EXPOSING CORRUPT POLITICIANS: THE EFFECTS OF BRAZIL'S PUBLICLY RELEASED AUDITS ON ELECTORAL OUTCOMES*

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This paper uses publicly released audit reports to study the effects of disclosing information about corruption practices on electoral accountability. In 2003, as part of an anticorruption program, Brazil's federal government began to select municipalities at random to audit their expenditures of federally transferred funds. The findings of these audits were then made publicly available and disseminated to media sources. Using a data set on corruption constructed from the audit reports, we compare the electoral outcomes of municipalities audited before versus after the 2004 elections, with the same levels of reported corruption. We show that the release of the audit outcomes had a significant impact on incumbents' electoral performance, and that these effects were more pronounced in municipalities where local radio was present to divulge the information. Our findings highlight the value of having a more informed electorate and the role played by local media in enhancing political selection.

I. INTRODUCTION

In a well-functioning democracy, citizens hold politicians accountable for their performance. This is predicated upon voters having access to the information that allows them to evaluate politician performance (Manin, Przeworski, and Stokes 1999). By enabling citizens to monitor policy makers and hold corrupt politicians accountable, improved information forces incumbent governments to act in the best interest of the public (Besley 2006). Although a large body of theoretical literature agrees that improvements in the information available to voters influences electoral accountability (Persson and Tabellini 2000; Besley and Pratt 2006), identifying these effects empirically has been difficult. Information about politicians' performance is seldom randomly assigned to voters. Instead, it is typically acquired and

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influenced by voters' efforts, personal traits, characteristics of the community, or the level of political competition (Downs 1957). Moreover, because information can often be politically manipulated when it is not based on independent and reliable sources, it may be potentially discounted or even ignored by citizens when casting their ballots.[1]

This paper studies the effects of the disclosure of local governmental corruption practices on the electoral outcomes of incumbents in Brazil's municipal elections. It overcomes previous data limitations and identification concerns by using an experimental design that generates exogenous variation in the exposure of corrupt politicians to the public. The analysis utilizes an anticorruption program in Brazil initiated in April of 2003, when the federal government began to randomly select municipal governments to be audited for their use of federal funds. To promote transparency, the outcomes of these audits were then disseminated publicly to the municipality, federal prosecutors, and the general media.

Our research design exploits the randomized timing and public dissemination of the audits. Specifically, the analysis compares the electoral outcomes of mayors eligible for reelection between municipalities audited before and after the 2004 municipal elections. We investigate whether the effects of the audits varied in terms of two important aspects of the program: the type of information disclosed in the audit reports and the presence of the local media. Using the public reports to construct an objective measure of corruption—the number of violations associated with corruption—we compare municipalities audited preelection versus postelection conditional on their level of reported corruption. This comparison captures the fact that the audits may have had a positive or negative effect depending on the severity of the report and whether voters had over- or underestimated the extent of their mayor's corrupt activities. Second, given that the media are used to disseminate these findings, we also test whether the audit policy had a differential effect in regions where local media are present.

We find that the electoral performance of incumbent mayors audited before the elections, although slightly worse, was not

1. Existing studies that analyze how charges of corruption affect electoral outcomes find only minor impacts. See for example Peters and Welch (1980), who use data from the U.S. House of Representatives, and Chang and Golden (2004), who study the case of Italy. However, there is also evidence consistent with biased media affecting voting behavior; see DellaVigna and Kaplan (2007).

significantly different from the electoral outcomes of mayors who were audited after the election. However, when we account for the level of corruption that was revealed in the audit, the effects of the policy were considerable. Based on our preferred specification, among municipalities where two violations were reported, the audit policy reduced the incumbent's likelihood of reelection by seven percentage points (or 17%) compared to the reelection rates in the control group. The effect increases to almost fourteen percentage points in municipalities with three violations associated with corruption. Thus, voters not only care about corruption, but once empowered with the information, update their prior beliefs and punish corrupt politicians at the polls.

Furthermore, in those municipalities with local radio stations, the effect of disclosing corruption on the incumbent's likelihood of reelection was more severe. Compared to municipalities audited after the elections, the audit policy decreased the likelihood of reelection by eleven percentage points among municipalities with one radio station and where two violations were reported. Although radio exacerbates the audit effect when corruption is revealed, it also promotes noncorrupt incumbents. When corruption was not found in a municipality with local radio, the audit actually increased the likelihood that the mayor was reelected by seventeen percentage points.

Although our research design is based on a randomized control methodology, there are two potential threats to our identification strategy. First, even though municipalities were randomly selected, the design would be compromised if the actual auditing process differed systematically before and after the elections. We do not, however, find any evidence that auditors were corrupt or that municipalities audited before the elections received differential treatment. We also show that mayors with more political power, those affiliated with higher levels of government, and those who obtained larger campaign contributions did not receive preferential audits.

A second concern is that, although the variation in the timing of audits is exogenous, this is not the case for a municipality's level of corruption or its availability of local media. As such, our measures of corruption and media could be capturing the effects of other characteristics of the municipality. We provide evidence that this is not the case. Our estimates remain unchanged even after allowing the effects of the audits to differ by various correlates of corruption and presence of local radio (e.g., political

competition, education, population size, urbanization, and other media sources). Furthermore, we show that the results are similar when an alternative measure of radio penetration is used—the share of households that own a radio.

Overall, this paper demonstrates not only that the disclosure of information enhances political accountability, but also 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 median level of corruption. When no corruption was revealed and voters had overestimated the incumbent's corruption level, the incumbent was rewarded at the polls. That these findings are more pronounced in areas with local media also suggests that the media influence the selection of good politicians both by exposing corrupt politicians and by promoting good ones (Besley 2005).

Our paper lends strong support to the value of information and the importance of local media in promoting political accountability. Thus, our findings are consistent with an emerging empirical literature that examines the role of information flows in shaping electoral accountability and public policy.[2] Whereas much of this literature has focused on how access to information affects the responsiveness of governments, our study demonstrates how voters respond to new information. These findings also complement a recent literature on policies designed to reduce corruption.[3] Information disclosure about corruption may reduce capture of public resources through an alternative mechanism: reducing asymmetrical information in the political process to enable voters to select better politicians (Besley 2005; Besley, Pande, and Rao 2005).

2. Besley and Burgess (2002) show that governments in India are more responsive in their relief of shocks to places with higher newspaper circulation and where voters are more informed. Stromberg (1999) finds that U.S. counties with more radio listeners received more relief funds from the New Deal program. Recently, Gentzkow (2006) discusses how the introduction of television in the United States resulted in a sharp drop in newspaper and radio consumption, which reduced citizens' knowledge of politics and consequently led to lower voter turnout. Gentzkow, Glaeser, and Goldin (2006) demonstrate that changes between 1870 and 1920 in the U.S. newspaper industry are related to the reduction of corruption in U.S. politics in the same period.

3. For instance, Reinikka and Svensson (2005) show that an information campaign designed to reduce the diversion of public funds transferred to schools in Uganda increased their share of the entitlement by 13%. Using a randomized field experiment in 608 Indonesian villages, Olken (2007) analyzes how different monitoring mechanisms might reduce corruption in infrastructure projects. He finds that central auditing mechanisms are more effective in controlling corruption than grassroots participation monitoring.

The remainder of the paper is organized as follows. Section II provides a brief background on Brazil's anticorruption program and a description of the data used in the analysis. Our empirical strategy is discussed in Section III, and the paper's main empirical findings are presented and interpreted in Section IV. Section V concludes the paper.

II. Background and Data

II.A. Brazil's Anticorruption Program

In May 2003 the government of Luiz Inácio Lula da Silva started an unprecedented anticorruption program based on the random auditing of municipal governments' expenditures. 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, a summary of the main findings from each municipality audited is posted on the Internet and released to the media.

The program started with the audit of 26 randomly selected municipalities, one in each state of Brazil. It 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.[4] The random selection of municipalities is held on a monthly basis and drawn in conjunction with the national lotteries. To ensure 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 to 2003 and service orders are generated. Each one of these orders stipulates an audit task that is associated with the audit of funds from a specific government project (e.g., school construction, purchase of medicine). Approximately 10 to 15 CGU auditors are then sent to the municipality to examine accounts and documents and to inspect 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

4. This includes approximately 92% of Brazil's 5,500 municipalities, excluding mostly state capitals and coastal cities. It represents about 73% of the total population.

order to get direct complaints about any malfeasance. These auditors, who are hired based on a competitive public examination and earn highly competitive salaries, receive extensive training prior to visiting the municipality. Each team of auditors is also accompanied by a supervisor.

After approximately ten days 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 municipal legislative branch. For each municipality audited, a summary of the main findings is posted on the Internet and disclosed to main media sources.

Although we do not have direct evidence showing that voters learned about the audit reports, anecdotal evidence suggests that the information from the audits not only reached voters, but was used widely during the municipal elections. For instance, an article from the newspaper *Diário de Pará* illustrates the use of the audit reports in the political campaign and how this information came as a complete surprise to the public: "The conclusions from the CGU were used extensively in the political campaigns, by not only the opposition parties but those that received positive reports as well. . . . The reports were decisive in several cities. In the small city of Vicosa, in Alagoas, where a lot of corruption was found, the mayor, Flavis Flaubert (PL), was not reelected. He lost by 200 votes to Pericles Vasconcelos (PSB), who during his campaign used pamphlets and large-screen television in the city's downtown to divulge the report. Flaubert blames the CGU for his loss." (*Diário de Pará* (PA), 10/18/2004).

Another mayor unhappy with the information disclosed by the audits was Giovanni Brillantino from Itagimirim, in Bahia, who just before the elections claimed that "We knew that the opposition party would exploit this information in the election" (*Folha de S. Paulo*, 10/1/2004). Another article suggests that in some municipalities, the release of the audit reports took the population by surprise. For example, in Taperoá, Bahia, where several incidents of fraud were uncovered, the local legislator Victor Meirelles Neto (PTB) claimed that the population was shocked when this information was revealed (Agência Folha, 12/06/2003).

Although these newspaper articles suggest that information from the audit reports were widely used in the political campaigns, they do not describe explicitly how this information reached the municipalities. Given the central role radio plays in

local politics in Brazil, it is the most natural medium to inform the public about the audits. As opposed to other developing countries with similar income per capita, the low level of education in Brazil makes newspapers an unimportant source of local news. Newspapers are seldom read and are essentially only important in the largest cities.[5] This is evident by the fact that Brazil has one of the lowest levels of newspaper penetration in the world, with only 42 newspaper copies per 1000 inhabitants (Porto 2003).

Moreover, since the redemocratization of Brazil in the early 1980s, local AM radio stations have emerged as the central source of information for local politics in smaller municipalities. Although television has the largest penetration on a national scale, only 8% of municipalities broadcast local TV, whereas 34 percent of municipalities have local AM radio stations. Not only are these AM stations an important source of local news, but many radio broadcasters typically host call-in talk shows where listeners can complain about poor public services and even corruption scandals. As a result, many local radio hosts have become important political figures by acting as intermediaries between the community and politicians Nunes (2002).

II.B. Data

Measuring Corruption from the Audit Reports. In this section we describe how we use the audit reports to construct our indicator of corruption. As of July 2005, reports were available for the 669 municipalities that were randomly selected across the first thirteen lotteries.[6] To estimate the effects of the policy on reelection chances, we have to restrict the sample to the set of first-term mayors who were eligible for reelection. This reduces our estimation sample to only 373 municipalities.

Each audit report contains the total amount of federal funds transferred to the current administration and the amount audited, as well as an itemized list describing each irregularity. Based on our readings of the reports, we codified the irregularities

5. Even in the largest cities, newspaper circulation is low. In São Paulo, the largest and richest state in Brazil, the newspaper with the largest circulation—*Folha de São Paulo*—only sold 307,700 newspapers in 2004. See the National Newspaper Association at www.anj.org.br.

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

listed into those associated with corruption and those that simply represent poor administration.[7]

Although local corruption in Brazil assumes a variety of forms, most corruption schemes used by local politicians to appropriate resources are based on a combination of frauds in procurements, the use of fake receipts or "phantom" firms, and over-invoicing the value of products or services. In addition, the audit reports also suggest that some politicians simply divert resources for personal purposes.[8] Hence, we define political corruption as any irregularity associated with fraud in procurements, diversion of public funds, or over-invoicing.[9] These types of practices not only have been shown to be the most common ways local politicians find to appropriate resources, but in many instances are complementary. As such, we combine these indicators into a single measure of corruption. For each municipality, we sum up the number of times each one of these three irregularities appears and define this as our measure of corruption.

To illustrate the type of irregularities found and the procedure used to code corruption, consider the following examples extracted from the audit reports. In São Francisco do Conde, Bahia, the firm Mazda was contracted, without a public call for bids, to build approximately 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 totaled 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 did not have any experience with construction and had subcontracted another firm for R$1.8 million to do the construction. Hence, the project was overpaid by more than R$3 million. As evidence of corruption, it was later found that the firm Mazda gave an apartment to the

7. We also used an independent research assistant to code the reports in order to provide a check on our coding. See Ferraz and Finan (2007a) for more details on how we coded the audit reports.

8. See Trevisan et al. (2004) for detailed description of corruption schemes in Brazil's local governments. Also see Geddes and Neto (1999) for an overview of political corruption in Brazil.

9. Specifically, we define a *procurement to be irregular* if (i) there was no call for bids; (ii) the minimum number of bids was not attained; or (iii) there was evidence of fraud (e.g., use of bids from nonexisting 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.

mayor and his family valued at R$600,000. We classified this violation as an incidence of over-invoicing.

Another example of corruption in Capelinha, Minas Gerais, illustrates diversion of resources. The Ministry of Health transferred to the municipality R$321,700 for a program called *Programa de Atenção Básica*. The municipal government used fake receipts valued at R$166,000 to provide proof of purchase of medical goods. Furthermore, there is no evidence that the goods were ever purchased, because no registered entries of the merchandise were found in stock.

Illegal procurement practices typically consist of benefiting friendly or family firms with insider information on the value of a project, or imposing certain restrictions to limit the number of potential bidders. This was the situation in Caculé, Bahia, where the call for bids on the construction of a sports complex required all participating firms 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.

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 the 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 sex, education, occupation, and party affiliation. With this information, we matched individuals across elections to construct our main dependent variable—whether the incumbent mayor was reelected—as well as other measures of electoral performance such as vote share and margin of victory.

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 are per capita income, income inequality, population density, share of the population that lives in urban areas, 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 and whether the municipality has a judge. 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 data set 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.

Summary Statistics. Basic descriptive statistics of our corruption measure, electoral outcomes, and municipal characteristics are presented in Table I. These statistics, as well as the analysis that follows, are estimated for the 373 municipalities that were both audited and governed by a first-term mayor, who is eligible for reelection.[10] Besides providing background on the average municipality's socioeconomic 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 168 municipalities that were audited after the election (control group), whereas column (2) presents the mean for the 205 municipalities that were audited before 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).

Panels A and B document the political outcomes and characteristics of the mayors in our sample. Reelection rates for the past two elections have been roughly 40% among the incumbent mayors who are eligible for reelection. Although it might appear that Brazilian mayors do not enjoy the same incumbent advantage that is reputed in other countries, reelection rates do increase to 59% when conditioned on the mayors that ran for reelection (approximately 70% of all eligible mayors; see column (1)). Reelection in most municipalities of Brazil requires only a plurality, and yet

10. Only 60% of all Brazilan mayors were eligible for reelection in 2004. The remaining 40%, who had been elected to a second term in 2000, were not eligible for reelection under the Brazilian constitution, which limits members of the executive branch to two consecutive terms. Ferraz and Finan (2007b) discuss the effects of term limits on corruption in Brazil.

TABLE I
CHARACTERISTICS OF THE MUNICIPALITIES

	Postelection audit (1)	Preelection audit (2)	Difference (3)	Standard error (4)
Panel A: Political characteristics				
Reelection rates for the 2004 elections	0.413	0.395	0.018	0.045
Reelection rates for the 2000 elections	0.423	0.443	−0.020	0.040
2004 reelection rates, among those that ran	0.585	0.559	0.026	0.044
Ran for reelection 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: Mayoral 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 PMDB	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	0.42	0.43	−0.01	0.02
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
Share of households that own a radio	0.79	0.77	−0.02	0.02
Municipalities with a radio station	0.31	0.24	0.07	0.06

TABLE I
(CONTINUED)

	Postelection audit (1)	Preelection audit (2)	Difference (3)	Standard error (4)
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
Sample size	168	205		

Notes. This table reports the mean political, mayoral, and socioeconomic characteristics of all the municipalities that were audited in the first thirteen lotteries. With the exception of reelection rates for the 2000 election, these statistics were only computed for the 373 municipalities where the mayor was eligible for reelection. The 2000 reelection rates, which include both first- and second-term mayors, were computed for 669 municipalities. Column (1) reports the means for the 168 municipalities that were audited after the elections and constitute our control group. Column (2) reports the mean for the 205 municipalities that were audited before the elections and hence constitute our treatment group. Column (3) reports the difference in means and column (4) presents the standard error of the difference. The political and mayor characteristics presented in Panels A and B were constructed using data from Brazil's electoral commission (Tribunal Superior Eleitoral: http://www.tse.gov.br/index.html). The socioeconomic characteristics presented in Panel C were constructed using data from Brazil's statistical bureau (Instituto Brasileiro de Geografia e Estatistica: http://www.ibge.gov.br). The corruption measure and the amount of resources audited were constructed from the audit reports conducted by Brazil's controller's office (Controladoria Geral da Uniao: http://www.cgu.gov.br). Definition of the variables: *Ran for reelection in 2004* is the proportion of eligible (first-term) mayors who ran for reelection in 2004; *Number of parties in 2000* is the average number of political parties that competed in the 2000 elections; *Margin of victory* is the average difference between the winner and the second highest vote share; *PSB, PT, PMB, PFL, PPB, PSDB* are major political parties in Brazil and accounts for approximately 70% of the mayors in 2004; *Urban* is the share of households that live in urban areas; *Log per capita income* is log of the average monthly per capita income of a household; *Income inequality* is the Gini coefficient computed for monthly income; *Zoning law* is an indicator for whether the municipality has zoning laws; *Economic incentives* is an indicator for whether the municipality provides economic incentives to businesses; *Paved roads* is an indicator for whether the municipality has paved roads; *Size of public employment* is the share of the budget in 1999 that was used to pay public employees; *Municipal guards* is an indicator for whether the municipality has its own police force; *Small claims court* is an indicator for whether the municipality has a small claims court; *Judiciary district* is an indicator for whether the municipality has a judiciary district; *Number of corrupt violations* is the sum of violations that are associated with corruption; *Total resources audited* is the amount of funds that was audited by the CGU, expressed in reais.

on average elected mayors win with over 50% of the votes.[11] Even though 18 political parties are represented in our sample, over 70% of the elected mayors belong to one of the six parties presented in Panel B, and on average only three 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% of the population of these municipalities lives in rural areas, and 21% of the adult population is illiterate. Local AM radio stations exist in only 27% of the

11. Mayors of municipalities with a population of less than 500,000 can win an election with a plurality; otherwise an absolute majority is required.

municipalities and 79% of households own a radio. 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% level of significance.[12] In fact, of 90 characteristics, only three variables—the number of museums, whether the municipality has a local constitution, and whether the municipality has an environmental council—were significantly different between the two groups of municipalities. Including these three characteristics in the regressions does not affect the estimated coefficients.

The last couple of rows of Table I 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 municipal corruption is widespread in Brazil. At least 73% of the municipalities in our sample had an incident of corruption reported, and the average number of corrupt irregularities found was 1.74. Municipalities that were audited after the elections tended to be slightly more corrupt than those audited before the election, but this difference was small and statistically indistinguishable from zero.

For a better sense of the corruption measure, Figure I presents the distributions of reported corruption for municipalities that were audited before and after the elections. As this figure depicts, the mass of the distribution falls mostly between zero and four corrupt violations, with less than 6% of the sample having more than four 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% level. This comparison further validates not only the program's randomized auditing, but also the integrity of the audit process.

III. Estimation Strategy

We are interested in testing whether the release of information about the extent of municipal government corruption affects

12. Whether the mayor belongs to PMDB is significantly different between the groups at the 10% level. As demonstrated in the Results section, controlling for this variable does not affect the estimation results.

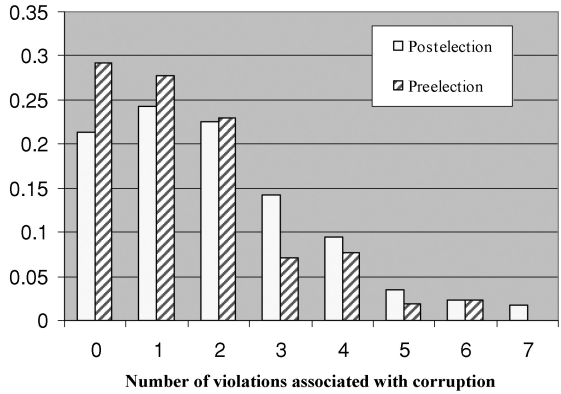

FIGURE I
Distribution of Corruption Violations by Pre- versus Postelection Audits

Notes. Figure shows the distribution of corruption incidents reported in the audits. The striped bars represent the 168 municipalities that were audited before the elections. The solid bars denote 205 municipalities audited after the elections. The figure was calculated based on our entire sample of municipalities with first-term mayors, using data from the CGU audit reports.

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, a 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 unfeasible. Our research design, which exploits the random auditing of the anticorruption program and the timing of the municipal elections, is perhaps the closest approximation to such an experiment.

Figure II depicts the timing of the release of the corruption reports. Prior to the October 2004 municipal elections, the federal government had audited and released information on the corruption levels of 376 municipalities randomly selected across eight lotteries. After the municipal elections, audit reports for 300 municipalities were released, providing us with information on corruption levels for two groups of municipalities: those whose corruption levels were released prior to the elections—potentially affecting voters' perceptions of the mayor's corruptness—and those that were audited and had their results released only after

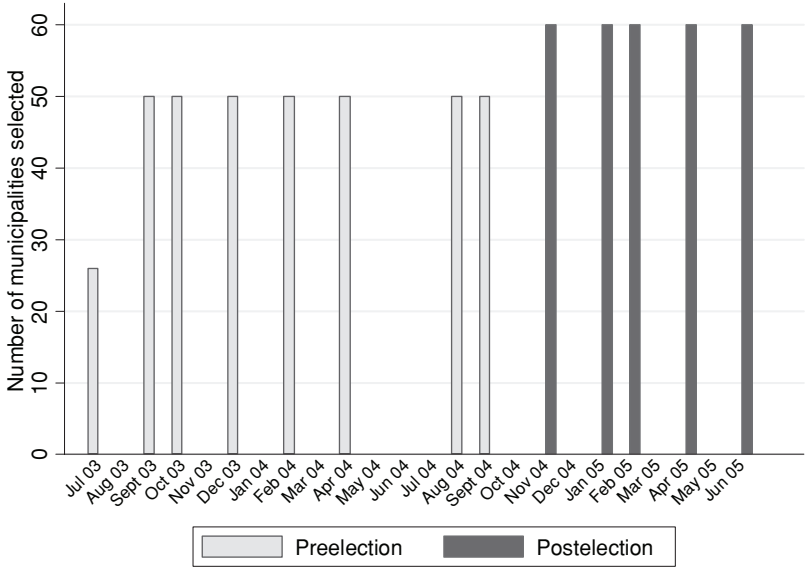

FIGURE II
Timing of the Release of the Audits

Notes. Figure shows the dates for the release of the audit reports for every municipality that was audited in the first thirteen lotteries. The lighter bars denote the 376 municipalities that were audited before the elections. The darker bars denote 300 municipalities audited after the elections. The figure was calculated based on data from the CGU.

the elections.[13] Because municipalities were selected at random, the set of municipalities whose audit reports were only made available after the elections represent a valid control group.

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

$$E_{ms} = \alpha + \beta A_{ms} + X_{ms}\gamma + \nu_s + \varepsilon_{ms}, \tag{1}$$

where E_{ms} denotes the electoral performance of an incumbent mayor eligible for reelection in municipality m and state s, A_{ms} is an indicator for whether 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. Because of the randomized auditing, the coefficient β

13. Recall that for the estimation we have to restrict our sample to only first-term mayors, who are eligible for reelection.

provides an unbiased estimate of the average effect of the program on the electoral outcome of the incumbent politician, capturing the effect both of being audited and of the public release of this information.

Although the comparison between municipalities audited before and after the elections identifies the average impact of the program on electoral outcomes, it does not capture the fact that the effects of the information will depend on voters' prior beliefs about the incumbent's corruption activities.[14] If the politician is revealed to be more corrupt than the voters expected, then this information may decrease his reelection chances. However, if the voters overestimated the incumbent's corruptness, then this information may actually increase his probability of reelection. Thus, unless voters systematically over- or underestimate the incumbent's corruption level, the simple average treatment effect of the audits will expectedly vary according to the level of corruption reported. The effects of the policy will likely be negative at higher levels of reported corruption, and presumably positive at lower levels of reported 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,

$$(2)\quad E_{ms} = \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 (A_{ms} \times C_{ms}) + X_{ms}\gamma + \nu_s + \varepsilon_{ms},$$

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 in the disclosure of information about corruption is the availability of local media. A critical design feature of the anticorruption program was the use of mass media to divulge the results of the audits. If the government audits and media serve as complements, then we would expect a more pronounced effect in areas where local media is present. 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

14. See Ferraz and Finan (2007c) for a simple theoretical model illustrating how the effects of the audits will likely depend on voters' prior beliefs in the mayor's corruptness.

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 the hypothesis that the impact of the disclosure of information about corruption depends on the existence of local media, we augment the specification in equation (2) with a set of terms to capture the triple interaction between whether the municipality was audited, its corruption level, and the availability of local media:

$$E_{ms} = \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 M_{ms} + \beta_3 (A_{ms} \times M_{ms}) + \beta_4 (A_{ms} \times C_{ms}) + \beta_5 (M_{ms} \times C_{ms}) + \beta_6 (A_{ms} \times C_{ms} \times M_{ms}) + X_{ms}\gamma + \nu_s + \varepsilon_{ms}. \quad (3)$$

Our measure of media, M_{ms}, is the number of local AM radio stations that exist in the municipality. As discussed in the background section, radio is the most important source of local news in Brazil and broadcasters play a key role in disseminating information about political irregularities. 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 local radio stations in the municipality.

Although our identification of the impact of releasing information on corruption is based on the random audits of municipalities, the audit experiment was unfortunately not randomized over the availability of local media. Hence, 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 reelection outcomes. We explore this possibility in the section of robustness checks using three alternative specifications. First we introduce interaction terms of the preelection audits with the number of corrupt violations and municipal characteristics that might be correlated with the presence of local AM radios. Second, we estimate an alternative specification where the share of households with radios in the municipality is used as a measure of radio penetration.[15] Third, despite radio being the most important source of local news, we estimate specifications using the number of newspapers in the municipality and the proportion of households that own a television.

15. This is the same measure used by Stromberg (2004).

IV. Results

IV.A. The Average Effects of the Audits on Electoral Outcomes

We begin this section by presenting estimates of the average effects of the audit policy on various electoral outcomes. Table II presents OLS regression results from estimating several variants to equation (1). The specification in the first column estimates the effects of the audit policy on the likelihood that an eligible mayor is reelected, controlling only for state intercepts. Column (2) extends the specification in column (1) to include various municipal and mayor characteristics. The regressions presented in columns (3)–(7) estimate the effects of the policy on other measures of electoral performance but restrict the estimation sample to only those mayors who actually ran for reelection.[16]

The results in columns (1)–(3) suggest that the audits and the associated release of information did not have, on average, a significant effect on the reelection probability of incumbent mayors. Although reelection rates are 3.6 percentage points lower in municipalities that were audited prior to the elections (column (1)), we cannot reject the possibility that this effect is not statistically different from zero (standard error is 0.053). The inclusion of municipal and mayoral characteristics (column (2)), 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 reelection provides similar results (column (3)).

Even though the audits do not appear to have significantly affected reelection probabilities, winning the election is a discontinuous outcome. The program might have impacted other measures of electoral performance such as vote shares and margin of victory without ultimately affecting the election outcome. However, as reported in columns (4)–(7), we find only minimal evidence that the audit policy affected these other measures of electoral performance. The change in vote share is 3.2 percentage points lower in municipalities audited prior to the elections, and statistically significant at 90% confidence. Even though this estimate implies a 52% decline from a baseline of -0.057, overall the results are based on a select sample of mayors.

The lack of evidence documenting an average effect of the anticorruption policy on electoral outcomes is to some extent

16. Also note that the sample has been restricted to the nonmissing observations of the various control variables, to keep its size constant across specifications.

TABLE II
THE AVERAGE EFFECTS OF THE RELEASE OF THE AUDITS ON ELECTORAL OUTCOMES

	All incumbent mayors		Only mayors that ran for reelection				
	Pr(reelection)		Pr(reelection)	Vote share	Win margin	Change in vote share	Change in win margin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Preelection audit (1/0)	−0.036	−0.036	−0.059	−0.055	−0.020	−0.032	−0.028
	[0.053]	[0.052]	[0.065]	[0.072]	[0.027]	[0.018]+	[0.027]
Observations	373	373	263	263	263	263	263
R^2	0.05	0.17	0.22	0.16	0.22	0.39	0.31
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Mayoral characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effects of the audits on various electoral outcomes. Each column presents the results of an OLS regression of the dependent variables listed in that column on an indicator variable for whether the municipality was audited before the elections. Except for column (1), all regressions include municipal characteristics: population density (persons/km), percentage of the population that is literate, 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); mayoral characteristics: sex (1/0 for male), age, married (1/0), education level, party dummies; and state intercepts. The sample in columns (1) and (2) includes all mayors who were eligible for reelection. The samples in columns (3)–(7) include only the mayors who chose to run for reelection. Robust standard errors are displayed in brackets. Significantly different from zero at 99 (**), 95 (*), 90 (+) % confidence.

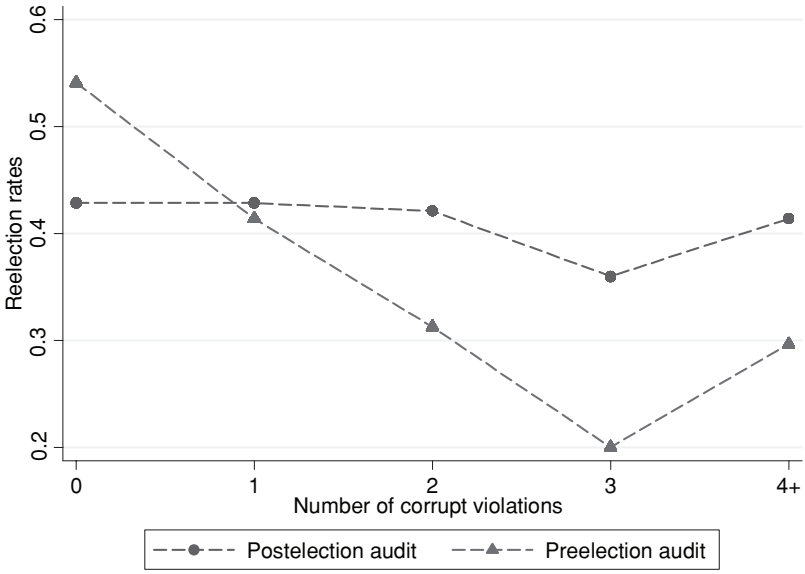

FIGURE III
Relationship between Reelection Rates and Corruption Levels

Notes. Figure shows the unadjusted relationship between the proportion of first-term mayors who were reelected in the 2004 elections and the number of corrupt incidents reported in the audit reports for municipalities audited before and after the elections. The points represented by circles are calculated for the municipalities that audited after the elections. The points represented by triangles are calculated for the municipalities audited before the elections. The figure was calculated for our entire sample of 373 municipalities based on data from Brazil's Electoral Commission and the CGU audit reports.

expected. As discussed above, the effects of the audits are likely to depend on both the type of information revealed and the presence of local media. In the next section, to test for these differential effects, we exploit the fact that we observe the corruption level of each audited municipality. Because of the random release of the audit reports, causal inference can still be made conditional on the municipality's corruption level.

IV.B. The Effects of the Audits by Corruption Levels

In this section, we investigate whether the policy's effect varies according to the extent of corruption found. To get an understanding for how the dissemination of corruption information might affect an incumbent's electoral performance, Figure III illustrates the unadjusted relationship between corruption and reelection rates. The figure plots the proportion of eligible mayors

reelected 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).[17]

Municipalities that were audited and had their findings disseminated prior to the municipal elections exhibit a striking downward-sloping relationship between reelection rates and corruption. Among the municipalities where not a single violation of corruption was discovered, approximately 53% of the incumbents eligible for reelection were reelected. Reelection 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, reelection rates were about 20% among municipalities where auditors reported three corrupt violations. For municipalities with four or more violations, reelection rates increase slightly but still remain low at less than 30% (ten percentage points 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 reelection rates and corruption among municipalities that experienced a preelection audit lies in stark contrast to the relationship depicted for municipalities that underwent a postelection audit. With only a minor exception, reelection rates remained steady across corruption levels at close to the population average of 40%. 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 reelection. 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 affected the incumbent's likelihood of reelection and that the impact depended on the severity of the corruption reported.

17. We group together municipalities where at least four incidents of corruption were uncovered. With this regrouping, each level of corruption contains approximately 20% of the sample.

Regression Analysis. Table III provides a basic quantification of the relationship depicted in Figure III. The estimation results are from a series of models based on equation (2), where the dependent variable is an indicator of whether an eligible incumbent was reelected in the 2004 elections. As in the previous table, the specification presented in the first column controls for state fixed effects, but excludes any other control variables, whereas the other columns present specifications that control for an additional twenty municipal and mayoral characteristics.

The models in columns (1) and (2) assume a linear relationship between reelection rate 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 -3.8 percentage points. However, despite the fact that these estimates represent a 9% decline in reelection rates, they are not statistically significant at conventional levels. Although it is possible that the audit policy did not elicit electoral retribution, the patterns presented in Figure III suggest that a linear regression model might be misspecified.

The models in columns (3) and (4) present alternative specifications that allow for more flexibility in the relationship between corruption and reelection. In column (3), we estimate a model that assumes a quadratic relationship between the probability of reelection and corruption and in doing so allows for the up-tick in reelection rates at the higher levels of corruption. The estimates suggest that the quadratic terms do have some predictive power (F-test $= 2.58$; P-value $= .08$ 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 reelection. Among the municipalities where only one corruption violation was discovered, which is approximately the intersection point in Figure III, the dissemination of this information reduced reelection rates by only 4.6 percentage points ($F(1,348) = 0.57$; P-value $= .45$). In contrast, the audit policy reduced reelection rates by 17.7 percentage points ($F(1,348) = 4.93$; P-value $= .03$) in municipalities where three corrupt violations were reported.

The specification in column (4) of Table III relaxes our parametric assumption even further. Here, we use a semiparametric specification to estimate the effects at each level of reported corruption. The estimates in column (4) present a pattern similar to

TABLE III
THE EFFECTS OF THE RELEASE OF THE AUDITS ON REELECTION RATES BY THE LEVEL OF REPORTED CORRUPTION

	Linear		Quadratic	Semiparametric	Corruption ≤ 5	Corruption ≤ 4
	(1)	(2)	(3)	(4)	(5)	(6)
Preelection audit	0.029	0.030	0.126	0.084	0.068	0.086
	[0.083]	[0.082]	[0.101]	[0.104]	[0.087]	[0.088]
Preelection audit × number of corrupt violations	−0.038	−0.038	−0.200		−0.070	−0.088
	[0.035]	[0.035]	[0.090]*		[0.041]+	[0.043]*
Preelection audit × number of corrupt violations^2			0.034			
			[0.017]*			
Preelection audit × corruption = 0				0.010		0.003
				[0.156]		[0.036]
Preelection audit × corruption = 2				−0.253		
				[0.148]+		
Preelection audit × corruption = 3				−0.321		
				[0.192]+		
Preelection audit × corruption = 4+				−0.159		
				[0.168]		
Number of corrupt violations	−0.013	−0.012	0.037		0.012	0.003
	[0.026]	[0.027]	[0.066]		[0.033]	[0.036]
Number of corrupt violations^2			−0.009			
			[0.011]			

TABLE III
(CONTINUED)

	Linear		Quadratic	Semiparametric	Corruption ≤ 5	Corruption ≤ 4
	(1)	(2)	(3)	(4)	(5)	(6)
Corruption = 0				0.028		
				[0.126]		
Corruption = 2				0.052		
				[0.114]		
Corruption = 3				−0.006		
				[0.129]		
Corruption = 4+				−0.002		
				[0.136]		
Observations	373	373	373	373	362	351
R^2	0.05	0.18	0.19	0.22	0.19	0.20
F-test (P-values)			.089	.192		
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	No	Yes	Yes	Yes	Yes	Yes
Mayoral characteristics	No	Yes	Yes	Yes	Yes	Yes

Notes. This table reports the effects of the release of the audits on the likelihood of reelection, by the level of corruption reported in the audits. Each column presents the results of an OLS regression where the dependent variable is an indicator for whether the mayor was reelected in the 2004. Except for column (1), all regression include municipal characteristics: population density (persons/km), percentage of the population that is literate, 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); mayoral characteristics: sex (1/0 for male), age, married (1/0), education level, party dummies; and state intercepts. The estimation sample includes all mayors who were eligible for reelection. Robust standard errors are displayed in brackets. Significantly different from zero at 99 (**), 95 (*), 90 (+) % confidence. In columns (3) and (4), the F-test tests the joint significance of the interaction terms.

the one depicted in Figure III. Relative to when one violation is reported (the excluded category), the likelihood of reelection decreases with each reported violation. For instance, with two violations associated with corruption, the probability of being reelected decreases by 25 percentage points (standard error = 0.148), relative to one violation. The effects become more pronounced at three violations but less so at more than four violations. Although given our sample size, it is difficult to identify the impact of the audit policy at each level of corruption jointly ($F(4,19) = 4.02$; P-value = .192), the effects are sizable and politically meaningful.

Is the relationship between reelection rates and corruption levels U-shaped or does this just reflect noise in the data? In column (5), which displays our preferred specification, we fit the linear model presented in the first two columns to the subset of municipalities that had no more than five corrupt violations, thus excluding eleven observations (five from treatment and six from control). These observations represent not only less than 3% of the sample, but corruption levels that are almost three 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% level. The estimate of the interaction term is −0.070 (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 reelection by 16% of the 43% baseline reelection rate for the control municipalities. If we restrict the sample further, excluding municipalities with more than five corrupt violations—less than 6% of the sample—the point estimate on the interaction increases even more to −0.088 (standard error = 0.043).

The remaining rows of column (5) contain the estimated counterfactual relationship between reelection rates and corruption. These estimates, which are close to zero and statistically insignificant, are expected to 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 six highly corrupt mayors in the sample creates a negative relationship between reelection rates and corruption in control municipalities. 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.[18] Moreover, we do not find any evidence that municipal or mayoral characteristics such as population, literacy, urbanization, political competition, income, and inequality are associated with having more than five corrupt violations.

Table IV presents a series of models similar to those reported in Table III but estimates the effects of the policy on other measures of electoral performance.[19] Overall the results reported in Table IV tell a similar story. For instance, the estimates in column (4) 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. The credibility of our research design stems from the fact that municipalities were randomly chosen to be audited, together with the exogenous 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. If the audits conducted before the elections differed systematically from those conducted after the elections, then our research design would be compromised.

The most obvious concern is that the auditors themselves might have been corrupted. 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.[20] There are at least four reasons that this is unlikely to be the case. First, auditors are hired based on a highly competitive public examination and are well-paid public employees.

18. An alternative way to account for these outliers is to estimate a linear spline model. Based on Figure III, we specify knot points at 3 and 5, to allow for differential slopes at each segment. These estimates suggest that for corruption less than or equal to 3, the audit policy reduced reelection rates by 12.5 percentage points (standard error = 0.054). But, for the other segments, we cannot reject that the change in the slope is statistically different (point estimate for the [3,5] segment = 0.241 with standard error = 0.166; point estimate for the [5,7] segment = −0.013 with standard error = 0.387).

19. These other electoral outcomes by construction limit the analysis—and thus inference—to the select group of mayors who ran for reelection. Interestingly, we find no evidence that the audit policy reduced the probability that the mayor would run for reelection.

20. This argument of course assumes that mayors audited after the elections do not have further reelection incentives.

TABLE IV

THE EFFECTS OF THE RELEASE OF THE AUDITS ON OTHER ELECTORAL OUTCOMES BY THE LEVEL OF REPORTED CORRUPTION

	Pr(reelection)			Margin of victory			Vote share			Change in vote share		
Dependent variables:	Full sample (1)	Corruption ≤ 5 (2)	Semi-parametric (3)	Full sample (4)	Corruption ≤ 5 (5)	Semi-parametric (6)	Full sample (7)	Corruption ≤ 5 (8)	Semi-parametric (9)	Full sample (10)	Corruption ≤ 5 (11)	Semi-parametric (12)
Preelection audit	0.045	0.072	0.058	0.037	0.053	0.018	0.078	0.104	0.077	−0.014	0.006	0.012
	[0.095]	[0.099]	[0.135]	[0.037]	[0.039]	[0.053]	[0.102]	[0.106]	[0.146]	[0.027]	[0.027]	[0.035]
Preelection audit ×	−0.06	−0.086		−0.034	−0.049		−0.078	−0.104		−0.01	−0.029	
corrupt violations	[0.039]	[0.046]+		[0.015]*	[0.019]**		[0.041]+	[0.048]*		[0.012]	[0.013]*	
Preelection audit ×			0.064			0.069			0.103			0.009
corruption = 0			[0.188]			[0.071]			[0.201]			[0.046]
Preelection audit ×			−0.335			−0.152			−0.42			−0.117
corruption = 2			[0.188]+			[0.079]+			[0.205]*			[0.054]*
Preelection audit ×			−0.321			−0.118			−0.371			−0.052
corruption = 3			[0.246]			[0.079]			[0.262]			[0.062]
Preelection audit ×			−0.156			−0.082			−0.182			−0.045
corruption = 4+			[0.195]			[0.083]			[0.208]			[0.062]
Number of corrupt	−0.016	0.001		0.011	0.019		−0.002	0.014		−0.001	0.01	
violations	[0.030]	[0.036]		[0.012]	[0.014]		[0.032]	[0.039]		[0.010]	[0.010]	
Corruption = 0			−0.006			0.011			−0.017			0.003
			[0.155]			[0.057]			[0.166]			[0.035]

TABLE IV
(CONTINUED)

	Pr(reelection)			Margin of victory			Vote share			Change in vote share		
Dependent variables:	Full sample (1)	Corruption ≤ 5 (2)	Semi-parametric (3)	Full sample (4)	Corruption ≤ 5 (5)	Semi-parametric (6)	Full sample (7)	Corruption ≤ 5 (8)	Semi-parametric (9)	Full sample (10)	Corruption ≤ 5 (11)	Semi-parametric (12)
Corruption = 2			0.06			0.082			0.11			0.027
			[0.145]			[0.060]			[0.158]			[0.039]
Corruption = 3			−0.014			0.048			0.012			0.011
			[0.162]			[0.059]			[0.172]			[0.038]
Corruption = 4+			−0.076			0.076			−0.016			0.009
			[0.161]			[0.068]			[0.171]			[0.048]
Observations	264	256	264	264	256	264	264	256	264	264	256	264
R^2	0.24	0.24	0.27	0.18	0.20	0.26	0.24	0.24	0.28	0.40	0.42	0.46
F-test (P-values)			.121			.011			.035			.113

Notes. This table reports the effects of the release of the audits on other electoral outcomes, by the level of corruption reported in the audits. Each column presents the results of an OLS regression, where the dependent variable is listed in that column. All regression include municipal characteristics: population density (persons/km), percentage of the population that is literate, 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); mayoral characteristics: sex (1/0 for male), age, married (1/0), education level, party dummies; and state intercepts. The estimation sample includes all mayors that ran for reelection and is listed in each column. Robust standard errors are displayed in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) % confidence. In columns (3) and (4), the F-test tests the joint significance of the interaction terms.

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 an incident in which auditors have even been offered bribes.[21] 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 I, the levels of corruption across the two groups were well-balanced not only on the 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 were manipulated, then we might expect mayors who were politically affiliated with either the federal or state governments to receive more favorable audit reports. To test for this possibility, column (1) of Table V reports a model that regresses the number of corruption violations on whether or not the municipality was audited prior to the elections, whether the mayor is a member of the governor's political party, party dummies, and a full set of interaction terms. From the results presented in column (1), we 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 $= 0.256$).[22] Moreover, there are no differential effects for any of the six major parties (P-value $= .97$).

Another possibility is that incumbents who won by narrow victories in the previous election have greater incentives to bribe the auditors to receive more favorable 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 municipality 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 is of the opposite sign (point estimate $= -0.638$ and standard error $= 0.865$).

The remaining columns of Table V provide further evidence of the robustness of our results. Columns (3) and (4) report the

21. Based on the interviews conducted by the authors with program officials in Brasilia.

22. The interaction between the Workers' Party (PT) and preelection audit controls for whether the mayor is in the same political party as the federal government.

TABLE V
TESTING FOR MANIPULATION OF THE AUDITING PROCESS

	Corruption		Pr(reelection)			
	Full sample		Corruption ≤ 5	Semiparametric	Corruption ≤ 5	Semiparametric
Dependent variables:	(1)	(2)	(3)	(4)	(5)	(6)
Preelection audit	−0.332	−0.231	0.096	0.096	0.094	0.038
	[0.261]	[0.298]	[0.125]	[0.138]	[0.129]	[0.162]
Preelection audit × corrupt violations			−0.071		−0.081	
			[0.039]+		[0.056]	
Preelection audit × corruption = 0				−0.012		0.032
				[0.155]		[0.185]
Preelection audit × corruption = 2				−0.173		−0.14
				[0.146]		[0.162]
Preelection audit × corruption = 3				−0.364		−0.24
				[0.214]+		[0.314]
Preelection audit × corruption = 4+				−0.153		−0.237
				[0.169]		[0.213]
Preelection audit × member of the governor's coalition	−0.155	−0.155	0.059	0.036		
	[0.256]	[0.388]	[0.134]	[0.136]		
Preelection audit × margin of victory in 2000 elections		−0.638	−0.198	−0.173		
		[0.868]	[0.316]	[0.313]		
Preelection audit × PT	−0.004	−0.034	0.3	0.274		
	[0.861]	[0.864]	[0.278]	[0.264]		
Preelection audit × PMDB	0.157	0.132	0.073	0.095		
	[0.389]	[0.398]	[0.130]	[0.129]		

TABLE V
(CONTINUED)

	Corruption		Pr(reelection)			
	Full sample		Corruption ≤ 5	Semiparametric	Corruption ≤ 5	Semiparametric
Dependent variables:	(1)	(2)	(3)	(4)	(5)	(6)
Preelection audit × PFL	0.064	0.052	−0.101	−0.146		
	[0.445]	[0.455]	[0.149]	[0.115]		
Preelection audit × PSDB	−0.456	−0.471	−0.533	−0.263		
	[0.989]	[0.978]	[0.241]*	[0.327]		
Preelection audit × PSB	0.093	0.073		−0.46	−0.447	
	[0.628]	[0.637]	[0.253]+	[0.259]+		
Preelection audit × PTB	−0.549	−0.562	0.232	0.313		
	[0.591]	[0.594]	[0.227]	[0.213]		
Observations	373	373	362	373	246	255
R^2	0.35	0.35	0.27	0.29	0.22	0.22
F-test on other interaction terms (P-values)	.97	.97	.08	.18		
F-test on corruption interaction terms (P-values)				.34		.57

Notes. Columns (1) and (2) present the results of an OLS regression where the dependent variable is the number of violations associated with corruption. Columns (3)–(6) present the results of an OLS regression where the dependent variable is an indicator for whether an eligible incumbent was reelected in the 2004 elections. All regressions include municipal characteristics: 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, margin of victory in the 2000 elections, an indicator for whether the mayors is a member of the governor's coalition, municipal police (1/0), small claims court (1/0), judiciary district (1/0); mayoral characteristics: sex (1/0 for male), age, married (1/0), education level, party dummies; and state intercepts. The regressions displayed in columns (3) and (5) also include the number of violations associated with corruption; the regressions displayed in columns (4) and (6) also include indictors for each level of corruption. The estimation sample includes all mayors who ran for reelection and is listed in each column. Robust standard errors are displayed in brackets. Significantly different than zero at 99 (**), 95 (*), 90 (+) % confidence. In columns (3) and (4), the F-test tests the joint significance of the interaction terms.

same set of models presented in Table III, 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 across corruption levels are very similar to those presented in Table III. Although the coefficients are not reported, we also test for whether our corruption measure is simply capturing a differential effect by population, education, or some other characteristic of the municipality. After allowing for differential effects in population, education, income, and inequality, our point estimate on the interaction term with corruption remains essentially unchanged at 0.074 (standard error = 0.041), compared to 0.071 (column (3) of Table V).

In columns (5) and (6), we investigate whether the audits had a differential effect among municipalities that were audited just before the elections. Audits that took place at the beginning of the program may have led incumbents to alter campaign strategies or induced opposition parties to run cleaner candidates. In column (5) we report the estimated effects of the audit policy on reelection rates based on our sample that excludes outliers, and in column (6) we present the semiparametric specification of the entire sample. In both columns, the estimates suggest that the audit policy did not have a differential effect based on when the municipality was audited. The effects of the policy on the municipalities that were audited just prior to the election are not statistically different from the average effect. Because political parties decide upon their candidates and receive campaign funds several months (if not years) before the elections, it appears unlikely that the audits induced such changes.

As another specification check of the research design, we also estimate whether the audit policy had a placebo effect on the previous mayoral elections. If the audit policy had an effect on the 2000 electoral outcomes, it would suggest that unobserved characteristics of the municipality that determine the association between reelection rates and corruption are driving the results presented in Table III. Although not reported in the table, we find no evidence that the audit policy affected either the incumbent's vote share or margin of victory in the 2000 elections. In each of the various specifications, the point estimates are close to zero and in some cases even slightly positive.

IV.C. The Effects of the Audits by Corruption and Local Media

Thus far, we have demonstrated that the audit policy had a negative effect on the reelection success of the mayors that were found to be corrupt. This reduced-form effect of the policy, although well identified from a randomized design, does not reveal the underlying mechanisms through which the policy operated. In this section we provide evidence that local radio played a crucial role in providing information to voters that allowed them to punish corrupt politicians at the polls.

Table VI presents the estimation results from a variety of specifications based on the regression model defined in equation (3). These specifications test whether the audit policy had a differential effect by both the level of corruption reported and the presence of local radio, where our measures are the number of AM radio stations in the municipality in columns (1)–(4) and the proportion of households with radios in column (5). In addition to the set of interaction terms, each regression controls for state intercepts and municipal and mayoral characteristics.

The first set of rows shows how the effects of the audits vary by both the level of corruption reported 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 municipalities that had both higher levels of reported corruption and more radio stations.[23] From the specification in column (1), the audit policy decreased the likelihood of reelection by 16.1 percentage points ($F(1,324) = 2.81$, P-value $= .09$) among municipalities with one radio station and where the audits reported three corrupt violations.[24] On the other hand, the reduction in the likelihood of reelection is only 3.7 percentage points where local radios are not available. Although radio exacerbates the audit effect when corruption is revealed, it also helps to promote noncorrupt incumbents. If corruption was not found in a municipality with local radio, the audit actually increased the likelihood that the mayor was reelected by 17 percentage points ($F(1,324) = 2.89$, P-value $= .09$). When we restrict the sample to exclude municipalities with more than five violations, the effects of the audits become even stronger (see column

23. We find similar results when we use other measures of electoral performance and restrict the sample to mayors who ran for reelection.

24. These are the municipalities in the 75th percentile of violations and number of radio stations.

TABLE VI
THE EFFECTS OF THE RELEASE OF THE AUDITS ON REELECTION RATES BY CORRUPTION LEVELS AND LOCAL RADIO

Dependent variable: Pr(reelection)	Full sample (1)	Corruption ≤ 5 (2)	Demographic interactions (3)	Demographic and institutional interactions (4)	Households w/ radio (5)
Preelection audit	−0.059	−0.033	0.296	0.208	−0.954
	[0.091]	[0.096]	[1.121]	[1.247]	[0.629]
Number of corrupt violations	−0.034	−0.013	−0.13	−0.069	−0.161
	[0.029]	[0.035]	[0.224]	[0.288]	[0.194]
Number of radio stations	−0.131	−0.150	−0.216	−0.253	
	[0.064]*	[0.063]*	[0.073]**	[0.083]**	
Preelection audit × number of radio stations	0.229	0.271	0.356	0.449	
	[0.099]*	[0.104]**	[0.115]**	[0.129]**	
Preelection audit × number of corrupt violations	0.007	−0.018	−0.236	−0.412	0.458
	[0.038]	[0.044]	[0.402]	[0.430]	[0.229]*
Number of corrupt violations × number of radio stations	0.050	0.058	0.082	0.09	
	[0.026]+	[0.025]*	[0.025]**	[0.028]**	
Preelection audit × corrupt violations × radio stations	−0.118	−0.157	−0.185	−0.238	
	[0.045]**	[0.067]*	[0.051]**	[0.064]**	

TABLE VI
(CONTINUED)

Dependent variable: Pr(reelection)	Full sample (1)	Corruption ≤ 5 (2)	Demographic interactions (3)	Demographic and institutional interactions (4)	Households w/ radio (5)
Proportion households with radio					−0.834
					[0.782]
Preelection audit × households w/ radio					1.225
					[0.752]
Number of corrupt violations × households w/ radio					0.181
					[0.243]
Preelection audit × corrupt violations × households w/ radio					−0.645
					[0.292]*
Observations	373	362	373	373	373
R^2	0.20	0.21	0.24	0.28	0.20
Demographic interactions	No	No	Yes	Yes	No
Institutional interactions	No	No	No	Yes	No

Notes. This table reports in each column the result of an OLS linear probability model where the dependent variable is an indicator for whether the mayor was reelected in the 2004 election. The sample in all columns includes all mayors that were eligible for reelection. Number of radio stations is the number of local AM radio stations in a municipality. Proportion households with radio is the total number of households that own at least one radio divided by the total number of households in the municipality. All regressions include the following municipal characteristics: population density (persons/km), percentage of the population that is literate, percentage of the population that lives in urban areas, per capita income expressed in logarithms, Gini coefficient for income, effective number of political parties in the 2000 mayor election, municipal police (1/0), small claims court (1/0), presence of a judge (1/0); mayoral characteristics: sex (1/0 for male), age, married (1/0), education level, party dummies; and state intercepts. Demographic interactions in column (3) are constructed by multiplying each one of the demographic variables by the preelection audit indicator and the number of corrupt violations. Demographic variables include: population density (persons/km), percentage of the population that is literature, percentage of the population that lives in urban areas, per capita income expressed in logarithms, Gini coefficient for income. All lower term interactions are also included in the regression. Institutional interactions in column (4) are constructed by multiplying each one of the institutional variables by the preelection audit indicator and the number of corrupt violations. Institutional variables include presence of a local judge (1/0), effective number of parties in the 2000 mayor election, and presence of a small claims court (1/0). Robust standard errors are displayed in brackets. Significantly different from zero at 99 (**), 95 (*), 90 (+) % confidence.

(2)). The reduction in the likelihood of reelection in municipalities with three violations and radio becomes 29 percentage points ($F(1,314) = 4.02$, P-value $= .05$). Overall the results in columns (1) and (2) suggest that the presence of local radio enables voters to further punish corrupt politicians once the anticorruption program reveals the true extent of their corruption.

Our research design, while randomized over which municipalities were audited, was unfortunately not randomized on the availability of radio stations. 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 reelection outcomes. One possibility, for instance, is that our measure of radio availability simply captures the effects of the audits across municipalities with different education levels. If more educated citizens are better informed about the corrupt activities of politicians, then the effect of the audits may be smaller in municipalities with more educated citizens. Alternatively, a more educated citizenry may also be more politically engaged and willing to take action against corrupt politicians, in which case the effects may be more pronounced in municipalities with more educated citizens (Glaeser and Saks 2006). Another potential confound is population size. If information about the irregularities of politicians flows better in larger cities, then the effects of the audits might be smaller. Finally, as voters become more economically diverse, electoral choices may be based on redistribution rather than on the honesty of politicians (Alesina, Baqir, and Easterly 2002; Glaeser and Saks 2006). Hence the effects of the audits might be smaller in municipalities with high income inequality.

To test for these potential confounds, we include in the estimation of equation (3) a series of interaction terms where we allow the preelection audit indicator and the number of corrupt violations to vary with several characteristics that might be correlated with the number of radio stations in the municipality.[25] Column (3) shows the results from introducing interactions with the following demographic characteristics: population density, literacy rates, share of urban population, per capita income, and the Gini coefficient. Our estimates of the differential impact of the audit by corruption level and the presence of local radio remain significant and the magnitudes remain similar. Even when we augment the

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

specification to include additional interactions terms with institutional characteristics, such as the presence of a local judge and political competition (column (4)), our estimate of the triple interaction between radio, corruption, and preelection audit remains remarkably stable and statistically significant.

Although not reported, we do not find any significant differences on the impact of the audits by literacy rates.[26] This result, although surprising, might be due to the fact that political participation is fairly high in Brazil. Because voting is compulsory, there is less of an educational gradient in political involvement. We also do not find any differential effects of the audits by the level of income inequality. However, we do find larger and significant impacts in municipalities where the population is less densely distributed. We interpret this as complementary evidence that the change in voting behavior occurred in places where people had less access to previous information due to lower communication flows between citizens.

In the last column of Table VI we examine whether our findings are robust to using an alternative measure of local radio. We re-estimate equation (3) using the share of households that own radios instead of the number of local AM radio stations. The results shown in column (5) suggest that the impact of the disclosure of corruption violations before the municipal election increases with the share of households that own radios. In those municipalities where a larger share of households own a radio (share of radio ownership is 90%), the disclosure of three corruption violations before the election decreased reelection likelihood by 22 percentage points ($F(1,325) = 4.44$, P-value $= .03$).[27] However, in locations with lower radio ownership (75% of households own a radio), the reduction in reelection likelihood was only 8 percentage points.

As a final robustness check on the importance of radio, we investigate whether the presence of other media sources influenced voters' awareness of the audit findings and thus affected electoral outcomes. We test whether the policy had a differential effect by local newspapers and television ownership. We find no significant

26. Similar results were obtained using two other measures of education—average years of schooling for adults and the proportion of adults with secondary education.

27. Using this alternative measure of radio may understate the impact of the program. There are several households that may own a radio but, because their municipality does not have its own radio station, may not have heard about the audit reports.

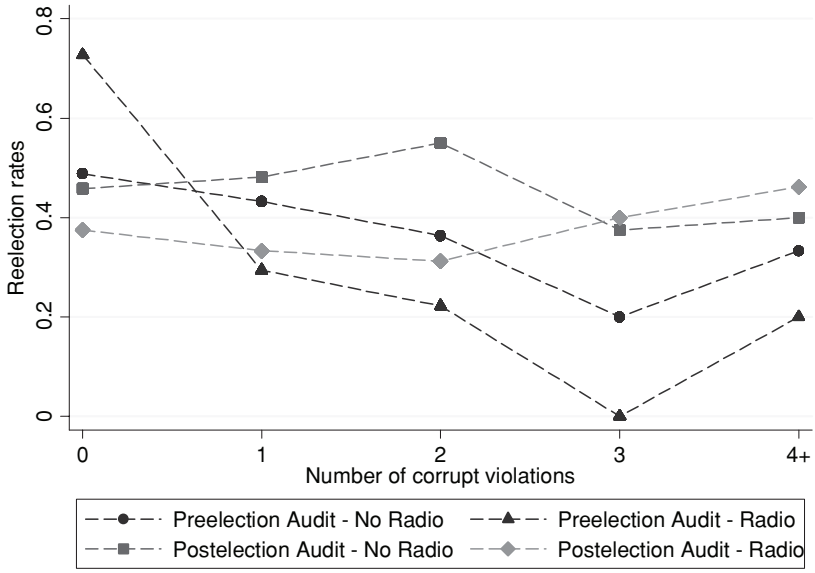

FIGURE IV
Relationship between Reelection Rates and Corruption Levels

Notes. Figure shows the unadjusted relationship between the proportion of first-term mayors who were reelected in the 2004 elections and the number of corrupt incidents reported in the audit reports for municipalities audited before and after the elections and the existence of local radio. The points represented by circles are calculated for the municipalities that were audited before the elections and do not have a local AM radio station. The points represented by triangles are calculated for the municipalities that were audited before the elections and have a local AM radio station. The points represented by squares are calculated for the municipalities that were audited after the elections and do not have a local AM radio station. The points represented by diamonds are calculated for the municipalities that were audited after the elections and had a local AM radio station. The figure was calculated for our entire sample of 373 municipalities based on data from Brazil's Electoral Commission, the CGU audit reports, and IBGE.

differential impacts of the disclosure of corruption information by the presence of local newspapers or the proportion of households with a television set. Given Brazil's generally low circulation rates and low literacy (particularly in small municipalities) and the lack of local news in television broadcasts, these results are not too surprising. Moreover, they emphasize the importance of radio in conveying local information in Brazil's smaller municipalities.

To get an even better sense for the estimates presented in Table VI, Figure IV plots the 2004 reelection rates among eligible mayors against the number of corrupt violations found in the audit, distinguishing the unadjusted 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 a slight negative association between reelection rates and corruption, consistent with the effects of the audit. However, when they are compared with 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 triangles), reelection rates fall drastically, as the number of corruption violations increases. In comparing these two relationships, we also observe the electoral advantage noncorrupt mayors of municipalities with local radio receive with an audit, as there is a 29-percentage-point difference in reelection rates between municipalities with and without local radio.

For municipalities audited postelection, there is little distinction by radio. Among these municipalities, the relationship between reelection 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 reelection rates tend to be higher in the municipalities audited postelection but without local radio.

Figure IV also illustrates how the existence of radio may influence 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 III). As radio serves to disseminate the findings of the audit more broadly, noncorrupt politicians 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 almost at zero violations. Thus, not only does the audit reduce the incumbent's likelihood of reelection independent of his corruption level, but also it may suggest that citizens make systematically more mistakes in their estimation of corruption when there exist fewer media.[28]

IV.D. Discussion

The results thus far support a simple model where the public release of the audits provided new information to voters about

28. This finding relates to studies that examine the effects of media on corruption levels (Ahrend 2002; Brunetti and Weder 2003).

corrupt practices of their mayors. Voters used this information to update their priors and punish politicians that were found be more corrupt than on average. The audit effects were in turn more pronounced in areas where the local media could disseminate these findings more widely.

These findings, however, may also suggest an alternative interpretation. Voters may not have punished politicians who were found to be corrupt, but rather incumbents who were found to be incompetent. If voters had interpreted these corruption incidents (or rather the inability to hide these acts of corruption) as a signal of poor political skills, then these results would not reflect a dislike of corruption per se, but rather a dislike of political incompetence. Unfortunately, without data on political ability, this alternative explanation is difficult to test.

There are, however, at least three reasons that this interpretation is less plausible. First, if political ability also influences electoral performance, then one would expect a negative relationship between the number of violations and reelection rates for the municipalities that were audited after the election. But as Figure III and Table III demonstrate, there is no association between the number of corrupt incidents and reelection rates for these municipalities. Second, as shown in Table V, our estimates are robust to allowing the audits to have a differential effect by the incumbent's margin of victory in the previous election. If the margin by which a politician is elected indicates political skill, then this would suggest that the audit's differential effects by corruption are not capturing the effect of political competency. Third, if political ability is correlated with the number of violations, then one might also expect the number of violations to be correlated with another measure of ability—mayor's education level. But not only is the mayor's education level not correlated with our measure of corruption, but our estimates are robust to controlling for a differential effect by mayor's education. Thus, the idea that voters punished incompetent politicians rather than corrupt politicians appears less likely.

Another possibility is that the effects of the audits on reelection rates may have come through channels other than information. For example, the audits may also have led the incumbent to alter his campaign strategies or induced the opposition parties to run a cleaner candidate or campaign more intensely. These possibilities are consistent with the newspaper articles (discussed in the background section) reporting that

the information disclosed in the audit reports were widely used by either the opposition parties or the incumbent himself. Without data on the actual campaigns, it is difficult to test for these potential mechanisms. However, there are at least two reasons that the data are inconsistent with these other potential mechanisms. First, if political parties were running cleaner candidates or altering their campaign platforms based on the audit reports, then presumably the effects of the audits would differ according to when the municipality was audited. But as Table V reports, this is not the case. Second, if these were the principal mechanisms, one would have expected the audit information to have been used more in the campaigns where political competition was more intense. Thus, the differential effects of the program by radio would have been attenuated once we accounted for the differential effect by electoral competition. Because our results remain unaltered, it is likely that campaigning and local radio produce a complementary effect, with radio increasing the efficacy of the information transmitted in the political campaigns.

Another possibility is that mayors that were revealed to be corrupt received less campaign contributions, which lowered their likelihood of reelection. To test this hypothesis, we estimate a model of the effects of the audits on reelection rates, including an interaction between preelection audit and the value of campaign contributions received per capita. Although not reported, we do not find any differential effect of the audits by the level of campaign contributions.[29] Moreover, the coefficient on the interaction between corruption irregularities and the preelection audit remain almost identical when we account for campaign contributions.[30] This mechanism is also inconsistent with the fact that the effects of the audits do not differ by when the audits took place (see Table V). The audits had a similar effect even among municipalities that were audited close to the election, when presumably incumbents had already received most of their campaign contributions.

V. Conclusions

In 2003 Brazil's federal government began to select municipalities at random to audit their expenditures of federally

29. We also test whether the audits had an impact on the amount of campaign contributions received, but we do not find any effects.

30. These results should be interpreted with caution because the data on campaign contributions are self-reported and contain 15 missing values for our sample.

transferred funds. This paper exploits the program's randomized auditing and public dissemination of its findings to study the electoral impact of disclosing information about politicians' performance. We show that the public dissemination of corruption in local governments had a significant effect on incumbents' electoral performance. For instance, compared to municipalities audited after the election, the policy reduced the incumbent's likelihood of reelection by 7 percentage points (or 17%) in municipalities where at least two violations associated with corruption were reported. These results highlight the importance of an informed electorate to enhance the accountability of politicians.

This paper also contributes to a growing literature that emphasizes the role of media in influencing political outcomes. We show that corrupt politicians were punished relatively more in places where local radio stations were present to divulge the findings of the audit reports. Moreover, while local radio exacerbates the audit effects when corruption is revealed, it also helps to promote noncorrupt incumbents by drastically increasing the likelihood of their reelection. The estimates are robust to controlling for access to other sources of media and local characteristics that are correlated with the presence of local radio (e.g., education, population, urbanization). Using the share of households that own a radio as an alternative measure of radio penetration also produces similar results. In sum, our findings highlight the role of local media in affecting political outcomes and particularly in helping to screen out bad politicians and promote good ones.

Our findings are important for understanding the effects of political selection on voter welfare. If the release of information about the performance of politicians enables voters to select better policy makers, then presumably over time, the quality of government will improve. To understand whether the public dissemination of the audits will upgrade the quality of the pool of politicians, reduce corruption, and improve public policy remains important topics for future research.

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