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or the Central Government? Evidence from
the Brazilian Anti-Corruption Program**

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Who Is Punishing Corrupt Politicians - Voters or the Central Government? Evidence from the Brazilian Anti-Corruption Program*

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Abstract

In 2003 the Brazilian central government (CG) launched an anti-corruption program. Since then municipalities have been randomly selected to be audited on a monthly basis. Evidence in the literature suggests that the probability of re-election of an incumbent mayor decreases as the number of reported corruption violations rises before the municipal elections. By exploiting the exogenous variation in the timing of the release of the audit reports and the Brazilian institutional scheme, this paper sheds light on the mechanisms through which the Brazilian anti-corruption program functions. After the release of the audit reports, municipalities where more than two corruption violations were reported receive 26% fewer transfers from the CG. Total expenditure on infrastructure is also reduced. While the CG increases the amount of transfers to municipalities where the mayor is both affiliated with the party's president and found to be honest, it helps politically aligned municipalities with high levels of released corruption to move through the punishment process more quickly. The effects of the dissemination of corruption information on the probability of re-election of incumbent mayors seem to gradually disappear with time. Yet, when these effects have completely faded and voters have time to feel the consequences of receiving fewer transfers, the probability of re-election of corrupt politicians decreases.

JEL codes: D72, D78, H41, H70.

Keywords: intergovernmental transfers, corruption, accountability, decentralization.

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Abstract

In 2003 the Brazilian central government (CG) launched an anti-corruption program. Since then municipalities have been randomly selected to be audited on a monthly basis. Evidence in the literature suggests that the probability of re-election of an incumbent mayor decreases as the number of reported corruption violations rises before the municipal elections. By exploiting the exogenous variation in the timing of the release of the audit reports and the Brazilian institutional scheme, this paper sheds light on the mechanisms through which the Brazilian anti-corruption program functions. After the release of the audit reports, municipalities where more than two corruption violations were reported receive 26% fewer transfers from the CG. Total expenditure on infrastructure is also reduced. While the CG increases the amount of transfers to municipalities where the mayor is both affiliated with the party's president and found to be honest, it helps politically aligned municipalities with high levels of released corruption to move through the punishment process more quickly. The effects of the dissemination of corruption information on the probability of re-election of incumbent mayors seem to gradually disappear with time. Yet, when these effects have completely faded and voters have time to feel the consequences of receiving fewer transfers, the probability of re-election of corrupt politicians decreases.

1 Introduction

In recent years, national governments have frequently adopted anti-corruption programs that attempt to increase political accountability, strengthen political participation in civil society, and improve public-sector management at the local level. In 2003, an anti-corruption program was launched by the Brazilian central government in order to examine the allocation of federal resources by local governments. Since then, municipalities have been randomly selected by lottery to be audited on a monthly basis. A few months after the audit event, audit reports are sent to all levels of government and are also made available on the Internet. In the literature, there is evidence that the probability of re-election of incumbent mayors decreased as reported corruption violations rose before the 2004 municipal elections (Ferraz and Finan 2008). According to their interpretation, this effect is a result of the dissemination of corruption information among voters.

However, other punishment channels may be in place. One potential channel that has not been previously discussed in the literature is that a reduction in federal discretionary transfers to local municipalities may affect voter behavior. If voters reward politicians for obtaining more public goods or services, a reduction in transfers by the central government could trigger punishment by voters at the polls. This is a plausible and relevant argument in a context where political clientelism is present and most of the municipalities are highly dependent on federal transfers. Extensive anecdotal evidence suggests that voters reward practices related to political clientelism and political patronage in Brazil. A popular saying regarding politicians is “*ele rouba mas faz*”, that is, he steals but he gets things done (Laranjeira 1999). In fact, an increase in transfers has been shown to have a positive effect on the probability of re-election of incumbent mayors/parties in Brazil (Brollo et al, 2009 and Litschig and Morrison, 2009).

Brazil presents ideal conditions for understanding how voters react when mayors are found to be corrupt, and when transfers are reduced to their municipalities as a result. It is plausible to suggest that the central government may reduce the flow of government funds to municipalities after instances of corruption are exposed in those same municipalities. According to the institutions that handle discretionary infrastructure transfers (*Instrução Normativa do Supremo Tribunal Nacional*, nº 1, 15 January 1997), in order to receive these transfers, each municipal administration has to sign an agreement with the central government. When the municipal administration is found to be in breach of contract, the municipality is prohibited from receiving

these discretionary transfers. The auditors are then required to implement specific procedures to evaluate whether the flow of transfers can be reestablished¹.

Therefore, there are two potential channels that could have triggered punishment by voters at the polls: (1) loss of reputation on the part of politicians because of corruption evidence that is still fresh in voters' memories due to the dissemination of corruption information, or (2) just a reduction in transfers by the CG. In reference to the former, voters could have punished corrupt politicians because they know about the audit reports and the corruption released was higher than their prior beliefs. In the latter possibility, voters may not know about the audit reports, and simply punish corrupt politicians at the ballots as a consequence of the reduction in transfers to their municipality. Note that there is also the possibility that voters are punishing corrupt politicians because they know about the corruption evidence, but only care about corruption because it may cause a reduction in transfers.

This paper sheds light on the mechanisms through which the Brazilian anti-corruption program acts by exploiting the exogenous variation in the timing of the release of the audit reports and the Brazilian institutional scheme. For this reason, federal transfers are included in the analysis. The focus relies on infrastructure transfers because they are the most discretionary ones.

The analysis is divided into two parts. After coding the information contained in the audit reports for municipalities in 15 lotteries, I first investigated how the central government reacts to the disclosure of local corruption by using data on infrastructure transfers and federal budgetary amendments to Brazilian municipalities. Here I consider two samples of municipalities: the random sample of audited municipalities (779 municipalities) and the sample of all municipalities (audited and non-audited) that participate in the anti-corruption program (5,490 municipalities)². I then identify the effects of the disclosure of corruption on transfers by considering a longitudinal dataset of infrastructure transfers for eight years (1999-2006) and by exploiting the exogenous variation in the timing of the release of the audit reports.

The results suggest that the central government reduces transfers to those municipalities where mayors are found to be corrupt. Transfers significantly decreased by 25.9% to municipalities with more than two corruption violations (30% of the sample) after the release of the audit reports. Moreover, this effect persisted for at least three years after the release of the audit reports. However, the results are not persistent over time in municipalities with mayors affiliated with the party of the president. This suggests that the CG helps politically aligned municipalities to move through the punishment process more quickly when high levels of corruption are released. In addition the CG increases transfers to municipalities whose mayors were found to be honest, but only to those that are politically aligned with the president. These results indicate that the CG attempts to minimize political capital losses and maximize political gain by minimizing punishment (reduction in transfers) selectively to municipalities according to their party affiliation. Broilo and Nannicini (2010) show evidence of politically motivated transfers in close Brazilian races³.

A natural concern that could arise is whether this reduction in transfers is translated into the lower levels of the supply of public goods. This study shows evidence that the release of corruption has no effect on the amount of constitutional transfers received, municipal budget surpluses, and revenues from local taxes for any level of corruption reported, suggesting that there is no crowding-out effect. Most importantly, the release of the audit reports has a negative

¹In the audit reports for some municipalities, I found evidence that the execution of projects was mired because of cuts in transfers from the central government regarding the execution of the projects. According to the audit reports, this cancelling of federal transfers had occurred because the previous municipal authority had been found to be in a breach of contract situation during a previous audit.

²All Brazilian municipalities with fewer than 450,000 inhabitants.

³see Solé-Ollé and Sorribas-Navarro (2008b) for evidence in Spain.

effect on infrastructure expenditure in municipalities with more than two reported corruption violations.

This part of the analysis is relevant per se in terms of policy implications. Since voters care about transfers, a reduction in transfers can have a negative impact on voter welfare. Furthermore, if the reduction in transfers to municipalities where corruption is found drives an electoral backlash, the central government would perform a vital role in this anti-corruption program. When political clientelism is present, punishment by the central government (reduction in transfers) can act as the linchpin in local political selection. Furthermore, a self-interested central government could use this program as a tool to increase its political capital.

Some concerns regarding this first part of the analysis are addressed. First, this reduction in transfers can be demand-driven. That is, mayors have to exert an effort in applying for these transfers. Therefore, the release of the audit reports could have created disincentives for mayors when applying for these transfers, because they plan to forgo elections⁴. Second, President Lula's first administration started in 2003 (the year when the Brazilian anti-corruption program was launched). It could be that since the beginning of Lula's administration, the amount of transfers to municipalities affiliated with the party in opposition to Lula has decreased as compared to before he took office. Opposition parties had also been part of the governing coalition during the two consecutive previous presidential terms (Fernando Henrique Cardoso, 1994-2002). Because corruption is likely correlated with transfers, it could also be associated with mayor's political party affiliation. Third, the political selections induced by the release of the audit reports (audits increase turnover, resulting in more first-term mayors) could drive the results if first-term mayors are disadvantaged in terms of luring federally transferred resources into municipalities. These and additional checks are discussed later.

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⁴Since most of these discretionary transfers depend on the execution of federal budgetary amendments, an alternative dataset of federal budgetary amendments is also employed in order to address this issue. It collects information on the value of the budgetary law initially approved by the president (corresponding to the value applied by the municipality), the value of the budgetary law amendment later authorized by the legislative branch and the value of the budgetary law amendment executed (paid) by the executive branch to each municipality in a given year. The results indicate that during the years following the audit, there was no change in the demand for transfers.

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The second part of the analysis tries to disentangle the possible channels that drive the electoral punishment. Corruption, transfers, and electoral outcomes might all be correlated. Therefore disentangling the effects of transfers and corruption on electoral outcomes is not a trivial task. However, by exploiting some particulars of the Brazilian institutional environment and the timing of transfers, it is possible to address this issue. It is crucial to check the timing of the central government punishment (timing of when transfers are decided and executed) in order to understanding the methodology used to identify the channel. Transfers are decided and executed at the end of the Brazilian fiscal year (December). By using the data on an alternative dataset of budgetary amendments, I found that there are some bureaucratic snags in the central government punishment process. Furthermore, I show that only municipalities with corruption released at least 15 months before the elections could have perceived the consequences of the reduction in transfers before the October 2004 municipal elections.

Following this, I use a semi-parametric model that makes it possible to estimate the dynamic treatment effect of “having corruption information released before the elections”, conditional on the exogenous timing of the release of the audit reports before the elections. The control group is those municipalities with audit reports released after the elections. In this framework, dummy variables that denote the distance in time (number of months) from the release of the audit reports to the municipal elections, before the elections, are considered in the analysis. By allowing for variation of the treatment effects across timing of release, i.e. these dynamic treatment effects, we can check how the effects of the dissemination of corruption information among voters (if any) varies across time of release. Most importantly, it is possible to disentangle the channels through which voters are punishing corrupt politicians at the polls. Thus, if the only source of electoral punishment is monitoring from the central government (cutting resources for infrastructure projects in municipalities with corruption released), these effects come from those municipalities with corruption revealed at least 15 months prior to the elections. Therefore, these effects are broken down into two components: the part of the effect that comes from the tightening of the transfers channel, and the part of the effect that comes from the loss of reputation due to the dissemination of corruption information among voters. Furthermore, this model allows us to check if there is an information fading-away factor.

The results indicate that the release of corruption information has a strong negative impact on an incumbent mayor’s probability of re-election (and on that of a political party) in municipalities with audit reports released close in time to the municipal elections. However, this effect (loss of reputation channel) gradually fades with time. The impact of the release of the audit reports on electoral outcomes fades after six months. Then, the results suggest that when the effects of loss of reputation on the part of politicians due to the dissemination of corruption information among voters have completely disappeared, voters punish corrupt politicians as a consequence of the reduction in transfers.

Additional evidence that ensures the channels are actually disentangled is presented. Note that local radio station is an important source of dissemination of information at the local level in Brazil, where almost 40% of the population above the age 20 is illiterate. According to Ferraz and Finan (2008) the average effect of the release of the audit reports are stronger in municipalities with local radio stations, suggesting that the local radio is the vehicle for the dissemination of corruption information. Therefore, considering only the sample of municipalities where local radio station is not present, we should expect no effects in municipalities with audit reports released soon before the elections. Actually, in places with no local radio station, negative effects of corruption disclosure on the probability of the re-election of an incumbent political party were found to be present only in those municipalities where audit reports were released at least 15 months prior to the elections⁶.

⁶I provide additional evidence that negative effect of corruption release on re-election outcomes in municipalities

Subsequently, I estimate the static average effect of disclosed corruption on the incumbent politicians probability of re-election. A surprising new result is that voters do not punish corrupt mayors who are affiliated with the political party of the president. Note that Solé-Ollé and Sorribas-Navarro (2008a) found that intergovernmental grants allocated to co-partisans buy more political support in Spain.

This article is organized as follows. Section 2 discusses the related literature; Section 3 reviews the Brazilian institutional context and explains the data employed; Section 4 presents the estimation strategy and results for the punishment by the central government, and Section 5 disentangles the channels. Finally, concluding remarks are provided in Section 6 .

2 Related Literature

Empirical literature that analyzes the behavior of voters in the presence of information on corruption is rare due to the difficulty of measuring corruption, especially at the local level and particularly in developing countries. At the aggregate level, most studies rely on indices of corruption perception (See Person et al. (2003), Kunicova and Ackerman (2005), Chang and Golden (2004, 2007), and Picci and Golden (2007)). At the micro level, most empirical studies that analyze voter behavior and corruption information consider accusations as a measure of corruption (Peters and Welch, 1980 for a U.S. case and Chang, 2005 for an Italian case). Using the information from the audit reports, Ferraz and Finan (2008) show the effect of the disclosure of corruption on politician’s electoral outcomes is stronger in municipalities with local radio stations⁷. With the same data, Ferraz and Finan (2007) have found that second term mayors are more corrupt than first term mayors.

When redistributive policies are also taken into account, evidence of the effect of corruption information on political selection is even scarcer⁸. On the other hand, there is evidence regarding the effect of intergovernmental transfers on the probability of re-election of incumbent mayors. Brollo et al (2009) find that constitutional federal transfers have positive effects on corruption, attract politicians of lower quality, and increase the probability of re-election of incumbent mayors in Brazil. However, there is no precedent in the literature that analyzes both the effect of corruption information and transfers.

This paper is also related to the literature that analyzes the impact of information on the efficient allocation of public goods (Reinikka and Svensson, 2004a). A growing empirical literature referring to developing countries has shown that more information provision increases accountability and improves the allocation of resources at the local level (as Besley et al. 2004, Galasso and Ravallion, 2005, Reinikka and Svensson, 2004b). On the basis of a randomized, controlled field experiment in 608 Indonesian villages, Olken (2007) finds that grassroots participation has limited effects on curbing corruption.

In a broader sense, this study is associated with the literature on corruption, decentralization, and accountability in developing countries⁹. In developing countries, the risk of losing office is

with the release of the audit reports 15 months prior to election seems to be due to the reduction in transfers. When the amount of transfers received after the release of the audit reports are interacted with these dummies that denote the distance in time, transfers capture the effects of the release of corruption only in municipalities where the audit reports are released 15 months prior to the elections.

⁷Stromberg (2004) provides evidence that governors allocated more funds to areas where a larger share of the population owned radios. Additionally, Besley et al (2006) argue that media effectiveness depends on the extent to which the media is controlled by politicians or social elites.

⁸Political determinants of redistributive policies are analyzed in Person Tabellini (2000). Examples of empirical evidence come from Italy (Picci and Goldman 2007), Albania (Case 2001) and Brazil (Ames 2001).

⁹See Cai and Treisman (2005), Rodden and Rose-Ackerman (1997), Shleifer and Vishny (1993), Waller, Verdier and Gardner (2002), Seabright (1996), Sonin (2003) Bardhan and Mookherjee (1999, 2005, 2006)

reduced, and local governments might be vulnerable to practices such as capture by special interest groups (Bardhan and Mookherjee 1999, 2005, 2006 and Sonin, 2003). This argument is reliable in the Brazilian context, where most municipalities are strongly dependent on federal transfers as a source of revenue, the illiteracy rate is high, and political clientelism is present.

Evidence provided by this study also contributes to the debate on whether extensive information provision is good for voters. Besley and Smart (2007)¹⁰ show that better information about government tends to reduce discipline and increase first-term rent-seeking. However, better information also improves selection, as bad incumbents are less likely to be re-elected. However, in some circumstances, additional issues should be taken into account. Take, as an example, a developing country with a high degree of decentralization and political clientelism, where local corruption is positively associated with poorer regions. In this case, an anti-corruption program that changes the allocation of federal resources may create additional undesirable effects in terms of the welfare of voters.

3 Institutional Context and Data

3.1 Federal Transfers

3.1.1 Decentralization of Social Programs and Infrastructure Goods and Services Delivery

Since the constitution was ratified in Brazil in 1988, municipal administrators have become increasingly responsible for bringing home a relevant share of the provision of public services. In this context, decentralization is related to the delivery of social programs (education, health, and social assistance) as well as the delivery of infrastructure goods and services (the paving of roads, the installation of sewer systems and water distribution systems, the provision of hospital equipment, etc.).

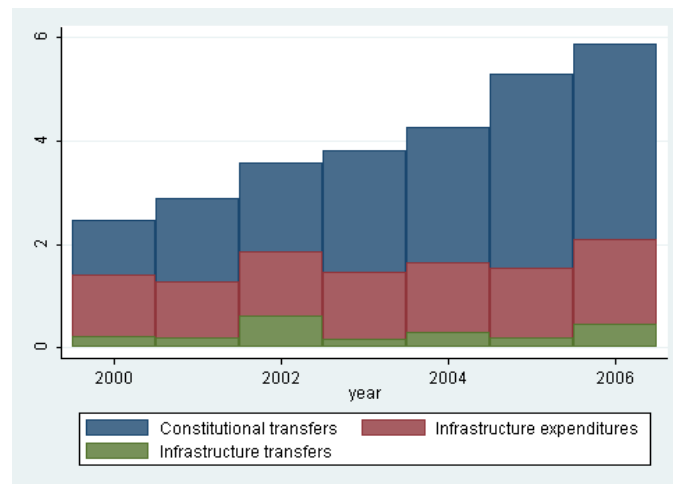
The resources of Brazilian municipalities come from (a) local revenues, such as fines, exemptions, service taxes (ISS), and residential property taxes (IPTU); (b) transfers from federal, state and municipal governments. The most important source of municipal revenue is federal transfers. Basically, they are from two different types: (1) constitutional automatic transfers (i.e. Fundo de Participação do Município - FPM); and (2) discretionary transfers (*CONVÊNIO*), most of them (82%) referring to infrastructure projects.

Excluding some big cities, such as Brazilian state capitals, municipalities are strongly dependent on these transfers as their sources of revenue (tax revenue represents only 6% of the municipal total revenues, on average). The allocation mechanism of automatic, constitutionally mandated transfers (which corresponds to 75% of federal transfers) depends on the population size and the state in which the municipality is located. From this total amount of federal transfers received by each municipality, there are also fixed coefficients that establish the amount of funds to be converted into education and health expenditures.

The focus of this study is federal transfers devoted to infrastructure projects, which represent 15% of the municipal total expenditure with infrastructure projects. The allocation process of these transfers is the most discretionary one and they are used to finance projects that are directly visible to voters. They are related to projects that consider the construction of houses and bridges, the paving of roads, the building of systems of water and sewer linkage, the purchase of ambulances, etc. Figure 1 illustrates the Evolution of constitutional transfers, infrastructure transfers and infrastructure expenditures (in R\$ 1,000,000) during the period 2000-2006.

¹⁰Lockwood (2005) shows that there is always a non-empty set of parameter values for which the hybrid equilibrium in the Besley and Smart model is unstable.

Figure 1: Evolution of constitutional transfers, infrastructure transfers and infrastructure expenditures



Notes. constitutional transfers, infrastructure transfers and infrastructure expenditures (in R\$ 1,000,000).

Note that these infrastructure transfers are highly discretionary and present peaks during the electoral years (2002 and 2006 - federal elections; 2000 and 2004 - municipal elections). Note also that infrastructure expenditures follow exactly the same trend of infrastructure transfers, suggesting that they are important to determining differences in the level of expenditures with infrastructure projects (most visible to voters) at the local level. Budgets of Brazilian municipalities are limited. Most constitutional transfers are tied to current expenses regarding education and health services, such as the payment of salaries. Discretionary transfers that refer to the delivery of infrastructure projects can make a difference. On average, 78% of the total amount of infrastructure transfers is discretionary. The accomplishment of these discretionary transfers depends on the completion of agreements, known as *CONVÊNIO*¹¹. These are agreements between the central government and the local administration. They are proposed by the interested party (municipal administration) to the title-holder of the ministry or the body responsible for the program. These *CONVÊNIOS* which finance infrastructure projects are directly related to budgetary amendments, that is, their realization depends on the execution of federal budgetary amendments. See Section 3.1.3 for obtaining more details regarding the sources of the data.

The municipal administration presents a work program that should contain the following information: the justification for the project, a full description of what is to be executed as well as the goals to be achieved, the stages of execution (specifying the start and end or forecast), proof that the municipality is not in a breach-of-contract situation, and proof to federal institutions that there are no irregularities. As explained in the law governing such agreements, if auditors find that the municipality is found to be in breach of contract, the local authorities are unable to continue receiving resources based on these agreements or to enter into new agreements. When the ministry responsible for the *CONVÊNIO* agreement receives the audit reports, the information on the violations contained in the audit reports should be entered into the federal government's financial system (SIAF). The managers of these agreements in each ministry are

¹¹ *Instrução Normativa do Supremo Tribunal Nacional*, nº 1, 15 January 1997.

also subject to audit procedures. When there is evidence of corruption (especially of fraud, irregularities in the procurement practice process or the diversion of funds) auditors should implement procedures to evaluate whether the flow of transfers can be re-established. These procedures are complex and can take time to be completed. If the violation is the fault of the former administration, the current mayor should request that auditors (TCU) begin this procedure as soon as possible in order to re-establish the flow of transfers or to carry out new agreements.

In addition, these agreements require the convening of a council comprised of members of the community who do not belong to the local administration¹². Information about the completion of the agreements is published on the Internet as well as in the *Diário Oficial da União* (the official government records). Payment can occur outright or in installments, depending on the project specifications. In cases in which installments are used, subsequent payments are conditional on accountability with regard to the previous installment. These agreements originate from the Brazilian Fiscal Budget and, in most cases, they depend on the federal deputies' collaboration in terms of proposing amendments to the federal budget.

3.1.2 Budget Process

In order to analyze the allocation of discretionary federal transfers, it is crucial to understand how the legislative bargaining process works at the federal level. The executive branch controls the establishment of all budgetary laws. Basically, all budgetary laws are subject to having amendments added by legislators. In most cases, the legislators choose the municipalities that will receive the grants. The bulk of the proposed amendments include benefits to local areas, trying to bring the pork home. Amendments can be proposed on an individual basis, by state or region, and by the parties. There are limits for both the number and the value of the amendments proposed¹³. The Budget Committee is responsible for the authorization of the bill. After a period of discussion, Congress votes for the budgetary law, which is then sent to the President for the final decision. As budget is not mandatory in Brazil, the President has a major role in deciding the allocation of the discretionary transfers, and he can use them to make congressmen follow the guidelines of the government coalition.

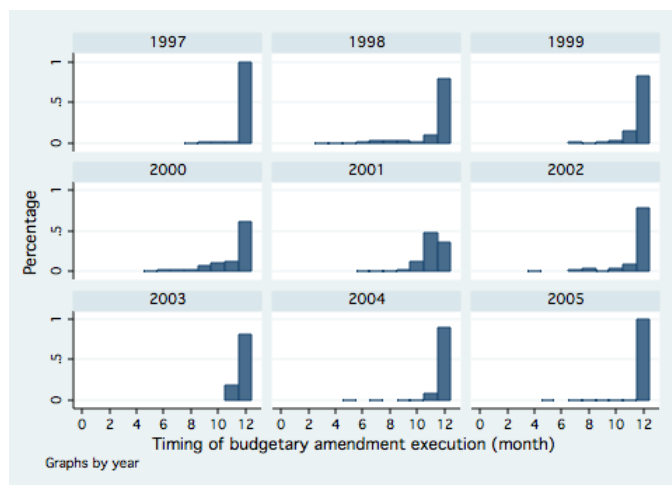
The executive branch chooses which projects authorized by the budget will be carried out first. Only expenditures that have been authorized by the legislative branch can be put into action by the executive branch. However, according to the Brazilian constitution, the executive branch does not necessarily have to carry out the expenditures that were authorized by the legislative branch. Naturally, that prerogative provides the government with an important weapon in terms of political negotiations. As a result, most budgetary amendments are decided and executed, at the same time, in December, at the end of the fiscal year. Figure 2 shows the timing of payments for budgetary amendments during the period from 1997-2005.

Section 3.1.2 explains this three-phase process, which attributes different values to laws associated with the budget process: 1) the value of the budgetary law initially approved in the Brazilian annual budget; 2) the value of budgetary amendments later authorized by the Budget Committee, which can be less than, equal to, or greater than the value previously approved; and 3) executed budgetary amendments, which can only be equal to or less than the value authorized

¹²A frequent occurrence reflected in the audit reports relates to the non-existence of these councils or irregularities in its structure. In most cases, there is evidence that they exist, but are not in operation. I classified this situation as an occurrence of bad administration. Also, with some of these occurrences, there is evidence that members of these councils ignore what is a procurement bid or the fact that this procedure is required.

¹³According to Pereira and Mueller (2002), the execution of individual amendments is an important mechanism that the executive branch has at its disposal in reference to negotiating its preferences with the congressional coalitions.

Figure 2: Timing of federal budgetary amendment execution



by the legislative branch.

At the end of the day, the voters of a municipality will receive discretionary transfers depending on three factors: (i) The effort of their municipal administration in applying for these transfers; (ii) the interests of a federal congressman in supporting the municipality; and (iii) the interest of the President in executing the budget amendment (that is, sending the money exactly to that municipality).

3.1.3 Transfers and Political Party Data

Data on infrastructure transfers is obtained on the Brazilian National Treasure Website (*Tesouro Nacional*) –*FIMBRA* dataset, which provides information from municipal and state annual balance sheets about assets, liabilities, revenues, and expenditures for all Brazilian municipalities and states. This is self-reported data from municipalities and the Brazilian National Treasure organizes it. Only since 2002 has it been possible to distinguish discretionary from constitutional transfers that finance infrastructure projects. For consistency, the period of analysis (1999-2006) considers the overall amount of federal infrastructure transfers. Note that, on average, 78% of the total amount of infrastructure transfers is discretionary (*CONVENIO* agreements for infrastructure projects. More precisely, the left-hand side variable used in the first part of the analysis is the log of the per-capita total amount of infrastructure transfers¹⁴.

Most *CONVENIO* agreements for infrastructure projects are subject to the execution of federal budgetary amendments. Amendments can be proposed on an individual basis, by state or region, and by the parties. Data on budgetary amendments were obtained on the Brazilian Senate Website. This study considers only individual amendments, which represent 27% of the value of the total amendments because only data on individual budgetary amendments make possible the identification of the municipalities that receive the benefits. These individual amendments are marked with the name of the deputy. The data contains information about

¹⁴For those municipalities that do not receive infrastructure transfers, are not dropped when I run the log specification. For the log transformation I considered a reported amount of R\$ 1,00, then, the log amount is zero. All budget variables are in real values, base year 2006 (IPCA - FGV deflator).

entire budget process: value of the budgetary law initially approved by the president; value of the budgetary law amendment later authorized by the legislative branch; and value of the budgetary law amendment executed (paid) by the executive branch. This study exploits these 3 different values according to each phase of the budget and the variables constructed follow the same log transformation criteria and deflator as described above.

Information about mayors' characteristics, such as party affiliation, was obtained in the survey *Perfil dos Municípios Brasileiros* for the years 2002 and 2004. For the years 1999, 2000, 2001, 2005 and 2006 data on mayors' party affiliations were obtained from the Tribunal Superior Eleitoral (TSE)¹⁵. The Brazilian Institute of Geography and Statistics (IBGE) elaborates this database.

The seven most important political parties considered are the PT (the federal governing party after 2002), PMDB, PDT and PTB (the governing party coalition after 2002), and the PFL, PSDB, and PP (the opposition parties after 2002). Data on municipal characteristics were obtained from the Brazilian Census (2000) and *Perfil dos Municípios Brasileiros* (2004).

3.2 Corruption

3.2.1 The Brazilian Anti-Corruption Program

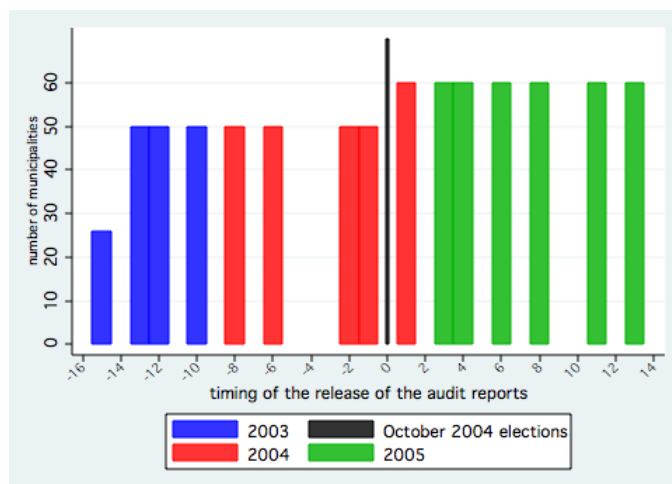
In 2003 an anti-corruption program was launched by the central government. Since then, municipalities have been randomly chosen, by lottery, to be audited on a monthly basis. Auditors examine the allocation of federal transfers at the local level. Members of the government, the media, and the public society are present during the lottery. *Corregedoria Geral da União - CGU* is the body that conducts the audits. Auditors collect documents and information from the period 2001 to the present, for each of the municipalities selected. A few months after the audit event, audit reports are sent to all levels of governments and are also made available on the CGU Web site. Each report contains information about the total amount of federal transfers audited. Most importantly, the audit report contains a list that describes the full details of irregularities. The federal transfers tied to specific projects or public works are examined for irregularities, such as diversion of funds, non-competitive bidding in procurement contracts, lack of completeness, or non-utilization of the funds.

Between the years 2003 and 2004, 50 municipalities in each lottery were randomly selected to be audited. After October 2004, 60 municipalities were selected in each lottery. To date, the number of municipalities audited is over 1,500. In every audit process, information in reference to all federal funds transferred to the municipal government from 2001 onward is collected.

This study considers 784 municipalities randomly selected through the first fifteen lotteries. The bad administration or corruption occurrences reported related to the municipal administration that had been in power during the period from 2001 to 2004. Figure 3 provides information about the number of municipalities by timing of the release of the audit reports. The horizontal axis denotes the distance in time (number of months) from the release of the audit reports to the municipal elections. The vertical axis denotes the number of municipalities whose audit reports had been released. In the first lottery 26 municipalities were selected. These municipalities had their audit reports released 15 months prior to the municipal elections. The black vertical line represents the timing of the elections. There were four lotteries (176 municipalities) in 2003; five lotteries (260 municipalities) in 2004; and six lotteries (360 municipalities) in 2005. Audit reports for a total of 376 municipalities had the audit reports released before the October 2004 elections (before tiny black line), and reports for 410 municipalities were released after the elections.

¹⁵For the years 2003 and 2006, I assumed the same information for years 2004 and 2005, respectively.

Figure 3: Timing of the release of the audit reports and number of municipalities selected



3.2.2 Measuring Corruption using the Audit Reports

According to Ferraz and Finan (2008), the occurrences described in the audit reports are mainly divided into corruption violations and poor administration. Illegal procurement practices, diversion of funds, over-invoicing of goods and services, and fraud are the most common irregularities reported. These irregularities regard the municipal administration mandate from 2001 to 2004.

These corruption irregularities are defined as follows: illegal procurement practices occur when 1) a required procurement procedure is not executed; 2) the minimum number of bids is not attained; or 3) when there is evidence that competition has been limited, for example, when businesses of a mayor's family or friends received non-public information related to the value of the project. In cases of diversion of funds, mayors diverted funds originally intended for social programs to public or private goods, such as purchases of computers, printers, motorcycles, cars, fuel, or the payment of associates salaries. There were also many cases in which expenses were not proven. Over-invoicing occurs when there is evidence that public goods or services are bought for a value above the market price. Many occurrences of fraud were also discovered. In most cases, they were related to an illegal procurement processes or documents that had been falsified to prove municipal expenses.

Summary statistics for municipal characteristics and corruption variables are reported in Table 1. Panel A presents the descriptive statistics of the number of corruption violations reported by year of the release of the audit reports. Note that the percent of reported violations slightly increases over time. Auditors check the allocation of federal transfers from 2001 up to the time of the audit. We might expect that the auditors would find more violations over time as mayors also had more time to commit irregularities. That is exactly what is revealed in the data. Ferraz and Finan (2008) tested for the possibility of mayors who were politically affiliated with either the federal or state governments receiving more favorable audit reports. They did not find any evidence of that. They also show that there are no statistically significant differences between the sample of municipalities audit before and after the 2004 elections. Of the 784 municipalities reviewed in this study, 79% reported at least one violation, and 30% reported at least three violations. Only 3.5% of the sample presents more than five irregularities associated

with corruption.

In Panel B, columns 1 and 2 present the mean of municipal characteristics according to the level of corruption reported. Column 1 considers means of municipalities with few corruption violations reported (from 0 to 2 corruption violations reported); column 2 presents the means of municipal characteristics where at least three corruption violations were reported; column 3 presents the differences between the means and column 4 presents the standard error of the differences. The difference between means reported in columns 1 and 2 is positive and significant for the literacy rate, the log of per-capita income, the percentage of persons living in urban areas, the percentage of houses linked to the general water system, and the percentage of houses linked to the general sewer system.

4 Punishment by The Central Government – Reduction in Transfers

The first part of this section analyze the effect of the release of the audit reports on infrastructure transfers and starts in Section 4.1 that explains the estimation strategy used. Section 4.2 presents the results and some checks regarding this part of the analysis. Additional validity checks are presented in section 4.2.1 which exploits three different stages of the federal budget execution process in order to check that the reduction in transfers is due to punishment by the federal executive branch. Section 4.2.2 analyzes the effect of the audit reports on other municipal accounts.

4.1 The Effects of Released Audit Reports on Transfers: Estimation Strategy

Ideally, in order to identify how corruption disclosure affects the allocation of federal transfers, all municipalities should have been audited at the same time, and only for a random sample of municipalities, the information would be released to the CG. Unfortunately, this experiment is not reliable. However, the random allocation of the audits, the exogenous variation in the timing of the release of the audit reports and the Brazilian institutional scheme allow us to identify these effects. With the exogenous timing of the release of the audit reports it is possible to compare the flow of transfers before and after the release of the audit reports.

By considering the federal transfers received by these municipalities during the period 1999-2006, I first investigated whether the amount of infrastructure transfers from the central government to these municipalities decreased after the release of the audit reports. The effects of the release of the audit reports on the amount of transfers received by the municipalities are estimated as:

$$y_{it} = \beta_1 A_{it} * H_i + \beta_2 A_{it} * F_i + \beta_3 A_{it} * M_i + \delta W_{it} + \tau_t + \eta_i + e_{it},$$

where the left-hand side of the equation is the log of the total per-capita amount of infrastructure transfers received by the municipality i in the year t . The variable A_{it} denotes the timing of the release of the audit reports and is equal to 1 in the year of the audit event and in subsequent years. Note that A_{it} varies not only across municipalities and within the years after the beginning of the anti-corruption program (2003, 2004, and 2005), but also across years within municipalities. This variable interacts with dummies that denote the level of corruption reported. H_i is equal to 1, if no violations are reported; F_i is equal to 1, if one or two violations are reported; and

M_i is equal to 1 if more than two violations are reported¹⁶ Note that these variables are time invariant and they refer to the level of corruption of the municipal administration during the 2001-2004 mandate. The time-varying control W_{it} is equal to 1 if a mayor is affiliated with the political party of the president: PT from 2003 onwards and PSDB before it¹⁷. Year fixed-effects τ_t and municipal fixed-effect η_i , are also included. The coefficients of interest are β_1 , β_2 and β_3 ; they measure the effect of the audit reports on the amount of infrastructure transfers for the three different levels of corruption reported. Under the assumption that $E(e_{it}|A_{it}, \eta_i) = 0$, $E(e_{it}|(A_{it} * H_i), \eta_i) = 0$, $E(e_{it}|(A_{it} * F_i), \eta_i) = 0$ and $E(e_{it}|(A_{it} * M_i), \eta_i) = 0$, the fixed effect estimator is consistently estimated.

4.2 The Effects of the Release of the Audit Reports on Transfers: Results and Robustness Checks

I started this analysis by considering an unbalanced Panel with data from 779 audited municipalities and eight years of observations to show that after the release of the audit reports, transfers decreased as the level of reported corruption rose. The results are presented in Table 2. Regressions displayed in columns 1, 2, 3, 4, 5 and 6 consider all audited municipalities. Columns 7, 8, 9 and 10 report the results when all municipalities in the anti-corruption program are included, regardless of whether they were audited¹⁸. The regressions displayed in columns 1, 2, 3, 4, 7 and 8 include the period 1999-2006. In order to verify whether the reduction in transfers started before the elections and after the release of the audit reports during the mandate 2001-2004, regressions displayed in columns 4, 5, 6 and 7 include only the 2001-2004.

Column 1 displays the results of the model specified above when the interaction terms are not considered. The amount of infrastructure transfers decreased significantly, by 25.2% ($e^\beta - 1$), after the release of the audit reports. Since 79% of these municipalities have at least one corruption violation reported, the number of violations reported should matter. Column 2 presents a surprising result. Municipalities in which the mayor is affiliated with the president's political party were compensated after the release of the audit reports. The results of the regressions reported in the remaining columns consider a semi-parametric specification with three different levels of corruption reported that are interacted with A_{it} : *no violations* (= 1 if no violation is reported); *few violations* (= 1 if 1 or 2 violations are reported); *many violations* (= 1 if at least three violations are reported). In column 3 (audited sample), the effects of the release of the audit reports increases in magnitude as the degree of corruption rises. After the audit event, for municipalities with many corruption violations reported, transfers decrease by 59.4% ($e^\beta - 1$) (standard error 0.122)¹⁹. Note that in column 4, the coefficients of the interactions terms between $A_{it} * H_i * President's\ party_{it}$ and $A_{it} * F_i * President's\ party_{it}$ are not statistically different.

The political selections induced by the release of the audit reports (audits increase turnover, resulting in more first-term mayors) could drive the results if first-term mayors, elected in October 2004, were disadvantaged in terms of obtaining federally transferred resources during their term of office. However, the results reported in Table 2 show that this does not seem to be the case. Municipalities with disclosed corruption information had their transfers reduced after the audit

¹⁶For those municipalities audited more than one time, I have considered only the first audit. Only 12 municipalities were audited more than one time and had more than one audit report.

¹⁷Note that there is a great variation in W_{it} also because Brazilian's mayors can change political party affiliation during their mandate.

¹⁸Brazil has more than 5,500 municipalities. During the years before 2007, only 8% of them are not included in the anti-corruption program (Brazilian municipalities with more than 450,000 inhabitants). This study considers all audited municipalities in the first 15 lotteries. Their audit reports were released until the end of 2005

¹⁹The results are also maintained if the lag in infrastructure transfers is considered on the right-hand side of the equation (GMM estimator). These results are available upon request.

release and before the municipal elections. The results in columns 5 and 6 (only the period 2001-2004 is considered) are similar to those reported in columns 3 and 4. These results indicate that the reduction in transfers after the release of the audit reports started before 2005.

However, this analysis would be compromised if unobservable time-varying variables are correlated with the timing of the audit and the number of violations reported. President Lula's first administration started in 2003 (the year when the Brazilian anti-corruption program was launched). It is possible that since the beginning of Lula's administration, the amount of transfers to municipalities affiliated with the party in opposition to Lula has decreased, as compared to before he took office. Opposition parties had also been part of the governing coalition during the two consecutive previous presidential terms (Fernando Henrique Cardoso, "FHC", 1994-2002). Because corruption is likely correlated with transfers, it could also be associated with mayor's political party affiliation.

Taking these possibilities into account, regressions reported in columns 7, 8, 9 and 10 include all municipalities that participated in the anti-corruption program (with fewer than 450,000 inhabitants). Note that audited municipalities (whether found to have no, few or many corruption violations reported, are treated differently from those non-audited. After the release of the audit reports, one of the interaction terms $Audit_{it} * no_violations_i$, $Audit_{it} * few_violations_i$, or $Audit_{it} * many_violations_i$ will be equal to 1, while the interaction terms always equal 0 for non-audited municipalities. In this specification, the number of observations rises from 5,327 to 37,775 when the entire period (1999-2006) is considered. Having many corruption violations reported reduces the amount of infrastructure transfers, on average, by 26% (point estimate 0.232 and standard error 0.094). In comparison with the results of regressions that consider only the audit sample, the point estimates are lower. Therefore the fact that the change in the governing party at the federal level might be correlated with the timing of the release of the audit reports may bias the estimates when only the audit sample is considered. However, the results remain robust.

The results in column 8 suggest that mayors affiliated with the political party of the president, for whom no corruption violation have been reported, are compensated with relatively more transfers after the release of the audit reports. Note that the estimate coefficient of the interaction term $A_{it} * M_i * President's\ party_{it}$ is still huge in magnitude and not statistically different from the coefficient of the interaction term $A_{it} * H_i * President's\ party_{it}$. Regressions in columns 9 and 10 consider all municipalities (audited and non-audited) but only the period 2001-2004, which does not include previews and subsequent municipal administration. Note that the estimate coefficient of $Audit_{it} * many_violations_i$ is now bigger in magnitude with respect to the regressions that consider the entire period. Additionally, in columns 10, the difference in the estimate coefficients for the interaction terms $A_{it} * F_i * President's\ party_{it}$ and $A_{it} * H_i * President's\ party_{it}$ are statistically different from zero according the Wald test (p-value 0.007). This result suggests that before the elections in 2004, the central government "compensated" municipalities where both no violations were reported and the mayor was affiliated with the party of the president. Moreover, it seems that after the municipal elections, municipalities politically aligned with the president freed themselves of the punishment phase faster than others.

Additionally, in order to verify whether the demand for transfers decreased following corruption disclosures I employed data regarding three different phases of the federal budget process: approval, authorization and payment. There is no evidence that reduction in transfers is driven by a reduction in the amount applied by municipal administrators. The results are shown in Section 4.2.1 (Table 4) and also in Section 5.1 (Table 6).

Subsequently I proceed with a falsification test. We should not expect any significant effect of corruption disclosure in the years before the audit release. In order to check that the reduction in transfers occurred only in the years after the release of the audit reports in municipalities where corruption was revealed, I considered two different samples of audited municipalities, separately:

those with no corruption violations reported and those with at least two violations. Since the period of analysis is 1999-2006 and the corruption data comes from audit reports release in 2003, 2004, and 2005, I specify six dummy variables: *will be audited in 3 years*; *will be audited in 2 years*; *will be audited in 1 year*; *audited this year*; *audited 1 year ago*; and, *audited 2 year ago* and *audited 3 years ago*.. Year fixed effect and municipality fixed effects are also included in these regressions. This specification also allows us to check for the trend of transfers before and after the audit reports. Note that the comparison group are the dummies: *will be audited in 6 years*; *will be audited in 5 years*; *audited in 4 years*.

These results are reported in Table 3. The release of the audit reports does not affect municipalities where the mayors are revealed to be honest, neither after nor before the release of the audit reports. On the other hand, for municipalities with more than one violation reported, the release of the audit reports had a negative and statistically significant impact on the amount of infrastructure transfers. The effects last at least 3 years after the release of the audit reports. Note that for both samples of municipalities there is a negative trend in the amount of transfers during the years before the release of the audit reports with respect to the comparison group (will be audited in 4, 5, or 6 years). However, this trend seems to be reversed for municipalities with no violations reported after the release of the audit reports. This is not the case for municipalities with at least two corruption violations reported. I also perform a Wald test to confirm that the estimate coefficient of *audited this year* is statistically different from those of *will be audited in 2 years* and *will be audited in 1 year*. These estimates are statistically different for both tests (p-values are 0.006 and 0.012, respectively). Note also that in order to proceed with this test, only subsamples of audit municipalities could be considered. As shown above, the estimates of the effect of the audit release on transfers might be biased upwards.

4.2.1 Checking for Punishment by the Executive Branch

As explained in Section 3.1.1, these discretionary transfers, in most cases, are related to the execution of federal budgetary amendments. Therefore, by exploiting different stages of the accomplishment of the budget process, we can proceed with some additional checks. First, it would be interesting to see whether legislators and the president are punishing corrupt local politicians. In this case, we must examine in which phase of the budget process punishment had occurred.

There are three values for each budgetary amendment that correspond to different stages in the process of the execution of a budgetary amendment. Based on these values, three different variables are considered in this analysis: APP_{it} is the log of the per-capita value of the budgetary law initially approved by the president in the Brazilian annual budget for the municipality i and year t . AUT_{it} is the log of the per-capita value of the budgetary law amendment later authorized by the legislative branch for the municipality i and year t . $PAID_{it}$ is the log of the per-capita value of the budgetary law amendment executed (paid) by the executive branch for the municipality i and year t . If punishment comes from the executive branch, we expect that the value of budgetary amendments transferred to municipalities with corrupt mayors is reduced, on average, after the released corruption. Conversely, if the legislative branch is promoting the punishment, the value of the authorized budgetary amendment should be reduced. Secondly, with this dataset it is also possible to verify whether the reduction in transfers is demand-driven; local mayors could reduce their efforts in terms of applying for discretionary federal funds after the release of the audit reports. In this case, the news of corruption should exert an effect on the value approved (first step in the budget execution process).

Using a semi-parametric specification similar to the one described in Section 4.1, I verify the effect of the reported corruption on the three variables APP_{it} , AUT_{it} and $PAID_{it}$. Then, I

estimate the effects of the release of the audit reports on the budgetary amendment execution process. The right-hand side variables considered are the interactions between A_{it} and the dummy variables denoting the number of violations reported.

Table 4 shows the results when $Audit_{it}$ equals 1 in the same month and year as the release of the audit reports, and in the months thereafter, and also in subsequent years²⁰. For instance, if a budgetary amendment for a given municipality was executed (paid) in the same year and one month before the release of the audit report, $Audit_{it}$ equals 0. The regressions displayed in columns 1, 2 and 3 consider only the sample of audited municipalities. The results of the regressions that consider all municipalities are reported in columns 4, 5, 6, 7, 8, 9 and 10. The dependent variable in the regressions reported in Columns 1 and 4 is APP_{it} ; in columns 2 and 5 it is AUT_{it} ; and in columns 3, 6, 7, 8, 9 and 10 it is $PAID_{it}$. The results of regressions that consider a semi-parametric model are displayed in the first six columns. Columns 7 and 9 report the results of a linear model and columns 8 and 10 consider a quadratic specification. Columns 9 and 10 do not include municipalities with more than five corruption violations reported.

The results shown in columns 3 and 6 suggest that the log of the value of the budgetary amendment executed (paid) is significantly reduced to municipalities with two or three corruption violations reported (estimate points -0.262 and -0.160; respectively) after the release of the audit reports. Note that the results reported in columns 1, 2, 4 and 5 do not capture the timing of the bill approval or authorization. This data brings information of the month of payment (execution) of the budgetary amendments. However, there is no information available regarding the month in which the approval and authorization of each amendment took place. According to the description of the budget process in Section 4.2.1, before being executed (paid), the amendment has to be approved by the executive branch and subsequently authorized by the legislative branch.

Taking account of this, I also tried a different specification in order to check for these possibilities. In this case, the variable $Audit1_{it}$ is considered. This variable equals 1 only in the years subsequent to the release of the audit report. This specification allows us to check whether corrupt local mayors reduce the degree of effort that they exert in applying for transfers after the release of the audit reports. There is no effect, for any level of corruption reported, on the per-capita amount of the budgetary amendment approved and further authorized by the legislative branch²¹. However, the coefficient of the interaction term $Audit1_{it} * n. \text{ of violation} = 0$ is significantly and positive when $PAID_{it}$ is the dependent variable. When all municipalities are considered, the interaction term $Audit1_{it} * n. \text{ of violation} = 2 \text{ or } 3$ has a statistically significant detrimental effect on the value paid. The results are available upon request. Additionally, the results reported in columns 7, 8, 9 and 10 suggest that the quadratic specification fits the data well. This result is an important check for the quadratic specification chosen in Section 5.1.

4.2.2 Checking for Effects on Other Municipal Accounts

Reduction in transfers *per se* does not guarantee a reduction in the amount of public goods delivered. In order to assure that the release of the audit reports had a negative effect on the realization of public goods I consider other municipal accounts as outcomes. Basically, the audit reports should have no effects on federal grants (FPM transfers) because they follow an allocation mechanism that is defined by the constitution. According to the rule, the central government cannot exert any discretionary judgments regarding constitutional transfers that are related to the delivery of education and health services. Additionally, I verified that the release of the audit

²⁰When an amendment is executed in installments during the same year, the first month of payment is considered.

²¹Section 4.1—when all municipalities, audited and non-audited, are included in the regressions—and Section 5.1 also provides additional evidence that this reduction in transfers does not seem to demand-driven.

report and level of corruption had no effects on municipal deficit and revenues from local taxes (crowding-out effect).

On the other hand, if reduction in transfers is translated into a lower amount of public goods delivered, the amount of infrastructure expenditures should have been reduced after the release of the audit reports. The results are presented on Table 5.

Columns 1, 2, 3, and 4 displays the results for the audit sample of municipalities while regressions in columns 5, 6, 7, and 8, consider all municipalities. The dependent variable in columns 1, and 4, is the log of the per-capita amount of constitutional transfers; in columns 2 and 6 it is the budget surplus (the difference between municipal revenues and municipal expenditures); in columns 3 and 7 it is the log of per-capita revenues with local taxes. And most important, columns 4 and 8 consider the log per-capita of infrastructure expenditures. Actually, corruption disclosure has a significantly negative effect only in the amount of infrastructure expenditures when more than two (many) corruption violations are reported.

5 Disentangling The Channels

This part of the analysis aims to disentangle the possible channels that drive the electoral punishment. Corruption, transfers, and electoral outcomes might all be correlated. Therefore, disentangling the effects of transfers and corruption on electoral outcomes is not a trivial task. However, by exploiting some particularities of the Brazilian institutional environment and the timing of transfers, it is possible to address this issue.

I begin by checking for the timing in the central government punishment (Section 5.1). Subsequently, I discuss the estimation strategy to identify the mechanisms through which the release of the audit reports affects the municipal electoral outcomes (Section 5.2). Then the results and heterogeneity effects are presented in Section 5.3. Finally, I show the checks for the linear specification model that examines the average effect of released corruption on the electoral outcomes with additional heterogeneity results (Section 5.4).

5.1 Checking for the Timing in Punishment by the Central Government (Bureaucratic Snags)

Based on data involving budgetary amendments, I present some evidence that a degree of bureaucracy is involved in punishment meted out by the central government. That is, a reduction in transfers does not occur in the subsequent month just after the release of the audit reports. Understanding this is extremely important to the analysis. This will help us to infer in which municipalities voters had time to perceive the consequences of the reduction in transfers before the municipal elections. Note that most budgetary amendments are executed at the end of each fiscal year (in December). If there are bureaucratic snags in the central government punishment process, voters in municipalities with audit reports released in the last months of the fiscal year would not have time to suffer the consequences of reductions in transfers, in the year in question. Municipalities with an audit reported released in the last months of year 2003 had their transfers reduced only in December of 2004, after the October 2004 municipal elections. This is also the case for municipalities with audit reports released in 2004. Therefore, only voters in municipalities with disclosed corruption, who had their transfers reduced in 2003, had time to perceive the consequences of the reduction in transfers before the municipal elections.

This check simply compares the effects of the corruption disclosure on the value of the budgetary amendment in different phases of the budget process in a given municipality in two different samples of audited municipalities. The first sample considers all municipalities with

corruption released in a given year. The second consider only municipalities with audits released not after July. If there are bureaucratic snags in the process of reducing in transfers, the results for the second sample should be stronger.

Please note that this check requires cross-section data. However, by taking differences between the per-capita values of the budgetary law amendment authorized (AUT) and executed (PAID), it is possible to control for any unobservable municipal characteristics that are constant within the time between the authorization and payment process. Then, a quadratic specification of the number of violations reported, justified by the results of the semi-parametric specification reported in Section 4.2.1 - Table 4, is employed. Municipal characteristics, mayoral political party affiliation, and state dummies are included.

Table 6 reports the results. Panels A and B show the results for municipalities with audit reports released from January to October 2005 and from January to July 2005, respectively. I used data for the year 2005 because there were few observations for individual budgetary amendments for the years 2003 and 2004. But there is no reason to think that this could compromise the validity of the results.

Note that the coefficients of the variable that denote the number of corruption violations reported are positive, but not statistically significant in all regressions that consider APP_i (column 1) and AUT_i (columns 2) as dependent variable. However, the results in column 3 of Panel B indicate that the value paid decreases as the number of violations rises. Therefore the difference between AUT and PAID should increase. The effect of the coefficient of the difference between the per-capita values of the budgetary law amendment authorized and executed is positive and statistically significant at 5%, and it is stronger in Panel B.

These results suggest that municipalities with corruption information released close in time to the end of the Brazilian fiscal year (December) did not have their transfers reduced in that year. The results displayed in column 4 are also an additional check for the argument that the reduction in transfers to unveiled corrupt mayors might be demand-driven. Following this, voters in municipalities with audit reports released in December 2003 (10 months before the municipal elections), October 2003 (12 months before the municipal elections), and September 2003 (13 months before the municipal elections) had their transfers reduced only in December 2004, after the municipal elections in October 2004. Therefore, only voters in municipalities with audit reports released at least 15 months prior to elections could have had time to perceive the consequences of reductions in transfers²².

5.2 The Effect of Corruption Disclosure on Electoral Outcomes, by Timing of Release: Estimation Strategy

Our first interest relies in estimating the causal effect of the treatment “having corruption information released”, conditioning on the fact that there is corruption, on the re-election outcomes. Thinking in terms of potential outcome, define $Y_i(1)$ as the probability of re-election of an incumbent in a municipality i , in case the municipal administration would have been the release of the audit reports before the elections (treatment). Similarly, define $Y_i(0)$ as the probability of re-election of this incumbent, in the same municipality i if there would have been no disclosure of corruption (control).

Unfortunately, this experiment is not feasible. However, the random allocation of the audit events and the exogenous variation of the release of the audit reports allow us to compare municipalities with audit reports released before and after the elections. Municipalities with audit reports released before the elections are the treatment group and those after, the control

²²Note that, as illustrated in Figure 3, I am mentioning the timing of the release of the audit reports exactly as they occurred.

group. This simple comparison would give us the average effect of the disclosure of corruption on the probability of re-election of an incumbent politician ($E[Y_i(1) - Y_i(0)]$). On the other hand, this specification would not provide any information about the dynamics in the timing of the release of the audit reports.

In order to understand the channels that drive this effect, we exploit the dynamic treatment effects, conditional on the timing of the release of the audit reports before the elections. Define j as the distance in time (number of months) from the release of the audit reports to the elections such as $j = \{-15, -13, -12, -10, -8, -6, -2, 0, +1\}$; and $+1$ is equivalent to ≥ 1 . For instance, if the release of the audit reports occurred 15 months prior to the elections $j = -15$. Therefore if $j \leq 0$, the release of the audit report occurred before the municipal elections; and if $j > 0$, it occurred after that.

The variable A_{ij} defines the treatment status for a municipality i and time j : $A_{ij} = 1$ if $j \leq 0$, and $A_{ij} = 0$ otherwise. The observed outcome is thus: $Y_{ij} = A_{ij} \cdot Y_{ij}(1) + (1 - A_{ij}) \cdot Y_{ij}(0)$. The estimand of interest, the average treatment effect $E[Y_{ij}(1) - Y_{ij}(0)]$, is denoted as τ_j . By allowing for variation of the treatment effects across timing of release, i.e. these dynamic treatment effects, we can check how the effects of the dissemination of corruption information among voters (if any) varies across time of release. Most importantly, it is possible to disentangle the channels through which voters are punishing corrupt politicians at the polls.

Refreshing some important points, we have discussed in previous sections that transfers are decided on and paid by the central government at the same time, and this occurs mostly (Figure 2) at the end of the Brazilian fiscal year (in December). Note that transfers to municipalities with audit reports released in 2004 were mostly decided and paid in December. Municipal elections were held in October 2004. Additionally, from the results reported in Table 6 there might be some bureaucracy in the punishment by the central government. Therefore only voters in municipalities with corruption released at least 15 months prior to the elections (October 2004) should have suffered the consequences of a reduction in transfers before municipal elections.

The above dynamic treatment effects, conditional on the exogenous timing of the release of the audit reports, on the probability of an incumbent party (or mayor) being re-elected τ_j is specified as:

$$\tau_j = \begin{cases} \beta^{|j|}\gamma + \delta & \text{if } j = -15 \\ \beta^{|j|}\gamma & \text{if } -15 \leq j \leq 0 \\ 0 & \text{if } j > 0 \end{cases}$$

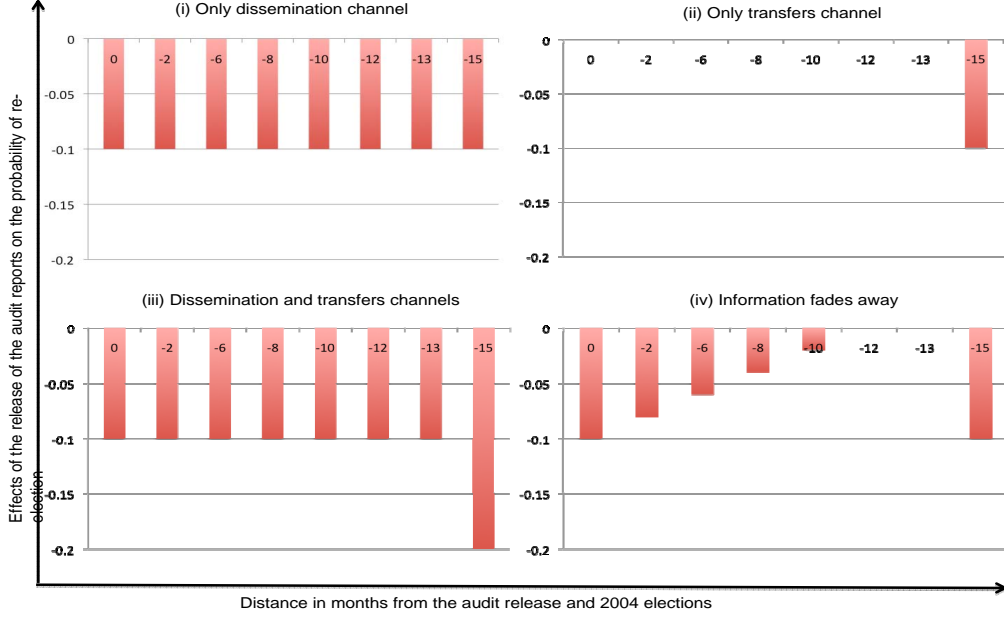
where δ is the component of the effect that comes from the reduction in transfers channel; γ is the component of the effect that comes from the loss of reputation channel due to the dissemination of corruption information among voters; and β is an information fading-away factor.

The estimations under this framework, allow us to test for the following hypothesis: (i) $H_0 : \delta > 0$ and $\gamma = 0$; if reject, only information channel matters; (ii) $H_0 : \delta = 0$ and $\gamma > 0$; if reject, only reduction in transfers channel matters; (iii) $H_0 : \delta = 0$ and $\gamma = 0$; if reject, both reduction in transfers channel and information channel matter; (iv) $H_0 : \beta \geq 1$, $H_0 : \delta = 0$ and $\gamma = 0$; if reject, both reduction in transfers channel and information channel matter, but information channel fades away with time.

Figure 4 illustrates these four assumptions where the vertical axes represent the effects of the release of the audit reports on the probability of re-election and the horizontal axes represent the distance in time (months) from the release of the audits to the October 2004 municipal elections.

The model is specified below. Municipalities in which the release of the audit reports occurred before the 2004 municipal elections are the treatment group. The control group is composed of municipalities in which the disclosure of corruption information occurred after the election. In order to test how the effects of the audit reports vary according to the timing of the release, the specification below includes dummy variables that denote the distance in time (number of

Figure 4: Assumptions to be tested



months) from the release of the audit reports to the municipal elections for those municipalities with audit released before the elections.

Define W_i as a set of relevant town-specific covariates and z_s states intercept. In the OLS estimation

$$R_i = \alpha + \sum_j \tau(D_{ji} * C_i) + \sum_j \omega D_{ji} + \varphi C_i + \chi' W_i + z_s + e_i,$$

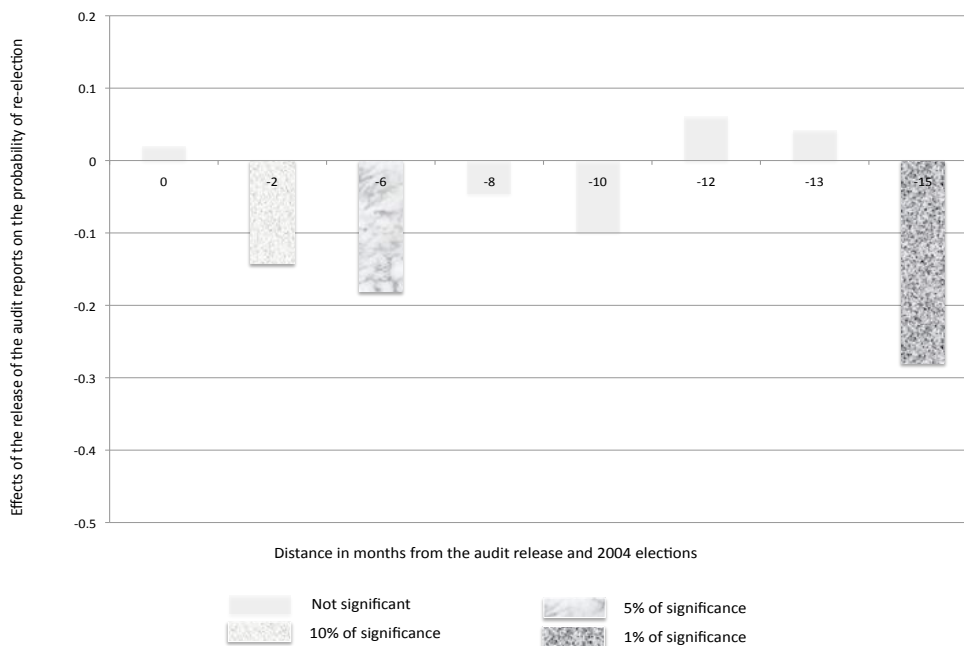
$$j = \{-15, -13, -12, -10, -8, -6, -2, 0, +1\},$$

where R_i is equal to 1, if the eligible incumbent political party (or mayor) in the municipality i was re-elected during the 2004 municipal elections. The variable C_i is the number of corruption violations reported in a municipality i . Section 5.4 tests this linear specification for corruption. Dummy variables D_{ji} denote the "distance" in time (number of months) from the release of the audit report to the municipal elections in the municipality i . For instance, $D_{-15,i}$ is equal to 1 if the release of the audit reports occurred 15 months before the elections. Note that these dummies encompass all municipalities with audit reports released before municipal elections in October 2004. Then τ represents the effect of the release of the audit reports of every additional corruption violation reported on an incumbent's probability of re-election, according to the timing of the release. Under the assumption that $E(\sum_j D_{ji} | e_i) = 0$, the coefficients are consistently estimated.

5.3 The Effect of Corruption Disclosure on Electoral Outcomes, by Timing of Release: Results and Heterogeneity Effects

Considering the dummies that denote the distance in time from the release of the audit reports to the municipal elections, the results of this linear model for corruption, and semi-parametric model of the timing of the audit release are illustrated in Figure 5 and reported in 7.

Figure 5: The Effect of Corruption Disclosure on Electoral Outcomes, by Timing of Release



Note: This regression is reported in Column 2 of Table 7

The dependent variable is the probability of re-election of a political party. The results suggest that revealed corruption has a negative effect on a political party's probability of re-election in municipalities with audit reports released close in time to the October 2004 municipal elections. However, this effect seems to fade with time. For every additional corruption violation reported, the incumbent political party's probability of re-election decreases significantly, by 14% and 18%, respectively in municipalities with audit reports released two and six months prior to the municipal elections in October 2004 of the baseline re-election rate for the control municipalities. The point estimate of the interaction terms C^*8 months to elections, C^*10 months to elections, C^*12 months to elections, and C^*13 months to elections are: -0.046 (standard error 0.066), -0.094 (standard error 0.103), 0.069 (standard error 0.100) and 0.014 (standard error 0.099) respectively. The point estimate of the interaction terms C^*10 months to elections and C^*8 months to elections are still negative but not statistically significant and, the coefficient of the interaction terms C^*12 months to elections and C^*13 months to elections are positive.

However, the release of the audit reports seems to have a stronger significant effect when it occurs 15 months prior to the elections²³. In those municipalities, every additional corruption violation reported decreases the probability of re-election of the incumbent political party by 28% (standard error 0.097). The point estimates for *C*15 m. to elections* and *C*13 m. to elections*, as well as those for *C*15 m. to elections* and *C*12 m. to elections* are statistically different at the 0.0161 and 0.047 significance level (p-value), respectively. Additionally, $H_0: C*13 m. to elections = C*6 m. to elections$ and $H_0: C*12 m. to elections = C*6 m. to elections$ are rejected (p-values 0.047 and 0.050, respectively).

In Table 7, the dependent variable in the regression displayed in columns 1, 2, and 3 is the probability of re-election of a political party. The regression in column 1 considers all audited municipalities in which the incumbent mayor is eligible for re-election. Columns 2 and 3 do not consider outliers ($c > 5$). The results are very similar if only municipalities with more than seven reported corruption violations (less than 1% of the sample) are excluded from the analysis. All regressions reported in Table 7 include municipal and mayoral controls²⁴.

When the dependent variable is the incumbent mayors' probability of re-elections (columns 4, 5, and 6), the results are similar. Note that the variable C is negative and not significant in almost all regressions (except in column 1). Brazilian mayors can run for state or federal elections during or after their first term. Taking this fact into account, I also consider the probability of re-election of a political party (including the party electoral coalition) as a dependent variable. In this specification, R_i is not necessarily equal to 0 in cases in which a mayor was eligible for re-election, but did not stand for election. Considering only municipalities in which the mayors are eligible for re-election, this variable is equal to 1, if the incumbent political party or its electoral political party's coalition was elected in the 2004 municipal elections. Note that only Brazilian municipalities in which mayors are eligible for re-election are considered²⁵. In 2000, for the first time, Brazilian municipal mayors were allowed to run for two consecutive terms. Therefore, in 2004, 40% of the audited sample was not eligible to run for re-election.

Note that local radio station is an important source of dissemination of information at the local level in Brazil where almost 40% of the population above the age 20 is illiterate. According to Ferraz and Finan (2008) the average effect of the release of the audit reports are stronger in municipalities with local radio stations, suggesting that the local radio is the vehicle for the dissemination of corruption information. Therefore, considering only the sample of municipalities where local radio station is not present we should expect no effects in municipalities with audit reports released close in time to the elections.

Figure 6 illustrates additional evidence that ensures that the channels are actually disentangled. Actually, in municipalities with no radio station, all the negative effect of the corruption disclosure on the probability of re-election of an incumbent political party is driven by those municipalities with audit reports released at least 15 months prior to the elections.

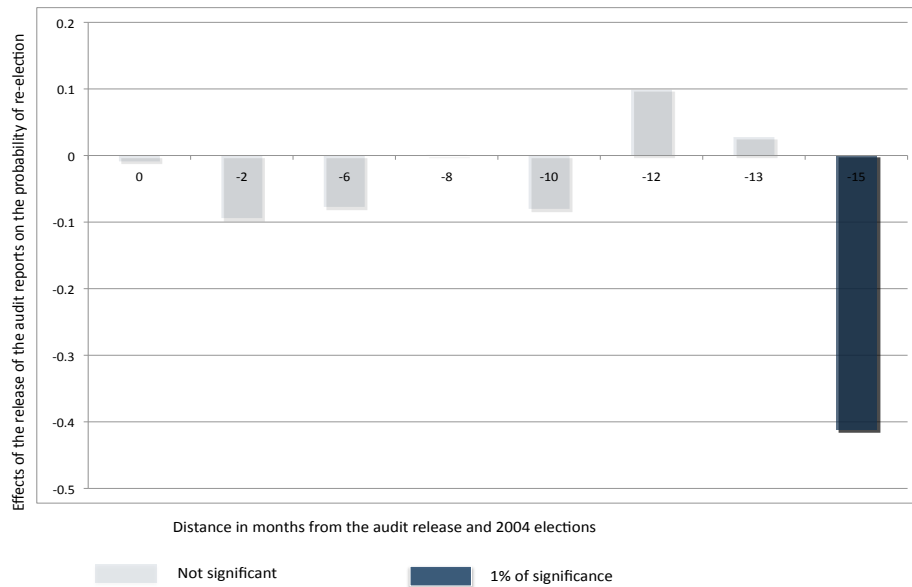
These results suggest that this effect is due to the reduction in transfers. Note that the point estimate of the variable *C*15 m. to elections* is statistically different from all other interaction terms. These results remain even if we do not consider municipal controls in the regressions and (or) the dependent variable is the probability of re-election of an incumbent mayor (these results

²³In the audited sample, with eligible mayors with fewer than six corruption violations reported, there are 14 municipalities with audit reports released 15 months before the elections.

²⁴The results are similar if municipal and mayoral controls are not considered. Note that 12 municipalities each have more than one audit report. This is due to the fact that they were audited more than one time. I am considering only the first audit. Note that, for these municipalities, the second release of the audit reports occurred after the municipal elections.

²⁵Taken into account only the sample of municipalities where the mayor is non-eligible for re-elections (two terms limit, in Brazil) I tested whether this audit reports have an effect only the probability of re-election of the incumbent political party and I found no evidence.

Figure 6: The Effect of Corruption Disclosure on Electoral Outcomes, by Timing of Release—Only in Municipalities with No Radio Station



are available upon request). The results suggest that when the effects of the dissemination of corruption information among voters had completely faded, voters punished corrupt politicians as a consequence of the reduction in transfers.

Column 3 of Table 7 provides additional evidence that this substantial effect of the estimate coefficient $C^{*15 \text{ months to the elections}}$ is due to the reduction in transfers, after the release of the audit reports. The interaction between the dummies $D_{ji} \# \text{ of months prior to elections}$ and the variable T , which denotes the average amount of transfers after the audit release, is also included in the regression. If transfers matter, this interaction term should capture the effects of the interaction between the dummies $D_{ji} \# \text{ of months prior to elections}$ and the number of corruption violations reported. According to the results displayed in column 3, the coefficient of $C^{*15 \text{ months to elections}}$ is no longer significant. The point estimate of $C^{*15 \text{ months to elections}}$ is -0.157 (standard error 0.097). However, the point estimate of $C^{*6 \text{ months to the elections}}$ is significant at 1% level (point estimate -0.187; standard error 0.068). Moreover, the interaction term $15 \text{ months to the elections} * T$ has a positive and significant effect on the probability of re-election (point estimate 0.018; standard error 0.008)²⁶. The results presented in columns 4, 5, and 6, where the dependent variable is the probability of re-election of an incumbent political

²⁶mean of T for municipalities with the release of the audit reports 15 months prior to the elections is 9.020 and s.d. 10.521. The mean of the outcome variable is 0.44. Therefore, a 10% increase in transfers after the release of the audit reports is associated with an increase of 39% in the probability of re-election

party (a noisier measure) is considered, are weaker but go in the same direction. Although not reported, the results are in the same direction when the total sample of audited municipalities (where the mayor is eligible for re-election) and a quadratic specification are considered.

These results suggest that the "loss of reputation on the part of corrupt politicians" channel works when the release of the audit reports occurs close to the municipal elections (it fades after six months prior to the elections). However, information fades with time and when voters have forgotten about the information contained in the audit reports but suffer the consequences of the reduction in transfers, the probability of incumbents being re-elected also decreases with the number of violations reported (the interaction term $T*15\text{ months to elections}$ captures the effect of $C*15\text{ months to elections}$).

5.4 Testing the Specification of the Linear Model for Corruption and Additional Heterogeneity Results

I first test the specification of the model that better fits the data collected. Then I also analyze the possibility of heterogeneous effects, according to mayors' political party affiliations.

The exogenous variation in the timing of the release of the audit reports allow us to consistently estimate the effect of corruption disclosures on the probability of re-election of an incumbent mayor. The average effect of the release of the audit reports on the electoral outcomes is estimated as following:

$$R_i = \alpha + \omega A_i + \tau(A_i * C_i) + \delta C_i + \chi' W_i + z_s + e_i,$$

the left-hand side variable R_i denotes the probability of re-elections and it is equal to 1 if the eligible incumbent mayor in the municipality i is re-elected during the 2004 municipal elections. $Audit_i$ is the treatment variable and equals 1 when the release of the audit reports occurred before the elections, zero otherwise. The variable C_i is the number of corruption violations reported in municipality i . Then, the coefficient β represents the effect of the release of the audit reports of every additional corruption violation reported on a mayor's probability of re-election. W_i is a vector of mayoral and municipal characteristics, z_s denotes state fixed effects, and e_i is the error term. Under the assumption that $E(A_i|e_i) = 0$, the coefficients are consistently estimated.

Tables 8 and 9 display the OLS estimates when the probability of re-election of the incumbent mayor and incumbent political party is the dependent variable, respectively. Table 8, columns 1, 3, and 5 consider a linear specification. The results of a quadratic specification are displayed in columns 2 and 4. Column 6 presents the results for a semi-parametric model. Columns 3, 4 and 5 do not include municipalities with more than five corruption violations reported (less than 4% of the sample). All regressions include state dummies, and municipal and mayoral characteristics. When all audit municipalities are considered, according to the results for the linear model reported in column 1, the release of the audit reports has no effects on the electoral outcomes. On the other hand, with the quadratic specification reported in column 2, the point estimate of the interaction term $Audit_i * C_i$ is -0.220 (p-value 0.106). However, when excluding municipalities with more than five corruption violations (column 3), the point estimate of the interaction term $Audit_i * C_i$ is statistically significant (estimate point -0.108, standard error 0.058) in the linear specification. On the other hand, when excluding these observations, the results for the quadratic specification are lost (column 4). Column 6 shows the results of a semi-parametric model. The estimate point $Audit_i * C_i = 3$ is -0.210 (standard error 0.125), is statistically significant at 10%. These results suggest that the quadratic specification simply captures some noise in the data.

A surprising result is presented in column 5, in which $Audit_i * C$ is interacted with PT, a dummy variable which is equal to 1, if a mayor is affiliated with the governing political party. For every additional corruption violation reported the probability of re-election of an incumbent mayor affiliated with PT significantly increases by 23% of the baseline re-election rate for the control municipalities (-22%). Note that corruption is not randomly allocated and having a mayor affiliated with PT is likely to be correlated with some municipal characteristics. The results suggest that voters do not punish unveiled corrupt mayors affiliated with PT (in the electoral year) when the release of the audit reports occurred before the municipal elections.

Table 9 presents the effects of the audit reports on the incumbent political party's probability of re-election. They are similar to those in Table 8. However, the results in the semi-parametric specification are stronger. The estimate point $Audit_i * C_i=3$ is -0.252 (standard error 0.119), which is statistically significant at 5% (column 7). On the other hand, the coefficient of the interaction term, neither in the linear nor in the quadratic specification, is statistically significant when municipalities with more than five corruption violations reported are not included in the analysis. Although not reported, I also verified whether the release of the audit reports and the number of corruption violations reported had an impact on the probability of re-election of the incumbent political party in the sample of municipalities where mayor is not eligible for re-elections. I found no effect. The reason for considering the probability of re-elections of an incumbent party is that it give us more accurate estimates. Note that in this case R_i is not necessarily equal to 0 in cases in which a mayor was eligible for re-election, but did not stand for election.

For every additional corruption violation reported, the probability of an incumbent political party's re-election significantly decreases by 7.5% when only municipalities with fewer than five corruption violations are considered. Note that the effect of the release of the audit reports on electoral outcomes may present heterogeneous effects across municipalities according to the timing of the release of the audit reports. For instance, if only municipalities with audit reports released close in time to the October 2004 elections drive this result, the estimated average effect is not the most accurate one.

6 Concluding Remarks

This study addresses the role that the central government plays in the Brazilian anti-corruption program in circumstances in which corrupt practices are accompanied by high levels of federally transferred resources. The main idea is that the effect of the loss of reputation of politicians due to the dissemination of corruption information may have a limited effect on improving the average quality of politicians at the local level when political clientelism is present.

In this case, the central government, by reducing the amount of transfers to politicians revealed to be corrupt can trigger punishment by voters at the polls because of the lower amount of transfers received. In the context of the Brazilian anti-corruption program, I found evidence that the central government reduces the amount of infrastructure transfers to municipalities with corrupt politicians when corruption is revealed. However, the results suggest that the CG attempts to minimize political capital losses and maximize political gain by reducing transfers selectively to municipalities according to their party affiliation.

Capitalizing on some particulars of the Brazilian institutional environment and the timing of transfers, we can disentangle the effects of the mayor's loss of reputation and of the reduction in federal transfers. Voters have time to feel the effect of the reduction in transfers before the municipal elections only in municipalities with audit reports released at least 15 months prior to elections. There is evidence that the dissemination of corruption information among voters affects the probability of the re-election of corrupt politicians when the release of the audit reports occurs not more than six months before the municipal elections. Therefore, the loss of reputation channel (because the evidence of corruption is still "fresh" in the voters memories) effect seems to gradually disappear with time. Then voters punish corrupt politicians, even after they forget about the audit reports, when they suffer the consequences of the reduction in transfers.

However, the idea that information fades, as the distance in time from the release of the audit reports to the elections increases, has more than one possible explanation. Mayors may start acting "strategically" (i.e., buying votes) in order to "cancel" the effects of the release of the audit reports before the elections. However, these "strategies" might take time to be implemented (at least six months prior to elections). On the other hand, punishment by the central government could disable corrupt mayors and block them from implementing their strategies because of budget constraints. Moreover, there is evidence in the literature that voters "care" about transfers. Therefore the effects of the dissemination of corruption information on municipal electoral outcomes could be a consequence of voter expectations regarding future transfers. That is, it is not necessarily because of the level of corruption revealed (i.e., not does happen necessarily just because voters get angry when they know how corrupt the politician is). A piece of evidence that this could be the case is presented in Section 4.1. Voters are not punishing corrupt politicians affiliated with Lula's political party at the ballots. On the other hand, the CG increases the amount of transfers to municipalities where the mayor who is affiliated to the party of the president is found to be honest, and it helps politically aligned municipalities with high levels of disclosed corruption to move through the punishment process more quickly. Voters might be anticipating the increase in the amount of transfers in municipalities where the mayor is affiliated with the president's political party, PT. However, there could be factors that could explain this result. Voters may prefer to stick with corrupt mayors affiliated with the PT for other reasons than transfers.

This paper leads to some important policy implications that can be taken into account in countries with high degrees of decentralization of the delivery of public goods and presence of political clientelism. Because voters care about these transfers, punishment by the central government (reduction in transfers) can compromise the welfare of voters. Releasing all audit reports 6 months before the elections, when the evidence of corruption is still fresh in voter's

memory, could be a possible solution. In this case, reduction in transfers are not required to promote political selection at the polls. However, if voters know about corruption information and punish corrupt politicians because they expect a reduction in transfers in the future, the CG must keep saying that transfers will be reduced in case corrupt politicians get re-elected. Otherwise, this anti-corruption program would not improve the quality of local politicians.

Whether dissemination of corruption information has been effective in increasing voters' welfare remains an open question for future research.

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Table 1: Descriptive statistics

	(1)	(2)	(3)	(4)
Panel A: Corruption				
Sample	All audited	audited in 2003	audited in 2004	audited in 2005
Mean of number of violations	1.844 (1.612)	1.486 (1.364)	1.591 (1.528)	2.211 (1.711)
<i>% of municipalities with:</i>				
Zero corruption violations reported	0.21	0.25	0.28	0.15
1 corruption violations reported	0.28	0.34	0.28	0.25
2 corruption violations reported	0.21	0.19	0.20	0.22
3 corruption violations reported	0.16	0.14	0.14	0.19
4 corruption violations reported	0.07	0.06	0.06	0.08
5 corruption violations reported	0.03	0.01	0.02	0.05
More than 5 corruption violations reported	0.04	0.01	0.03	0.05
Total number of municipalities	784	175	259	350
Panel B: Municipal characteristics by corruption category				
	Many violations=0	Many violations=1	Difference	P-value
Literacy rate (%)	82.8	75.9	6.9	0.000
Income (per-capita, R\$)	172	121	51	0.000
Population	24,665	24,282	382	0.900
Persons living in urban areas (%)	61.5	56.1	5.4	0.003
Houses linked to the general system of water (%)	58.7	55.1	3.6	0.083
Houses linked to the general system of sewer (%)	21.9	15.2	6.7	0.002
Houses with electricity (%)	88.0	80.1	7.9	0.000
Transfers (log, per-capita)	2.363	2.418	-0.056	0.544
Re-election rates for the 2004 elections (eligible) (%)	44.2	30.8	13.6	0.045

Notes: Many corruption violations = 1 if at least 3 corruption violations are reported.

Table 2: The effects of release of the audit reports on transfers
 Dependent variable: log of per-capita infrastructure transfers

Sample Period	(1) Audited 1999- 2006	(2) Audited 1999- 2006	(3) Audited 1999- 2006	(4) Audited 1999- 2006	(5) Audited 2001- 2004	(6) Audited 2001- 2004	(7) All 1999- 2006	(8) All 1999- 2006	(9) All 2001- 2004	(10) All 2001- 2004
Audit _{it}	-0.225** (0.088)	-0.242*** (0.089)								
Audit _{it} *Pres. party _{it}		0.518** (0.224)								
Audit _{it} *no_violations _i			-0.118 (0.120)	-0.179 (0.120)	-0.103 (0.155)	-0.128 (0.155)	0.107 (0.098)	0.048 (0.097)	0.009 (0.144)	-0.006 (0.143)
Audit _{it} *few_violations _i			-0.165* (0.100)	-0.161 (0.101)	-0.140 (0.120)	-0.188 (0.122)	0.065 (0.072)	0.069 (0.072)	-0.018 (0.105)	-0.056 (0.107)
Audit _{it} *many_violations _i			-0.466*** (0.122)	-0.493*** (0.124)	-0.505*** (0.184)	-0.492*** (0.187)	-0.232** (0.094)	-0.261*** (0.097)	-0.388** (0.172)	-0.367** (0.175)
Audit _{it} *no_violations _i *Pres. party _{it}				0.998*** (0.355)		0.463 (0.698)		0.806** (0.348)		0.267 (0.685)
Audit _{it} *few_violations _i *Pres. party _{it}				0.123 (0.357)		1.276*** (0.275)		-0.057 (0.343)	1.079*** (0.255)	
Audit _{it} *many_violations _i *Pres. party _{it}				0.629** (0.317)		-0.482 (0.574)		0.458 (0.311)		-0.641 (0.587)
President's party _{it}	-0.000 (0.074)	-0.101 (0.075)	0.001 (0.074)	-0.100 (0.075)	0.003 (0.095)	-0.059 (0.099)	0.112*** (0.028)	0.103*** (0.029)	0.124*** (0.039)	0.118*** (0.039)
Observations	5,327	5,327	5,327	5,327	2,757	2,757	37,775	37,775	19,362	19,362
N. of municipalities	779	779	779	779	768	768	5,490	5,490	5,415	5,415

Notes: Standard errors, clustered by municipalities, are reported in parentheses. * * * (**) [*] denote significance at the P < 0.01, 0.05, and 0.1 levels, respectively. All regressions include municipal fixed effects and year fixed effects. Audit_{it} (1/0) is equal to 1 in the year of release of the audit report and the subsequent years. Panel data (1999-2006). President's party_{it} (1/0) is equal to 1 when the mayor is affiliated with the political party of the president (PT after 2002 and PSDB before 2003). When all municipalities are considered, the interaction terms equal 0 for non-audited municipalities. few_violations = 1 if the municipality has fewer than 3 corruption violations reported. many_violations = 1 if the municipality has at least 3 corruption violations reported.

Table 3: The effects of release of the audit reports on transfers, by timing of release
 Dependent variable: log of per-capita infrastructure transfers

Sample	(1)	(2)	(3)	(4)
	municipalities with no violations		municipalities with at least two violations	
audit in 3 years	-0.040 (0.181)	-0.046 (0.191)	-0.186 (0.129)	-0.215 (0.134)
audit in 2 years	-0.099 (0.299)	-0.092 (0.311)	-0.179 (0.220)	-0.167 (0.227)
audit in 1 year	-0.285 (0.455)	-0.268 (0.467)	-0.323 (0.307)	-0.346 (0.313)
audit this year	-0.280 (0.580)	-0.303 (0.588)	-0.758* (0.396)	-0.770* (0.401)
audit 1 year ago	-0.088 (0.741)	-0.168 (0.749)	-0.868* (0.480)	-0.892* (0.482)
audit 2 years ago	-0.013 (0.897)	-0.229 (0.908)	-1.193** (0.566)	-1.234** (0.566)
audit 3 years ago	0.575 (1.067)	0.263 (1.073)	-1.324** (0.671)	-1.368** (0.672)
audit in 3 years*pres. party		0.016 (0.314)		0.204 (0.215)
audit in 2 years*pres.party		-0.059 (0.348)		-0.087 (0.292)
audit in 1 years*pres. party		-0.545 (0.513)		0.373 (0.336)
audit this year*pres. party		0.247 (0.442)		0.317 (0.574)
audit 1 year ago*pres. party		0.673 (0.660)		0.803* (0.455)
audit 2 years ago*pres. party		1.827*** (0.368)		0.774 (0.564)
audit 3 years ago*pres. party		1.822*** (0.346)		1.401** (0.644)
president party	-0.020 (0.110)	-0.144 (0.190)	-0.060 (0.102)	-0.228* (0.133)
Observations	1,204	1,204	2,598	2,598
Number of municipalities	165	165	392	392

Notes: Standard errors, clustered by municipalities, are reported in parentheses. * * * (**) [*] denote significance at the $P < 0.01$, 0.05 , and 0.1 levels, respectively. All regressions include municipal fixed effects and year fixed effects. $Audit_{it}$ (1/0) is equal to 1 in the year of release of the audit report and the subsequent years. Panel data (1999-2006). President's party $_{it}$ (1/0) is equal to 1 when the mayor is affiliated with the political party of the president (PT after 2002 and PSDB before 2003). The reference group is the dummies audited in 6 years, audit in 5 years, and audit in 4 years.

Table 4: The effects of release of the audit reports on the federal budgetary amendments process

Dependent variable Sample	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)	
	app audited	semi parametric	aut audited	semi parametric	paid audited	semi parametric	app all	semi parametric	aut all	semi parametric	paid all	semi parametric	paid all	linear	quadratic	paid all	linear	quadratic	paid c<6	quadratic
Audit _{it} *n. of violations=0	0.082 (0.137)		0.367* (0.212)		-0.052 (0.141)		0.017 (0.063)		0.064 (0.090)		0.059 (0.069)		0.066 (0.069)		0.060 (0.069)		0.075 (0.096)		-0.065 (0.115)	
Audit _{it} *n. of violations=1	-0.097 (0.113)		0.163 (0.184)		-0.161 (0.138)		-0.060 (0.040)		-0.037 (0.050)		-0.069 (0.063)									
Audit _{it} *n. of violations=2 or 3	0.027 (0.116)		0.255 (0.186)		-0.262** (0.132)		0.046 (0.036)		0.026 (0.055)		-0.160** (0.065)									
Audit _{it} *n. of violations> 3	-0.056 (0.127)		0.251 (0.201)		0.063 (0.181)		-0.083 (0.059)		-0.019 (0.074)		0.056 (0.152)									
Audit _{it} *C _i																				
Audit _{it} *C _i ²																				
President's party	0.037 (0.041)		0.071* (0.041)		0.000 (0.054)		-0.004 (0.013)		0.003 (0.017)		0.078*** (0.025)		0.079*** (0.025)		0.078*** (0.025)		0.010 (0.057)		0.007 (0.057)	
Observations	4,836		4,836		4,836		27,821		27,821		27,821		27,821		27,821		4,661		4,661	
Number of municipalities	692		692		692		4,743		4,743		4,743		4,743		4,743		666		666	

Notes: Standard errors clustered by municipality are reported in parentheses. * * * (**) [*] denote significance at the P < 0.01, 0.05, and 0.1 level, respectively. APP_{it} denotes the log of the per-capita value of the budgetary law initially approved in the Brazilian annual budget to the municipality *i* which were executed in year *t*. AUT_{it} denotes the log of per-capita value of the budgetary amendment later authorized by the Budget Committee to the municipality *i* as executed in year *t*. It could be less than, equal to, or greater than APP_{it}. PAID_{it} denotes the log of per-capita value of the budgetary amendments paid to the municipality *i* in year *t*. Panel data (1997-2006). Audit_{it} = 1 in the same year as the release of the audit reports, the months thereafter, and subsequent years.

Table 5: The effects of release of the audit reports on other municipal accounts

Sample Dependent variable	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	Audited Federal grants	Audited Budget surplus	Audited Local taxes	Audited Infrastructure expenditures	Audited Local taxes	Audited Infrastructure expenditures	Federal grants	All Federal grants	Audited Infrastructure expenditures	All Infrastructure expenditures	Budget surplus	All Budget surplus	Local taxes	All Local taxes	Infrastructure expenditures	All Infrastructure expenditures
Audit _{it} *no_violations _i	0.003 (0.007)	0.024 (0.047)	-0.001 (0.002)	-0.015 (0.049)	-0.001 (0.008)	-0.010 (0.008)	0.010 (0.008)	-0.004 (0.009)	-0.002 (0.002)	-0.002 (0.002)	-0.004 (0.009)	-0.004 (0.009)	-0.002 (0.002)	-0.002 (0.002)	0.006 (0.039)	0.006 (0.039)
Audit _{it} *few_violations _i	0.006 (0.005)	0.065 (0.085)	-0.001 (0.001)	-0.001 (0.041)	0.001 (0.005)	0.001 (0.005)	0.001 (0.005)	0.034 (0.040)	0.001 (0.001)	0.022 (0.030)	0.034 (0.040)	0.034 (0.040)	0.001 (0.001)	0.001 (0.001)	0.022 (0.030)	0.022 (0.030)
Audit _{it} *many_violations _i	-0.001 (0.007)	0.025 (0.052)	-0.001 (0.002)	-0.091* (0.053)	0.006 (0.007)	0.006 (0.007)	0.006 (0.007)	-0.009* (0.005)	-0.001 (0.002)	-0.070* (0.043)	-0.009* (0.005)	-0.009* (0.005)	-0.001 (0.002)	-0.001 (0.002)	-0.070* (0.043)	-0.070* (0.043)
President party	-0.009* (0.005)	0.013 (0.010)	-0.002 (0.001)	0.017 (0.032)	-0.003* (0.002)	-0.003* (0.002)	-0.003* (0.002)	-0.004 (0.004)	-0.001 (0.001)	0.038*** (0.011)	-0.004 (0.004)	-0.004 (0.004)	-0.001 (0.001)	-0.001 (0.001)	0.038*** (0.011)	0.038*** (0.011)
Observations	6,068	5,118	5,709	5,118	42,801	42,801	42,801	36,359	40,658	36,359	36,359	40,658	40,658	40,658	36,359	36,359
Number of municipalities	779	779	779	779	779	779	779	5,489	5,489	5,489	5,489	5,489	5,489	5,489	5,489	5,489

Notes: Standard errors, clustered by municipalities, are reported in parentheses. * * * (**) [*] denote significance at the P < 0.01, 0.05, and 0.1 levels, respectively. All regressions include municipal fixed effects and year fixed effects. Audit_{it} (1/0) is equal to 1 in the year of release of the audit report and the subsequent years. President's party_{it} (1/0) is equal to 1 when the mayor is affiliated with the political party of the president (PT after 2002 and PSDB before 2003). When the log of per-capita revenues from local taxes are considered as dependent variable, regression displayed in Columns 3, and 7, Audit_{it} (1/0) is equal to 1 in the subsequent years because local taxes for t+1 are decided in t. When all municipalities are considered, the interaction terms equal 0 for non-audited municipalities, few violations = 1 if the municipality has fewer than 3 corruption violations reported, many violations = 1 if the municipality has at least 3 corruption violations reported.

Table 6: Checking for the timing of the central government punishment

	(1)	(2)	(3)	(4)
Dependent variable	APP	AUT	PAID	AUT-PAID
Panel A	municipalities with audit reports released between February 2005 and October 2005			
N. of violations	0.089 (0.085)	0.117 (0.088)	-0.083 (0.057)	0.161* (0.093)
(N. of corruption violations) ²	-0.018 (0.013)	-0.015 (0.012)	0.018 (0.012)	-0.023* (0.013)
Observations	255	255	255	255
Panel B	municipalities with audit reports released between February 2005 and June 2005			
N. of violations	0.138 (0.093)	0.173 (0.117)	-0.181** (0.071)	0.260** (0.118)
(N. of corruption violations) ²	-0.033** (0.014)	-0.026 (0.017)	0.036*** (0.014)	-0.041** (0.017)
Observations	190	190	190	190

Notes: Standard errors clustered by municipality are reported in parentheses. Robust standard errors are reported in brackets. *** (**) [*] denote significance at the $P < 0.01, 0.05,$ and 0.1 level, respectively. Only year 2005 is considered. Panel A considers a sample of municipalities with audit reports released between February 2005 and October 2005. Panel B considers a sample of municipalities with audit reports released between February 2005 and June 2005. APP_i denotes the log of the per-capita value of the budgetary law initially approved in the Brazilian annual budget to the municipality i . AUT_i denotes the log of per-capita value of the budgetary amendment later authorized by the Budget Committee to the municipality i . It could be less than, equal to, or greater than APP_i . $PAID_i$ denotes the log of per-capita value of the budgetary amendments paid to the municipality i . It can be only equal to or less than AUT_i . Column 4 reports the results for the log of the difference $AUT-PAID$. The executive branch can only enact expenditures that have been authorized by the legislative branch. However, according to the Brazilian constitution, the executive branch does not necessarily have to execute the expenditures that are authorized by the legislative branch. Municipal characteristics are considered in all regressions, they are: income per-capita, literacy rate, population, %persons living in urban areas. % of houses linked to the general system of water, % of houses linked to the general system of sewer, % of houses with electricity, area. All regressions include dummy variables for mayor political party affiliation in the electoral year (2004), they are: PT PFL PMDB PSDB PDT PTB PP

Table 7: The effects of release of the audit reports on electoral outcomes, by timing of release

Dependent variable	probability of re-election of incumbent political party			probability of re-election of incumbent mayor		
	(1)	(2)	(3)	(4)	(5)	(6)
	all	c<6	c<6	all	c<6	c<6
C *15 m. to elections	0.002 (0.091)	-0.280*** (0.097)	-0.157 (0.097)	0.021 (0.075)	-0.178* (0.105)	-0.155 (0.107)
C *13 m. to elections	0.069 (0.098)	0.042 (0.100)	0.036 (0.100)	0.090 (0.094)	0.066 (0.095)	0.071 (0.098)
C *12 m. to elections	0.063 (0.103)	0.061 (0.108)	0.091 (0.110)	-0.492 (0.452)	-0.497 (0.456)	-0.491 (0.480)
C *10 m. to elections	-0.088 (0.070)	-0.098 (0.073)	-0.104 (0.075)	-0.035 (0.077)	-0.046 (0.079)	-0.054 (0.077)
C *8 m. to elections	0.032 (0.057)	-0.046 (0.066)	-0.039 (0.067)	0.014 (0.065)	-0.076 (0.071)	-0.068 (0.075)
C *6 m. to elections	-0.159** (0.067)	-0.181** (0.070)	-0.187*** (0.068)	-0.130* (0.074)	-0.152* (0.078)	-0.148* (0.081)
C *2 m. to elections	0.052 (0.060)	-0.142* (0.085)	-0.106 (0.091)	-0.024 (0.061)	-0.132 (0.092)	-0.090 (0.097)
C *few days to elections	0.033 (0.076)	0.020 (0.078)	0.020 (0.079)	-0.056 (0.066)	-0.067 (0.066)	-0.071 (0.064)
T*15 m. to elections			0.018** (0.008)			-0.000 (0.013)
T*13 m. to elections			-0.004 (0.018)			-0.001 (0.016)
T*12 m. to elections			0.008 (0.006)			0.001 (0.013)
T*10 m. to elections			0.006 (0.006)			0.007 (0.005)
T*8 m. to elections			-0.005 (0.004)			-0.005 (0.004)
T*6 m. to elections			-0.005 (0.005)			-0.004 (0.005)
T*2 m. to elections			-0.000 (0.002)			-0.001 (0.002)
T*few days to elections			0.003 (0.012)			0.009 (0.012)
C	-0.052** (0.026)	-0.039 (0.030)	-0.041 (0.031)	-0.031 (0.028)	-0.017 (0.032)	-0.019 (0.033)
T			0.001 (0.002)			0.002 (0.001)
Observations	437	426	419	441	429	421

Notes: Robust standard errors are reported in parentheses. * * * (**) [*] denote significance at the $P < 0.01$, 0.05 , and 0.1 levels, respectively. Pre-election audit (1/0) is equal to 1 if the release of the audit reported comes before the 2004 municipal elections. C=number of corruption violations reported. T denotes the amount of infrastructure transfers received by municipality i after the release of the audit reports. All regressions consider only municipalities where the incumbent mayor is eligible to be re-elected. All regressions include municipal and mayoral characteristics, dummies that denote the number of months prior to elections and state dummies. **In column 2, the point estimated for C*15 m. to elections and C*13 m. to elections, as well as those for C*15 m. to elections and C*12 m. to elections are statistically different at the 0.0161 and 0.047 significance level (p-value), respectively. Additionally, $H_0: C*13 \text{ m. to elections} = C*6 \text{ m. to elections}$ and $H_0: C*12 \text{ m. to elections} = C*6 \text{ m. to elections}$ are rejected (p-values 0.047 and 0.050, respectively)..**

Table 8: The effects of release of the audit reports on electoral outcomes by mayoral political affiliation
 Dependent variable: the probability of re-election of incumbent mayor

Specification	linear	quadratic	linear	quadratic	linear	semi parametric
Sample	audited	audited	c<6	c<6	c<6	audited
	(1)	(2)	(3)	(4)	(5)	(6)
$Audit_i$	0.062 (0.136)	0.171 (0.159)	0.139 (0.142)	0.133 (0.171)	0.138 (0.143)	
$Audit_i * C_i$	-0.037 (0.053)	-0.220** (0.106)	-0.108* (0.058)	-0.117 (0.156)	-0.113* (0.059)	
$Audit_i * C_i^2$		0.036** (0.015)		0.004 (0.032)		
$Audit_i * C_i * PT$					0.232** (0.115)	
C_i	-0.039 (0.026)	-0.020 (0.064)	-0.027 (0.031)	-0.068 (0.078)	-0.026 (0.031)	
C_i^2		-0.004 (0.011)		0.010 (0.018)		
$C_i * PT$					-0.216** (0.099)	
$Audit_i * C_i = 0$						0.111 (0.177)
$Audit_i * C_i = 1$						0.052 (0.106)
$Audit_i * C_i = 2$						-0.054 (0.111)
$Audit_i * C_i = 3$						-0.210* (0.125)
$Audit_i * C_i = 4$						-0.047 (0.158)
$C_i = 0$						0.212 (0.139)
$C_i = 1$						0.097 (0.134)
$C_i = 2$						0.061 (0.130)
$C_i = 3$						0.139 (0.122)
PT (governing party)	0.223 (0.152)	0.219 (0.149)	0.252 (0.153)	0.241 (0.148)	0.331* (0.171)	0.214 (0.148)
Observations	441	441	429	429	429	441

Notes: Robust standard errors are reported in parentheses. * * * (**) [*] denote significance at the P < 0.01, 0.05, and 0.1 levels, respectively. Pre-election audit (1/0) is equal to 1 if the release of the audit reported comes before the 2004 municipal elections. C=number of corruption violations reported. All regressions consider only the sample of municipalities where the incumbent mayor is eligible for re-elections. Municipal characteristics are considered in all regressions, they are: income per-capita, literacy rate, population, %persons living in urban areas. % of houses linked to the general system of water, % of houses linked to the general system of sewer, % of houses with electricity, area. All regressions include dummy variables for mayor political party affiliation in the electoral year (2004): PT PFL PMDB PSDB PDT PTB PP

Table 9: The effects of release of the audit reports on electoral outcomes
 Dependent variable: the probability of re-election of incumbent political party

Specification	linear	quadratic	linear	quadratic	linear	linear	semi parametric
Sample	audited (1)	audited (2)	c<6 (3)	c<6 (4)	c<5 (5)	c<5 (6)	audited (7)
$Audit_i$	-0.082 (0.078)	0.030 (0.091)	-0.005 (0.081)	-0.010 (0.096)	0.014 (0.083)	0.015 (0.083)	
$Audit_i * C_i$	0.018 (0.035)	-0.164** (0.078)	-0.054 (0.039)	-0.043 (0.114)	-0.075* (0.042)	-0.079* (0.042)	
$Audit_i * C_i^2$		0.036*** (0.013)		-0.003 (0.029)			
$Audit_i * C_i * PT$						0.230* (0.118)	
C_i	-0.053** (0.025)	-0.011 (0.063)	-0.040 (0.030)	-0.043 (0.076)	-0.033 (0.033)	-0.029 (0.033)	
C_i^2		-0.009 (0.011)		0.001 (0.017)			
$C_i * PT$						-0.242** (0.102)	
$Audit_i * C_i = 0$							-0.077 (0.106)
$Audit_i * C_i = 1$							0.063 (0.095)
$Audit_i * C_i = 2$							-0.123 (0.108)
$Audit_i * C_i = 3$							-0.252** (0.119)
$Audit_i * C_i = 4$							0.082 (0.161)
$C_i = 0$							0.139 (0.118)
$C_i = 1$							0.001 (0.111)
$C_i = 2$							0.044 (0.107)
$C_i = 3$							-0.144 (0.128)
Observations	437	437	426	426	415	415	437

Notes: Robust standard errors are reported in parentheses. * * * (**) [*] denote significance at the $P < 0.01$, 0.05, and 0.1 levels, respectively. Pre-election audit (1/0) is equal to 1 if the release of the audit reported comes before the 2004 municipal elections. C=number of corruption violations reported. All regressions consider only the sample of municipalities where the incumbent mayor is eligible for re-elections. Municipal characteristics are considered in all regressions: income per-capita, literacy rate, population, %persons living in urban areas. % of houses linked to the general system of water, % of houses linked to the general system of sewer, % of houses with electricity, area. All regressions include dummy variables for mayor political party affiliation in the electoral year (2004): PT PFL PMDB PSDB PDT PTB PP