Corruption, Intimidation, and Whistleblowing: An Empirical Approach

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August 5, 2023

Abstract

I exploit an anti-corruption program consisting of random audits to investigate the effects of municipal level audits on reported wrongdoing. I show that reports about wrongdoing spike immediately after the announcement of an audit. The probability of filing at least one complaint peaks in the month of the announcement and decays in the months that follow. The effect is stronger in areas with less social capital and those where higher corruption prevails. I argue that audits improve whistleblower protection against retaliation and increase the likelihood of being heard. These findings have important policy implications and contribute to the growing literature on whistleblowers' motivations and the effects of audits.

Keywords: Whistleblowing, audit, corruption, plausible deniability, policy design.

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1 Introduction

In this paper, I show that the announcement of audit inspections incentivizes reports of unlawful acts. I exploit an anti-corruption program in Brazilian municipalities which consists of frequent random audits. The program is implemented by the Controladoria Geral da União (CGU) – an independent body – utilizing the national lottery machinery.[1] If a municipality is selected for audit, a team of auditors is sent for inspection within 2-3 weeks after the announcement. Once the investigation is concluded, the auditors draft a detailed report that is sent to the CGU headquarters. Reports from the same inspection round are posted online simultaneously on the CGU official website. Between 2003 and 2011 the CGU carried out more than 40 rounds of inspection with 50-60 municipalities audited in each round.

I use a unique dataset with detailed information about reports of unlawful acts spanning the period 2000-2010. The dataset comprises 45,346 complaints out of which 9,443 are filed anonymously. I complement this dataset with rich information about the audit program and investigation outcomes made publicly available by the CGU. Using the exact timing of the announcement of the random audit and a flexible event study framework, I investigate the impact of the announcement on the probability that at least one complaint is filed from the municipality that is selected for audit. This setting allows me to circumvent two main identification challenges. i) Reverse causality, complaints incentivize audits and vice-versa. ii) Omitted variable bias, and high levels of corruption trigger simultaneously audit and complaints.

I find that the announcement of an audit increases the probability that at least one report of unlawful practice is filed, anonymously or not. The effect peaks the month after the audit announcement and decays the third month after the announcement. The jump is 1.5 times higher than the pre-announcement probability and indicates that individuals are taking full advantage to report wrongdoing. A similar pattern is shown by anonymous complaints. The probability of filing a complaint in the month of the announcement is 3 times higher than the pre-audit mean. (The point estimate is 0.021 and the pre-audit mean is 0.007.) The probability of filing at least one complaint persists in the 12 months that follow the announcement indicating that positive spillovers emerge.

Finally, I also explore heterogeneous effects. Without an audit announcement, citizens in more corrupt municipalities are less likely to file complaints, anonymously or not. However,

[1] The program continues to date but some features of the program have changed.

corrupt areas are more likely to report wrongdoing if an audit is announced. Similarly areas with low social capital such as areas where literacy rates, household income, and the share of female population is below the Brazilian average are more likely to report wrongdoing if an audit is announced.

The findings are consistent with two interpretations. The first, similar in spirit with Chassang and Pardo i Miguel (2014) suggests that audits may be a mechanism for introducing noise in the reporting system. Audits provide plausible deniability to whistleblowers; the malfeasant cannot verify if a complaint is filed or the auditors uncovered the wrongdoing, independently, during the auditing process. As the risk of retaliation goes down, the probability that the whistleblower files a complaint goes up. The second implies that audits increase the probability that the complaint is going to be acted upon; the perceived quality of government goes up. Both mechanisms predict that complaints should intensify around the date of the audit announcement.

I also rule out alternative interpretations. The first interpretation is that malfeasant individuals anticipating the audit and exposure of their own wrongdoing will attempt to negotiate their expected penalty or fine. To rule out this interpretation, I rely on the complaints filed anonymously. If fine negotiation is the reason behind the spike-decay patterns, we should not observe changes in anonymous complaints. After all, anonymous complainants cannot be used to carry out negotiations. I show that anonymous complaints also follow the spike-decay pattern indicating that this interpretation is not consistent with the data.

Second, audits provide more information about municipal corruption which might lead individuals to file complaints about wrongdoings. In this case, the spike-decay pattern exhibited by complaints should be centered around the date of the publication of the audit report rather than the audit announcement. Using the publication date as the event date, I find that the spike and decay pattern is not present after the publication of the report. Hence, this alternative interpretation is not consistent with the findings in this paper.

This paper contributes to the literature that attempts to understand whistleblowers' motives and incentives to report wrongdoing. Dyck, Morse and Zingales (2010) document determinants of whistleblowing in corporate finance, analyzing over 200 securities class action lawsuits filed against large US corporations. They show that fraud detection does not rely on standard actors such as investors, and auditors but includes several nontraditional players such as employees, media, and industry regulators. This paper, instead, shows that random audits and whistleblowing are complements. Increased perceived action (and protection)

incentivizes actual reporting of wrongdoing.

This paper also contributes to the literature on the effectiveness of policies designed to reduce corruption.[2] Moreover it relates to the literature that considers audits to be an effective anti-corruption tool in developing countries (Di Tella and Schargrodsky, 2003; Olken, 2007; Ferraz and Finan, 2008; Pomeranz, 2015). Differently from these studies, I show that audits have unintended – positive – consequences on other anti-corruption tools.

The rest of this paper is structured as follows. Section 2 provides the institutional background. Section 3 describes the data and summary statistics. Section 4 outlines the empirical analysis, whereas section 5 discusses the findings. Section 6 provides additional evidence and concludes the paper.

2 Institutional Framework

Complaints, Whistleblowing, and Leniency

In Brazil, newspapers report frequently stories of whistleblower retaliation. A well-known case is that of Marcia Reis, who was jailed for reporting corruption. A more extreme case is the decapitated journalist in Minas Gerais, who exposed a case of corruption and other illegal activities, such as child prostitution and drug trafficking.[3]

The country has no specific whistleblower protection laws. In 2013, the government passed an anti-corruption law with some provisions to i) protect and encourage whistleblowers, and ii) determine penalties for those that engage in corruption. According to Transparency International the law has not been effective.

In general, Brazilians are reluctant to report wrongdoing. Reporting is difficult because citizens are uninformed of existing channels. Also, financial constraints play a crucial role: resources required to process a case are high. Moreover, there is a long time required for a judicial process. Very often malfeasants are left unpunished.[4] Lastly, during the judicial processes, whistleblowers do not enjoy adequate protection from the authorities. All these factors, individually or combined, discourage reporting.

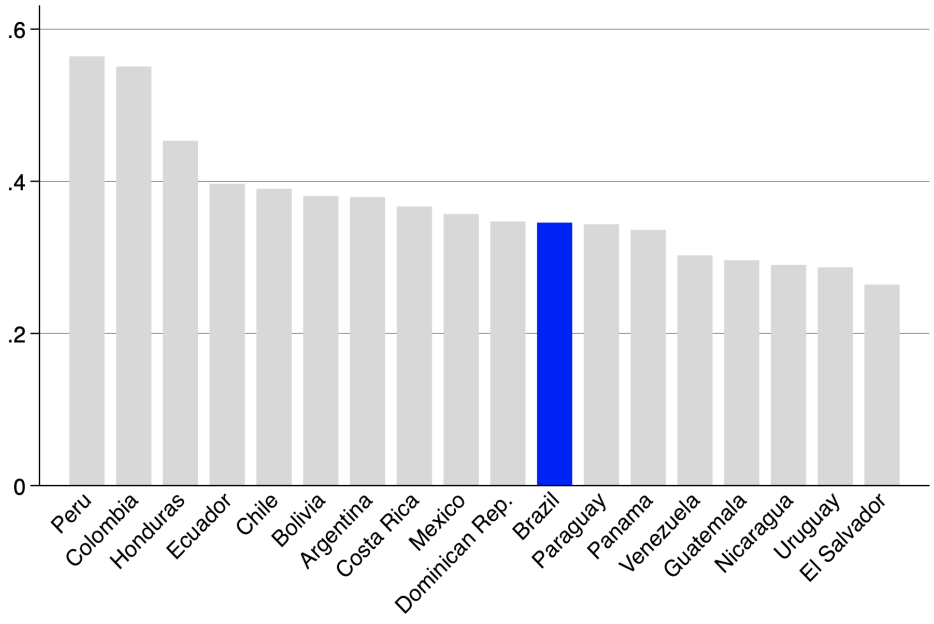
[2] See Olken and Pande (2012) and Banerjee, Mullainathan and Hanna (2012) for a literature review

[3] `https://www.theguardian.com/world/2015/may/20/brazil-blogger-known-for-reporting-on-corruption-found-decapitated`

[4] Marcia Reis, for example, raised awareness about the length of the time between when the accusations are made and the beginning of investigations.

Brazilians are reluctant to file complaints. This reluctance is also captured by *Latinobarómetro*, a yearly public opinion survey that conducts roughly 20,000 interviews in 18 Latino-American countries. In 2004, *Latinobarómetro* introduced a question to measure the likelihood of filing a complaint with respect to the poor quality of a good. As shown in Figure 1 Brazil is ranked eleventh out of eighteen Latin American countries in the survey. This indicates that when compared to other Latin American countries, Brazilians are less likely to report that they file complaints about the scarce quality of a product or service.[5]

Figure 1: Fraction of individuals that agree



Notes: The fraction of citizens that respond yes to the question: "There are different ways to improve things in (country) or to help to prevent them from breaking down. Have you ever done any of the following? Complain about the poor quality of a product or service."

Local governments and the audit program

In 2003, the Brazilian federal government initiated an anti-corruption program in local governments. The program – which continues to date – is implemented by the Controladoria Geral da Uniaõ (CGU) and consists of random selection, for audit purposes, of Brazilian local governments. Before the lottery (sorteio) is held, the CGU publishes the list of eligible municipalities jointly with the lottery numbers attributed to each municipality. State

[5]Brazilian mean is below the sample average (of 0.37).

capitals and municipalities with populations above certain thresholds are excluded.[6] If the number attributed to a municipality is selected, a team of auditors is sent in the municipality to inspect the use of the transfers, following guidelines from CGU headquarters.

In the first rounds of the program, all federally transferred funds were audited. As the program proceeded, it was decided that only a subset of funds would be inspected. Areas under inspection are also randomly selected and announced shortly before the lottery takes place. However, auditors were given full discretion; once in the municipality, they can investigate all funds if necessary.[7] The audit program of the CGU designated specific periods in which the regional units, and auditors located in each region/state, were expected to carry out inspections. Hence, the inspection period was almost pre-determined.

Over time, the CGU granted more flexibility to regional units. This implied that during the waiting times for a response from a municipal administration, progress was made in the evaluation of the information sent by another. The time needed for inspection, size, and composition of audit teams varied substantially depending on the scope of the inspections.

3 Data

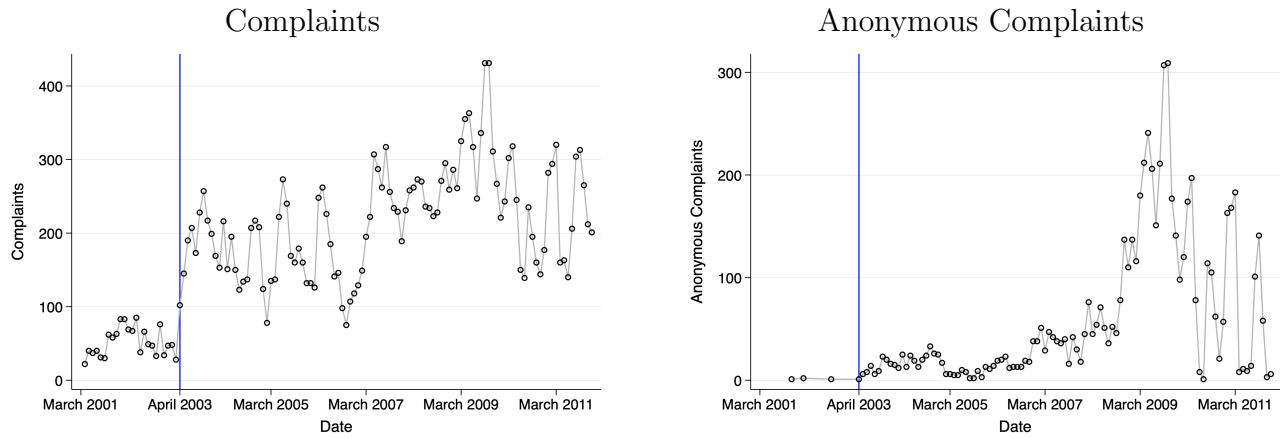
I use data on unlawful acts or denunciations (Denúncia) filed by public or private entities and sent to the CGU. The dataset covers the period from April 2001 to December 2011. The CGU records the municipality where the complaint originates as well as the municipality to which the complaint is directed. In some cases, the reason for the complaint is also included. Complaints for which I observe the reason are mostly related to government transfers such as Fundef/Fudeb, Bolsa Familia, and construction of public works.

In the left panel of Figure 2, I plot the number of municipalities from which at least one complaint is filed in a month. The right panel plots the number of municipalities filing at least one anonymous complaint. The vertical blue bar depicts the month of the audit pilot.

[6] More information about eligibility rules can be found in the Appendix.

[7] Education and Health are the areas that receive more audits, also areas which have the most cases of mismanagement. 25 percent of overall irregularities in the sample have occurred in Education related funds.

Figure 2: Monthly time-series of the number of complainants



Notes: This plot depicts the number of municipalities from which at least one complaint is filed in a specific month. The left panel depicts all complaints; the right panel, instead, shows anonymous complaint. The vertical blue line represents the announcement of the audit program (the pilot).

Pre-audit complaints are relatively rare events. Only 10 percent – less than 600 out of 5570 municipalities – filed complaints in the pre-audit period. Anonymous complaints are almost non-existent in the pre-audit period: only 5 municipalities filed anonymous complaints.

4 Empirical Strategy

I exploit the exact timing of the audit announcement and a flexible event study approach to estimate non-parametrically the effect of an audit on the probability that at least one complaint is filed from the municipality announced to be audited.

$$Y_{i,m,t} = \sum_{m=-12}^{-2} \beta_m Announced_{i,m,t} + \sum_{m=0}^{12} \beta_m Announced_{i,m,t} + \alpha_i + \lambda_{s,t} + \delta_{m,t} + \varepsilon_{i,m,t}, \quad (1)$$

where $Y_{i,m,t}$ is equal to one if at least a citizen complaint is filed from municipality i in month m of year t. $Announced_{i,m,t}$ are indicator variables tracking the weeks that immediately precede and follow the announcement in municipality i. The indicator variable $Announced_{i,0,t}$ equals one in the announcement month. The aggregation at the monthly level allows to track the behavior of complaints in a municipality shortly before and after the audit announcement. The coefficients on the event time indicator, β, estimate the probability that at least one complaint was filed from the municipality in a given week relative to the omitted category $Announced_{i,-1,t}$.

I also include municipality fixed effects, α_i, to control for unobserved (and unchanging) municipality characteristics; year-state fixed effects $\lambda_{s,t}$ to control nonparametrically for differential trends at the state level; month-year fixed effect $\delta_{m,t}$ to nonparametrically account for unobservables at the year-month level, they also control for potential seasonality in the complaint filing activity; $\varepsilon_{i,w,t}$ is the idiosyncratic error term. The standard errors are clustered at the municipality level which is also the treatment level.

5 Estimation Results

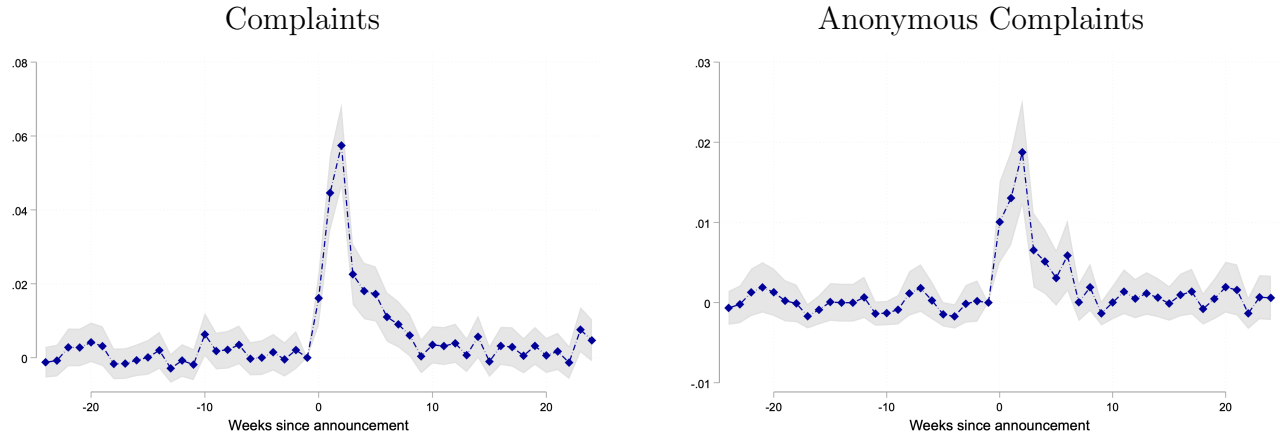
Estimated coefficients from Equation 1 are plotted in Figure 3. Month 0 corresponds to the week of the audit announcement. Month -1, ... , -12 and 1, ...,12 are the month before and after the audit announcement, respectively. The plotted event time coefficients can be interpreted as the percentage points change in the probability of filing a complaint from the audited municipality in the 12 months preceding and following the audit announcement. The shaded area represents the 95 percent confidence interval showing that each point estimate is statistically different from zero. The nonparametric study approach allows examining patterns in the outcome of interest before and after the announcement. The high frequency of the data at hand allows us to visually assess the change around the event date and credibly estimate the effect of the audit announcement. The randomness of the audit process ensures no pre-trends.

Figure 3 plots the event study coefficients estimates.[8] The plot reveals that after an audit announcement, the probability that at least one complaint is filed goes up. Ceteris paribus, being exposed to an audit increases the probability that at least a complaint is filed from the audited municipality. The coefficient on the audit month is 0.048, which implies that the probability of filing a complaint is 1.5 times higher than in the pre-audit period (the sample mean is .032). The coefficient estimate on the month immediately after the audit announcement is 2.5 higher than the mean pre-audit period.

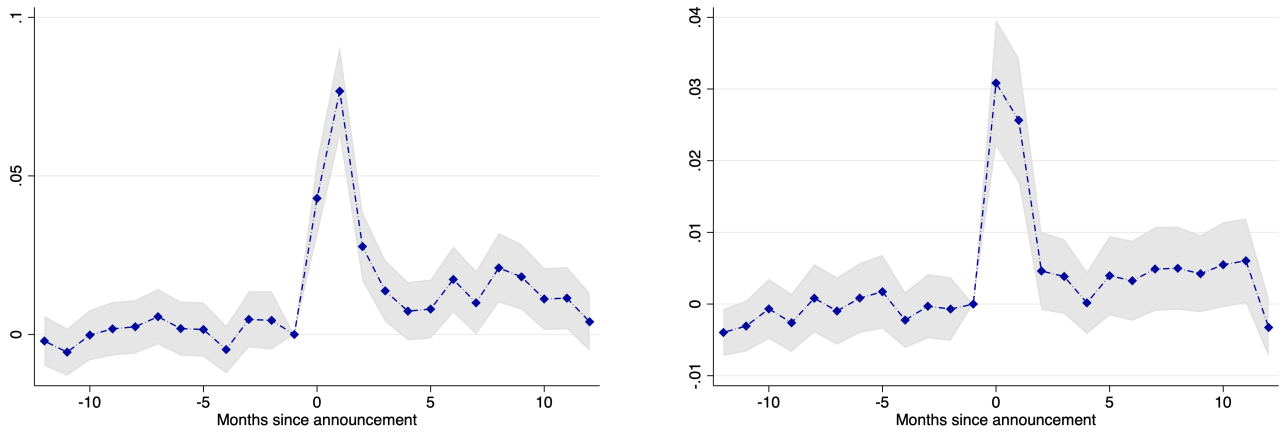
Moreover, the probability that at least one anonymous complaint is filed from the audited municipality also increases. The coefficient on the audit month is .021 and is statistically indistinguishable from the coefficient on the months that follows the announcement. This

[8]The main analysis is carried aggregating the data at the monthly level as the weekly level has low variation. Audits occur every 2-3 months.

Figure 3: Complaints before and after audit announcement



Notes: The figure plots event time coefficients and 95 percent confidence interval resulting from regressing of a dummy taking value one if at least a complaint is filed on a set of dummies from w-24 to w+24, where w=0 is the week of the audit announcement in the municipality. All coefficients can be interpreted as percentage point increase in the probability of filing a complaint relative to the week before the announcement. Municipal and year-state fixed effects are also included in the specification. Standard errors are clustered at the municipality level.



Notes: The figure plots event time coefficients and 95 percent confidence interval resulting from regressing of a dummy taking value one if at least a complaint is filed on a set of dummies from m-12 to m+12, where m=0 is the month of the audit announcement in the municipality. All coefficients can be interpreted as percentage point increase in the probability of filing a complaint relative to the month before the audit announcement. Municipal and year-state fixed effects are also included in the specification. Standard errors are clustered at the municipality level.

coefficient is 3.2 times higher than the mean probability of filing a complaint in the pre-audit period (the sample mean is .007). The effect is sizable.

Throughout the audit year, the probability that at least a complaint is filed is higher than the pre-announcement level. The estimated coefficients on lags 0 - 12 are larger than zero and jointly significant indicating that the effect persists throughout the announcement year.

Selection

The lack of anticipatory effect of the audit in the audited municipalities, due to audit randomness, allows to interpret the findings as causal. The coefficients on the lead dummies are very small in magnitude and statistically insignificant (see Figure 4 and Table A.1). Next, I test if areas that file pre-audit complaints are more likely to get a future audit. I investigate whether a complaint filed in the two years preceding the audit affects the probability of getting a future audit using the following specification:

$$Audit_i = \beta_0 + \beta_1 \text{Past C}_i + \eta_c + \varepsilon_i, \tag{2}$$

where $Audit_i$ is an indicator that takes value one if the municipality i receives any audit in the future. Note that the audit only occurs if announced.[9] Past C_i is a dummy variable that takes value one if the municipality placed at least a complaint in the two years preceding the audit program. η_c are state fixed effects, necessary as the lottery is stratified at the state level, and ε_i is the error term. I estimate variations of Equation 2, with and without the inclusion of state fixed effects and a vector of pre-audit level municipal characteristics, measured in the year 2000, as controls. Results are displayed in Table 1.

Column (1) shows estimation results that include only state-fixed effects. The coefficient on the complaint dummy is statistically insignificant across all specifications suggesting that there are no differences between municipalities that filed a past complaint and those that did not. In columns (2) and (3) where population and other controls are included in the estimation.

In columns (4) - (6) of Table 1, I use the number of complaints filed from a municipality as the main regressor. The variable counts the number of complaints filed from municipality i in the pre-audit period. All point estimates are statistically insignificant and close to zero suggesting that municipalities from which a past complaints was placed are equally likely to get future audits.[10]

[9] There has been no instance of an audit announcement without the audit taking place.

[10] There were almost no complaints in the pre-audit period.

Table 1: Are complainants more likely to get future audits?

	Future Audit in the municipality					
	(1)	(2)	(3)	(4)	(5)	(6)
𝟙(Complaint)	0.015	0.032	0.025			
	(0.021)	(0.023)	(0.024)			
Complaints				-0.009	-0.006	0.000
				(0.006)	(0.008)	(0.008)
Observations	5475	5475	5475	5475	5475	5475
State FE	✓	✓	✓	✓	✓	✓
Population		✓	✓		✓	✓
Controls			✓			✓

Notes: The dependent variable is a dummy variable that takes value one if the municipality is audited at any point in the future. All regressions include municipal population as control. $\mathbb{1}(Complaint)$ is a dummy variable if at least a complaint was filed from the municipality in the pre-audit period. *Complaints* is the number of complaints filed from a municipality in the pre-audit period. Standard errors are clustered at the municipality level, shown in parentheses.*** Significant at the 1 percent level. ** Significant at the 5 percent level.* Significant at the 10 percent level.

Silenced channels

The absence of complaints does not necessarily imply the absence of corruption. In areas where corruption prevails, citizens' voices might be silenced due to retaliation fear; these areas file fewer complaints in the absence of an audit. Theoretical predictions in Chassang and Pardo i Miguel (2014) suggest that the optimal policy should introduce noise in the reporting mechanism through: i) being less responsive and investigating only a fraction of the received complaints, and ii) triggering interventions in areas with no complaints. Noise in the system provides whistleblowers with plausible deniability and, in turn, should incentivize reporting. If audits indeed act as a device to provide plausible deniability, we should expect reports to increase after an audit announcement.

Municipal-level corruption is needed to test if more corrupt areas benefit more from an audit announcement in the municipality. However, corruption measures are mostly unavailable to policymakers and researchers; if available, they are measured with noise. Throughout the detailed audit reports, the audits program in Brazilian local governments allows constructing (pre-) existing corruption levels of the municipal government. Using a dictionary-based text classifier classification and a principal component analysis, I rank municipalities from least to most corrupt. The distribution of the corruption index is plotted in Figure A.5 in the

Appendix.[11]

For each audited municipality, I assume that the uncovered corruption in the municipality remains constant throughout the audit year. For example, if a municipality is audited in June, I assume that the level of exposed corruption in June was also prevailing from January to December of the same calendar year.[12] The assumption is not far-fetched for several reasons. First, corruption can be thought of as a manifestation of social norms that are slowly changing and can be considered constant over short time periods.[13] Second, most contracts are fixed within the same year.[14] Third, throughout the year, the municipal administration, and the mayor, are likely unchanged.

For this exercise, I restrict the sample to audited municipalities. The treatment group consists of municipalities audited in month m in year y. Municipalities are considered treated for each month of year y. The control group is the set of municipalities that are audited in year $y-1$ or are going to be audited in the future, say in year $y+1$.

$$Y_{i,t,m} = \beta_0 + \beta_1 Announced_{i,t,m} + \beta_2 \times C_{i,t} + \beta_3 Announced_{i,t,m} \times C_{i,t} + \alpha_i + \lambda_t + \delta_m + \varepsilon_{i,t,m} \quad (3)$$

Where $C_{i,t}$ is the underlying corruption in the municipality in the year t uncovered from an audit in the municipality during month m and varies at the yearly level. $Announced_{i,t,m}$ is an indicator variable taking value one within the month of the audit announcement and as well as three months post-audit announcement. Thus, the coefficients on $Announced_{i,t,m}$ should be interpreted as the average impact of the audit announcement within the first three months since the announcement. The main coefficient of interest is β_3 which estimates the causal impact of the audit announcement, conditional on the municipality's level of disclosed corruption. Results are shown in Table 2.

The coefficients on the *Announced* indicators remain positive and statistically significant across columns suggesting that an announcement positively affects the probability of filing a complaint. The interactions with the corruption index, in different forms, reveal interesting patterns; with no announcement, (citizens in) municipalities in the second and third tercile of

[11] See Muço (2017) for more detail on the construction of the corruption index.

[12] The same type of reasoning is valid for a municipality audited in February or November.

[13] For example, Fisman and Miguel (2007) find that parking violation corruption measure of diplomats is strongly and positively correlated with other (survey-based) country corruption measures at the country level. Barr and Serra (2010) find a correlation between individual behavior in the lab among students in the UK and their home country's level of perceived corruption. Roland (2004) considers "culture", including values, beliefs, and social norms, as slow-moving institution.

[14] This is particularly true with procurement-related contracts where corruption is more likely to emerge.

the corruption distribution are less likely to file a complaint when compared to municipalities in the first tercile. The coefficient on the interaction term remains positive and statistically significant across specifications, indicating that when an audit is announced, the probability that citizens file at least one complaint increases. The announcement effect is amplified when citizens reside in more corrupt municipalities.

Table A.2, in the Appendix, shows results where the corruption index is winsorized at the 4 and 94 percentile.[15] Results are also robust to assuming that corruption is constant within the electoral term rather than within the same calendar year.[16]

[15]Other levels of winsorizing are available upon request.

[16]Results are available upon request.

Table 2: Silenced Channels

	Complaint				Anonymous			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Announced	0.034***	0.034***	0.020***	0.021***	0.012***	0.011***	0.002	0.004
	(0.004)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)
Corruption	0.008	0.008			-0.009	-0.008		
	(0.007)	(0.007)			(0.006)	(0.006)		
Announced x Corruption	0.006**	0.006**			0.006***	0.005**		
	(0.003)	(0.003)			(0.002)	(0.002)		
Announced x 2Tercile			0.018**	0.019**			0.009**	0.007*
			(0.008)	(0.008)			(0.004)	(0.004)
Announced x 3Tercile			0.025***	0.024***			0.021***	0.017***
			(0.009)	(0.009)			(0.006)	(0.005)
2Tercile			-0.062**	-0.062**			-0.038**	-0.037**
			(0.030)	(0.030)			(0.015)	(0.015)
3Tercile			-0.046*	-0.046*			-0.030*	-0.029*
			(0.025)	(0.025)			(0.016)	(0.016)
Observations	22212	22212	22212	22212	22212	22212	22212	22212
Year FE								
Month FE	✓		✓		✓		✓	
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Month FE		✓		✓		✓		✓

Notes: The dependent variable is an indicator variable that takes value one in case the municipality receives a complaint and zero otherwise, shown in columns (1) - (4). In columns (5) - (8) the outcome variable is an indicator that takes value one the complaint was placed anonymously and zero otherwise. 2Tercile and 3Tercile are respectively the second and the third tercile of the corruption distribution. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Social Capital

The effects of an audit might be heterogeneous along different dimensions of the socioeconomic status. I interact different variables such as literacy rate, the fraction of income per capita, the fraction of female population with the main regressor of interest. The interacted specification is expressed in Equation 4:

$$Y_{i,t,m} = \beta_0 + \gamma_1 Announced_{i,t,m} + \gamma_2 (Announced_{i,t,m} \times x_i) + \alpha_i + \lambda_t + \delta_m + \varepsilon_{i,t,m} \quad (4)$$

x_i is an indicator variable that takes value one if, alternatively, literacy rate, the fraction of the urban population, income per capita and the fraction of the female population is below the Brazilian mean. x_i varies only at the municipality level and is measured pre-audit, namely in year 2000. The main effects x_i are omitted from the specification as they are collinear to the municipality fixed effect. $Announced_{i,t,m}$ is an indicator variable that takes value one in the first three months of the audit announcement. Thus, γ_1 and γ_2 can be interpreted as averages as they measure the effect three months from the announcement.

The coefficient of interest lies on the interaction term γ_2. Results are shown in Table 3. All columns in Table 3 show a positive effect of the audit announcement in the municipality. They indicate that areas with a lower social capital benefit more as they are more likely to file a complaint after an audit announcement. For robustness purposes, in the Appendix in Table 3, I create an indicator variable that takes value one for the first three post-announcement months and interact it with each socio-economic indicator.

Based on the estimates reported in Table 3, an audit in the municipality increases complaints by 5.8 percentage points in low literacy areas. In areas where individual wealth is below Brazil's average, an audit increases the probability of a complaint by 5.2 percentage points whereas in areas with a female population share higher than the Brazilian average, an audit increases the probability of a complaint by 5.3 percentage points. The above findings are important from a policy perspective because it is exactly for the poorer and less educated individuals that we should expect audits to trigger more response. Rich and educated households might be well informed about complaint channels. These findings are in line with those in Section 5; usually areas with lower social capital are those that exhibit higher corruption levels. Indeed, these variables are positively correlated with the corruption index.

Table 3: Heterogeneous Effects by Socio-Economic Characteristics

	Complaints			
	(1)	(2)	(3)	(4)
Announced	0.027***	0.038***	0.028***	0.027***
	(0.004)	(0.004)	(0.005)	(0.004)
Announced ×𝟙(Literacy rate)	0.027***			
	(0.007)			
Announced ×𝟙(Urban population)		0.004		
		(0.007)		
Announced ×𝟙(Income per capita)			0.019***	
			(0.006)	
Announced ×𝟙(Male population)				0.022***
				(0.006)
Observations	731676	731676	731676	731676
Year FE	✓	✓	✓	✓
Month	✓	✓	✓	✓

Notes: The dependent variable is an indicator that takes value one if at least one complaint is filed. Literacy, Urban Fraction, and Low-Income PC are indicator variables that take value one in case literacy, urban population, and income per capita in the municipality are below the Brazilian mean. In each regression, the main effects of the variables are omitted from the specifications as they are collinear with municipality fixed effects. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

6 Discussion and Conclusion

This paper documents a robust and positive impact of the audits on reports of wrongdoing, anonymous and not. These are novel causal findings and data as most of existing research has relied on lab experiments and descriptive analysis. There are, however, several other alternative interpretations of these findings which I attempt to rule out in the discussion here below.

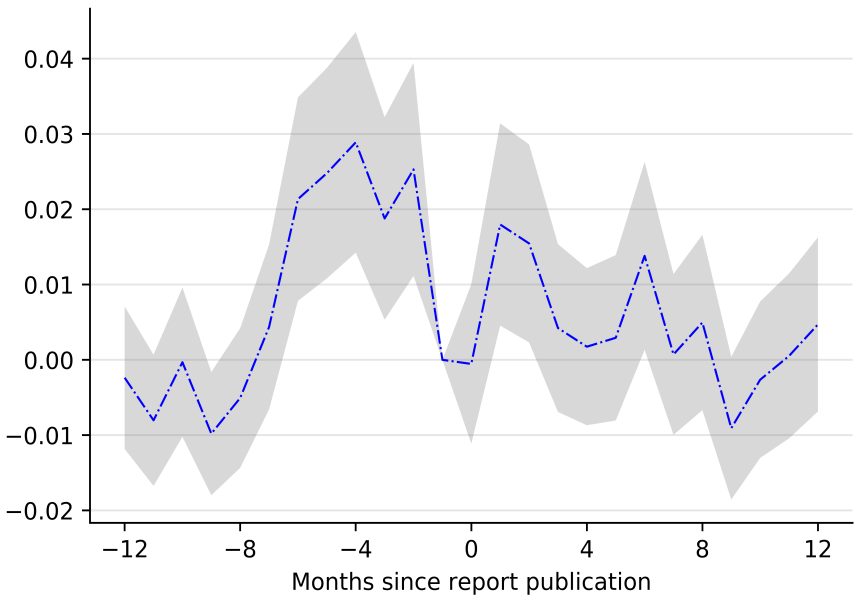
Fine reduction: The possibility for the complainant to negotiate a fine reduction affects the probability of filing a complaint immediately after the audit announcement. The anticipation of an investigation triggers denunciations from wrongdoers as a preventive measure to negotiate penalty reduction.

If fine negotiation is the primary objective, anonymous complaints should not react to an audit announcement. The simple idea is that individuals that intend to negotiate a fine

are not likely to file their complaints anonymously.[17] In contrast, and shown in Section 5, anonymous complaints react to the announcement of an audit in the municipality.

Information Revelation: would instead imply that once the auditors show up in the municipality, more information is revealed about the underlying corruption in that municipality. If information revelation is the mechanism behind these findings, we expect complaints to be filed after the publication of the audit report rather than immediately after the audit announcement. To further investigate this channel, I perform a similar analysis to that in Section 4. I center the event at the time of the publication of the audit report; month zero corresponds to the month of the report publication where m = -1 and m = 1 correspond to the month before and the month after the publication of the audit report, respectively. For this exercise, I restrict the sample to the first 19 lottery rounds. Round 19 is the last round in which the investigation period was predetermined by the CGU headquarters and similar across the states.[18]

Figure 4: Complaints before and after the report publication



Notes: This figures plots the coefficient estimates and the 95 percent confidence interval from a regression of an indicator variable that takes value one if at least one complaint is filed on a set of dummies from m-12 to m+12, where m=0 is the month of the publication of the report; m=-1 is normalized at zero. Standard errors are clustered at the municipality level.

[17] These individuals will make sure to report non-anonymously so that they are identifiable. As mentioned in Section 2, however, the Clean Company Act was introduced in 2013 and my sample stops in 2014.

[18] See section 2 for a discussion on how the lottery rules have changed over time.

Coefficient estimates plotted in Figure 4 clearly show that the probability of filing a complaint is higher in the pre rather than the post-publication period. The pattern indicates that citizens continue to issue more complaints before the publication date hoping their complaints will be taken into account.[19] Interpreting these findings as causal requires that the publication date of the audit report should not be affected by the complaints filed. In Appendix section A.3, I show that the publication date of the audit reports is not affected by the complaints or number of complaints filed.

Quality of government: If citizens view audits as an increase in the government quality they are more likely to complain; audits serve as a signal that their voices are more likely to be heard. Trucco (2016) documents positive externalities of government maintenance work in the neighborhoods of Buenos Aires, such as sidewalk repairs, tree pruning, which affects citizens' demands for new government projects in nearby neighborhoods.[20] She finds also that the increase in complaints is mostly concentrated during the days following the repair which would resemble the spike-decay pattern documented in this paper.

The main finding of this paper is that audits incentivize reports of unlawful practices. The increase in the probability of complaint might be due to an increase in whistleblower protection or an increase in the likelihood of being heard. Also, areas with less social capital are more likely to complain if an audit is announced. These results are robust to alternative specifications. A deeper understanding of the mechanisms is left for future research.

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[19]The pattern in Figure 4 suggests that information revealing occurs before rather than post report publication.

[20]She interprets complaints filed by citizens as "demand" for repairs.

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Appendix

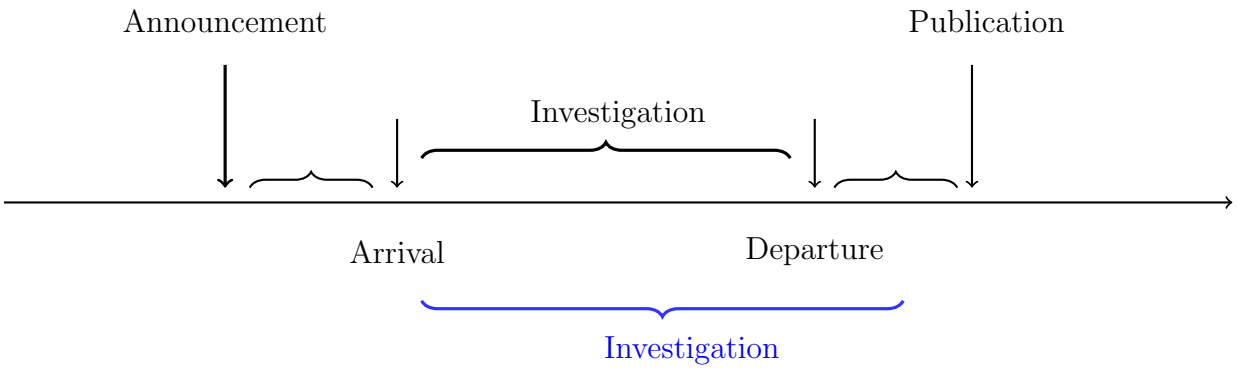
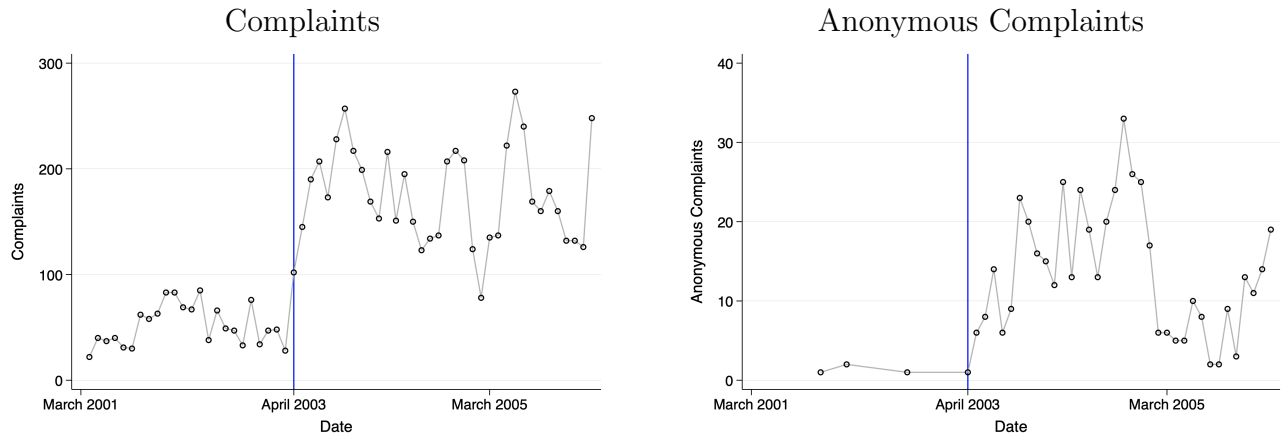
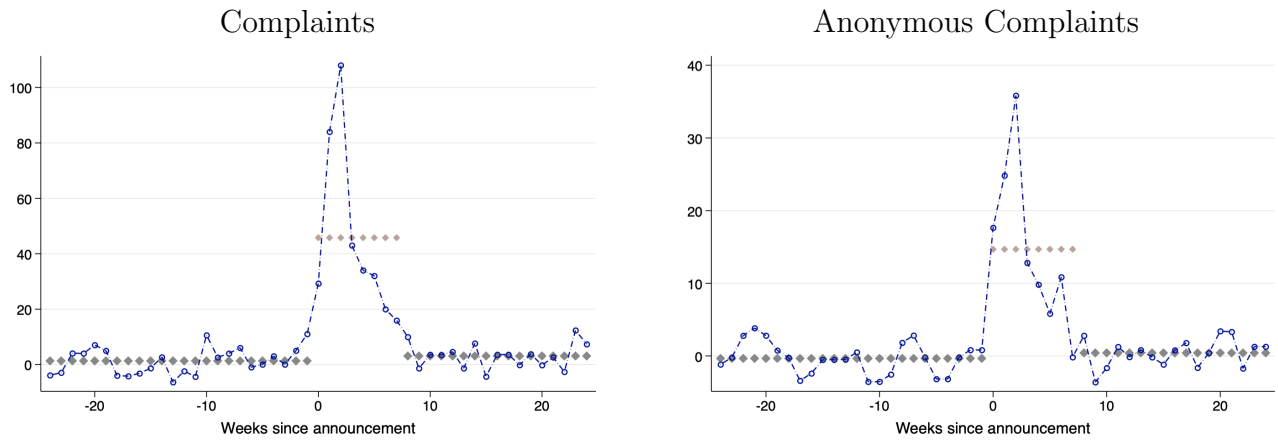

Announcement
Publication
Investigation
Arrival
Departure
Investigation

Figure A.1: Monthly time-series of the number of complainants



Notes: This plot depicts the number of municipalities from which at least one complaint is filed in a specific month. The left panel depicts all complaints; the right panel, instead, shows anonymous complaint. The focus is the time-period around the pilot. The vertical blue line represents the announcement of the audit program.

Figure A.2: Residualized complaints before and after audit announcement (weekly)



Notes: Estimated coefficients and 95 percent confidence interval from a regression of an indicatora variable that takes value one if at least one complaint is filed in from the municipality on a set of dummies from w - 24 to w + 24, where w = 0 is the week of the audit announcement in the municipality. Municipal, year, and month fixed effects are also included in the specification. Standard errors are clustered at the municipality level.

Table A.1: Complaints

	Complaints			Anonymous Complaints		
	(1)	(2)	(3)	(4)	(5)	(6)
Lead 12	-0.002	-0.003	-0.002	-0.003*	-0.003*	-0.003*
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 11	-0.004	-0.005	-0.005	-0.002	-0.001	-0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 10	-0.000	0.000	-0.000	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 9	0.002	0.002	0.001	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 8	0.002	0.003	0.002	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 7	0.005	0.005	0.005	-0.002	-0.003	-0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 6	0.002	0.002	0.001	0.001	0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 5	0.003	0.003	0.003	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 4	-0.005	-0.004	-0.006	-0.003	-0.002	-0.003
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Lead 3	0.006	0.006	0.005	0.000	0.001	0.000
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Lead 2	0.008	0.007	0.007	-0.001	-0.001	-0.001
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Audit	0.048***	0.047***	0.047***	0.021***	0.021***	0.021***
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Lag 1	0.081***	0.080***	0.080***	0.020***	0.021***	0.020***
	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)
Lag 2	0.029***	0.029***	0.028***	0.003	0.003	0.003
	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)
Lag 3	0.012**	0.013***	0.012**	0.003	0.003	0.003
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 4	0.006	0.007	0.006	-0.001	-0.002	-0.002
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Lag 5	0.009*	0.009*	0.008*	0.006**	0.005*	0.006**
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 6	0.019***	0.019***	0.018***	0.003	0.002	0.003
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 7	0.012**	0.012**	0.011**	0.006**	0.006**	0.006**
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 8	0.022***	0.023***	0.022***	0.003	0.003	0.003
	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)
Lag 9	0.018***	0.018***	0.017***	0.003	0.004	0.003
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 10	0.011**	0.011**	0.010**	0.006**	0.006**	0.006**
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 11	0.012**	0.012**	0.012**	0.006**	0.006**	0.006**
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Lag 12	0.007	0.005	0.006	-0.003*	-0.003	-0.003*
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Observations	731676	731676	731676	731676	731676	731676
Year FE	✓			✓		
Month FE		✓			✓	
Year-State FE			✓			✓

Notes: The dependent variable in columns (1) - (3) is an indicator that takes value one if at least a complaint is filed from the municipality. In columns (4) - (6) the dependent variable is an indicator that takes value one if the complaint is filed anonymously. Municipal fixed effects are included in all regressions. Standard errors are clustered at the treatment level, municipality, shown in parentheses.*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A.2: Silenced Channels

	Complaint				Anonymous			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Announced	0.034***	0.034***	0.020***	0.021***	0.012***	0.011***	0.002	0.004
	(0.004)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)
Corruption	0.001	0.001			-0.001	-0.000		
	(0.007)	(0.007)			(0.007)	(0.007)		
Announced x Corruption	0.008***	0.008***			0.008***	0.007***		
	(0.003)	(0.003)			(0.002)	(0.002)		
Announced x 2Tercile			0.018**	0.019**			0.009**	0.007*
			(0.008)	(0.008)			(0.004)	(0.004)
Announced x 3Tercile			0.025***	0.024***			0.021***	0.017***
			(0.009)	(0.009)			(0.006)	(0.005)
2Tercile			-0.062**	-0.062**			-0.038**	-0.037**
			(0.030)	(0.030)			(0.015)	(0.015)
3Tercile			-0.046*	-0.046*			-0.030*	-0.029*
			(0.025)	(0.025)			(0.016)	(0.016)
Observations	22212	22212	22212	22212	22212	22212	22212	22212
Year FE								
Month FE	✓		✓		✓		✓	
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Month FE		✓		✓		✓		✓

Notes: The dependent variable is an indicator variable that takes value one if the municipality receives a complaint in columns (1) - (4). In columns (5) - (8) the outcome variable is an indicator that takes value one the complaint was placed anonymously in the last four columns. 2Tercile and 3Tercile are respectively the second and the third tercile of the corruption distribution. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

A.1 Longer run

I investigate if the effect persists in the longer run. Using data aggregated at the monthly level, I include other 12 lags to the estimating equation and test if these lags are jointly significant. The F-statistics on the joint significance of the coefficients on the audit dummies for leads 1 - 12, lags 0 - 12, and lags 13 - 24 are shown in Table A.3. The test indicates that the lead dummies are not jointly significant, confirming the non-anticipatory effect of the audit in the municipality. Similarly, the test also indicates that the effect vanishes one year after the audit announcement: lags 13 - 24 are jointly insignificant. The F-statistics on lags 0 - 12 instead rejects the null hypothesis that all coefficients are jointly zero.

Table A.3 shows the resulting p-values from a F-test of joint significance. The test supports the hypothesis that the effect persists in the longer run.

Table A.3: Long(er) run persistence?

	Leads	Lags 0-12	Lags 13-24
Complaints	0.49	0.00	0.13
Anonymous Complaints	0.23	0.07	0.30

Notes: Each cell presents p-values from an F-test on joint, leads or lags, significance. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

A.2 Further Randomization test

To make sure that audited and non-audited municipalities do not exhibit significant pre-audit differences, I use municipal characteristics measured in year 2000.

For this, I estimate the following linear model:

$$Audit_{i+t} = \beta_0 + \beta_1 x_i + \eta_c + \varepsilon_i \tag{5}$$

Where $Audit_{i+t}$ is an indicator variable if the municipality is audited in the future and x_i are municipal characteristics retrieved from the Brazilian census of the year 2000 (as mentioned above). I estimate the specification including each of these municipal characteristics, controlling for state fixed effect and population measured at baseline (the year 2000). I also test whether these characteristics jointly determine treatment.

Results are shown in Table A.4 and they support the fairness of the lottery. Column (1) depicts the regression of the audit indicator on a series of covariates controlling for population and state fixed effects. Coefficients shown in columns (2) are coefficients from separate regressions of the audit dummy on each covariate controlling for population and state fixed effects. Table A.4 shows, as expected, that there is no significant effect of any of these covariates on the future treatment.[21]

[21] Other randomization checks on the audit program such as controlling for the selection probability, the fairness of the lottery using numbers attributed and selected to municipalities see Muço (2017).

Table A.4: Randomization Check

Treatment	All	Pairwise
GDP	0.0017	0.0016
	(0.0017)	(0.0016)
HDI	-0.1462	0.0984
	(0.4039)	(0.1493)
Literacy	0.2399	0.5471
	(2.1777)	(1.0183)
Electricity TV	-1.3212	0.1798
	(1.2848)	(0.4742)
Income per Capita	0.0196	0.0527
	(0.1817)	(0.1087)
Urban population	0.0940	0.0782
	(0.0446)*	(0.0321)*
Male population	-0.4859	-0.9369
	(0.6479)	(0.5553)
Residual Vote (2002)	-0.0713	-0.0792
	(0.2279)	(0.1675)
Municipal Area	-0.0012	-0.0011
	(0.0015)	(0.0015)
Distance State Capital	0.0193	0.0044
	(0.0611)	(0.0430)
Latitude	-0.0025	-0.0011
	(0.0047)	(0.0043)
Longitude	0.0063	0.0042
	(0.0054)	(0.0039)
Altitude	0.0198	0.0185
	(0.0313)	(0.0285)
Illumination	-0.0093	-0.0087
	(0.0050)	(0.0044)*
Electorate (2002)	-0.0012	-0.0015
	(0.0012)	(0.0012)
F-test	1.482	
R-squared	.039	
Observations	5475	5475

Notes: The dependent variable is a dummy which takes value one if the municipality is audited throughout the sample period and zero otherwise. All covariates have been measured before the introduction of the audit program. Population and Literacy rate population and literacy rate in the municipality. Fraction Urban, Female, and Illumination are, respectively, the fraction of the urban population, female population, and households reporting to have sources of illumination. Electricity TV is the rate of households reporting to have current electricity and TV in the household. HDI stands for Human Development Index, measured at the municipality. Income per capita is measured in $Reais. All the above are measured in the year 2000. Latitude, Longitude, and Altitude are coordinates of the population centroid in the municipality. Distance to State and Federal capital measure the distances of municipalities from the state capital and Brasília, the federal capital. Surface is the municipal area measured in square km. Electorate is the total electorate in the municipality, Residual votes is the fraction of invalid votes, both measured in the electoral term previous to the audit, namely the year 2002 national elections. The first column depicts the regression of the treatment dummy on a series of covariates controlling for population state fixed effects. The second columns show coefficients from separate regressions. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

A.3 Time before publication

I investigate whether there is a strategic delay in publication of the audit reports influenced by the complaints placed. To do so, I restrict the sample to the first nineteen rounds of the audit program and estimate the following linear equation:[22]

$$Time\ pub_i = \beta_0 + \beta_1 Complaint_i + \eta_c + \varepsilon_i \tag{6}$$

Where $Time\ pub_i$ is the number of days it takes the auditors to publish the audit report since the announcement of the audit in municipality i. $Complaint_i$ if at least a complaint – or anonymous complaint – is filed in the municipality during that timeframe. As shown in Table A.5 there is no significant effect of complaints on the time it takes the auditors to publish the audit reports. β_1 is nonsignificant across specifications.

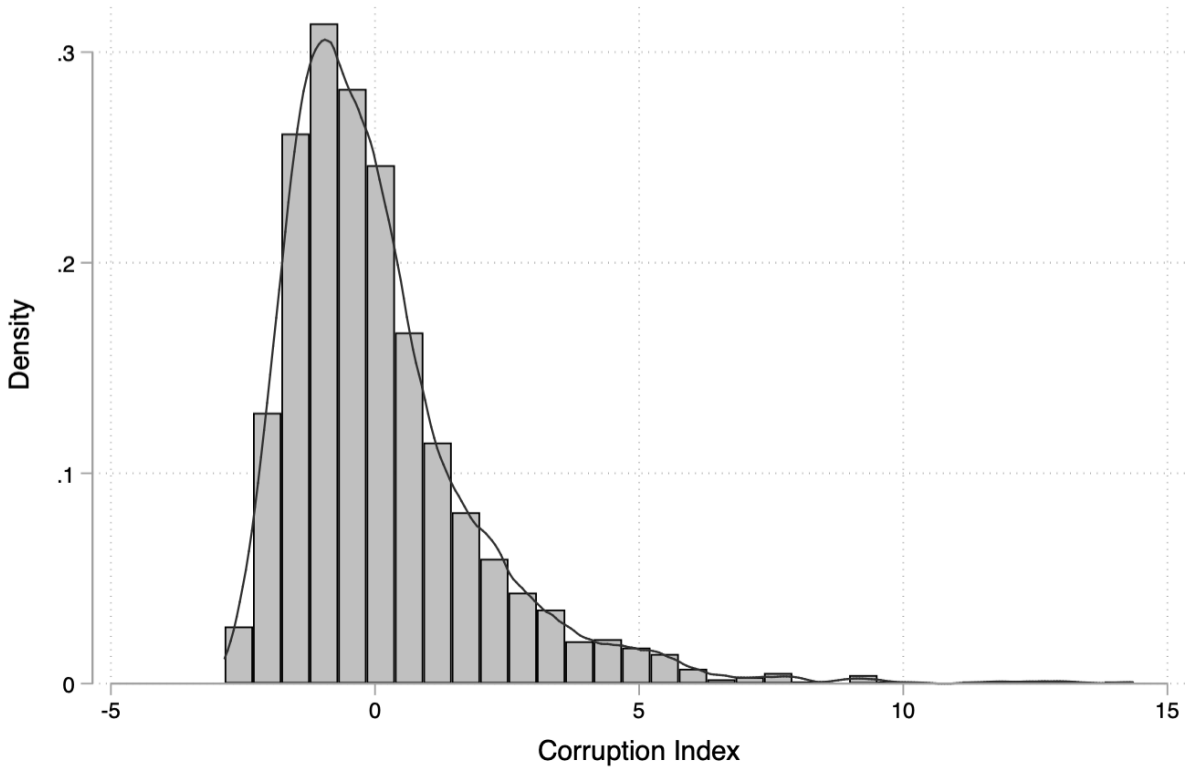
Table A.5: Do complaints delay publication of the audit reports?

	Days to Publication					
	(1)	(2)	(3)	(4)	(5)	(6)
Complaint	-3.320	-2.326	-2.780			
	(5.911)	(5.990)	(6.262)			
Anonymous Complaint				-0.940	0.217	-0.466
				(3.942)	(4.096)	(4.238)
Observations	880	880	880	880	880	880
State FE	✓	✓	✓	✓	✓	✓
Population		✓	✓		✓	✓
Controls			✓			✓

Notes: In columns (1) - (3) the main regressor is a dummy if a complaint in the municipality. In columns (4) - (6) if an anonymous complaint is filed. All regressions include state fixed effects. The sample is restricted to municipalities audited in the first 19 rounds of the program. Standard errors are clustered at the municipality level, shown in parentheses. *** Significant at the 1 percent level. Significant at the 5 percent level. * Significant at the 10 percent level.

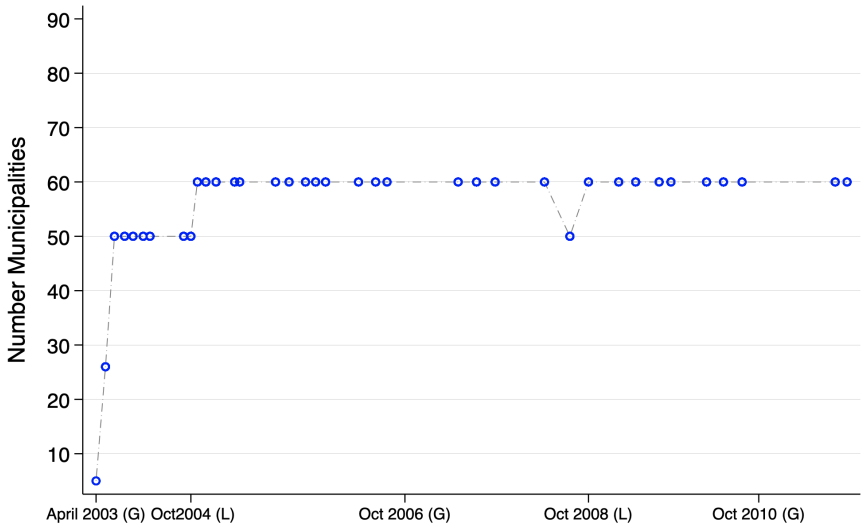
[22]The use of the first 19 rounds of the lottery is necessary because of the changes in audit rules.

Figure A.3: Distribution of the Corruption Index



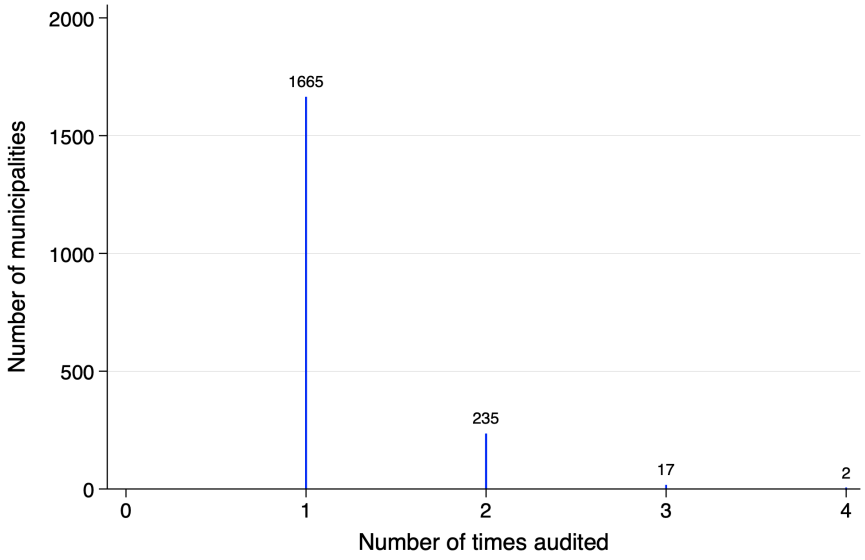
Notes: The figure depicts the distribution of the corruption index.

Figure A.4: Timeline of the Audit Program



Notes: In the vertical axis are plotted the number of municipalities audited per round.

Figure A.5: Audit counts



Notes: The vertical axis depicts the number of municipalities audited. The horizontal axis counts the number of times the municipality is audited.