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THE EFFECTS OF REVEALED CORRUPTION ON VOTER ATTITUDES AND
PARTICIPATION:
EVIDENCE FROM BRAZIL

BY

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DISSERTATION

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Abstract

How do voters respond to information about political corruption? Prior research provides conflicting evidence about whether information about political corruption drives voters to participate or withdraw from electoral politics, and about whether those who participate are able to remove corrupt politicians from office. This three-paper dissertation makes several contributions to this literature. The first chapter studies voters' ability to remove corrupt politicians by reevaluating Ferraz and Finan's (2008) seminal study about the effects of corruption audits in Brazil. By attempting to reproduce their results from the 2004 Brazilian municipal elections and by extending their analysis to the 2008 and 2012 elections, I call into question their well-known result that voters react to information about political corruption by removing incumbent politicians from office. The second chapter questions whether revealing corruption causes voters to withdraw from the political system entirely. Focusing on electoral turnout and the proportion of blank and null votes cast in Brazilian municipal elections, I find that releasing information about high levels of local government corruption actually causes citizens to become more engaged in the political system over time. The third chapter studies voters' political attitudes in order to understand why participation increases following the revelation of corruption. I hypothesize that because the institution that revealed corruption in Brazil was a governmental institution, this information had a positive impact on voters' political attitudes. Using survey data, I find evidence that revealing corruption increases citizens' trust in institutions and their sense of political efficacy, thereby increasing their propensity to participate in elections. Despite discouraging results in the first chapter, my findings in chapters 2 and 3 highlight additional ways in which information about corruption can enhance political accountability by increasing citizen engagement in the electoral process.

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Chapter 1

Rethinking the Effects of Exposing Corruption on Electoral Outcomes in Brazil: 2004 and Beyond

1.1 Introduction

In June of 2006, a team of auditors from Brazil's Supreme Audit Institution, the Contoladoria Geral da União (CGU) completed their audit report of the municipality Boa Vista do Buricá in the state of Rio Grande do Sul. The report, which was published on the CGU's website nine months later, detailed numerous financial irregularities, including fraud in bidding procedures worth hundreds of thousands of reais. The national news website *Universo Online* (UOL) picked up the story of Boa Vista do Buricá's and several other municipalities' corruption (Universo, 2007). It was also covered by the *Tribuna do Paraná*, a more local news source, which reported 'a lack of accountability' with 'suspected favoritism' (Brasil, 2007). Despite strong evidence of corruption and media coverage by several outlets, the mayoral incumbent with a documented record of corruption was reelected in 2008 with more than 57% of the vote.

Stories of corruption without electoral consequences are common not only in Brazil, but also in numerous other countries where corruption is a pervasive problem. Many prominent quantitative and experimental studies of the electoral consequences of corruption show, at best, modest effects of providing voters with information about corruption on electoral outcomes (Peters and Welch, 1980; Reed, 1999; Humphreys and Weinstein, 2012). Ferraz and Finan's (2008) study challenges the common wisdom that corruption does not have electoral consequences. In their seminal study, Ferraz and Finan (2008) provide convincing empirical evidence that the information contained in Brazil's municipal audit reports had a significant effect on mayors' electoral performances in the 2004 municipal elections. By taking advantage of the random selection of Brazilian municipalities for auditing, and the release of these audit reports before and after the 2004 election, Ferraz and Finan (2008) provide evidence that each corruption violation exposed in audit reports led to an eight percentage-point drop in incumbents' probability of reelection. What is not clear is whether Ferraz and Finan's (2008) results are confined to the 2004 election, or whether they describe a more general

phenomenon that will repeat in future municipal elections.

Replicating Ferraz and Finan's (2008) study in the 2008 and 2012 elections is important for two reasons. First, when municipal elections took place in October of 2004, the Brazilian audit institution the *Controladoria Geral da União* (CGU) had only been operating and conducting municipal audits for about 17 months. The newness of both the CGU and the municipal audit program likely caused the media and general public to pay more attention to audit reports in 2004 than in later years. Corruption uncovered in these audits, therefore, may have had an oversized effect on electoral outcomes in 2004. Second, although academics and practitioners have been quick to call Ferraz and Finan's (2008) results 'strong' Larreguy et al. (2014) and 'influential' (Gertler et al., 2016), Ferraz and Finan's results are not as conclusive as the literature purports. The effect of exposing corruption on the probability of reelection is not significant for their entire sample. Only when Ferraz and Finan (2008) restrict the sample to municipalities with 5 or fewer violations does the coefficient for releasing corruption information on the probability of reelection reach significance. Although Ferraz and Finan also demonstrate that information about corruption decreases the incumbent's margin of victory and vote share, this is not much different from existing studies that demonstrate modest effects of exposing corruption on electoral outcomes (Peters and Welch, 1980; Humphreys and Weinstein, 2012).

Understanding the ongoing impact of Brazil's audits on electoral outcomes is important not only for its theoretical implications for political accountability, but also for its practical implications for supreme audit institutions' (SAI) abilities to achieve their goals. The use of SAIs as institutions of accountability is widespread across the globe: 188 countries and territories are members of The International Organization of Supreme Audit Institutions (INTOSAI), a worldwide affiliation. SAIs use audits to detect the misuse of public resources, and as a result, are important actors in efforts to minimize corruption. According to a report by the World Bank, audit institutions are increasingly making their reports more accessible to the public, and are creating citizen participation mechanisms with the purpose of "increas[ing] [citizens'] demand for accountability from public administration and thus enhanc[ing] the credibility of governmental policies and programs" (Nino, 2010, pp 1). Even the CGU's website explicitly states that they disseminate their municipal audit reports to media outlets in order to 'promote the exercise of citizen monitoring' and to 'ensure the participation of society in the monitoring of acts and facts of public management.' Although Ferraz and Finan (2008) provided evidence that making audit reports public and encouraging media outlets to disseminate audit information leads to normatively desirable electoral outcomes, few practitioners would

be satisfied with evaluating the impact of a new program only one year after its creation.

Therefore, this chapter will have two objectives. First, I will attempt to reproduce Ferraz and Finan's study of the effects of corruption information in the 2004 Brazilian municipal elections. As one of the most highly cited articles in the corruption and electoral accountability literature, and an article with one of the most promising findings for thinking about the provision of information as a means of strengthening political accountability, it is important to verify that we can independently reproduce the results. My second objective is to extend Ferraz and Finan's analysis to the 2008 and 2012 municipal elections using the CGU's codings of their municipal audit reports, and my own codings of the CGU's municipal audit reports. Because the CGU's audit program had been in effect for just over one year when the 2004 municipal elections took place, its novelty and extensive media coverage could have had an oversized effect on political behavior. By testing the effect of releasing audit results using Ferraz and Finan's (2008) framework in the 2008 and 2012 municipal elections, I will be able to look at whether there is evidence that audit information continued to affect electoral outcomes.

Regarding the reconstruction of the Ferraz and Finan (2008) dataset, I was unable to exactly reconstruct several variables. Among the problems I have encountered are incorrectly identified municipalities, differences in the distribution of audits released before and after the 2004 election, and a different sample size for the AM radio data. Because the AM radio data used in the reproduction dataset is from the same data source as Ferraz and Finan's (2008) AM radio data, this is especially puzzling. My descriptive statistics are also considerably different from those reported in Ferraz and Finan (2008). Notably, I find a significant difference in the average number of corruption violations between the group of municipalities audited before the 2004 election, and the group of municipalities audited after the 2004 election. In light of this, I am unable to reproduce Ferraz and Finan's preferred model, which should show a significant negative effect of audit publication on the probability of reelection, conditioning on the level of corruption. I am also unable to reproduce their analyses which show that the effects of audit information is conditional on the presence of local radio stations.

In other models, I do find some evidence that the audits had an effect on electoral outcomes, conditioning on the level of corruption. For example, I am able to reproduce Ferraz and Finan's general results (sign and significance) for their quadratic model, which shows that reelection rates decrease as corruption increases for municipalities with fewer than five violations, but then increases for municipalities with five or more

violations. These findings, however, are undermined by the significant difference in the average number of corruption violations between the treatment and control groups. Because the average number of corruption violations in municipalities audited after the 2004 election are significantly higher than the average number of corruption violations in municipalities audited before the 2004 election, we cannot rule out the possibility that average treatment effects of audits conditioning on corruption are due to underlying differences between the two groups.

When I extend Ferraz and Finan's analysis to the 2008 and 2012 elections using the CGU's codings of their own audits, I fail to find any effect of audit release on electoral outcomes, conditioning on the level of corruption. Using my own codings of audit reports released around the 2008 election, I also find no evidence that audit publication decreases incumbents' probability of reelection when conditioning on the level of corruption.

The results of these exercises strongly undermine Ferraz and Finan's conclusions about the importance of information and local media for improving political accountability. The provision of information about corruption does not appear to have a significant effect on incumbents' probability of reelection in 2008 and 2012, and it has only small effects in some models for the 2004 election. These effects are undermined by the significant difference in corruption violations among municipalities audited before and after the election. Although a large body of research argues that local media enhances political accountability, the findings here do not support that conclusion.

The first half of this paper will focus on reconstructing Ferraz and Finan's (2008) data and reproducing their results for the 2004 election. The second half of this paper will focus on replicating Ferraz and Finan's (2008) analysis for the 2008 and 2012 elections. In Section 2, I will outline the theory and extant results in the literature concerning information provision and electoral accountability. I will then turn to describing Ferraz and Finan's (2008) article. Section 3 details how I reconstructed Ferraz and Finan's dataset using their own corruption data and data from several Brazilian institutions. In Sections 4 and 5, using the reproduction dataset, I will attempt to replicate Ferraz and Finan's descriptive statistics and analyses, discussing the possible reasons for discrepancies, and their implications for our understanding of corruption and political accountability. In Sections 6-8, I will extend Ferraz and Finan's analysis to the 2008 and 2012 elections using the CGU's coding of their own audit reports. Finally, in Section 9, I will use my own coding of the CGU's audit reports that more closely follow Ferraz and Finan's (2008) coding rules in order to provide robustness

check of the first 2008 extension. Section 10 concludes.

1.2 Information Provision and Accountability

The social contract argument for democratic governance posits that governments exist by the consent of the governed. As such, elected office holders should have to answer to voters for the ways in which they exercise the powers invested in their office. Elections can function as the means by which voters hold politicians accountable. The pure accountability model of electoral institutions entails a principal-agent relationship in which the principals (citizens) make agents (politicians) responsive by conditioning reelection on previous performance in office (Key, 1966; Ferejohn, 1986; Barro, 1973). In order to function well, this pure accountability mechanism requires that politicians attempt to justify their actions to their constituents and that citizens subsequently subject those politicians to the appropriate consequence: reelection or replacement (Przeworski et al., 1999; Fearon, 1999; Schedler, 1999; Maskin and Tirole, 2004). By definition, corruption is a misuse of public authority for personal gain and one component of performance that voters should factor into their electoral calculations. All else equal, office holders who engage in honest (non-corrupt) governance should be rewarded with reelection while those who engage in dishonest (corrupt) governance should be sanctioned by being replaced in the following election.

The literature on performance-based voting generally agrees that economic performance has a strong impact on voters' evaluations of politicians and vote-choices (Fiorina, 1981; Whitten and Palmer, 1999; Anderson, 2000). The strength of the impact may vary depending on factors such as the amount of electoral party competition (Van der Brug et al., 2007), economic openness (Duch and Stevenson, 2010), and the degree to which policy-making power is concentrated rather than dispersed (Becher and Donnelly, 2013).

The literature specifically about corruption performance-based voting, however, provides conflicting evidence about whether providing information about corruption actually leads to normatively desirable electoral outcomes. Peters and Welch (1980), for example, find that US representatives with formal corruption charges lose some of their expected vote share but are almost all reelected. Similarly, Welch and Hibbing (1997) find that charges of corruption in congressional elections cause incumbents to lose votes in general elections but 'rarely cause incumbent U.S. Representatives to resign, retire, or lose in primary races' (Welch and Hibbing, 1997, p.237). The 2009-10 UK expenses scandal was the subject of several studies about retro-

spective voting, most of which found that MPs who were implicated in the scandal fared only slightly worse in the following election than non-implicated MPs (Eggers and Fisher, 2011; Pattie and Johnston, 2012). Vivyan et al. (2012) expand on these studies of the UK expenses scandal and conclude that the failure of accountability in the 2009 Parliamentary elections occurred even though 1) information about implicated MPs was readily available to voters, and 2) information about the implicated MPs strongly influenced voters' perceptions of corruption. Finally, in a field experiment in Mexico, Chong et al. (2015) find that information about severe corruption only decreased support for the corrupt incumbent by a small amount: about 0.43 percentage points. The incumbent's loss in support, however, is likely due to an overall decrease in turnout; the challenger party's vote-share in corrupt municipalities also decreased about 0.86 percentage points, and was associated with an overall 2.5 percentage point drop in turnout.

In one of the most well-cited articles on the effects of exposed corruption on electoral outcomes, Ferraz and Finan (2008) use Brazil's municipal audits to examine the effects of exposing local corruption to the public. They find that corrupt politicians indeed lose support and are more likely to lose office than honest politicians. Highlighting the importance of a strong media for the dissemination of information about corruption, Ferraz and Finan find, in particular, that corrupt politicians in municipalities with local AM radio stations are electorally punished more severely than corrupt politicians in municipalities without local AM radio stations.

In the nearly 10 years since its publication, Ferraz and Finan's article has become an important piece of research for academics and practitioners alike. In a manuscript about the role of media in electoral sanctioning in Mexico, Larreguy et al. (2014) claims that Ferraz and Finan (2008) provide the strongest evidence in the literature that 'the media actually inform significant numbers of voters about the behavior of malfeasant politicians' (Larreguy et al., 2014, pp. 2). In a book that itself was cited more than 500 times within two years of publication, Gertler et al. (2016) calls Ferraz and Finan (2008) an "influential" evaluation of the effects of audits on political performance. Ferraz and Finan's research also has been cited numerous times by the CGU and by researchers associated with the CGU as evidence of the CGU's effectiveness (Santana, 2008; CGU, 2008, 2009, 2011).

1

¹Their research has been referred to as 'seminal' (La Ferrara, 2016; Colonnelli and Prem, 2017; Lagaras et al., 2017), and their design and data have been called 'clever' (Healy and Malhotra, 2013) and 'innovative' (Ashworth, 2012). According to a blog called "DemocracySpot", Ferraz and Finan (2008) caused the CGU's audit program to become 'famous' (Peixoto, 2012). The Poverty Action Lab summarized Ferraz and Finan's (2008) work, and wrote about numerous 'Policy Lessons' that practitioners should draw from their research, including "In response to credible information, voters update their beliefs about politicians and change their voting behavior" and "Local media can enhance political accountability and candidate selection" (Lab, 2008).

1.3 Ferraz and Finan (2008) Data Reconstruction

Ferraz and Finan (2008) were among the first in the political accountability literature to use government audits as an objective measure of corruption. The audit reports used by Ferraz and Finan (2008) come from an audit program run by Brazil's supreme audit institution, The Controladoria Geral da União (CGU).² In 2003, the federal government granted the CGU the ability to randomly choose municipalities with populations below 500,000 for audit. The CGU began with 26 municipal audits in 2003, but raised the number to 50 and then 60 municipalities per lottery in 2004. The public is invited to all rounds of the audit lottery, which take place every 2-4 months. After a municipality is chosen, a group of 10-15 auditors are sent to each municipality to look for irregularities in the municipal spending of federal transfers. Once the auditors have completed their inspection of municipal accounts, construction, and public goods delivery, they create a comprehensive report detailing all irregularities they uncovered. These reports are posted on the CGU's website and disseminated to all levels of the government. There is also ample anecdotal evidence that the information contained in these reports reaches voters, and is directly attributed to the CGU in reports.³

Ferraz and Finan (2008) define political corruption as 'any irregularity associated with fraud in procurement, diversion of public funds, or over-invoicing' (pp. 710). Of all violations described in an audit report for a given municipality, Ferraz and Finan (2008) only use the number of violations that involve fraud in procurement, diversion of public funds, or over-invoicing as their measure of corruption. Ferraz and Finan take advantage of the randomized selection of municipalities for audit before and after Brazil's 2004 municipal elections in order to estimate the effects of disclosing information about corruption on electoral outcomes. Their assumption is that the 'treatment group' of 205 municipalities that had their audit information released before the 2004 election are not systematically different from the 'control group' of 168 municipalities that had their audit information released after the election.⁴

In recreating their dataset, I use Ferraz and Finan's coding of the audit reports to measure corruption. These data were obtained from Frederico Finan by way of an email exchange with Professor Matthew Winters of the University of Illinois in October of 2013. The dataset contains two variables: the number of corruption violations associated with each municipal audit, and a municipal indicator from the *Instituto Brasileiro de Geografia e Estatísticas* (IBGE). I cross referenced the municipal indicator with the CGU's audit reports to

²The Controladoria Geral da União (CGU) was Brazil's supreme audit institution until 2016, when it was absorbed into the the Ministério da Transparência, Fiscalização e Controle (MTFC). Because all data in this paper comes from CGU audits, I will refer to Brazil's supreme audit institution as the CGU from this point forward.

³For a few examples, see (Oliveira, Edilson, 2016; ClickPB, 2008; Melo Aranha, Ana Luiza, 2016)

⁴From this point forward I will refer to municipalities that were audited and had their audit reports published before the 2004 election as 'pre-election municipalities' and I will refer to municipalities that were audited and had their audit reports published after the 2004 election as 'post-election municipalities.'

code the lottery numbers associated with each municipal audit.

In this process, I discovered that two municipalities, Riberão Preto, São Paulo, and São Francisco do Piauí, Piauí, did not have associated audit reports for the time period covered by Ferraz and Finan's analysis (2003-2005). Two municipalities with similar names were audited within the time frame but were not included in Ferraz and Finan's sample: Riberão Branco, São Paulo and São Francisco de Assis do Piauí, Piauí. In order to replicate FF's dataset exactly, I coded the two municipalities for which I originally could not identify lottery numbers with the latter two municipalities' lottery numbers.

Because the corruption count and municipal identifier came directly from the authors, it is likely that Ferraz and Finan used the IBGE codes from the former two municipalities when coding other variables. Therefore, across all variables in their dataset, there is likely a mismatch between the municipalities that were audited by the CGU (Riberão Branco, São Paulo, and São Francisco de Assis do Piauí, Piauí) and the municipalities that were included in their analysis (Riberão Preto, São Paulo, and São Francisco do Piauí, Piauí). Although it is possible that the rest of their accompanying data are from the correct municipalities, and that only the IBGE identifiers are incorrect, this is unlikely: municipalities in Brazil frequently change names, and occasionally have multiple spellings of the same name. Municipal identifiers are the best way to match municipalities with governmental data, because the IBGE identifiers do not change across time, and are available on most government data sets. Therefore, as I continued to recreate the Ferraz and Finan dataset, I did not change the IBGE codes. I collected the rest of the variables using only the codes contained in the dataset provided by Finan.

Next, I coded each incumbent's 2004 electoral outcome. Because Ferraz and Finan's sample only includes mayors who were eligible to run for reelection in 2004, I used the *Tribunal Superior Eleitoral's* (TSE) electoral return data to code mayors as 1) having chosen not to run in the 2004 municipal elections, 2) as having run and lost the 2004 municipal elections, or 3) as having run and won the 2004 municipal elections. In this process, I discovered that one mayor (José Wilson Alves Chaves of Pacajus, Ceará) was term-limited, and therefore should not have been included in the sample, which only includes mayors eligible for reelection. Because his name was not on the 2004 ballot in Pacajus, CE, it is likely that Ferraz and Finan coded Alves as having chosen not to run for reelection. In order to recreate the FF dataset as faithfully as possible, I nonetheless coded this incumbent as "did not run."

Descriptive Statistics

Ferraz and Finan present their descriptive statistics by reporting the mean political outcomes, municipal characteristics, and mayoral characteristics of their sample, separated by whether the municipality was audited before or after the election. Tables 1.1, 1.2, and 1.3 report the Ferraz and Finan (2008) descriptive statistics next to the descriptive statistics that come from the reproduction dataset. None of the descriptive statistics from the reproduction dataset exactly match the means, differences or standard errors stated in the original article, though some are extremely close. Most of Ferraz and Finan’s substantive conclusions are similar to the conclusions drawn from the descriptive statistics that come from the reproduction dataset. For example, Ferraz and Finan note that most of the municipalities in their sample dataset are rural, poor, and have higher illiteracy rates than is average in Brazil. This is also true of the reproduction dataset: about 38% of the population in the reproduction dataset live in a rural area, compared to 17% in all of Brazil, and about 21% of the population in the reproduction dataset are illiterate, compared to 11% in all of Brazil. This is consistent with what we would expect given the sample of municipalities included in the CGU’s municipal audit lottery: in 2004, only municipalities with populations higher than 450,000 were eligible for randomized audit. Therefore, more densely populated, urban municipalities are not included in the sample.

Despite the similarities between the descriptive statistics calculated from Ferraz and Finan (2008) and reproduction datasets, there are some notable differences. For example, in the summary statistics from the reproduction dataset presented in Table 1.3, there is a larger difference between the average number of corruption violations in municipalities audited before the 2004 election and the average number of corruption violations in municipalities audited after the 2004 election (0.434 in the reproduction dataset vs 0.369 in the Ferraz and Finan (2008) dataset). More importantly, the difference between average corruption violations in pre-election municipalities and average corruption violations in post-election municipalities is statistically significant ($p < 0.05$). This difference is highly problematic for causal inference. While we might expect treatment and control municipalities to have a small percentage significantly different covariates with significant differences, the average number of corruption violations is one of our key independent variables of interest. If the difference in corruption violations among pre-election municipalities and post-election municipalities is indicative of other substantive differences between the treatment and control groups, any effects of exposed corruption on electoral outcomes that we uncover may actually be caused by another underlying variable.

The average number of corruption violations, however, is not the only important difference between the pre- and post-election groups. The average number of radio stations in municipalities with at least one radio station also presents some noticeable differences. Whereas Ferraz and Finan find that municipalities with AM radio stations average about 1.32 stations, I find that municipalities with radio stations average about 1.7 stations. I also find that, overall, about 42% of municipalities have a radio station, whereas the original data in Ferraz and Finan (2008) show that about 27% of municipalities have a radio station. Because I collected the AM radio data from the 1999 municipality survey, *Perfil dos Municípios Brasileiros: Gestão Pública*, the source cited in Ferraz and Finan (2008, pp 711-712), I am unable to explain the discrepancy.⁵

⁵Although Ferraz and Finan argue that their AM radio variable is a measure of community radio stations, this is not the case. Community radio stations, which are awarded to local community organizations on a competitive basis, can be either AM or FM (Boas and Hidalgo, 2011). Additionally, not all AM radio stations are community radio stations.

	Postelection		Preelection		Difference		St. Error	
	Rep	FF	Rep	FF	Rep	FF	Rep	FF
Reelection rates in 2004 elections	0.407	0.413	0.408	0.395	-0.001	0.018	0.051	0.045
2004 reelection rates, those that ran	0.586	0.585	0.568	0.559	0.019	0.026	0.061	0.044
Ran for Reelection in 2004 election	0.695	0.707	0.718	0.707	-0.024	-0.001	0.048	0.060
Number of Parties in 2000 election	2.850	2.881	2.956	2.933	-0.106	-0.052	0.108	0.140
Margin of Victory in 2000 election	0.146	0.142	0.143	0.131	0.003	0.012	0.016	0.019
Mayor's vote share in 2000 election	0.530	0.529	0.524	0.525	0.006	0.004	0.011	0.013

Table 1.1: Political Characteristics, 2004

	Postelection		Preelection		Difference		St. Error	
	Rep	FF	Rep	FF	Rep	FF	Rep	FF
Age	51.665	47.500	51.883	48.000	-0.219	-0.500	0.951	0.900
Years of education	11.599	12.200	10.971	12.000	0.628	0.300	0.396	0.300
Male	0.964	0.960	0.942	0.940	0.022	0.020	0.022	0.030
Member of PSB	0.030	0.083	0.039	0.072	-0.009	0.011	0.019	0.044
Member of PT	0.030	0.030	0.049	0.048	-0.019	-0.018	0.020	0.023
Member of PMDB	0.269	0.254	0.165	0.172	0.104	0.082	0.043	0.047
Member of PFL	0.162	0.178	0.170	0.163	-0.008	0.015	0.039	0.052
Member of PPB	0.108	0.030	0.131	0.038	-0.023	0.009	0.034	0.017
Member of PSDB	0.132	0.130	0.170	0.167	-0.038	-0.037	0.037	0.043

Table 1.2: Mayoral Characteristics, 2004

	Postelection		Preelection		Difference		St. Error	
	Rep	FF	Rep	FF	Rep	FF	Rep	FF
Population Density	0.56	0.57	0.73	0.73	-0.16	-0.16	26.36	0.33
Literacy Rate	0.79	0.81	0.78	0.80	0.01	0.01	1.26	0.03
Urban	0.62	0.62	0.62	0.62	-0.00	0.00	0.02	0.05
Log Income	7.87	7.64	0.23	0.14	7.87	7.64	0.23	0.14
Income Inequality	0.57	0.55	0.56	0.54	0.01	0.00	0.01	0.01
Zoning Laws	0.31	0.29	0.20	0.21	0.11	0.08	0.05	0.07
Economic Incentives	0.64	0.66	0.59	0.58	0.06	0.07	0.05	0.06
Paved Roads	59.19	58.99	57.67	58.30	1.51	0.69	3.00	7.74
Size public employment	0.41	0.42	0.42	0.43	-0.01	-0.01	1.18	0.02
Municipal Guards	0.20	0.20	0.20	0.21	-0.01	-0.01	0.04	0.07
Small Claims Court	0.44	0.38	0.44	0.34	0.00	0.04	0.05	0.08
Judiciary District	0.60	0.59	0.59	0.56	0.01	0.03	0.05	0.07
Number Newspapers	3.08	3.58	1.52	2.21	1.56	1.37	1.41	0.79
Munic with a radio station	0.46	0.31	0.39	0.24	0.07	0.07	0.05	0.06
Number Radio Stations	1.54	1.37	1.84	1.29	-0.30	0.08	0.17	0.11
Resources Audited	9,140,968	5,770,189	5,802,301	5,270,001	3,338,667	500,188	3,591,753	1,361,431
Number of corruption violations	1.96	1.95	1.52	1.58	0.43	0.37	0.16	0.36

Table 1.3: Municipal Characteristics, 2004

Ferraz and Finan report the distribution of corruption incidents reported in audits in their Figure I, which I present in Figure 1.1b. In Figure 1.1a, I report the reproduction of their figure based on the reproduced coding of whether the audit reports were released before or after the election. Consistent with Ferraz and Finan, I coded audit reports in lotteries 2-9 as being released before the election, and audit reports in lotteries 10-14 as being released after the election.⁶

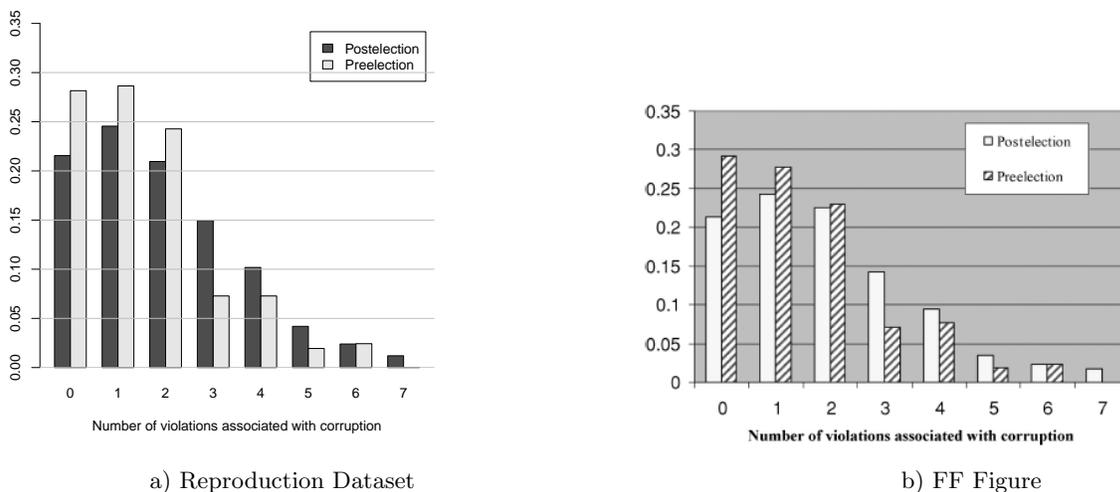


Figure 1.1: Distribution of Corruption Violations by Pre- versus Post-election Audits

Although Figure 1.1a appears generally similar to Figure 1.1b, there are some noticeable differences. For example, among municipalities with two corruption violations, there is a greater disparity in the proportion of violations between the pre- and post election municipalities in the reproduction dataset's figure, 0.03, than in the FF figure, about 0.01.⁷ Also, in the replication figure, pre-election municipalities with one violation comprise a greater proportion of municipalities (0.29) than pre-election municipalities with no violations

⁶Ferraz and Finan state that they include the 'first 13 lotteries' in their analysis (page 717). This comprises lotteries 2-14 because the CGU's first lottery used different selection criteria, and only included 5 audits. Ferraz and Finan also appear to include one municipality, from the 15th lottery (Chapadinha, MA).

⁷In the reproduction dataset, there are 50 municipalities with two reported violations, among municipalities with audit reports released before the election. There are 35 municipalities with two violations among municipalities with audit reports released after the election. In Ferraz and Finan's figure, given that Ferraz and Finan code 205 audits as having been released before the election, and given that municipalities with two violations appear to constitute 23% of municipalities with audits released before the election, Ferraz and Finan appear to have about 47 municipalities with two reported violations, among municipalities with audit reports released before the election. This is three fewer municipalities than found in the reproduction dataset. Given that Ferraz and Finan code 168 audits as having been released after the election, and given that municipalities with two violations appear to constitute 22% of municipalities with audits released after the election, Ferraz and Finan appear to have about 37 municipalities with two reported violations, among municipalities with audit reports released after the election. This is two more municipalities than the replication data.

(0.28). In Ferraz and Finan’s figure, the reverse is true: pre-election municipalities with one violation comprise a lesser proportion of municipalities (about 0.27) than pre-election municipalities with no violations (about 0.29).⁸

The dissimilarities in Figures 1a and 1b may be related to a discrepancy between the original and reproduction datasets in the distribution of audits released before the 2004 election and the distribution of audits released after the 2004 elections. FF (2008) has 205 municipalities with audits released before the election and 168 municipalities with audits released after the election, whereas the replication data has 206 audits released before the election and 167 audits released after the election. It is likely that the differences are due to one audit from the post-election group being coded as being released after the election. If we look at the average number of corruption violations in Table 1.3, we see that the replication data averages 1.524 violations for the pre-election group, and 1.958 violations for the post-election group. Ferraz and Finan’s data averages 1.584 violations for the pre-election group, and 1.952 violations for the post-election group. If only one of the replication’s pre-election municipalities should be in the post-election category, it must be a municipality with a low number of violations that is misplaced. Only this switch would raise the replication data’s average of pre-election violations and lower the average of post-election violations. By switching one municipality with one violation from the pre-election group to the post-election group, I can reproduce Ferraz and Finan’s average violations for post-election audits (1.952) but there remains a discrepancy for the average violations among pre-election audits (replication: 1.5902; FF: 1.584). As a result, I am unable to exactly reproduce Ferraz and Finan’s (2008) dataset.

1.4 Hypothesis Tests

Although I could not precisely replicate the descriptive statistics found in Ferraz and Finan (2008), I proceed with replicating Ferraz and Finan’s analysis using the reproduction dataset. In their study, Ferraz and Finan (2008) first test the effects of being audited, regardless of the audit’s findings, on electoral outcomes. They only find a small effect of the audit policy on the incumbents’ change in vote-share. Ferraz and Finan then

⁸In the reproduction dataset, there are 59 municipalities with one reported violation and 58 municipalities with no reported violations, among municipalities with audit reports released before the election. Given that Ferraz and Finan code 205 audits as having been released before the election, and given that municipalities with one violation appear to constitute about 27% of municipalities with audits released before the election, Ferraz and Finan appear to have about 55 municipalities with one reported violation, among municipalities with audit reports released before the election. This is three fewer municipalities than the replication data. Given that municipalities with no violations appear to constitute about 29% of municipalities with audits released before the election, Ferraz and Finan appear to have about 59 municipalities with no reported violations, among municipalities with audit reports released before the election. This is one more municipality than the replication data.

use several specifications to test the effects of being audited on electoral outcomes, conditioning on the level of corruption. They describe the specification in which the sample size is limited to municipalities with five or fewer reported violations as their ‘preferred’ specification. In their preferred specification, Ferraz and Finan find that each additional corruption violation decreases an incumbent’s probability of being elected by seven percentage points. Third, Ferraz and Finan test alternative theories, including the possibility that the selection process was rigged, that members of the governor or president’s political party received favoritism, or that incumbents who narrowly won the 2000 election had an incentive to bribe auditors. They do not find any evidence in favor of these alternative theories. Finally, Ferraz and Finan test the effect that audit outcomes had on reelection rates, conditioning on the presence of local radio. They find that the presence of local radio had a magnifying effect on the relationship between corruption information and electoral outcomes: the audit decreased the probability of reelection by 15.7 percentage points compared to similarly corrupt municipalities that had their audits released before the election, but did not have a radio station. The presence of local radio also had a positive effect on reelection rates for clean politicians: incumbents in municipalities in which no corruption violations were reported enjoyed a 17 percentage point bump in their probability of reelection in municipalities with a local radio station.

Effect of Audit Policy on Electoral Outcomes To test the effects of being audited, regardless of the audit’s findings, on electoral outcomes, Ferraz and Finan (2008) estimate the following model:

$$E_{ms} = \alpha + \beta A_{ms} + X_{ms}\gamma + v_s + \epsilon_{ms} \quad (1.1)$$

Where E_{ms} is the electoral performance⁹ of the incumbent, and A_{ms} indicates that the audit took place before the election. Using this reduced form model, Ferraz and Finan do not find any significant effect of being audited on reelection rates. However, they do find some evidence (significant at $p < 0.1$) that audited incumbents lose vote share.

⁹Electoral performance measures include the probability of reelection, vote share, win margin, change in vote share, and change in win margin.

Table 1.4: Average Effects of Release of Audits on Electoral Outcomes

	All Eligible Mayors		Only Mayors Who Ran				
	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	0.003 (0.053)	0.030 (0.054)	-0.007 (0.071)	-0.002 (0.021)	0.007 (0.037)	-0.003 (0.023)	0.011 (0.040)
Constant	0.664*** (0.204)	1.875*** (0.604)	2.276*** (0.786)	0.793*** (0.234)	0.655* (0.378)	0.013 (0.252)	0.359 (0.416)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	Yes	Yes	Yes	Yes
N	373	373	264	264	264	264	264

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Running the same analysis on the reproduction dataset fails to reproduce FF's exact coefficients or substantive effect of audit policy on vote-share. Specifically, Ferraz and Finan (2008) find that there is a 3.2 percentage point decrease in the change in vote-share between the 2000 and 2004 elections for incumbents in post-election municipalities compared to incumbents in pre-election municipalities. This effect is significant ($p = 0.05$). The replication data produces a 0.3 percentage-point increase in the change in vote-share, and the effect is insignificant ($p = 0.90$). The original table in Ferraz and Finan (2008) also reveals consistent estimates for the effect of having audit reports released on the probability of reelection for the model with and without controls. For both models, incumbents are 3.6 percentage-points less likely to be reelected if their municipality's audit reports were released before the election, however the estimates are not significant. Using the replication data, the model without controls reveals a 0.3 percentage point increase in the probability of reelection. With controls, that percentage-point increase jumps to 3, however neither estimate is significant at conventional levels. It is possible that the different results are due to a discrepancy in the coding of incumbents' decisions to run for reelection. Columns 3-7 in Table 1.4 report only the results for the restricted sample of incumbents who chose to run for reelection in 2004. Whereas FF (2008) have 263 observations in columns 3-7, I have 264 observations.

Moving onto their analysis of how the content of audit reports condition the effect of audit policy on electoral outcomes, Ferraz and Finan (2008) present a very persuasive figure of the unadjusted relationship between the number of corruption violations and reelection rates. In their Figure 3, which is presented in Figure 1.2b, Ferraz and Finan show that reelection rates for mayors in post-electoral municipalities remain almost exactly the same regardless of the number of corruption violations that were later revealed in audit reports. Among the municipalities with audits released before the election, however, reelection rates steadily decrease as corruption increases, except for municipalities with four or more corruption violations. Notably, the reelection rates for mayors with no corruption violations in pre-election municipalities is higher than the reelection rates for mayors with no corruption violations in post-election municipalities. That suggests that voters have some baseline expectation that politicians will be corrupt, and choose to reward politicians when they are less corrupt than voters' expectations. Figure 1.2a displays the results derived from the reproduction dataset. Although there is a bit more variation in data representing the reelection rates for post-election audits from the reproduction dataset, the two trends are very similar. The most noticeable difference between the two figures is the higher reelection rate among the 25 pre-election municipalities with 3 corruption violations, and the higher reelection rate among the 24 pre-election municipalities with four or

more violations found in the reproduction dataset. This is troubling since the unadjusted data suggests that there is hardly any difference in reelection rates among the most corrupt politicians in pre- and post-election municipalities. In fact, the largest gap between pre- and post-election municipal reelection rates occurs at only 2 corruption violations.

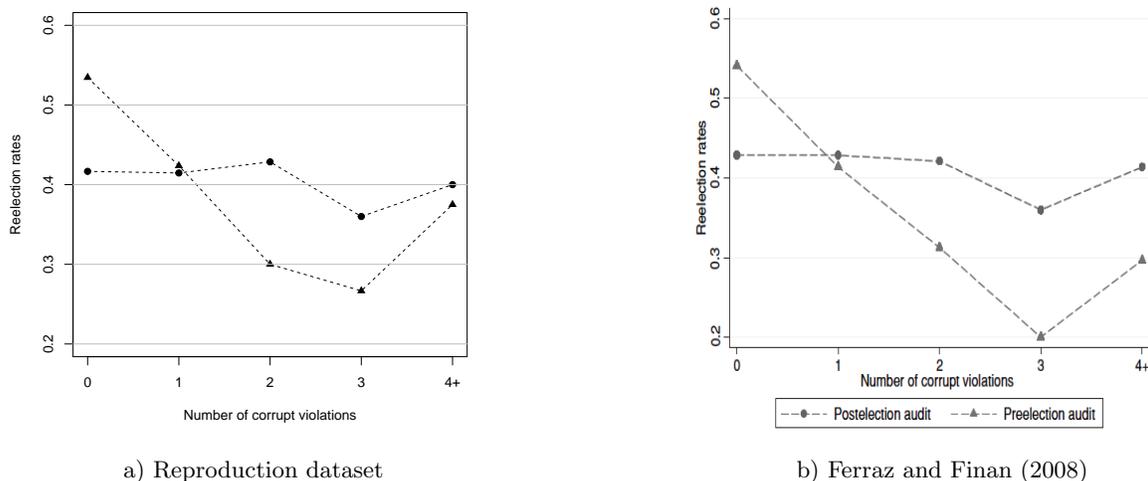


Figure 1.2: Reelection Rates by the Number of Corruption Violations

It is also important to note that Ferraz and Finan include all incumbents eligible for reelection in their figure. Because they find no evidence that the audit policy affected the probability that an incumbent ran for reelection (page 728, footnote 19), it makes sense to also plot reelection rates among only mayors who chose to run for reelection. Figure 1.3 reports incumbents' reelection rates by the number of corruption violations and audit timing, only for those incumbents who actually ran for office in 2004. The trend is a bit more complicated. The trend in pre-election incumbents in Figure 1.3 is similar to the trend in pre-election incumbents in Figure 1.2. Reelection rates decline as the number of violations increases, except among the 19 municipalities that had 4 or more corruption violations. Among incumbents in municipalities in which audit reports were released after the 2004 election, there is a significant increase in reelection rates between 0 and 1 violations, which is followed by a steady decline in the probability of reelection as the number of violations increases. Interestingly, among municipalities in which audit reports were released after the election, clean incumbents have the lowest reelection rates.

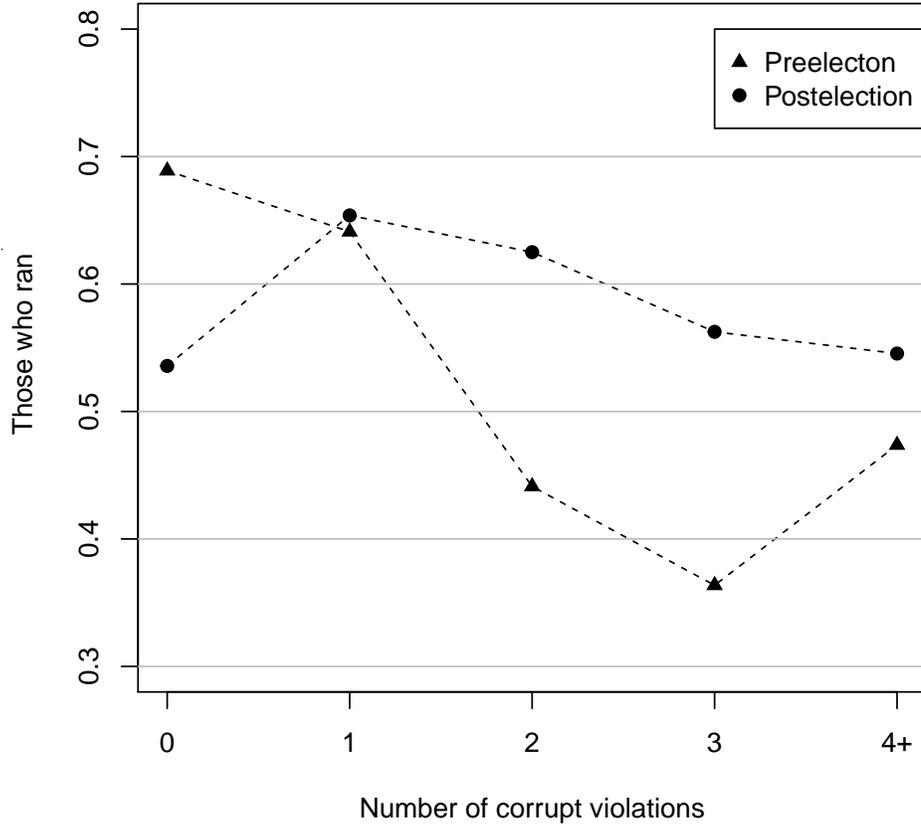


Figure 1.3: Reelection Rates for Incumbents Who Ran by the Number of Corruption Violations

Effect of Audit Policy on Electoral Outcomes, Conditioning on Reported Corruption Moving onto the heart of their analysis, FF (2008) reports the effects of the release of audits on reelection rates conditioning on the level of reported corruption. They run several variants of the following model:

$$E_{ms} = \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 (A_{ms} \times C_{ms}) + X_{ms} \gamma + v_s + \epsilon_{ms} \quad (1.2)$$

where C_{ms} is the number of corruption violations in the municipality, and once again, A_{ms} indicates that the audit took place before the election. β_2 measures the causal effect of releasing the audit before the election, conditional on the level of municipal corruption.

They run linear, quadratic and semiparametric models using all mayors eligible for reelection. The quadratic model includes a squared term of the number of corruption violations, and allows for a curvilinear-

ear relationship between the number of corruption violations and electoral outcomes. The semi-parametric model includes dummies for each number of corruption violations, using one reported corruption violation as the reference category. Ferraz and Finan also run a model including only municipalities with fewer than six violations, and a model with municipalities with fewer than five violations. They find significant effects in two models: the quadratic model and the model that limits the sample to those municipalities with fewer than six violations. In the quadratic model, they report a negative and significant coefficient on the interaction between the number of corruption violations and an indicator for audits released before the election. They also report a positive and significant coefficient on the interaction between the squared count of corruption violations reported in the audits, and the indicator of a pre-election release of the audits. In the model restricted to municipalities with fewer than six violations, which they describe as their ‘preferred’ model, Ferraz and Finan find a negative and significant effect of the audit release on reelection rates, conditioning on the level of corruption.

I replicate these models in Table 1.5. I am able to reproduce Ferraz and Finan’s general results (sign and significance) for the quadratic model: models using the replication data report a negative and significant coefficient on the interaction between the number of corruption violations and an indicator for audits released before the election, and a positive and significant coefficient on the interaction between the squared count of corruption violations reported in the audits, and the indicator of a pre-election release of the audits. This is consistent with Figure 1.1, which showed a decrease in reelection rates for municipalities with a low level of corruption violations reported, and an increase in reelection rates for municipalities with the highest levels of corruption violations reported.

The semi-parametric model is also consistent with Figure 1.1: having two corruption violations exposed before the 2004 election is associated with a 25.4 percentage-point reduction in the probability of reelection over having one corruption violation exposed. This estimate is also nearly identical to Ferraz and Finan’s (2008) 25.3 percentage point estimate for two corruption violations. However, unlike Ferraz and Finan’s model, the indicator for two corruption violations exposed before the election is the only statistically significant indicator.

Finally, the reproduction of their preferred model, which restricts municipalities to those with fewer than 6 corruption violations, reveals a negative coefficient on the interaction between the number of corruption

violations and pre-election indicator. However the coefficient is not significant at conventional levels. The model with its sample restricted to fewer than 5 corruption violations is also negative and insignificant. As one of the cornerstones of Ferraz and Finan's results, the failure to reproduce these two models has serious implications for their study. The fact that Ferraz and Finan presented clear evidence of exposing corruption on electoral *outcomes* - that is, the probability of reelection - is the reason why the research gained such notoriety.

Table 1.5: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption

	Linear (1)	Linear (2)	Quadratic (3)	Semi-Parametric (4)	≤ 5 violations (5)	≤ 4 violations (6)
Preelection Audit	0.038 (0.083)	0.051 (0.084)	0.146 (0.101)	0.077 (0.107)	0.100 (0.089)	0.099 (0.090)
Corruption Violations	-0.020 (0.027)	-0.030 (0.028)	0.011 (0.068)		-0.004 (0.034)	-0.024 (0.037)
Corruption Violations ²			-0.008 (0.011)			
Preelection Audit X Corruption Violations	-0.028 (0.035)	-0.020 (0.037)	-0.187** (0.090)		-0.060 (0.042)	-0.062 (0.044)
Preelection Audit X Corruption Violations ²			0.036** (0.017)			
Corruption=0				0.014 (0.123)		
Corruption=2				0.054 (0.119)		
Corruption=3				-0.120 (0.135)		
Corruption=4+				-0.103 (0.140)		
Preelection Audit x corruption=0				0.042 (0.155)		
Preelection Audit x corruption=2				-0.254* (0.151)		
Preelection Audit x corruption=3				-0.137 (0.202)		
Preelection Audit x corruption=4+				-0.003 (0.185)		
Constant	0.671*** (0.227)	1.954*** (0.607)	1.955*** (0.612)	1.984*** (0.619)	2.769*** (0.629)	2.708*** (0.635)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	Yes	Yes	Yes
N	373	373	373	373	362	351
R ²	0.075	0.229	0.241	0.244	0.241	0.257

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. All models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Despite these disappointing results, I continue to reproduce Ferraz and Finan's analysis of revealed corruption on other measures of electoral performance. As Table 1.6 demonstrates, these models more strongly reproduce their substantive results: almost all models with fewer than 6 violations report a negative and statistically significant effect of revealed corruption on electoral performance. Exposed corruption only fails to significantly affect incumbents' change in vote share.

Although these results are more promising than the results in Table 1.5, they do not represent a sharp contrast from the existing literature on corruption and political accountability. Rather than present evidence of a strong negative effect of exposed corruption on electoral *outcomes* (probability of reelection), these models only provide evidence of a negative effect of exposed corruption on electoral *performance* (vote-share and win margin). This is comparable to other studies that similarly show a negative effect of exposed corruption on electoral performance, but no significant negative effect of exposed corruption on electoral outcomes (Peters and Welch, 1980).

Table 1.6: Average Effects of Release of Audits on Electoral Performance Conditioned by the Level of Corruption

	Pr(reelection)			Margin of Victory		
	Full Sample	≤ 5 violations	Semi-Parametric	Full Sample	≤ 5 violations	Semi-Parametric
	(1)	(2)	(3)	(4)	(5)	(6)
Preelection Audit	0.044	0.088	0.034	0.043	0.065	0.075
	(0.103)	(0.107)	(0.148)	(0.048)	(0.050)	(0.080)
Corruption Violations	-0.028	-0.008		0.003	0.014	
	(0.032)	(0.037)		(0.013)	(0.017)	
Preelection Audit × Corruption Violations	-0.044	-0.080*		-0.024	-0.045**	
	(0.042)	(0.048)		(0.017)	(0.021)	
Corruption=0			-0.036			0.036
			(0.157)			(0.075)
Corruption=2			0.080			0.132
			(0.158)			(0.086)
Corruption=3			-0.106			0.046
			(0.183)			(0.076)
Corruption=4+			-0.148			0.070
			(0.173)			(0.084)
Preelection Audit × corruption=0			0.088			0.012
			(0.196)			(0.095)
Preelection Audit × corruption=2			-0.314			-0.251**
			(0.197)			(0.104)
Preelection Audit × corruption=3			-0.147			-0.102
			(0.251)			(0.097)
Preelection Audit × corruption=4+			-0.054			-0.092
			(0.221)			(0.103)
Constant	2.354***	2.175***	2.428***	0.646*	0.615	0.657*
	(0.795)	(0.805)	(0.820)	(0.379)	(0.386)	(0.378)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
N	264	256	264	264	256	264
R ²	0.287	0.296	0.303	0.213	0.222	0.251

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. All models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.7: Average Effects of Release of Audits on Electoral Performance Conditioned by the Level of Corruption

	Vote share			Change in vote share		
	Full Sample	≤ 5 violations	Semi-Parametric	Full Sample	≤ 5 violations	Semi-Parametric
	(1)	(2)	(3)	(4)	(5)	(6)
Preelection Audit	0.013	0.031	0.037	-0.009	0.009	0.025
	(0.029)	(0.030)	(0.045)	(0.032)	(0.033)	(0.047)
Corruption Violations	0.001	0.010		-0.007	-0.0005	
	(0.009)	(0.010)		(0.011)	(0.011)	
Preelection Audit × Corruption Violations	-0.010	-0.027**		0.002	-0.017	
	(0.011)	(0.013)		(0.014)	(0.014)	
Corruption=0			0.006			0.017
			(0.042)			(0.045)
Corruption=2			0.059			0.026
			(0.048)			(0.053)
Corruption=3			0.006			0.008
			(0.046)			(0.052)
Corruption=4+			0.027			-0.012
			(0.050)			(0.058)
Preelection Audit × corruption=0			-0.002			-0.025
			(0.054)			(0.058)
Preelection Audit × corruption=2			-0.131**			-0.077
			(0.059)			(0.064)
Preelection Audit × corruption=3			-0.058			-0.043
			(0.060)			(0.080)
Preelection Audit × corruption=4+			-0.050			-0.024
			(0.064)			(0.077)
Constant	0.790***	0.814***	0.802***	0.033	0.089	0.040
	(0.235)	(0.232)	(0.235)	(0.250)	(0.245)	(0.248)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
N	264	256	264	264	256	264
R ²	0.221	0.242	0.250	0.342	0.373	0.349

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. All models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Effect of Audit Policy on Electoral Outcomes, Conditioning on Reported Corruption and the Presence of Radio Finally, Ferraz and Finan argue that municipalities that have an AM radio station see a stronger effect of the audit results on electoral outcomes. This is a key part of their argument. By providing evidence that AM radio stations strengthen the relationship between exposed corruption and electoral outcomes, Ferraz and Finan (2008) demonstrate the important role that media plays in the accountability process. Without the presence of AM radio stations, which Ferraz and Finan claim are run by the local community, many voters may not have learned about their incumbents' corrupt practices and may not have been able to act upon that knowledge in the 2004 election.

In order to estimate the effect of local radio on electoral outcomes, they estimate a model with a triple interaction term:

$$E_{ms} = \alpha + \beta_0 C_{ms} + \beta_1 A_{ms} + \beta_2 M_{ms} + \beta_3 (A_{ms} \times M_{ms}) + \beta_4 (A_{ms} \times C_{ms}) + \beta_5 (M_{ms} \times C_{ms}) + \beta_6 (A_{ms} \times C_{ms} \times M_{ms}) + X_{ms} \gamma + v_s + \epsilon_{ms} \quad (1.3)$$

Where M_{ms} is the number of radio stations in a municipality. β_6 is the effect of being audited before the election, conditional on the number of local radio stations, and amount of corruption. Their findings suggest that the presence of local radio stations has a significant conditional effect on corruption's impact on electoral outcomes. The reduction in reelection rates is only about 3.7 percentage points in corrupt municipalities without radio stations, whereas the reduction in reelection rates is 16.1 percentage points in municipalities with radio stations.

Table 1.8: Average Effects of Release of Audits on Probability of Reelection Conditioned by the Level of Corruption and Presence of Local Radio

	Full Sample	Pr(reelection) ≤ 5 violations
	(1)	(2)
Preelection Audit	0.040 (0.113)	0.087 (0.121)
Corruption Violations	-0.021 (0.038)	-0.007 (0.049)
AM Radio Stations	0.020 (0.077)	-0.0005 (0.082)
Preelection Audit X Corruption Violations	-0.031 (0.047)	-0.068 (0.057)
Preelection Audit X AM Radio Stations	-0.039 (0.088)	-0.037 (0.093)
Corruption Violations X AM Radio Stations	-0.024 (0.030)	-0.012 (0.040)
Preelection Audit X Corruption Violations X AM Radio Stations	0.030 (0.043)	0.029 (0.052)
Constant	2.743*** (0.646)	2.532*** (0.674)
State Fixed Effects	Yes	Yes
Control Variables	Yes	No
N	354	343
R ²	0.232	0.241

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.8 reports the results obtained using the reproduction dataset. I find no significant effects of radio stations on the probability of reelection. One possible cause of this discrepancy is our different sample sizes. Ferraz and Finan use their entire sample (their table has 373 observations), whereas I am missing 19 observations. This is due to missing observations for the “AM Radio” variable. Ferraz and Finan report that they used the 1999 Municipal Survey to construct their measure of local radio. In the 1999 Municipal Survey, however, 19 of the municipalities included in this sample ‘ignored’ the question for the number of AM radio stations (possible answers were between 0-9 radio stations, ‘Ignored’ or ‘Not Available’).

It is possible that Ferraz and Finan assumed that all ‘ignored’ responses were due to a lack of radio stations. Therefore, they may have recorded that the 19 municipalities that ignored the question did not have any radio stations. To test this possibility, I filled all missing radio data with ‘0’ and reran the models. The results, which still do not reproduce FF’s findings, are reported in Table 1.9.

Table 1.9: Average Effects of Release of Audits on Probability of Reelection Conditioned by the Level of Corruption and Presence of Local Radio

	Full Sample	Pr(reelection) ≤ 5 violations
	(1)	(2)
Preelection Audit	0.111 (0.107)	0.159 (0.114)
Corruption Violations	-0.002 (0.036)	0.015 (0.046)
AM Radio Stations	0.056 (0.080)	0.037 (0.083)
Preelection Audit X Corruption Violations	-0.052 (0.045)	-0.090 (0.055)
Preelection Audit X AM Radio Stations	-0.076 (0.090)	-0.074 (0.094)
Corruption Violations X AM Radio Stations	-0.034 (0.030)	-0.022 (0.039)
Preelection Audit X Corruption Violations X AM Radio Stations	0.039 (0.043)	0.039 (0.051)
Constant	2.861*** (0.629)	2.683*** (0.657)
State Fixed Effects	Yes	Yes
Control Variables	Yes	Yes
N	373	362
R ²	0.233	0.243

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

The strict replication of Ferraz and Finan’s analysis does provide tepid support for the positive effects of corruption information on political accountability. However, because Ferraz and Finan’s study is an outlier among the political accountability literature, the evidence in favor of corruption information’s utility should be higher before we are confident that the provision of information audits are the silver bullet for local corruption. Out of 17 models run using the reproduction dataset, 4, or 23.5%, revealed a negative and significant effect of Brazil’s audit policy conditioning on the level of corruption, on either the probability of reelection. If we combine models with electoral outcomes and all measures of electoral performance, 9 out of 26, or 33.6% revealed a negative and significant effect of Brazil’s audit policy conditioning on the level of corruption, on electoral outcomes. Because the evidence supporting corruption information’s effect on electoral accountability is not clear, I will continue to test the provision of corruption information on electoral outcomes in Brazil’s 2008 and 2012 municipal elections in the next section.

1.5 Extension of Ferraz and Finan (2008) to the 2008 and 2012 elections

In my first extension of Ferraz and Finan’s analysis to the 2008 and 2012 elections, I will rely on Brazil’s audit institution’s, the CGU’s, internal coding of their own audit reports. Because the CGU’s coding of their audit reports do not directly map onto Ferraz and Finan’s coding scheme, I will follow this replication with a second replication of the 2008 election using my own coding of the CGU’s audits.

In 2006, the CGU began coding audit irregularities into one of three categories: ‘formal,’ ‘medium,’ and ‘severe.’ Formal irregularities are errors that do not impact the management of a governmental program, and usually result from non-compliance with regulations. They occur as a result of ‘involuntary acts or omissions’(Vieira, 2011). Medium irregularities do impact the performance of the governmental program in question, but are not serious enough to be considered “severe irregularities.” They result from acts or omissions that “do not comply with the parameters of legality, efficiency, economics, effectiveness, or quality” and are usually due to insufficient oversight (Vieira, 2011). Serious irregularities severely impact the performance of the governmental program in question, and are characterized by one of the following:

1. The failure to provide accounts or statements
2. The practice of an illegal, illegitimate, or economically inefficient act, or an infraction of a legal or regulatory rule of an accounting, financial, budgetary, operational or patrimonial nature that has the

potential to cause harm to the treasury or that constitutes a grave deviation from the principles to which it is subjected.

3. Damage to the treasury resulting from an illegitimate or economically inefficient act; and
4. Embezzlement or misappropriation of money, goods, or public securities.

I will use the sum of the number of ‘medium’ and ‘severe’ violations for each municipality as my measure of corruption. Avis et al. (2016a) also use the sum of medium and severe violations as their measure of corruption in an article in which they test the effect of being previously audited on corruption violations revealed in future municipal audits. In their article, the authors define corruption as ‘fraud in the procurement of goods and services, diversion of funds, and over-invoicing of goods and services,’ which is almost identical to the definition used in Ferraz and Finan (2008). Therefore, using the sum of medium and severe violations is a good start to extending the analysis of information about corruption and electoral behavior to the 2008 and 2012 elections.

Description of the data

The CGU has continuously chosen 60 municipalities for audit in each lottery since the end of Ferraz and Finan’s study, however the frequency with which lotteries occur has slowed. In setting up their analysis, Ferraz and Finan (2008) choose audits that were released within 15 months before the 2004 elections as their treatment group, and they choose audits that were released 9 months after the 2004 election as their control group. In order to closely approximate Ferraz and Finan’s study with the CGU data, I chose the same period of time before the 2008 and 2012 municipal elections, 15 months, as my treatment group. Therefore I only include audit reports that were published within the 15 month period before the 2008 election as my treatment group for the 2008 extension, and I only include audit reports that were published within the 15 month period before the 2012 election as my treatment group for the 2012 extension. In order to increase my sample size, I increase the number of months after each election when choosing my control group from 9 months to 15 months. Therefore, I include audit reports that were published during the 15 month period after the 2008 election as my control group for the 2008 extension, and I only include audit reports that were published within the 15 month period after the 2012 election as my control group for the 2012 extension. Because I am increasing the N , the larger window after the 2008 and 2012 elections should only increase the probability that I find significant results for the effect of audit policy on electoral outcomes.

Figure 1.4 shows when the CGU published their audit reports.

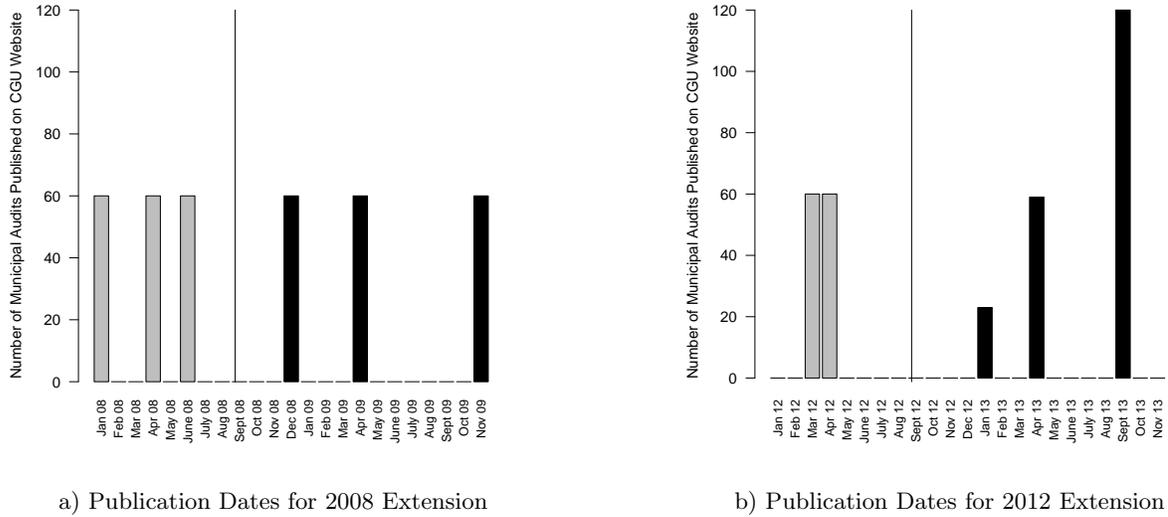


Figure 1.4: Release of Audit Reports on CGU’s Website

The CGU published 180 municipal audits in both the 15-month period before and the 15-month period after the 2008 election. The CGU also published 120 municipal audits in the 15-month period before the 2012 election, and it published 212 municipal audits in the 15-month period after the 2012 election. The 2012 extension has an odd number of audits because lottery 36 only saw the release of 23 audits in January 2013 due to a strike by members of the CGU. Lotteries 38 and 39 were also released at the same time in September 2013. My sample is further restricted because I only include municipalities in which the mayor was eligible for reelection. This reduces the 2008 sample from 360 observations to 252 observations, and it reduces the 2012 sample from 263 observations to 160 observations.

Tables 1.10, 1.11, and 1.12 present descriptive statistics of electoral, municipal, and political characteristics. None of the variables in Tables 1.10 or 1.11 are significantly different before and after either the 2008 or the 2012 elections. In Table 1.12, only the amount of fiscal resources audited in the 2008 replication (here reported as a log) is significantly different between municipalities audited before and after the 2008 election. This is a good indication that the lottery process continued to be fair in its random selection of municipalities

for audit. It is also a good indication that our pre- and post-election groups are adequately matched across many characteristics, and therefore represent satisfactory treatment and control groups.

	2008 Election				2012 Election			
	Pre	Post	Diff	SE	Pre	Post	Diff	SE
Reelection rates in current elections	0.576	0.504	0.072	0.063	0.391	0.356	0.035	0.077
Current reelection rates, those that ran	0.706	0.653	0.053	0.066	0.642	0.520	0.122	0.066
Ran for Reelection in current election	0.816	0.772	0.044	0.051	0.609	0.685	-0.076	0.076
Number of Parties in previous election	2.871	2.944	-0.073	0.130	2.839	2.521	0.319	0.155
Margin of Victory in previous election	0.124	0.128	-0.004	0.015	0.194	0.189	0.005	0.041
Mayor's vote share in previous election	0.524	0.511	0.013	0.013	0.554	0.571	-0.017	0.024

Table 1.10: Political Characteristics, 2008 and 2012

	2008 Election				2012 Election			
	Pre	Post	Diff	SE	Pre	Post	Diff	SE
Age	48.120	50.575	-2.455	1.236	50.529	50.808	-0.279	1.491
Years of Education	11.184	11.921	-0.737	0.456	11.471	12.068	-0.597	0.557
Proportion Male	0.912	0.906	0.006	0.036	0.897	0.877	0.020	0.051
PSB Member	0.016	0.024	-0.008	0.018	0.069	0.055	0.014	0.038
PT Member	0.080	0.102	-0.022	0.036	0.080	0.110	-0.029	0.047
PMDB Member	0.144	0.213	-0.069	0.048	0.115	0.233	-0.118	0.061
PFL Member	0.184	0.110	0.074	0.045	0.000	0.000	0.000	0.000
PPB Member	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
PSDB Member	0.128	0.126	0.002	0.042	0.138	0.110	0.028	0.052

Table 1.11: Mayoral Characteristics, 2008 and 2012

	2008 Election				2012 Election			
	Pre	Post	Diff	SE	Pre	Post	Diff	SE
Population Density	0.717	0.559	0.159	21.990	1.303	1.490	-0.186	110.130
Literacy	0.740	0.752	-0.012	1.649	0.764	0.762	0.002	2.070
Daily Newspapers	0.931	0.826	0.105	0.193	1.867	1.197	0.671	1.222
Economic Incentives	0.608	0.480	0.128	0.062	0.535	0.634	-0.099	0.079
Public Employment	42.350	40.409	1.940	1.546	39.447	42.371	-2.924	1.951
Zoning Laws	0.192	0.184	0.008	0.050	0.186	0.171	0.015	0.062
Paved Roads	58.800	61.111	-2.311	3.504	57.930	55.437	2.494	4.850
Proportion Urban	0.605	0.594	0.011	0.029	0.625	0.615	0.010	0.037
Log of Income	5.502	5.502	-0.034	0.075	5.609	5.589	0.020	0.099
GINI Index	0.514	0.529	-0.015	0.008	0.503	0.506	-0.003	0.011
Municipal Police	0.208	0.165	0.043	0.049	0.161	0.274	-0.113	0.066
Small Claims Court	0.280	0.370	-0.090	0.059	0.379	0.301	0.078	0.075
Judiciary District	0.456	0.559	-0.103	0.063	0.437	0.466	-0.029	0.079
Percent with Radio	0.152	0.205	-0.053	0.048	0.207	0.164	0.043	0.062
Log of Amount Audited	17.632	17.275	0.357	0.159	17.796	17.692	0.104	0.216
Corruption Violations	71.608	67.449	4.159	4.624	54.655	54.655	1.381	4.578

Table 1.12: Municipal Characteristics, 2008 and 2012

Figure 1.5 reports the distribution of violations associated with corruption for the 2008 and 2012 samples, separated by whether the audit reports were published before or after their associated election. Because audit reports are coded differently by the CGU than by Ferraz and Finan, there are significantly more violations reported.¹⁰ Additionally, the audit reports that the CGU released to the public in 2003-2004 were considerably shorter and less-detailed than the audit reports released after 2004 (Ferraz and Finan, 2011). Therefore, the municipal audits released around the 2008 and 2012 elections had significantly more violations reported than the municipal audits released in 2003-2004.

¹⁰The median number of corruption violations reported in Ferraz and Finan (2008) is one violation, whereas the median number of violations reported in the 2008 and 2012 samples are 59 and 48 violations, respectively.

In order to better visualize the distribution of corruption violations reported around the 2008 and 2012 elections, I count the number of municipalities that fall within 20-count bins. I also group all municipalities with fewer than 30 reported violations and greater than 130 reported violations. The distribution of violations is similar to the distribution reported for the 2004 election with a few noticeable differences. First, neither the 2008 nor the 2012 samples have any municipalities with no reported violations.¹¹ Additionally, the lowest end of the distribution for the 2004 sample, which was comprised of municipalities with 0 violations, had about the same proportions as the next lowest part of the distribution, which comprised municipalities with 1 violation. In the 2008 and 2012 extensions, the lowest end of the distribution comprising municipalities with fewer than 30 violations, has a noticeably lower proportion of the total number of municipalities than the next largest bin with 30-49 violations.

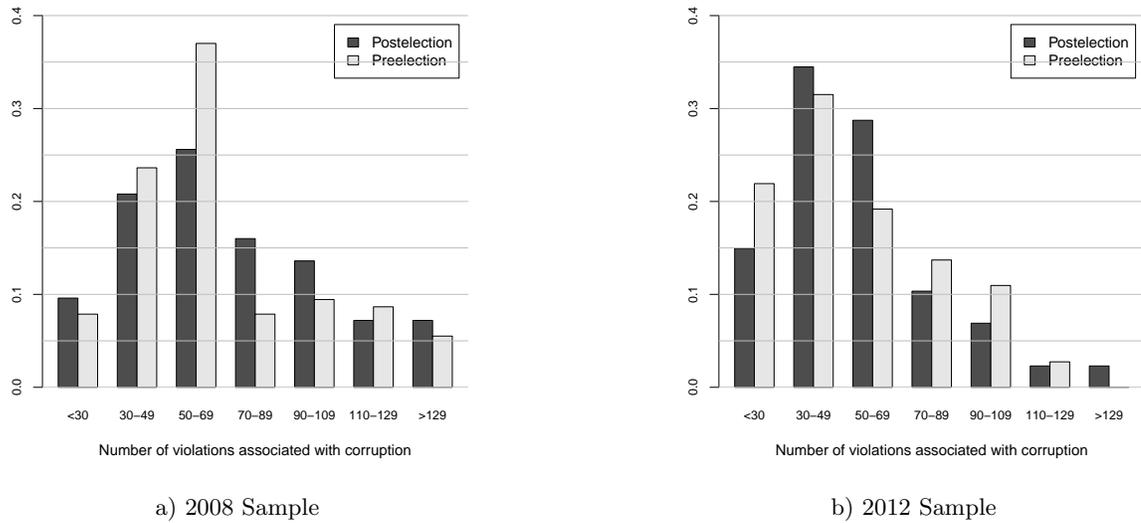


Figure 1.5: Distribution of Corruption Violations by Pre- versus Post-election Audits

¹¹The minimum number of reported violations for 2008 is 10 and the minimum number of reported violations for 2012 is 4. The maximum number of reported violations for 2008 is 276 and the maximum number of reported violations for 2012 is 196.

1.6 Hypothesis Tests, 2008 and 2012 elections

In this section, I will extend Ferraz and Finan's analyses to the 2008 and 2012 elections. To test the effects of being audited, regardless of the audit's findings, on electoral outcomes, I begin by estimating the baseline model:

$$E_{ms} = \alpha + \beta A_{ms} + X_{ms}\gamma + v_s + \epsilon_{ms} \quad (1.4)$$

Where E_{ms} is the electoral performance¹² of the incumbent, and A_{ms} indicates that the audit took place before the election.

¹²electoral performance measures include the probability of reelection, vote share, win margin, change in vote share, and change in win margin

Table 1.13: Average Effects of Release of Audits on Electoral Outcomes, 2008 Election

	All Eligible Mayors			Only Mayors Who Ran			
	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	-0.019 (0.070)	-0.059 (0.064)	0.009 (0.080)	0.006 (0.025)	0.002 (0.035)	0.003 (0.025)	-0.012 (0.036)
Constant	1.290 (0.836)	0.686** (0.270)	1.856** (0.936)	0.935** (0.364)	0.663 (0.531)	0.138 (0.357)	0.437 (0.571)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	No	Yes	Yes	Yes	Yes	Yes

* p < .1; ** p < .05; *** p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.14: Average Effects of Release of Audits on Electoral Outcomes, 2012 Election

	All Eligible Mayors			Only Mayors Who Ran			
	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	0.026 (0.095)	-0.024 (0.081)	-0.137 (0.138)	-0.039 (0.037)	-0.054 (0.061)	-0.063 (0.043)	-0.082 (0.067)
Constant	0.599 (1.046)	0.512 (0.396)	0.567 (1.373)	0.532 (0.357)	0.091 (0.604)	0.464 (0.489)	1.070 (0.745)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	No	Yes	Yes	Yes	Yes	Yes

* p < .1; ** p < .05; *** p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

We see in Tables 1.13 and 1.14 that on average, the release of audits does not have a significant effect on electoral outcomes. Interestingly, the coefficients for the 2008 extension do not retain the same sign across all models. Among the models in Columns 1 and 2 in which all eligible mayors are included in the analysis, there is a negative, albeit insignificant relationship between the release of audits and the probability of reelection. In almost all of the models in which the sample is restricted to incumbents who ran for reelection, there is a positive and insignificant relationship between the release of audits and electoral outcomes. The only negative relationship between audits and electoral outcomes for the restricted sample occurs in Column 7, which tests the effect of audit release on the change in incumbents' win margin.

In the 2012 extension, all relationships are negative except for Column 1, which presents a positive relationship between audits and the probability of reelection for all eligible mayors.

In keeping with the Ferraz and Finan (2008) analysis, Figure 1.6 reports the unadjusted relationship between the number of corruption violations and reelection rates, separated by municipalities whose audits were published before the 2008 and 2012 elections, and municipalities whose audits were published after the 2008 and 2012 elections. In these figures, I use the same bins as Figure 1.5, except that I group all municipalities with greater than 90 corruption violations.

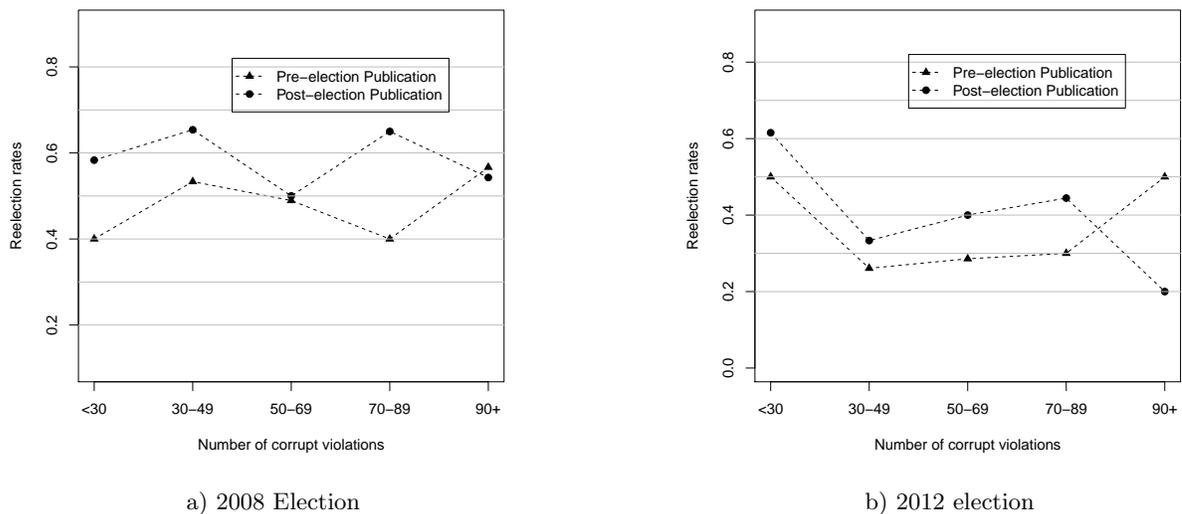


Figure 1.6: Reelection Rates by the Number of Corruption Violations

Both graphs in Figure 1.6 suggest that having audits published before the election leads to a noticeable decrease in the probability of reelection, regardless of how many corruption violations were reported. This is true except in the 2012 extension in Figure 1.6b, in which the pre and post-election groups follow similar trajectories until we reach the bin with the highest number of corruption violations. Among incumbents in municipalities with 90 or more reported corruption violations, the incumbents whose audit reports were published before the 2012 election have a much higher reelection rate than incumbents whose audit reports were published after the 2012 election. In Figure 1.6a, the difference between the reelection rates in pre- and post-election municipalities are very similar in the bin with the highest number of corruption violations. However, like the figure in Ferraz and Finan (2008), and like Figure 1.6a, incumbents do seem to get a bump in their probability of reelection if they are in a pre-election municipality with the highest number of corruption violations. This may suggest that like Ferraz and Finan (2008), eliminating the outliers at the highest end of the distribution is the best way to estimate the effect of exposed corruption on the incumbent's probability of reelection.

Table 1.15: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption, 2008 Election

	Linear (1)	Linear (2)	Quadratic (3)	Semi-Parametric (4)	<3% violations (5)	<6% violations (6)
Preelection Audit	-0.195 (0.155)	-0.162 (0.137)	-0.175 (0.259)	-0.027 (0.230)	-0.221 (0.169)	-0.162 (0.188)
Corruption Violations	-0.002 (0.002)	-0.001 (0.001)	-0.004 (0.005)		-0.003 (0.002)	-0.002 (0.002)
Corruption Violations ²			0.00001 (0.00003)			
Preelection Audit X Corruption Violations	0.002 (0.002)	0.001 (0.002)	0.002 (0.006)		0.003 (0.002)	0.002 (0.003)
Preelection Audit X Corruption Violations ²			0.00000 (0.00003)			
30-50 Violations				0.211 (0.225)		
50-70 Violations				-0.008 (0.226)		
70-90 Violations				0.157 (0.242)		
> 90 Violations				-0.154 (0.230)		
Preelection Audit x 30-50 Violations				-0.171 (0.277)		
Preelection Audit x 50-70 Violations				0.053 (0.267)		
Preelection Audit x 70-90 Violations				-0.004 (0.353)		
Preelection Audit x >90 Violations				0.119 (0.261)		
Constant	1.679* (0.889)	0.810*** (0.313)	1.853** (0.912)	1.405 (0.889)	1.855** (0.822)	1.835** (0.839)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	No	Yes	Yes	Yes	Yes
N	246	252	246	246	237	230
R ²	0.281	0.128	0.284	0.304	0.288	0.282

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above.

Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.16: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption, 2012 Election

	Linear (1)	Linear (2)	Quadratic (3)	Semi-Parametric (4)	<3% violations (5)	<6% violations (6)
Preelection Audit	-0.273 (0.192)	-0.194 (0.175)	-0.372 (0.359)	-0.099 (0.220)	-0.259 (0.219)	-0.326 (0.233)
Corruption Violations	-0.005** (0.002)	-0.004** (0.002)	-0.003 (0.006)		-0.005 (0.003)	-0.005 (0.004)
Corruption Violations ²			-0.00001 (0.00003)			
Preelection Audit X Corruption Violations	0.005 (0.003)	0.003 (0.003)	0.010 (0.013)		0.005 (0.004)	0.007 (0.004)
Preelection Audit X Corruption Violations ²			-0.00004 (0.0001)			
30-50 Violations				-0.185 (0.187)		
50-70 Violations				-0.197 (0.214)		
70-90 Violations				-0.254 (0.276)		
> 90 Violations				-0.444 (0.281)		
Preelection Audit x 30-50 Violations				-0.067 (0.298)		
Preelection Audit x 50-70 Violations				0.243 (0.321)		
Preelection Audit x 70-90 Violations				-0.003 (0.363)		
Preelection Audit x >90 Violations				0.446 (0.320)		
Constant	0.261 (1.040)	0.845** (0.414)	0.271 (1.114)	-0.080 (1.175)	0.669 (1.139)	0.573 (1.202)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	No	Yes	Yes	Yes	Yes
N	157	160	157	157	151	146
R ²	0.376	0.116	0.378	0.393	0.388	0.388

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above.

Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Effect of Audit Publication on Electoral Outcomes, Conditioning on Reported Corruption

In Tables 1.15 and 1.16, I report the results for the effect of the audit publication on the probability of reelection in 2008 and 2012, conditioning on corruption. Column 1 presents the results of an OLS regression for the entire sample of municipalities without controls, and Column 2 presents the results of an OLS regression for the entire sample of municipalities with the full list of municipal and political controls. Surprisingly, the coefficient on the interaction between audit publication before the election and the number of corruption violations is *positive* in both models, although not significant. This is also true for Column 3 in the 2008 extension, which presents a quadratic model. The quadratic model in the 2012 extension in Table 1.16 has a positive coefficient on the linear term and a negative coefficient on the squared term, although both are insignificant as well. Column 4 presents a semi-parametric model, with dummy variables for each bin of corruption violations. Using the lowest bin of fewer than 30 corruption violations as the reference point, the semi-parametric model presents a smattering of coefficient sizes and signs, none of which are significant.

In the final two columns of Tables 1.15 and 1.16, I present a restricted sample without municipalities that represented outliers in the number of reported corruption violations. In Ferraz and Finan's analysis, they restrict their sample to municipalities with fewer than 6 violations, and then restrict their sample to municipalities with fewer than 5 violations. This represents the elimination of the highest 3% and the highest 6% of their sample of municipalities, respectively. To replicate their research design in the 2008 replication, I also eliminate municipalities in the highest 3% and 6% of the distribution, which represents all municipalities with fewer than 143 violations and municipalities with fewer than 129 violations respectively. For the 2012 replication, I eliminate municipalities in the highest 3% and 6% of the distribution of reported corruption violations, which represents all municipalities with fewer than 115 and 100 reported violations respectively.

The results of these restricted models also have positive but insignificant coefficients on the interaction between the number of corruption violations and the audit's pre-election release. This further undermines the possibility that corruption revealed in Brazilian audits has any discernible negative effect on electoral outcomes: regardless of how I cut the sample or loosen assumptions of linearity, exposing corruption does not have a negative effect on reelection rates in the 2008 and 2012 elections.¹³

¹³In Ferraz and Finan (2008), this section is followed by a series of tests of alternate theories. I reproduce those tests in the Appendix.

The Effect of Audit Publication on Electoral Outcomes, Conditioning on Reported Corruption and the Presence of Radio

Finally, I test the effect of corruption information on electoral outcomes, conditioning on the presence of local radio stations. In my first analysis of the effect of radio on electoral outcomes, I will use data from the 1999 Municipal survey, which provides the number of AM radio stations in each municipality. Two other municipal surveys, the 2009 and 2012 surveys, provide an indicator of whether a municipality has an AM radio station or not, but no other survey includes a count. Tables 1.17 and 1.18 present the results using the count of AM radio stations using the 1999 Municipal Survey. It show no effect of audit publication on electoral outcomes in the 2008 and 2012 elections, conditioning on corruption violations and the presence of local radio stations. Table 1.18, however, shows a positive effect of audit publication on electoral outcomes in the 2012 election, conditioning on the level of corruption.

Table 1.17: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption and Number of Radio Stations in 1999, 2008 Election

	Linear	<143 violations
	(1)	(2)
Preelection Audit	-0.186 (0.199)	-0.246 (0.221)
Corruption Violations	-0.003 (0.002)	-0.002 (0.002)
AM Radio	0.034 (0.112)	0.102 (0.123)
Preelection Audit X Corruption Violations	0.002 (0.002)	0.003 (0.003)
Preelection Audit X AM Radio	-0.029 (0.143)	0.016 (0.205)
Corruption Violations X AM Radio	0.001 (0.001)	-0.001 (0.002)
Preelection Audit X Corruption Violations X AM Radio	-0.0002 (0.001)	0.00004 (0.002)
Constant	1.679* (0.998)	1.700* (0.949)
State Fixed Effects	Yes	Yes
Control Variables	Yes	No
N	228	219
R ²	0.293	0.293

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.18: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption and Number of AM Radio Stations in 1999, 2012 Election

	Linear (1)	<115 violations (2)
Preelection Audit	-0.473** (0.215)	-0.616** (0.249)
Corruption Violations	-0.005** (0.002)	-0.008* (0.004)
AM Radio	0.122 (0.190)	0.117 (0.197)
Preelection Audit X Corruption Violations	0.007** (0.003)	0.011** (0.004)
Preelection Audit X AM Radio	0.227 (0.212)	0.293 (0.228)
Corruption Violations X AM Radio	-0.003 (0.003)	-0.003 (0.004)
Preelection Audit X Corruption Violations X AM Radio	-0.002 (0.004)	-0.003 (0.004)
Constant	0.599 (1.128)	1.492 (1.239)
State Fixed Effects	Yes	Yes
Control Variables	Yes	No
N	147	141
R ²	0.449	0.480

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

In order to test the argument using more recent data, I also run these models using the 2009 and 2012 municipal surveys. These provide measures of whether an AM radio station is present or not, but do not provide the number of radio stations. They also provide an indicator variable for whether a community radio station is present in the municipality.

Table 1.19: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption and Presence of Radio Stations in 2009, 2008 Election

	Linear (1)	<6% violations (2)	Linear (3)	<6% violations (4)
Preelection Audit	-0.107 (0.170)	-0.154 (0.185)	0.007 (0.203)	-0.043 (0.230)
Corruption Violations	-0.002 (0.002)	-0.003 (0.002)	-0.001 (0.002)	-0.002 (0.003)
AM Radio 2009	0.271 (0.365)	0.053 (0.372)		
Community Radio			0.354 (0.237)	0.264 (0.263)
Preelection Audit X Corruption Violations	0.002 (0.002)	0.003 (0.002)	0.002 (0.002)	0.002 (0.003)
Preelection Audit X AM Radio 2009	-0.708 (0.441)	-0.495 (0.459)		
Corruption Violations X AM Radio 2009	-0.001 (0.004)	0.003 (0.004)		
Preelection Audit X Corruption Violations X AM Radio 2009	0.007 (0.006)	0.004 (0.006)		
Preelection Audit X Community Radio			-0.414 (0.297)	-0.379 (0.328)
Corruption Violations X Community Radio			-0.001 (0.003)	-0.0002 (0.003)
Preelection Audit X Corruption Violations X Community Radio			0.001 (0.003)	0.001 (0.004)
Constant	1.619* (0.908)	1.891** (0.835)	1.135 (0.912)	1.483* (0.865)
State Fixed Effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
N	246	237	246	237
R ²	0.297	0.311	0.308	0.312

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.20: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption and Presence of Radio Stations in 2012, 2012 Election

	Linear (1)	<115 violations (2)	Linear (3)	<115 violations (4)
Preelection Audit	-0.223 (0.214)	-0.136 (0.253)	-0.222 (0.374)	-0.118 (0.391)
Corruption Violations	-0.004* (0.002)	-0.003 (0.004)	-0.004 (0.004)	-0.002 (0.005)
AM Radio 2012	0.312 (0.264)	0.568* (0.345)		
Community Radio			0.134 (0.268)	0.363 (0.309)
Preelection Audit X Corruption Violations	0.005 (0.003)	0.004 (0.004)	0.003 (0.006)	0.002 (0.006)
Preelection Audit X AM Radio 2012	0.255 (0.534)	0.044 (0.596)		
Corruption Violations X AM Radio 2012	-0.001 (0.004)	-0.008 (0.009)		
Preelection Audit X Corruption Violations X AM Radio 2012	-0.007 (0.009)	-0.001 (0.012)		
Preelection Audit X Community Radio			-0.075 (0.427)	-0.272 (0.471)
Corruption Violations X Community Radio			-0.0004 (0.005)	-0.006 (0.006)
Preelection Audit X Corruption Violations X Community Radio			0.002 (0.006)	0.006 (0.008)
Constant	0.401 (1.049)	0.594 (1.154)	0.435 (1.117)	0.629 (1.209)
State Fixed Effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
N	157	151	157	151
R ²	0.396	0.409	0.385	0.398

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

1.7 Robustness check of 2008 election extension using new audit codings

Although the CGU's coding of audit reports is a useful start for testing the effects of the contents of Brazil's audit reports on electoral outcomes, it presents some problems for the extension of Ferraz and Finan (2008) because do they do not reflect the exact coding rules that Ferraz and Finan use in their 2008 article. Ferraz and Finan (2008) define political corruption as "any irregularity associated with fraud in procurements, diversion of public funds, or over-invoicing" (Ferraz and Finan, 2008, pp 710). They code audit reports by summing the number of irregularities that fall under one of these three categories and use this as their measure of corruption.

Like the CGU's coding rules state, no "formal" violations constitute an act of corruption. The other two categories, medium and severe, are more complicated and do not map directly onto Ferraz and Finan's (2008) definition and coding rules for corruption. First, like the CGU's coding rules would suggest, many "medium" violations do not constitute corruption. Therefore, a better replication of Ferraz and Finan (2008) would eliminate violations that represent mismanagement rather than corruption. For example, the municipality Japaratinga, AL received a "medium" violation because the municipality only had one car available for the Epidemiology and Disease Control Program. The municipality did not receive the violation because money had been allotted for more cars and the funds had been embezzled. Instead, it received the violation because the auditors believed that the municipality should have used its own funds to buy more cars for the program. With one car, the audit team claimed that the disease control program was inefficient. Another example occurred in Manicoré, AM, which received a medium violation for inadequately storing medicine in the Municipal Health Department warehouse. The audit team found that the warehouse was too small and lacked sufficient refrigeration. As a result, many containers of medication were in boxes stacked on the floor.

On the other hand, there are some medium violations that would be coded as "corruption" under Ferraz and Finan's (2008) coding rules. Therefore, I cannot completely exclude medium violations from my measure of corruption. For example, the municipality Manicoré, AM received a medium violation for failing to provide evidence of a bidding process for school lunches. Although the municipality was able to retroactively provide documentation of the winning bid, they did not present adequate evidence that there were other bidders. Additionally, there were numerous items purchased that were not included in the winning bid. This would fall into the category of 'irregular procurement.'

Most "severe" violations constitute corruption. However, some severe violations are examples of corruption

(colloquially defined), but do not fall under Ferraz and Finan's (2008) coding rules. For example, Japaratinga received a severe violation because the Secretary of Finance was the mayor's mother and the Secretary of Health was the mayor's aunt. Nepotism, however, is not included in Ferraz and Finan's definition of corruption.

Because both the medium and severe categories contain both clear examples of corruption, and clear examples of mismanagement, I code the audit reports using Ferraz and Finan's (2008) coding rules. To code the CGU's audit reports, I use the CGU's disaggregated data in which each observation is a reported irregularity (*Constatação*) on a municipal audit report. Variables include a brief description of the irregularity, the department under which the irregularity occurred (Department of Education), and the irregularity type (formal, medium, severe). I code each irregularity as corruption if it is associated with fraud in procurements, diversion of public funds, or over-invoicing. If the description in the CGU's dataset is sufficient, I only use their description. For example, Nova Palma, Rio Grande do Sul was audited after being chosen in the 27th lottery, and received a 'medium' violation for 'Inadequate storage of medication in the Municipal Pharmacy.' This clearly does not constitute an example of the diversion of funds, over-invoicing or fraud in procurement. Therefore, I coded the irregularity as being unrelated to corruption without cross-checking the irregularity in the original audit report. Similarly, the municipality Divina Pastora, Sergipe was audited after being selected in the 27th lottery, and received a 'medium' violation for 'Evidence of fraud in a bidding procedure.' This is clearly an example of fraud in procurement, and I coded the irregularity as an example of corruption without cross-checking the irregularity in the original audit report.

Otherwise, for the majority of irregularities, I cross-reference each irregularity with the audit report available on the CGU's website. I use the more thorough description of the irregularity that is available in the audit report to code each irregularity as corruption or not corruption. For example, the municipality Jaraparatinga, Alagoas was audited after being selected in the 23rd lottery, and received a 'medium' violation with the description 'The PESMS (Education in Health and Social Mobilization Programs) was not implemented.' If the federal government provided the municipality with money for a program that the municipal government never implemented, this would constitute an example of a diversion of public funds. If the municipal government did not receive money from the government, or if the money was returned, this would not constitute an example of a diversion of public funds. Therefore, I cross-checked this irregularity with the audit report available on the CGU's website and found that, indeed, the money was returned to the federal government. Therefore, I coded this irregularity as being unrelated to corruption.

2008 Election Results

The newly coded data had significantly fewer corruption violations than the CGU's codings: I coded approximately 26% of reported violations as corruption. Whereas the average number of corruption violations using the CGU's coding was 70, the average number of corruption violations using the new coding scheme is about 15. Tables 1.21 and 1.22 present the results of our main test using the newly coded data.

Table 1.21: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption, 2008 Election

	Linear (1)	Linear (2)	Quadratic (3)	Semi-Parametric (4)	<3% violations (5)	<6% violations (6)
Preelection Audit	-0.038 (0.110)	-0.096 (0.095)	0.006 (0.141)	0.005 (0.115)	0.020 (0.128)	-0.014 (0.116)
Corruption Violations	-0.005 (0.007)	-0.005 (0.006)	-0.007 (0.020)		-0.003 (0.012)	-0.007 (0.008)
Corruption Violations ²			0.00003 (0.001)			
Preelection Audit X Corruption Violations	0.007 (0.009)	0.006 (0.007)	-0.008 (0.024)		-0.011 (0.015)	-0.001 (0.013)
Preelection Audit X Corruption Violations ²			0.0002 (0.001)			
7-13 Violations				0.011 (0.175)		
13-17 Violations				0.156 (0.299)		
17-30 Violations				-0.128 (0.261)		
> 30 Violations				-0.296 (0.250)		
Preelection Audit x 7-13 Violations				-0.129 (0.236)		
Preelection Audit x 13-17 Violations				0.132 (0.306)		
Preelection Audit x 17-30 Violations				-0.122 (0.327)		
Preelection Audit x >30 Violations				0.593 (0.369)		
Constant	1.729* (0.892)	0.721*** (0.278)	1.757** (0.892)	1.672* (0.924)	1.721* (0.921)	1.855** (0.909)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	No	Yes	Yes	Yes	Yes
N	202	207	202	202	192	197
R ²	0.370	0.130	0.379	0.388	0.386	0.378

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above.

Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Table 1.22: Average Effects of Release of Audits on Reelection Rates Conditioned by the Level of Corruption and Presence of Radio Stations in 2009, 2008 Election

	Linear (1)	<6% violations (2)
Preelection Audit	-0.065 (0.121)	-0.002 (0.128)
Corruption Violations	-0.011 (0.007)	-0.009 (0.009)
AM Radio 2009	-0.031 (0.264)	0.028 (0.294)
Preelection Audit X Corruption Violations	0.012 (0.009)	-0.002 (0.013)
Preelection Audit X AM Radio 2009	-0.024 (0.325)	-0.123 (0.341)
Corruption Violations X AM Radio 2009	0.020* (0.011)	0.014 (0.018)
Preelection Audit X Corruption Violations X AM Radio 2009	0.004 (0.032)	0.022 (0.035)
Constant	1.840** (0.913)	1.838* (0.940)
State Fixed Effects	Yes	Yes
Control Variables	Yes	Yes
N	202	197
R ²	0.389	0.390

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. Except for column 2, all models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Like the replication using the CGU's codings of audits, revealing corruption does not have a significant effect on incumbents' probability of reelection for any model specifications. As Table 1.22 shows, the presence of AM radio also does change audit publication's effects on incumbents' probability of reelection. One interesting finding is that the presence of AM radio actually increases the marginal effect of corruption on the probability of reelection. This is offset, however, by the fact that corruption has an overall negative effect on reelection rates.

These results solidify the conclusions drawn in the previous section: Ferraz and Finan's (2008) analysis does not replicate for the 2008 and 2012 elections.

1.8 Conclusion

This chapter reproduces Ferraz and Finan's 2008 study about the effects of exposing corruption in municipalities in Brazil, and extends the analysis to the 2008 and 2012 municipal elections. In the first part of this chapter, in which I only attempted to reproduce Ferraz and Finan's (2008) study, I was unable to exactly reproduce the Ferraz and Finan (2008) dataset. This led to divergences in the descriptive statistics provided by Ferraz and Finan (2008) and the descriptive statistics calculated from the reproduction dataset. Most importantly, I found a significant difference in the average number of corruption violations between the group of municipalities audited before the 2004 election, and the group of municipalities audited after the 2004 election. This discrepancy leads to questions about whether any findings about the effects of exposed corruption on electoral outcomes are due to underlying differences between the pre-election and post-election municipal groups. I was also unable to reproduce Ferraz and Finan's preferred model, which should show that exposed corruption decreases the incumbents' probability of reelection, and their analyses showing that the effects of audit information is conditional on the presence of local radio stations. In other models, I do find some evidence that exposed corruption affected electoral outcomes, conditioning on the level of corruption. These findings, however, are not less convincing due to the significant differences in corruption violations between treatment and control groups. In the second portion of this chapter, I extended Ferraz and Finan's analysis to the 2008 and 2012 elections. I fail to find any effect of audit release on electoral outcomes, conditioning on the level of corruption or the presence of local radio.

The findings in this chapter have important implications for our understanding of the role of information provision and the media in improving political accountability. Although numerous theoretical studies agree

that the provision of information *should* improve electoral accountability, as Ferraz and Finan (2008) argue, “identifying these effects empirically has been difficult.” The findings in this chapter provide compounding evidence that providing citizens with information about corruption does not function in reality as we would expect it to function theoretically. Regarding the reproduction of Ferraz and Finan’s (2008) analysis of the 2004 election, I only find shaky evidence that exposing corruption affected electoral outcomes. Even if we are to ignore the problematic differences between the average number of corruption violations before and after the 2004 election, the strongest and most robust model in the first section is the quadratic model, showing that citizens are more likely to throw out politicians who have a few corruption violations than they are likely to throw out a politician who has many corruption violations. The extension of the Ferraz and Finan analysis to the 2008 and 2012 elections suggest that if there is any effect of exposing corruption on electoral outcomes in the 2004 election, these effects do not replicate in future elections.

The findings in this chapter also have practical implications for Supreme Audit Institutions (SAIs). According to The International Organization of Supreme Audit Institutions (INTOSAI), a worldwide affiliation of SAIs, the goals of SAIs include “enhanc[ing] transparency, ensur[ing] accountability, maintain[ing] credibility, fight[ing] corruption, [and] promot[ing] public trust” (INTOSAI, 2016, pp 2). The method by which audit reports translate into corrupt politicians being held to account, however, is not straightforward. SAIs frequently rely on other governmental institutions to act as agents of “top-down” accountability by punishing corrupt politicians with fines or jail time, or by preventing corrupt politicians from holding public office.¹⁴ However, “top-down” accountability mechanisms do not always work,¹⁵ and as a result, SAIs also rely on vertical mechanisms of accountability by encouraging citizen engagement, with the expectation that voters will respond to exposed corruption by punishing offenders electorally. Reflecting a repeated theme in INTOSAI’s resolutions, INTOSAI’s Director General argued in an address to the International Anti-Corruption Conference, that SAIs must “improv[e] public awareness” of corruption by “strengthen[ing] [SAIs’] ties with the media” (Borge, 2001, pp 6). Given the ample evidence that the CGU circulated their audit reports to media outlets, and evidence that the media did cover reports of corruption¹⁶, the findings reported in this chapter suggest that forming close ties with media outlets is not an effective way to encourage citizens to punish corrupt politicians. In fact, considering the increasing number of citizen outreach programs created

¹⁴For example, Brazil’s *Ficha Limpa* (Clean Slate) Law barred politicians charged with corruption by the CGU from running for office.

¹⁵For example, the Supreme Court of Brazil watered down Brazil’s *Ficha Limpa* law by ruling that candidates for executive office could only be disqualified from running for office if their charges were confirmed by the local legislature. These legislatures are frequently controlled by the politicians (governor or mayor) who are charged with corruption (Boas et al., 2017).

¹⁶See (Ferraz and Finan, 2008; Oliveira, Edilson, 2016; ClickPB, 2008; Melo Aranha, Ana Luiza, 2016)

by the CGU since 2003 (see OECD (2012)), the lack of findings for the 2008 and 2012 elections suggest that audit institutions may want to focus their efforts on improving horizontal or “bottom-down” accountability mechanisms over vertical accountability mechanisms.

Chapter 2

Turning out or turning in? The effects of exposed corruption on political participation

2.1 Introduction

On November 3, 2016, three days after the second round of Brazil’s municipal elections, the newspaper *O Diário de Mogi* ran the opinion piece “Null votes, blank votes and abstentions” in which the author, Olavo Câmara, attributed Brazil’s decreasing political participation to an increase in corruption scandals. “Scandals, arrests of authorities and businessmen, corruption, and the economic crisis leads to the dismay of voters,” he concluded, pointing to the fact that some mayors who won elections garnered fewer votes than the sum of blank votes, null votes, and abstentions (Câmara, 2016). Câmara is not the only journalist to make a connection between corruption scandals and low participation. In September of 2015, the online journal *Sputnik Mundo* ran the headline “Electoral participation decreases in Guatemala due to corruption scandals” (Sputnik, 2015). News organizations such as *Al Jazeera* and Chile’s *El Mostrador* also published similar stories about Morocco and Chile (Ríos Tobar, 2016; Bennis, 2016).

Although journalists seem to agree that exposed corruption has a detrimental effect on political participation, systematic empirical evidence is less conclusive.¹ Much of the literature on perceived and exposed corruption’s effects on electoral turnout focuses on the United States and European democracies, and show that corruption either increases turnout (Karahan et al., 2006; Escaleras et al., 2012; Stockemer and Calca, 2013) or has no effect on turnout at all (Peters and Welch, 1980; Pattie and Johnston, 2012; Kauder and Potrafke, 2015; Lacombe et al., 2016). Our understanding of the relationship between corruption and turnout is complicated by several cross-national studies, and single-country studies in non-Western democracies that provide evidence in favor of corruption having a *dampening* effect on voter turnout (Stockemer et al., 2013; Sundström and Stockemer, 2015; Chong et al., 2015).

¹Some scholars do not consider the literature on the effects of corruption and turnout to be developed at all. In a meta-analysis of the determinants of electoral turnout, Cancela and Geys (2016) do not test the effects of corruption on turnout because the topic does not have a ‘sufficient number of studies’ available.

If we are to believe that low electoral turnout undermines democratic legitimacy and electoral outcomes (Cavanagh, 1981; Salisbury, 1975), proponents of democratic governance need to better understand the relationship between corruption scandals and political participation. This is especially true as we see a marked increase in the creation of transparency and accountability programs developed with the aim of exposing corruption in new democracies (CEPA, 2009; IMF, 2016a; Chile Transparente, 2016). Transparency International’s recently published report, for example, outlines its strategy for combating corruption and specifically argues that “the results of work against corruption must be visible and tangible” (Transparency International, 2010). That is to say, their unambiguous goal is to bring corrupt practices to a public reckoning. However, if watchdog organizations and domestic audit institutions continue to uncover political corruption, will the resulting scandals undermine democratic legitimacy by depressing political participation? How do citizens respond to new information about political corruption?

This chapter studies the effects of exposed corruption on aggregate levels of turnout, blank votes, and null votes in Brazil’s 2004 municipal elections. I argue that exposed corruption will negatively affect aggregate levels of turnout in Brazil through its individual-level effects on voters’ trust in institutions and political efficacy. First, exposed corruption can negatively affect trust in institutions which is directly related to citizens’ propensity to vote. If exposing corruption causes voters to believe that political institutions cannot prevent corruption, citizens will be less apt to trust those institutions. This will likely cause citizens to “withdraw from political activity altogether or to engage in nonconventional, sometimes illegal, activities such as participating in sit-ins ...” (Citrin, 1974). In other words, citizens who lose trust in their political institutions no longer believe that working within the political system makes sense, and are less likely to engage in conventional forms of political participation. Second, exposing corruption can damage external efficacy,² because corruption represents the politician’s betrayal of voters’ best interests (Olsson, 2014; ?). By using public office for private gain, elected officials demonstrate that they are not responsive to their constituents wishes, which negatively impacts voters’ beliefs that they can impact political processes. Exposed corruption can also damage *internal* political efficacy³ by causing voters who previously voted for the corrupt incumbent to doubt their own political aptitude. Citizens who neither believe that they are capable of making intelligent political decisions nor that their politicians are responsive to their participatory efforts, have no reason to participate in politics (Citrin, 1974).

²External efficacy is the belief that politicians are responsive to voters’ wishes

³Internal efficacy is the belief that one understands the political process and can effectively participate in politics.

I develop my expectations that exposed corruption will negatively affect turnout based on a pattern observed in the literature. Corruption seems more likely to positively affect turnout in countries that have been democracies for long periods of time, and more likely to negatively affect turnout in countries that have been democracies for shorter periods of time. In other words, the age of democratic institutions seems to mediate the relationship between exposed corruption and turnout. Older democracies like the United States and Great Britain are more likely to see exposed corruption positively affect turnout (Karahan et al., 2006; Escaleras et al., 2012), whereas younger democracies like Portugal and Mexico are more likely to see exposed corruption negatively affect turnout (Sundström and Stockemer, 2015; Chong et al., 2015).

To test the effects of exposed corruption on citizen engagement, I use randomly-administered municipal audits in Brazilian mayoral elections. My research design contributes in several ways to the literature on the effects of corruption exposure on turnout. First, by using randomized municipal audit reports provided by the CGU, I am able to study the effect of exposed corruption on turnout for a large, representative sample of municipalities.⁴ Most previous studies focus only on a small sample of municipalities, or on country-level turnout (Stockemer et al., 2013; Chong et al., 2015). Second, by collecting turnout data for several elections before and after the 2004 election, I can use a difference-in-differences design to compare trends in turnout before corruption was exposed to trends in turnout after corruption was exposed. If exposed corruption has a lasting effect on voters political attitudes, we might expect to exposed corruption to affect turnout over the course of several elections.

I find that municipalities with high levels of corruption are indeed associated with lower overall levels of turnout. However, contrary to my expectations, I find that providing voters with concrete evidence of local corruption *increases* political engagement in the following elections. Although there is no discernible effect of exposed corruption on null votes, blank votes significantly decrease and overall turnout significantly increases in several elections following the exposure of corruption. Furthermore, I find that the outcome of the 2004 elections have little effect on political engagement in the 2008-2016 municipal elections: turnout remains high regardless of whether the corrupt incumbent is thrown out of office. My findings contribute to the literature first by distinguishing between the effects of corruption and the effects of *exposed* corruption on turnout. If we were to analyze the cross-sectional relationship between turnout and corruption without

⁴I will provide more detail of the CGU's process of selecting municipalities for audits later. However, only municipalities with fewer than 450,000 inhabitants are eligible for selection. Therefore, my sample is representative of municipalities with fewer than 450,000 inhabitants, which in 2003-2004, comprised 99.2% of all Brazilian municipalities.

looking at the effects of exposing corruption over time, we might have mistakenly believed that the negative relationship between corruption and turnout was the end of the story. To the contrary, exposing corruption through municipal audits has a marginally *positive* effect on turnout levels in corrupt municipalities. Second, the results suggest that, despite Brazil's relatively recent transition to democracy in 1985, exposing corruption does not have the expected negative effect on turnout. The cause of these unexpected results, which I will discuss more extensively in the conclusion, could be due to 1) the fact that Brazilians' relationship with their political institutions more closely resemble the relationship of citizens in older democracies and their political institutions, 2) it could be due to the specific circumstances under which corruption was uncovered in Brazil, or 3) both. In the conclusion and the following chapter I will more thoroughly explore the second possibility: that the circumstances under which corruption was exposed changed our expected understanding of the relationship between exposed corruption and political engagement. In particular, I will explore the possibility that exposing corruption actually had a positive effect on trust in institutions and political efficacy largely because the exposor of corruption, the CGU, was a trusted, non-partisan, Brazilian institution.

The next section surveys the literature on exposed corruption, trust in institutions, and political efficacy. I will argue that trust in institutions and political efficacy are more malleable in new democracies, and can erode following the exposure of corruption. Section III connects this decrease in political trust and political efficacy to a decrease in turnout. I hypothesize that exposed corruption in an unconsolidated democracy will depress electoral turnout in the following elections. I will also argue that countries with mandatory voting are more likely to experience an increase in blank and null votes rather than a decrease in turnout. Therefore, I will hypothesize that exposed corruption in younger democracies will inflate null and blank votes. Finally, I argue that the outcome of the first election following the exposure of corruption should have important consequences for future participation. Therefore, I expect that the effects of exposed corruption on electoral behavior will be more pronounced in localities in which the corrupt politician was not initially thrown out of office. Section IV describes my case selection, data, and research design. Section V presents my results, and Section VI concludes.

2.2 Corruption Scandals, Political Trust, and Political Efficacy

In representative democracies, politicians acquire political office only by the consent of those who elected them. Normatively, office-holders entrusted with the responsibility of governing are expected to act in the best interests of their constituents. By engaging in political corruption, politicians violate their implicit

contract with voters by using their public office for private gain (Rose-Ackerman, 1999). Elections can only afford citizens the opportunity to punish politicians for engaging in corruption if citizens learn about corrupt practices in the first place. In this sense, increased transparency and information is crucial for a well-functioning representative democracy. On the other hand, providing citizens with new information about corruption may have detrimental effects on political trust and political efficacy. Because political trust and efficacy are crucial for political engagement, exposing corruption may, by extension, negatively impact political participation (Fraser, 1970).

Corruption and Trust in Institutions Trust, or the belief that others will actively look out for our interests (Newton, 2007; Gambetta, 1988), can be broken down into two types: 1) interpersonal trust, characterized by horizontal relationships between citizens, and 2) political trust, characterized by vertical relationships between citizens, and their political leaders and institutions.

We can further deconstruct political trust into two types: trust in individual politicians, and trust in political institutions. The former is susceptible to short-term evaluations of performance (Solé-Ollé and Sorribas-Navarro, 2014). The extant literature provides evidence that the latter, trust in political institutions, can be either sticky, coming from deeply entrenched cultural norms and levels of societal trust (Putnam, 1993; Inglehart, 1997), or dynamic, changing in response to citizens' evaluations of institutional performance (Hetherington, 1998; Coleman and Coleman, 1994). In older more established democracies, cultural norms are more likely to have more weight in determining citizens' trust in political institutions. Trust in institutions may change over time, but this change is more likely to occur over the course of many years (Miller, 1974a; Nye et al., 1997). In young democracies, however, political institutions are new, and more recent institutional performance is more likely to take precedence over earlier experiences. For example, Mishler and Rose (2001) studied the determinants of trust in political institutions in post-Communist regimes in Eastern Europe and concluded that, due to their recent transitions to democracy, trust in political institutions was most strongly affected by more proximate evaluations of institutional effectiveness than by cultural norms and early socialization.

The pervasiveness of corruption is one aspect of performance that citizens factor into their evaluations of political institutions (Anderson and Tverdova, 2003). Citizens' trust in political institutions in response to corruption mirrors the effects we see in the general institutional performance literature. For example, Maier (2010) studied German citizens' political attitudes in the wake of the 2004 Bavarian 'Dossier Affair,' in which

a state minister resigned from her post after allegations of electoral fraud and nepotism. Leveraging this real world instance of corruption, Maier conducted an experiment in which he provided information about the scandal to survey participants and tested their attitudes toward the corrupt politician, German institutions, and democracy. Although the implicated state minister lost support, trust in institutions and satisfaction with democracy were not affected by the scandal. Maier concluded that the effects of the scandal were not more diffuse because a single corruption scandal is not sufficient to cause citizens to lose trust in institutions and democracy: German citizens had deeply entrenched trust in their political system.

In younger democracies, however, information about corruption can breed distrust in political institutions because political institutions are supposed to discourage and prevent political corruption. Svobik (2013) explains the logic behind the fragility of new democratic institutions in terms of politicians' reputations. He argues that because politicians in new democracies have not formed reputations as public officials who will respond appropriately to electoral incentives, a few bad experiences with individual politicians may lead voters to believe that all politicians are corrupt and that electoral institutions will not work to discourage corrupt behavior.

Several scholars have found empirical evidence demonstrating that recent negative experiences with politicians and institutions can have a strong effect on citizens' evaluations of the political system in a new democracy (Mishler and Rose, 2001). Focusing on Eastern European post-communist societies, Mishler and Rose (1997) conclude that political culture and early socialization played only a small and indirect role in citizens' trust in their political institutions. More important were their recent evaluations of policies and economic performance. Levi (1998) emphasizes the importance of the perceived fairness of institutions when citizens are evaluating their trustworthiness. "A trustworthy government is one that has procedures for making and implementing policy that meet prevailing standards of fairness, and it is a government that is capable of credible commitments" (pp. 88). As citizens are learning about their institutions, therefore, exposed corruption may signal that either their institutions are not committed to standards of fairness, or are not capable of making credible commitments. These citizens are more likely to find themselves asking, 'Is the system incapable of preventing these transgressions?'

Corruption and Political Efficacy Political efficacy is the extent to which citizens believe that they can impact the political process. Scholars specify two types of efficacy that are important for political participa-

tion: internal efficacy and external efficacy.

External efficacy is the degree to which citizens believe that governments are responsive to their demands (Milbrath 1965). External political efficacy, much like political trust, is both the product of early socialization (Easton and Dennis, 1967; Davies, 1965) and is contingent upon short-term evaluations of performance. Both systemic and individual-level factors can affect external political efficacy. For example, proportional representation increases aggregate levels of external efficacy, by increasing voters' choices over the number of parties representing their interests (Banducci et al., 1999; Karp and Banducci, 2008). Individual-level characteristics that affect external political efficacy include strong party preferences (Karp and Banducci, 2008) and socio-economic status (Murphy, 2011).

Patterns in the literature suggest that learning about corruption is less likely to depress external political efficacy as democracies age (Olsson, 2014; ?). Because corruption violates the implicit contract between politicians and their constituents, exposed local corruption is likely to make voters feel as though the government is not responsive to their preferences. As a result, learning about corrupt practices erodes voters' sense of external political efficacy. In more established democracies, recent institutional performance is less likely to affect external political efficacy. A limited number of studies directly discuss the connection between corruption, external efficacy and participation. Some evidence does support the idea that political efficacy is the causal link between corruption perceptions and low turnout (Olsson, 2014). The logic is that citizens who perceive that politicians are not responsive to their constituents will not participate in the political process.

Internal efficacy is the degree to which citizens feel like they understand and can participate in politics (Karp and Banducci, 2008). Existing work suggests that internal efficacy is relatively stable over time (Krampen, 2000), and increases with perceived political knowledge (Hofstetter et al., 1999; Bennett, 1997). Scholars have conducted a limited amount of research connecting internal political efficacy to exposed corruption. When an elected official is exposed for being corrupt, a plurality of the electorate must face the reality that they voted for a corrupt politician. Assuming that corruption is an important factor in voters' electoral calculations, learning about high levels of corruption may cause voters to doubt their own political aptitude and lose their sense of internal efficacy. If citizens do not believe that they are capable of meaningfully participating in politics, why bother voting?

2.3 Corruption and Political Participation

In advanced democracies, exposing corruption may have positive short-term implications for electoral engagement. Learning new information about corruption provides citizens with a reason to participate in elections in order to throw their malfeasant public officials out of office. If political institutions help citizens identify the guilty party and citizens believe that corruption is not an inherent part of the political system, they will not lose trust in political institutions. If citizens are able to use electoral institutions to remove the corrupt incumbent from office, they will not lose their sense of political efficacy. In older democracies, then, exposing corruption may cause citizens to conclude that the process functioned as it should: corruption was detected, the public became aware of it, and elections served to excise the corrupt individual from public office. Thus, turnout in consolidated democracies, might actually *increase* following the exposure of corruption. Research in advanced democracies generally supports this conclusion. Karahan et al. (2006) and Karahan et al. (2009) find that as the number of county supervisor corruption convictions increases, voter participation in Mississippi also increases. In an observational study, Escaleras et al. (2012) find that US states with higher levels of gubernatorial corruption also have higher levels of turnout.

This is not to say that advanced democracies never experience a decline in electoral turnout. To the contrary, an extensive body of literature outlines a long-term decline in voter participation across a number of advanced industrial democracies (Abramson and Aldrich, 1982; Shaffer, 1981; Flickinger and Studlar, 1992). This declines in turnout, however, do not occur as the result of a single corruption scandal. Instead, they decline slowly and steadily over decades (Franklin et al., 2002).

In new democracies, citizens do not have a long history of democratic experiences to draw upon when faced with new information about corruption. A citizen in a new democracy cannot look to many previous elections in which she and her fellow citizens elected competent politicians who governed on their behalf. As a result, she may see the scandal as evidence that she has no real impact on political outcomes. A citizen in a new democracy also cannot look to the many years in which political institutions discouraged politicians from engaging in dishonest behavior. A corruption scandal, therefore, is more likely to be viewed as an example of how democratic institutions have failed.

A nascent literature generally agrees that corruption in new democracies depresses political participation.

Several experiments and quasi-experiments have also uncovered a negative effect of exposed corruption on turnout. Chong et al. (2015) conduct a study in which they released information about politicians' corrupt behaviors in Mexico and test the effect of corruption on voter turnout and party identification. They found that corruption allegations decreased support for the incumbent, but this decrease was accompanied by a 2.5 percentage point drop in voter turnout. De Figueiredo et al. (2011) study the effect of corruption on turnout through an experiment in which they distribute partisan fliers with information about corruption convictions for both the incumbent and challenger running for mayor of São Paulo. Despite Brazil's mandatory voting laws, they still find a small negative effect on turnout for locations that received information about the left-party candidate, but no effect on turnout for locations that received information about the center-right candidate.

Corruption likely has this depressing effect on turnout because trust in political institutions and political efficacy are important determinants of political participation: as trust in institutions and efficacy decreases, citizens are less likely to engage with the political system (Putnam, 1993; Brehm and Rahn, 1997; Norris, 1999).⁵ Mishler and Rose (2005) find that perceived corruption in Russia has an indirect dampening effect on political participation, which operates through its effect on trust in political institutions. Olsson (2014) also argues that lower political efficacy is the variable through which corruption perceptions lower turnout levels. When citizens lose trust in political institutions, they do not believe that institutions are shaping politicians' incentives to act in citizens' best interest. Additionally, lower external efficacy means that citizens doubt the governments' responsiveness to their preferences. As a result, lower political trust and political efficacy reduce citizens' likelihood of expressing their preferences through participation in their political system.

Through these two mechanisms, political efficacy and trust in political institutions, exposed corruption affects citizens' propensity to participate in elections. If institutions are just the medium through which politicians extract rents, why should citizens engage with political institutions? If the politicians for whom citizens vote will not act on behalf of their constituents, why bother voting? This brings me to my first hypothesis:

H1: Turnout in municipal elections will decrease following the exposure of corruption in Brazil.

⁵Trust and efficacy are strongly related, and in fact some scholars argue that political trust can only mobilize voters to participate in politics when they also feel politically efficacious (Gamson, 1968; Fraser, 1970).

Citizens may also express their lack of desire to participate in political institutions in other ways. In fact, in countries with mandatory voting laws, like Brazil, invalid or ‘blank’ votes are common (Hirczy, 1994), and are often viewed as the “functional equivalent of abstention.” (Lavareda, 1991, p.40) Following the exposure of corruption, the same determinants that decrease political participation in non-mandatory voting countries may increase the number of blank and null votes in mandatory-voting democracies like Brazil. First, blank votes are frequently used to express dissatisfaction with the choice of candidates, as third-party votes are often viewed in the United States (Power and Roberts, 1995). If citizens are compelled to vote in Brazil following the revelation of corruption, citizens may similarly express their dissatisfaction with political institutions and politicians’ lack of responsiveness through null and blank votes. Additionally, Power and Garand (2007) provides evidence that blank votes are the result of low internal political efficacy. Therefore, following the exposure of corruption, citizens have low internal efficacy and feel as though they cannot adequately distinguish between corrupt and honest politicians, they may spoil their ballots rather than risk voting for another corrupt candidate. This brings me to my second hypothesis:

This leads to my second hypothesis:

H2: Blank and null votes in municipal elections will increase following the exposure of corruption in Brazil.

Finally, building on the idea that the context of the corruption exposure matters, the electoral consequences of exposed corruption should also matter for political behavior. Electoral accountability can only function if politicians believe that voters will punish them for poor performance, and if voters actually follow through by punishing poor performance (Svolik, 2013). If either of these expectations break down, elections fail as instruments of accountability.

Therefore, in unconsolidated democracies, the period of time after a corruption scandal is important for demonstrating to politicians and voters that both of these expectations will be upheld. Kostadinova (2009) presents evidence of what can happen when these expectations break down in Eastern Europe’s post-communist countries. She finds that corruption initially mobilized voters to turn out and remove corrupt politicians. After several years of increased corruption and failing to remove corrupt politicians from office, turnout began to decline as citizens lost trust in the political process and withdrew from politics.

I expect to see a similar phenomenon in municipalities exposed for corruption in Brazil. If the corrupt

politician is thrown out of office in the election after audits expose corruption, I expect that trust in politicians and in political institutions will not be as strongly affected as municipalities in which the corrupt politician was not initially thrown out of office. In these municipalities, in which the electoral accountability mechanism did not function to remove a corrupt office-holder, I expect to see a larger depression in turnout and a higher proportion of null and blank votes. This leads me to my third hypothesis:

H3: Effects of corruption on electoral behavior will be more pronounced in localities in which the corrupt politician was not initially thrown out of office.

2.4 Case Selection, Data, and Research Design

Case Selection To study the effect of exposed corruption on political behavior, I will focus on subnational corruption in Brazil. Brazil presents an ideal case for studying exposed corruption and political behavior in newer democracies for several reasons. First, for the time-period included in this study, Brazil was a relatively new democracy. Brazilians have only had experience with their current democratic institutions since the first direct presidential election in 1989. The first time that Brazilians witnessed an elected president finish his term in office and hand over power to another elected president was in 2003.⁶ Brazil's *local* institutions also changed significantly due to the 1988 constitution, allowing for all municipalities to elect mayors without restrictions for the first time in 1988. As I will explain in the next section, the corruption 'scandals' that I will use in this study took place between 2003 and 2012, 15-24 years after Brazil's first free municipal elections, 14-23 years after Brazil's first direct presidential election, and 0-9 years after the first time an elected president completed his term and stepped down from office. Therefore, as a result of Brazil's more recent transition, corruption scandals are more likely to have a strong effect on Brazilian citizens' trust in institutions and sense of political efficacy than in older and more consolidated democracies.

Second, Brazil is highly decentralized, with twenty-six states, one federal district and 5,570 municipalities.⁷ Each municipality has a mayor and legislative body. As one of the most decentralized countries in the world, Brazilian municipalities have a significant amount of autonomy over their own budget and public spending (Melo and Rezende, 2004). The municipal budgets come from taxes, state transfers, and federal

⁶The peaceful transition of power between elected officials of different political parties is often a metric used to determine whether a democratic transition was truly democratic.

⁷Because the federal district is not broken up into municipalities, having both characteristics of a state and municipality, I exclude it from my analysis.

transfers, with federal transfers constituting the highest proportion of the municipal budget. The mayor and local legislators have the power to decide how to spend most of these transfers, meaning that these officials are important local actors from whom voters should expect good performance. Because of their power over the local budget, voters can also more easily attribute blame to the mayor and local legislators when money goes missing.

Third, Brazil has struggled with pervasive political corruption at all levels of the government for many years (Fleischer, 1996; Power and Taylor, 2011). Several organizations estimate that between 1.35% (FIESP 2006) and 5% (Epoca 2008) of Brazil's GDP disappears to corruption every year. Furthermore, both Brazilian citizens and firms looking to do business are aware of the problem. The 2015 Latinobarometer found that corruption was the most important concern for Brazilians. In the 2009 World Enterprises Survey, 68% of companies surveyed said that corruption was among the biggest barriers to doing business in Brazil, with 33% of companies saying they expected to give gifts in order to receive a government contract.

Finally, Brazil is an especially useful case for studying the exposure of corruption because it has a supreme audit institution, the Controladoria Geral da União (CGU).⁸ In 2003, the federal government granted the CGU the ability to randomly choose municipalities for audit. The CGU began with 26 municipal audits in 2003, but raised the number to 50 and then 60 municipalities per round in 2004. The public is invited to all rounds of the audit lottery, which take place every 2-4 months. After a municipality is chosen, a group of 10-15 auditors are sent to each municipality to look for irregularities in the municipal spending of federal transfers. Once the auditors have completed their inspection of municipal accounts, construction, and public goods delivery, they create a comprehensive report detailing any irregularities they uncovered. These reports are posted on the CGU's website and disseminated to all levels of the government. There is also a considerable amount of anecdotal evidence that audit information reaches voters, and is directly attributed to the CGU.⁹ My main measure of corruption comes from Ferraz and Finan's coding of these audits.

Data

⁸The Controladoria Geral da União (CGU) was Brazil's supreme audit institution until 2016, when it was absorbed into the the Ministério da Transparência, Fiscalização e Controle (MTFC). From this point onward, I will refer to Brazil's Supreme Audit Institution as the CGU.

⁹For a few examples, see Oliveira, Edilson (2016); ClickPB (2008); Melo Aranha, Ana Luiza (2016)

Municipal Audits Some scholars refer to operationalizing corruption as an exercise in ‘measuring the immeasurable’ (Galtung, 2006). Although measuring something as covert as corruption is difficult, municipal audits provide an unusually objective way of quantifying corruption. Given that the purpose of this chapter is to study the effects of providing voters information about local corruption on their behavior, randomized local audits provide an ideal measure of corruption compared to other potential measures such as expert surveys, voter perceptions, and non-random audits.

First and foremost, municipal audits provide voters with evidence of actual corruption. Expert surveys such as Transparency International’s Corruption Perception Index (CPI), and the World Bank’s Control of Corruption indicator only measure ‘experts’ *perceptions* of the existence and severity of corruption. In fact, experts may have no actual experience with the country or government they are evaluating. Although survey respondents, such as those in the Latin American Public Opinions Project (LAPOP) have experience in the locality they are evaluating, their responses are still only perceptions of corruption and we therefore cannot be sure that their responses change in response to actual increases and decreases in corruption.

Second, randomized municipal audits are ideal because they not influenced by other factors that correlate with corruption. Because experts’ responses are subjective, experts may base their evaluations on other known correlates of corruption rather than on actual information about the prevalence and severity of corruption (Treisman, 2007). For example, Razafindrakoto and Roubaud (2005) compare expert surveys with public surveys on the prevalence of corruption in eight African countries and find no correlation between the two surveys’ estimation of corruption. In fact, experts’ evaluations of corruption were considerably higher than both the public’s perceptions and public’s experiences. However, the experts’ evaluations *were* correlated with the World Bank’s Control of Corruption Indicator, suggesting that their evaluations were either influenced by these indicators, or that both their evaluations and the World Bank index were influenced by a third variable, such as economic development and democratic institutions. Expert evaluations can also be susceptible to other influences, such as ideological biases. For example, only when Chile changed its trade policy to be more inline with the trade policy of the United States, did its Corruptions Perception ranking for Transparency International began to improve (Abramo, 2005).

Third, municipal audits are exogenous to voters’ political attitudes and behaviors. Because I am interested in the effects of information about corruption on voters’ political attitudes and behavior, corruption

measures that rely on citizens' perceptions of corruption may be endogenous to the outcomes that I am interested in measuring. Morris and Klesner (2010), for example, find that perceptions of corruption and political trust are mutually causal, which would make untangling the causal relationship between corruption and political attitudes and behavior much more difficult.

Finally, Brazil's municipal audits are randomized, giving us corruption measures for a representative¹⁰ sample of municipalities. Audits that were triggered by suspicions or reports of crime are problematic due to selection effects. First, audit institutions like Brazil's CGU may conduct special audits in municipalities that are already expected to have high levels of corruption due to previously discussed third-variables, such as low GDP per capita. When audits are correlated with these third-variables, we cannot untangle the causal relationship. Second, special audits may be conducted at the request of citizens. This is problematic for measuring the effects of revealing corruption on political behavior because highly motivated and politically active citizens are responsible for the municipalities being selected for nonrandom audits. Any positive effects we find of revealing corruption on political behavior may be due to the fact that the individuals in municipalities with exposed corruption are already highly politically active. Finally, if special audits are triggered by the suspicion of corruption, special audits are more likely to reveal the worst corruption offences. This also leads to a bias in the kinds of corruption uncovered, and does not give researchers a sample of 'clean' or 'somewhat corrupt' municipalities for comparison.

Independent Variables My measure of corruption comes from 419 coded municipal audits that took place in 2003 and 2004, and were then released before the 2004 election (Ferraz and Finan, 2011). Ferraz and Finan code a municipality as corrupt when there is evidence of fraud in the procurement of public goods and services, the diversion of public funds for private gains, or the over-invoicing of goods and services. Their coding rules are presented in Table 2.1.

The main corruption variable that I will use includes four values: "high corruption", "low corruption", "clean", and "never audited." I distinguish between high and low levels of corruption because numerous studies have demonstrated a qualitative difference in how voters perceive low levels of corruption and high levels of corruption (Rothstein and Uslander, 2005; Ferraz and Finan, 2008; Eggers and Fisher, 2011; Chong et al., 2015). Therefore, I code a municipality with "high corruption" if there are 4 or more instances of corruption

¹⁰As I explained in an earlier footnote, only municipalities with fewer than 450,000 inhabitants are eligible for selection. Therefore, my sample is representative of municipalities with fewer than 450,000 inhabitants, which in 2003-2004, comprised 99.2% of all Brazilian municipalities.

Table 2.1: Coding Procedures from Ferraz and Finan (2011)

Type of Corruption	Coding Procedure
Diversion of resources	Any irregularity involving the embezzlement of funds. Generally occurs when federally transferred resources 'disappear' from municipal bank accounts, or when the municipality claimed to purchase goods or services that were never provided.
Overinvoicing of goods and services	Occurs when auditors determined that goods and services were purchased at a value above market price.
Fraud in procurement of public goods and services	Includes the use of nonexistent firms in the bidding process, the use of fake receipts to pay for goods and services, or overinvoicing of prices to increase the amount paid for the goods and services

and with “low corruption” when there are fewer than 4 instances of corruption. For robustness checks, I also code several other measures of corruption, including (a) a “high percent corruption” variable, which is coded as ‘high’ if more than 5% of audited resources were involved in corruption,¹¹ ‘low’ if less than 5% of audited resources involved in corruption, and ‘clean’ if no resources were involved in corruption; (b) a “high value corruption” variable, which is coded as ‘high’ if more than R\$156,800 was involved in corruption,¹² ‘low’ if less than R\$156,800 was involved in corruption, and ‘clean’ if no resources were involved in corruption; and (c) a simple dummy variable representing whether an audit found corruption or did not find corruption.

In order to capture voters’ ‘Corruption Accountability Experience,’ I code the outcome of the 2004 election with respect to the mayor in office between 2001-2004. The variable takes on one of four values: ‘reelected’ if the mayor in office between 2001-2004 was reelected in 2004; ‘thrown out’ if the mayor in office between 2001-2004 ran for reelection, but was not reelected; ‘term-limited’ if the mayor in office between 2001-2004 was in her second term; ‘chose not to run’ if the mayor in office between 2001-2004 was eligible to run for reelection but chose not to run.

Dependent Variables I measure aggregate electoral participation from the first round of municipal elections. The variables come from data available through the *Tribunal Superior Eleitoral’s* (TSE) database. Turnout is the number of votes, including blank and null votes, as a proportion of total eligible voters in a municipality. Blank votes are the number of blank votes as a proportion of total votes, and null votes are

¹¹ 5% is the median percent associated with corruption among those municipalities that had corruption violations

¹² R\$156,800 is the median amount of money associated with corruption among those municipalities that had corruption violations

the number of null votes as a proportion of total votes. Finally, I combine the total number of blank and null votes, and eligible voters who did not turn out, as a proportion of eligible voters to measure the total proportion of ‘*protest ballots*’.

Research Design: Difference-in-Differences My primary empirical strategy for identifying the causal effect of exposing corruption on political participation will be a difference-in-differences method. Difference-in-differences (DD) estimation is a quasi-experimental method that uses observational panel data. The simplest form of the DD method involves a treatment and control group with observations for at least two time-periods: one before the treatment and one after the treatment. Estimation of the causal effect involves measuring the changes in the outcome variable in the treatment group before and after the treatment period, and comparing this difference to the changes in the outcome variable in the control group before and after the treatment period. For simplicity, if we wanted to measure the effect of being audited on turnout, and we had data in two municipalities, we would estimate the model:

$$Y_{it} = \gamma_i + \lambda_t + \delta T_{it} + \varepsilon_{it} \quad (2.1)$$

Where Y_{it} is the turnout of municipality i at time t , λ_t is time variable that is 0 for the election prior to the audit treatment and 1 for the election after audit treatment. Variable γ_i is a municipality indicator, and ε_{it} is the error term. δT_{it} is a dummy variable for the audited municipality in a post-audit time period. This equation says that, absent treatment, turnout is determined by the sum of a time-invariant municipal effect γ_i and a time effect λ_t that is common across municipalities (Angrist and Pischke, 2008). With treatment, turnout is determined by the sum of the municipal effect γ_i , time effect λ_t , and treatment effect δT_{it} .

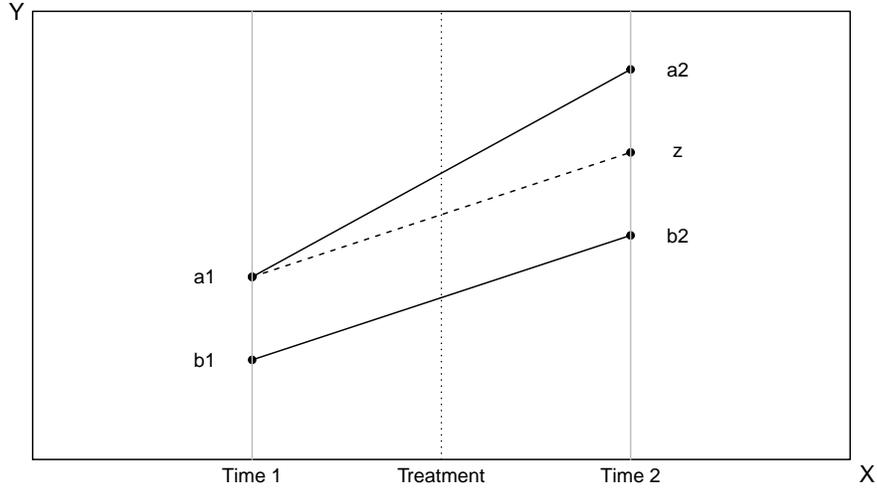
Figure 2.1 illustrates the intuition behind the model. In Figure 2.1, the outcomes are measured both before and after treatment for treatment group A and control group B . Simply comparing the outcomes between a_2 and b_2 would not accurately measure the causal effect of treatment because group A and B did not start at the same value. Instead, by assuming that the treatment group would have followed the parallel path of the control group, we can measure the treatment effect by comparing a_2 to z .

We can also run a difference-in-differences estimation with multiple time periods using regression:

$$Y_{it} = \alpha + \gamma A_i + \lambda T_t + \delta(A_i * T_t) + \varepsilon_{it} \quad (2.2)$$

Where Y_{it} is the outcome of municipality i at time t , T_t is a dummy time variable that is 0 for time

Figure 2.1: Illustration of Basic Difference-in-Differences Estimation



periods before treatment, and 1 for time periods after treatment, and A_i is the time-invariant variable that indicates whether the municipality was in the treatment or control group. The interaction of A_i with T_t measures the average treatment effect (ATE) by isolating treatment groups in the post-treatment period. Figure 2.2 illustrates the similar intuition behind DD with multiple time periods.

In my particular model, I use a DD design with multiple cross-sections and multiple time-periods, and, using turnout as an example, I estimate the model:

$$Y_{it} = \alpha + \lambda d_t + \gamma audit_i + \delta(audit_i * d_t) + \varphi_i + T_t + X_{it} + \varepsilon_{it} \quad (2.3)$$

Where Y_{it} is the outcome (turnout, blank votes, null votes) of municipality i at time t , d_t is a dummy time variable that is 0 for elections before 2004, and is 1 for audited municipalities for elections in 2004 and 2008, and $audit_i$ is a series of indicators that describe whether the municipality falls into one of four categories: 1) not audited; 2) audited with high corruption; 3) audited with low corruption; 4) audited and clean. I interact $audit_i$ with d_t to get δ , a series of conditional average treatment effects (ATE) of exposing audit outcomes on municipal outcomes over time. φ_i is a series of municipal fixed effects, T_t are time fixed effects, and X_{it} is a vector of control variables including GDP per capita, and the log of population. To

Figure 2.2: Illustration of Difference-in-Differences Estimation with Multiple Time Periods

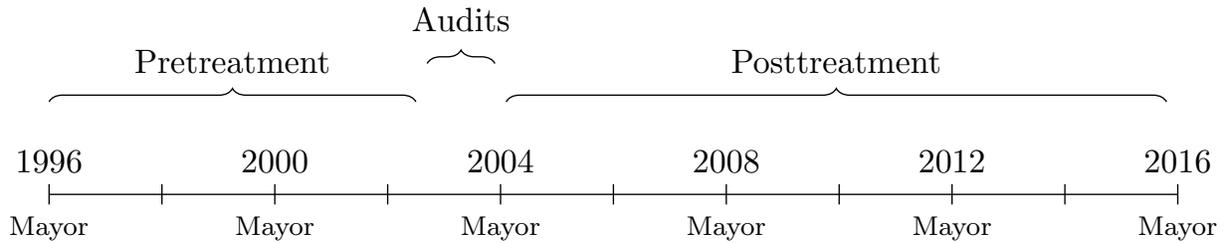
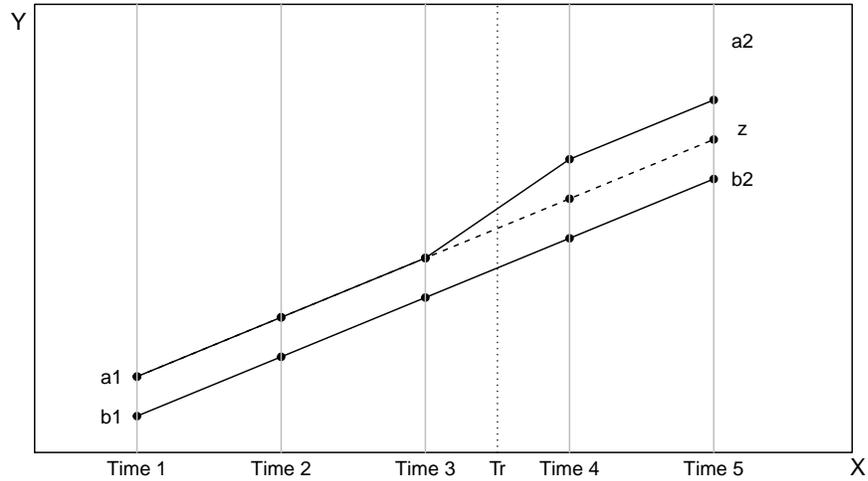


Figure 2.3: Electoral Timeline

correct for correlated errors at the treatment level, I include clustered standard errors at the municipal level.

Because difference-in-differences designs are best suited for medium-term analyses, I first restrict my analysis to the two municipal elections preceding audit publication, 1996 and 2000, and the two municipal elections following audit publication, 2004 and 2008. I also eliminate municipalities that were audited between 2005-2016 to avoid including the effects of future audits in my sample. I will then extend my analysis to also include the 2012 and 2016 municipal elections as well. Figure 2.3 describes the timeline of events.

To test my third hypothesis, I will analyze the effects of the outcome of the 2004 election in corrupt

municipalities on electoral behavior in the 2008, 2012, and 2016 municipal elections using an OLS regression with clustered standard errors. I will separate my sample into three categories: municipalities in which audits found corruption, municipalities in which audits found no corruption, and municipalities that were not audited. I will then test the effects of the 2004 electoral outcomes on turnout in each subgroup. I expect that municipalities in which audits found corruption and the incumbent was reelected, turnout will experience an even sharper decline in future elections. This symbolizes a failure in political accountability. In municipalities that were found to be clean, I expect the opposite effect: if the incumbent was reelected, turnout should increase in future elections. This represents a success of political accountability. Similarly, corrupt municipalities that successfully threw the corrupt incumbent out of office should experience an increase in political participation in the following elections. I will compare these results to the results of the subsample of municipalities that were not audited. I do not expect to find any effect of electoral outcomes on future participation in this subsample.

Control Variables All models will include a series of control variables. First, because economic development leads to higher literacy rates, life expectancy, educational attainment, and income, it is strongly and positively correlated with mass levels of political participation (Powell, 1982; Paik, 2012; Suh, 2008). Economic development is also strongly and negatively correlated with corruption levels (Mauro, 1995; Ugur and Dasgupta, 2011; Gupta et al., 2002). Therefore, to control for the possible confounding influence of economic development, I include the municipal GDP per capita.

Additionally, municipalities with higher populations are likely to have higher levels of urbanization and larger social networks, leading to higher levels of corruption (Goel and Nelson, 2011). Citizens in higher population centers are also more likely to have better access to public information leading to better information flows among citizens (Goel and Nelson, 2011). With better information flows, citizens are more likely have learned about the corruption information released in audit reports, and are more likely to participate in politics. Therefore, to account for these possible confounding factors, I also include the log of municipal population.

Figure 2.4 describes the structure of my data for those politicians who were eligible to run for reelection. Because I only include municipalities that were not audited in the period after the 2004 election, I begin with 4052 total municipalities. Of these 4052 municipalities, 419 underwent municipal audits between 2003-2004.

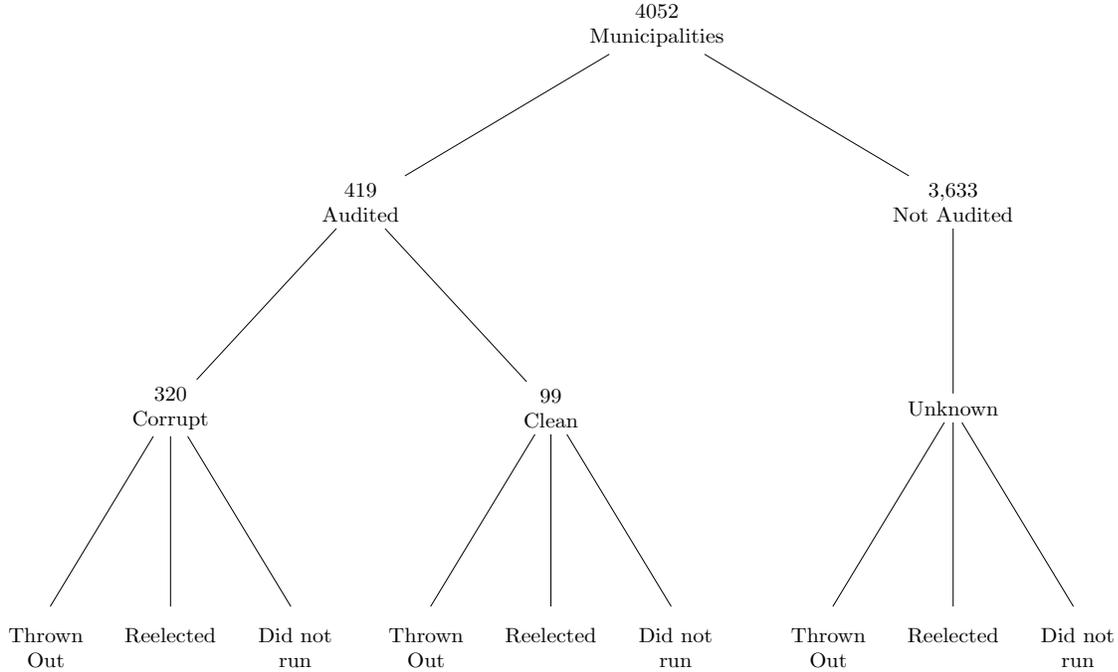


Figure 2.4: Structure of Municipal Data

320 audits revealed some form of corruption, whereas 99 audits revealed no corruption. These two categories are broken down into municipalities in which the local mayor either was term limited, was able to run for reelection but chose not to, was not reelected, or was reelected. 3,633 municipalities were not audited at all between 2004-20016. As a result, we cannot be sure how many municipalities have corrupt or clean municipal governments. This category is also broken into four groups: term limited, able to run but chose not to, not reelected, or reelected.

2.5 Results

Table B.2 displays the results for four fixed effects OLS regressions with clustered standard errors for the years 1996-2008. We are primarily interested in the direction and significance of the interaction term between the high corruption indicator and treatment variable. Surprisingly, the coefficient is positive and statistically significant, albeit small in magnitude: turnout is 1.6 percentage points higher on average in post-audit elections (2004 and 2008) in highly corrupt municipalities compared to municipalities that were never audited. There is a positive coefficient on the low corruption and treatment interaction term, but it is not significant. Also interesting is that the coefficient on blank votes is significant and in the opposite direction that

I hypothesized: blank votes are 0.4 of a percentage point lower on average in post-audit elections in highly corrupt municipalities compared to municipalities that were never audited.

Table 2.2: The effect of audits on political participation, conditional on the level of corruption. 1996-2008

	<i>Dependent variable:</i>			
	Turnout (1)	Blank Votes (2)	Null Votes (3)	All Protest 'Votes' (4)
High Corruption*Treatment	0.016* (0.007)	-0.004*** (0.001)	0.009 (0.009)	-0.010 (0.009)
Low Corruption*Treatment	0.002 (0.004)	-0.0005 (0.002)	0.008 (0.005)	0.004 (0.005)
Clean*Treatment	0.008 (0.006)	-0.002 (0.003)	-0.0004 (0.008)	-0.009 (0.008)
GDP per capita	0.001*** (0.0002)	-0.0001 (0.0001)	0.0002 (0.0001)	-0.0005* (0.0002)
log(population)	-0.054*** (0.006)	0.001 (0.002)	-0.014* (0.006)	0.041*** (0.008)
Observations	15,636	15,636	15,636	15,636
R ²	0.326	0.039	0.069	0.078
Adjusted R ²	0.090	-0.297	-0.257	-0.244

Note: *p<0.05; **p<0.01; ***p<0.001

Table 2.3 extends the number of elections included in the analysis to the 2012 and 2016 municipal elections. Contrary to what I would expect, the point-value on turnout actually increases: on average, turnout is 2.3 percentage points higher in corrupt municipalities than in municipalities that were not audited, which is 0.7 percentage points more of an increase than in the model that only included the elections between 1996-2008.

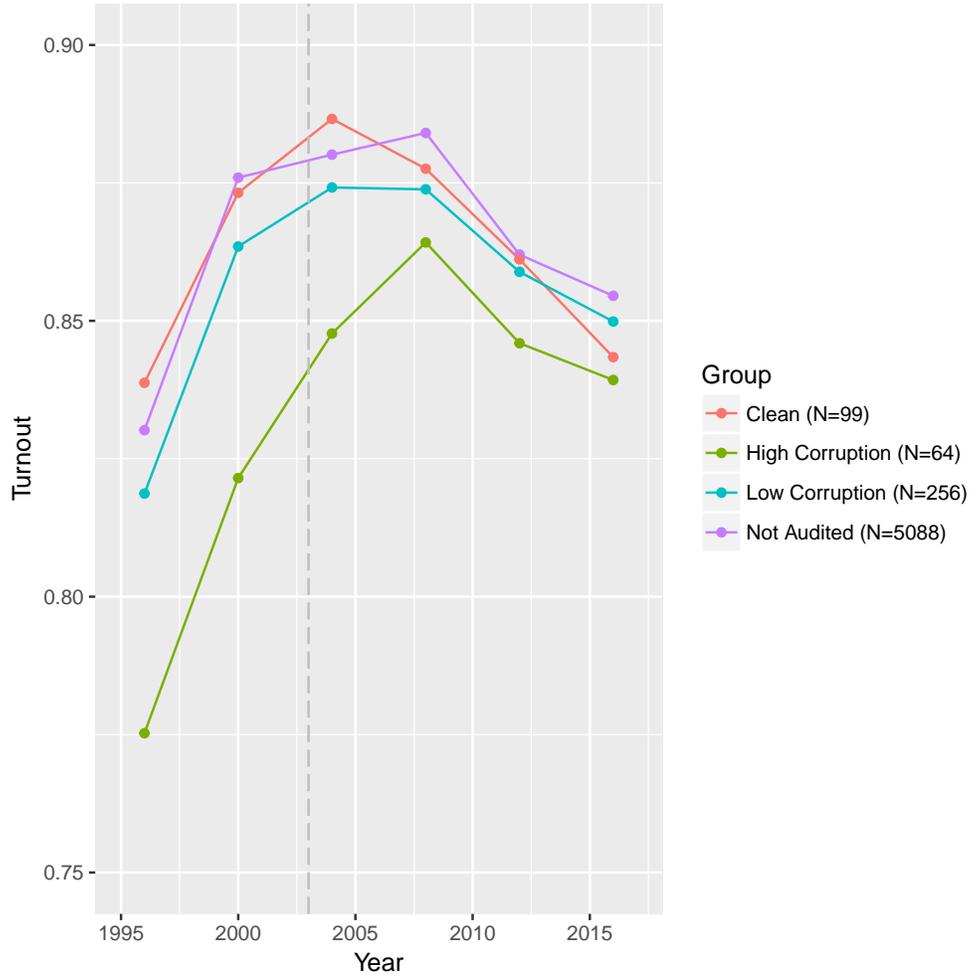
Table 2.3: The effect of audits on political participation, conditional on the level of corruption. 1996-2008

	<i>Dependent variable:</i>			
	Turnout (1)	Blank Votes (2)	Null Votes (3)	All Protest 'Votes' (4)
High Corruption*Treatment	0.033*** (0.008)	-0.005*** (0.001)	0.003 (0.006)	-0.024** (0.008)
Low Corruption*Treatment	0.006 (0.005)	-0.001 (0.002)	0.002 (0.003)	-0.003 (0.005)
Clean*Treatment	0.0005 (0.007)	-0.0003 (0.002)	0.002 (0.006)	-0.003 (0.008)
GDP per capita	-0.00000 (0.00000)	-0.00000*** (0.00000)	0.00001*** (0.00000)	0.00001* (0.00000)
log(population)	-0.043*** (0.005)	0.003 (0.002)	0.002 (0.005)	0.042*** (0.007)
Observations	18,546	23,724	23,724	23,724
R ²	0.176	0.027	0.037	0.052
Adjusted R ²	-0.172	-0.174	-0.162	-0.144

Note: *p<0.05; **p<0.01; ***p<0.001

As Figure 2.5 shows, revealing corruption only appears to mitigate the negative effect that corruption has on turnout in the first place: municipalities that were revealed to be corrupt still have a much lower turnout than municipalities that were revealed to be clean.

Figure 2.5: Average Turnout by Year and Audit Outcome (Descriptive Statistics Only)



Next, I look at whether the results of the 2004 election has a conditional effect on political behavior for the municipal elections in 2008, 2012, and 2016. In order to test this hypothesis, I divide the municipalities into three groups: audited and corrupt, audited and clean, and never audited. As a reminder the independent variable we are interested in is the outcome of the 2004 elections.¹³

Table 2.4 reports the results for the effects of the 2004 election on future turnout. Column 1 reports the results for the 2004 election on turnout in municipalities that were exposed for corruption. A ‘good’ accountability outcome for these municipalities would be ‘lost,’ meaning that the corrupt incumbent was held accountable and thrown out of office. Although hypothesized that this would lead to a stronger increase

¹³ This variable is coded as “Reelected,” for an incumbent who was eligible to run and won reelected, “Lost” for an incumbent who was eligible to run but lost the election, “Did not run” for an incumbent who was eligible to run but chose not to, and “Term-limited” for an incumbent who was not eligible to run for reelection. Term-limited is the reference category.

in turnout compared to other corrupt municipalities, the coefficient on ‘Lost’ is not significant. Similarly, a ‘bad’ accountability outcome for these corrupt municipalities would be ‘reelected,’ which I expected to lead to a stronger decrease in turnout compared to other corrupt municipalities. The coefficient on “Reelected,” although negative, is not significant. This suggests that the outcome of the 2004 election does not have an effect on long term political behavior: turnout will increase in corrupt municipalities regardless of whether the corrupt incumbent is thrown out of office.

Table 2.4: Turnout for Different Electoral Outcomes

	<i>Dependent variable:</i>		
	Corrupt (1)	Turnout Clean (2)	Not Audited (3)
Did not run	0.003 (0.007)	-0.013 (0.011)	-0.003 (0.002)
Lost	-0.002 (0.007)	0.004 (0.009)	-0.002 (0.002)
Reelected	-0.003 (0.006)	-0.004 (0.010)	0.001 (0.002)
GDP per capita	0.00001 (0.00001)	-0.0001*** (0.00002)	-0.00000 (0.00000)
log(Population)	-0.024*** (0.002)	-0.019*** (0.005)	-0.023*** (0.001)
Constant	1.080*** (0.028)	1.020*** (0.047)	1.074*** (0.014)
Observations	939	297	10,645
R ²	0.254	0.545	0.333
Adjusted R ²	0.227	0.496	0.331
F Statistic	9.625*** (df = 32; 906)	11.043*** (df = 29; 267)	165.567*** (df = 32; 10612)

Note:

*p<0.05; **p<0.01; ***p<0.001

Table 2.5 reports the results for blank votes. Here we can draw the same conclusions: the outcome of the 2004 election does not have an effect on future electoral participation. Blank votes will decrease in corrupt municipalities regardless of whether the corrupt incumbent is thrown out of office or reelected.

Table 2.5: Proportion of Blank Votes for Different Electoral Outcomes

	<i>Dependent variable:</i>		
	Corrupt (1)	Turnout Clean (2)	Not Audited (3)
Did not run	0.003 (0.002)	-0.001 (0.005)	0.001 (0.001)
Lost	-0.001 (0.001)	0.0003 (0.004)	-0.001 (0.001)
Reelected	-0.0003 (0.001)	-0.006 (0.003)	-0.0001 (0.001)
GDP per capita	0.00000 (0.00000)	-0.00000 (0.00001)	-0.00000*** (0.00000)
log(Population)	0.005*** (0.001)	0.005** (0.002)	0.004*** (0.0002)
Constant	-0.033*** (0.006)	-0.038* (0.017)	-0.032*** (0.003)
Observations	939	297	10,645
R ²	0.217	0.202	0.091
Adjusted R ²	0.189	0.115	0.088
F Statistic	7.833*** (df = 32; 906)	2.330*** (df = 29; 267)	33.289*** (df = 32; 10612)

Note:

*p<0.05; **p<0.01; ***p<0.001

2.6 Conclusion

This chapter argued that exposed corruption should negatively affect citizens' political efficacy and trust in political institutions, which should then decrease political engagement. However, several tests of exposed corruption on aggregate political behavior in Brazilian municipalities revealed the opposite: high levels of exposed political corruption led to a 1.6 percentage point increase in turnout and a 0.4 percentage point decrease in blank votes in the two elections following corruption's exposure. Furthermore, the outcome of the first election following the exposure of corruption has no subsequent effect on political participation.

Why, then, did the exposure of corruption in Brazilian municipalities cause citizen engagement to increase when the majority of the literature suggests we should have seen citizens in Brazil disengage from politics? One possibility is that Brazilians' relationship with their political institutions more closely resemble the relationship of citizens in older democracies and their political institutions.

Another possible explanation for the increase in political participation is related to the institution that uncovered corruption: because a trusted, nonpartisan domestic institution, the CGU, exposed political corruption, the new information did not have a net detrimental effect on citizens' trust in political institutions. Because the CGU is a governmental institution, citizens may have been more likely to view Brazilian political institutions as being part of a self-policing, trustworthy political system. The fact that the CGU was

able to expose corruption may have signaled to voters that institutions are both competent and trustworthy, thereby making citizens *more* likely to trust political institutions, and participate in subsequent elections.

The exposure of corruption may have increased external political efficacy because the CGU demonstrated the Brazilian government's commitment to eradicating corruption among its ranks. Voters have signaled their desire for the Brazilian government to eradicate corruption through large protests and through their voting behavior. Because the CGU's actions demonstrated that the Brazilian government created a competent institution to serve that function, this may have led to an increase in voters' external efficacy, which increased voters' propensity to participate in elections.

Finally, the exposure of corruption may have caused internal efficacy to increase because the audit reports contained credible information upon which voters could act. Internal efficacy is the belief that one understands politics and can competently participate in politics. Because the CGU is a nonpartisan institution engaged in policing members of other Brazilian institutions, citizens may have viewed the information it presented as both credible and consequential. Therefore, it is possible that receiving credible information about local corruption from the CGU increases voters' internal efficacy, which in turn increases political participation. The next chapter will use individual-level data to explore these possibilities.

Chapter 3

Exposed Corruption, Political Attitudes, and Political Participation

3.1 Introduction

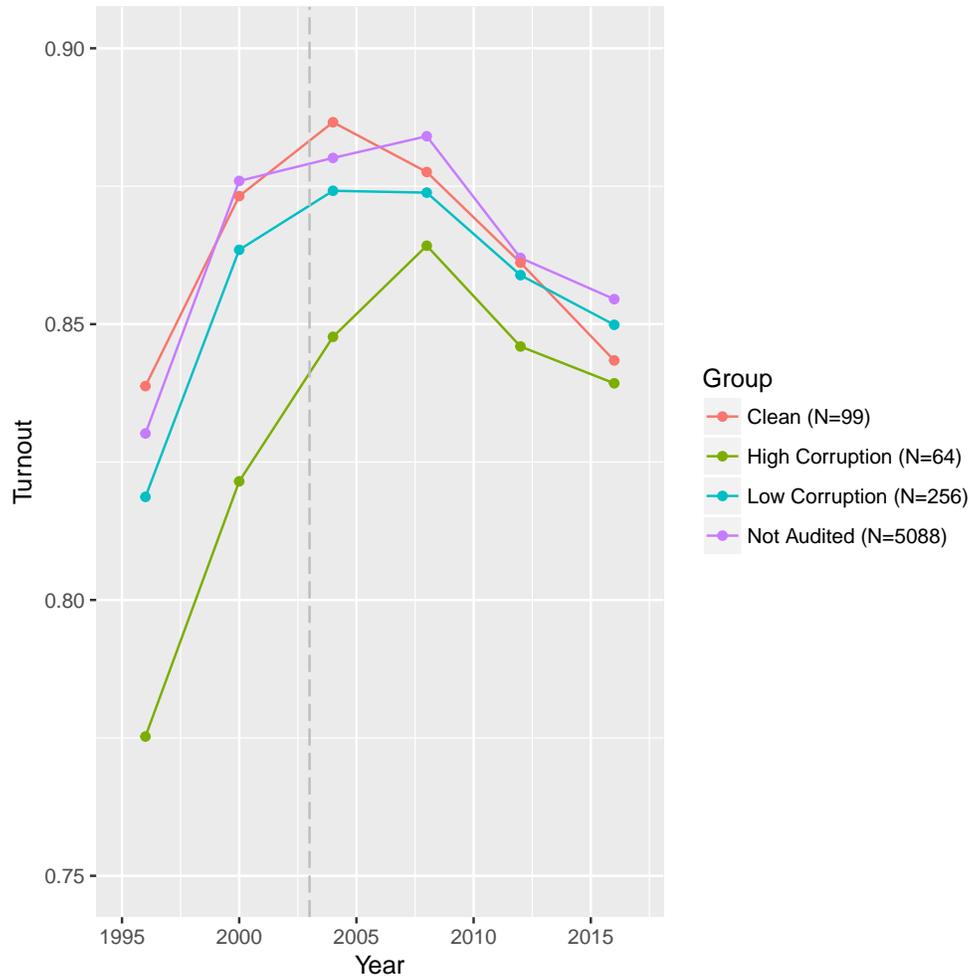
Many governmental institutions and organizations champion transparency as the best way to combat corruption in new democracies (IMF, 2016a,b; Chile Transparente, 2016). Numerous studies show that the presence of an unbiased media enhances representatives' performance (Snyder and Strömberg, 2010), improves government provision of public goods (Besley and Burgess, 2002), and decreases political mismanagement and corruption (Adsera et al., 2003). The mechanism through which information about performance and corruption achieves these desirable outcomes is not clear. Theoretically, the provision of information should enable citizens to hold politicians electorally accountable. However, the literature on the effects of providing information about corruption on electoral outcomes is inconclusive at best (Peters and Welch, 1980; Reed, 1999; Humphreys and Weinstein, 2012).

Even less encouraging for political accountability is the literature on the effects of providing information about corruption on aggregate levels of electoral turnout. Most recent studies provide evidence that exposed corruption has a dampening effect on political engagement in developing democracies (Olsson, 2014; Mishler and Rose, 2005; De Figueiredo et al., 2011; Chong et al., 2015). That is, after local corruption is exposed, citizens are less likely to turnout and vote.

My second chapter challenges the literature's conclusions that revealing corruption depresses electoral participation. In *Turning out or turning in? The effects of exposed corruption on political participation*, I provide evidence that publishing municipal audits containing evidence of high levels of political corruption leads to an *increase* in turnout. As Figure 3.1 demonstrates, highly corrupt municipalities are indeed associated with lower levels of turnout than clean and unaudited municipalities. However, when municipal audits containing evidence of corruption were published prior to the 2004 election, turnout in highly corrupt

municipalities increased significantly relative to other municipalities.

Figure 3.1: Average Turnout by Year and Audit Outcome (Descriptive Statistics Only)



Theoretical and empirical scholarship demonstrates that turnout suffers after the exposure of corruption through the exposed corruption’s effects on political efficacy and trust in political institutions (Mishler and Rose, 2001; Anderson and Tverdova, 2003; Olsson, 2014; ?). In this chapter, I will explore whether exposed corruption in Brazil had an unexpected effect on trust in institutions or political efficacy. I will argue that the identity of the audit institution that revealed corruption, the *Controladoria Geral da União* (CGU)¹ is the likely reason why trust in institutions and political efficacy *increased* following the exposure of corruption.

First, as a governmental institution performing an important public function, the CGU may have caused

¹The *Controladoria Geral da União* (CGU) was Brazil’s supreme audit institution until 2016, when it was absorbed into the the *Ministério da Transparência, Fiscalização e Controle* (MTFC). From this point onward, I will refer to Brazil’s Supreme Audit Institution as the CGU.

voters' trust in institutions to increase. Because the CGU is a governmental institution, auditing municipalities and successfully uncovering corruption signals to voters that Brazilian political institutions are a part of a self-policing political system. If voters believe that institutions are both competent and trustworthy, they will be more likely to participate in subsequent elections.

Second, the exposure of corruption may increase external political efficacy² because the CGU demonstrated the Brazilian government's commitment to eradicating corruption among its ranks. Despite the common belief that Brazilian voters are willing to elect corrupt politicians if the benefits outweigh the costs, recent evidence suggests that Brazilians are not so tolerant of malevolent government officials (Winters and Weitz-Shapiro, 2013). Because the CGU's competence demonstrates that the Brazilian government is committed to eliminating corruption, voters' external efficacy may increase in response to the release of audit reports. External efficacy is positively related to political participation.

Finally, the exposure of corruption may increase internal efficacy. Survey evidence suggests that voters view the CGU as a trustworthy institution. Because the CGU is a nonpartisan institution engaged in policing members of other Brazilian institutions, the information about corruption contained in audit reports represented credible information upon which voters could act. This credible information likely made voters feel more internally efficacious, which increased their desire to participate in politics.

The next section surveys the literature on trust in institutions, information credibility and exposed corruption. I will argue that receiving credible information about local corruption from a governmental institution may have increased voters' trust in the CGU and other national institutions. Because audit reports revealed corruption in municipal governments, however, I hypothesize that the exposure of corruption decreased citizens' trust in local institutions. In Section 3, I survey the literature on external efficacy. I will argue that corruption exposed by a governmental institution will cause voters to believe that the government is responsive to their demands for corruption eradication. As a result, their external efficacy should increase. Section 4 will survey the literature on internal efficacy. I will argue that credible, nonpartisan institutions that uncover corruption are more likely to cause voters to feel knowledgeable about politics. As a result, their internal efficacy should also increase.

²External efficacy is the belief that the political system responds to the desires of voters. Internal efficacy is the belief that one understands politics and can competently participate in politics. I will expand on the definition and determinants of efficacy in later sections.

Sections 5-7 will describe my data and methodological approach. To empirically test the effect of exposed corruption on political attitudes, I exploit the timing of two surveys in order to compare the political attitudes of citizens in audited Brazilian municipalities in the time-period *before* the audit reports were published to the political attitudes of citizens in audited Brazilian municipalities in the time-period shortly *after* the audit reports were published. Section 8 will present my main results. Overall, I find some evidence that exposing corruption caused trust in local institutions to decrease, and I find more substantial evidence that exposing corruption caused internal and external political efficacy increase. Trust in the CGU also increased following the exposure of corruption, but this effect did not extend to other national-level institutions. Furthermore, in Section 9, I find evidence that citizens with high political efficacy are more likely to say that they will vote in the following elections. The findings suggest that voters who believe that they can have a positive impact on political outcomes, but who also feel as though they need to closely monitor their local government are more likely to participate when audits reveal corruption. Section 10 concludes.

3.2 The origins and consequences of political trust

Trust is the belief that others will not deliberately cause us harm and will actively look out for our interests (Newton, 2007; Gambetta, 1988). Trust in institutions refers to the belief citizens have that institutions are competent, reliable, and serve the general interest (Miller, 1974b; Levi and Stoker, 2000; Devos et al., 2002). Thus, to say that a citizen trusts an institution is to say that the citizen approves of both the institution's purpose and the institution's performance. Depending on the political environment, trust in political institutions either may be difficult to change, coming from repeated experiences and entrenched cultural norms (Aberbach and Walker, 1970; Putnam, 1993; Inglehart, 1997), or it may be dynamic, responding to citizens' evaluations of recent institutional performance (Hetherington, 1998; Coleman and Coleman, 1994).

The theoretical relationship between corruption and trust in institutions is fairly straightforward. Democratic political institutions should encourage elected officials to engage in behaviors that benefit their constituents, and should discourage elected officials from engaging in behaviors that only benefit themselves. Because corruption is the abuse of public office for private gain, it is by definition antithetical to these goals. Therefore, learning about corruption may cause voters to conclude that their political institutions provide politicians with perverse incentive structures. If political institutions cannot prevent politicians from engag-

ing in self-serving behavior, voters will likely conclude that institutions are not competent or trustworthy.

There is a sparse body of existing empirical literature on the effect of exposed corruption on trust in institutions. Most of the literature focuses on *perceptions* of corruption rather than on new evidence of corruption, making it impossible to rule out issues of endogeneity. A fair amount of literature has demonstrated that perceptions of corruption are strongly correlated with distrust in the political system (Morris, 1991; Seligson, 2002; Canache and Allison, 2005). Chang and Chu (2006) find that even in Asian democracies, where corruption does not necessarily undermine economic growth, perceptions of corruption are still strongly associated with low trust in institutions. Using surveys across old and new democracies alike, Anderson and Tverdova (2003) demonstrate that citizens in countries with high levels of corruption have lower levels of trust in civil servants and are more likely to negatively evaluate the performance of political institutions. Morris and Klesner (2010) find that there is a reciprocal relationship between high perceptions of corruption and low trust in institutions, with both low trust in institutions increasing perceptions of corruption, and perceptions of corruption decreasing levels of trust in institutions.

Although existing literature on the effect of exposed incidents of corruption and trust in institutions is very limited, there is some evidence that exposed corruption decreases trust in institutions. Ares and Hernández (2017), for example, use a natural experiment in Spain to test the effects of a corruption scandal on political attitudes. They find that a corruption scandal involving only one party had a more systemic effect on political opinions, strongly eroding citizens' trust in all politicians.

Information Source and Trust in Institutions Given the scholarly evidence summarized in the previous section, we might hypothesize that providing citizens with information about corruption would have a detrimental effect on their trust in institutions. After all, politicians and bureaucrats are responsible for the corruption revealed in municipal audits, which demonstrates political institutions were not able to prevent illegal activities in the first place. It seems logical, therefore, that voters would lose confidence in political institutions after receiving this new information, and therefore be less likely to vote in future elections.

This perspective, however, neither distinguishes between local and national institutions, nor considers the source from which the corruption information originated. Because the CGU is a nonpartisan governmental institution, citizens are more likely to A) believe that the information contained in audit reports is accurate, B) update their beliefs about the trustworthiness of local institutions and C) separately update their beliefs

about the trustworthiness of federal institutions.

First, in the Brazilian case, corruption was not exposed by a biased opposition party, a partisan organization, or disgruntled business contacts. These actors may have incentives to fabricate or exaggerate evidence of corruption, in order to further their partisan or personal goals. Instead, corruption was exposed by a team of professionals who conducted a systematic audit of federal-municipal transfers at the behest of a federal governmental institution. These actors have stronger incentives to report accurate information about corruption³. Therefore, citizens are more likely to believe that information about corruption revealed in municipal audits is accurate.

An important assumption here is that citizens are able to distinguish between actors who have incentives to distort information and actors who have incentives to relay accurate information. The literature on information credibility, while sparse, does provide consistent evidence that citizens make these judgments and act accordingly. For example, Botero et al. (2015) conduct a survey experiment in Colombia and compare the effect of receiving information about corruption from the leading national newspaper, NGOs and the judiciary. Given Colombia's political culture surrounding these three institutions, Botero et al. (2015) deem the national newspaper as the most 'credible' source of information, and demonstrate that it has a significantly stronger effect on citizens' attitudes than the NGO and judiciary. (Muñoz et al., 2016) run a survey experiment in Catalonia in which the political party of the accused incumbent either acknowledges the incumbent's actions as corrupt (credible information) or in which the accused incumbent denies all wrongdoing (less credible). The authors find that citizens are significantly less likely to vote for the corrupt incumbent when the information they receive about corruption violations is deemed more credible.

Finally, in an article that provides the most important results for this chapter, (Weitz-Shapiro and Winters, 2017) conduct a survey experiment in Brazil in which they test whether Brazilian citizens distinguish between credible and non-credible sources of information about corruption. Although Weitz-Shapiro and Winters (2017) do not specifically name the CGU in their vignette, they use a 'federal audit' as their example of a credible source of information about corruption. They also use an opposition party as their example of a non-credible source of information about corruption. Weitz-Shapiro and Winters (2017) demonstrate that all voters deem non-partisan sources of information (federal audit) as credible, and view partisan sources

³I detail the reasons why we should trust the integrity of municipal auditors in the research design section below.

of information (an opposition party) as less-credible.⁴ Furthermore, they provide evidence that voters are more likely to act on information from a federal audit than information from an opposition party.

Both conclusions are pertinent to this chapter. First, because the CGU released information about corruption, it is unlikely that voters ignored the information provided. Just as Weitz-Shapiro and Winters (2017) demonstrate that voters distinguish between credible and non-credible sources of information in a vignette, voters should also be more likely to believe that actual information about corruption contained in a CGU audit report is credible. Second, Weitz-Shapiro and Winters (2017) demonstrate that voters are more likely to punish corrupt politicians when information is credible. This implies that citizens are more likely to update their beliefs based on credible sources of information, like audit reports.

In particular, we should expect that information about corruption causes voters to update their beliefs about local politicians and their municipal government. Because politicians and bureaucrats are responsible for the corruption revealed in municipal audits, citizens have clear evidence that local politicians violate the fundamental bases of political trust by failing to work in their constituents' best interests (Newton, 2007; Gambetta, 1988). Local institutions also violated citizens' trust by demonstrating their inability to prevent the illegal activities in the first place (Miller, 1974b; Levi and Stoker, 2000; Devos et al., 2002). As a result, citizens will likely conclude that local institutions are neither competent nor trustworthy. This brings me to my first hypothesis:

H1: Following an exposure of local government corruption by a governmental institution, trust in local institutions will decrease.

Conversely, information about corruption signals something very different about the CGU and federal institutions. If a political institution like the CGU were responsible for exposing corruption, learning about high levels of corruption may cause voters to conclude that their federal political system is self-policing and trustworthy. Trust in institutions refers to the belief citizens have that institutions are competent, reliable, and serve the general interest (Miller, 1974b; Levi and Stoker, 2000; Devos et al., 2002). By uncovering corruption that is detrimental to the local economy, the CGU has demonstrated that it meets all three of these criteria. Additionally, because other national-level institutions created the CGU in 2003 in response to citizens' demands for more transparency and accountability, citizens' trust in other national-level institutions

⁴In particular, they show that highly educated citizens are more likely to distinguish between credible and non-credible sources of information.

may also increase in response to this new information. This brings me to my second hypothesis:

H2: Following an exposure of local government corruption by a governmental institution, trust in the CGU and other national-level institutions will increase.

3.3 Exposed Corruption and External Efficacy

Trust in institutions is not the only political attitude that may affect citizens' propensity to vote. The CGU's audit reports may have also caused citizens' external efficacy to increase, thereby increasing political participation.

Efficacy refers to an individual's sense that they are competent and powerful enough to affect political outcomes. General political efficacy is composed of two elements: external and internal efficacy. External political efficacy refers to the belief in one's own ability to influence the political process (Milbrath 1965). Citizens with high external political efficacy believe that the political system is responsive to voters' demands; whereas citizens with low external political efficacy believe that the political system is not responsive to voters' demands. Both systemic and individual-level factors can affect external political efficacy. For example, the presence of representative term-limits increase electoral competitiveness, and as a result, is linked to higher aggregate levels of external political efficacy (Bowler and Donovan, 2012). Individual-level characteristics that affect external political efficacy include strong party preferences (Karp and Banducci, 2008), socio-economic status (Murphy, 2011), and early socialization (Davies, 1965).

Existing empirical work suggests that learning about corruption has a negative effect on external political efficacy (Olsson, 2014; ?). Corruption, such as bribery or over-billing, represents a loss of public funds that should have benefited constituents through public goods provision. Because most voters prefer that public money funds public projects rather than politicians' bank accounts, learning about corrupt practices signals to voters that politicians are not responsive to their demands for honest civil service. As a result, learning about corrupt practices erodes voters' sense of external political efficacy. The literature generally supports this relationship, providing evidence that political efficacy mediates the relationship between corruption perceptions and low turnout (Olsson, 2014). If politicians will not respond to their constituents, voters have less desire to participate in the political process.

In the case of exposed corruption in Brazil, however, the identity of the CGU as a governmental institution may alter the relationship between exposed corruption and external political efficacy. Instead of demonstrating that the political system does *not* respond to voters' demands for more accountability, the CGU demonstrates that the federal government has already responded to voters' demands by creating a competent audit institution. By exposing local corruption that was previously hidden from public view, the CGU's actions demonstrated that the Brazilian government is committed to eradicating corruption among its ranks. As a result, citizens may feel as though they have already exerted influence over the political process, thereby increasing their sense of external efficacy. This brings me to my third hypothesis:

H3: Following an exposure of local government corruption by a governmental institution, external political efficacy will increase.

3.4 Exposed Corruption and Internal Efficacy

Internal political efficacy is the belief that one understands the political process and can effectively participate in politics (Karp and Banducci, 2008). Existing work suggests that internal efficacy is relatively stable over time (Krampen, 2000), but does increase with perceived political knowledge (Hofstetter et al., 1999; Bennett, 1997), types of political media coverage (Pedersen, 2012), and whether the citizen believes her in-group is politically powerful (Koch, 1993).

Most of the literature on the effects of corruption on efficacy focuses on how corruption corrodes external, rather than internal, efficacy (Olsson, 2014; ?). However, parallel arguments might be made as to why exposed corruption can negatively affect internal political efficacy as well. When an elected official is exposed for being corrupt, a plurality of the electorate must face the reality that they voted for a corrupt politician. Assuming that corruption is an important factor in voters' electoral calculations, learning about high levels of corruption may cause voters to doubt their own political aptitude and lose their sense of internal efficacy.

The identity of the CGU as a governmental institution is unlikely to interrupt this specific internal efficacy mechanism: regardless of the source of information, some citizens will recognize that they voted for a corrupt politician, and doubt their ability to make intelligent political decisions. However, the CGU may

affect internal political efficacy through a different mechanism. Specifically, if voters believe that the CGU is an unbiased and credible source of information about corruption, the information contained in audit reports will represent new, useful political knowledge. This new political knowledge is likely to increase citizens' confidence in their ability to make smart political decision, which would increase their internal political efficacy.

This argument once again relies on the assumption that citizens are able to distinguish between actors who have incentives to distort information and actors who have incentives to relay accurate information. As I discussed in the previous section, the literature provides consistent evidence that citizens can distinguish between politically motivated sources of information and unbiased sources of information (Muñoz et al., 2016). Weitz-Shapiro and Winters (2017) once again provide important evidence for this argument. By demonstrating that Brazilian citizens believe that federal audits are more credible than opposition parties, their research suggests that citizens will be likely to use audits to instruct their future political decisions. Because political knowledge is a key factor in individuals' sense of internal efficacy (Reichert, 2016), this credible information will likely increase internal efficacy. This brings me to my fourth hypothesis:

H4: Following an exposure of local government corruption by a governmental institution, internal political efficacy will increase.

3.5 Data and Research Design

In order to test the effect of exposed corruption on political attitudes, I exploit the timing of two surveys in order to compare the political attitudes of citizens in audited Brazilian municipalities in the time-period before the audit reports were published to the political attitudes of citizens in audited Brazilian municipalities in the time-period shortly after the audit reports were published. I estimate the following OLS models with clustered standard errors:

$$D_{msy} = \alpha + \beta_0 A_{msy} + \beta_1 C_{msy} + \beta_2 (A_{msy} \times C_{msy}) + X_{msy} \gamma + v_s + w_y + \epsilon_{msy} \quad (3.1)$$

A_{msy} indicates that the audit took place after the election in municipality m in state s in year y , and C_{msy} is the number of corruption violations in an audit report. D_{msy} is a battery of political attitude variables including trust in national institutions, trust in local institutions, and internal and external efficacy. X_{msy}

is a matrix of control variables, v_s are state fixed effects, w_y are year fixed effects and ϵ_{msy} is the error term. β_2 estimates the effect of revealing corruption on political attitudes.

I use data from three primary sources: the 2nd through 6th waves of the Latin American Public Opinion Project (LAPOP), the 2008-2011 Latinobarometer surveys, and coded audit data provided by *Controladoria Geral da União* (CGU). Both LAPOP and Latinobarometer provide the names of municipalities in which each survey respondent lived, and the date that the survey took place. I use this information to match respondents in municipalities that were audited to their audit report data as provided by the CGU. My ‘treatment’ group consists of respondents that were interviewed in the 15-month time-period after audit reports were published. This window is large enough to encompass a sufficient number of respondents, but small enough that the audit reports should still remain fresh in the respondents’ minds.⁵ In order to increase my N , however I expand my control group to 30 months prior to the publication of audit reports.

Audit Reports and the CGU

Beginning in 2003, Brazil’s supreme audit institution, the CGU began randomly choosing municipalities with fewer than 450,000 residents for audit. The CGU began with 26 municipal audits in 2003, but raised the number to 50 and then 60 municipalities per round in 2004. The public is invited to all rounds of the audit lottery, which take place every 2-4 months. After a municipality is chosen, a group of 10-15 auditors are sent to each municipality to look for irregularities in the municipal spending of federal funds.

The selection of auditors is highly competitive. Mean salaries for auditors more than doubled between 2002 and 2008 alone, and have remained comparable to the position of ‘financial and control analyst’ (*Analista de Finanças e Controle*), which is considered “one of the best-paid careers within the federal public administration” (OECD, 2012, pg 208). Combined with the CGU’s stringent standards for hiring auditors, this makes it far less likely that auditors will be corrupt when faced with mayors who are desperate to hide their illicit activities. Brazilians also have a considerable amount of trust for the CGU compared to other Brazilian institutions. Survey respondents in the 2008 wave of LAPOP were asked to rank their trust for various governmental institutions on a scale of 1 (no trust) to 7 (a lot of trust). When asked about the CGU, 75.6% of respondents responded with a 4 or above. Only 59.5% gave the same score for their mu-

⁵It is also the window of time that (Ferraz and Finan, 2008) use when constructing their treatment group.

municipal government, 46% gave the same score for Congress, and 33% gave the same score for political parties.⁶

Once the auditors have completed their inspection of municipal accounts and public goods delivery, they create a report detailing all irregularities they uncovered. These reports are posted on the CGU's website and disseminated to all levels of the government.

Dependent Variables We are interested in several political attitudes including trust in local institutions and national political institutions, and internal and external efficacy. My primary measure of trust in local institutions will be the respondent's trust in their municipal government. I also test trust in two mid-level institutions, the state police and the state government. My primary measure of trust in national-level institutions is trust in congress, the judiciary, the president, and the CGU. Finally, I construct two additive variables that represent trust in all federal institutions: one with the sum of trust scores for all four national-level institutions, and one with the sum of trust scores for congress, the judiciary, and the president.

I operationalize trust in political institutions using a seven-category variable based on question in the LAPOP and Latinobarometer surveys. The LAPOP survey question is "To what extent do you trust [political institution]?" Answers range from 1, which is "no trust" to 7, which is "a lot of trust." The Latinobarometer survey asks a similar question: "Please look at this card and tell me, how much trust do you have in each of the following groups/institutions. Would you say you have a lot, some, a little, or no trust?" I code the Latinobarometer question so that "No trust" takes the value 1, "A little trust" takes the value 3, "Some trust" takes the value 5, and "A lot of trust" takes the value 7.

One additional distinction we can make in order to get at the causal effect of the source of information on trust in institutions is to distinguish between citizens' preferences of national-level control over local resources versus local-level control over local resources. In other words, if voters believe that the revelation of corruption indicates that national-level institutions are competent and local-level institutions are incompetent, this should also impact their preferences for increased federal control over local resources.

A shift in preference away from local control over resources towards federal control of resources is equivalent to a shift in preference for centralization over decentralization. Generally speaking, citizens prefer

⁶Only the President and military scored higher with 85% and 80% respectively.

more decentralization because subnational governments are “closer” to the people, and are believed to be more accountable to citizens’ policy preferences. In the literature, this usually means that as a country becomes more democratic, citizens will demand a more decentralized government (Bird and Vaillancourt, 1998; World Bank, 1997; Nickson, 1995; Souza, 1997). For example, Eaton (2004) finds that during Brazil’s democratization, local elections generated pressures to decentralize fiscal resources.

Although much of the literature on decentralization demonstrates that citizens have a general preference for decentralization over centralization, there is some evidence that preferences for decentralization responds to short-term evaluations of government performance. Montalvo (2009), for example, finds that satisfaction with municipal services and trust in municipal government is strongly associated with a preference for fiscal decentralization, whereas satisfaction with the national economic situation and trust in the national government is associated with a preference for fiscal centralization. Because I expect that citizens’ trust in national institutions increases after the exposure of corruption, it therefore follows that their preferences for centralization should increase as well.

To measure citizens’ preference for centralization, I will use two questions from the 2008 LAPOP survey. First, to test respondents’ preferences for political centralization, I will use the question “Taking into account existing public services in the country, who should be given more responsibilities?” For financial centralization, I will use the question, “Taking into account existing economic resources in the country, who should administer more money?” Possible answers include, “Much more [responsibility should be given] to the federal government,” “A little more [responsibility should be given] to the federal government,” “The same amount of responsibility [should be given] to the federal government and municipality,” “A little more [responsibility should be given] to the municipal government,” and “Much more [responsibility should be given] to the municipal government.” I code these as ranging from 1 to 5, with 1 signifying that the respondent wants the municipal government to have more responsibility, and 5 signifying that the respondent wants the federal government to have more responsibility. I also use an additive variable of the political and fiscal centralization variables to measure the general centralization.

Finally, I measure internal and external efficacy using two questions from 2008, 2010, 2012 and 2014 LAPOP surveys. For external efficacy, I use the question, “Those who govern this country are interested in what people like you think. How much do you agree or disagree with this statement?” For internal efficacy,

I use the question “You feel that you understand the most important political issues of this country. How much do you agree or disagree with this statement?” Answers range from 1, ‘strongly disagree’ to 7, ‘strongly agree.’ I also use an additive variable of the external and internal efficacy variables to measure general political efficacy.

Independent Variables My main explanatory variable is an interaction between the number of corruption violations revealed in the CGU’s audit report, and whether the survey took place before or after the audit report was released.

My measure of corruption is the number of corruption violations reported in municipal audits that took place between 2006 and 2015. Beginning in 2006, the CGU began coding irregularities into three categories: ‘formal,’ ‘medium,’ and ‘severe.’ Formal violations generally constitute procedural issues, such as failing to fill out the proper paperwork. In order to create my measure of corruption, I will sum the number of these medium and severe violations for each municipality.⁷

Control Variables To control for confounding factors that may affect both corruption and political attitudes, I include several individual and municipal-level characteristics. First, municipal characteristics include population, and the amount of money audited by the CGU. These data are from the CGU and the *Instituto Brasileiro de Geografia e Estatísticas* (IBGE). Individual-level variables are derived from both surveys and include gender, years of education, and age. Finally, all models include year, state, and survey fixed-effects.

Data Description

The sample contains a total of 830 respondents across 45 municipalities, for an average of 18.4 respondents per municipality. 615 respondents were surveyed before their municipality’s audit report was published, and 215 respondents were surveyed after their municipality’s audit report was published.⁸ The majority of respondents, 599, were interviewed by LAPOP. The remaining 231 respondents were interviewed by the Latinobarometer. Figure 3.2 reports the distribution of respondents by the survey year, and by whether the

⁷Avis et al. (2016b) also use this as their measure of corruption in a paper that tests whether Brazil’s municipal audit program reduces corruption.

⁸There is a large discrepancy between the number of respondents before and after the audit report is released because I extend the pre-audit window to 1000 days, but keep post-audit window to only 500 days. Because we are interested in knowing the level of corruption in respondents’ municipalities before the audit information is released, it is important to keep the window within the time period that the CGU’s audit teams will retroactively audit financial documents. A smaller window after audit publication is preferable because audit results will be more fresh in respondents’ minds.

survey took place before or after the respondents' municipal audit report was published.

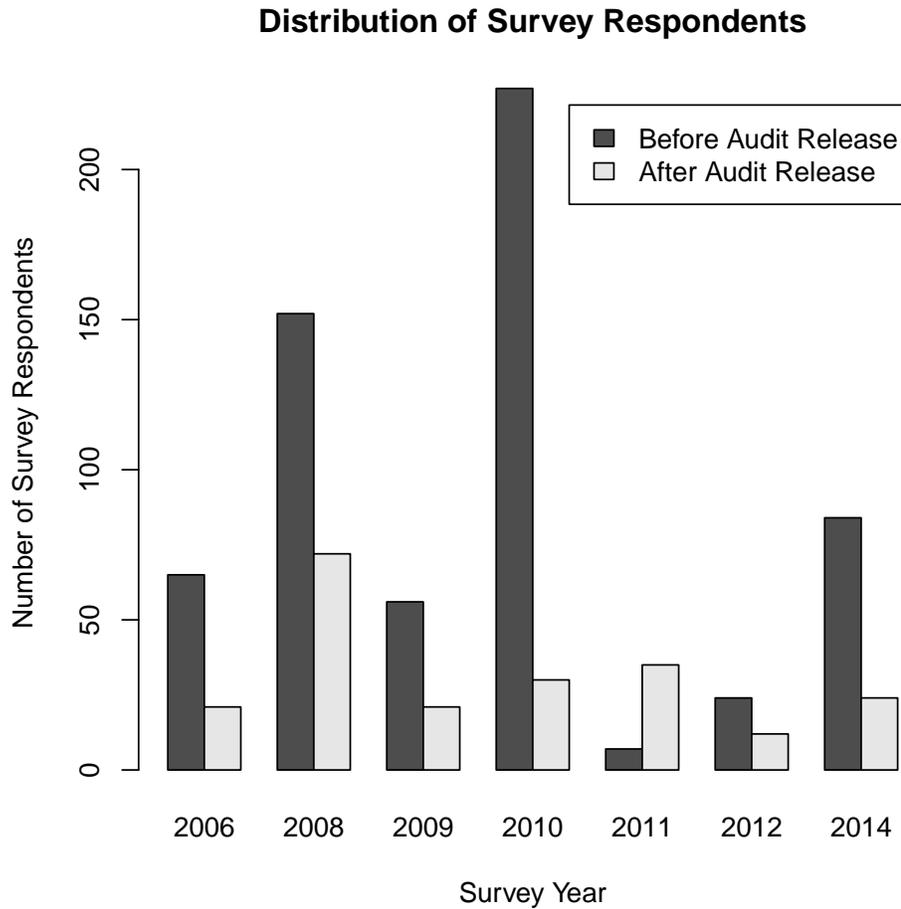


Figure 3.2: Distribution of Survey Respondents by Survey Wave and Survey Timing

Both the LAPOP and Latinobarometer conduct interviews with Brazilians on a fairly regular interval: LAPOP interviews generally take place at the end of March, and Latinobarometer interviews generally take place in October. With few exceptions, the CGU publishes audit reports for all 60 audited municipalities in a lottery at the same time. However there is no pattern to their publication dates. Because surveys and audit publications do not coincide, and because many respondents surveyed did not live in a municipality that was audited within the specified time frame, some years, specifically 2008 and 2010, have considerably more respondents than others.

Descriptive Statistics

The LAPOP and Latinobarometer surveys are representative samples of Brazil. However, each sample of residents within a municipality is not a representative sample of the municipality’s residents.⁹ Additionally, although the CGU takes a random sample of municipalities for audit, the audited municipalities that had residents surveyed within the designated window of time before or after their audit was published is also not a random sample of municipalities. Therefore, to get a better idea of how representative my respondents and municipalities are, I present descriptive statistics comparing my sample of respondents to all Brazilians. I also compare my sample of municipalities to all audited municipalities, and to all municipalities regardless of whether they were audited.

	My Data	Audited Munic	Brazil
GINI Index	0.53	0.51	0.52
Percent Living in Rural Area	19.42	37.76	16.00
Log(Amount Audited)	20.11	18.49	
Number of Corruption Violations	68.27	61.42	
Number of Parties, 2008 election	3.36	2.62	2.65
Turnout, 2008 election	85.55	88.03	84.30

Table 3.1: Municipal Characteristics

	My Data	Brazil
Average Age	40.15	31.30
Years Formal Education	7.04	7.20
Percent Male	48.67	49.24
Percent Single	28.80	25.60

Table 3.2: Individual Characteristics

My sample is remarkably similar to the rest of audited municipalities and to the rest of the Brazilian population. The most noticeable discrepancies are the municipal percent living in a rural area, and respondents’ ages. The difference in the average age between my sample and the general population is large because the survey excludes children under the age of 16.

The percent living in a rural area is significantly different in my sample due to the difference between the CGU’s and the two surveys’ sampling methods. LAPOP and Latinobarometer are more likely to sample from larger, more urban municipalities whereas the CGU gives all municipalities with fewer than 450,000 inhabitants the same weight.

⁹LAPOP changed their sample design beginning in 2012 to make samples representative at the municipal level. However, the vast majority of my sample is drawn from survey waves conducted before 2012.

3.6 Results

Table 3.3 presents the results for Hypothesis 1. I expect that trust in local institutions will decrease following the exposure of corruption because citizens will blame local institutions for failing to prevent corruption. Models 1 and 2, which estimate the effects of exposed corruption on trust in the municipal government and state police respectively, provide preliminary support for this hypothesis: the interaction between the after-audit indicator and the number of corruption violations is negative and significant for both models, indicating that corruption has a marginally negative effect on trust in these institutions after corruption is revealed. The effect of releasing audit information itself, however, has a strong positive effect on citizens trust in these two local institutions when corruption is zero.

Table 3.3: Average Effects of Release of Audits on Trust in Local Institutions Conditioned on the Level of Corruption

	Trust Munic Gov (1)	Trust State Police (2)	Trust State Gov (3)
Number Corrupt x After Audit	-0.01** (0.004)	-0.01*** (0.004)	0.01 (0.01)
Number Corrupt	0.001 (0.002)	0.01** (0.003)	0.01** (0.005)
After Audit	0.67** (0.34)	1.46*** (0.36)	-0.38 (1.01)
Education	0.01 (0.02)	-0.02 (0.02)	0.04 (0.03)
Age	0.02*** (0.005)	0.01** (0.005)	0.01 (0.01)
Male	0.13 (0.13)	-0.04 (0.13)	0.20 (0.18)
Amount Audited	-0.00* (0.00)	-0.00 (0.00)	0.00 (0.00)
Population	0.0000* (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)
Constant	2.89*** (0.77)	3.21*** (0.67)	1.01 (0.92)
N	803	811	431
R ²	0.16	0.17	0.11

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and state, year, and survey fixed effects.

Because my sample does not include any municipalities with zero corruption violations, I plot the marginal effect of releasing audits on trust in the municipal government, conditioning on the corruption in Figure 3.3. We first note that releasing audits with very low levels of corruption does have a *positive* effect on voters' trust in institutions. This is consistent with the idea that voters have prior expectations about their local government's behavior, and adjust their trust evaluations with respect to how the municipal government deviates from those expectations. If voters observe a significantly lower amount of corruption than anti-

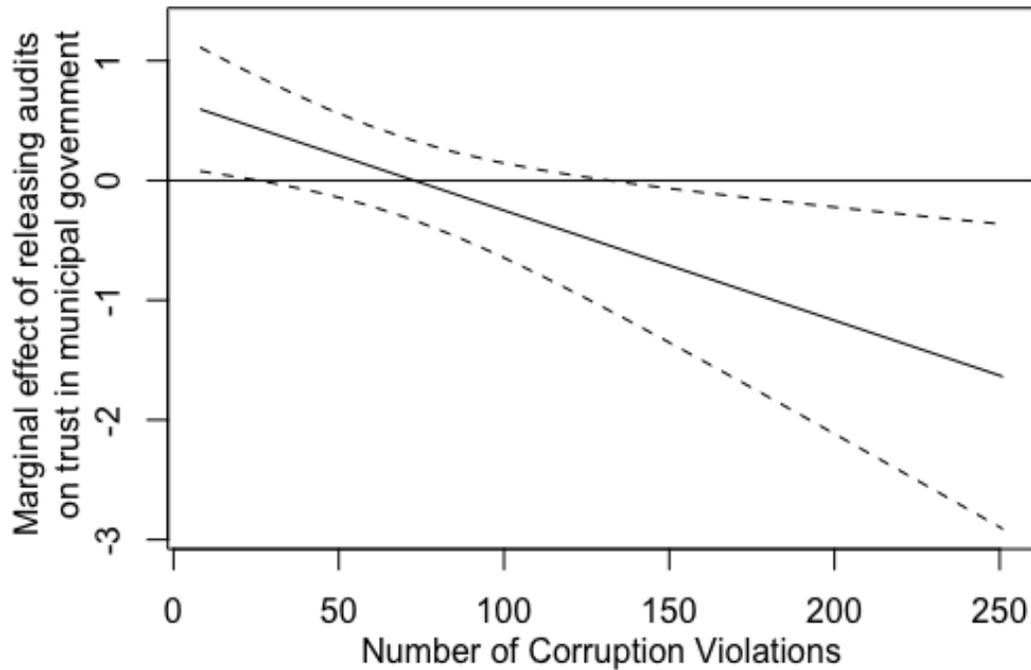


Figure 3.3: Marginal effect of releasing audit information on trust in municipal government, conditioning on corruption

patented, they gain trust in their municipal government. Trust decreases, however, as corruption increases. In the middling levels of corruption, voters neither gain nor lose trust in their municipal institutions. At the highest levels of corruption (about 140 violations), voters begin to lose trust in their municipal institutions. It takes a high number of violations, however, for audit information to have a significant effect on trust in municipal institutions. Because there are only 68 respondents in 4 municipalities with more than 140 corruption violations, it's unwise to draw any strong conclusions about the negative effect of high levels of exposed corruption on trust in the municipal government.

To get a more holistic view of local institutions, I also look at trust in the state police in Model 2, and the State government in Model 3. Although the state government is not included in the audit information released, it is possible that the effect of revealing audits on attitudes toward local government bled into citizens' attitudes toward other non-federal institutions. Although I do find a negative and significant effect

of revealing corruption on state police, I do not find the same effect of revealing corruption on the state government. These results suggest that voters correctly attributed municipal corruption to only the municipal government; no other local institution is affected by exposed corruption.

Moving on to voters' trust in national-level institutions, Table 3.4 presents the results for Hypothesis 2. I expect that trust in national-level institutions will increase following the exposure of corruption due to the CGU demonstrating its competence and signaling that the government is part of a self-policing system. Column 1 shows that trust in the CGU decreases as corruption increases in municipalities that were not yet audited. This suggests that voters in municipalities with more corruption have a lower baseline of trust in Brazil's supreme audit institution before audit reports provide concrete evidence of corruption. Voters in corrupt municipalities likely see evidence of poorly executed or incomplete public works projects and suspect that their local government is corrupt. Their lower trust in the CGU may reflect their frustration that the CGU had yet to uncover the malfeasance. However, the negative effect of corruption on trust in the CGU is mitigated by revealing the results of audits in corrupt municipalities. As the interaction between the number of corruption violations and the after-audit indicator shows, trust in the CGU increases significantly after the audit's publication, conditioning on the level of corruption. Figure 3.4 presents the marginal effect of releasing audits on trust in the CGU, conditioning on the level of corruption. Releasing audits has a positive effect on trust in the CGU even at low levels of corruption. The marginal effect of released audits on trust in the CGU hits ceiling at about 100 corruption violations, however, because the trust variable reaches its maximum at a value of 7.

Table 3.4: Average Effects of Release of Audits on Trust in National Institutions Conditioned by the Level of Corruption

	Trust CGU	Trust President	Trust Congress	Trust Judiciary	Trust Fed Inst 1	Trust Fed Inst 2
	(1)	(2)	(3)	(4)	(5)	(6)
Number Corrupt x After Audit	0.07* (0.04)	0.01 (0.01)	-0.01 (0.004)	-0.005 (0.004)	0.26* (0.15)	0.02 (0.02)
Number Corrupt	-0.16*** (0.06)	0.01** (0.003)	0.002 (0.002)	0.003 (0.003)	-0.49** (0.24)	0.02** (0.01)
After Audit	1.46 (1.10)	-0.61 (0.75)	0.36 (0.34)	0.50 (0.35)	1.53 (3.56)	-1.25 (1.75)
Education	0.07* (0.04)	-0.04 (0.02)	-0.05*** (0.02)	-0.01 (0.02)	0.03 (0.12)	-0.15*** (0.05)
Age	0.02** (0.01)	0.01** (0.01)	-0.001 (0.005)	0.01 (0.005)	0.05* (0.03)	0.02 (0.01)
Male	0.34 (0.24)	0.17 (0.15)	-0.05 (0.13)	0.05 (0.13)	0.79 (0.75)	0.11 (0.36)
Amount Audited	0.0000*** (0.0000)	0.00** (0.00)	-0.00** (0.00)	0.00 (0.00)	0.0000** (0.0000)	0.00 (0.00)
Population	-0.0002*** (0.0001)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)	-0.001** (0.0003)	-0.0000 (0.0000)
Constant	3.64 (2.44)	4.00*** (0.70)	3.27*** (0.71)	3.99*** (0.69)	19.75** (8.14)	10.75*** (1.85)
N	201	582	788	803	198	559
R ²	0.17	0.23	0.14	0.12	0.27	0.24

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and state, year, and survey fixed effects.

Federal Institutions 1 is a measure of trust in all four national-level variables (CGU, President, Congress and Judiciary).

Federal Institutions 2 is a measure of trust in only the Presidency, Congress, and the Judiciary.

The results for the CGU provide some support for my first hypothesis: voters in more corrupt municipalities were more likely to trust the CGU, a federal institution, after the release of their municipality's audit. Columns 2-4, however, show that this effect is confined to CGU. Publishing audit reports do not have a marginal effect on trust in any other federal institution, regardless of the number of corruption violations revealed.

Finally, in Model 5, we find a positive and significant effect of exposed corruption on trust in a composite national-level institutions variable, which includes the CGU, Presidency, Congress, and the Judiciary. However, it is likely that this is primarily driven by trust in the CGU. In Model 6, I present the results for a composite national-institutions variable that excludes the CGU. Therefore, at best, there is only very tepid evidence for Hypothesis 1: Trust in the CGU increases following the release of audits, conditioning on the level of corruption. However, trust in other national-level institutions does not increase under the same conditions.

Thus far, I have presented some evidence for the differential effects of exposed corruption on citizens' trust in local versus federal institutions: at high levels of corruption, voters' trust in their local government decreases, and their trust in the CGU increases. If voters indeed believe that federal institutions are more competent at handling resources than local government, this may extend to a preference for centralization. Table 3.5 reports the results for Hypothesis 3, which posits that the exposure of corruption will cause citizens to prefer more centralization.

Model 1 presents the results for a preference for fiscal centralization, and Model 2 presents the results for a preference for political centralization. Although the interaction terms are positive, they are not significant. Model 3 presents the results for a composite variable of both types of centralization without state fixed effects. Here, we see the expected positive and significant association between revealed corruption and increased preference for centralization. Every additional corruption violation revealed from an audit's publication leads to a 0.02-point increase on the 10-point composite centralization scale. Figure 3.5 presents the marginal effect of releasing audit reports on preference for centralization, conditioning on corruption. It shows that, for low levels of corruption, releasing audit reports has a negative marginal effect on preferences for centralization. Although higher levels of corruption decrease the marginal effect of releasing audits, the effect is never significant and positive. When we add state fixed effects in Model 4, the coefficient increases by 50%, but loses its significance.

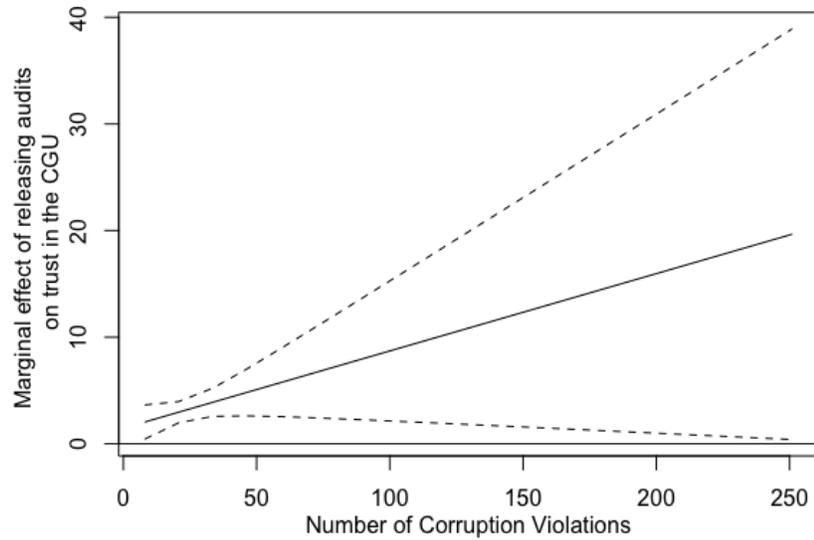


Figure 3.4: Marginal effect of releasing audits on trust in the CGU, conditioning on corruption

Table 3.5: Average Effects of Release of Audits on Preference for Centralization

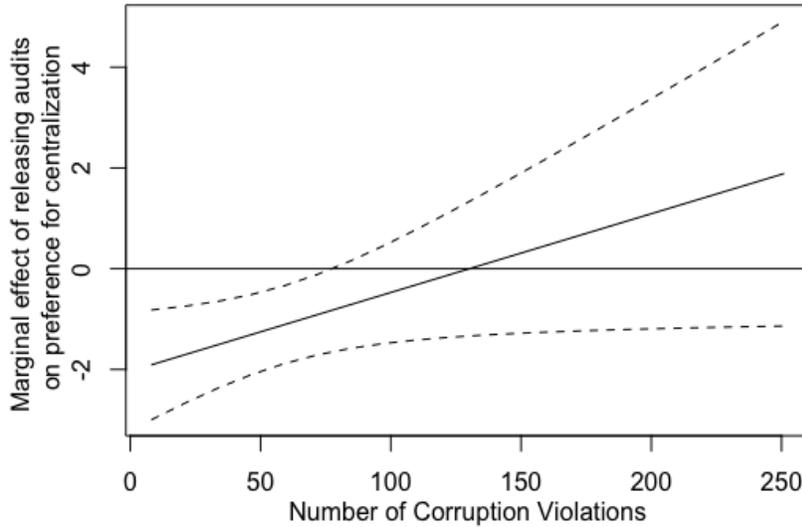
	Fiscal (1)	Political (2)	Fiscal and Political (3)	(4)
Number Corrupt x After Audit	0.02 (0.04)	0.01 (0.04)	0.02** (0.01)	0.03 (0.06)
Number Corrupt	-0.03 (0.06)	0.002 (0.06)	0.003 (0.003)	-0.03 (0.10)
After Audit	-0.46 (1.02)	-0.25 (1.05)	-2.04*** (0.60)	-0.73 (1.51)
Education	0.004 (0.03)	0.03 (0.03)	0.003 (0.05)	0.03 (0.05)
Age	0.004 (0.01)	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Male	-0.04 (0.17)	0.004 (0.18)	-0.10 (0.31)	-0.03 (0.29)
Amount Audited	0.0000 (0.0000)	0.00 (0.0000)	0.00 (0.00)	0.0000 (0.0000)
Population	-0.0000 (0.0001)	-0.0000 (0.0001)	0.0000 (0.0000)	-0.0000 (0.0001)
Constant	2.18 (2.12)	0.81 (2.15)	5.60*** (0.78)	3.02 (3.26)
State Fixed Effects	Yes	Yes	No	Yes
N	169	170	168	168
R ²	0.25	0.25	0.13	0.30

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and year and survey fixed effects.

All models except for Model 3 also include state fixed effects.

Figure 3.5: Marginal Effects of Exposed Corruption on Preference for Centralization



Without evidence in favor of voters' preference for centralization, I now move onto exposed corruption's effects on political efficacy. Table 3.6 presents the results for Hypotheses 5 and 6. I expect that both external and internal efficacy will increase after corruption is exposed. In columns 1 and 2, we see that both internal and external efficacy decrease following the release of audits, regardless of number of corruption violations reported. On the 5-point scale, audit publication is associated with a 1.62 and 2.58 point decrease in internal efficacy and external efficacy, respectively. This effect is strongly mitigated by the number of corruption violations reported. Each additional corruption violation is associated with a 0.023-point increase in internal efficacy, and a 0.033-point increase in external efficacy. Figure 3.6 presents the marginal effect of releasing audits on efficacy, conditioning on the level of corruption.

Curiously, audits have a negative effect on both external and internal efficacy for low levels of corruption. This finding is difficult to explain. For internal efficacy, it's possible that receiving information about low levels of corruption only causes confusion for voters. On one hand, voters may believe that any amount of corruption is normatively unacceptable and should be punished. On the other hand, voters may also believe that such a low level of corruption is not worth sacrificing a competent municipal government by throwing the incumbents out of office. As a result, this information may make voters feel less confident in their own

abilities to make intelligent political decisions.

At high levels of corruption, however, we see evidence in favor of Hypotheses 5 and 6. With 95% confidence intervals, releasing audits has a positive effect on internal efficacy at about 115 violations, and a positive effect on external efficacy at about 125 violations.¹⁰ This suggests that releasing audit reports with high levels of corruption causes citizens to believe that the government is responsive to citizens' interests, and that the citizen can participate effectively in politics.

Table 3.6: Average Effects of Audit Release on Political Efficacy Conditioning the Level of Corruption

	Internal Efficacy (1)	External Efficacy (2)
Number Corrupt x After Audit	0.023** (0.009)	0.033*** (0.011)
Number Corrupt	0.008* (0.004)	0.007 (0.005)
After Audit	-1.620** (0.674)	-2.580*** (0.762)
Education	0.066*** (0.021)	-0.040 (0.026)
Age	0.011** (0.005)	-0.003 (0.006)
Male	0.393*** (0.142)	-0.047 (0.161)
Amount Audited	0.000 (0.000)	0.000* (0.000)
Population	-0.00000 (0.00000)	-0.00000*** (0.00000)
Constant	1.709** (0.779)	2.401** (0.969)
N	480	483
R ²	0.240	0.184

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and state and year fixed effects.

Participation

The previous section provided some evidence that exposed municipal corruption in Brazil did not affect political attitudes as the literature would have expected. Following the revelation of high levels of corruption, trust in the local municipal government decreased while trust in the CGU, a national-level audit institution, increased. The strongest evidence, however, was for efficacy: revealing high levels of corruption also increased internal and external efficacy, suggesting that the information contained in audit reports caused citizens to feel like they understand politics and can have an impact on political outcomes.

¹⁰With 90% confidence intervals, that number changes to about 100 violations for internal efficacy and 105 violations for external efficacy.

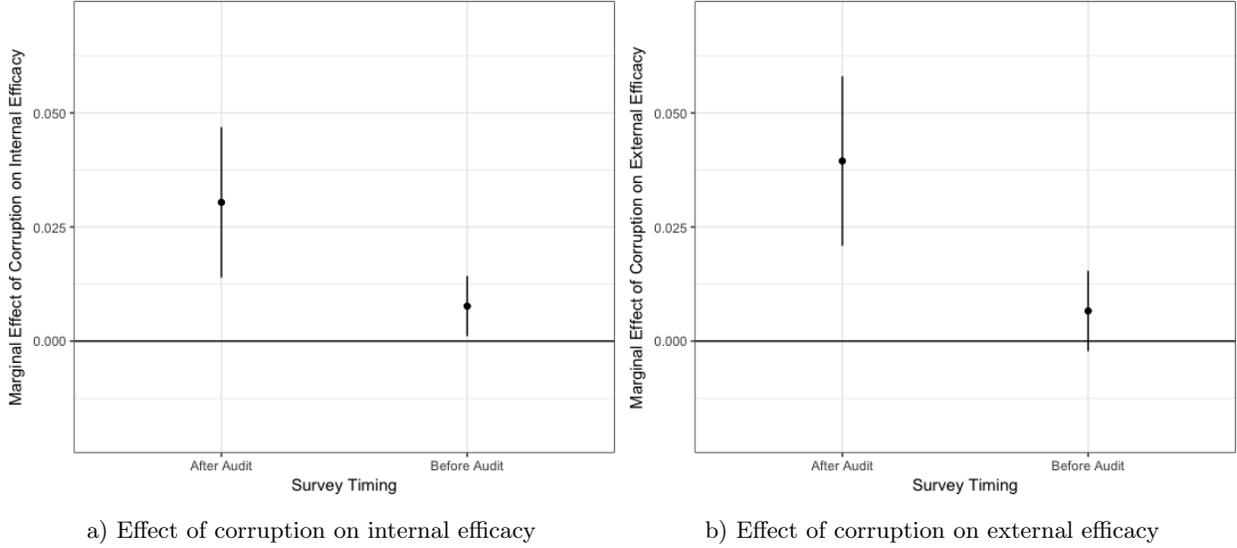


Figure 3.6: The marginal effect of corruption on efficacy

There was no indication, however, that trust in the CGU causes spillover effects for all national-level institutions. Revealing corruption had no significant effect on trust in the three branches of the federal government. Therefore, it is possible that the change in political attitudes that most strongly affected voters' propensity to turn out on election day were their higher levels of political efficacy. To a lesser extent, it suggests that lower levels of trust in local government may also play a role in increasing political participation. This combination is intuitive: Higher levels of internal and external political efficacy may increase voters' belief that they will impact the political system if they participate. When trust in local institutions are low, voters believe that keeping local politicians in line requires vigilance. Combined, this leads an increased interest in political participation.

Although we cannot directly test the effects of attitudes on political behavior, I run several models to check the plausibility of my arguments that high efficacy and low trust in local government should increase one's propensity to vote. I estimate the following logistic regression:

$$V_{msy} = \alpha + \beta_1 A_{msy} + \beta_2 X_{msy} + \beta_3 U_{msy} + v_s + w_y + \epsilon_{msy} \quad (3.2)$$

Where V_{msy} is an indicator variable of whether respondent i plans to vote in the following election. A_{msy} is individual i 's political attitudes including, for different models: internal efficacy, external efficacy, trust in

the municipal government, and trust in the CGU. X_{msy} is a matrix of individual-level control variables, and U_{msy} is a matrix of municipal-level control variables. Finally, v_s are state fixed effects, w_y are year fixed effects and ϵ_{msy} is the error term.

Table 3.7: Political Attitudes and the Propensity to Vote

	Dependent Variable: Plans to Vote in Next Election			
	(1)	(2)	(3)	(4)
Internal Efficacy	0.14* (0.08)			
External Efficacy		0.12* (0.07)		
Trust in Munic			-0.07 (0.06)	
Trust in CGU				-0.04 (0.16)
Education	0.10** (0.04)	0.12*** (0.04)	0.08** (0.03)	0.10 (0.08)
Age	0.02 (0.01)	0.02** (0.01)	0.02** (0.01)	0.02 (0.02)
Male	0.19*** (0.24)	0.24 (0.23)	0.20 (0.20)	-0.11 (0.48)
Amount Audited	0.00 (0.00)	0.00** (0.00)	0.00 (0.00)	0.00 (0.00)
Population	-0.39 (0.18)	-0.33* (0.18)	-0.22 (0.14)	-1.01 (0.72)
N	435	440	595	120
Log Likelihood	-230.85	-233.28	-318.58	-64.74
AIC	509.70	516.55	691.16	165.48

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, state and year fixed effects.

Table 3.7 displays the results for four models. Models 1 and 2 show that, as expected, internal and external efficacy have a positive effect on respondents' propensity to vote. Model 3's coefficient is the expected sign, indicating that respondents with lower levels of trust in the municipal government are more likely to vote. However, it is not significant. Finally, in Model 4, trust in the CGU is neither positively nor significantly associated with the propensity to vote.

These results suggest that, of the political attitudes that were most effected by exposed corruption, internal and external efficacy are the strongest determinants of the propensity to vote.

3.7 Conclusion

This paper contributes to the literature evaluating the effects of corruption information on political attitudes and political participation. It explored the reasons for why my second chapter revealed that high levels of exposed corruption unexpectedly increased turnout in Brazilian municipalities. These findings challenged

the literature's by showing that, although highly corrupt municipalities have lower levels of turnout than clean and unaudited municipalities, providing concrete evidence of this corruption increased political participation significantly relative to other municipalities. Theoretical and empirical scholarship demonstrates that turnout suffers after the exposure of corruption through the exposed corruption's effects on political efficacy and trust in political institutions (Mishler and Rose, 2001; Anderson and Tverdova, 2003; Olsson, 2014; ?). Working off this strain of the literature, I explored whether exposed corruption in Brazil had an unexpected effect on trust in institutions or political efficacy, which could have led to an increase in political participation. I argued that the identity of the audit institution that revealed corruption, the *Controladoria Geral da União* (CGU) is the likely reason why trust in institutions and political efficacy likely increased following the exposure of corruption, thereby causing turnout to also increase.

Overall, I found some evidence that exposing corruption causes trust in local institutions to decrease, and I found more substantial evidence that exposing corruption causes internal and external political efficacy increase. Trust in the CGU also increases following the exposure of corruption, but this effect does not extend to other national-level institutions. Furthermore, I found evidence that citizens with high political efficacy are more likely to say they will vote in the following elections. The findings suggest that voters who believe that they can have a positive impact on political outcomes, but who also feel as though they need to closely monitor their local government are more likely to participate when audits reveal corruption. The results also suggest that, of the political attitudes that were most affected by exposed corruption, internal and external efficacy are the strongest determinants of the propensity to vote.

The results of this paper suggest that the source of information about corruption may complicate the expected relationship between exposed corruption, political attitudes, and political participation. If the source of information is a federal governmental institution, exposing corruption can have positive effects on both citizens' perceptions of the institution that revealed corruption, and on citizens' sense of external political efficacy. Because citizens believe that the information is credible, corruption information is more likely to decrease citizens' trust in local institutions and increase their internal efficacy.

The results of this paper also raise one particularly important issue: how do we reconcile the results of Chapter 1 with the results of Chapters 2 and 3? That is, the second and third chapters of this dissertation provide evidence that exposing corruption in Brazil increases voters' propensity to vote in elections. The first

chapter, however, provides evidence that this increase in aggregate levels of participation does not translate into punishment for corrupt politicians. If information about corruption is leading to noticeable change in electoral behavior, but little or no change in electoral outcomes, where is the accountability mechanism breaking down? De Vries and Solaz (2017) suggests, based on a general model of retrospective voting by Healy and Malhotra (2013), that retrospective voting based on corruption information takes place in three stages: information acquisition, blame attribution, and behavioral response.

First, information acquisition involves receiving information about corruption. Although we cannot directly demonstrate that all voters receive the information contained in the CGU's audit reports, we have a fair amount of anecdotal evidence that suggests they do.¹¹ Additionally, the fact that we witness a relative increase in turnout only among voters who live in municipalities that were revealed to be highly corrupt suggests that they did receive *some* sort of information that other citizens did not receive. It remains unclear, however, whether voters cleared the second hurdle to electoral accountability: blame attribution. Blame attribution involves assigning responsibility for corruption to public officials and updating evaluations of officials' performance accordingly. In the Brazilian context, local municipal governments have a significant amount of autonomy over their own budget and public spending (Melo and Rezende, 2004). The mayor and local legislators have a great deal of power over the municipal budget, meaning that voters should be more easily able to attribute blame to the mayor and local legislators when money goes missing. It is possible that, because local government consists of multiple actors, voters may have divided blame among the mayor and other legislators, or they may not have attributed blame to the mayor at all.

This leads into the last possible stage: behavioral response. As De Vries and Solaz (2017) note, voters have three possible behavioral options:¹² switching to another candidate, voting for the same candidate, or abstaining from voting altogether. We can eliminate the third option because there is ample evidence that Brazilian voters did not respond to information about corruption by abstaining. A second option is that voters *did* turn their blame attribution into electoral punishment, but they punished someone other than the mayor. In other words, if voters believed that other members of the *Câmara Municipal* (*vereadores*) were responsible for the corruption uncovered in the CGU's municipal reports, they may have switched their vote to other vereador candidates, thereby punishing the incumbent vereador. Another possibility is that

¹¹(See examples in Ferraz and Finan (2008), as well as Oliveira, Edilson (2016); ClickPB (2008); Melo Aranha, Ana Luiza (2016))

¹²Like De Vries and Solaz (2017), I only focus on electoral options.

voters successfully attributed blame to their mayor, but did not believe that they had any viable non-corrupt alternatives. On one hand, this is possible due to high perceptions of corruption in Brazil (Fleischer, 1996). Klačnja and Tucker (2013), for example, show that voters in a high corruption country (Moldova) responded less strongly to information about corruption than voters in a low corruption country (Sweden). This seems like an unlikely explanation in Brazil's case, however, because it cannot explain why so many more Brazilians would have turned out after the exposure of corruption if they did not plan to vote for someone other than the incumbent. Future work should try to explain these seemingly irreconcilable findings, and understand why higher turnout does not translate into punishment for corrupt politicians.

Appendix A

Supplemental Information for Chapter 1

A.1 Testing Alternative Theories

Ferraz and Finan acknowledge that the credibility of their results is dependent on the integrity of audit process. They consider the possibility that the audit process itself was manipulated. If the audits conducted before the election were systematically different from audits conducted after the election, this would undermine the conclusions drawn from the analysis. Incumbents in municipalities audited before the election, for example, may be motivated to bribe the auditors to report fewer violations. Ferraz and Finan argue that this is unlikely considering that they do not find a significant difference in the average number of corruption violations in reports published before the election and reports published after the election. They more rigorously test their assumption by regressing the number of corruption violations on several independent variables, including 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 interactions. They do not find any evidence of preferential treatment for mayors in the same party as the governor or federal government, and do not find a significant effect of the audit timing on the number of reported corruption violations.

However, in the replication data in Table 3, I did find a significant difference between the number of corruption violations reported in municipalities with reports published before and after the elections. In fact, I found that municipalities with audits published after the election had a significantly higher average number of corruption violations than municipalities with audits published before the election. This is consistent with the argument that mayors in municipalities audited before the election manipulated the audit process to increase their probability of reelection. In order to better evaluate the possibility of audit manipulation, I also replicate Ferraz and Finan's analysis in Table A.1.

Table A.1: Alternative Tests

	(1)	(2)	(3)	(4)	(5)	(6)
Preelection Audit	-0.080 (0.281)	0.065 (0.302)	-0.027 (0.174)	-0.149 (0.284)	0.068 (0.102)	-0.108 (0.206)
Preelection Audit x Corruption Violations			-0.030 (0.039)	0.124 (0.182)	-0.012 (0.037)	0.225 (0.159)
Preelection Audit x Governor's Party	-0.365 (0.372)	-0.373 (0.373)	0.013 (0.165)	-0.014 (0.171)	-0.028 (0.123)	-0.040 (0.126)
Preelection Audit x Win Margin		-0.865 (0.876)	-0.251 (0.359)	-0.233 (0.383)	-0.104 (0.308)	-0.114 (0.319)
Preelection Audit X PT	-0.108 (0.903)	-0.136 (0.907)	0.227 (0.346)	0.236 (0.345)		
Preelection Audit X PMDB	0.202 (0.437)	0.159 (0.438)	0.333 (0.205)	0.331 (0.204)		
Preelection Audit X PFL	-0.235 (0.452)	-0.257 (0.451)	-0.044 (0.190)	0.00004 (0.194)		
Preelection Audit X PSDB	-0.825* (0.499)	-0.842* (0.499)	-0.197 (0.212)	-0.168 (0.219)		
Preelection Audit X PSB	-0.467 (0.978)	-0.466 (0.948)	0.057 (0.394)	0.055 (0.372)	-0.214 (0.298)	
Preelection Audit X PTB	-0.684 (0.637)	-0.712 (0.639)	0.417* (0.229)	0.505** (0.233)		
Preelection Audit x corruption=0				0.156 (0.262)		0.201 (0.218)
Preelection Audit x corruption=2				-0.324 (0.266)		-0.454** (0.210)
Preelection Audit x corruption=3				-0.585 (0.451)		-0.628 (0.389)
Preelection Audit x corruption=4+				-0.485 (0.696)		-0.818 (0.590)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
N	373	373	264	264	373	373
R ²	0.359	0.361	0.339	0.352	0.285	0.300

*p < .1; **p < .05; ***p < .01

Notes: Like Ferraz and Finan, each column presents the results of an OLS regression on the dependent variable listed above. All models include the following municipal control variables: population density, literacy rate, percent of the population living in a rural area, the log of average per capita income, GINI coefficient, effective number of political parties in the 2000 mayor elections, municipal police (1/0), small claims court (1/0), judiciary district (1/0); Mayoral control variables: sex (1/0), age, marital status, education level, party indicators.

Column 1 in Table A.1 displays the results using the reproduction dataset for a regression of the number of corruption violations on audit timing, whether the incumbent was a member of the governor's party, and party dummies. Like Ferraz and Finan's results, I find no evidence that mayors of the governor's political party received preferential treatment in the audit process. Unlike Ferraz and Finan, I find that incumbents who were members of the party *Partido da Social Democracia Brasileira* (PSDB) received fewer corruption violations when their reports were published before the 2004 election.

I find the same results for members of PSDB in column 2, which regresses the number of corruption violations on audit timing, the incumbent's win margin in the 2000 municipal elections, and party dummies. I do not find a significant effect of the mayor's win margin on the number of corruption violations, which suggests that mayors who were more vulnerable in the 2004 elections did not try to manipulate the audit process.

Columns 3-6 test the robustness of Ferraz and Finan's results. Columns 3 and 4 run the same tests as Table 1.6, with the additional interaction terms that were in Columns 1 and 2. In Ferraz and Finan's analysis, they find almost identical results to their first analyses: In the model with the sample restricted to municipalities with fewer than 6 reported corruption violations, the interaction between the number of corruption violations and the timing of the audit publication before the election is negative and significant. In Column 3 in Table A.1 which uses the reproduction dataset, I do not find the same negative and significant effect.

Appendix B

Supplemental Information for Chapter 2

B.1 Assumptions and Robustness Checks

Difference-in-differences models rely on several assumptions that I will outline here. First, the general assumptions of OLS apply to DD designs with multiple cross-sections and multiple time-periods. I include municipal-level fixed effects to control for unobserved heterogeneity across municipalities. With municipal fixed effects, the model estimates the association between revealing corruption (or lack of corruption) and within-municipality changes in political participation. In other analyses of TSCS analyses, using individual-level fixed effects does not rule out the possibility that an unobservable event caused both the independent and dependent variables to change simultaneously. In my analysis, however, even though corruption is not random, because audits are exogenous and random, it is very unlikely that an unobservable event coincided with the *revelation* of corruption. It is, however, possible that an unobserved event occurred after the audit took place, which affects political participation.

Similarly, reverse causality is also unlikely. Although political participation and levels of corruption are likely reciprocally related, the revelation of corruption is exogenous. Because I account for heterogeneity among municipalities, I only have to be concerned with political participation causing the revelation of corruption, which is not possible.

The most important DD-specific assumption is the “parallel trends” assumption. That is, DD assumes that absent treatment, the outcomes of the treated and control units would follow parallel paths. One way that scholars test this assumption is by comparing the the average outcomes of the treated and control units in the pre-treatment period. To do this, I run a difference-in-differences regression (with municipal fixed effects and clustered standard errors) using only elections before audits occurred, and I interact the time and treatment variables. If, as we assume in our DD model, no differential trending is occurring, the interaction

between the time and treatment variables will not be significant (Malesky 2014). Table B.1 reports the results.

Table B.1: Effect of Revealing Corruption on Electoral Outcomes, Parallel Trends Test

	<i>Dependent variable:</i>	
	Turnout, Mayoral	Blank Votes, Mayoral
	(1)	(2)
Year	0.012*** (0.0004)	18.424* (10.017)
GDP per capita	-0.0005* (0.0002)	-3.120 (5.010)
log(population)	-0.035** (0.014)	368.655*** (95.757)
Clean*Year	0.0002 (0.002)	-18.192 (13.074)
High Corruption*Year	0.002 (0.004)	-35.682 (27.616)
Low Corruption*Year	0.001 (0.002)	-31.623** (13.894)
Observations	7,550	7,550
R ²	0.361	0.005
Adjusted R ²	-0.365	-1.124
F Statistic (df = 6; 3536)	332.398***	2.944***

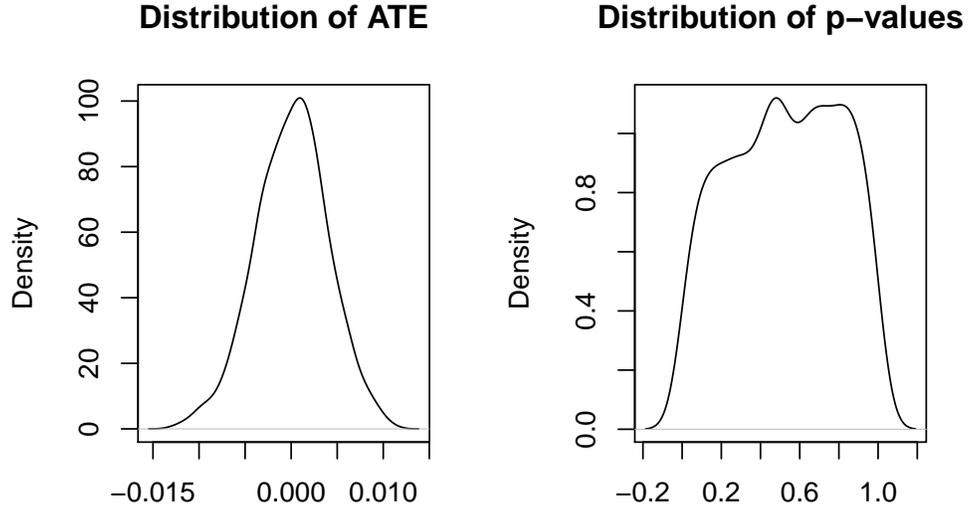
Note: *p<0.1; **p<0.05; ***p<0.01

For the turnout model, the interactions between the treatment and year are not significant, suggesting that the parallel trend assumption is upheld in the model. In the second model testing the effect of audit release on blank votes, however, the interaction between low corruption treatment with year are significant at the $p < 0.05$ levels. This indicates that we should not give the model testing audit release on blank votes as much weight as the the model testing audit release on turnout.

Another possible concern is that the effects of exposing corruption on turnout are random, and that assigning a placebo treatment effect would provide the same results. To test this argument, I run 1000 Monte Carlo simulations in which I shuffle the treatment variable in order to randomly assign treatment to municipalities, and rerun the DD models as specified above. Only 18 out of 1000, or 0.18% of the simulations found a significant and positive effect ($p < 0.05$) of revealing high levels of corruption on turnout, and 23 simulations (0.23%) found a negative and significant effect of revealing high levels of corruption on turnout.¹ These results suggest that the effect of revealing corruption on turnout is not random chance.

¹38 out of 1000, or 0.38% were positive and significant at $p < 0.1$, and 41 or 0.41% were negative and significant at $p < 0.1$.

Figure B.1: Results of Monte Carlo Simulations



B.2 Analysis with presidential elections

Table B.2: The effect of audits on political participation in Presidential Elections, conditional on the level of corruption. 1996-2008

	<i>Dependent variable:</i>			
	Turnout (1)	Blank Votes (2)	Null Votes (3)	All Protest 'Votes' (4)
High Corruption*Treatment	0.029*** (0.007)	-0.015 (0.009)	-0.004 (0.006)	-0.033*** (0.008)
Low Corruption*Treatment	0.003 (0.004)	0.005 (0.004)	0.0003 (0.002)	0.002 (0.005)
Clean*Treatment	0.006 (0.006)	0.013* (0.006)	-0.008* (0.004)	-0.002 (0.007)
GDP per capita	0.002*** (0.0004)	-0.004*** (0.001)	0.002*** (0.0003)	-0.003*** (0.001)
log(population)	-0.104*** (0.011)	0.125*** (0.014)	-0.053*** (0.006)	0.129*** (0.012)
Observations	11,656	11,656	11,656	11,656
R ²	0.326	0.769	0.232	0.767
Adjusted R ²	-0.033	0.646	-0.178	0.642

Note:

*p<0.05; **p<0.01; ***p<0.001

Appendix C

Supplemental Information for Chapter 3

C.1 Questions and Survey Waves

Table C.1: Survey Waves and Available Survey Questions

	LAPOP Wave II	LAPOP Wave III	LAPOP Wave IV	LAPOP Wave V	LAPOP Wave VI	LB 2008	LB 2009	LB 2010	LB 2011
Trust CGU	X	X							
Trust President	X	X	X	X	X				
Trust Congress	X	X	X	X	X	X	X	X	X
Trust Munic Gov	X	X	X	X	X	X	X	X	X
Trust Judiciary	X	X	X	X	X	X	X	X	X
Trust Police	X	X	X	X	X	X	X	X	X
Fiscal Centralization		X							
Political Centralization		X							
Trust State Gov	X	X	X						
Internal Efficacy		X	X	X	X				
External Efficacy		X	X	X	X				
Political Bribe	X	X	X	X	X				
Bureaucratic Bribe	X	X	X	X	X				
Would Vote		X	X	X	X	X	X	X	X

C.2 Placebo Tests

I found strong evidence that information about corruption revealed by a governmental institution causes trust in local institutions to decrease, and causes political efficacy to increase. One of the underlying reasons for why exposing corruption in Brazilian municipalities may have had an unexpected effect on political efficacy is due to the CGU's status as a non-partisan national-level institution. If my results are due to the information being revealed by a governmental institution, we should expect that corruption revealed through other means will not have the same effects on trust and efficacy. In order to test this implication, I use the same sample of respondents and a LAPOP question in which survey respondents were asked whether they were asked for a bribe from a politician or bureaucrat within the last 12 months. Although personal experience with corruption is not the same as learning about large-scale corruption, it is an example of receiving information about corrupt local government officials. I run OLS regressions with clustered standard errors, and include the same individual-level and municipal-level control variables as earlier models. I also include the number of corruption violations uncovered by municipal audits. This controls for the municipality's baseline level of corruption, which better isolates the effect of learning new information about corruption rather than the effect of living in a corrupt municipality.

Table C.2 reports the results for respondents who were asked for a bribe by a politician, and Table C.3 reports the results for respondents who were asked for a bribe by a bureaucrat. None of the significant coefficients on being asked for a bribe reflect the same substantive results as my earlier tests of revealing

Table C.2: Relationship between Being Asked for Bribe by a Politician, and Political Attitudes

	Trust Munic Gov (1)	Trust CGU (2)	Internal Efficacy (3)	External Efficacy (4)
Bribed Politician	-0.12 (0.35)	-1.32** (0.59)	-0.41 (0.32)	-0.30 (0.37)
Number Corrupt in Audit	0.01 (0.004)	-0.001 (0.01)	0.01** (0.004)	0.01 (0.005)
Education	0.002 (0.03)	0.08** (0.04)	0.07*** (0.02)	-0.04 (0.03)
Age	0.01** (0.01)	0.02** (0.01)	0.01** (0.01)	-0.004 (0.01)
Male	0.16 (0.17)	0.52** (0.24)	0.49*** (0.15)	-0.09 (0.17)
Population	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Constant	37.29 (111.03)	-470.65 (303.78)	289.06** (118.42)	-109.06 (140.20)
N	546	199	445	450
R ²	0.18	0.17	0.23	0.18

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and state and year fixed effects.

Table C.3: Relationship between Being Asked for Bribe by a Bureaucrat, and Political Attitudes

	Trust Munic Gov (1)	Trust CGU (2)	Internal Efficacy (3)	External Efficacy (4)
Bribed Bureaucrat	-0.07 (0.52)	-1.00 (0.96)	0.17 (0.60)	-1.14** (0.54)
Number Corrupt in Audit	0.01 (0.004)	-0.001 (0.01)	0.01* (0.004)	0.01 (0.005)
Education	0.001 (0.03)	0.06* (0.04)	0.07*** (0.02)	-0.03 (0.03)
Age	0.01** (0.01)	0.02** (0.01)	0.01** (0.01)	-0.004 (0.01)
Male	0.13 (0.17)	0.44* (0.24)	0.46*** (0.15)	-0.07 (0.16)
Population	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000* (0.0000)	-0.0000 (0.0000)
Constant	49.80 (111.78)	-597.31* (308.98)	299.99** (119.26)	-73.00 (140.58)
N	556	192	457	461
R ²	0.16	0.15	0.21	0.18

*p < .1; **p < .05; ***p < .01

All models use clustered standard errors, and state and year fixed effects.

corruption from municipal audits. Most interestingly, being asked for a bribe by a politician is significantly negatively associated with trust in the CGU. Similar to the conclusions that I drew from Table 3.4, this suggests that when citizens are aware of local corruption that has not been uncovered or punished by federal institutions, they become frustrated with the institutions that are supposed to eradicate corruption. I also find a negative and significant effect of being bribed by a bureaucrat on external efficacy, which the opposite effect of revealing municipal corruption in audit reports.

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