteristics	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 3: Effects of the audits on re-election rates by corruption levels

	Full sample		Corruption ≤ 5	Full sample		Corruption ≤ 5
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variables:	Pr(re-election)			Margin of victory		
Preelection audit	0.045	0.153	0.072	0.037	0.098	0.053
	[0.095]	[0.115]	[0.099]	[0.037]	[0.042]*	[0.039]
Preelection audit × Number of corrupt violations	-0.06	-0.256	-0.086	-0.034	-0.147	-0.049
	[0.039]	[0.104]*	[0.046]+	[0.015]*	[0.040]**	[0.019]**
Preelection audit × Number of corrupt violations²		0.041			0.024	
		[0.019]*			[0.007]**	
Number of corrupt violations	-0.016	0.045	0.001	0.011	0.041	0.019
	[0.030]	[0.076]	[0.036]	[0.012]	[0.029]	[0.014]
Number of corrupt violations²		-0.011			-0.005	
		[0.012]			[0.005]	
Observations	264	264	256	264	264	256
R-squared	0.24	0.25	0.24	0.18	0.22	0.2
F-test (p-values)		0.03			0.00	
Panel B. Dependent variables:	Vote share			Change in vote share		
Preelection audit	0.078	0.209	0.104	-0.014	0.035	0.006
	[0.102]	[0.121]+	[0.106]	[0.027]	[0.030]	[0.027]
Preelection audit × Number of corrupt violations	-0.078	-0.315	-0.104	-0.01	-0.098	-0.029
	[0.041]+	[0.109]**	[0.048]*	[0.012]	[0.031]**	[0.013]*
Preelection audit × Number of corrupt violations²		0.049			0.018	
		[0.021]*			[0.006]**	
Number of corrupt violations	-0.002	0.069	0.014	-0.001	0.026	0.01
	[0.032]	[0.080]	[0.039]	[0.010]	[0.023]	[0.010]
Number of corrupt violations²		-0.013			-0.005	
		[0.013]			[0.005]	
Observations	264	264	256	264	264	256
R-squared	0.24	0.26	0.24	0.4	0.43	0.42
F-test (p-values)		0.01			0.00	
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Mayor characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 4: Effects of the audits on other electoral outcomes by corruption levels

	Full sample				Corruption ≤ 5		Corruption ≤ 4	
Dependent variable:	Number of corrupt violations				Pr(re-election)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preelection audit	-0.332	-0.231	0.067	0.079	0.043	0.096	0.056	0.111
	[0.261]	[0.298]	[0.121]	[0.132]	[0.110]	[0.125]	[0.115]	[0.129]
Preelection audit × Number of corrupt violations			-0.208	-0.180	-0.076	-0.071	-0.094	-0.088
			[0.091]*	[0.090]*	[0.040]+	[0.039]+	[0.043]*	[0.041]*
Preelection audit × Number of corrupt violations²			0.035	0.031				
			[0.017]*	[0.017]+				
Preelection audit × Member of the governor's coalition	-0.155	-0.155	0.056	0.055	0.06	0.059	0.1	0.103
	[0.256]	[0.388]	[0.134]	[0.132]	[0.136]	[0.134]	[0.140]	[0.138]
Preelection audit × Margin of victory in 2000 elections		-0.638		-0.089		-0.198		-0.22
		[0.868]		[0.311]		[0.316]		[0.315]
Preelection audit × PT	-0.004	-0.034	0.269	0.299	0.28	0.3	0.186	0.208
	[0.861]	[0.864]	[0.286]	[0.278]	[0.290]	[0.278]	[0.280]	[0.267]
Preelection audit × PMB	0.157	0.132	0.19	0.141	0.145	0.073	0.106	0.033
	[0.389]	[0.398]	[0.130]	[0.128]	[0.134]	[0.130]	[0.136]	[0.134]
Preelection audit × PFL	0.064	0.052	-0.003	-0.014	-0.082	-0.101	-0.015	-0.033
	[0.445]	[0.455]	[0.153]	[0.147]	[0.157]	[0.149]	[0.160]	[0.151]
Preelection audit × PSDB	-0.456	-0.471	-0.282	-0.252	-0.483	-0.533	-0.515	-0.566
	[0.989]	[0.978]	[0.262]	[0.295]	[0.244]*	[0.241]*	[0.249]*	[0.248]*
Preelection audit × PSB	0.093	0.073	-0.325	-0.435	-0.322	-0.46	-0.285	-0.422
	[0.628]	[0.637]	[0.262]	[0.253]+	[0.262]	[0.253]+	[0.264]	[0.255]+
Preelection audit × PTB	-0.549	-0.562	0.324	0.272	0.295	0.232	0.274	0.216
	[0.591]	[0.594]	[0.207]	[0.221]	[0.212]	[0.227]	[0.216]	[0.231]
Observations	373	373	373	373	362	362	351	351
R-squared	0.35	0.35	0.19	0.28	0.21	0.27	0.22	0.28
F-test of the additional interaction terms (P-value)	0.97	0.97	0.20	0.39	0.09	0.08	0.15	0.13

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. All regressions include mayor and municipal characteristics, in addition to state intercepts. For each interaction term, the regression also controls for the direct effect. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies. There are mayors from 18 political parties, the six party indicators listed in the regression represent the major ones in the Brazil, and account for 70 percent of the mayors.

Table 5: Testing for manipulation of the auditing process

Dependent variable:	Vote share in 2000				Margin of victory in 2000			
	Full Sample		Corruption ≤ 5	Corruption ≤ 4	Full Sample		Corruption ≤ 5	Corruption ≤ 4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preelection audit	-0.001	0.007	0.000	0.001	-0.011	-0.003	-0.012	-0.011
	[0.014]	[0.016]	[0.014]	[0.015]	[0.022]	[0.027]	[0.023]	[0.024]
Preelection audit × Number of corrupt violations	-0.003	-0.015	-0.003	-0.004	0.000	-0.014	0.001	0
	[0.006]	[0.015]	[0.006]	[0.007]	[0.010]	[0.024]	[0.010]	[0.012]
Preelection audit × Number of corrupt violations²		0.002				0.003		
		[0.003]				[0.005]		
Number of corrupt violations	0.003	0.009	0.005	0.005	-0.001	0.005	0.001	0.002
	[0.005]	[0.011]	[0.005]	[0.006]	[0.007]	[0.018]	[0.008]	[0.009]
Number of corrupt violations²		-0.001				-0.001		
		[0.002]				[0.003]		
Observations								
R-squared								
F-test (p-values)								
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mayor characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 6: A placebo analysis on the effects of the audits on 2000 election outcomes by corruption

Dependent variable: *Pr(re-election)*	Full sample				Corruption ≤ 5		Corruption ≤ 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preelection audit	-0.046	-0.059	0.041	0.003	-0.011	-0.033	0.000	-0.017
	[0.091]	[0.091]	[0.112]	[0.114]	[0.096]	[0.096]	[0.098]	[0.098]
Preelection audit × Number of radio stations	0.188	0.229	0.291	0.402	0.216	0.271	0.255	0.312
	[0.097]+	[0.099]*	[0.138]*	[0.145]**	[0.103]*	[0.104]**	[0.100]*	[0.105]**
Preelection audit × Corrupt violations × Radio stations	-0.107	-0.118	-0.271	-0.367	-0.140	-0.157	-0.198	-0.230
	[0.045]*	[0.045]**	[0.140]+	[0.145]*	[0.064]*	[0.067]*	[0.053]**	[0.057]**
Preelection audit × Corrupt violations² × Radio stations			0.030	0.046				
			[0.026]	[0.027]+				
Preelection audit × Number of corrupt violations	0.001	0.007	-0.155	-0.106	-0.031	-0.018	-0.043	-0.028
	[0.038]	[0.038]	[0.104]	[0.103]	[0.044]	[0.044]	[0.046]	[0.046]
Preelection audit × Number of corrupt violations²			0.033	0.025				
			[0.019]+	[0.019]				
Number of corrupt violations	-0.032	-0.034	0.030	-0.004	-0.007	-0.013	-0.018	-0.022
	[0.028]	[0.029]	[0.075]	[0.077]	[0.034]	[0.035]	[0.037]	[0.038]
Number of corrupt violations²			-0.012	-0.005				
			[0.012]	[0.012]				
Number of radio stations	-0.127	-0.131	-0.160	-0.237	-0.133	-0.150	-0.140	-0.151
	[0.054]*	[0.064]*	[0.104]	[0.115]*	[0.053]*	[0.063]*	[0.052]**	[0.063]*
Number of corrupt violations × Number of radio stations	0.045	0.050	0.073	0.158	0.045	0.058	0.052	0.064
	[0.021]*	[0.026]+	[0.108]	[0.113]	[0.021]*	[0.025]*	[0.020]*	[0.024]**
Number of corrupt violations² × Number of radio stations			-0.005	-0.020				
			[0.020]	[0.021]				
Observations	373	373	373	373	362	362	351	351
R-squared	0.07	0.20	0.10	0.22	0.08	0.21	0.11	0.24
F-test (p-values)								
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Mayor characteristics	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 7: Effects of the audits on re-election rates by corruption levels and radio

Dependent Variable:	*Pr(Reelection)*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Preelection audit	-0.046	0.219	0.15	0.607	-0.427	-0.215	-0.123	-0.33	-0.293
	[0.091]	[0.252]	[0.255]	[0.754]	[0.938]	[1.165]	[1.216]	[1.224]	[1.223]
Preelection audit × Number of corrupt violations	0.001	-0.196	-0.168	-0.190	0.090	-0.085	-0.122	-0.134	-0.202
	[0.038]	[0.107]+	[0.108]	[0.287]	[0.367]	[0.415]	[0.429]	[0.414]	[0.416]
Preelection audit × Number of radio stations	0.188	0.233	0.256	0.274	0.254	0.253	0.284	0.295	0.309
	[0.097]+	[0.105]*	[0.108]*	[0.110]*	[0.112]*	[0.113]*	[0.117]*	[0.119]*	[0.121]*
Preelection audit × Corrupt violations × Radio stations	-0.107	-0.152	-0.156	-0.163	-0.155	-0.152	-0.162	-0.182	-0.190
	[0.045]*	[0.048]**	[0.048]**	[0.049]**	[0.050]**	[0.051]**	[0.053]**	[0.056]**	[0.057]**
Number of corrupt violations	-0.032	0.027	0.011	-0.075	-0.044	-0.054	-0.007	-0.079	-0.084
	[0.028]	[0.086]	[0.088]	[0.228]	[0.231]	[0.231]	[0.256]	[0.251]	[0.257]
Number of radio stations	-0.127	-0.145	-0.179	-0.181	-0.176	-0.17	-0.166	-0.144	-0.167
	[0.054]*	[0.055]**	[0.061]**	[0.061]**	[0.062]**	[0.063]**	[0.067]*	[0.069]*	[0.073]*
Number of corrupt violations × Number of radio stations	0.045	0.055	0.063	0.066	0.064	0.062	0.064	0.050	0.056
	[0.021]*	[0.020]**	[0.021]**	[0.021]**	[0.021]**	[0.021]**	[0.022]**	[0.022]*	[0.024]*
Triple interactions terms:									
Urban	N	Y	Y	Y	Y	Y	Y	Y	Y
Population density (Population/Area)	N	N	Y	Y	Y	Y	Y	Y	Y
Literacy rate	N	N	N	Y	Y	Y	Y	Y	Y
Per capita income	N	N	N	N	Y	Y	Y	Y	Y
Income inequality	N	N	N	N	N	Y	Y	Y	Y
Judiciary district	N	N	N	N	N	N	Y	Y	Y
Electoral Competition	N	N	N	N	N	N	N	Y	Y
Municipal Characteristics	N	N	N	N	N	N	N	N	Y
State Intercepts	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	373	373	373	373	373	373	373	373	373
R-squared	0.07	0.08	0.09	0.1	0.12	0.12	0.13	0.15	0.16

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. Each regression controls for the number of corrupt violation, the square of the number of corruption violations, the number of radio stations, the number radio stations interacted with the number of corrupt violations, and the number of radio stations interacted with the square of the number of corrupt violations. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, , municipal police (1/0), small claims court (1/0), judiciary district (1/0), number of daily newspapers, number of AM radio stations. Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 8: Testing for compositional confounds

Dependent variable: Pr(re-election)	Full Sample	Sep-03 - Jun-05	Oct-03 - Jun-05	Dec-03 - Jun-05	Feb-04 - Jun-05	Aug-04 - Jun-05	Sep-04 - Jun-05
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Full sample							
Preelection audit	0.126	0.113	0.166	0.176	0.194	0.107	0.143
	[0.101]	[0.104]	[0.105]	[0.109]	[0.110]+	[0.126]	[0.138]
Preelection audit × Number of corrupt violations	-0.200	-0.180	-0.202	-0.212	-0.211	-0.144	-0.195
	[0.090]*	[0.095]+	[0.096]*	[0.098]*	[0.103]*	[0.115]	[0.125]
Preelection audit × Number of corrupt violations²	0.034	0.029	0.029	0.030	0.031	0.019	0.027
	[0.017]*	[0.018]	[0.018]	[0.019]	[0.019]	[0.022]	[0.023]
Number of corrupt violations	0.037	0.034	0.026	0.037	0.040	0.025	0.038
	[0.066]	[0.067]	[0.068]	[0.068]	[0.068]	[0.067]	[0.067]
Number of corrupt violations²	-0.009	-0.009	-0.008	-0.009	-0.009	-0.008	-0.008
	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
Observations	373	358	335	310	284	255	228
F-test (p-values)	0.19	0.18	0.2	0.21	0.22	0.22	0.24
Panel B: Corruption ≤ 4							
Preelection audit	0.086	0.077	0.138	0.154	0.176	0.112	0.146
	[0.125]	[0.127]	[0.121]	[0.127]	[0.123]	[0.131]	[0.122]
Preelection audit × Number of corrupt violations	-0.088	-0.083	-0.113	-0.124	-0.126	-0.096	-0.122
	[0.050]+	[0.049]	[0.044]*	[0.047]*	[0.049]*	[0.050]+	[0.052]*
Number of corrupt violations	0.003	-0.002	-0.005	0.002	0.002	-0.003	0.008
	[0.049]	[0.053]	[0.050]	[0.049]	[0.051]	[0.053]	[0.054]
Observations	351	337	314	289	264	238	211

Notes: Robust standard errors in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) percent confidence. All regression include municipal, mayor and state fixed-effects. Municipal characteristics include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in the urban sector, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0). Mayor characteristics include: gender (1/0 for male), age, married (1/0), education level. party dummies.

Table 9: Effect of the audits on re-election rates by corruption, and timing of the lotteries

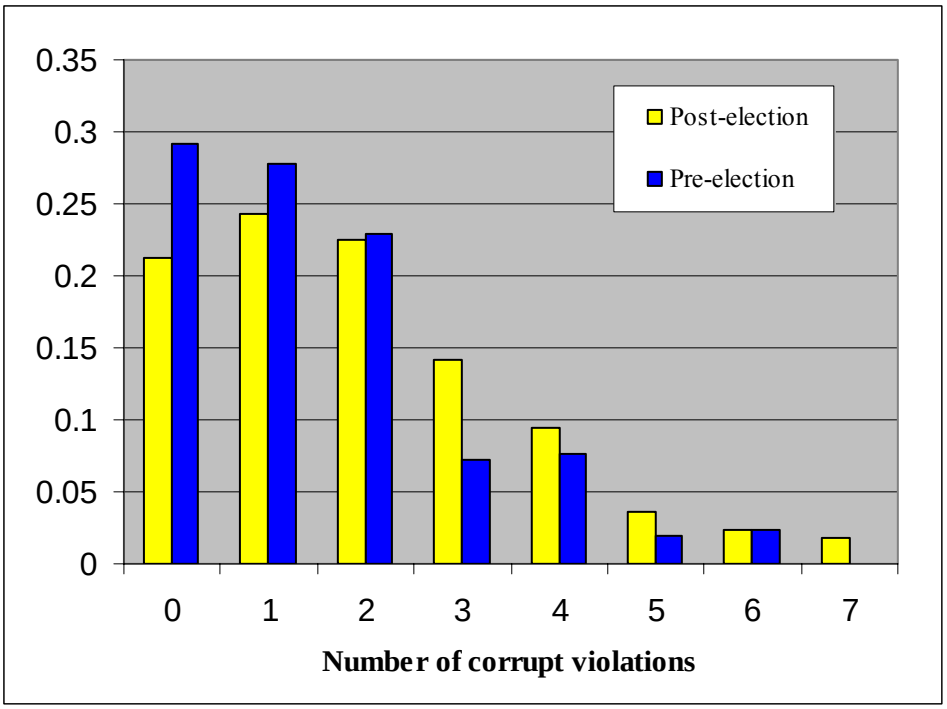

Figure 1: Distribution of corruption violations by pre versus post-election audits

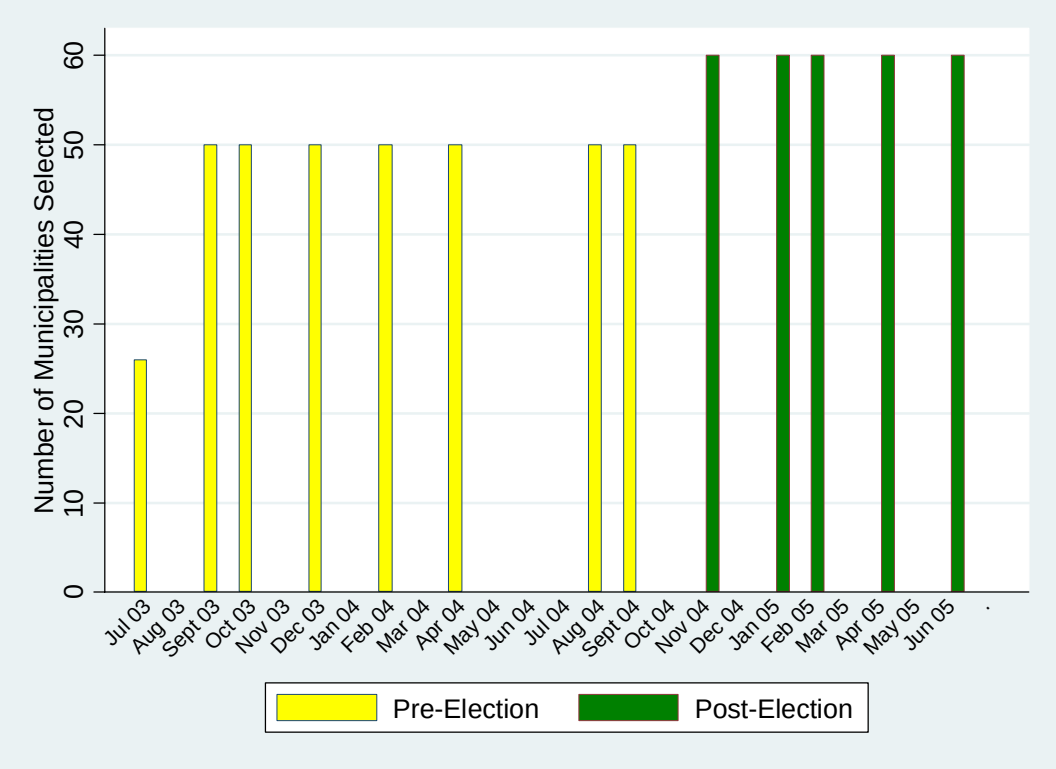

Figure 2: Timing of the release of the audits

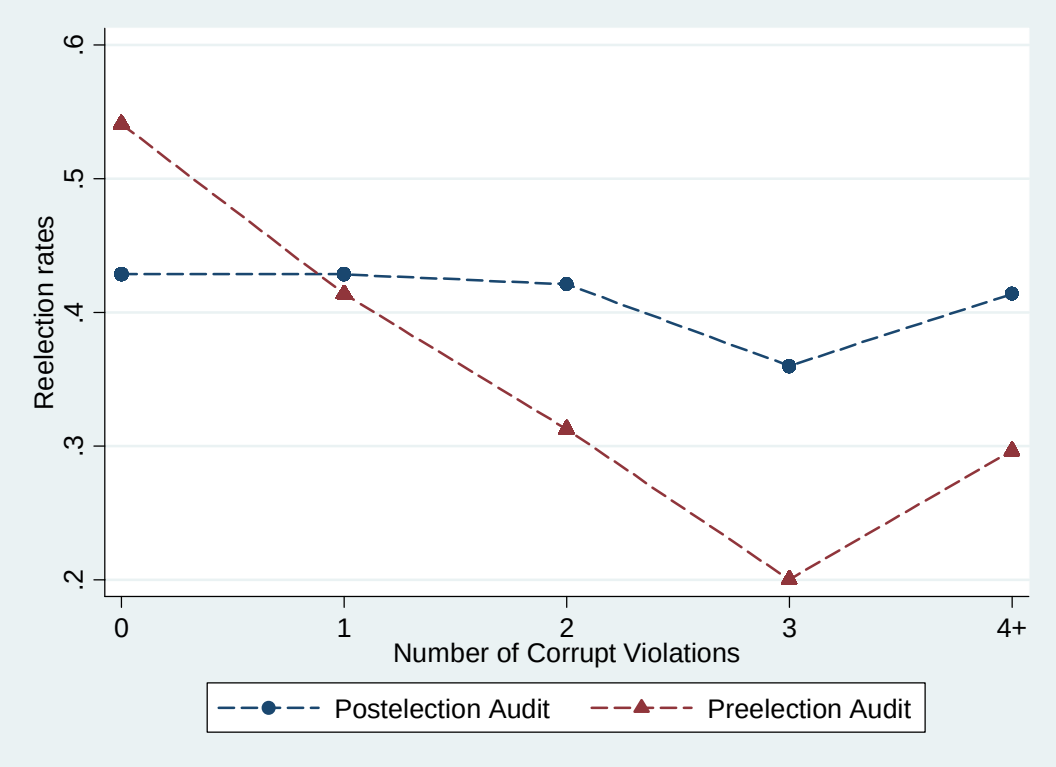

Figure 3: Relationship between re-election rates and corruption levels for municipalities audited before and after the elections

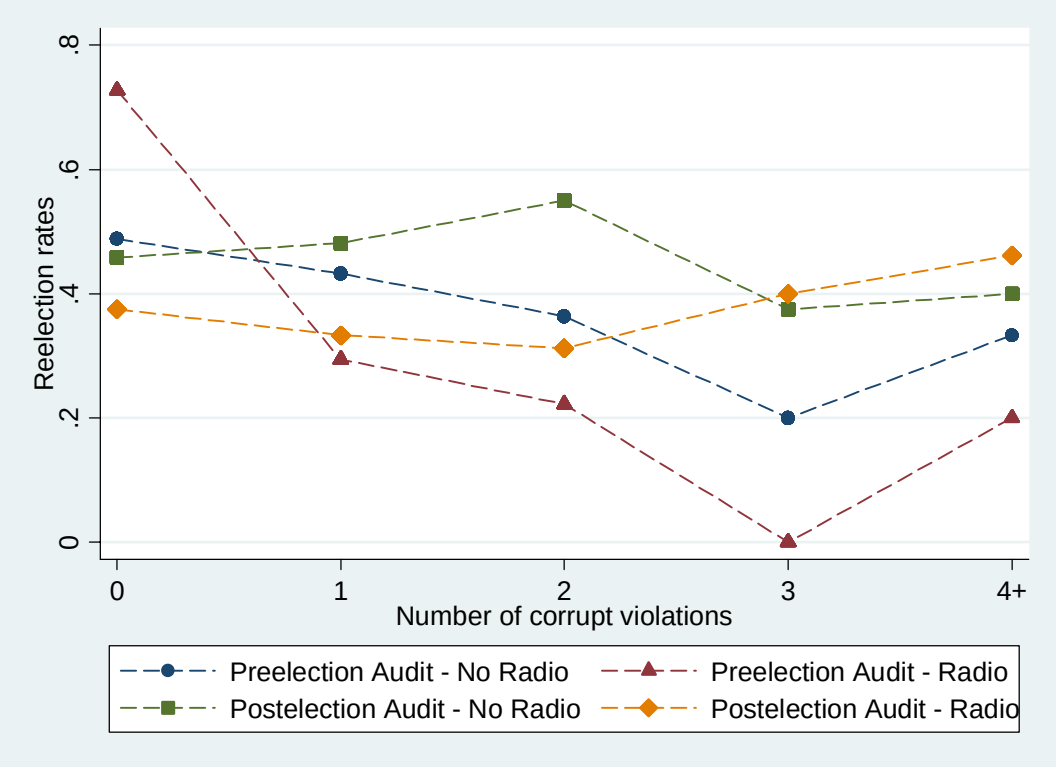

Figure 4: Relationship between re-election rates and corruption levels for municipalities audited before and after the elections and the existence of local radio