Wp

WORKING PAPERS

ESTIMATING THE NATURE OF POLITICAL CORRUPTION: EVIDENCE FROM A POLICY EXPERIMENT IN BRAZIL

November 2018

No. 2018/19

ESTIMATING THE NATURE OF POLITICAL CORRUPTION: EVIDENCE FROM A POLICY EXPERIMENT IN BRAZIL

Ramos, M.

CAF DEVELOPMENT BANK OF LATIN AMERICA

WORKING PAPERS

ESTIMATING THE NATURE OF POLITICAL CORRUPTION: EVIDENCE FROM A POLICY EXPERIMENT IN BRAZIL

Ramos, M.

CAF – Working paper No. 2018/19
November 2018

ABSTRACT

This paper proposes a test to estimate the nature of political corruption in developing countries: embezzlement by self-enriching politicians versus corruption that originates as a quid-pro-quo from campaign contributions. If politicians decide whether to be corrupt rationally, then increasing the punishment for corrupt practices or the probability of getting caught should reduce corrupt practices (Becker, 1968). If corruption is a response of politicians to firms that finance their campaigns, an increase in punishment should yield not only a reduction in corruption but also a reduction in the demand for projects that are corruptible, such as projects on infrastructure. We test these explanations for corrupt practices using a randomized policy experiment in Brazil. We exploit the fact that some municipalities were randomly chosen to have their probability of being audited increased and we analyze public data of block grants. We find a significant decrease in the resources requested by the mayors to execute projects in infrastructure. This effect is stronger if the municipality has been audited in the past, evidence that mayors respond to credible policies. Moreover, this effect is larger if the mayor's campaign was strongly financed by construction companies. Finally, treated mayors have their performance in subsequent elections worsened and get less financed by construction companies after the experiment happened. In sum, our findings suggest that mayors are committed to campaign contributors and respond to larger probability of audits by reducing the amount of resources requested for infrastructure projects.

ESTIMANDO LA NATURALEZA DE LA CORRUPCIÓN POLÍTICA: EVIDENCIA DE UN EXPERIMENTO DE POLÍTICA EN BRASIL

Ramos, M.

CAF - Documento de trabajo N° 2018/19
Noviembre 2018

RESUMEN

Este artículo presenta un análisis para estimar la naturaleza de la corrupción en los países en desarrollo: malversación por parte de políticos que desean enriquecerse versus corrupción que se origina como un quid-pro-quo de las contribuciones de la campaña. Si los políticos deciden si ser corruptos racionalmente, aumentar el castigo por las prácticas corruptas o la probabilidad de ser atrapado debería reducir las prácticas corruptas (Becker, 1968). Si la corrupción es una respuesta de los políticos a las empresas que financian sus campañas, un aumento en el castigo debería producir no solo una reducción de la corrupción sino también una reducción en la demanda de proyectos que son corruptibles, como los proyectos de infraestructura. Analizamos estas explicaciones alternativas utilizando un experimento de políticas en Brasil. Usamos el hecho de que algunos municipios fueron elegidos al azar para aumentar su probabilidad de ser auditados y analizamos los datos públicos de transferencias del gobierno federal a los municipios. Encontramos una disminución significativa en los recursos solicitados por los alcaldes para ejecutar proyectos en infraestructura. Este efecto es más fuerte si el municipio ha sido auditado en el pasado, evidencia de que los alcaldes responden a políticas creíbles. Además, este efecto es mayor si la campaña del alcalde fue fuertemente financiada por las empresas de construcción en elecciones anteriores. Finalmente, encontramos evidencia de una ruptura en la relación entre los políticos y los donantes de la campaña después del experimento, con una disminución en las tasas de reelección para los alcaldes tratados. En resumen, nuestros hallazgos sugieren que los alcaldes están comprometidos con los contribuyentes de campaña y responden racionalmente a una mayor probabilidad de auditorías contra la corrupción.

ESTIMATING THE NATURE OF POLITICAL CORRUPTION: EVIDENCE FROM A POLICY EXPERIMENT IN BRAZIL

Murilo Ramos[1]

November 2018

ABSTRACT

This paper proposes a test to estimate the nature of political corruption in developing countries: embezzlement by self-enriching politicians versus corruption that originates as a quid-pro-quo from campaign contributions. If politicians decide whether to be corrupt rationally, then increasing the punishment for corrupt practices or the probability of getting caught should reduce corrupt practices (Becker, 1968). If corruption is a response of politicians to firms that finance their campaigns, an increase in punishment should yield not only a reduction in corruption but also a reduction in the demand for projects that are corruptible, such as projects on infrastructure. We test these explanations for corrupt practices using a randomized policy experiment in Brazil. We exploit the fact that some municipalities were randomly chosen to have their probability of being audited increased and we analyze public data of block grants. We find a significant decrease in the resources requested by the mayors to execute projects in infrastructure. This effect is stronger if the municipality has been audited in the past, evidence that mayors respond to credible policies. Moreover, this effect is larger if the mayor's campaign was strongly financed by construction companies. Finally, treated mayors have their performance in subsequent elections worsened and get less financed by construction companies after the experiment happened. In sum, our findings suggest that mayors are committed to campaign contributors and respond to larger probability of audits by reducing the amount of resources requested for infrastructure projects.

[1] University of California, Berkeley, Department of Economics (murilo@berkeley.edu)

1. INTRODUCTION

Understanding the determinants and the consequences of corruption is vital to enhance the quality of public service delivery, especially in developing countries. Although the fact that corruption is extremely harmful is well documented in the literature[2], there is less understanding about the nature of political corruption and the mechanisms behind illicit acts. The literature has already reported some evidence of a *quid pro quo* relationship between politicians and their campaign financiers, where the former receive resources for their campaign and the latter are privileged in securing public contracts.[3]

In this paper, we will attempt to differentiate between two hypotheses: whether political corruption is motivated by the politician's self-enrichment purposes or whether corrupt practices occur because politicians have obligations to the private firms that financed their previous election campaigns. In order to do so, we exploit a policy experiment that randomly increased the probability for municipalities to be selected into a well-established anti-corruption audits' program in Brazil.

This audit program is known as *Programa de Fiscalização por Sorteios Públicos* and is arranged by the *Controladoria Geral da União* (CGU). Since 2003, CGU has maintained a program of supervision of public accounts of small and medium-sized Brazilian municipalities. This program identifies and punishes corrupt mayors with criminal prosecution, since CGU forwards the corruption findings to public prosecutors. Furthermore, even if no conviction takes place, mayors face an additional political cost of the negative publicity in subsequent elections outcomes (Ferraz & Finan 2008). The program is periodic and randomly selects 60 municipalities to receive a group of auditors to inspect their public accounts. Since 2003, CGU has audited approximately 33% of the 5570 Brazilian municipalities at least one time.

In 2009, in order to evaluate the effectiveness of the audits, CGU randomly selected 120 municipalities and increased their probabilities of participating in the program during the period

---

[2] The literature has given considerable attention to the determinants and consequences of corruption. For example, Schleifer et al (1993) argues that corruption causes misallocation of public resources and acts as a tax on firms, distorting their private investments. Moreover, Taylor & Power (2011) report estimations of corruption costs from 1.35% of GDP through 5% of GDP in Brazil.

[3] See, for example, Ackerman (1999), Speck (2010), Fleischer (1997), Gueddes & Neto (1992) and Cordis & Milyo (2013).

of May 2009 through May 2010 from 5% to 25%[4]. CGU sent letters to inform treated mayors about their status before the beginning of the treatment period. Zamboni and Litschig (2015) used this experiment to evaluate the effectiveness of the program and found a decrease of 17 percentage points in findings of corruption in treated municipalities. This is consistent with the predictions of Gary Becker's model about criminal activities (1968), which predicts a decrease in illicit acts with the increase of the probability of punishment.

In order to investigate the two hypotheses discussed above, we analyze the treatment effects of a mayor receiving the warning letter described above on block grants transferred to that mayor's municipality from the central government. In Brazil, there is a process where mayors actively request block grants from the central government. Most block grants originate from this process, suggesting that they are discretionary and might facilitate corruption. One of the more corrupt sectors is public works (Mauro, 1998), so we will use block grants awarded to public works as a proxy measure of corrupt activity. In particular, we will link campaign support by construction and engineering firms to these block grants. If corruption is driven by campaign finance, then mayors that are highly supported construction and engineering firms should respond more to the treatment.

We contribute to the growing literature on political corruption (Olken 2007, 2009; Ferraz & Finan 2008,2011,2016; Niehaus & Sukhtankar 2013; Bobonis et al 2011; Mauro 1998); to the literature of audits and their effects on agents (di Tella et al 2003; Kleven et al 2011; Bar-Ilan & Sacerdote 2001); and finally to the knowledge of the interaction of campaign financing and political corruption (Ackerman 1999, Speck 2010, Fleischer 1997, Gueddes & Neto 1992, Cordis & Milyo 2013).

We find considerable changes in the composition of requests of block grants to the central government using difference-in-differences estimators. More specifically, treated mayors request fewer resources to spend in public works and mayors with considerable support from construction and engineering firms reduce their public works block grants by more than mayors with less support from these firms do. This evidence is consistent with a mechanism of commitment between mayors and their campaign financiers: treated mayors predict that campaign financiers would request to be favored in corrupt bids for providing public works in

[4] These probabilities are calculated in Zamboni and Litschig (2013) considering the average number of audits in a year and the number of municipalities audited in each event.

the local administration. Treated mayors will fear that their corrupt activities might be detected and will seek to reduce their corrupt activities while seeking to maintain their financiers' campaign support. Taking into account that the information on the amount of approved block grants transferred to municipalities is public, treated mayors can exert less effort to obtain discretionary federal resources, and then provide convincing arguments that there are no available resources for public works to their financial supporters.

We also find dynamic treatment effects even when the experiment has expired. There is no evidence of increases in corruption after the treatment expires, as opposed to the findings in Niehaus & Sukhtankar (2012). Furthermore, the evidence of long-term effects of the treatment goes against the prediction of Bobonis et al (2013). In short, our findings suggest that political corruption has complex roots beyond the search of rent extraction by politicians and reflects an intricate mechanism between them and a network of campaign financiers.

Finally, we find evidence that mayors in the treatment group worsen their performance in 2012 polls, mainly because they are less likely to get campaign donations from construction companies after the experiment happened in 2009.

This paper is organized as follows. Section 2 presents a literature review. In section 3 the institutional background is presented. Section 4 describes the main characteristics of our database and the empirical strategy. Section 5 shows the results of our estimations. Section 6 concludes the paper and presents future research agenda.

2. LITERATURE REVIEW

There is a growing literature investigating the relationship between politicians and private campaign financing. Fleischer (1997) gives a historical overview of political corruption in Brazil from 1985 through 1994. He describes a mechanism during president Collor's administration where sizable firms financed his campaign because he was aligned with their interests, and could facilitate their success in future public procurements. Fleischer presents this *quid pro quo* relationship in Brazilian politics by pointing out that PC Farias, the campaign treasurer of Collor, visited a large group of business leaders in order to exchange current campaign help with future privileges in public procurement.

Gueddes & Neto (1992) described this same mechanism in Brazil, also qualifying the political corruption as a *quid pro quo* relationship between politicians and campaign financiers, but also revealing the existence of illicit behavior already in the campaign, when some firms illegally donate resources to hide excess profits from taxation[5]. Ackerman (1999) connects private campaign financing to both a favoritism of campaign donors' preferences in the legislative process concerning concessions and contracts privileging the financiers.

On the other hand, Pereira et al (2008) consider private campaign financing as a mechanism that elites can use to make politicians accountable. As they argue, there are a relatively small number of campaign donors in Brazil, which provide the majority of non-governmental funds to elections. Therefore, these wealthy agents feel empowered to pressure and punish corrupt politicians.

Cordis and Milyo (2013) address the issue of whether different forms of campaign financing should impact a measure of corruption convictions in the U.S. Using cross-states variation in the limits on private contribution and in the presence of public campaign financing, they do not find evidence of different campaign frameworks' effects on their measure of political corruption. The main disadvantage of their approach is the absence of an exogenous variation for the levels and origins of the resources in campaigns.

In order to understand the distortions in the provision of public goods due to corruption, Mauro (1998) finds that this illicit activity distorts government expenditures and decreases the investments in education. He argues that it is easier for politicians to divert resources in public works than in construction because of the usual small number of firms that are able to offer these public works and the ease of strategically dealing with these firms. Moreover, he argues that the average value of each project related to public works is generally greater than average expenditures in other sectors like education. Thus, corrupt politicians find it easier to engage in corrupt activities in public works, since they can steal more resources "in one shot." Additionally, they can negotiate closer with the firms that offer the public goods. We will investigate later if there is evidence of this distortion in public expenditures due to corruption.

In turn, Olken (2007) runs an experiment in road projects in Indonesia and finds that the rise of the probability of government audits in fact induces a decrease in missing expenditures. In the

[5] These are the so called *caixa dois* or off-the-books donations.

literature of corruption, precisely measuring corruption is usually a concern, due to the hidden nature of this illicit activity. Olken addresses this issue by measuring corruption through missing expenditures, measuring them as differences between officially reported costs and technical estimates of the costs. In the last few years, the literature on corruption has made a considerable effort to estimate this activity in an objective way, rather than relying on perceptions (Ferraz & Finan (2008, 2011,2016), Olken (2009)).

In fact, Ferraz & Finan (2008) used objective corruption data from CGU's audits against corruption in Brazil to show that the release of information about political corruption indeed impact the results of the elections, since more informed citizens tend to punish corrupt politicians. In addition, the effect is stronger in places where the presence of radio could intensify the release of the information. Therefore, they report evidences that audits against corruption affect the payoff of politicians, who may change their behavior when faced with events like these.

Zamboni & Litschig (2015) employ the same policy experiment used in this paper to check whether Brazilian mayors respond to an increase in the probability of punishment of corrupt activities. They calculated the control municipalities' probability of being subject of an audit in a period of one year as 5%[6]. Additionally, the 120 treated municipalities could not be drafted in the lotteries from June 2009 through May 2010, but 30 of them would be randomly selected in June of 2010 to participate in the audits program. Therefore, treated municipalities would face a probability of 25% of receiving an audit throughout the period of one year. According to the predictions of Gary Becker's approach to criminal activity (1968), we should expect a decrease in the illicit activities when the probability of punishment increases.

In fact, Zamboni & Litschig report a decrease of 17 percentage points in the findings of corruption for treated municipalities in comparison to municipalities not subject to the treatment. However, they do not investigate which mechanisms operate behind this result, more specifically, whether this reported decrease in political corruption could reflect a complex mechanism of a *quid pro quo* relationship between mayors and campaign financiers instead of only variation in the extraction of rents by the politicians. We will use the same experiment they

[6] In one year, four lotteries normally would occur, on average.

studied to investigate if political corruption could work as a commitment device between politicians and campaign financiers.

Finally, we investigate the existence of dynamic effects of this experiment on politicians' behavior. Niehaus and Sukhtankar (2012) studied that the importance of expected future rents in the decision of rational agents is emphasized. There could be a substitution between periods of the illicit activity, and the agents could compensate, or even overcompensate, for the rent extraction when the treatment ceases. In our context, we consider that even the temporary nature of our experiment could result in dynamic effects. For example, mayors could compensate a possible decrease in corruption in the present with more corruption in the future.

Moreover, Bobonis et al (2013) argues that possibly there would be no effects of audits against corruption in the long-term. Non-corrupt mayors could explore their reputational gain by the audit reports in the initial period by changing their behavior towards being corrupt after they were subject to audits. We will empirically check if there are dynamic treatment effects in the request of block grants in section 5.

3. INSTITUTIONAL BACKGROUND

Since 2003, the *Controladoria Geral da União* (CGU) has been performing a public lottery to randomly select Brazilian municipalities to have their bills checked for findings of corruption. On average, 60 municipalities are drawn of a sample of more than 5400 municipalities with fewer than 500,000 inhabitants, and the frequency of the lottery is (approximately) quarterly.

Ferraz and Finan (2008) have investigated this program and its effect in the probability of reelection of incumbent mayors. They have found a considerable impact of the audits in informing the citizens about the quality of the mayor and a relevant electoral punishment to corrupt politicians.

With these findings in mind, there are two main channels through which the audits impact the payoff of corrupt incumbent mayors. The first one is the electoral punishment, when citizens electorally punish the corrupt mayor after the findings of corruption are revealed by the media and by the opposing candidate. The second channel is the criminal punishment, since CGU sends its reports to public prosecutors, and corrupt mayors can be arrested or lose their rights to run for elections.

Given the considerable costs of corruption we reported in the introduction of this paper, audits against corruption could be an effective way of combating corruption in developing countries with at least a minimum level of Rule of Law and Checks & Balances (Shah 2007). These institutional features are important because they can prevent corrupt agents from committing fraud during the audits without expecting a punishment in a context of a weak institutional framework. In Brazil, the existence of an independent institution to prosecute corrupt public agents and the seriousness of the audits themselves turn the audit program into a powerful tool to combat corruption at the municipality level[7].

Moreover, Olken (2007) argued that top down audits, as the CGU's program presented above, could be more effective than grassroots participation. He measured the corruption in road projects in Indonesia in an objective way to reach this conclusion[8]. Thus, there is evidence in the literature that this audit program by CGU should be an effective tool against corruption in Brazil, due to its top down nature and the institutional frame in which it is inserted.

In May of 2009, six years after the beginning of the program, CGU decided to evaluate its policy by conducting a policy experiment to check for its experimental impact on corrupt activity. More specifically, CGU randomly chose 120 municipalities to have their probability of being audited in the period of one year increased by 20 percentage points and immediately sent letters to the treated mayors to make them aware of their status[9]. This randomization makes the identification of the treatment effect almost trivial with a simple regression. We will explore the panel structure of the database to estimate fixed effects specifications and we will take into account heterogeneous treatment effects, explained below.

In order to check whether the nature of corruption in Brazilian municipalities is directly related to the relationship between mayors and campaign financiers, we decided to investigate treatment effects on block grants received by the mayors from the federal government, which are

[7] This institution is the *Ministério Público* which receives the audit reports from CGU and can prosecute corrupt political agents.

[8] To measure corruption he contracted some engineers to estimate the project values and then compared estimates to the actual reported values.

[9] The *Portaria* describing the policy experiment and the letter they received are in the appendix of this paper.

known as *convênios*[10]. Mayors can actively ask federal ministries for resources to execute public works in many areas, from education to infrastructure projects.

They have access to an online platform that lists available resources, and they just need to send a list of their requested projects to be evaluated by the central government. If the central government considers the request fair enough to receive the resources, the block grants are transferred to the municipalities. Because the municipalities usually do not have enough resources to execute their public projects without federal intervention, block grants represent a relevant fraction of the revenue of the local administrators[11].

Also, this discretionary nature of block grants allows us to estimate the effects of higher probability of being audited on the behavior of treated mayors when asking for federal resources. We will investigate the existence of a complex mechanism behind corrupt activities, analyzing changes in block grants transferred from several ministries, but focusing mainly on the Ministry of Cities, whose resources are generally designated to infrastructure projects and construction of public works. We will also estimate if these changes vary according to the intensity of campaign financing by construction companies and engineering firms, in order to investigate the existence of a mechanism of commitment between corrupt politicians and their campaign financiers.

As emphasized in section 2, Mauro (1998) argues that infrastructure projects and construction works are more corruptible than other public projects such as teacher training or school supply purchases. The smaller competition in the public bidding process between construction companies and engineering firms, together with the fact that public works usually require larger amounts of resources to execute, makes it easier for corrupt politicians to steal resources in block grants designated to these construction projects.

With this fact in mind, we will investigate whether the fall in corruption reported by Zamboni & Litschig (2013) is followed by changes in the composition of block grants from public works towards nonpublic works, especially for treated mayors who were highly financed by construction companies and thus might be more committed to those company interests. One could that the composition of block grants following a fall in corrupt activity should not change,

[10] From now on, we will use the expression "block grants" when we refer to *convênios.*

[11] Municipalities can also receive transfers via Parliamentary Amendments. Block grants and Parliamentary Amendments are generally asked by mayors, while there is also a possibility that a Ministry identifies some needs in a municipality and send resources directed to a specific spent.

because the mayors could temporarily quit their involvement in illicit acts until the treatment period is over, continuing to deliver the same profile of public works as in the period prior to the treatment.

However, if mayors are somehow committed to construction companies that financed their campaigns, than they could decrease their requests of block grants to spend in public works in an attempt to not be involved in corrupt activities when their campaign financiers charge back the favor they carried out for the elected mayors in the polls. This mechanism of commitment is reasonable if some elements are true. First of all, campaign financiers cannot observe the real effort exerted by mayors when asking for block grants. We can argue that this fact is a typical situation of moral hazard since campaign financiers generally are not part of the routine inside the city hall and are not able to perfectly supervise the actions taken by the mayors.

Second, the overall punishment cost should be higher for mayors than for campaign financiers. Otherwise we would expect that the latter would be more worried about the risk of being caught and therefore would not be pressuring mayors for resources during the treatment period. It is understandable that this expectation may actually be the case since mayors directly receive letters informing them about their status, while campaign financiers are possibly not aware of the treatment. Moreover, the costs of being caught for the mayors can go beyond criminal punishment and the loss of future wages in their jobs, since mayors can extract ego rents from being in power. Third, as Mauro (1998) argues, corruption should be concentrated mostly in public works, such as the construction of bridges, popular housing, and roads[12]. Finally, a reasonable hypothesis is that politicians highly financed by these construction companies should be more influenced by them and therefore more committed to paying back the resources the companies invested in their campaign[13].

An alternative mechanism to explain a decrease in the resources transferred to the municipalities to be invested in public works could be the collusion between campaign financiers and political parties. In this context, the increase of the probability of audits could cause a re-

[12] We are not testing directly this hypothesis in this paper, but in the future we will be able to test it with corruption data.

[13] In this framework, the commitment between mayors and politicians would change the composition of the public goods provided when the probability of punishing corruption is increased. In the future we will develop a welfare analysis to check whether corruption can work as “grease in the wheels” and help politicians to overcome costs with bureaucracy to get public works done.

optimization in the network of corruption involving great construction firms and parties. More objectively, they could strategically reallocate block grants to municipalities that are not subject to the treatment, and thus the aggregate corruption would not be less impacted by this policy if this alternative mechanism is true.

With these possible mechanisms in mind, we will investigate if political corruption exists in a *quid pro quo* mechanism, in which elected mayors get involved in corrupt activities mainly to remain in power and to repay their campaign financiers instead of using corruption only as a self enrichment source. Moreover, we will also take into account the prior occurrence of audits in each municipality to analyze the response to the treatment, since the previous contact of the municipality with the CGU's program should change the mayors' perception about the seriousness of the punishment.

We finish this section presenting some anecdotal evidence that Brazilian politicians are committed to their campaign financiers. A recent report from ABC News about the World Cup highlights this relationship. It is said that there is a clear relationship between the campaign donations of big construction companies and their success in being chosen by the public sector to construct the stadiums for the World Cup. A brief excerpt of the report is given in the following lines[14]:

"(...) now, an Associated Press analysis of data from Brazil's top electoral court shows skyrocketing campaign contributions by the very companies involved in the most Cup projects. The lead builder of Brasilia's stadium increased its political donations 500-fold in the most recent election."

Elected politicians may feel obliged to repay favors they received from financiers during their campaign, maybe because they do not want to lose their financial allies for future polls or because they fear the threat of physical harm if they do not repay the favor[15]. The following

[14] https://news.yahoo.com/high-cost-corruption-claims-mar-brazil-world-cup-040235493.html

[15] Even when they cannot get reelected, they could promote a successor or try to continue their political career in another working position.

report of *O Estado de São Paulo* presents evidence that mayors could be physically harmed if they do not repay their campaign financiers[16]:

"(...) with the increase in the transfers from federal government, the mayors got more power, especially in places where the industry and the labor market are not doing well. (...). The mayors are more at risk of death than the president. (...) A new common crime in Piaui, Rondonia and Maranhao is the murder of mayors by campaign financiers. In the polls, the last invest resources in financing campaign of candidates facing the promise of receiving illegal contracts of federal transfers when the politician is elected."

Besides testing this complex mechanism involving campaign financiers to explain the nature of corruption, we test whether there are dynamic treatment effects after the temporary treatment expires. In more detail, we test whether there are persistent treatment effects and whether there may be a substitution of behavior between periods as a way to compensate for the end of the treatment, inspired by Bobonis et. al (2013) and Niehaus (2012).

4. DATA AND EMPIRICAL STRATEGY

The data used in this paper is provided by several sources as *Controladoria Geral da União* (CGU), *Portal da Transparência*, *Censo 2010* from *Instituto Brasileiro de Geografia e Estatística* (IBGE) and *Tribunal Superior Eleitoral* (TSE). All used data is public and available online.

To create the data with the 120 municipalities randomly chosen on May 5th of 2009 into the policy experiment we described in previous section, we accessed the online platform of CGU. Also, on this website is a list of the chosen municipalities for each lottery that has taken place since 2003. With this information we were able to create a dummy variable to indicate the

[16] http://www.estadao.com.br/noticias/nacional,com-mais-verbas-federais-prefeituras-sao-alvo-da-cobica,1084697,0.htm – translated from Portuguese

treatment status and another dummy variable to indicate whether the municipality had been audited by this program before May of 2009[17].

The block grants data was extracted from the Transparency Portal (*Portal da Transparência*) and is known in Brazil as *convênios*. The database includes: i) a description of the expenditure and the project to which the money is designated; ii) the Ministry that is providing the resources; iii) the value of the block grants; iv) dates of duration of the block grants' agreement[18].

The unity of observation in this data is each block grant celebrated between a municipality and the central government. Some municipalities have several block grants in one year and others do not have any block grants in a given year.

We collapsed the original data to construct a new database, in which each entry is a municipality-year observation, from year 2000 through 2012. When a municipality did not receive any block grants in a year, we fill the data with zero entries for the block grants in this given year.

Also, as our treatment begins in mid-May of 2009, whilethe letter that warned the treated mayors about their status probably reached them at the end of this month, we shifted a given year's observational period to start in June instead of January[19].

We created dummies to indicate each ministry that was providing the resources and we categorized the data using the description of the block grants, matching this string variable to dummies to indicate the type of the expenditure to which the block grants were designated[20]. We create several dummies to indicate the total value and number of expenditures related to public works, such as infrastructure, paving, and popular housing[21].

[17] It is important to emphasize that state capitals and municipalities with more than 500,000 habitants are excluded of our sample, because they are excluded from the audit program by CGU. Our sample contains 5515 out of 5570 municipalities in Brazil.

[18] There are few other variables in the database but the most important are listed above.

[19] For example, the 2009 observation for a given municipality in our collapsed data represents the sum of block grants for this municipality from June of 2009 through May of 2010.

[20] We double checked this categorization in Matlab and Excel. Also, we created other dummies for non public works.

[21] All the keywords used to create the variable of public works are: *urbanização, revitalização, saneamento, canalização, construção, reforma, restauração, obra, pavimentação, ponte, infraestrutura, habitação popular* and *drenagem.*

Later, we added up the numbers and values of block grants in the aforementioned types of expenditures to create a single measure of public works, reflecting the block grants designated to construction-related projects. Per capita block grants variables were generated by including the population data from *Censos* of IBGE and between censuses estimations released by IBGE every year.

In table 1 there are summary statistics of block grants and block grants per capita expressed in current *reais* (R$). The first fact common to all variables is that all the distributions are skewed to the right, as all the median values are smaller than the respective mean values. Still, the standard deviation is considerably high in magnitude when compared to mean values, indicating a high dispersion of these variables.

In figure 1 we represented the share of value block grants by selected ministries. The Ministry of Cities, which constitutes our main interest in the estimations, represents almost one quarter of the total value of block grants transferred to the municipalities, a considerable participation, smaller only than the share of Health & Education ministries.

In figure 2 we analyze the types of expenditures within each ministry and also the aggregate value for all ministries, by public and nonpublic works. The main fact in this figure is that public works are very relevant to all ministries, especially to the Ministry of Cities, for which the share of public works almost reaches 90%. This is consistent with the role of this ministry, since this it transfers resources to projects related to infrastructure, popular housing, and urbanization.

Returning to table 1, we see in the second column of panel B that the average amount of resources transferred to the municipalities from Ministry of Cities is R$9.64 per capita, but the median value of the same variable is zero, indicating a considerable number of zero observations in the collapsed sample for this ministry's block grants.

This fact is graphically illustrated in figure 3, which shows that for the Ministry of Agriculture, for example, more than 70% of the municipality-year observations do not have any block grants transferred by this ministry. This fact will be relevant when we set up our empirical strategy, because we will need to take into account that these frequent zero observations would be missed with the logarithm transformation. So, we will adapt the estimation of the semi-elasticity to avoid missing a significant portion of our sample.

We collected socio-demographic data in *Censo 2010* from IBGE and political characteristics from TSE, which includes each mayor's term, political competition, polls results, personal characteristics and campaign finance data[22].

In table 2, we compare the observable variables of treated and control groups. We cannot reject the hypothesis of equality of these observable characteristics between treated and control municipalities, even at 10% significance levels. Therefore, there is no evidence of selection on observables and the randomization must be correctly made by CGU[23].

The specifications we use to test the effect of increasing the probability of audits on the profile of block grants transferred by the central government to Brazilian municipalities are presented next. Our primary focus will be the response of value of block grants per capita to the treatment, since we believe that this measure is the most related to the intensity of public resources directed to the citizens. Almost all the dependent variables considered in this paper will be a variation of block grants per capita, though we will test alternative measures for block grants in the robustness section.

If the mechanism of commitment between mayors and construction companies described in section 3 is correct, we would expect changes in the composition of block grants, more specifically a reduction in block grants requested by municipalities to make public works such as infrastructure projects. As explained earlier, the main mechanism could be the following: as treated mayors tend to get less involved in corrupt activities, they can rationally choose to reduce the amount of resources requested to make public works if they are committed to their campaign financiers such as construction companies. Afraid of getting caught commiting corrupt acts, mayors could conclude that it is necessary not to have available resources from the central government to make public works since, if they have these resources, corrupt campaign financiers would probably charge back the favor of financial support[24]. Moreover, construction companies and engineering firms were the biggest financers in 2008 elections, contributing more than 50% of the total contributions directly to the parties, and they are powerful agents that influence the behavior of politicians in Brazil.

[22] Censo 2010 was extracted from PNUD database, but the primary source is IBGE.
[23] In other words, there is no manipulation of the treatment status by CGU.
[24] We need also to keep in mind the possibility of an alternative mechanism which is the collusion between campaign financiers and parties, which could rearrange the network of corruption and to reallocate their resources to develop corrupt activities in non-treated municipalities.

Keeping in mind this framework and the possibility that the nature of corruption is not simply a self-embezzlement behavior of mayors but rather, reflects a more complex mechanism between them and their campaign financiers, we investigate this mechanism by estimating several specifications which are detailed below. We will start estimating the average treatment effect on block grants by selected ministries and aggregate values of block grants for the municipality level. Once there is randomization of the treatment, the identification of the average treatment effect is straightforward via the following reduced form:

(1) $y_{it} = \beta_1 T_{it} + c_i + \lambda_t + u_{it}$

where y_{it} represents an algebraic transformation of the variable of value of block grants[25]; β_1 is the average treatment effect; T_{it} is a dummy for treated municipalities in year 2009; c_i and λ_t are fixed effects of municipalities and time, respectively; and u_{it} is the unobservable term[26]. We will separate the estimation between audited and non-audited municipalities prior to the receipt of the letter[27], since the change of their perceived probability of punishment can differ with respect to previous contact with CGU's audits.

The second specification aims to estimate heterogeneous treatment effects due to different intensities of campaign financing in 2008 polls, in which mayors were elected to a term beginning in 2009. The reduced form is:

(2) $y_{it} = \beta_1 T_{it} + \beta_2 T_{it} construction_i + \beta_3 \text{year}_{2009} construction_i + c_i + \lambda_t + u_{it}$

where the variables are similar to the first specification, except that we add a triple interaction of the treatment dummy, the time dummy in 2009, and the campaign financing variable, as well as a double interaction of the time dummy in 2009 and the campaign financing variable.

[25] We will generally express the value of block grants per capita z_{it} as $y_{it} = \ln(1 + z_{it})$ in the estimation because there are many observations with $z_{it} = 0$ in our sample (see figure 3).

[26] The unity of observation is the municipality, and the time unit is year. We shifted the beginning of each year to the month June instead of January, since the treated municipalities were aware of their status in mid-May of 2009.

[27] We create a dummy indicating whether the municipality has been audited in the period from 2003 through May of 2009.

Additionally, the campaign financing variable $construction_i$ is a standardized variable of the share of construction companies' donation in the overall amount raised by each mayor's campaign in 2008[28].

We also take into account dynamic treatment effects in our estimations. Our goal when estimating dynamic effects is to test whether there are permanent effects when the treatment ceases and the probability of being audited return to the benchmark level of 5% a year.

To test dynamic effects, we run our third specification:

$$(3)\ y_{it} = \sum_{\mathrm{j=2009}}^{2012} \beta_{1,\mathrm{j}} T_{ij} + c_i + \lambda_t + u_{it}$$

where the parameters $\beta_{1,j}$ represent the treatment effects from 2009 until 2012. The contemporary effect is expressed by the $\beta_{1,2009}$ as in the first specification, but in this specification we allow for dynamic effects from 2010 until 2012.

The next specification takes into account dynamic treatment effects and heterogeneous effects according to the intensity of campaign financing by construction companies and takes the form:

$$(4)\ y_{it} = \sum_{\mathrm{j=2009}}^{2012} \beta_{1,\mathrm{j}} T_{ij} + \sum_{\mathrm{j=2009}}^{2012} \beta_{2,\mathrm{j}} T_{ij} \mathrm{construction_i} + \sum_{\mathrm{j=2009}}^{2012} \beta_{3,\mathrm{j}} \mathrm{year_j} \mathrm{construction_i} + c_i + \lambda_t + u_{it}$$

where the parameters $\beta_{1,j}$ again represent the treatment dynamic effects from 2009 until 2012 and the parameters $\beta_{2,j}$ represent the heterogeneous dynamic effects according to the intensity of campaign financing by construction companies.

5. RESULTS

The main results are presented in this section. There is evidence that mayors in the treatment group change their requests of block grants to the central government, especially when we

[28] The intensity of campaign financing by construction companies is the main heterogeneity, though we tested the intensity of campaign financing by individual person as a robustness check.

consider the ministries from which the mayors request the resources and the type of expenditure to which these resources are directed. The most relevant finding is that treated mayors, who are concerned about the higher probability of being caught in corrupt activities, request fewer block grants to spend in public works, which are generally more corruptible (Mauro, 1998). We also test for treatment heterogeneities, such as the intensity of campaign financing by builder companies, as evidence of the commitment of the elected mayors to their financiers. Additionally, we test whether there are different treatment effects for mayors in different terms. Our main results are robust to several measures of block grants and specifications.

5.1. **Effects of higher audit probability on block grants**

In this subsection we estimate the first specification, in which we focus on treatment effect on different measures of block grants.

First of all, in order to assess pre-trends, figure 4 compares the trends of block grants per capita of the Ministry of Cities and Health & Education Ministries. We can see that the pre-treatment trends are quite similar for the treatment and control groups, but there is a visible break in block grants from the Ministry of Cities for the treated municipalities.

In table 3 column 1 we find a significant treatment effect for Ministry of Cities' block grants of -31.1% (s.d.=0.151) [29]. There is initial evidence that treated mayors are requesting fewer resources of this ministry, without changing their requests to other ministries. As we saw in figure 2, almost 90 percentage points of the block grants from the Ministry of Cities are directed to public works, such as infrastructure projects, popular housing and other constructions and thus there seems to be a reallocation of resources away from expenditures in public works.

In the following tables, mayors from municipalities which had been audited in the period prior to the receipt of the letter respond differently to the treatment. The prior occurrence of audits makes the policy experiment more credible to this subsample of municipalities, since these municipalities have probably confirmed the seriousness of CGU's audits and the political and penal punishment given to corrupt mayors. For this reason, it is worthwhile to separate the data

[29] From now on, we will only report the exact semi-elasticity, calculated using the estimates from the tables. We calculate it as $semi-elasticity = e^{\beta} - 1$

into these two groups to estimate different responses due to the credibility of the policy experiment[30].

In table 4 we present these estimates taking into account possible changes in the composition of block grants by ministries dividing the sample between audited and non-audited municipalities as explained above. In first column of panel A, we estimate a stronger treatment effect for the Ministry of Cities when compared to table 3 (-58.9% s.d.=0.222). In fact, mayors from municipalities which were not audited prior to the treatment do not seem to respond to the treatment. Perhaps when they receive the letter to make them aware of their status, they are not as aware of the real punishment of the audits in comparison to mayors who knew that previous mayors in their municipalities had been audited in the past or who have personally faced a previous audit themselves. This same result can be graphically seen in figure 5, where we compare the trends of block grants per capita for the Cities and Health & Education ministries. For the former ministry there is a clear break in the trend of block grants exclusively for the treated municipalities that had been audited before May of 2009, while we do not see breaks for the last ministries.

In an attempt to further investigate these results, we extensively categorized the block grants variables by the type of expenditure to which they are designated. We divided the value of block grants per capita of each ministry (as well as the aggregate measure) into public works and nonpublic works. This categorization is explained better in the data section, but in few words, we call block grants as public works if keywords such as construction, paving, infrastructure, housing, urbanization, drainage, and so on appear in the description of the object of the block grants in our data.

The motivation behind this categorization is that public works must be more corruptible because the average amount designated to each project is larger than the amount designated to other projects as teacher training or school supply purchases. Moreover, the competition in the bidding process tends to be smaller for public works, since there are usually a few construction companies or engineering companies capable of offering this type of public goods. Thus, less competition in the bidding process and greater amounts involved in block grants directed to

[30] 39 out of 120 municipalities of the treatment group had been audited in the period prior to May of 2009, corresponding to 32.5% of the municipalities in this group. For the control group, 1381 out of 5395 municipalities had been previously audited (25.6%).

public works should facilitate the coordination between a corrupt mayor and corrupt firms to extract rents or exchange favors[31].

In table 5 we consider the type of expenditure in the construction of dependent variables. The odd columns show us the treatment effect on public works and the even columns repeat the estimation for the equivalent nonpublic works variables. The most important findings in this table are in panel A, columns 3 and 4. We can see that treatment effects for Ministry of Cities' block grants for public works account for almost all of the results found in table 5. Though we see a significant estimate on column 4 of table 6, its magnitude is too small compared to what we found for the public works. Perhaps nonpublic works in the Ministry of Cities are directly related to public works in the bidding process, and for this fact they could be related.

So far, we have found evidence that mayors respond to the increase of the probability of being audited against corruption requesting fewer resources to spend in public works, mainly in the Ministry of Cities. Also, there is evidence that treatment affects particularly the municipalities which have been audited in the past. In the next section we provide evidence on the mechanism that links an increase in probability of audits to the choice of block grants by the mayors.

5.2. Treatment effects and commitment with campaign financiers

A possible explanation to the findings in the previous subsection is that once elected, politicians have to pay back those who financed them. It is possible that treated mayors highly financed by construction companies and engineering firms receive fewer resources as a commitment device to not give contracts to the firms to which they are committed[32]. In other words, there may be a commitment between those mayors and these financiers that makes the mechanism of the corrupt activities more complex than a simple rent extraction by mayors. The nature of corruption could be a *quid pro quo* scheme, as a result of a charging of the elected mayors by campaign financiers. To test this hypothesis, in this subsection we estimate the second specification, allowing

[31] These arguments are consistent with Mauro (1998).

[32] Construction companies and engineering firms are the biggest offers in the bidding processes of public works and are also the major contributors to the largest parties in Brazil. For example, PMDB, the biggest party at municipality level, had half of its budget financed by construction companies and engineering companies in 2008, and these donations are concentrated near the polls.

heterogeneous treatment effects according to the intensity of campaign financing by construction companies and engineering firms in the 2008 election.

In table 6 we see a considerable heterogeneous treatment effect for the Ministry of Cities in panel A, column 1. A municipality previously audited in the period prior to May of 2008 and whose mayor was highly financed by construction companies has a treatment effect of -82.9%[33]. Again, there are no treatment effects for the non-audited subsample, providing additional evidence that only mayors whose municipalities had experienced previous audits truly believed in the political and penal punishments and therefore changed their behavior in response to the higher probability of being audited against corruption.

In table 7 we estimate the same specification, but now we categorize the dependent variable according to the type of expenditure in the same way we did in table 5. The main results give credence to treatment effects for the Ministry of Cities in panel A once again. In column 3 the average treatment effect for public works of Ministry of Cities is estimated at -62.2% (s.d. 0.215), and the total effect for a mayor financed by construction companies in one standard deviation above the mean value of this variable is -82.7%. This negative estimate to the triple interaction also happens in column 1 when we consider all the public works for all ministries[34].

Still, we find evidence of substitution between public works and nonpublic works inside the Health and Education ministries, as we can see in columns 7 and 8 in panel A. In figure 2, we showed that for these ministries together there was a balance between public and nonpublic works (46% vs. 54%). For this reason, in table 6 we do not find a significant estimate to the heterogeneous treatment effect, since the substitution between public works and nonpublic works occurs inside these ministries.

In table 8 we take a step forward and disaggregate the block grants according to the type of main public works in the data. Once again, the dependent variable in this table is the logarithm of value of block grants per capita, but now we categorize the dependent variable into six types as

[33] Mayors highly financed by construction companies here have their share of financing by these companies one standard deviation above the mean value of this variable in the sample.

[34] A similar result can be seen in columns 9 and 10 for the category called "Other Ministries". In those ministries, there are many kinds of public works, but the heterogeneity inside them makes the categorization harder. More work is needed in categorization of public works in order to understand the real effects in these ministries.

follows: i) paving; ii) infrastructure; iii) drainage; iv) bridge; v) popular housing; vi) urbanization[35][36].

When we focus on municipalities whose mayors were highly financed by construction companies, all the estimates are negative and highly significant. Therefore, since these types of public works occur in several ministries but with higher intensity in Ministry of Cities, there is evidence that the heterogeneous treatment effects reported in tables 6 and 7 reflect the impact of each public work in the disaggregated data presented table 8.

5.3. Dynamic effects of higher audit probability

In this subsection we test whether our policy experiment, which was temporary and lasted only one year, had longer term effects. Bobonis et al (2013) argued that there may be no long run effects of temporary audit programs since politicians could exploit their reputational gain after an audit when they were not caught in corrupt acts to engage in more corrupt activities in the subsequent periods. We will check whether there are permanent treatment effects on value block grants per capita and whether mayors can compensate, or even overcompensate, in the future for the initial decrease in requests for public works in 2009.

In table 9 we use the third specification in order to estimate dynamic treatment effects from 2010 until 2012. In the first column of panel A, we find evidence of dynamic treatment effects for the Ministry of Cities in 2010 and 2012. There is no evidence of substitution between periods or even of absence of permanent effects for the block grants of this ministry.

In table 10 we estimate the fourth specification taking into account dynamic treatment effects varying with the intensity of campaign financing by construction companies. This is similar to what we have done in previous subsection, but now we consider the possibility of permanent effects when the treatment ceases. Focusing again on panel A column 1, there is evidence of persistent effects for the Ministry of Cities, especially for mayors who were highly financed by construction companies and engineering firms. Furthermore, in year 2010 we find negative

[35] These types are not mutually exclusives. For example, if there is a block grant named in the data as “Infrastructure project in Porto de Galinhas to build a bridge between two neighborhoods of the town”, then both “infrastructure” and “bridge” variables will capture this observation.

[36] Remember the zero observations made us to use log(1+y) instead of log(y).

treatment effects for both Cities and Health & Education ministries. However in years 2011 and 2012 we find a substitution from the Ministry of Cities to the Agriculture Ministry and “Other Ministries”, composed by the Ministries of Tourism, Sports, Science and Social Development.

If the described mechanism of commitment is correct, then the experiment of increasing the probability of punishment of corrupt politicians generates dynamic changes and perhaps is followed by a reduction in corrupt activities even when the treatment is ceased. However, as emphasized in section 3, it could be the case that an alternative mechanism of collusion between great campaign financiers and parties is causing a re-optimization in the corruption network, in a way such that the aggregate corruption is not decreasing due to a reallocation of corrupt activities to other municipalities[37].

Also, a future investigation is also needed to understand whether this variation in the composition of block grants is welfare-enhancing or not. It could be the case that political corruption works as “grease in the wheels” and allows politicians to overcome transaction costs and bureaucracy in order to more efficiently deliver the public goods the population needs[38].

5.4. **Effects of higher audit probability and mayor’s term**

In this subsection we test if there are different treatment effects according to the term in which the mayor is governing. Ferraz & Finan (2011) showed that reelection incentives can shape mayors’ decision regarding corrupt activities and found that mayors in their first term are less corrupt than reelected mayors. Niehaus (2012) provided evidence that agents can consider future rent expectations when deciding whether to engage in corrupt activities.

In our context, mayors could reduce their current corrupt activities to increase the probability of reelection and then increase their corrupt behavior in order to compensate for the initial change in their behavior. To check for a difference in their behavior according to the term, we present tables 11 and 12, where we estimate the first and second specifications, respectively[39]. The main

[37] We would need aggregate data of corruption in order to test it, which is currently not available.
[38] We can evaluate this effect by assessing data of concluded works or even data about socioeconomic indicators of the municipalities.
[39] Actually they were adapted to consider the time-varying nature of the variable of first term.

difference between these is that in the former we do not control for the intensity of campaign financing, while doing soin the latter specification.

In tables 11 and 12 we do not find different treatment effects according to the term in which the mayor is governing. There should be two opposite effects driving this result. Although first term mayor could be more concerned about being caught in corrupt acts and losing the chances of being reelected (and thus responding more to the treatment), they could respond with less intensity to the treatment since they are already less corrupt than second term mayors[40]. Moreover, we find that mayors in first term tend to exert more effort in order to acquire block grants, probably because they want to raise their political capital to get reelected in next polls[41].

5.5. **Effects on campaign financing on the subsequent election**

The goal of this subsection is to investigate whether there were treatment effects over outcomes related to the subsequent election after the treatment was implemented. We restrict our sample to the municipalities in which mayors decided to run for election in this analysis. In the first column of table 13, we find an estimate of -11p.p. for the average treatment effect on reelection rates, but the result is only statistically significant at 10% level test. We find more robust estimates when we take into account a second source of heterogeneity in the treatment effect: an indicator variable on whether the mayors were audited during the term (from 2009 to 2012). The results are stronger for municipalities who were audited after the experiment happened in 2009. In the last column of this table, we restrict our sample to municipalities that were audited in this period and find a high estimate for the heterogeneous treatment affect, around -25p.p.. We find strong evidence that treated mayors highly financed by construction companies receive some punishment in the polls that happened in 2012.

Additionally, table 14 shows that mayors in the treatment group actually run more for reelection, maybe believing that would be rewarded in the polls for not engaging in corruption during their term. To investigate what might have driven the decrease in their performance in 2012 elections, we estimate treatment effects on the campaign donations from construction companies in 2012.

---

[40] Ferraz & Finan (2011)
[41] This effect is not related with our policy experiment, though it could be object of future investigation.

In table 15 we provide some evidence that the treatment might have caused a discontinuation of the relationship between mayors and construction companies. The latter chose to decrease the amount of campaign financing to treated mayors in 2012.

5.6. **Robustness checks**

In this subsection we run several robustness checks in order to evaluate the robustness of the results presented in previous sections. In table 16 we test whether there were differences in value block grants by ministries in the four years before the treatment. Since there was randomization in the allocation of the treatment, we do not expect differences both in trends and in levels of block grants before the treatment. In other words, we would expect to have similar municipalities among the treatment and control groups before the receipt of the letter informing their treatment status. We observe that expectation in table 16, except for some marginal significance for the Agriculture ministry.

In table 17 we use alternative measures of block grants for the Ministry of Cities. In previous tables we have always used the logarithm of value of block grants per capita, to capture the semi-elasticity of the treatment on variables that reflect the intensity of expenditure per citizen. In column 1 we repeat the benchmark dependent variable, which is the logarithm of the value of block grants per capita[42]. In column 2 we consider the logarithm of the value of block grants of the Ministry of Cities. In column 3 we use the logarithm of the number of block grants per capita and the respective non per capita values in column 4.

In columns 5 and 6 we test whether the level of value block grants respond to the treatment, instead of its logarithm. In column 5 we estimated the fixed effect specification and in column 6 we estimated the Tobit model, since our dependent variable in the level is truncated below in zero. As an inspection of table 17 shows, our main treatment effect in Ministry of Cities is robust to several alternative measures of block grants.

In table 18 we check if there are heterogeneous treatment effects on the aggregate block grants data. The heterogeneity considered here is once again the intensity of campaign financing by construction companies and engineering firms. When we disentangle the treatment effect

[42] It is worth remebering that we use log(1+y) instead of log(y) since we have many zero observations in the data.

considering the intensity of commitment to construction companies, the results are similar to those reported in tables 6 and 7: mayors highly financed by construction companies request fewer resources from the federal government when they receive the treatment. This finding is robust to several measures of aggregate block grants: number per capita, value per capita, released value per capita and duration[43].

In tables 19 and 20 we change the variable that measures the intensity of campaign financing by construction companies to a variable measuring the intensity of campaign financing by individuals[44]. The estimated coefficient for the triple interaction is not significant in almost all the estimations[45]. As this coefficient measures the heterogeneous treatment effects, there is evidence that the heterogeneous effects found in tables 6 through 8 are neither a noise nor a spurious effect.

It seems that the changes in requests of block grants due to the treatment are directly related to the intensity of campaign financing by construction companies and that the corruption mechanism must be more complex than a simply rent extraction by the mayors, reflecting a *quid pro quo* relationship between elected mayors and construction companies that have financed them.

There are three main ways of getting resources from the central government via block grants: i) via direct request of the mayor to the respective ministry depending on the destination of the expenditure and on the relative need of the resources; ii) via parliamentary amendments; iii) via direct ministries transfers when they identify a need in the municipality. In our estimations up to now, we do not separate these different ways of getting the resources and we just analyze the value of block grants approved in the official budget to be transferred to the municipalities.

In table 21 we estimate the first specification to check treatment effects on parliamentary amendments and we do not find significant estimates. However, in table 22 when we run the second specification allowing heterogeneous effects, we find a considerable decrease in the parliamentary amendments to Ministry of Cities and for the aggregate measure of parliamentary amendments. This effect is just relevant for the audited mayors prior to the treatment, as we

---

[43] All variables are considered in the form log(1+y).

[44] In Brazil they are called “Pessoas Físicas”.

[45] Except in the Agriculture ministry, which could be caused by some noise or could be deeply investigated in the future.

found in all estimations we did in previous subsections. This is evidence that treated mayors not only change their direct requests to the central government, which comprises the biggest share of the block grants, but also change their requests to the parliamentarian that represents their region in order to obtain parliamentary amendments.

6. CONCLUSION

In this paper, we aim to investigate the mechanisms behind political corruption in Brazil. Corruption seems to be a result of a more complex mechanism rather than a simple self-enrichment behavior by corrupt politicians.

Using a policy experiment in Brazil in which 120 municipalities were randomly chosen to have their probability of being audited against corruption raised by 20 percentage points from a default probability of 5%, we analyze the changes in requests of block grants by mayors to the central government, since the discretionary nature of these transfers allows a response to the treatment by the politicians. We find a considerable decrease in requests to block grants designated to public works, and this effect is stronger if the mayor was highly financed by construction companies, consistent with the described mechanism of commitment between mayors and campaign financiers. Moreover, our findings are restricted to politicians in municipalities which had been audited in a period previous to the treatment. This is consistent with the fact that mayors respond to credible policies, and the prior occurrence of audits in the municipality made them aware of the seriousness of the program and the punishments applied to corrupt politicians.

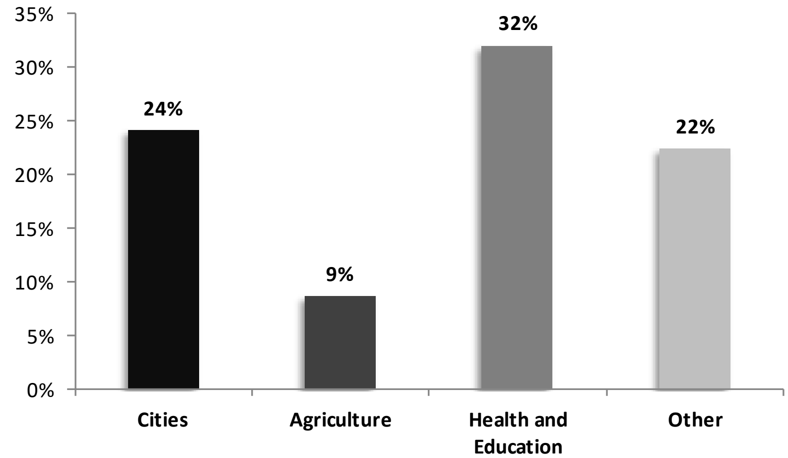
Additionally, we find evidence of dynamic and persistent effects of the policy experiment in our measures of block grants, and we do not find evidence of different treatment effects according to the term in which the mayor is governing. Furthermore, all of our results presented in previous section are robust to several specifications and alternative measures of the dependent variable.

We learned that corrupt politicians may not use corruption just as a self-enrichment tool, but as commitment device in a complex relationship involving their campaign financiers, mostly construction companies and engineering firms. In this context, corruption might be used as an instrument for politicians to raise resources to remain in the power, since they may extract ego rents from maintaining power.

In a future project, we will more thoroughly investigate this mechanism of commitment between politicians and campaign financiers using nonpublic corruption data. This will allow us to assess whether corruption is in fact concentrated in public works and whether politicians that are more corrupt are generally highly financed by construction companies. Additionally, it is worthwhile to investigate if our reported changes in the composition of public expenditure are welfare-enhancing or if political corruption can work as "grease in the wheels" and allow politicians to fulfill more public works. In order to assess these questions, we will need to work on getting data on concluded public works and disaggregated socio-economic indicators for the municipalities.

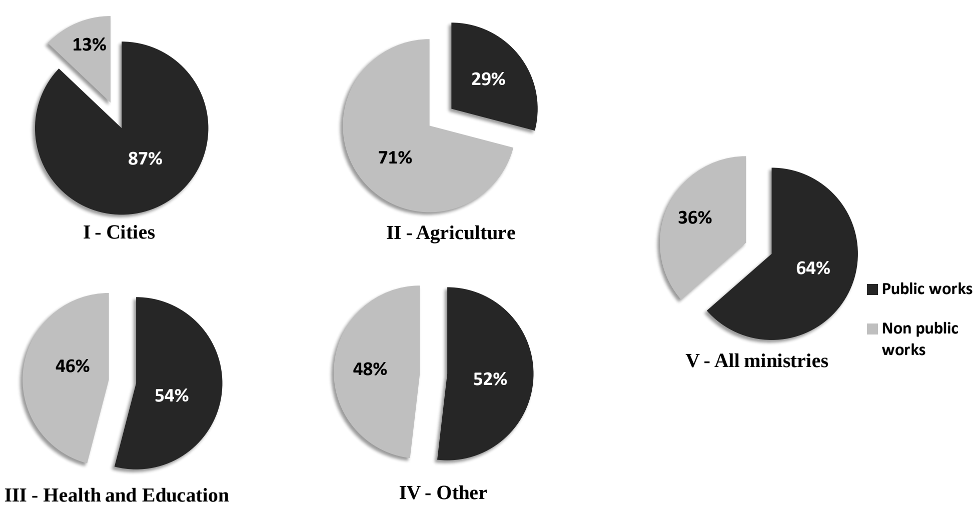
TABLES AND FIGURES

Figure 1 – Share of value block grants by ministry



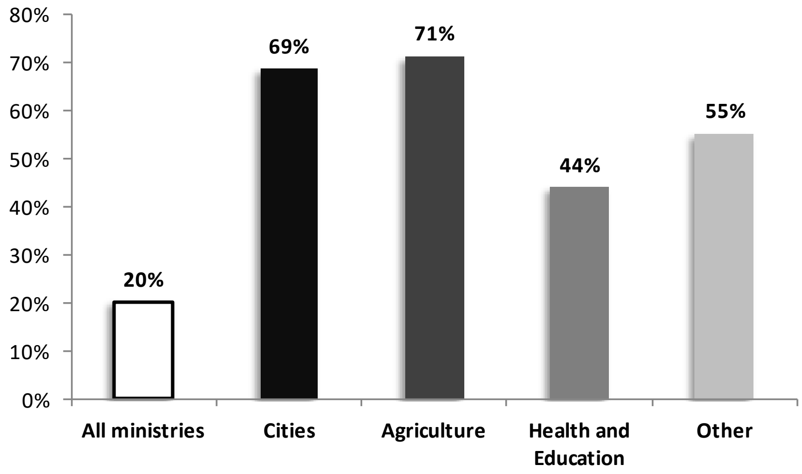
Notes: In this figure we plot the average participation of each ministry in the provision of block grants. The category "Other" includes the following ministries: Science, Social Development, Sports and Tourism. Some residual ministries are not included in category "Other" and for this reason the sum of the shares is smaller than 100%.

Figure 2 – Share of types of expenditure by ministry



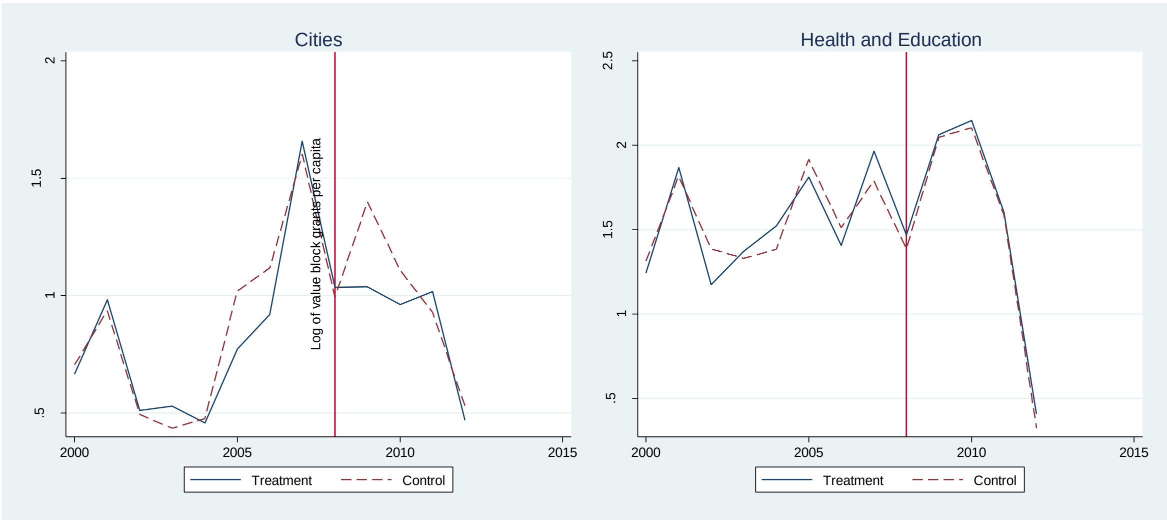
Notes: In this figure we plot the share of public and non public works for the block grants of each selected ministry, as well as for all the ministries together. These values were calculated using our categorization of public works with the block grants data. The category "Other" includes the following ministries: Science, Social Development, Sports and Tourism.

Figure 3 – Share of zero observations by ministry



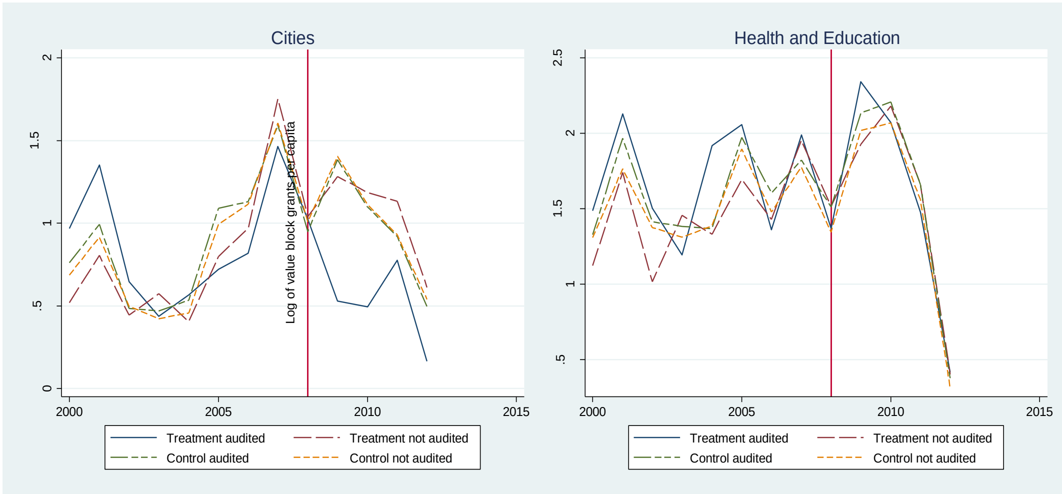
Notes: In this figure we calculated the relative frequency of zero observations in the block grants data by ministry in a municipality-year basis. The high number of zero observations draws our attention when using the logarithm transformation in the dependent variable. We choose to use the transformation log(1+y) in order to maintain these observations in the estimations. The category "Other" includes the following ministries: Science, Social Development, Sports and Tourism.

Figure 4 – Trends of block grants per capita



Notes: In this figure we compare the pre-trends for treatment and control municipalities, for block grants of the Ministry of Cities and of the Health & Education ministries. Data goes from 2000 through 2012. Solid lines represent the treatment group and dashed lines represent the control municipalities.

Figure 5 – Trends of block grants per capita II



Notes: In this figure we compare the pre-trends for treatment and control municipalities, for block grants of the Ministry of Cities and of the Health & Education ministries. Data goes from 2000 through 2012. Solid lines represent the treatment group previously audited and dashed lines represent the control municipalities (audited and non audited) as well as treatment group previously not audited.

Table 1 – Summary statistics of block grants

Panel A: value of block grants					
	Aggregate	Cities	Agriculture	Health and Education	Other
Mean	995,579	240,019	86,642	318,992	223,101
Standard deviation	4,433,784	2,047,702	383,564	2,813,718	986,682
Median	248,694	0	0	36,900	0
Panel B: value of block grants per capita					
	Aggregate	Cities	Agriculture	Health and Education	Other
Mean	57.38	9.64	8.08	20.80	15.42
Standard deviation	127.00	30.88	29.11	66.40	47.03
Median	22.51	0.00	0.00	2.01	0.00

Notes: This table reports summary statistics of block grants for selected ministries. All the values in panel A are in Brazilian currency (R$) and in panel B the values are in *reais per capita* (R$/inhabitants). The category "Other" includes the following ministries: Science, Social Development, Sports and Tourism.

Table 2 – Municipalities and mayor's characteristics

	Control	Treatment	Difference
	Socio-demographic characteristics		
Life expectancy (years)	73.08	72.79	0.28
			[0.27]
Child Mortality (under 5 years old)	19.27	20.09	-0.83
			[0.24]
Years of study at 18 years old (expectation)	9.46	9.41	0.06
			[0.58]
Illiteracy Rate (%)	20.61	21.89	-1.28
			[0.29]
College graduate (%)	5.41	5.08	0.33
			[0.23]
Gini coefficient	0.49	0.50	-0.01
			[0.12]
Per capita income (R$)	489.41	464.68	24.73
			[0.26]
Households with eletric power (%)	97.18	96.62	0.56
			[0.39]
Human Development Index	0.66	0.65	0.01
			[0.18]
Population	23951.31	26276.93	-2325.62
			[0.59]
Radio (%)	0.55	0.59	-0.04
			[0.40]
	Political characteristics		
First term mayor (%)	0.61	0.65	-0.04
			[0.41]
Number of campaign donations	26.67	32.65	-5.98
			[0.44]
Total revenue of campaign donations in 2008 polls (R$)	72873.86	83849.43	-10975.57
			[0.57]
Total revenue of construction companies' donations in 2008 polls (R$)	2517.21	4771.75	-2254.53
			[0.45]
Total revenue of Individuals donations in 2008 polls (R$)	20164.77	21644.07	-1479.30
			[0.67]
Total mayor's candidate in 2008	2.61	2.61	0.01
			[0.95]
Win margin of the elected mayor in 2008 (%)	0.20	0.22	-0.02
			[0.37]
Mayor's Gender (male=1)	0.91	0.94	-0.03
			[0.14]
Mayor's education (years of schooling)	12.81	12.78	0.03
			[0.92]
Mayor with former high occupation (%)	0.37	0.38	-0.01
			[0.78]
Sample Size	5401	120	

Notes: This table reports socio-demographic and political variables of the municipalities, by their status of treatment. The first column reports the mean variables for the control municipalities, the second for the treated municipalities and the third column reports the difference of the variables together with the p-values. The data sources are the Censo 2010 of IBGE and the Repositório de Dados of the Tribunal Superior Eleitoral of Brazil. The treatment group is composed by the 120 municipalities that received the letter and whose mayors were aware of the treatment. The control group is consisted by the remaining 5401 municipalities of Brazil.

Table 3 – Treatment effects on block grants by ministries

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	[2]	[3]	[4]
Treatment	-0.331**	0.102	0.00341	0.156
	(0.151)	(0.110)	(0.176)	(0.154)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	71,681	71,681	71,681	71,681
R^2	0.069	0.020	0.081	0.145
Number of Municipalities	5,518	5,518	5,518	5,518

Notes: This table reports the average treatment effects for selected ministries. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita).

***** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 4 – Treatment effects on block grants by ministries

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	[2]	[3]	[4]
Panel A: audited municipalities				
Treatment	-0.764***	0.134	0.178	-0.0471
	(0.222)	(0.220)	(0.331)	(0.275)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,438	18,438	18,438	18,438
R^2	0.066	0.025	0.087	0.144
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	-0.118	0.0842	-0.0839	0.255
	(0.191)	(0.123)	(0.206)	(0.184)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	53,243	53,243	53,243	53,243
R^2	0.070	0.018	0.079	0.145
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports the average treatment effects for selected ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 5 – Treatment effects on block grants by type of expenditure

	Block grants by type of expenditure						
	All Ministries		Cities		Agriculture		Health Educa
	Public works	Non public works	Public works	Non public works	Public works	Non public works	Public works
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
ities							
	-0.204	-0.0861	-0.747***	-0.0616***	0.0662	0.0212	0.0862
	(0.324)	(0.304)	(0.219)	(0.0224)	(0.115)	(0.218)	(0.330)
	Y	Y	Y	Y	Y	Y	Y
	Y	Y	Y	Y	Y	Y	Y
	18,438	18,438	18,438	18,438	18,438	18,438	18,438
	0.124	0.112	0.064	0.040	0.069	0.027	0.049
	1,419	1,419	1,419	1,419	1,419	1,419	1,419
cipalities							
	-0.0761	0.368**	-0.139	-0.0140	-0.0428	0.118	-0.146
	(0.228)	(0.181)	(0.191)	(0.0425)	(0.0412)	(0.115)	(0.183)
	Y	Y	Y	Y	Y	Y	Y
	Y	Y	Y	Y	Y	Y	Y
	53,243	53,243	53,243	53,243	53,243	53,243	53,243
	0.128	0.104	0.070	0.034	0.055	0.028	0.045
	4,099	4,099	4,099	4,099	4,099	4,099	4,099

Notes: This table reports the average treatment effects for selected ministries. In odd columns we estimate the treatment effect for block grants designated to public works. In even columns we estimate the treatment effect for block grants designated to projects other than public works. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 6 – Treatment effects on block grants by ministries and campaign financiers

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment	-0.847***	0.166	0.330	-0.212
	(0.218)	(0.216)	(0.316)	(0.251)
Treatment*construction_companies	-0.544***	-0.151	0.0514	-1.445***
	(0.119)	(0.118)	(0.171)	(0.141)
construction_companies*year2009	-0.0407	0.0189	-0.0998***	0.0301
	(0.0325)	(0.0292)	(0.0352)	(0.0413)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,326	18,326	18,326	18,326
R^2	0.066	0.025	0.087	0.145
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	-0.130	0.0607	-0.0438	0.311
	(0.196)	(0.123)	(0.214)	(0.190)
Treatment*construction_companies	-0.0706	0.0974	0.0918	-0.0379
	(0.0589)	(0.119)	(0.0625)	(0.0907)
construction_companies*year2009	-0.0354	0.0223	-0.00241	0.0211
	(0.0281)	(0.0211)	(0.0305)	(0.0250)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	52,759	52,759	52,759	52,759
R^2	0.070	0.019	0.079	0.143
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports the treatment effects for selected ministries considering heterogeneous effects by the intensity of campaign financing by construction companies. The variable construction_companies is the standardized variable of the share of the contributions of construction companies and engineering firms over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 7 – Treatment effects on block grants by ministries and campaign financiers (construction companies and engineering firms)

	Block grants by type of expenditure									
	All Ministries		Cities		Agriculture		Health and Education		Other Ministries	
Log value of block grants per capita	Public works [1]	Non public works [2]	Public works [3]	Non public works [4]	Public works [5]	Non public works [6]	Public works [7]	Non public works [8]	Public works [9]	Non public works [10]
Panel A: audited municipalities										
Treatment	-0.414	0.0806	-0.831***	-0.0597***	0.0805	0.0442	0.0376	0.240	-0.197	-0.196
	(0.290)	(0.286)	(0.215)	(0.0227)	(0.113)	(0.216)	(0.321)	(0.278)	(0.256)	(0.244)
Treatment*construction_companies	-2.122***	0.225	-0.553***	0.0220*	0.0142	-0.144	-0.918***	0.555***	-1.274***	-0.379***
	(0.164)	(0.163)	(0.117)	(0.0132)	(0.0596)	(0.118)	(0.173)	(0.156)	(0.147)	(0.145)
construction_companies*year2009	0.00186	-0.0502	-0.0436	0.00383	-0.00576	0.0222	-0.0648**	-0.0558*	0.0759*	-0.0543*
	(0.0455)	(0.0372)	(0.0323)	(0.00952)	(0.00805)	(0.0287)	(0.0303)	(0.0324)	(0.0447)	(0.0327)
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326
R^2	0.125	0.111	0.065	0.041	0.069	0.026	0.049	0.093	0.110	0.088
Number of Municipalities	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419
Panel B: non audited municipalities										
Treatment	-0.0614	0.369*	-0.150	-0.0145	-0.0526	0.105	-0.129	0.0290	0.0484	0.364*
	(0.235)	(0.190)	(0.197)	(0.0452)	(0.0437)	(0.114)	(0.186)	(0.190)	(0.196)	(0.198)
Treatment*construction_companies	0.130*	0.0743*	-0.0614	-0.00434	-0.00278	0.103	0.170**	-0.0250	0.0261	0.0408
	(0.0760)	(0.0452)	(0.0603)	(0.0159)	(0.0151)	(0.117)	(0.0673)	(0.0702)	(0.0832)	(0.0817)
construction_companies*year2009	-0.0226	0.00885	-0.0397	0.000387	0.0161	0.00387	0.00170	0.00875	0.00975	0.00826
	(0.0289)	(0.0242)	(0.0271)	(0.0147)	(0.0141)	(0.0176)	(0.0288)	(0.0279)	(0.0267)	(0.0266)
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759
R^2	0.126	0.103	0.070	0.034	0.055	0.028	0.045	0.080	0.106	0.094
Number of Municipalities	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099

Notes: This table reports the treatment effects for selected ministries considering heterogeneous effects by the intensity of campaign financing by construction companies. The variable construction_companies is the standardized variable of the share of the contributions of construction companies and engineering firms over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 8 – Treatment effetcs on disaggregated data and campaign financiers

	Disaggregated data					
Log value of block grants per capita	Paving	Infrastructure	Drainage	Bridge	Popular Housing	Urbanization
Panel A: audited municipalities						
Treatment	0.0196	0.000122	-0.0636	-0.0905	-0.258***	-0.220**
	(0.270)	(0.167)	(0.165)	(0.0567)	(0.0996)	(0.0946)
Treatment*construction_companies	-1.181***	-0.199**	-0.406***	-0.128***	-0.571***	-0.267***
	(0.153)	(0.0946)	(0.0925)	(0.0298)	(0.0592)	(0.0524)
construction_companies*year2009	-0.0461	0.0589	0.0451	-0.0114	0.00362	0.00651
	(0.0315)	(0.0366)	(0.0328)	(0.0111)	(0.0237)	(0.0203)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	18,326	18,326	18,326	18,326	18,326	18,326
R^2	0.183	0.082	0.038	0.011	0.099	0.031
Number of Municipalities	1,419	1,419	1,419	1,419	1,419	1,419
Panel B: non audited municipalities						
Treatment	0.133	-0.0911	0.0918	0.00344	-0.00277	-0.0407
	(0.219)	(0.133)	(0.131)	(0.0872)	(0.0881)	(0.0779)
Treatment*construction_companies	0.0259	-0.0229	-0.0424	0.149	-0.0448*	-0.0179
	(0.0820)	(0.0543)	(0.0435)	(0.147)	(0.0249)	(0.0302)
construction_companies*year2009	-0.0387	0.00632	0.0173	-0.00575	-0.00873	0.0296
	(0.0279)	(0.0197)	(0.0192)	(0.0108)	(0.0187)	(0.0202)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	52,759	52,759	52,759	52,759	52,759	52,759
R^2	0.191	0.080	0.032	0.011	0.090	0.025
Number of Municipalities	4,099	4,099	4,099	4,099	4,099	4,099

Notes: This table reports the treatment effects for selected expenditures considering heterogeneous effects by the intensity of campaign financing by construction companies. The variable construction_companies is the standardized variable of the share of the contributions of construction companies and engineering firms over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 9 – Dynamic effects on block grants by ministries

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment*year2009	-0.854***	0.123	0.138	0.0459
	(0.232)	(0.231)	(0.344)	(0.296)
Treatment*year2010	-0.604***	0.0980	-0.203	0.184
	(0.208)	(0.202)	(0.305)	(0.322)
Treatment*year2011	-0.143	-0.123	-0.252	0.323
	(0.253)	(0.124)	(0.370)	(0.309)
Treatment*year2012	-0.328*	-0.101	-0.0184	0.606**
	(0.175)	(0.126)	(0.191)	(0.291)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,438	18,438	18,438	18,438
R^2	0.066	0.025	0.087	0.145
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment*year2009	-0.0780	0.0757	-0.0469	0.255
	(0.198)	(0.129)	(0.205)	(0.185)
Treatment*year2010	0.118	-0.0542	0.157	-0.125
	(0.187)	(0.106)	(0.204)	(0.192)
Treatment*year2011	0.246	0.0307	0.147	0.257
	(0.197)	(0.0919)	(0.230)	(0.200)
Treatment*year2012	0.118	-0.0787	0.139	-0.128
	(0.180)	(0.0756)	(0.133)	(0.186)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	53,243	53,243	53,243	53,243
R^2	0.070	0.018	0.079	0.146
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports dynamic treatment effects for selected ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 10 – Dynamic effects on block grants by ministries and campaign financiers

	Block grants by ministries			
Log value of block grants per capita	Cities	Agriculture	Health and Education	Other
	[1]	[2]	[3]	[4]
Panel A: audited municipalities				
Treatment*year2009	-0.955***	0.160	0.289	-0.103
	(0.226)	(0.226)	(0.330)	(0.278)
Treatment*year2009*construction	-0.731***	-0.136	0.117	-1.303***
	(0.124)	(0.123)	(0.180)	(0.155)
Treatment*year2010	-0.683***	0.113	-0.392	0.143
	(0.202)	(0.197)	(0.293)	(0.319)
Treatment*year2010*construction	-0.748***	-0.121	-1.006***	0.222
	(0.111)	(0.108)	(0.169)	(0.196)
Treatment*year2011	-0.223	-0.0983	-0.0647	0.512*
	(0.245)	(0.121)	(0.335)	(0.298)
Treatment*year2011*construction	-0.910***	0.152**	2.156***	0.849***
	(0.131)	(0.0657)	(0.184)	(0.169)
Treatment*year2012	-0.394**	-0.0800	-0.0379	0.655**
	(0.171)	(0.123)	(0.188)	(0.277)
Treatment*year2012*construction	-0.586***	0.144**	-0.364***	0.634***
	(0.101)	(0.0657)	(0.105)	(0.158)
Construction*year2009	-0.0397	0.0190	-0.110***	0.0260
	(0.0337)	(0.0311)	(0.0387)	(0.0453)
Construction*year2010	-0.00392	0.0396	-0.0705*	-0.0936**
	(0.0321)	(0.0308)	(0.0398)	(0.0370)
Construction*year2011	0.00956	-0.0114	-0.0265	0.00117
	(0.0291)	(0.0164)	(0.0437)	(0.0469)
Construction*year2012	0.00653	-0.0268**	-0.0221	0.0431
	(0.0310)	(0.0136)	(0.0250)	(0.0508)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	17,990	17,990	17,990	17,990
R^2	0.068	0.025	0.085	0.146
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment*year2009	-0.0889	0.0519	0.00203	0.314
	(0.205)	(0.129)	(0.213)	(0.191)
Treatment*year2009*construction	-0.0671	0.106	0.0865	-0.0363
	(0.0654)	(0.120)	(0.0560)	(0.0907)
Treatment*year2010	0.114	-0.0456	0.193	-0.105
	(0.192)	(0.112)	(0.213)	(0.199)
Treatment*year2010*construction	0.112	0.0144	-0.0652	0.112
	(0.125)	(0.0267)	(0.0805)	(0.0940)
Treatment*year2011	0.215	0.0278	0.179	0.213
	(0.197)	(0.0977)	(0.241)	(0.206)
Treatment*year2011*construction	0.0495	0.0332*	0.0768	0.0482
	(0.188)	(0.0187)	(0.0822)	(0.0818)
Treatment*year2012	0.160	-0.0882	0.178	-0.0715
	(0.188)	(0.0774)	(0.139)	(0.195)
Treatment*year2012*construction	-0.121***	0.0596*	-0.0738**	-0.140***
	(0.0439)	(0.0316)	(0.0340)	(0.0390)
Construction*year2009	-0.0410	0.0183	-0.00694	0.0174
	(0.0286)	(0.0220)	(0.0309)	(0.0247)
Construction*year2010	-0.0700***	-0.00489	-0.0282	-0.0329
	(0.0264)	(0.0196)	(0.0376)	(0.0285)
Construction*year2011	0.0181	-0.0234	-0.0549*	-0.0110
	(0.0274)	(0.0152)	(0.0321)	(0.0291)
Construction*year2012	-0.0155	-0.0202	0.0287	-0.00107
	(0.0287)	(0.0188)	(0.0260)	(0.0301)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	51,307	51,307	51,307	51,307
R^2	0.073	0.019	0.075	0.143
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports dynamic treatment effects for selected ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 11 – Treatment effects on block grants and mayor term

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment	-0.737**	0.753	0.0665	-0.283
	(0.342)	(0.504)	(0.552)	(0.526)
Treatment*firstterm	-0.0769	-0.894	0.127	0.318
	(0.470)	(0.550)	(0.726)	(0.609)
Firstterm*year2009	0.304***	-0.00581	0.205*	0.0575
	(0.0897)	(0.0605)	(0.109)	(0.0921)
Firstterm*Treated	-0.178	-0.0486	0.0976	-0.266
	(0.170)	(0.117)	(0.237)	(0.163)
Firstterm	0.0847***	0.0210	0.144***	0.192***
	(0.0292)	(0.0186)	(0.0312)	(0.0304)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,330	18,330	18,330	18,330
R^2	0.068	0.025	0.088	0.148
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	0.128	-0.0197	-0.0659	-0.0940
	(0.310)	(0.168)	(0.342)	(0.299)
Treatment*firstterm	-0.420	0.177	-0.0859	0.509
	(0.388)	(0.237)	(0.437)	(0.393)
Firstterm*year2009	0.134**	0.0468	0.135**	0.0777
	(0.0542)	(0.0324)	(0.0627)	(0.0555)
Firstterm*Treated	-0.202	-0.0103	-0.102	-0.115
	(0.127)	(0.0615)	(0.130)	(0.115)
Firstterm	0.111***	0.0157	0.192***	0.149***
	(0.0170)	(0.0107)	(0.0185)	(0.0181)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	52,982	52,982	52,982	52,982
R^2	0.072	0.019	0.082	0.148
Number of Municipalities	4,098	4,098	4,098	4,098

Notes: This table reports the treatment effects for selected ministries considering different effects in according to the term in which the mayor is governing. The variable Treatment is equal one for treated municipalities in year 2009. The variable Treated is equal one for treated municipalities in all periods. Firstterm is equal one in the period in which the mayor is in first term and varies in time (in opposition of the variable of construction_companies which was fixed in time). Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 12 – Treatment effects on block grants, mayor term and campaign financiers

	Block grants by ministries			
Log value of block grants per capita	Cities	Agriculture	Health and Education	Other
	[1]	[2]	[3]	[4]
Panel A: audited municipalities				
Treatment	-0.774**	0.753	0.114	-0.312
	(0.332)	(0.499)	(0.553)	(0.462)
Treatment*firstterm	-0.132	-0.919	0.338	0.137
	(0.494)	(0.586)	(0.750)	(0.583)
Treatment*construction	-0.514***	-0.433*	0.271	-1.398***
	(0.152)	(0.241)	(0.256)	(0.232)
Firstterm*year2009	0.284***	-0.0261	0.233**	0.0412
	(0.0935)	(0.0621)	(0.112)	(0.0946)
Construction*year2009	-0.0410	0.0171	-0.108***	0.0235
	(0.0337)	(0.0311)	(0.0376)	(0.0439)
Firstterm*treated	-0.177	-0.0469	0.102	-0.261
	(0.171)	(0.118)	(0.239)	(0.164)
Firstterm	0.0862***	0.0214	0.143***	0.193***
	(0.0293)	(0.0186)	(0.0313)	(0.0304)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,224	18,224	18,224	18,224
R^2	0.068	0.025	0.088	0.148
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	0.00555	-0.00514	-0.0381	-0.0307
	(0.307)	(0.175)	(0.354)	(0.304)
Treatment*firstterm	-0.246	0.120	-0.0649	0.510
	(0.397)	(0.241)	(0.458)	(0.406)
Treatment*construction	-0.0633	0.0876	0.0843	-0.0639
	(0.0617)	(0.119)	(0.0649)	(0.0945)
Firstterm*year2009	0.132**	0.0491	0.124*	0.0838
	(0.0577)	(0.0344)	(0.0663)	(0.0589)
Construction*year2009	-0.0346	0.0238	0.00213	0.0229
	(0.0282)	(0.0211)	(0.0301)	(0.0252)
Firstterm*treated	-0.205	-0.0105	-0.103	-0.114
	(0.127)	(0.0614)	(0.130)	(0.116)
Firstterm	0.112***	0.0153	0.193***	0.150***
	(0.0171)	(0.0107)	(0.0186)	(0.0181)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	52,509	52,509	52,509	52,509
R^2	0.072	0.019	0.082	0.145
Number of Municipalities	4,098	4,098	4,098	4,098

Notes: This table reports the treatment effects for selected ministries considering different effects in according to the term in which the mayor is governing. The variable Treatment is equal one for treated municipalities in year 2009. The variable Treated is equal one for treated municipalities in all periods. Firstterm is equal one in the period in which the mayor is in first term and varies in time (in opposition of variable of construction_companies, which is a standardized variable fixed in time). Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 13 – Treatment effects on reelection rates in 2012

Dependent variable	Reelection in 2012			
Mayors in sample	All mayors that run for reelection			Mayors that run for reelection and audited between 2009 and 2012
Heterogeneity	Construction donations in 2008	Audited between 2009 and 2012	Both	Construction donations in 2008
Treatment	-0.113*	-0.170**	-0.155**	-0.00318
	(0.0664)	(0.0746)	(0.0764)	(0.126)
Treatment*construction_donations_2008	-0.0445***		-0.0366**	-0.249***
	(0.0151)		(0.0143)	(0.0638)
Construction_donations_2008	-0.00236		-0.00217	-0.0127
	(0.00890)		(0.00889)	(0.0184)
Treatment*audited_2009_2012		0.183	0.153	
		(0.153)	(0.147)	
Audited_2009_2012		-0.0214	-0.0211	
		(0.0356)	(0.0357)	
Treatment*audited_2009_2012* Construction_donations_2008			-0.223***	
			(0.0617)	
Observations	2,374	2,374	2,338	232
R-squared	0.002	0.002	0.026	0.007

Notes: This table reports treatment effects on the reelection rates for mayors in the subsequent election after the treatment. We restrict our sample to the municipalities in which the mayors decided to run for reelection. In the last columns, we restrict the sample further to mayors that were audited in the period of the term (between 2009 and 2012).

Table 14 – Treatment effects on the decision of running for reelection in 2012

Dependent variable	Run for reelection in 2012			
Mayors in sample	Mayors in first term			Mayors in first term and audited between 2009 and 2012
Heterogeneity	Construction donations in 2008	Audited between 2009 and 2012	Both	Construction donations in 2008
Treatment	0.0352	0.0535	0.0465	-0.000389
	(0.0503)	(0.0563)	(0.0577)	(0.100)
Treatment*construction_donations_2008	0.0184*		0.0157	0.124**
	(0.0110)		(0.0106)	(0.0506)
Construction_donations_2008	0.0114*		0.0112*	0.00359
	(0.00605)		(0.00607)	(0.0137)
Treatment*audited_2009_2012		-0.0649	-0.0456	
		(0.119)	(0.115)	
Audited_2009_2012		0.0199	0.0183	
		(0.0268)	(0.0268)	
Treatment*audited_2009_2012* Construction_donations_2008			0.100**	
			(0.0492)	
Observations	3,239	3,239	3,239	309
R-squared	0.001	0.001	0.001	0.001

Notes: This table reports treatment effects on the decision of running for reelection in the subsequent election after the treatment. We restrict our sample to the municipalities in which the mayors were in first term and eligible to run for reelection. In the last columns, we restrict the sample further to mayors that were audited in the period of the term (between 2009 and 2012).

Table 15 – Treatment effects on the campaign donations from construction companies in 2012

Dependent variable	Construction donations in 2012			
Mayors in sample	All mayors that run for reelection			Mayors that run for reelection and audited between 2009 and 2012
Heterogeneity	Construction donations in 2008	Audited between 2009 and 2012	Both	Construction donations in 2008
Treatment	0.0302	0.167	0.147	-0.348***
	(0.175)	(0.234)	(0.240)	(0.0946)
Treatment*construction_donations_2008	-0.0899		-0.0981	-0.0445
	(0.102)		(0.104)	(0.0466)
Construction_donations_2008	0.162***		0.161***	0.0445
	(0.0489)		(0.0491)	(0.0466)
Treatment*audited_2009_2012		-0.523**	-0.473*	
		(0.252)	(0.258)	
Audited_2009_2012		0.0897	0.0671	
		(0.0973)	(0.0987)	
Treatment*audited_2009_2012* Construction_donations_2008			-0.0633	
			(0.0920)	
Observations	2,338	2,338	2,338	231
R-squared	0.025	0.001	0.026	0.008

Notes: This table reports treatment effects on the reelection rates for mayors in the subsequent election after the treatment. We restrict our sample to the municipalities in which the mayors decided to run for reelection. In the last columns, we restrict the sample further to mayors that were audited in the period of the term (between 2009 and 2012).

Table 16 – Robustness check I: previous treatment effects

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment*year2005	-0.234	0.211	0.00743	-0.131
	(0.206)	(0.189)	(0.246)	(0.246)
Treatment*year2006	-0.178	0.361*	-0.322	0.00103
	(0.240)	(0.192)	(0.283)	(0.228)
Treatment*year2007	0.00434	-0.227*	0.0886	-0.413
	(0.291)	(0.130)	(0.324)	(0.261)
Treatment*year2008	0.210	0.0599	-0.220	0.0551
	(0.270)	(0.187)	(0.335)	(0.271)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,438	18,438	18,438	18,438
R^2	0.065	0.055	0.087	0.144
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment*year2005	-0.195	0.00297	-0.172	-0.0351
	(0.144)	(0.109)	(0.175)	(0.146)
Treatment*year2006	-0.144	-0.0821	-0.0183	-0.111
	(0.170)	(0.0983)	(0.159)	(0.164)
Treatment*year2007	0.148	0.186	0.208	0.00689
	(0.195)	(0.135)	(0.190)	(0.177)
Treatment*year2008	0.0323	0.144	0.203	0.0392
	(0.175)	(0.121)	(0.214)	(0.172)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	53,243	53,243	53,243	53,243
R^2	0.070	0.056	0.079	0.145
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports treatment effects prior to the administration of treatment as a robustness check for selected Ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 17 – Robustness check II: alternative measure for Ministry of Cities' block grants

	Block grants by the Cities Ministry					
	Logarithm				Level	
	Value		Number		Total Value	
Dependent Variable	Per capita	Total	Per capita	Total	Fixed effects	Tobit
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: audited municipalities						
Treatment	-0.764***	-2.937***	-0.0370***	-0.249***	-10.33**	-51.27***
	(0.222)	(0.877)	(0.0125)	(0.0556)	(4.181)	(16.70)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	N
Observations	18,438	18,460	18,438	18,460	18,438	18,438
R^2	0.066	0.069	0.040	0.073	0.024	
Number of Municipalities	1,419	1,420	1,419	1,420	1,419	
Panel B: non audited municipalities						
Treatment	-0.118	-0.559	0.00153	-0.0315	1.216	-1.001
	(0.191)	(0.675)	(0.0155)	(0.0550)	(4.385)	(8.016)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	N
Observations	53,243	53,651	53,243	53,651	53,243	53,243
R^2	0.070	0.067	0.041	0.074	0.043	
Number of Municipalities	4,099	4,127	4,099	4,127	4,099	

Notes: This table reports the average treatment effects for selected ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 18 – Robustness check III: aggregate block grants and campaign financiers

Log value of block grants per capita	Aggregate block grants			
	Number	Value	Released value	Duration
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment	-0.0275	-0.0761	-0.276	-0.0209
	(0.0277)	(0.236)	(0.268)	(0.0932)
Treatment*construction_companies	-0.137***	-1.016***	-1.846***	0.135***
	(0.0150)	(0.133)	(0.157)	(0.0508)
construction_companies*year2009	-0.0122***	-0.0201	-0.0562	-0.00473
	(0.00424)	(0.0376)	(0.0442)	(0.0151)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,326	18,326	18,326	14,834
R^2	0.117	0.156	0.205	0.219
Number of Municipalities	1,419	1,419	1,419	1,420
Panel B: non audited municipalities				
Treatment	0.0217	0.252	0.130	-0.0420
	(0.0288)	(0.171)	(0.197)	(0.0648)
Treatment*construction_companies	0.0106	0.0683	0.113	0.00751
	(0.00824)	(0.0540)	(0.0735)	(0.0162)
construction_companies*year2009	-0.00473*	-0.0117	-0.0214	-0.00809
	(0.00271)	(0.0223)	(0.0232)	(0.00854)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	52,759	52,759	52,759	42,105
R^2	0.112	0.153	0.205	0.213
Number of Municipalities	4,099	4,099	4,099	4,095

Notes: This table reports the treatment effects for aggregate measures of block grants by municipality, considering heterogeneous effects by the intensity of campaign financing by construction companies. The variable construction_companies is the standardized variable of the share of the contributions of construction companies and engineering firms over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 19 – Robustness check IV: treatment effects on block grants by ministries and individual campaign financiers

Log value of block grants per capita	Block grants by ministries			
	Cities	Agriculture	Health and Education	Other
	[1]	[2]	[3]	[4]
Panel A: audited municipalities				
Treatment	-0.780***	0.132	0.397	0.0355
	(0.243)	(0.204)	(0.343)	(0.289)
Treatment*individual	0.131	0.261	-0.281	-0.329
	(0.210)	(0.185)	(0.363)	(0.256)
individual*year2009	-0.100**	-0.0258	0.0136	-0.0461
	(0.0453)	(0.0287)	(0.0547)	(0.0445)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	18,326	18,326	18,326	18,326
R^2	0.067	0.025	0.086	0.145
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	-0.135	0.0729	-0.0210	0.316*
	(0.195)	(0.123)	(0.210)	(0.192)
Treatment*individual	0.226	-0.184*	-0.00627	0.128
	(0.232)	(0.107)	(0.193)	(0.219)
individual*year2009	0.0146	0.000760	0.0569*	-0.0187
	(0.0284)	(0.0173)	(0.0322)	(0.0285)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	52,759	52,759	52,759	52,759
R^2	0.070	0.019	0.079	0.143
Number of Municipalities	4,099	4,099	4,099	4,099

Notes: This table reports the treatment effects for selected ministries considering heterogeneous effects by the intensity of campaign financing by individual person. The variable individual is the standardized variable of the share of the contributions of individuals over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 20– Robustness check V: treatment effects on block grants by Type of expenditure and individual campaign financiers

	Block grants by type of expenditure									
	All Ministries		Cities		Agriculture		Health and Education		Other Ministries	
Log value of block grants per capita	Public works	Non public works	Public works	Non public works	Public works	Non public works	Public works	Non public works	Public works	Non public works
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Panel A: audited municipalities										
Treatment	-0.143	0.142	-0.760***	-0.0699***	0.0584	0.0353	0.177	0.229	-0.0449	-0.0458
	(0.354)	(0.302)	(0.239)	(0.0251)	(0.111)	(0.210)	(0.363)	(0.295)	(0.293)	(0.266)
Treatment*individual	0.0489	-0.399	0.125	0.0152	0.132	0.109	-0.0303	-0.267	-0.0117	-0.431**
	(0.325)	(0.267)	(0.206)	(0.0246)	(0.105)	(0.171)	(0.336)	(0.271)	(0.271)	(0.191)
individual*year2009	-0.121**	0.0197	-0.109**	0.0166	-0.0290	-0.00313	-0.0546	0.0475	-0.0459	-0.0200
	(0.0535)	(0.0453)	(0.0445)	(0.0181)	(0.0182)	(0.0253)	(0.0542)	(0.0450)	(0.0462)	(0.0417)
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326	18,326
R^2	0.125	0.111	0.065	0.041	0.070	0.026	0.049	0.092	0.110	0.088
Number of Municipalities	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419	1,419
Panel B: non audited municipalities										
Treatment	-0.0256	0.388**	-0.152	-0.0184	-0.0465	0.111	-0.102	0.0402	0.0653	0.390**
	(0.228)	(0.185)	(0.195)	(0.0422)	(0.0442)	(0.113)	(0.185)	(0.185)	(0.196)	(0.194)
Treatment*individual	0.203	0.00559	0.260	-0.0466	0.0275	-0.211**	-0.118	0.109	0.146	0.212
	(0.231)	(0.189)	(0.230)	(0.0369)	(0.0358)	(0.0994)	(0.182)	(0.169)	(0.221)	(0.204)
individual*year2009	-0.0209	0.0169	0.00577	0.00423	0.0118	-0.00869	0.00249	0.0796***	-0.0229	-0.00936
	(0.0329)	(0.0275)	(0.0283)	(0.00885)	(0.0116)	(0.0148)	(0.0311)	(0.0265)	(0.0278)	(0.0263)
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759	52,759
R^2	0.126	0.103	0.070	0.034	0.055	0.028	0.045	0.081	0.106	0.094
Number of Municipalities	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099	4,099

Notes: This table reports the treatment effects for selected ministries considering heterogeneous effects by the intensity of campaign financing by individual person. The variable individual is the standardized variable of the share of the contributions of individuals over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of block grants per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 21 – Robustness check VI: treatment effects on Parliamentary amendments

Log value of parliamentary amendments per capita	Parliamentary Amendments			
	All ministries	Cities	Agriculture	Health and Education
	[1]	**[2]**	**[3]**	**[4]**
Panel A: audited municipalities				
Treatment	0.191	0.0730	-0.0141	0.0292
	(0.268)	(0.190)	(0.135)	(0.111)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	15,600	15,600	15,600	15,600
R^2	0.034	0.065	0.004	0.018
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	0.199	-0.0661	-0.00639	-0.0321
	(0.170)	(0.0918)	(0.0664)	(0.0197)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	45,034	45,034	45,034	45,034
R^2	0.044	0.044	0.007	0.015
Number of Municipalities	4,098	4,098	4,098	4,098

Notes: This table reports the average treatment effects on parliamentary amendments to selected ministries. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of parliamentary amendments per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

Table 22 – Robustness check VII: treatment effects on Parliamentary amendments and campaign financiers

Log value of parliamentary amendments per capita	Parliamentary Amendments			
	All ministries	Cities	Agriculture	Health and Education
	[1]	[2]	[3]	[4]
Panel A: audited municipalities				
Treatment	0.00285	-0.0434	0.0209	0.00289
	(0.258)	(0.165)	(0.147)	(0.108)
Treatment*construction_companies	-0.927***	-0.215**	0.145	0.00261
	(0.168)	(0.0898)	(0.183)	(0.0448)
construction_companies*year2009	0.000526	-0.0115	-0.00900	0.00566
	(0.0331)	(0.0126)	(0.00666)	(0.0143)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	15,488	15,488	15,488	15,488
R^2	0.035	0.066	0.005	0.008
Number of Municipalities	1,419	1,419	1,419	1,419
Panel B: non audited municipalities				
Treatment	0.233	-0.0749	0.00135	-0.0367*
	(0.178)	(0.0939)	(0.0704)	(0.0208)
Treatment*construction_companies	-0.104*	-0.0136	-0.0107	-0.0171
	(0.0536)	(0.0391)	(0.0151)	(0.0163)
construction_companies*year2009	0.0385	0.00483	-0.000343	0.00758
	(0.0279)	(0.0181)	(0.00559)	(0.0111)
Year Fixed Effects	Y	Y	Y	Y
Municipality Fixed Effects	Y	Y	Y	Y
Observations	44,551	44,551	44,551	44,551
R^2	0.044	0.045	0.007	0.009
Number of Municipalities	4,098	4,098	4,098	4,098

Notes: This table reports the treatment effects on parliamentary amendments to selected ministries considering heterogeneous effects by the intensity of campaign financing by construction companies. The variable construction_companies is the standardized variable of the share of the contributions of construction companies and engineering firms over the total contributions by municipality in elections of 2008. Panel A shows the estimation for municipalities that had been audited before the receipt of the letter in May of 2009. Panel B shows the estimation for municipalities that had not been audited before the receipt of the letter in May of 2009. All the regressions are estimated by fixed effects and all standard errors are clustered at municipality level. All the dependent variables are calculated as: log(1+value of parliamentary amendments per capita). * Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.**

APPENDIX

Letter sent to the treated mayors by CGU

Anexo - Ofício de Comunicação aos Prefeitos do Grupo de Tratamento

PRESIDÊNCIA DA REPÚBLICA
Controladoria-Geral da União no Estado de <NOME DO ESTADO>
<ENDEREÇO DA CGU NO ESTADO COM CEP/ TELEFONE/ E-MAIL>

Ofício nº /CGU-<SIGLA DO ESTADO>

Brasília, de de 2009.

A Sua Excelência o Senhor
<NOME DO PREFEITO>
<CARGO/ENTIDADE>
<NOME DO MUNICÍPIO/UF>

Assunto: **Programa de Fiscalização a partir de Sorteios Públicos.**

Senhor Prefeito,

Cumprimentando-o, refiro-me ao sorteio do Programa de Fiscalização a partir de Sorteios Públicos da Controladoria-Geral da União realizado no dia 12/05/2009, na sede da Caixa Econômica Federal em Brasília, que selecionou esse município, conforme Portaria CGU nº 994, de 22/05/2009, publicada no Diário Oficial da União do dia 25/05/2009, para compor grupo de 120 unidades municipais, que servirá de base para um novo sorteio de 30 municípios, que será realizado em maio de 2010.

2. Este sorteio específico e diferenciado foi estabelecido pela Portaria CGU nº 930, de 08/05/2009, do Excelentíssimo Senhor Ministro de Estado do Controle e da Transparência, publicada no Diário Oficial da União do dia 09/05/2009, com o objetivo definir o universo de municípios a serem sorteados e avaliar a metodologia do Programa de Fiscalização a partir de Sorteios Públicos da CGU.

3. Nesse sentido, em sorteio público a ser realizado em maio de 2010, ao invés de serem selecionados 60 municípios dentre aqueles com população de até 500.000 habitantes, exceto capitais, serão sorteados 30 a partir do referido grupo de 120 que foram pré-sorteados. Dessa forma, esse município, como integrante do grupo dos 120 pré-selecionados, terá uma chance maior de ser sorteado na seleção que ocorrerá em maio de 2010, em comparação com os sorteios tradicionalmente realizados por esta CGU.

4. Informo, por outro lado, que, em virtude de ter sido selecionado para compor o grupo em referência, esse Município de <Nome do Município> não participará, até maio de 2010, dos sorteios ordinários realizados pela CGU.

Atenciosamente,

<NOME DO CHEFE DA CGU DO ESTADO>
Chefe da Controladoria-Geral da União no Estado de <NOME DO ESTADO>

Notes: Extracted from Zamboni & Litschig (2012)

Portaria 994

PORTARIA Nº 994, DE 22 DE MAIO DE 2009

O MINISTRO DE ESTADO DO CONTROLE E DA TRANSPARÊNCIA, no exercício de suas atribuições e tendo em conta o que estabelece a Portaria nº 247, de 20 de junho de 2003,

RESOLVE:

Art. 1º Tornar público, na forma do Anexo a esta Portaria, o resultado do sorteio par fins de fiscalização de unidades municipais e avaliação da metodologia do Programa de Fiscalização a partir de sorteios Públicos, conforme previsto na Portaria nº 930, de 08 de maio de 2009.

§ 1º No sorteio a que se refere o **caput** foram pré-selecionadas 120 unidades municipais, distribuídas por Estados da Federação.

§ 2º As unidades municipais sorteadas, para os fins a que se refere o **caput**, não entrarão na lista dos sorteios ordinários do Programa de Fiscalização a partir de Sorteios Públicos, até maio de 2010.

§ 3º Os 120 municípios pré-selecionados na forma do §1º, comporão grupo a partir do qual serão selecionadas, para fins de fiscalização, 30 unidades municipais em novo Sorteio a ser realizado em maio de 2010.

Art. 2º Esta Portaria entra em vigor na data de sua publicação.

JORGE HAGE SOBRINHO

REFERENCES

Ackerman, S. R. (1999). *Corruption and Government: Causes, Consequences, and Reform,* .

Banerjee, A., Mullainathan, S., & Hanna, R. (2012). Corruption. *NBER Working Paper Series* .

Becker, G. S., & Stigler, G. J. (Jan de 1974). Law Enforcement, Malfeasance, and Compensation of Enforcers. *The Journal of Legal Studies* , pp. 1-18.

Besley, T. (2006). *Principled Agents? The Political Economy of Good Government.*

Bo, E. d., Bó, P. d., & Tella, R. d. (2006). “Plata o Plomo?”: Bribe and Punishment in a Theory of Political Influence. *American Political Science Review* .

Bobonis, G. J., Fuertes, L. R., & Schwabe, R. (2013). The Dynamic Effects of Information on Political Corruption:Theory and Evidence from Puerto Rico.

Cordis, A., & Milyo, J. (2013). Do State Campaign Finance Reforms Reduce Public Corruption? *Working paper* .

Fernanda, B., Nannicini, T., Perotti, R., & Tabellini, G. (2010). The Political Resource Curse. *Working paper* .

Ferraz, C., & Finan, F. (2011). Electoral Accountability and Corruption:Evidence from the Audits of Local Governments. *American Economic Review* .

Ferraz, C., & Finan, F. (2008). Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes. *The Quarterly Journal of Economics* .

Fleischer, D. (1997). Political corruption in Brazil. *Crime, Law & Social Change* .

Gueddes, B., & Neto, A. R. (1992). Institutional sources of corruption in Brazil. *Third World Quarterly* .

Kleven, H. J., Knudsen, M., Kreiner, C., Pedersen, S., & Saez, E. (2011). UNWILLING OR UNABLE TO CHEAT? EVIDENCE FROM A TAX AUDIT EXPERIMENT IN DENMARK. *Econometrica* .

Lederman, D., Loayza, N., & Soares, R. R. (2001). Accountability and Corruption: Political Institutions Matter.

Mauro, P. (1995). Corruption and Growth. *The Quarterly Journal of Economics* , pp. 681-712.

Mauro, P. (January de 1998). Corruption and the composition of government expenditure. *Journal of Public Economics* .

Niehaus, P., & Sukhtankar, S. (2012). Corruption Dynamics: The Golden Goose Effect.

Olken, B. A. (2009). Corruption perceptions vs. corruption reality. *Journal of Public Economics* .

Olken, B. A. (2007). Monitoring Corruption: Evidence from a Field Experiment in Indonesia. *Journal of Political Economy* .

Olken, B. A., & Pande, R. (s.d.). Corruption in Developing Countries. *NBER Working Papers Series* .

Pande, R. (s.d.). Can Informed Voters Enforce Better Governance? Experiments in Low Income Democracies.

Power, T., & Taylor, M. *Corruption and Democracy in Brazil.*

Reinikka, R., & Svensson, J. (s.d.). The power of information : evidence from a newspaper campaign to reduce capture of Public Funds.

Shah, A. (2007). *Performance Accountability and Combating Corruption.* World Bank.

Shleifer, A., & Vishny, R. W. (1993). Corruption. *The Quarterly Journal of Economics*, .

Speck, B. (2010). O dinheiro e a política no Brasil. *Le Monde Diplomatique Brasil* .

Tella, R. D., & Schargrodsky, E. (2003). The Role of Wages and Auditing during a Crackdown on Corruption in the City of Buenos Aires. *Journal of Law and Economics* .

Zamboni, Y. (2012). Avaliando o avaliador: Evidências de um experimento de campo sobre as auditorias da CGU. *Tese de Doutorado* .

Zamboni, Y., & Litschig, S. (March de 2013). Audit Risk and Rent Extraction: Evidence from a Randomized Evaluation in Brazil. *Barcelona GSE Working Paper Series* .