DECLARATORIA SULLA TESI DI DOTTORATO

Da inserire come prima pagina della tesi

Il/la sottoscritto/a

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Essays in Political Economy Evidence from Brazil

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Introduction

Recently, one of the most dominant topics in the political economy literature is that the type of resources that finance a state is central to understanding the accountability of governments and their capacity to govern in ways that promote growth, transparency and broad-based participation. This debate has gained considerable attention in federalist systems in developing countries where transfers from the central government to lower-tier administrative units are a crucial ingredient for the provision of public goods and services. Therefore understanding the incentives and constraints that local governments face in this context is of great importance.

A large body of research in public finance and political economy has investigated the (normative) implications and the (positive) determinants of intergovernmental transfers, respectively. From a political economy perspective, it is hard to believe that the central government—based on either its own preferences or the presence of institutional and political constraints—ends up acting as a benevolent social planner. Indeed, although federations usually adopt allocation rules that shelter the distribution of intergovernmental grants from political distortions, incumbent politicians at various layers of government can still take advantage of a lot of discretionary policy instruments to tease voters.

While the principles of fiscal federalism have been studied extensively, much of this literature has focused on developed countries. In a lesser-developed country context, intergovernmental relations are quite different and more complicate, political accountability is weaker and political clientelism might be present. Our understanding of intergovernmental relations in such contexts is limited and the empirical literature is scarce.

This dissertation provides relevant contribution towards our understanding of the incentives and constraints that local governments face in the context of a federalist system. In particular, this study aims to understand how intergovernmental transfers affect the behavior of voters and politicians and its consequences to the political process. The analysis goes through theoretical and empirical analysis at the microeconomic level with specific reference

to Brazil, which presents all ideal conditions for this analysis.

Since the constitution was ratified in Brazil in 1988, municipal administrators have become increasingly responsible for a relevant share of the provision of public services. At the same time, most municipalities are financially constrained and intergovernmental transfers represent a relevant share of municipal revenues. On the other, extensive anecdotal evidence suggests that voters reward practices related to political clientelism and political patronage in Brazil. In 2003, the Brazilian central government launched an anti-corruption program with randomly allocated audits that disclosure corruption information at the local level.

For the purpose of this analysis, I used an original dataset that I collected on evidence of corruption from these random audits of approximately 1,000 municipalities, detailed biographical and electoral data for local politicians, demographic and financial information for all Brazilian municipalities, and budgetary amendments submitted by members of Congress over four electoral cycles.

The analysis developed along the three chapters of these dissertation uses appropriate econometric methods with sources of clean variation to identify causal effects. Using the exogenous variation in the timing of the release of the audit reports and taking advantage of the Brazilian institutional scheme, the first chapter sheds light on the mechanisms through each this anti-corruption program works. Indeed I am interested in understanding if the dissemination of corruption works *per se* in increasing political accountability, in strengthening political participation in civil society, and in improving public-sector management at the local level. One potential channel that has not been previously discussed in the literature is that a reduction in federal discretionary transfers to local municipalities may affect voter behavior. If voters reward politicians for obtaining more public goods or services, a reduction in transfers by the central government could trigger punishment by voters at the polls. This argument makes sense in the Brazilian context where voters reward practices related to political clientelism and political patronage.

In the second chapter analyzes whether federal transfers deteriorate the functioning of government institutions and the mechanisms though which it happens. Here we focus on

two mechanisms that are of fundamental importance in a variety of situations: the effects of additional resources on political corruption and on the incentives to participate in politics. At the margin, higher exogenous revenues induce more corruption, because incumbents have more rooms to grab rents without disappointing voters. Moreover, if the benefit of corrupt activities is more valuable to those with worse outside options, individuals of lower quality are attracted into politics. The interaction between these two effects gives rise to a complementarity: precisely because his/her opponents are now of lower quality, an incumbent can afford to grab even more rents while at the same time increasing his/her probability of reelection. In order to identify these effects this analysis employ a fuzzy regression discontinuity design that exploits the rules in the allocation of constitutional transfers[1].

The third chapter uses a regression discontinuity design in close races to investigate whether there are tactical motivations in the allocation of federal transfers by the Brazilian federal government. As far as local governments can claim political credit for the money the central government is allocating, partisan alignment between the two levels of government should increase the amount of transfers. There are different explanations for politically motivated transfers, or tactical redistribution. The econometric strategy allow us to analyze the interaction between the degree of political competition and partisan alignment in determining the tactical distribution of central-level transfers. If the central government were able to obtain full political credit for the transferred resources, it would be indifferent between benefiting aligned or unaligned municipalities. On the contrary, if voters were unable to distinguish the source of the grants and there were some leakage in their goodwill toward the federal government, aligned municipalities should receive more transfers. Furthermore, among aligned municipalities, those where the incumbent won by a narrow margin should receive more[2].

[1]This chapter is a joint study with Tommaso Nannicini, Roberto Peroti and Guido Tabellini

[2]This chapter is a joint study with Tommaso Nannicini

CHAPTER 1: Who Is Punishing Corrupt Politicians - Voters or the Central Government? Evidence from the Brazilian Anti-Corruption Program

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Abstract

In 2003 the Brazilian central government (CG) launched an anti-corruption program. Since then municipalities have been randomly selected to be audited on a monthly basis. Evidence in the literature suggests that the probability of re-election of an incumbent mayor decreases as the number of reported corruption violations rises. This paper sheds light on the mechanisms through which the Brazilian anti-corruption program acts. After the release of the audit reports, municipalities where more than two corruption violations were reported receive 26% fewer transfers from the CG. The CG increase the amount of transfers to municipalities where the mayor who is affiliated to the part of the president is found to be honest, and it helps politically aligned municipalities with high levels of released corruption to move through the punishment process more quickly. The effects of the dissemination of corruption information on the probability of reelection of incumbent mayors seem to gradually disappear with time. Yet, when these effects have completely faded and voters had time to feel the consequences of receiving fewer transfers, the probability of reelection of corrupt politicians decreases.

1 Introduction

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

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

Brazil presents all ideal conditions to understanding how voters react when mayors are found to be corrupt and when transfers are reduced to municipalities after high levels of corruption are released. The argument that the central government may reduce transfers to municipalities after the release of corruption is plausible. According to the institutions

that handle discretionary infrastructure transfers (*Instrução Normativa do Supremo Tribunal Nacional*, n^o 1, 15 January 1997), in order to receive these transfers, each municipal administration has to sign an agreement with the central government. When the municipal administration is found to be in breach of contract, the municipality is prohibited from receiving these discretionary transfers. The auditors are then required to implement specific procedures to evaluate whether the flow of transfers can be reestablished[1].

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

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

The analysis is divided into two parts. After coding the information contained in the audit reports for municipalities in 15 lotteries, I first investigated how the central government reacts to the disclosure of local corruption by using data on infrastructure transfers and federal budgetary amendments to Brazilian municipalities. Here I consider two samples

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

of municipalities: the random sample of audited municipalities (779 municipalities) and the sample of all municipalities (audited and non-audited) that participate in the anti-corruption program[2] (5,490 municipalities). I then identify the effects of the disclosure of corruption on transfers by considering a longitudinal dataset of infrastructure transfers for eight years (1999-2006) and exploiting the exogenous variation in the timing of the release of the audit reports.

The results suggest that the central government reduces transfers to those municipalities where mayors are found to be corrupt. Transfers significantly decrease by 25.9% to municipalities with more than two corruption violations (30% of the sample) after the release of the audit reports. Moreover, this effect persists for at least three years after the release of the audit reports. However, the results are not as time persistent in municipalities with mayors affiliated with the party of the president. In addition the CG increases transfers to municipalities whose mayors were found to be honest, but only to those that are politically aligned with the president. These results suggest that the CG attempts to minimize political capital losses and maximize political gain by reducing transfers selectively to municipalities according to their party affiliation. While the CG increases the amount of transfers to municipalities where the mayor who is affiliated to the part of the president is found to be honest, it helps politically aligned municipalities with high levels of released corruption to move through the punishment process more quickly[3]. Brollo and Nannicini (2010) show that municipalities where the mayor is affiliated with the coalition of the President receive larger transfers in close races because the CG ties the hands of their enemies[4].

A natural concern that could arise is whether this reduction in transfers is translated into the lower levels of the supply of public goods. This study shows evidence that the release of

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

[3]These effects remain even when the sample of all municipalities (including non-audited ones) are taking into account and do not seem to be driven by any unobservable variable that could be correlated with both being affiliated with the political party of the president and the timing of the release of the audit reports

[4]With Spanish data, Solé-Ollé and Sorribas-Navarro (2008b) found that municipalities aligned with upper-tier grantor governments receive more grants than those that are unaligned.

corruption have no effects on the amount of constitutional transfers received, municipal budget surpluses, and revenues from local taxes for any level of corruption reported, suggesting that there is no crowding-out effect. Most importantly, the release of the audit reports has a negative effect on infrastructure expenditure in municipalities with more than two reported corruption violations.

This part of the analysis is relevant per se in terms of policy implications since poorer municipalities are positively associated with corruption, a reduction in transfers can disproportionately hurt the poorer. Furthermore, if the reduction in transfers to municipalities with released corruption drives the electoral punishment, the central government would perform a vital role in this anti-corruption program. When political clientelism is present, punishment by the central government (reduction in transfers) can be the linchpin to promoting local political selection. This program could be an important tool that allows a self-interested central government to increase its political capital.

Some concerns regarding this first part of the analysis need to be addressed. First, with respect to the estimation strategy used to evaluate the effects of unveiled corruption on the amount of transfers, a concern arises. This reduction in transfers can be demand-driven. That is, mayors have to exert an effort in applying for these transfers. Therefore the release of the audit reports could have created disincentives for mayors when applying for these transfers because they forgo elections. Since most of these discretionary transfers depend on the execution of federal budgetary amendments, an alternative dataset of federal budgetary amendments is also employed in order to address this issue. It brings information on the value of the budgetary law initially approved by the president (it corresponds to the value applied by the municipality), the value of the budgetary law amendment later authorized by the legislative branch and the value of the budgetary law amendment executed (paid) by the executive branch to the municipality to each municipality in a given year. The results indicate that during the years following the audit there was no change in the demand for transfers.

Second, the President Lula's first administration started in 2003 (the year when the

Brazilian anti-corruption program was launched). It could be that since the beginning of Lula's administration, the amount of transfers to municipalities affiliated with the party in opposition to Lula has been decreased as compared to before he took office. Opposition parties had also been part of the governing coalition during the two consecutive previous presidential terms (Fernando Henrique Cardoso, 1994-2002). Because corruption is likely correlated with transfers, it could also be associated with mayor's political party affiliation.

Third, the political selections induced by the release of the audit reports (audits increase turnover, resulting in more first-term mayors) could drive the results if first-term mayors are disadvantaged in terms of luring federally transferred resources into municipalities. The results suggest that this is not the case. After the release of the audit reports, municipalities with reported corruption receive fewer transfers even before the October 2004 municipal elections. Additional checks are discussed later.

The second part of the analysis tries to disentangle the possible channels that drive the electoral punishment. Corruption, transfers, and electoral outcomes might all be correlated. Therefore disentangling the effects of transfers and corruption on electoral outcomes is not a trivial task. However, by exploiting some particularities of the Brazilian institutional environment and the timing of transfers, it is possible to address this issue. Checking the timing of the central government punishment (timing of when transfers are decided and executed) is crucial to understanding the methodology used to identify the channel. Transfers are decided and executed at the end of the Brazilian fiscal year (December). By using the data on budgetary amendments mentioned above, I found that there are some bureaucratic snags in the central government punishment process. By taking the difference between the amount authorized by legislators and the amount actually paid to each municipality, it is possible to control for unobservables, which guarantees the credibility of the results. Voters in municipalities with audit reports released in the last months of the fiscal year do not have time to have the amount of transfers reduced in the year in question; municipalities with audit reports released in the last months of 2003 (the fiscal year ends in December) had their transfers reduced only in December of 2004, after the October 2004 municipal elections. Note

that because of the bureaucratic snags in the punishment process by the CG (reduction in transfers), only municipalities whose transfers were reduced earlier in 2003 had enough time to perceive the consequences of the reduction in transfers before the October 2004 municipal elections.

Subsequently, I check the average effects corruption released on the electoral outcomes. Following Ferraz and Finan (2008), municipalities in which the release of the audit reports occurred before the 2004 municipal elections are the treatment group. The control group is composed of municipalities in which the disclosure of corruption information occurred after the election. The results suggest that the release of the audit reports, on average, has a detrimental impact on the probability of re-election of mayors who were revealed to be corrupt before the October 2004 municipal elections. A surprising new result is that voters do not punish corrupt mayors who are affiliated with the political party of the president. Note that Solé-Ollé and Sorribas-Navarro (2008a) found that intergovernmental grants allocated to co-partisans buy more political support in Spain.

It is reasonable that the loss of reputation channel due to the dissemination of corruption information among voters should fade with time: corruption evidence that is still "fresh" in the voters' memories should be stronger in municipalities with audit reports released closer in time to municipal elections. Thus, if the only source of electoral punishment is monitoring from the central government (cutting resources for infrastructure projects in municipalities with mayors revealed to be corrupt), those municipalities that were examined as a result in the first lotteries, with audit reports released long before the October 2004 elections, had more time to be punished by the central government before the municipal elections. Therefore they likely experienced more negative impacts on the probability of re-election of incumbent mayors. Conversely, if the punishment evolved only the dissemination of corruption information at the local level through the reputation channel because corruption evidence that is still "fresh" in the voters' memories, the electoral punishment should be driven by those municipalities in which the audit reports were released closer in time to the 2004 municipal

elections[5].

In order to verify the effects of the release of the audit reports according to the timing of the audit release, dummy variables that denote the distance in time (number of months) from the release of the audit reports to the municipal elections are considered in the analysis. I found that the release of corruption information has a great negative impact on an incumbent mayor's probability of re-election (and on that of a political party) in municipalities with audit reports released close to the time of municipal elections. However, this effect (loss of reputation channel) gradually fades with time. The impact of the release of the audit reports on electoral outcomes fades after six months. Then, the results suggest that when the effects of loss of reputation on the part of politicians due to the dissemination of corruption information among voters have completely disappeared, voters punish corrupt politicians as a consequence of the reduction in transfers.

An additional evidence that ensures the channels are actually disentangled is presented. It is reasonable to think the effects of the dissemination of corruption information should be lower in municipalities with no local radio stations. Note that local radio station is an important source to disseminate information at the local level in Brazil where almost 40% of the population above the age 20 is illiterate. Therefore, considering only the sample of municipalities where local radio station is not present we should expect that municipalities no effects in municipalities with audit reports release close in time to the elections. Actually, all the negative effect of the corruption disclosure on the probability of re-election of an incumbent political party is driven by those municipalities with audit reports released at least 15 months prior to the elections[6].

This article is organized as follows. Section 2 discusses the related literature; Section 3

[5]Possible channels are discussed more in detail in the conclusion of this paper.

[6]I provide an additional evidence that negative effect of corruption release on reelection outcomes in municipalities with the release of the audit reports 15 months prior to election seems to be due to the reduction in transfers. When the amount of transfers received after the release of the audit reports are interacted with these dummies that denote the distance in time, transfers capture the effects of the release of corruption only in municipalities where the audit reports are released 15 months prior to the elections.

reviews the Brazilian institutional context; Section 4 explains the data employed; Section 5 presents the estimation strategy and results. Finally, concluding remarks are provided in Section 6 .

2 Related Literature

Empirical literature that analyzes the behavior of voters in the presence of information on corruption is rare due to the difficulty of measuring corruption, especially at the local level and particularly in developing countries[7]. Using evidence from the Brazilian audit reports, Ferraz and Finan (2008) show that the probability of reelection of mayors decrease when they are found to be corrupt before the elections, and that this effect is positively associated with the presence of local radio stations[8]. However, Stromberg (2004) provides evidence that governors allocated more funds to areas where a larger share of the population owned radios. Additionally, Besley et al (2006) argue that media effectiveness depends on the extent to which the media is controlled by politicians or social elites. When redistributive policies are also taken into account, evidence of the effect of corruption information on political selection is even scarcer[9]. On the other hand, there is evidence regarding the effect of intergovernmental transfers on the probability of re-election of incumbent mayors. Brollo et al (2009) find that constitutional federal transfers have positive effects on rent-seeking, attract politicians of lower quality, and increase the probability of re-election of incumbent mayors in Brasil.

[7]At the aggregate level, most studies rely on indices of corruption perception (See Person et al. (2003), Kunicova and Ackerman (2005), Chang and Golden (2004, 2007), and Picci and Golden (2007)). Most empirical studies that analyze voter behavior and corruption information consider accusations as a measure of corruption (See Peters and Welch (1980) for a U.S. case and Chang (2005) for an Italian case).

[8]With the same data, Ferraz and Finan (2007) have found that second term mayors are more corrupt than first term mayors

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

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

In a border sense this study is associated with the literature on corruption, decentralization, and accountability in developing countries[10]. In developing countries, the risk of losing office is reduced, and local governments might be vulnerable to practices such as capture by special interest groups (Bardhan and Mookherjee 1999, 2005, 2006 and Sonin, 2003). This argument is reliable in the Brazilian context where most municipalities are strongly dependent on federal transfers as a source of revenue, the illiteracy rate is high, and political clientelism is present.

Evidence provided by this study also contributes to the debate on whether extensive information provision is good for voters, Besley and Smart (2007)[11] show that better information about government tends to reduce discipline and increase first-term rent-seeking. However, better information also improves selection as bad incumbents are less likely to be re-elected. However, in some circumstances, additional issues should be taken into account. Take, as an example, a developing country with a high degree of decentralization and political clientelism where local corruption is positively associated with poorer regions. In this case, an anti-corruption program that changes the allocation of federal resources may create additional undesirable effects in terms of the welfare of voters.

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

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

3 Institutional Context

3.1 Decentralization of Social Programs and Infrastructure Goods and Services Delivery

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

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

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

The focus of this study is federal transfers devoted to infrastructure projects which represents 15% of the municipal total expenditure with infrastructure projects. The allocation process of these transfers is the most discretionary one and are used to financed projects that are directly visible to voters. They are related to projects that consider the construction of houses and bridges, the paving of roads, the building of systems of water and sewer linkage,

the purchase of ambulances, etc. Figure 1 illustrates the Evolution of constitutional transfers, infrastructure transfers and infrastructure expenditures (in R$ 1,000,000) during the period 2000-2006.

Note that these infrastructure transfers are the very discretionary and present peaks during the electoral years(2002 and 2006 - federal elections; 2000 and 2004 - municipal elections). Note also that infrastructure expenditures follow exactly the same trend of infrastructure transfers, suggesting that they are important to determining differences in the level of expenditures with infrastructure projects (most visible to voters) at the local level. Budgets of Brazilian municipalities are limited. Most constitutional transfers are tied to current expenses regarding education and health services, such as the payment of salaries. Discretionary transfers that refer to the delivery of infrastructure projects can make a difference. On average, 78% of the total amount of infrastructure transfers is discretionary. The accomplishment of these discretionary transfers depends on the completion of agreements, known as *CONVÊNIO*.[12]. These are agreements between the central government and the local administration. They are proposed by the interested party (municipal administration) to the title-holder of the ministry or the body responsible for the program. These *CONVÊNIOS* that finance infrastructure projects are directly related to budgetary amendments, that is, their realization depends on the execution of federal budgetary amendments. See section 4 for obtaining more details regarding the sources of the data.

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

[12] *Instrução Normativa do Supremo Tribunal Nacional*, n^o 1, 15 January 1997.

receives the audit reports, the information on the violations contained in the audit reports should be inputted into the federal government's financial system (SIAF). The managers of these agreements in each ministry are also subject to audit procedures. When there is evidence of corruption evidence (especially evidence of fraud, irregularities in the procurement practice process or the diversion of funds) auditors should implement procedures to evaluate whether the flow of transfers can be re-established. These procedures are complex and can take time to be completed. If the violation is due to the former administration, the current mayor should request to auditors (TCU) the instauration of this procedure in order to re-establish the flow of transfers or to carry out new agreements.

In addition, these agreements require the convening of a council comprised of members of the community who do not belong to the local administration[13]. Information about the completion of the agreements is published on the Internet as well as in the *Diário Oficial da União* (the official government records). Payment can occur outright or in installments, depending on the project specifications. In cases in which installments are used, subsequent payments are conditional on accountability with regard to the previous installment. These agreements originate from the Brazilian Fiscal Budget and, in most cases, they depend on the federal deputies' collaboration in terms of proposing amendments to the federal budget.

3.2 Budget Process

In order to analyze the allocation of discretionary federal transfers, it is crucial to understand how the legislative bargaining process works at the federal level. The executive branch controls the establishment of all budgetary laws. Basically, all budgetary laws are subject to having amendments added by legislators. In most cases, the municipalities that will receive

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

the grants are chosen by the legislators. The bulk of the proposed amendments include benefits to local areas, trying to bring the pork home. Amendments can be proposed on an individual basis, by state or region, and by the parties. There are limits for both the number and the value of the amendments proposed[14]. The Budget Committee is responsible for the authorization of the bill. After a period of discussion, Congress votes for the budgetary law, which is then sent to the President for the final decision. As budget is not mandatory in Brazil, the President has a major role in deciding the allocation of the discretionary transfers, and he can use them to make congressmen follow the guidelines of the government coalition.

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

Section 6 explains this three-phase process, which attributes different values to laws associated with the budget process: 1) the value of the budgetary law initially approved in the Brazilian annual budget; 2) the value of budgetary amendments later authorized by the Budget Committee, which can be less than, equal to, or greater than the value previously approved; and 3) executed budgetary amendments, which can only be equal to or less than the value authorized by the legislative branch.

At the end of the day, the voters of a municipality will receive discretionary transfers depending on three factors: (i) The effort of their municipal administration in applying for

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

these transfers; (ii) the interests of a federal congressman in supporting the municipality; and (iii) the interest of the President in executing the budget amendment (that is, send the money exactly to that municipality).

3.3 The Brazilian Anti-Corruption Program

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

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

This study considers 784 municipalities randomly selected through the first fifteen lotteries. The bad administration or corruption occurrences reported related to the municipal administration that had been in power during the period from 2001 to 2004. Figure 3 provides information about the number of municipalities by timing of the release of the audit reports. The horizontal axis denotes the distance in time (number of months) from the release of the audit reports to the municipal elections. The vertical axis denotes the number

of municipalities whose audit reports had been released. In the first lottery 26 municipalities were selected. These municipalities had their audit reports released 15 months prior to the municipal elections. The black vertical line represents the timing of the elections. There were four lotteries (176 municipalities) in 2003; five lotteries (260 municipalities) in 2004; and six lotteries (360 municipalities) in 2005. Audit reports for a total of 376 municipalities had the audit reports released before the October 2004 elections (before tiny black line), and reports for 410 municipalities were released after the elections.

4 Data

4.1 Measuring Corruption using the Audit Reports

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

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

Summary statistics for municipal characteristics and corruption variables are reported in Table 1. Panel A presents the descriptive statistics of the number of corruption violations reported by year of the release of the audit reports. Note that the % of reported violations slightly increases over time. Auditors check the allocation of federal transfers from 2001 up to the time of the audit. We might expect that the auditors would find more violations over time as mayors also had more time to commit irregularities. That is exactly what is revealed in the data[15]. Of the 784 municipalities reviewed in this study, 79% reported at least one

[15]Ferraz and Finan (2008) tested for the possibility of mayors who were politically affiliated with either the federal or state governments receiving more favorable audit reports. They did not find any evidence of

violation, and 30% reported at least three violations. Only 3.5% of the sample presents more than five irregularities associated with corruption.

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

4.2 Transfers and Political Party Data

Data on infrastructure transfers is obtained on the Brazilian National Treasure Website (*Tesouro Nacional*) –*FIMBRA* dataset, which provides information from municipal and state annual balance sheets about assets, liabilities, revenues, and expenditures for all Brazilian municipalities and states. This are self-reported data from municipalities and organized by the Brazilian National Tresure. Only since 2002 it has been possible to distinguish discretionary from constitutional transfers that finance infrastructure projects. For consistency, the period of analysis (1999-2006) considers the overall amount of federal infrastructure transfers. Note that, on average, 78% of the total amount of infrastructure transfers is discretionary (*CONVENIO* agreements for infrastructure projects). More precisely, the left-hand side variable used in the first part of the analysis is the log of the per-capita total amount of infrastructure transfers[16].

that. They also show that there is no statistically significant differences between the sample of municipalities audit before and after the 2004 elections

[16]For those municipalities that do not receive infrastructure transfers, are not dropped when I run the log specification. For the log transformation I considered a reported amount of R$ 1,00, then, the log amount is

Most part of (*CONVENIO* agreements for infrastructure projects are subject to the execution of federal budgetary amendments. Amendments can be proposed on an individual basis, by state or region, and by the parties. Data on budgetary amendments were obtained on the Brazilian Senate Website. This study considers only individual amendments which represents 27% of the value of the total amendments because only data on individual budgetary amendments makes possible the identification of the municipalities that receive the benefits. These individual amendments are en-marked with the name of the deputy. This data contains information about entire budget process: value of the budgetary law initially approved by the president; value of the budgetary law amendment later authorized by the legislative branch; and value of the budgetary law amendment executed (paid) by the executive branch. This study exploits these 3 different values according to each phase of the budget and the variables constructed follow the same log transformation criteria and deflator as described above.

Information about mayors' characteristics, such as party affiliation was obtained in the survey *Perfil dos Municipios Brasileiros* for the years 2002 and 2004. For the years 1999, 2000, 2001, 2005 and 2006 data on mayors party affiliations were obtained from the Tribunal Superior Eleitoral (TSE)[17]. The Brazilian Institute of Geography and Statistics (IBGE) elaborates this database.

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

zero. All budget variables are in real values, base year 2006 (IPCA - FGV deflator.

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

5 Estimation Strategy and Results

5.1 Punishment by The Central Government – Reduction in Transfers

This analysis is divided in two main parts. The first part of this section analyze the effect of the release of the audit reports on infrastructure transfers and starts in Section 5.1.1 that explains the estimation strategy used to estimate the effects of the release of the audit reports on infrastructure transfers. Section 5.1.2 exploits three different stages of the federal budget execution process in order to check that reduction in transfers is due to punishment by the federal executive branch. Section 5.1.3 analyze the effect of the audit reports on other municipal accounts. Following this, section 5.2 presents the second part of the analysis, allowing for disentangling the two channels: loss of reputation on the part of politicians and reductions in transfers by the central government. The results are presented after each model specification.

5.1.1 The Effects of Released Audit Reports on Transfers

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

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

are estimated as:

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

where the left-hand side of the equation is the log of the total per-capita amount of infrastructure transfers received by the municipality i in the year t. The variable A_{it} denotes the timing of the release of the audit reports and is equal to 1 in the year of the audit event and in subsequent years. Note that A_{it} varies not only across municipalities and within the years after the beginning of the anti-corruption program (2003, 2004, and 2005), but also across years within municipalities. This variable interacts with dummies that denote the level of corruption reported in the audit. H_i is equal to 1, if no violations are reported; F_i is equal to 1, if one or two violations are reported; and M_i is equal to 1 if more than two violations are reported[18] Note that these variables are time invariant and they refer to level of corruption of the municipal administration during the 2001-2004 mandate. The time-varying control W_{it} is equal to 1 if a mayor is affiliated with the political party of the president: PT from 2003 onwards and PSDB before it[19]. Year fixed-effects τ_t and municipal fixed-effect η_i, are also included. The coefficients of interest are β_1, β_2 and β_3; they measure the effect of the audit reports on the amount of infrastructure transfers for the three different levels of corruption reported. Under the assumption that $E(e_{it}|A_{it}, \eta_i) = 0$, $E(e_{it}|(A_{it} * H_i), \eta_i) = 0$, $E(e_{it}|(A_{it} * F_i), \eta_i) = 0$ and $E(e_{it}|(A_{it} * M_i), \eta_i) = 0$, the fixed effect estimator is consistently estimated.

I started this analysis by considering unbalanced Panel with data from 779 audited municipalities and eight years of observations to show that after the release of the audit reports, transfers decreased as the level of reported corruption rose. The results are presented in Table 2. Regressions displayed in columns 1, 2, 3, 4, 5 and 6 consider all audited municipalities. Columns 7, 8, 9 and 10 include all municipalities in the anti-corruption program, regardless

[18]For those municipalities audited more than one time, I have considered only the first audit. Only 12 municipalities was audit more than one time and had more than one audit report.

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

of whether they were audited[20]. The regressions displayed in columns 1, 2, 3, 4, 7 and 8 include the period 1999-2006. In order to verify whether the reduction in transfers started before the elections and after the release of the audit reports during the mandate 2001-2004, regressions displayed in columns 4, 5, 6 and 7 include only the 2001-2004.

Column 1 displays the results of the model specified above when the interaction terms are not considered. The amount of infrastructure transfers decreased significantly, by 25.2% (e^{β}-1), after the release of the audit reports. Since 79% of these municipalities have at least one corruption violation reported, the number of violations reported should matter. Column 2 presents a surprising result. Municipalities in which the mayor is affiliated with the president's political party were compensated after the release of the audit reports. The results of the regressions reported in the remain columns consider a semi-parametric specification with three different levels of corruption reported that are interacted with A_{it}: *no violations* (= 1 if no violation is reported); *few violations* (= 1 if 1 or 2 violations are reported); *many violations* (= 1 if at least three violations are reported). In column 3 (audited sample), the effects of the release of the audit reports increases in magnitude as the degree of corruption rises. After the audit event, for municipalities with many corruption violations reported, transfers decrease by 59.4%(e^{β}-1) (standard error 0.122))[21]. Note that in column 4, the coefficients of the interactions terms between A_{it}*H_i**President's party*$_{it}$ and A_{it}*F_i**President's party*$_{it}$ are not statistically different.

The results in columns 5 and 6 (only the period 2001-2004 is considered) are similar to those reported in columns 3 and 4. Because only the years during the electoral mandate 2001-2004 are considered, these results indicate that the reduction in transfers after the release of the audit reports started before 2005. The political selections induced by the release of the

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

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

audit reports (audits increase turnover, resulting in more first-term mayors) could drive the results if first-term mayors, elected in October 2004 were disadvantages in terms of obtaining federally transferred resources during their term of office. However, the results reported in Table 2 show that this does not to seem the case. Municipalities whose corruption information had been released had their transfers reduced even before the municipal elections.

However, these results would be compromised if unobservable time-varying variables are correlated with the timing of the audit and the number of violations reported. This is an important concern as mayors have to apply for these discretionary transfers. therefore, we need to make sure that during the years following the audit there was no change in the demand for transfers. Additionally, the President Lula's first administration started in 2003 (the year when the Brazilian anti-corruption program was launched). It could be that since the beginning of Lula's administration, the amount of transfers to municipalities affiliated with the party in opposition to Lula has been decreased as compared to before he took office. Opposition parties had also been part of the governing coalition during the two consecutive previous presidential terms (Fernando Henrique Cardoso, "FHC",1994-2002). Because corruption is likely correlated with transfers, it could also be associated with mayor's political party affiliation.

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

Therefore the fact that the change in the governing party at the federal level might be correlated with the timing of the release of the audit reports possibly biased the estimates when only the audit sample is consider. However, the results remain robust.

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

Additionally, in order to verify whether the demand for transfers decreased following corruption disclosures I employed data regarding three different phases of the federal budget process: approval, authorization and payment. There is no evidence that reduction in transfers is driven by a reduction in the amount applied by municipal administrators. The results are shown in Section 5.1.2 (Table 4) and also in Section 5.2.1 (Table 6).

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

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

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

5.1.2 Checking for Punishment by the Executive Branch

As explained in Section 5.1.1,most of the discretionary transfers depends on the execution of federal budgetary amendments. by exploiting different stages of the accomplishment of the budget process we can proceed with some additional checks. First, it would be interesting to see in which legislators and the president are punishing corrupt administrations at the municipal level. In the case in which phase of the budget process, punish had been occurred.

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

This data brings information of the month of payment (execution) of the budgetary amendments. However, there is no information available regarding the month in which the approval and authorization of each amendment took place. According to the description of the budget process in Section 5.1.2, before being executed (paid), the amendment has to be approved by the executive branch, and then, subsequently authorized by the legislative branch.

Using a semi-parametric specification similar to the one described in Section 5.1.1, I verify the effect of the reported corruption on the three variables APP_{it}, AUT_{it} and PAID_{it}. Then, I estimate the effects of the release of the audit reports on the budgetary amendment execution process. The right-hand side variables considered are the interactions between A_{it} and the dummy variables denoting the number of violations reported.

Table 4 shows the results when Audit_{it} equals 1 in the same month and year as the release of the audit reports, and in the months thereafter, and also in subsequent years[22]. For instance, if a budgetary amendment for a given municipality was executed (paid) in the same year and one month before of the release of the audit report, Audit_{it} equals 0. The regressions displayed in columns 1, 2 and 3 consider only the sample of audited municipalities. The results of the regressions that consider all municipalities are reported in columns 4, 5, 6, 7, 8, 9 and 10. The dependent variable in the regressions reported in Columns 1 and 4 is APP_{it}; in columns 2 and 5 it is AUT_{it}; and in columns 3, 6, 7, 8, 9 and 10 it is PAID_{it}. The results of regressions that consider a semi-parametric model are displayed in the first six columns. Columns 7 and 9 report the results of a linear model and columns 8 and 10 consider a quadratic specification. Columns 9 and 10 do not include municipalities with more than five corruption violations reported.

The results shown in columns 3 and 6 suggest that the log of the value of the budgetary amendment executed (paid) is significantly reduced to municipalities with two or three corruption violations reported (estimate points -0.262 and -0.160; respectively) after the release of the audit reports. Note that the results reported in columns 1, 2, 4 and 5 do not capture the timing of the bill approval or authorization. Although not reported, I also tried a different specification in order to check for these possibilities. In this case, the variable Audit1_{it} is considered. This variable equals 1 only in the years subsequent to the release of the audit report. With this specification it is possible to verify whether the legislative branch is authorizing less for those municipalities with many corruption violations reported. Moreover,

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

it is also possible to check whether corrupt local mayors reduce the degree of effort that they exert in applying for transfers after the release of the audit reports. In this specification, the release of the audit reports also has no effect, for any level of corruption reported, on the per-capita amount of the budgetary amendment approved and further authorized by the legislative branch[23]. However, the coefficient of the interaction term $Audit1_{it}$*n. of violation = 0* is significantly and positive when PAID_{it} is the dependent variable. When all municipalities are considered, the interaction term $Audit1_{it}$*n. of violation = 2 or 3* has a statistically significant detrimental effect on the value paid. The results are available upon request. Additionally, the results reported in columns 7, 8, 9 and 10 suggest that the quadratic specification fits the data well. This result is an important check for the specification chosen in Section 5.2.1.

5.1.3 Checking for Effects on Other Municipal Accounts

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

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

Columns 1, 2, 3, and 4 displays the results for the audit sample of municipalities while

[23]Sections 5.1.1 (when all municipalities, audited and non-audited, are included in the regressions) and **??** also provides additional evidence that this reduction in transfers does not seem to demand-driven.

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

*Audit*many violations* is statistically significant at 10%. However, despite of the fact that is a border line result, note that the point estimates of the interaction terms *Audit*no violations* and *Audit*many violations* are not statistically different from each other.

5.2 Disentangling the Channels

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

I start checking for the timing in the central government punishment (Section 5.2.1). Subsequently, a briefly explanation regarding the estimation strategy to analyze the effects of the release of the audit reports on the electoral outcomes is provided. Then I check which model more appropriately fits the data collected (Section 5.2.2). Finally, I discuss the estimation strategy to identify the mechanisms through which the release of the audit reports affects the municipal electoral outcomes (Section 5.2.3).

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

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

Please note that this check requires cross-section data. By comparing to different sample of municipalities audited in a given year, if is possible to check for bureaucratic snags. The first sample considers all municipalities audited in a given year. The second consider only municipalities with audit released not after July. I used data for the year 2005 because there were few observations for individual budgetary amendments for the years 2003 and 2004. But there is no reason to think that this could compromise the validity of the results. By taking differences between the per-capita values of the budgetary law amendment authorized (AUT) and executed (PAID), it is possible to control for any unobservable municipal characteristics that are constant within the time between the authorization and payment process. Then, a quadratic specification of the number of violations reported, which is justified from the

results of the semi-parametric specification reported in Section 5.1.2 - Table 4, is employed.

Table 6 reports the results. Panels A and B show the results for municipalities with audit reports released from January to October 2005 and from January to July 2005, respectively. Note that the coefficients of the variable that denote the number of corruption violations reported are positive, but not statistically significant in all regressions that consider APP_i (column 1) and AUT_i (columns 2) as dependent variable. However, the results in column 3 of Panel B indicate that the value paid decreases as the number of violations rises. Therefore the difference between AUT and PAID should increase. The effect of the coefficient of the difference between the per-capita values of the budgetary law amendment authorized and executed is positive and statistically significant at 5%, and it is stronger in Panel B.

These results suggest that municipalities with corruption information released close in time to the end of the Brazilian fiscal year (December) did not have their transfers reduced in that year. The results displayed in column 4 are also an additional check for the argument that the reduction in transfers to unveiled corrupt mayors might be demand-driven. Following this, voters in municipalities with audit reports released in December 2003 (10 months before the municipal elections), October 2003 (12 months before the municipal elections), and September 2003 (13 months before the municipal elections) had their transfers reduced only in December 2004, after the municipal elections in October 2004. Therefore, only voters in municipalities with audit reports released at least 15 months prior to elections could have had time to perceive the consequences of reductions in transfers[24].

5.2.2 The Average Effect of Corruption Disclosure on Electoral Outcomes

I first briefly explain the strategy to estimate the average effect of corruption disclosures on municipal electoral outcomes similar to that of Ferraz and Finan, 2008. Then I show the results and choose the specification of the model that better fits the data collected. Additionally, heterogeneous effects according to mayors' political party affiliations are included in the

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

analysis. The exogenous variation in the timing of the release of the audit reports allow us to consistently estimate the effect of corruption disclosures on the probability of re-election of an incumbent mayor. The treatment group are those municipalities where the release occurred before the municipal elections. the control group are those with audit released after the elections. This effect is estimated as following:

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

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

Brazilian mayors can run for state or federal elections during or after their first term. Taking this fact into account, I also consider the probability of re-election of a political party (including the party electoral coalition) as a dependent variable. In this specification, R_i is not necessarily equal to 0 in cases in which a mayor was eligible for re-election, but did not stand for election. Considering only municipalities in which the mayors are eligible for re-election, this variable is equal to 1, if the incumbent political party or its electoral political party's coalition was elected in the 2004 municipal elections. Note that only Brazilian municipalities in which mayors are eligible for re-election are considered[25]. In 2000, for the first time,

[25]Taken into account only the sample of municipalities where the mayor is non-eligible for re-elections (two terms limit, in Brazil) I tested whether this audit reports have an effect only the probability of re-election of

Brazilian municipal mayors were allowed to run for two consecutive terms. Therefore, in 2004, 40% of the audited sample was not eligible to run for re-election.

Tables 7 and 8 display the OLS estimates when the probability of re-election of the incumbent mayor and incumbent political party is the dependent variable, respectively. Table 7, columns 1, 3, and 5 consider a linear specification. The results of a quadratic specification are displayed in columns 2 and 4. Column 6 presents the results for a semi-parametric model. Columns 3, 4 and 5 do not include municipalities with more than five corruption violations reported (less than 4% of the sample). All regressions include state dummies, and municipal and mayoral characteristics. When all audit municipalities are considered, according to the results for the linear model reported in column 1, the release of the audit reports has no effects on the electoral outcomes. On the other hand, with the quadratic specification reported in column 2, the point estimate of the interaction term $Audit_i * C_i$ is -0.220 (p-value 0.106). However, when excluding municipalities with more than five corruption violations (column 3), the point estimate of the interaction term $Audit_i * C_i$ is statistically significant (estimate point -0.108, standard error 0.058) in the linear specification. On the other hand, when excluding these observations, the results for the quadratic specification are lost (column 4). Column 6 shows the results of a semi-parametric model. The estimate point $Audit_i * C_i = 3$ is -0.210 (standard error 0.125), is statistically significant at 10%. These results suggest that the quadratic specification simply captures some noise in the data.

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

the incumbent political party and I found no evidence.

municipal elections.

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

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

5.2.3 The Effect of Corruption Disclosure on Electoral Outcomes, by Timing of Release

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

Refreshing some important point, we have discussed in previous sections that transfers are decided on and paid by the central government at the same time, and this occurs mostly (Figure 2) at the end of the Brazilian fiscal year (in December). Note that transfers to municipalities with audit reports released in 2004 were mostly decided and paid in December. Municipal elections were held in October 2004. Additionally, from the results reported in Table 6 there might be some bureaucracy in the punishment by the central government. Therefore, only voters in municipalities with corruption reported in 2003, but not in the last months of the fiscal year, should have suffered the consequences of a reduction in the amount of transfers before municipal elections (October 2004).

It is reasonable to assert that the loss of reputation channel due to the dissemination of corruption information among voters should fade with time: corruption evidence that is still "fresh" in the voters' memories should be stronger in municipalities with audit reports released closer in time to municipal elections. Thus, if the only source of electoral punishment is monitoring from the central government (cutting resources of infrastructure projects in municipalities with mayors revealed to be corrupt), those municipalities that were examined as a result of the first lotteries, with audit reports released long before the October 2004 elections, had more time to be punished by the central government before the municipal elections. More precisely, according to the results discussed in Section 5.2.1, only voters in municipalities with audit reports released 15 months prior to the elections had time to perceive the effects of reductions in transfers before the elections. Therefore, they likely experienced more negative impacts on the probability of re-election of incumbent mayors. Conversely, if the punishment involved only the dissemination of corruption information among voters through the reputation channel, those municipalities in which the audit reports were released closer in time to the 2004 municipal elections should have experienced a more negative effects as a result of the audit reports on incumbents probabilities of re-election[26]

The model is specified below. Municipalities in which the release of the audit reports occurred before the 2004 municipal elections are the treatment group. The control group is

[26]I discuss other possible interpretations for the channels in the conclusion of this paper.

composed of municipalities in which the disclosure of corruption information occurred after the election. In order to test how the effects of the audit reports vary according to the timing of the release, the specification below includes dummy variables that denote the distance in time (number of months) from the release of the audit reports to the municipal elections for those municipalities with audit released before the elections. Therefore the effects of the pre-election release of the audit reports (treatment group) according to the timing of the audit release, are estimate as following:

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

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

where R_i is equal to 1, if the eligible incumbent political party (or mayor) in the municipality i was re-elected during the 2004 municipal elections. D_{ji} are dummy variables that denote the "distance" in time (number of months) from the release of the audit report to the municipal elections in the municipality i. For instance, $D_{15,i}$ is equal to 1 if the release of the audit reports occurred 15 months before the elections. Note that these dummies encompass all municipalities with audit reports released before municipal elections in October 2004 {0, 2, 6, 8, 10, 12, 13 or 15 months to elections}. The variable C_i is the number of corruption violations reported in a municipality i. Then the coefficient β represents the effect of the release of the audit reports of every additional corruption violation reported on mayors' probabilities of re-election, according to the timing of the release. W_i is a vector of mayoral and municipal characteristics, z_s denotes state fixed effects, and e_i is the error term. Under the assumption that $E(\sum_j D_{ji}|e_i) = 0$, the coefficients are consistently estimated.

Considering the dummies that denote the distance in time from the release of the audit reports to the municipal elections, the results of this linear model for corruption, but at the same time, semi-parametric in the timing of the audit release are illustrated in Figure 5 and reported in Table 9.

The dependent variable is the probability of re-election of a political party. The results suggest that revealed corruption has a negative effect on a political party's probability of

re-election in municipalities with audit reports released close in time to the October 2004 municipal elections. However, this effect seems to fade with time. For every additional corruption violation reported, the incumbent political party's probability of re-election decreases significantly, by 14% and 18%, respectively in municipalities with audit reports released two and six months prior to the municipal elections in October 2004 of the baseline re-election rate for the control municipalities. The point estimate of the interaction terms *C*8 months to elections*, *C*10 months to elections*, *C*12 months to elections*, and *C*13 months to elections* are: -0.046 (standard error 0.066), -0.094 (standard error 0.103), 0.069 (standard error 0.100) and 0.014 (standard error 0.099) respectively. The point estimate of the interaction terms *C*10 months to elections* and *C*8 months to elections* are still negative but not statistically significant and that 12 months prior to elections, the coefficient of the interaction term is positive. However, the release of the audit reports seems to have a stronger significant effect when it occurs 15 months prior to the elections[27]. In those municipalities, every additional corruption violation reported decreases the probability of re-election of the incumbent political party by 28% (standard error 0.097). The point estimated for *C*15 m. to elections* and *C*13 m. to elections*, as well as those for *C*15 m. to elections* and *C*12 m. to elections* are statistically different at the 0.0161 and 0.047 significance level (p-value), respectively. Additionally, H_0: *C*13 m. to elections=C*6 m. to elections* and H_0: *C*12 m. to elections=C*6 m. to elections* are rejected (p-values 0.047 and 0.050, respectively).

Figure 6 illustrates additional evidence that ensures the channels are actually disentangled is presented. It is reasonable to think the effects of the dissemination of corruption information should be lower in municipalities with no local radio stations. Note that local radio station is an important source to disseminate information at the local level in Brazil where almost 40% of the population above the age 20 is illiterate. Therefore, considering only the sample of municipalities where local radio station is not present we should expect that municipalities no effects in municipalities with audit reports release close in time to

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

the elections. Actually, all the negative effect of the corruption disclosure on the probability of re-election of an incumbent political party is driven by those municipalities with audit reports released at least 15 months prior to the elections. Note that the point estimate of the variable *C*15 m. to elections* is statistically different from all other interaction terms. These results remain even if we do not consider municipal controls in the regressions and (or) the dependent variable is the probability of re-election of an incumbent mayor (these results are available upon request).

In Table 9, the dependent variable in the regression displayed in columns 1, 2, and 3 is the probability of re-election of a political party. The regression in column 1 considers all audited municipalities in which the incumbent mayor is eligible for re-election. Columns 2 and 3 do not consider outliers (c>5). The results are very similar if only municipalities with more than seven corruption violations reported (less than 1% of the sample) are excluded from the analysis. All regressions reported in Table 9 include municipal and mayoral controls[28]. When the dependent variable is the incumbent mayors' probability of re-elections (columns 4, 5, and 6), the results are similar. Note that in the linear specification, the variable C is negative and not significant in almost all regressions (except in column 1).

The results suggest that when the effects of the dissemination of corruption information among voter had completely faded, voters punished corrupt politicians as a consequence of the reduction in transfers. Section 5.2.1 provides evidence that voters had time to feel the consequences of the reduction in transfers before the elections only in municipalities in which the audit reports were released 15 months prior to the elections. Additionally, in order to check whether this substantial effect of the estimate coefficient *C*15 months to the elections* is due to the reduction in transfers, after the release of the audit reports, one more additional procedure in included that is reported in column 3. The interaction between the dummies

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

D_{ji} *# of months prior to elections* and the variable T, which denotes the average amount of transfers after the audit release is also included in the regression. If transfers matter, this variable should capture the effects of the number of corruption violations reported. According to the results displayed in column 3, the coefficient of *C*15 months to elections* is no longer significant. The point estimate of *C*15 months to elections* is -0.157 (standard error 0.097). However, the point estimate of *C*6 months to the elections* is significant at 1% level (point estimate -0.187; standard error 0.068). Moreover, the interaction term *15 months to the elections*T* has a positive and significant effect on the probability of re-election (point estimate 0.018; standard error 0.008)[29]. The results presented in columns 4, 5, and 6, where the dependent variable is the probability of re-election of an incumbent political party (a measure with more noise) is considered are weaker but go in the same direction. Although not reported, the results are in the same direction when all sample of audited municipalities and a quadratic specification are considered.

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

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

6 Concluding Remarks

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

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

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

However, the idea that information fades as the distance in time from the release of the audit reports to the elections increases has more than one possible explanation. Mayors may start acting "strategically" (i.e., buying votes) in order to"cancel" the effects of the release

of the audit reports before the elections. However, these "strategies" might take time to be implemented (at least six months prior to elections). On the other hand, punishment by the central government could disable corrupt mayors and block them from implementing their strategies because of budget constraints. Moreover, there is evidence in the literature that voters "care" about transfers. Therefore the effects of the dissemination of corruption information on municipal electoral outcomes could be a consequence of voter expectations regarding future transfers. That is, it is not necessarily because of the level of corruption revealed (i.e., not does necessarily just because voters get angry when they know how corrupt the politician is). A piece of evidence that this could be the case is presented in Section 5.1.1. Voters are not punishing at the ballots corrupt politicians affiliated with Lula's political party. Onthe other hand, the CG increases the amount of transfers to municipalities where the mayor who is affiliated to the part of the president is found to be honest, it helps politically aligned municipalities with high levels of released corruption to move through the punishment process more quickly. Voters might be anticipating the increase in the amount of transfers in municipalities where the mayor is affiliated with the president's political party, PT. However, there could be factors that could explain this result. Voters may prefer to stick with corrupt mayors affiliated with PT for other reasons than transfers.

This paper leads to some important policy implications that can be taken into account in countries with high degrees of decentralization of the delivery of public goods and presence of political clientelism. Because poorer municipalities are positively associated with corruption, punishment by the central government with regard to municipalities with released corruption can compromise the welfare of voters. Releasing all audit reports 6 months before the elections, when the evidence of corruption is still fresh in voter's memory, could be a possible solution. In this case reduction in transfers are not required to promote political selection at the polls.

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

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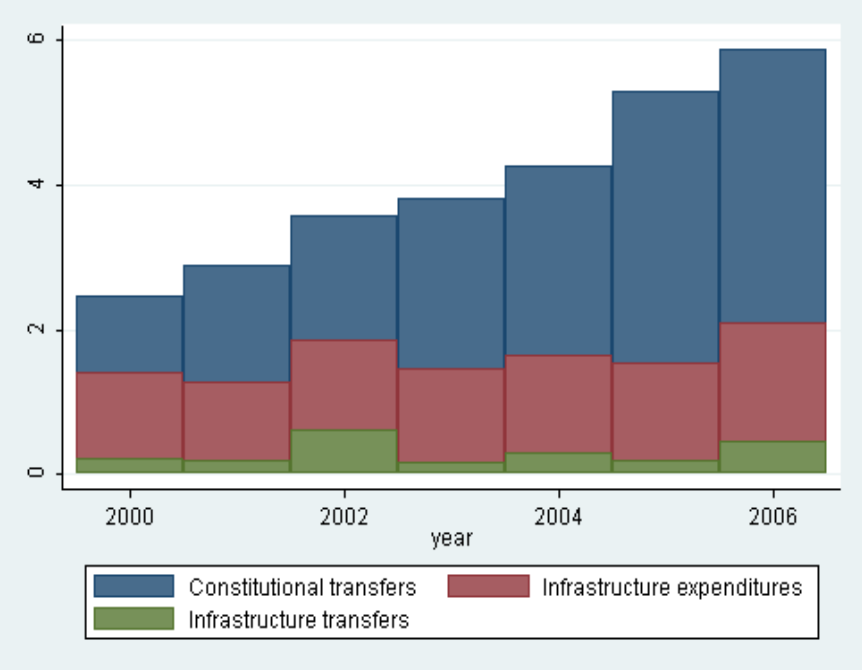
Figures and Tables

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



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

Figure 2 – Timing of federal budgetary amendment execution

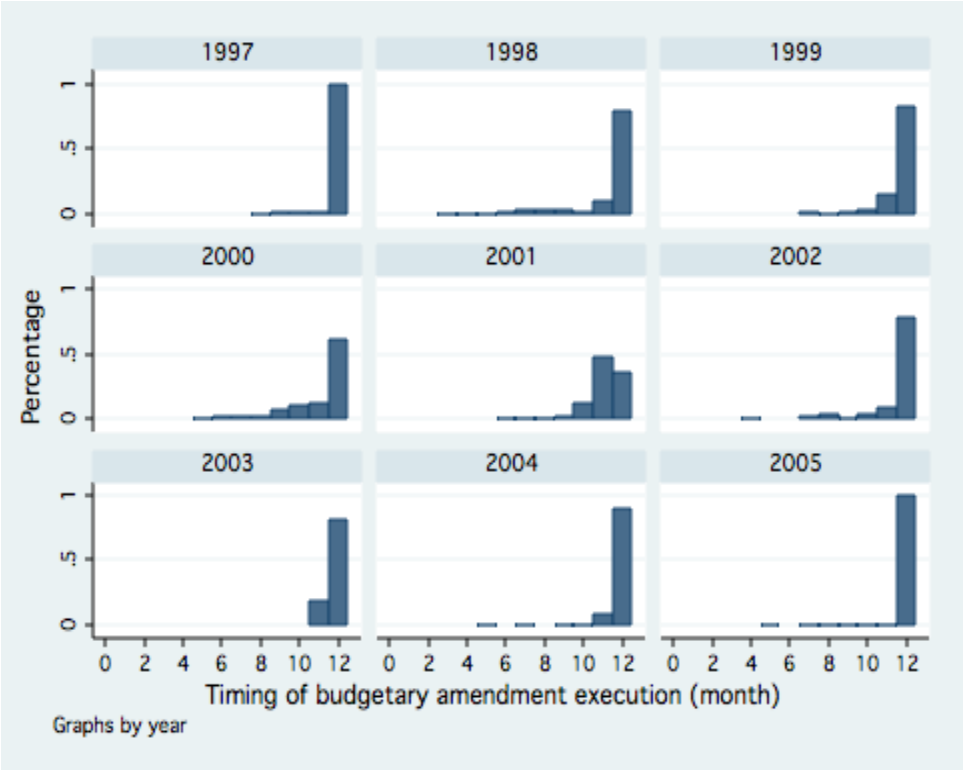


Figure 3 – Timing of the release of the audit reports and number of municipalities selected

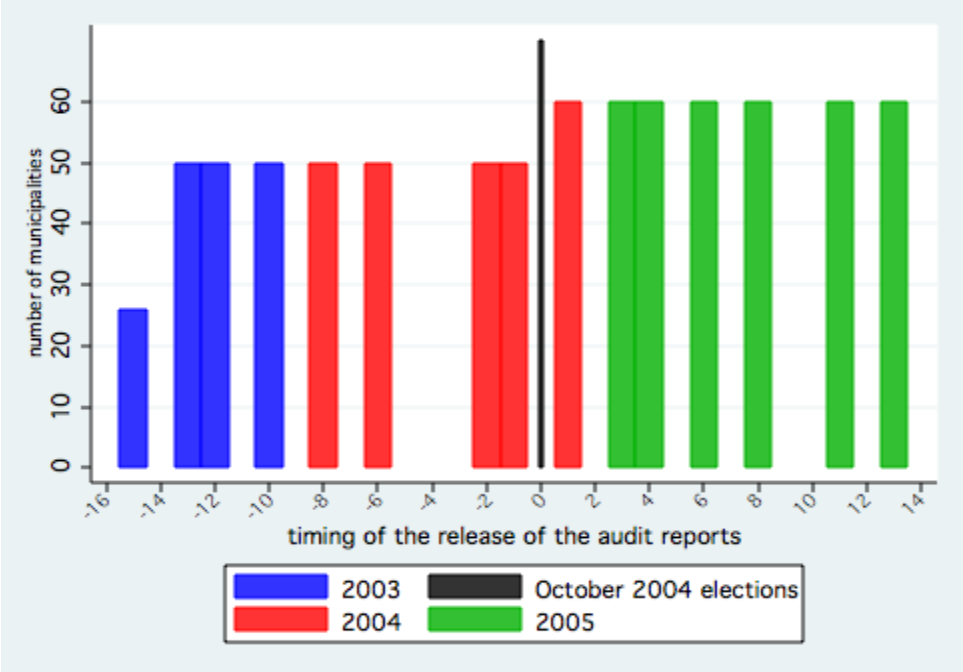
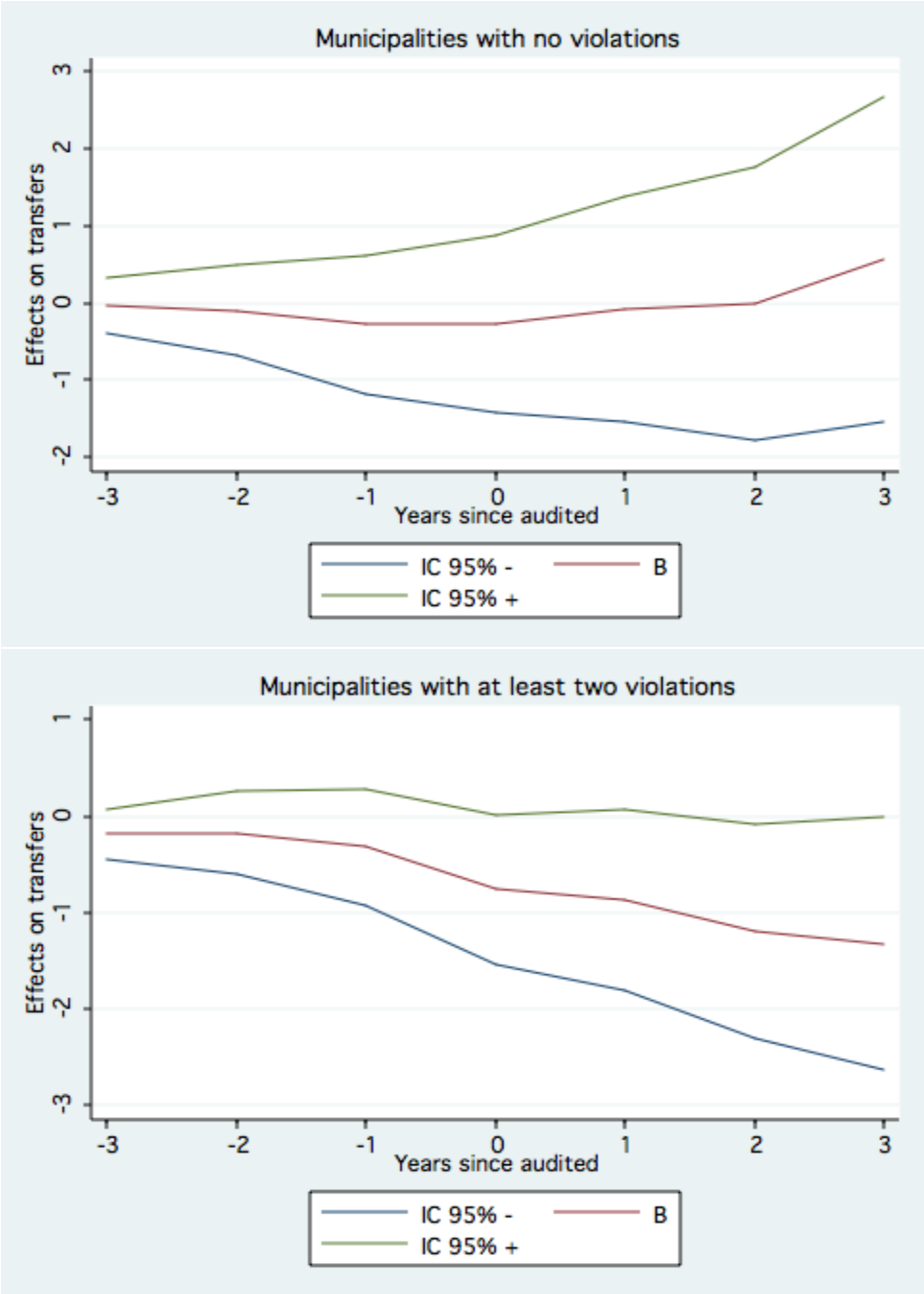
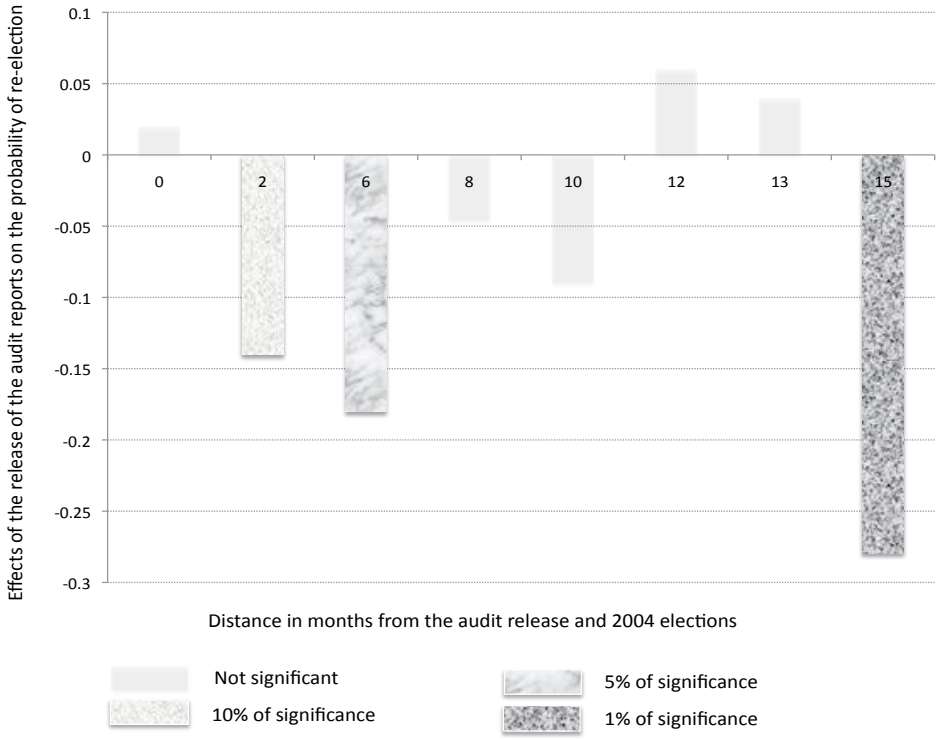


Figure 4 – The effects of the release of the audit reports on transfers



Note: These regressions are reported in Table 3

Figure 5 – The Effect of Corruption Disclosure on Electoral Outcomes, by timing of release



Note: This regression is reported in Column 2 of Table 9

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

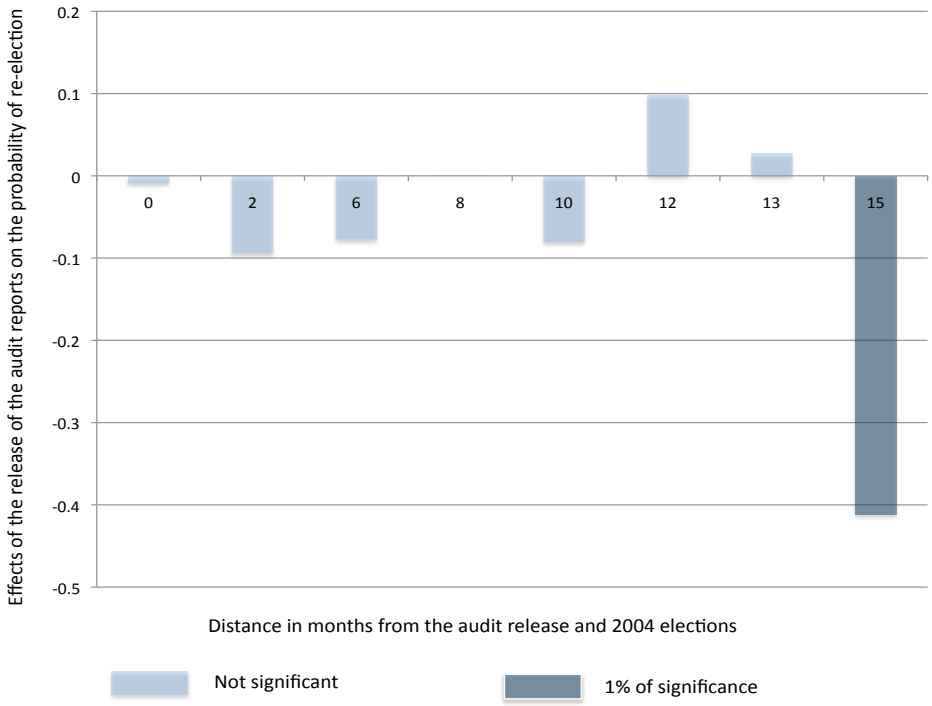


Table 1: Descriptive statistics

	(1)	(2)	(3)	(4)
Panel A: Corruption				
Sample	All audited	audited in 2003	audited in 2004	audited in 2005
Mean of number of violations	1.844	1.486	1.591	2.211
	(1.612)	(1.364)	(1.528)	(1.711)
% of municipalities with:				
Zero corruption violations reported	0.21	0.25	0.28	0.15
1 corruption violations reported	0.28	0.34	0.28	0.25
2 corruption violations reported	0.21	0.19	0.20	0.22
3 corruption violations reported	0.16	0.14	0.14	0.19
4 corruption violations reported	0.07	0.06	0.06	0.08
5 corruption violations reported	0.03	0.01	0.02	0.05
More than 5 corruption violations reported	0.04	0.01	0.03	0.05
Total number of municipalities	784	175	259	350

Panel B: Municipal characteristics by corruption category				
	Many violations=0	Many violations=1	Difference	P-value
Literacy rate (%)	0.828	0.759	0.069	**0.000**
Income (per-capita, R$)	172	121	0.000	**0.000**
Population	24,665	24,282	382	**0.900**
Persons living in urban areas (%)	61.5	56.1	5.4	**0.003**
Houses linked to the general system of water (%)	58.7	55.1	3.6	**0.083**
Houses linked to the general system of sewer (%)	21.9	15.2	6.7	**0.002**
Houses with electricity (%)	88.0	80.1	7.9	**0.000**
Tranfers (log, per-capita)	2.363	2.418	-0.056	**0.544**
Re-election rates for the 2004 elections (eligible) (%)	44.2	30.8	13.6	**0.045**

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Audited	Audited	Audited	Audited	Audited	Audited	All	All	All	All
Period	1999-2006	1999-2006	1999-2006	1999-2006	2001-2004	2001-2004	1999-2006	1999-2006	2001-2004	2001-2004
$Audit_{it}$	-0.225**	-0.242***								
	(0.088)	(0.089)								
$Audit_{it}$*Pres. $party_{it}$		0.518**								
		(0.224)								
$Audit_{it}$*$no\_violations_i$			-0.118	-0.179	-0.103	-0.128	0.107	0.048	0.009	-0.006
			(0.120)	(0.120)	(0.155)	(0.155)	(0.098)	(0.097)	(0.144)	(0.143)
$Audit_{it}$*$few\_violations_i$			-0.165*	-0.161	-0.140	-0.188	0.065	0.069	-0.018	-0.056
			(0.100)	(0.101)	(0.120)	(0.122)	(0.072)	(0.072)	(0.105)	(0.107)
$Audit_{it}$*$many\_violations_i$			-0.466***	-0.493***	-0.505***	-0.492***	-0.232**	-0.261***	-0.388**	-0.367**
			(0.122)	(0.124)	(0.184)	(0.187)	(0.094)	(0.097)	(0.172)	(0.175)
$Audit_{it}$*$no\_violations_i$*Pres. $party_{it}$				0.998***		0.463		0.806**		0.267
				(0.355)		(0.698)		(0.348)		(0.685)
$Audit_{it}$*$few\_violations_i$*Pres. $party_{it}$				0.123		1.276***		-0.057		1.079***
				(0.357)		(0.275)		(0.343)		(0.255)
$Audit_{it}$*$many\_violations_i$*Pres. $party_{it}$				0.629**		-0.482		0.458		-0.641
				(0.317)		(0.574)		(0.311)		(0.587)
President's $party_{it}$	-0.000	-0.101	0.001	-0.100	0.003	-0.059	0.112***	0.103***	0.124***	0.118***
	(0.074)	(0.075)	(0.074)	(0.075)	(0.095)	(0.099)	(0.028)	(0.029)	(0.039)	(0.039)
Observations	5,327	5,327	5,327	5,327	2,757	2,757	37,775	37,775	19,362	19,362
N. of municipalities	779	779	779	779	768	768	5,490	5,490	5,415	5,415

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

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

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

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	app	aut	paid	app	aut	paid	paid	paid	paid	paid
Sample	audited	audited	audited	all	all	all	all	all	c<6	c<6
Model especification	semi parametric	semi parametric	semi parametric	semi parametric	semi parametric	semi parametric	linear	quadratic	linear	quadratic
$Audit_{it}$*n. of violations=0	0.082	0.367*	-0.052	0.017	0.064	0.059	0.066	0.060	0.075	-0.065
	(0.137)	(0.212)	(0.141)	(0.063)	(0.090)	(0.069)	(0.069)	(0.069)	(0.096)	(0.115)
$Audit_{it}$*n. of violations=1	-0.097	0.163	-0.161	-0.060	-0.037	-0.069				
	(0.113)	(0.184)	(0.138)	(0.040)	(0.050)	(0.063)				
$Audit_{it}$*n. of violations=2 or 3	0.027	0.255	-0.262**	0.046	0.026	-0.160**				
	(0.116)	(0.186)	(0.132)	(0.036)	(0.055)	(0.065)				
$Audit_{it}$*n. of violations> 3	-0.056	0.251	0.063	-0.083	-0.019	0.056				
	(0.127)	(0.201)	(0.181)	(0.059)	(0.074)	(0.152)				
$Audit_{it}*C_i$							-0.016	-0.082*	-0.042	-0.232**
							(0.025)	(0.044)	(0.033)	(0.093)
$Audit_{it}*C_i^2$								0.014		0.044**
								(0.011)		(0.019)
President's party	0.037	0.071*	0.000	-0.004	0.003	0.078***	0.079***	0.078***	0.010	0.007
	(0.041)	(0.041)	(0.054)	(0.013)	(0.017)	(0.025)	(0.025)	(0.025)	(0.057)	(0.057)
Observations	4,836	4,836	4,836	27,821	27,821	27,821	27,821	27,821	4,661	4,661
Number of municipalities	692	692	692	4,743	4,743	4,743	4,743	4,743	666	666

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Audited	Audited	Audited	Audited	All	All	All	All
Dependent variable	Federal grants	Budget surplus	Local taxes	Infrastructure expenditures	Federal grants	Budget surplus	Local taxes	Infrastructure expenditures
$Audit_{it}$*$no\_violations_i$	0.003	0.024	-0.001	-0.015	0.010	-0.004	-0.002	0.006
	(0.007)	(0.047)	(0.002)	(0.049)	(0.008)	(0.009)	(0.002)	(0.039)
$Audit_{it}$*$few\_violations_i$	0.003	0.024	-0.001	-0.015	0.010	-0.004	-0.002	0.006
	(0.007)	(0.047)	(0.002)	(0.049)	(0.008)	(0.009)	(0.002)	(0.039)
$Audit_{it}$*$many\_violations_i$	-0.001	0.025	-0.001	-0.091*	0.006	-0.009*	-0.002	-0.070*
	(0.007)	(0.052)	(0.002)	(0.053)	(0.007)	(0.005)	(0.002)	(0.043)
President party	-0.009*	0.013	-0.002	0.017	-0.003*	-0.004	-0.001	0.038***
	(0.005)	(0.010)	(0.001)	(0.032)	(0.002)	(0.004)	(0.001)	(0.011)
Observations	6,068	5,118	5,709	5,118	42,801	36,359	40,658	36,359
Number of municipalities	779	779	779	779	5,489	5,489	5,489	5,489

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

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

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

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

Table 7: The effects of release of the audit reports on electoral outcomes by mayoral political affiliation Dependent variable: the probability of re-election of incumbent mayor

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

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

Table 8: The effects of release of the audit reports on electoral outcomes

Dependent variable: the probability of re-election of incumbent political party

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

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

Table 9: The effects of release of the audit reports on electoral outcomes, by timing of release

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

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

CHAPTER 2: The Political Resource Curse*

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Abstract

The paper studies the effect of additional government revenues on political corruption and on the quality of politicians, both with theory and data. The theory is based on a version of the career concerns model of political agency with endogenous entry of political candidates. The evidence refers to municipalities in Brazil, where federal transfers to municipal governments change exogenously according to given population thresholds. We exploit a regression discontinuity design to test the implications of the theory and identify the causal effect of larger federal transfers on political corruption and the observed features of political candidates at the municipal level. In accordance with the predictions of the theory, we find that larger transfers increase political corruption and reduce the quality of candidates for mayor.

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1 Introduction

Suppose new oil is discovered in a country, or more funds are transfered to a locality from a higher level of government. Are these windfalls of resources unambiguously beneficial to society? This is a key question in the study of a variety of issues in macroeconomics and development economics, such as intergovernmental relations, transfers to lagging regions like the European Union's Structural Funds, and international aid to developing countries.

Until a few years ago, the only reason for a negative answer to this question would have been provided by the "Dutch disease literature:" a natural resource windfall, such as oil revenues, can lead to a decline in income via a market mechanism, notably an appreciation of the real exchange rate. In the last few years a growing literature, and much anecdotal evidence, has argued that a windfall of natural resources can have further adverse effects through the political process and the interaction among interest groups, leading for instance to increased rent-seeking (as in the dynamic common pool models of Tornell and Lane, 1999; and Velasco, 1999) or even to civil war (as in Besley and Persson, 2008; Caselli and Coleman, 2008; and Ross, 2006).[1]

In this paper, we argue that windfall government revenues can worsen the functioning of political institutions, because they exacerbate the political agency problem and deteriorate the quality of political candidates. This idea has been voiced before in policy debates, for instance with reference to the Italian South (Rossi 2006), but without spelling out a precise mechanism and only on the basis of anecdotal evidence. Here we show that it is supported by both rigorous theory and systematic evidence.

The theory is based on a political agency model with career concerns and endogenous entry of political candidates. The model focuses on the electoral competition between an incumbent and a set of challengers, all with different political abilities and different opportunity costs of entering politics. The incumbent faces a trade-off between using public resources for personal gains (corruption) and maximizing the probability of election. Although the model has been studied before (Persson and Tabellini, 2000), we emphasize some new implications on the effects of a windfall of revenues, and we extend it to allow for endogenous entry and selection

[1] See also Ross (1999), Rosser (2006), and the references cited therein.

of political candidates with different abilities.

The model highlights three specific channels of operation of windfall government revenues through the political process. First, an increase in resources available to a government leads to an increase in corruption of the incumbent (a *moral hazard effect*). This happens because, with a larger budget size, the incumbent has more room to grab political rents without disappointing rational but imperfectly informed voters. Second, a larger budget induces a decline in the average ability of the pool of individuals entering politics (a *selection effect*). This is a byproduct of the first result (that rents increase with budget size) and of the assumption that political rents tend are more valuable for political candidates of lower ability. Third, there is an *interaction* between these two effects that further increases the adverse consequences of a windfall of revenues on political corruption: an incumbent facing less able opponents can marginally grab more rents without hurting his reelection prospects. Finally, the selection effect highlighted above also implies that windfall revenues increase the equilibrium probability of reelection of the incumbent, depsite his grabbing more rents.

We then test the implications of this model on micro data from a sample of Brazilian municipalities. The obvious problem in testing the effects of government revenues is, as always, how to identify exogenous changes: one can think of a number of reasons why local government revenues might be correlated with corruption and the composition of the pool of politicians. For instance, corrupt politicians might have a comparative advantage in obtaining higher transfers from other levels of government; or poorer areas might select low-quality politicians and, at the same time, receive more transfers for redistribution purposes. To address this issue, we combine three different datasets. The first contains information on a program of federal transfers to municipal governments, determined in a stochastic but discontinuous fashion by population size; the second consists of data on a program of random audits on local governments, with detailed reports on corruption charges; the third provides biographical and electoral information on the incumbent mayors and their opponents in municipal elections.

We exploit a key feature of the federal transfers program: all municipalities in the same state and in a given population bracket *should* receive the same amount of transfers. Indeed, although in the data there exist multiple cases of misassignments around the policy

thresholds, the amount of federal transfers received by municipal governments displays visible jumps at each threshold. We therefore use a (fuzzy) regression discontinuity approach—with population discontinuities as an instrument for the transfers actually received—to study the impact of a discrete change in revenues between municipalities just above or below the thresholds on the corruption of the incumbent mayors (as measured by the random audit program) and on the composition of the pool of opponents (as captured by their years of schooling and private sector occupation).

The empirical findings accord well with the implications of the theory. Specifically, an (exogenous) increase in federal transfers by 10% raises the incidence of a broad measure of corruption by 12 percentage points (about 17% with respect to the average incidence), and the incidence of a more restrictive measure—including only severe violation episodes—by 10.1 percentage points (about 24%). At the same time, larger transfers (by 10%) worsen the quality of the political candidates challenging the incumbent, decreasing the fraction of opponents with at least a college degree by 3 percentage points (about 7%). As a result, the incumbent who receives higher transfers experiences a raise in his probability of reelection by 4.1 percentage points (about 7%).

At the theoretical level, our paper combines three separate strands of literature, besides the career concerns model discussed by Persson and Tabellini (2000). The first is the literature on windfall resources and rent-seeking mentioned above. Our closest antecedent here is Robinson, Torvik, and Verdier (2006), who use a partisan model with patronage to study the optimal extraction of resources and the optimal patronage by a government facing reelection. A second strand of literature studies the selection of politicians, and how different institutions affect the pool of elected officials and candidates (Besley, 2004; Caselli and Morelli, 2004; Besley and Smart, 2007; Mattozzi and Merlo, 2008; Galasso and Nannicini, 2009). A third, older strand of literature studies the allocation of talents in economies characterized by different incentives to different types of talents (Baumol, 1990; Murphy, Vishny, and Shleifer, 1991).

With regard to the evidence, to our knowledge, we are the first to estimate the effect of transfers from a higher level of government on political corruption and on the quality of political candidates of local governments. Each one of these three Brazilian datasets has been

used before to study related outcomes, but they have never been combined and they have not been used to study how federal transfers affect political corruption and the quality of candidates for mayor. Litschig (2008a) is our closest antecedent: he uses the same Brazilian dataset on federal transfers and a similar regression discontinuity methodology to show that higher federal transfers increase municipal spending on public schools and improve literacy rate outcomes. Although he does not talk about corruption, his findings are consistent with ours. Using a tailored household survey, Vicente (2009) shows that the discovery of oil in the island of Sao Tome and Principe was associated with a significant rise in perceptions of corruption, relative to the control island of Capo Verde. Caselli and Michaels (2009) show that oil discoveries in Brazilian municipalities have a positive impact on public good spending, but little or no effect on the quality of public good provision. They also provide indirect and anecdotal evidence that this might be due to rent-seeking and corruption. Ferraz and Finnan (2008, 2009a) use instead the dataset on randomized audits to study, respectively, the effects of corruption disclosure on the election outcome and of electoral accountability on political corruption: they find that mayors found to be corrupt have a lower reelection probability, and that municipalities where mayors can be reelected experience less corruption. Brollo (2008) uses similar data and finds that corrupt municipalities are also punished by a reduction in the (discretionary) infrastructure transfers they receive from higher levels of government after the release of the reports.

Our paper is also related to a recent literature on political selection, which has focused on the impact of monetary and non-monetary incentives on the decision of citizens to run for an elective office (Diermeir, Keane, and Merlo, 2005; Messner and Polborn, 2004; Gagliarducci, Nannicini, and Naticchioni, 2008; Gagliarducci and Nannicini, 2009; Ferraz and Finan, 2009b). So far, however, this literature has not investigated how the quality of political candidates is affected by the size of the government budget or by transfers from higher levels of government.

The outline of the paper is as follows. Section 2 presents the theory and derives its empirical implications. Section 3 discusses the relevant Brazilian institutions and describes the data. Section 4 illustrates the econometric strategy. Section 5 presents a number of validity tests and the estimation results. We conclude with Section 6.

2 Theory

2.1 A career concerns model

This section studies a version of the "career concerns" model of Persson and Tabellini (2000). In order to focus on the selection of politicians, we extend that framework by introducing differences in the ability of candidates and endogenous entry in politics. Although that model can be formulated with an infinite horizon (see Section 4.5.2 in Persson and Tabellini, 2000), for simplicity we assume only two periods. Throughout, we refer to the politician in office as the incumbent mayor.

In the first period ($t = 1$) an incumbent mayor sets policy for that period. Then elections are held, and the elected mayor sets policy once more for a second ($t = 2$) and last period. In both periods, a budget of fixed size τ can be allocated to two alternative uses: rents r_t that only benefit the mayor; and a public good g_t that only benefits the voters. The cost of providing the public good depends on the identity of the mayor, and more competent mayors can provide the same public good (expressed in terms of voters' utility) at a lower resource cost. Specifically, the government budget constraint is:

$$g_t = \theta(\tau - r_t) \tag{1}$$

where θ reflects an individual's competence (if elected to office) in providing the public good: a higher value of θ corresponds to a lower cost of providing the public good, and hence a more competent mayor. Thus, the policy can be thought of as rents (r_t) captured by the mayor in that period, while the public good g_t is residually determined from the budget constraint.

We assume political competence to be a random but permanent feature of an individual. Specifically, θ is a random variable uniformly distributed with density ξ and a known mean. The realization of θ is drawn from two alternative distributions, with the same density but different means, depending on the individual's type. Specifically, for an individual of type J the mean of θ is $1 + \sigma^J$, where $J = H, L$, and $\sigma^H = \sigma = -\sigma^L$, with $1 > \sigma > 0$ a known parameter. Thus, individuals of type H on average are more competent if elected to office. But in specific instances it could very well be that the actual competence of an individual of

type H is lower than that of an individual of type L.[2]

In keeping with the career concerns model, we assume that the realization of θ becomes known to each individual, and also to voters if that individual is elected to office and becomes mayor, only at the end of period 1. The mayor's type is known beforehand to everyone, however. At the time of elections, voters also observe their own utility (i.e., the public good g_1), but do not observe political rents. All the parameters of the model are known to the voters.

This formulation captures two important features of political agency conflicts. On the one hand, as in the standard career concerns model, the voters' imperfect information about the incumbent's true competence creates an incentive for the incumbent to please the voters through public good provision, so as to appear competent. On the other hand, not all politicians are ex-ante identical: voters know something about political candidates, besides what is learned by observing policy outcomes. Throughout this section we refer to the mayor's type J as simply high or low quality, but more generally J stands for any observable variable (other than policy outcomes) that enables voters to predict the mayor's performance if elected. In the empirical section, we measure J by the politicians' education or income. For now, the politician's type is exogenous. In the next subsection, we make it endogenous by analyzing the entry decision of candidates.

In line with the institutions in Brazil, we assume that rent-seeking (corruption) by the mayor is discouraged by an audit technology. Specifically, with probability $d(r_t) = qr_t$ a mayor who grabbed political rents r_t is caught and suffers utility loss of λ^J, where $\lambda^H > \lambda^L > 0$. [3] Thus, the loss of utility for a high quality mayor who is caught cheating is harsher. This assumption plays a crucial role below, where we analyze the entry of political candidates, and it is further discussed there. It is meant to capture the idea that a highly educated or very talented politician has more valuable opportunities outside of politics. Hence, for such

[2] Under our assumptions, the range of realizations of θ for a mayor of type J is: $[1+\sigma^J-\frac{1}{2\xi}, 1+\sigma^J+\frac{1}{2\xi}]$, $J = H, L$.

[3] As explained in footnote 5 below, the results of interest would be reinforced if we assumed that the probability of being caught depends on the fraction of the budget devoted to rents (rather than in the absoulte amount of rents as assumed here).

a politician the reputation cost of being caught in an act of corruption is higher than for someone with lower opportunity costs from being in politics.

As standard in the literature on political agency, politicians care about political rents (net of the expected penalty), and enjoy other exogenous benefits from being in office (ego rents), summarized by the exogenous variable R. Thus, the expected utility of a mayor of type J who is in office in periods 2 and 1 respectively is:

$$V_2^J = \alpha^J r_2 + R \tag{2}$$

$$V_1^J = \alpha^J r_1 + R + p^J V_2^J \tag{3}$$

where $\alpha^J = 1 - \lambda^J q$ denotes the expected value of political rents for type J, and p^J is the probability of being reelected, as perceived by the incumbent in period 1, when setting the optimal rent r_1. We assume that $\lambda^J < 1$, so that $\alpha^J > 0$ for all J.

Voters only care about the public good, hence their preferences in each period are:

$$W_t = g_t \tag{4}$$

Finally, we assume that rents cannot exceed a given upper bound that depends on the size of the budget, namely:

$$r_t \leq \psi\tau \equiv \overline{r} \tag{5}$$

The timing of events is as follows:

- At the start of period 1, the incumbent sets r_1. He knows his own type, but he does not yet know the actual realization of his competence, θ, nor the identity of his future opponent. Specifically, the incumbent expects his opponent to be of type L with probability π, and of type H with probability $1 - \pi$, where for now $1 > \pi > 0$ is given, but will be endogenized later (the assumption that the incumbent does not yet know his opponent's identity is made to simplify notation and with no loss of generality).

- The identity of the opponent is revealed and his type H or L (but not the actual realization of his competence θ) becomes known to all.

- Elections are held. When voting, voters observe g_1, but not r_1. They also know the incumbent's as well as the opponent's type. After the elections, the audit takes place and the penalty is paid (if cheating is detected).

- In period 2 the elected mayor sets r_2, and then a second and final audit takes place.

2.2 Equilibrium rents

To solve the model, we work backwards. In the last period, whoever is in office sets maximal rents. This follows from the assumption that the expected penalty is insufficient to deter corruption ($\alpha^J > 0$ for all J). Hence, $r_2 = \bar{r} \equiv \psi\tau$ irrespective of who is elected.

Next, consider the voters' behavior in period 1. Since the period 2 policy is the same irrespective of who is in office, voters only care about competence, and they vote for the candidate with the higher expected competence. Thus, an incumbent of type J wins against an opponent of type O if:

$$E(\theta|g_1, J) \geq 1 + \sigma^O \qquad J, O = H, L \tag{6}$$

where the left hand side of (6) is the expected value of θ conditional on the voters observation of g_1 and their knowledge of the incumbent's type J, while the right hand side is the unconditional mean of θ for an opponent of type O.

By (1) it is easy to see that (see also Persson and Tabellini, 2000):

$$E(\theta|g_1, J) = \frac{g_1}{(\tau - r_1^{eJ})} \tag{7}$$

where r_1^{eJ} denotes the voter's expectation of how an incumbent of type J sets rents in period 1. Exploiting (1) once more we also have that, from the point of view of the incumbent

$$E(\theta|g_1, J) = \theta\frac{\tau - r_1^J}{\tau - r_1^{eJ}} \tag{8}$$

where r_1^J denotes the rents actually set by a type J incumbent. Thus, by (6)-(8), an incumbent of type J running against an opponent of type O wins the election with probability

$$p^{JO} = Pr[\theta \geq \frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O)] \tag{9}$$

$$= \frac{1}{2} + \xi(1 + \sigma^J) - \xi\frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O) \tag{10}$$

where the first equation follows from (6)-(8), and the second equation from the assumption about the distribution of θ.[4]

When the incumbent sets policy, however, he does not yet know the identity of his future opponent, and he assigns probabilities π and $1-\pi$ to the events that the opponent will be of type L and H, respectively. Thus, as perceived by the incumbent when choosing rents, the relevant probability of reelection is:

$$p^J = \frac{1}{2} + \xi(1+\sigma^J) - \xi\frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1+\hat{\sigma}) \tag{11}$$

where $\hat{\sigma}$ is the expected competence of the opponent, as perceived by the incumbent when setting rents in period 1:

$$1+\hat{\sigma} \equiv 1+\sigma(1-2\pi) \tag{12}$$

We are now ready to discuss the determination of public policy in period 1. The incumbent maximizes (3) with respect to r_1, subject to (11) and, by the incentive compatibility condition, taking the voters expectations r_1^{eJ} as given. At an interior optimum, the first order condition of the incumbent's problem is:

$$\frac{\partial V_1^J}{\partial r_1} = \alpha^J + \frac{\partial p^J}{\partial r_1} V_2^J = 0 \tag{13}$$

where in equilibrium the expected utility from being in office in period 2 is:

$$V_2^J = \alpha^J \overline{\tau} + R \equiv \alpha^J \psi \tau + R \tag{14}$$

Taking the partial derivative of p^J with respect to r_1^J, for a given value of r_1^{eJ}, and then imposing the equilibrium condition that $r_1^{eJ} = r_1^J$, by (11) we have that in equilibrium:

$$\frac{\partial p^J}{\partial r_1^J} = -\frac{\xi(1+\hat{\sigma})}{\tau - r_1^J} < 0 \tag{15}$$

[4]Specifically, given that θ is drawn from a uniform distribution with density ξ and mean $1+\sigma^J$,

$$prob(\theta > X) = \frac{1}{2} + \xi(1+\sigma^J - X)$$

Thus, a higher rent reduces the probability of reelection because it reduces g_1 and therefore, given r_1^{eJ}, the voters' estimate of the incumbent's ability. We call the absolute value of (15) the "electoral punishment" of the marginal rent.

Combining (13)-(15), the equilibrium rent set in period 1 by an incumbent of type J is:

$$r_1^J = \tau - \xi(1+\hat{\sigma})(\psi\tau + R/\alpha^J) \tag{16}$$

where, to have an interior optimum, we implicitly assume that the right hand side of (16) is positive. We call this the "partial equilibrium" rent, to emphasize the fact that it is conditional on a given expected competence of the opponent $\hat{\sigma}$; later we will endogenize $\hat{\sigma}$. For future reference, we call the expression $(\psi\tau + R/\alpha^J)$ "value of reelection" and the expression $\xi(1+\hat{\sigma})$ "electoral threshold" (strictly speaking, these expressions are transformations of the expressions capturing these concepts). Thus, at an optimum the incumbent grabs the whole budget less a quantity that is a function of the electoral threshold times the value of reelection. Intuitively, a higher electoral threshold (i.e., a higher expected competence of the opponent) reduces the rent because, from (15), it increases the electoral punishment of the marginal rent.

Finally, imposing the equilibrium condition that actual and expected rents coincide, the equilibrium probability that an incumbent of type J defeats an opponent of type O is:

$$p^{*J,O} = \frac{1}{2} + \xi(\sigma^J - \sigma^O) \tag{17}$$

where we have used (10) and the "*" superscript denotes equilibrium. Correspondingly, the equilibrium probability of reappointment, based on the information available to the incumbent, is:

$$p^{*J} = \frac{1}{2} + \xi(\sigma^J - \hat{\sigma}) \tag{18}$$

Note that these equilibrium probabilities only depend on the difference in expected competence between the incumbent and the (actual or expected) opponent. Intuitively, voters have the same information as the incumbent. Hence, they correctly guess political rents and the incumbent's true competence. In equilibrium, election outcomes are only determined by the relative expected competences of the two candidates, and not by actual policies. Nevertheless, electoral incentives exert a powerful influence on public policies.

We can now state the main properties of the equilibrium, giving particular emphasis to the effects of a larger budget size, since these are the implications that are tested in the empirical analysis below. We confine attention to period 1, which is more interesting.

Proposition 1 *Rents are an increasing function of budget size:* $\frac{\partial r_1^J}{\partial \tau} > 0$.

This is an immediate implication of (16), together with the assumptions needed to have strictly positive rents at an interior optimum. Intuitively, the electoral punishment for rents, $\frac{\partial p^J}{\partial r_1}$, becomes smaller in absolute value as τ rises (see equation 15). This in turn is implied by how voters form their inferences: from (8), as the budget grows in size, a dollar stolen has a smaller impact on voters' inferences about the incumbent's unobserved ability. At the margin this diminishes the incentive of political incumbents to please the voters. This result is quite intuitive: if the budget size is very large, there is more room to grab political rents without disappointing the voters.[5]

Proposition 2 *Rents are a decreasing function of the expected competence of the opponent:* $\frac{\partial r_1^J}{\partial \hat{\sigma}} < 0$.

This result too follows immediately from (16) and (15). From (6), the expectation of a more competent opponent entails a higher competence threshold to reappoint the incumbent, and reduces the probability of reappointment, for any level of rents consistent with voters' expectations. At this higher reelection threshold, the probability of winning the election is more sensitive to political rents (see equation 11). This sharpens the incumbent's incentive to please the voters, and as a result equilibrium rents fall. Note that the expected competence of the opponent (as perceived by the incumbent) in turn depends on π, the probability that

[5]Note that, almost by assumption, period 2 rents are also an increasing function of budget size. This dampens the effect of budget size on period 1 rents, because it raises the value of reelection, but (at an interior optimum) it is not enough to offset the effect of τ on r_1^J that operates through the term $\frac{\partial p^J}{\partial r_1}$. It is also easy to see that Proposition 1 would be strengthened if we assumed that the probability of being caught was increasing in the fraction of the budget devoted to rents ($d(r_t) = qr_t/\tau$), rather than in the absolute amount of rents ($d(r_t) = qr_t$) as assumed above. Intuitively, under the alternative assumption, a larger budget would reduce the probability of detection, inducing the incumbent mayor to grab even more rents.

the opponent is a low quality type. Thus, the higher is this probability, the lower is the expected quality of the opponent and the higher are equilibrium rents.

Proposition 3 *The effect of budget size on rents is larger the lower is the expected competence of the opponent:* $\frac{\partial^2 r_1^J}{\partial \tau \partial \hat{\sigma}} < 0$.

This interaction effect between τ and $\hat{\sigma}$ reflects the same forces that account for the previous two propositions. Intuitively, when the budget size increases by one dollar, we know from (16) that the incumbent grabs the extra dollar less a quantity which is a function of the electoral threshold times the value of reelection; hence, a higher expected competence of the opponent (a higher electoral threshold) reduces the share of the extra dollar of budget that the politician appropriates. Hence, not only does a larger budget size increase political rents (Proposition 1), but it also does so to a larger extent if the opponent is more likely to be of low quality (if $\hat{\sigma}$ is small or, equivalently, if π is large).

2.3 The quality of political candidates

The model emphasizes the role of elections in selecting the more competent candidate, and the implied effects on the incumbent's incentives. But the pool of candidates was taken to be exogenous, neglecting how individuals respond to incentives in deciding whether or not to stand as a political candidate. In this subsection we address this issue, and allow the proportion of high and low quality types in the pool of candidates to be determined endogenously in equilibrium. For this we need additional assumptions.

Let $2N$ be the overall population, with N a discrete large number. In the population there are two groups of individuals indexed by $J = H, L$, with each group of size N. All the assumptions outlined above continue to hold. In particular, if an individual in group J holds office, his competence is drawn from a uniform distribution with mean $1 + \sigma^J$.

Within each group, individuals differ by the opportunity cost of entering into politics: individual i in group J has opportunity cost $\beta_i y^J$, for $i = 1, 2...N$. To simplify the algebra, we assume that $\beta_i = i$. Thus, for the first individual in group J the opportunity cost of being into politics is y^J, for the second individual it is $2y^J$, and so on until the last one

has opportunity cost Ny^J. Throughout we assume that $y^H > y^L > 0$. Thus, consistently with the previous political interpretation, high quality individuals ($J = H$) have a higher expected competence if they become mayor and also have a higher opportunity cost of being in politics. The parameter β_i instead is unrelated to political competence, so that the relationship between political competence and the opportunity cost of being in politics is not one for one. This formulation captures the idea that political competence is related to features, such as education or sheer talent, that also make an individual more productive in the private sector. But the decision to enter politics also reflects other considerations besides income, and the skills needed to be a successful politician do not coincide with those that yield high income or success in other professions. The positive correlation between market skills (outside opportunities) and political competence is common in the models on political self-selection, such as Caselli and Morelli (2004) and Besley (2004).

At the start of period 1 individuals decide whether or not to enter politics. Entering politics means that, with some probability, the individual is selected to run as the single opponent to the incumbent mayor in the elections that are held at the end of period 1. In other words, entering politics is equivalent to entering the pool of candidates from which the opponent is selected. We do not model how parties select a hierarchy of political candidates, and simply assume that all individuals in the pool of candidates have the same probability to be selected as the opponent, irrespective of their types J and i. Specifically, suppose that n^J individuals from group J have decided to enter politics, $J = H, L$. Then the pool of candidates has size $n = n^H + n^L$, and each one of them has probability $\frac{1}{n}$ to become the single opponent who will challenge the incumbent. This captures the notion that not all politicians get a chance to become serious political candidates for mayor.

To simplify the notation and with no loss of generality, we also assume that, when deciding whether or not to enter politics, individuals know their own type but do not know yet the identity of the incumbent and assign equal probabilities to the event that the incumbent is of type H or L. Thus, by (17) in the previous subsection, the expected probability that an opponent of type J wins the election is $(1/2 + \xi\sigma^J)$, where with a slight abuse of notation here we use the symbol J to denote the opponent (rather than the incumbent) type.

Under these assumptions, if individual i in group J stays out of politics, then he gets utility

iy^J. If he enters politics, then with probability $\frac{1}{n}$ he is selected to become the opponent, and with probability $(1/2+\xi\sigma^J)$ he wins the election and gains office in period 2. By the notation in the previous subsection, the expected utility of being in office in period 2 for an individual of type J is V_2^J. A political candidate who loses the election or is not selected to be the opponent, gets zero utility.

With this notation, the i-th individual in group J prefers to enter politics if

$$iy^J \leq \frac{\left[\frac{1}{2}+\xi\sigma^J\right]}{n} V_2^J \tag{19}$$

Ignoring integer constraints, n^J is determined by the indifference condition:

$$y^J n^J = \frac{\left[\frac{1}{2}+\xi\sigma^J\right]}{n} V_2^J \tag{20}$$

Using (20) we can solve for n:

$$n = \sqrt{\frac{V_2^H}{y^H}(\frac{1}{2}+\xi\sigma) + \frac{V_2^L}{y^L}(\frac{1}{2}-\xi\sigma)} \tag{21}$$

Then from (20) we have

$$n^J = \frac{V_2^J}{y^J}\frac{\left[\frac{1}{2}+\xi\sigma^J\right]}{n}, \quad J = H, L \tag{22}$$

Hence, the share of L types in the pool of opponents is:

$$\pi = \frac{n^L}{n^H + n^L} = \frac{1}{1+x} \tag{23}$$

where

$$x \equiv \frac{V_2^H}{V_2^L}\frac{y^L}{y^H}\frac{\frac{1}{2}+\xi\sigma}{\frac{1}{2}-\xi\sigma} \gtrless 1 \tag{24}$$

Note that $\pi \lesseqgtr \frac{1}{2}$. This is intuitive: high quality individuals have higher opportunity costs ($y^H > y^L$) and lower expected benefits from being in office ($V_2^H < V_2^L$), but they also have higher probability of winning against the yet unknown incumbent, so the net effect is ambiguous.

We now briefly discuss the properties of π, again focusing on the effect of budget size.

Proposition 4 *The fraction of low quality types in the pool of opponents is an increasing function of budget size:* $\frac{\partial\pi}{\partial\tau} > 0$.

To see this, note that:

$$\frac{V_2^H}{V_2^L} = \frac{\alpha^H \psi\tau + R}{\alpha^L \psi\tau + R}$$

So that, after some transformations:

$$\partial \frac{V_2^H}{V_2^L} / \partial\tau = \frac{\psi R}{(V_2^L)^2}(\alpha^H - \alpha^L) < 0 \tag{25}$$

which in turn implies that $\partial\pi/\partial\tau > 0$—see (23-24). In words, a larger budget size τ leads to a worse composition of the pool of opponents. Intuitively, because the value of rents is higher for the low quality mayors, a larger budget increases the value of office by more for the low quality than for the high quality candidates. Hence, at the margin more low quality candidates enter the pool of opponents, deteriorating the composition.

This result reflects two important assumptions in the model. First, we assumed that the penalty if caught is higher for a high quality type($\lambda^H > \lambda^L$), which implies that rents are less valuable for a high quality type ($\alpha^H < \alpha^L$). If this assumption was reversed, the empirical implication too would be the opposite. Thus, although we find our assumption a priori plausible, it can be jointly tested with the model. Second, the model focuses on the decision of individual candidates to enter politics, but it has nothing to say on how parties select amongst alternative candidates (since we assumed that all prospective candidates have the same probability $1/n$ of running as the opponent). Without a richer model of intra-party politics it is difficult to assess how restrictive this omission is.[6]

2.4 The total effect of budget size

Putting it all together, we can now determine the total effect of budget size, taking into account also its effects on the quality of the opponents. Combining (16) with the definition

[6]In this simple model, if we assumed that parties maximize expected rents, they would always choose the high quality type as candidate. The reason is that he woul have a higher probability of winning and second period rents are the same for all types. But this is clearly too simplistic, because of both the two period restriction and the neglect of intra-party conflict. The literature on how parties choose candidates is still rather scarce - but see Carillo and Mariotti (2001) and Persico, Rodriguez Pueblita and Silverman (2009).

of $\hat{\sigma}$ (12) and with (23), we get

$$r_1^J = \tau - \xi \left[1 - \sigma\left(\frac{1-x}{1+x}\right)\right](\psi\tau + R/\alpha^J) \tag{26}$$

which we call the "general equilibrium" rent to distinguish it from the "partial equilibrium" rent (16). It is easy to see that the equivalent of Proposition 1 holds also for the general equilibrium rent (26).

Proposition 5 *The overall effect of budget size on rents is positive:* $\frac{dr_1^J}{d\tau} > 0$.

In fact, the total derivative of r_1^J with respect to τ is:

$$\frac{dr_1^J}{d\tau} = \frac{\partial r_1^J}{\partial \tau}\Big|_{\widehat{\sigma}} + \frac{\partial r_1^J}{\partial \widehat{\sigma}}\Big|_{\tau} \frac{\partial \widehat{\sigma}}{\partial \tau} > 0 \tag{27}$$

where both terms of the sum on the right hand side are positive; the first term by Proposition 1, the second because, from Proposition 4, $\partial\widehat{\sigma}/\partial\tau < 0$.

Equation (27) illustrates well the two main forces at work in this model. The first is the positive effect of τ on rents holding constant the composition of the pool of opponents, i.e., holding constant π; this is the moral hazard effect. The second is the positive effect of τ on rents due to the response of the composition of the pool of opponents; this is the interaction between the moral hazard and the opponent selection effects.

2.5 The probability of reelection

The model also has predictions on the effect of budget size on the probability of reelection. Consider expression (18), the probability of reelection based on the information available to the incumbent. By the law of large numbers, this is also the average probability of reelection of an incumbent of type J.

Proposition 6 *The probability of reelection of an incumbent of type J is an increasing function of budget size:* $\frac{dp^{*J}}{d\tau} > 0$.

This follows directly from the effect of a larger budget size on the average competence of the opponents: as the budget size increases, more low quality individuals are drawn into the

pool of opponents (Proposition 4). Thus, despite grabbing more rents, in equilibrium the incumbent is more likely to be reappointed. This result reflects voters' rationality. Voters realize that equilibrium rents have increased with a larger budget, but they only care about the competence of future mayors. Hence, as the pool of opponents deteriorates in quality, voters become less demanding and apply a a lower quality threshold for reelecting the incumbent. As a result, the incumbents' chances of winning go up.

Propositions 4 and 6 highlight an important implication of the analysis: a windfall of revenues is harmful not only because it tempts public officials into more corruption, but also because over time it leads to a deterioration of the quality of elected officials. This result is related to those obtained by Murphy, Shleifer, and Vishny (1991). But whereas they consider the allocation of talent between productive and rent-seeking activities in the private sector, here we highlight the implications of windfall revenues for the selection of talents into public office.

2.6 Discussion

Although the model is highly stylized in its description of the political process, it generates several interesting implications. In this paper, we highlight one such set of results, namely those relating to the effects of a windfall of government revenues. The remainder tests these implications on Brazilian municipal data, exploiting an institutional feature whereby federal transfers to municipal governments vary exogenously according to given population thresholds. Thus the parameter τ in the model corresponds to federal transfers received by municipal governments.

The theory generates prediction about the size of corruption (political rents, r_t) and the frequency of detection (qr_t). In the data, we observe only the frequency of detection (and possibly the size of corruption conditional on being detected). By the law of large numbers, the theory predicts that larger federal transfers should be associated with:

i) more frequent episodes of political corruption by the mayor (Propositions 1 and 5);

ii) a lower observed quality of the pool of political opponents in the elections for mayor (Proposition 4);

iii) more frequent reappointment of the incumbent mayor (Proposition 6).

Given the richness of the data, we can also test two additional implications of the theory concerning the interactions between these effects, namely:

iv) episodes of political corruption are more frequent when the opponents are of lower quality (Proposition 2);

v) the positive effect of federal transfers on the frequency of corruption is more pronounced when the opponents are of lower quality (Proposition 3).

Finally, the model has other implications, that we do not take to the data because they have already been investigated before. In particular, Ferraz and Finan (2009a) have used this same dataset to show that term limits induce more frequent corruption in the last term of office of the mayor (one of the implications of this model). And several empirical studies (such as Persson and Tabellini, 2003) have investigated the presence of electoral business cycles in different countries, also an implication of infinite horizon versions of this model where elections take place in different periods.

3 Institutions and Data

This section describes the institutional framework and the data we use in the empirical analysis. The main variables of interest refer to federal transfers to municipal governments (the variable τ in the model), corruption (the variables r_t and qr_t in the model), and the observed quality of political candidates (their type J). The empirical counterpart of each of these variables is described in a separate subsection below.

3.1 Federal transfers to municipal governments

3.1.1 Institutional framework

Brazilian municipal governments are managed by an elected mayor (*Prefeito*) and an elected city council (*Camera dos Vereadores*). Mayors are directly elected by voters with plurality

rule. In 2000, the term limit for mayors was extended from one to two terms. The mayoral term lasts four years, and elections are usually held in October (oath of office taking place in January of the following year).

Municipal governments are in charge of a relevant share of the provision of public goods and services related to education, health, and infrastructure projects. Most of the municipal resources are intergovernmental transfers from either the federal or state government.[7] For municipalities with less than 50,000 inhabitants—those included in our sample—local taxes represent only 6% of total revenues. The single most important source of municipal revenues (40%) is the *Fundo de Participação dos Municipios* (FPM), consisting of automatic federal transfers established by the Federal Constitution of Brazil (see Art. 159 Ib). FPM transfers amount to 75% of all federal transfers and, according to the rules that regulate the allocation of these funds, municipal governments must spend 15% of them for education and 15% for health care, while the remainder is unrestricted.[8] Our study focuses on this type of transfers, both for their relevance and because the amount of FPM resources received by each municipality depends on population size in a discontinuous fashion that is crucial for the identification strategy discussed in the next section.

According to the the FPM allocation mechanism, municipalities are divided into population brackets that determine the coefficients used to share total state resources earmarked for the FPM, with smaller population brackets corresponding to lower coefficients. Since each state receives a different share of the total resources earmarked for FPM, two municipalities in the same population bracket receive identical transfers only if they are located in the same state. More precisely, define FPM_i^k as the amount of FPM transfers received by municipality i in the state k. The revenue-sharing mechanism is:

$$FPM_i^k = \frac{FPM_k \lambda_i}{\sum_{i \in k} \lambda_i}$$

where FPM_k is the amount of resources allocated to state k and λ_i is the FPM coefficient

[7] Brazil is divided into 26 states and 1 federal district (Brasilia).

[8] There are other current transfers that follow a constitutional rule and are completely tied to education (FNDE), social assistance (FNAS), and health care (SUS). However, FPM transfers represent 79% of all current federal transfers, SUS 8%, FNAS 1%, and FNDE 2%.

of municipality i based on its population size.[9]

Table 1 reports the population brackets and the associated FPM coefficients.[10] As discussed below, because of sample size limitations, we restrict the empirical analysis to municipalities in the population interval 6,793–50,940 (about 90% of Brazilian municipalities and 32% of population) and focus on the initial seven thresholds: 10,189; 13,585; 16,981; 23,773; 30,564; 37,356; and 44,148. The intervals between the initial three thresholds are equal to 3,396, while the intervals between the subsequent thresholds amount to twice as much (6,792). For the sake of symmetry, we then restrict our sample to municipalities from 3,396 below the first threshold to 6,792 above the seventh threshold. Within this population range, there are no other legislative or institutional discontinuities, with only one exception: at 10,000 inhabitants, the cap in the wage of city councelors increases by 50% (from 1,927 to 2,891 Brazilian *reais*, as of 2004).

The coefficient of each municipality is set by the Federal Court Account (*Tribunal de Contas União*, TCU) on the basis of the population estimates calculated yearly by an indipendent statistical agency, the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE). IBGE uses a top-down approach so that the municipality estimates are consistent with the state estimates, which in turn are consistent with the estimated population of the whole country, calculated on the basis of birth rates, mortality rates, and net immigration between Censuses. In Appendix I, we describe the exact statistical procedure followed by IBGE to calculate its population estimates.

As further discussed below, population estimates from IBGE in a given year, however, do not perfectly predict the FPM transfers each municipality receives in the subsequent year. There may be various reasons for that. During the 1990s, several municipalities split and this reduced the population size of pre-existing municipalities. As a result, a municipality that had lost part of its population should have had its coefficient reduced according to the new population. However, several law amendments froze the FPM coefficients and this prac-

[9] At the federal level, the resources earmarked for FPM transfers are 22.5% of total revenues from the federal income tax and industrial products tax. The resources are then allocated to the different states (FPM_k), with poorer states generally receiving a larger share.

[10] See Decree No. 1881/81, August 1981.

tice generated major distortions. In order to avoid these distortions, the federal government established that by 2008 all municipalities should be framed in FPM coefficients corresponding to their actual population estimate.[11] To avoid shocks in the finance of the involved municipalities, however, the law established a transition period to the new regime, so that in the period 2001–08 some municipalities still received FPM transfers that were not consistent with their population. Furthermore, the FPM allocation procedure is not audited. The population figures used by TCU and the associated coefficients are published in the *Diário Oficial da União*. For some years, we compared population estimates from IBGE (available from the IBGE website) and those used by TCU, and they do not perfectly coincide.[12]

3.1.2 Data on transfers

Our data cover two mayoral terms: January 2001–December 2004 and January 2005–December 2008. We measure two key variables of the FPM revenue-sharing mechanism: the amount of federal transfers and the IBGE population estimates.

Data on FPM transfers received by each municipality are available from the website of the Brazilian National Treasury (*Tesouro Nacional*). The variable we use in the empirical analysis is the average amount of transfers in the first three years of each term (in real values), therefore excluding the year in which the next election is held.[13] This value is a proxy for the amount of transfers that mayoral candidates in the 2000 and 2004 elections should expect to receive during the next term, in case they won the electoral race.

Population estimates are directly available from the IBGE website. We use them to construct the "theoretical transfers" that each municipality in each state should receive, if other factors did not play any role. In theory, the amount of transfers each municipality

[11] See Supplementary Law No. 91/97, as amended by Law No. 106/2001.

[12] We could retrieve only a few years for the population estimates used by TCU, because they are not available in electronic format. Litschig (2008b) detects some evidence of manipulative sorting above the FPM thresholds in the TCU population figures for the years 1989 and 1991.

[13] We cannot use 2008 (the electoral year at the end of term 2005–2008) because the IBGE population estimates for 2007 are not available; we therefore exclude also 2004 (the electoral year at the end of term 2001–2004) for consistency. Estimation results are not sensitive to this choice.

receives should be calculated according to the IBGE population estimates that are sent to TCU in the previous year. Therefore, for the term 2001–2004, we use an average of the IBGE population estimates for the years 2000, 2001, and 2002; for the term 2005–2008, we use estimates for the years 2004, 2005, and 2006.

As explained below, for reasons of data availability, we exploit two samples of municipalities: a small and a large sample. Table 2 reports descriptive statistics, by population intervals, on the actual and theoretical FPM transfers in both samples. On average, municipalities in our large sample receive 33.79 hundred thousand Brazilian *reais* at 2000 prices (standard deviation 12.63). Theoretical transfers are slightly lower, with an average of 33.44 (standard deviation 13.20).

Figure 1 depicts the actual (top panel) and theoretical (bottom panel) FPM transfers against the IBGE population estimates in the large sample. The left figure in the top panel displays the scatterplot of the received transfers over the period 2001–2007; the seven vertical lines represent the FPM population thresholds. The right figure in the top panel shows the same association in a different way: a scatterplot where FPM transfers are averaged over cells of 100 inhabitants, plus the smoothed average of transfers (solid line) calculated separately in each interval from one threshold to the next. Both figures display visible jumps at the FPM thresholds, with the exception of the seventh, where sample size is also starting to get smaller.[14] Some noise, however, persists around each threshold, pointing to possible cases of misassignment. This is evident when the above figures are compared with those in the bottom panel of Figure 1, which display the theoretical transfers. There—by construction—the jumps at the seven thresholds are clean. Note that also theoretical transfers show some within-bracket variability because of the different shares received by the states, and this variability increases with population size.

Figure 2 emphasizes an additional peculiarity of the FPM allocation mechanism: since, within each state k and population bracket λ, municipalities obtain the same resources, the per-capita amount of both received and theoretical transfers is a decreasing function of population size within each bracket.

Finally, to check whether the increase in FPM transfers completely crowd-out other types

[14]The results of the empirical analysis are not sensitive to the exclusion of the seventh threshold.

of revenues, leaving the budget size unchanged, we also collected data on municipal finance, available from the Brazilian National Treasury website. These budget data, however, are self-reported by the municipalities and come from a different source with respect to the data on FPM transfers.

3.2 The Brazilian anti-corruption program

3.2.1 Institutional framework

In 2003, the Brazilian federal government launched a major anti-corruption program. Since then, municipalities have been randomly chosen by lottery to be audited on a monthly basis. Auditors examine the use of federal transfers at the local level. Members of the government, the media, and the general public may attend the lottery. The *Corregedoria Geral da União* (CGU) is the independent body that conducts the audits. For each municipality selected by lottery, auditors collect documents and information from the period 2001 to the present. A few months after the audit, reports are sent to all levels of governments and are also made available on the CGU website. Each report contains information on the total amount of federal transfers audited. More importantly, the report contains a list that describes the full details of the irregularities found by the auditors and the related sector (health, education, social assistance, or infrastructure). Example of irregularities are: fraud, non-competitive bidding in procurement contracts, over-invoicing, diversion of funds, lack of completeness, non-utilization of the funds, as well as others.

Between 2003 and 2004, in each lottery, 50 municipalities were randomly selected to be audited. Since 2004, 60 municipalities have been selected in each lottery. To date, the total number of audited municipalities is over 1,500. The program thus provide a valuable source of information on budget irregularities and corruption episodes in municipal governments.

Most of the audits concern projects or public works financed by specific federal transfers other than the FPM transfers, although some projects financed by the municipality unconstrained resources (including FPM transfers) are also audited. Thus, in the analysis below, we ask how an exogenous increase in FPM transfers around the population thresholds affects corruption in the use of *all* sources of municipal revenues. Since 70% of FPM transfers

are unrestricted and given that FPM transfers account for the largest fraction of municipal revenues, this question corresponds to a test of Propositions 1 and 5 in the model (how rents react to a change in overall budget size τ). Specifically, the theory predicts that, as FPM transfers increase, municipal governments feel less restrained in pleasing the voters and engage in more abuses of all kinds, and not just concerning FPM transfers.

We now describe in more detail how we classify each occurrence in the audit reports, in the spirit of Ferraz and Finan (2008).

3.2.2 Data on corruption

Because of sample size limitations in the audited local governments, we restrict the sample to municipalities with less than 50,940 inhabitants, corresponding to the first seven FPM thresholds (see Table 1). In the two mayoral terms of our analysis, 606 municipalities were randomly selected through the first 17 lotteries of the Brazilian anti-corruption program.[15] The bad administration and corruption occurrences reported in the audit reports are thus related to the municipal administration that was in power during the two terms (551 municipalities in 2001–2004 and 55 municipalities in 2005–2008).

Many types of irregularities are detected by the audits. Illegal procurement practices, diversion of funds, over-invoicing of goods and services, and fraud are the most common occurrences. We introduce two definitions of corruption: *broad corruption*, which includes irregularities that could also be interpreted as bad administration rather than as overt corruption; and *narrow corruption*, which only includes severe irregularities. For both definitions, we construct a binary variable (whether any irregularity was found or not) and a discrete indicator (the number of detected violation episodes). As a robustness check, we also consider an additional measure for each definition of corruption, namely the log of the ratio between the total amount of funds involved in the violation and the total amount audited. The results for these additional measures are similar to those for the number of violations reported in Section 5 (available upon request).

[15] Starting with the 18th lottery, the audit reports changed structure, making the classification of violation episodes more difficult.

The definition of broad corruption includes the following categories of violation episodes: 1) *illegal procurement practices*, occurring when any of these episodes are reported: a) competition has been limited, for example, when associates of the mayor's family or friends receive non-public information related to the value of the project, b) manipulation of the bid value, c) an irregular firm wins the bid process, d) the minimum number of bids is not attained, or e) the required procurement procedure is not executed; 2) *fraud*; 3) *favoritism* in the good receipt; 4) *over-invoicing*, occurring when there is evidence that public goods or services are purchased for a value above the market price; 5) *diversion of funds*; 6) *paid but not proven*, occurring when expenses are not proven. In Appendix II, we report relevant examples for each violation category.

The definition of narrow corruption includes the following irregularities: 1) severe *illegal procurement practices*; 2) *fraud*; 3) *favoritism*; 4) *over-invoicing*. In our opinion, many of the irregularities regarding the two categories *diversion of funds* and *paid but not proven* do not necessarily imply corruption (see Appendix II). Also some illegal procurement practices might result more from bad administration than from outright corruption: therefore, narrow corruption includes these episodes only if they resulted in severe violations, such as favoring one specific firm or manipulating the bid value.

In the following, we refer to "small sample"—consisting of 606 observations—as the (random) sample for which we have information on these corruption variables. Descriptive statistics on the four corruption measures—by population intervals—are reported in Table 3. According to our broad measure of corruption, 71% of mayors in municipalities with population in the interval 6,793–50,940 are found to be corrupt. This figure is decreasing with population size. For the more restrictive measure of narrow corruption, 42% of the mayors are found to be corrupt. This measure shows higher variability, but with no clear pattern across intervals. The number of corruption episodes, on average, is 1.99 and 0.73 for the broad and narrow definition, respectively.[16]

[16]Note that our definition of broad corruption is close to the measure used by Ferraz and Finan (2009a, Table 1), whose incidence is 78%. We also considered an intermediate definition of corruption including *diversion of funds* and excluding *paid but not proven*. The empirical results for this intermediate definition are similar to the results presented in Section 5 for narrow corruption (available upon request).

Note that, among the 606 observations in the small sample, 229 (about 38%) refer to mayors who are in their first term and then stand for reelection. This corresponds exactly to the first period analyzed in the model. Since the model predicts that the behavior of the mayor could differ depending on the term of office, as a robustness check below we also restrict attention to these mayors.

3.3 Measuring the quality of politicians

In the model of Section 2, the observed quality of political candidates (their type J) is correlated both with their potential talent in government, and with their opportunity cost of being in politics. We measure these individual features with reference to education and to the previous occupation outside of politics. Since the unit of analysis is the municipality in a legislative term, we refer to the average features of the pool of candidates in each municipal election included in our sample. Specifically: 1) *college* denotes the fraction of candidates with at least a college degree; 2) *years of schooling* denotes the candidates' average years of schooling; and 3) *high-skilled occupation* denotes the fraction of candidates previously employed in occupations associated with a high opportunity cost of entering politics.[17] The source for these variables is the dataset on elected officials from the Brazilian Electoral Court (*Supremo Tribunal Eleitoral*) website. We collected data for all municipalities in the relevant population brackets, for the elections held in 2004 and 2008, irrespective of whether or not they were audited. Therefore, this corresponds to a much larger sample of municipal governments than the small sample for which we can measure corruption.

The relevant variable in the model (π) refers to the quality (or type) composition of the pool of opponents in the first-term reelection of the incumbent mayor. We thus restrict attention to municipalities and mayoral terms in which the mayor is actually running for

[17]We have classified as high-skilled these seven occupation categories: lawyers (7% of the sample), physicians (8%), managers (3%), entrepreneurs (11%), agricultural entrepreneurs (15%), and other professionals (12%). The remaining occupation categories include: blue collars (2%), general employees, such as office assistants, waiters, secretaries, etc. (2%), self-employed (15%), politicians (5%), public employees (10%), retired (3%), and other (7%).

reelection, within the relevant population brackets . We refer to this set of observations as the "large sample" (2,788 observations). Here, in accordance with the model, the set of candidates for which we measure education and previous occupation corresponds to the pool of opponents faced by the incumbent mayor. Thus, the variable *college* measures the fraction of opponents with a college degree, and so on.

For this large sample, Table 4 reports descriptive statistics on the opponents' characteristics and the reelection frequency of incumbent mayors, by population intervals. On average, the political opponents in our sample have about 11.9 years of schooling, and 44% of them went to college. As one would expect, educational attainments increase with population size. Local politicians are relatively highly educated, as only 8% of the Brazilian population aged between 25 and 64 have a college degree.[18]. As for occupation, 57% of politicians had a high-skilled job before entering politics. Finally, 59% of the incumbent mayors running for another term win their bid for reelection.[19]

Clearly, this sample is not random, since it only refers to the elections in which the incumbent mayor has chosen to run for reappointment. As a robustness check, below we also report results for the larger sample referring to all municipalities of the relevant population size on which data are available, and that includes also the observations where the mayor does not run for reelection (either because he/she is in the second term, or because he/she chooses not to run). There, the set of candidates for which the average quality is reported corresponds either to all political candidates (since we cannot distinguish between an incumbent and a set of opponents), or to all political candidates but the candidate of the political party of the incumbent mayor.

4 Econometric Strategy

In this section, we formalize the econometric strategy that allows us to identify the effect of federal transfers on both corruption and the patterns of political selection in Brazilian munic-

[18]Source: *Pesquisa Nacional de Amostra por Domicílios*, PNAD, 2004.

[19]Although we do not consider gender and age as outcome variables, note that the politicians in our sample are predominantly male (89%) and, on average, 50.4 years old.

ipalities. Basically, the institutional setup described in the previous section delivers a treatment assignment mechanism typical of a (fuzzy) Regression Discontinuity Design (RDD). Treatment assignment—receiving high versus low federal transfers—depends on the running variable—population size—in a stochastic manner, but in such a way that the propensity score—the probability of being treated conditional on the running variable—is known to have relevant discontinuities at multiple thresholds. The fuzzy design arises from the fact that, as discussed in the previous section and shown in the top panel of Figure 1, there are cases of misassignment around the cutoffs, with municipalities near each threshold appearing both in the treatment and control group. In other words, not all municipalities receive the amount of (theoretical) transfers they should receive based on their IBGE population estimate (P_i) and the state they belong to.

At each threshold P_j, separating population brackets j and $j+1$ in the FPM revenue-sharing mechanism, "theoretical" transfers ($\hat{\tau}$) sharply increase from a lower (ℓ_j) to a higher level (h_j): $\hat{\tau}_i = \ell_j$ if $P_{j-1} < P_i < P_j$, and $\hat{\tau}_i = h_j$ if $P_j < P_i < P_{j+1}$, with $h_j > \ell_j$. Theoretical transfers are thus a step function of P_i. Actual transfers (τ), however, do not necessarily follow through. One can think of theoretical transfers as the treatment assignment and actual transfers as the observed treatment, in a situation of imperfect compliance. Treatment assignment is exogenous around the policy thresholds, although the observed treatment may also be influenced by additional factors, such as politicians' ability in sidestepping the exogenous assignment rule or other random elements. As long as actual transfers depend on theoretical transfers, however, we can use the latter as an instrument in a (fuzzy) regression discontinuity setup. To capture that both the outcome of interest (y) and actual transfers depend on theoretical transfers and other stochastic elements, we can use a potential outcome notation, where $y_i(\hat{\tau})$ and $\tau_i(\hat{\tau})$ are the potential values of the outcome variable and actual transfers, both expressed as a function of theoretical transfers (i.e., treatment assignment).[20]

Formally, under the assumption of continuity of the conditional regression functions of potential outcomes at the cutoff P_j (see Hahn, Todd, and Van der Klaauw, 2001; Imbens and

[20]For the sake of simple notation, we omit time subscripts, but in our data observations also vary across (two) periods. In the empirical analysis, we control for that by including time dummies in all specifications and clustering the standard errors at the municipality level.

Lemieux, 2008), we can identify the reduced-form (or intention-to-treat) effects of theoretical transfers on both actual transfers and corruption as:

$$E[\tau_i(h_j) - \tau_i(\ell_j)|P_i = P_j] = \lim_{P \downarrow P_j} E[\tau_i|P_i = P] - \lim_{P \uparrow P_j} E[\tau_i|P_i = P], \tag{28}$$

$$E[y_i(h_j) - y_i(\ell_j)|P_i = P_j] = \lim_{P \downarrow P_j} E[y_i|P_i = P] - \lim_{P \uparrow P_j} E[y_i|P_i = P]. \tag{29}$$

In our framework, the continuity assumption simply requires that: i) there are no other policies using a population discontinuity at P_j; ii) municipalities cannot manipulate population estimates to sort above P_j and receive more transfers. We already checked the first condition in Section 3.1; we will formally test the second in Section 5.1.

The above reduced-form effects can be consistently estimated in the following way (see Imbens and Lemieux, 2008; Garibaldi et al., 2009):

$$\tau_i = g(P_i) + \alpha_\tau \hat{\tau}_i + \delta_t + \gamma_p + u_i, \tag{30}$$

$$y_i = g(P_i) + \alpha_y \hat{\tau}_i + \delta_t + \gamma_p + \eta_i, \tag{31}$$

where $g(.)$ is a high-order polynomial in P_i, δ_t time fixed effects, γ_p state fixed effects, and both error terms u_i and η_i are clustered at the municipality level. In a trade-off between accuracy and transparency, we estimate these equations both in the overall sample and around each threshold P_j, as long as sample size allows us to do that.

The next step is to use the above reduced-forms to identify the causal effect of FPM transfers on the outcome of interest. Under the same continuity conditions, we have that the quantity

$$\frac{\lim_{P \downarrow P_j} E[y_i|P_i = P] - \lim_{P \uparrow P_j} E[y_i|P_i = P]}{\lim_{P \downarrow P_j} E[\tau_i|P_i = P] - \lim_{P \uparrow P_j} E[\tau_i|P_i = P]} \tag{32}$$

identifies the average effect of actual transfers on the outcome y for compliers, that is, for those municipalities above (below) the cutoff that receive more (less) transfers exactly because of their higher theoretical transfers (i.e., because of their treatment assignment based on the IBGE population estimates).

The causal interpretation of this IV estimand rests on two additional assumptions (see Angrist and Lavy, 1999; Angrist, Imbens, and Rubin, 1996): i) exclusion restriction; ii) monotonicity. The first condition states that theoretical transfers—which are a deterministic

(and discontinuous) function of population estimates—affect the outcome only through the transfers actually received by municipalities; and this is plausible as long as other policies do not share the same discontinuities. The monotonicity condition states that, at each threshold, municipalities assigned below the cutoff do not effectively receive more transfers than if they had been assigned above the cutoff. This assumption—like the exclusion restriction—is untestable because it involves potential outcomes, but it is more than plausible in our context. Indirectly, in Figure 1, the visible jumps in observed transfers at the FPM thresholds (all of them in the same, positive direction) are reassuring about the validity of the monotonicity condition.

Finally, it is worth noting that the causal effect we are identifying is local in a twofold meaning. First, because of the RDD setup, it only refers to observations around the thresholds. Second, because of the IV setup, it only refers to *compliers*, that is, municipalities that received larger transfers because of the (exogenous) FPM revenue-sharing mechanism. The external validity of our exercise is of course enhanced by the presence of multiple thresholds. Yet, the identification on compliers leaves aside a subpopulation that might be of interest on its own: the *always takers*, that is, municipalities receiving larger transfers irrespective of their position above or below each population threshold.

We can implement (32) by estimating the following equation:

$$y_i = g(P_i) + \beta_r \tau_i + \delta_t + \gamma_p + \epsilon_i, \tag{33}$$

where theoretical transfers $\hat{\tau}_i$ are used as an instrument for τ_i, $g(.)$ is a high-order polynomial in P_i, δ_t time fixed effects, γ_p state fixed effects, and the error terms ϵ_i are clustered at the municipality level. As above, we estimate (33) both in the overall sample and around each threshold P_j. This estimation, depending on the outcome, delivers direct tests of Propositions 1 and 5 (if y measures corruption), Proposition 4 (if y measures opponents' quality), and Proposition 6 (if y is incumbent's reelection) in our theoretical model.

5 Empirical Findings

5.1 Validity tests and preliminary results

Our identification strategy is valid if the population estimate we use as an instrument—the IBGE population data—is not manipulated by local governments to sort above the thresholds. Figure 3 shows the frequency of municipalities with less than 50,941 inhabitants, using different binsizes (283, 566, and 1,132 inhabitants) that never contain our seven thresholds, identified by the vertical lines. The population distribution is positively skewed. More importantly, visual inspection does not reveal any discontinuity at the FPM thresholds.

We formally test for the presence of a density discontinuity at the seven thresholds in Figures 4 and 5, where we perform a battery of McCrary tests by running kernel local linear regressions of the log of the density separately on both sides of each threshold (see McCrary, 2008). In Figure 4, we run the tests using our population measure — averaged over the term of office —b oth in the pooled thresholds usde in our estimations (1–7 and 1–3) and separately in each of the seven thresholds. We implement the pooling of thresholds 1–7 and 1–3 by merging the thresholds together and normalizing population size as the distance from the closest threshold (with symmetric intervals around each threshold so that no municipality belongs to more than one interval). As a result, each interval runs from the midpoint below to the midpoint above every threshold (with a length of 3,396 around the first three thresholds and of 6,792 around the others). As we can see from the figure, the log-difference between the frequency to the right and to the left of each threshold is never statistically significant.[21]

In Figure 5, we perform the same test for the pooled threshold 1–7 but separately in every year, in order to control that our average population over the term is not masking manipulative sorting in a particular year. Again, the log-difference between the frequency to the right and to the left of each threshold is never statistically significant, despite some

[21]Point estimates (standard errors) for the thresholds in Figure 4 are as follows. Thresholds 1–7: -0.080 (0.198); thresholds 1–3: -0.168 (0.205); threshold 1: -0.229 (0.352); threshold 2: 0.319 (0.325); threshold 3: -0.690 (0.397); threshold 4: 0.304 (0.351); threshold 5: 0.719 (0.691); threshold 6: -0.518 (0.761); threshold 7: -0.405 (1.240). Optimal bandwidth and binsize as in McCrary (2008).

(visual) evidence of a little sorting in the population estimates for 2001.[22]

In Table 5, we further check for manipulative sorting by performing balance tests on the available invariant town characteristics. If there were nonrandom sorting, we should expect some of these characteristics to differ systematically between treated and untreated municipalities around each threshold. The invariant characteristics we look at are the size of the municipal area (measured in km^2) and the geographical location according to Brazilian macro-regions (North, Northeast, Center, South, Southeast), because all the other variables in our dataset are endogenous to the policy. The balance tests are performed by estimating discontinuities in the invariant characteristics at every pooled or individual threshold as the jump in a (split) third-order polynomial fitted separately on either side of each threshold. No pre-treatment characteristics show a significant discontinuity.

As the FPM revenue-sharing mechanism was established in 1981, we can also use information from the 1980 Brazilian Census to check whether some proxies for the (pre-treatment) development level of the municipalities are balanced around the (future) thresholds. For this purpose, we use data from La Ferrara, Chong, and Duryea (2008) on the average employment, the average ownership of durables (such as car, radio, and refrigerator), and the average house access to public infrastructures (such as water and sewer) at the municipal level. These additional balance tests, however, can be performed only on a (selected) subsample of municipalities in our dataset, that is, those that already existed in 1980. From the original 2,788 municipalities in our large sample, we thus end up with 2,217 observations. Table 6 reports the estimation results. No (pre-treatment) employment or wealth variables show a significant discontinuity.

All of the above suggests that the running variable of our fuzzy RDD does not show any evidence of manipulation, so that we can safely use it as a (local) source of exogenous variation in the neighborhoods of our seven FPM thresholds. This is indeed what we should expect, given how IBGE population estimates are constructed by combining past Census information and imputing a certain rate of population growth to each municipality according

[22]Point estimates (standard errors) for the thresholds in Figure 5 are as follows. Year 2000: 0.040 (0.159); year 2001: 0.258 (0.175); year 2002: 0.169 (0.171); year 2004: -0.157 (0.154); year 2005: 0.130 (0.166); year 2006: -0.221 (0.187). Optimal bandwidth and binsize as in McCrary (2008).

to the cell it belongs to (see Appendix I for more details). If manipulative sorting were at work in the actual population numbers—for example, if mayors were able to attract more inhabitants to obtain larger transfers—we would expect the IBGE estimates to remove this problem by means of the estimation procedure. If manipulative sorting were instead at work in the official figures released to obtain the transfers, we would expect this to happen in the TCU data, and the use of IBGE estimates as an instrument would thus serve the purpose of remving this problem.

Finally, to verify that indeed our seven FPM thresholds correspond to relevant changes in municipal fiscal policy, we regress some observed budgetary items against our measure of theoretical transfers. This is relevant, because FPM transfers do not correspond to the totality of federal or state transfers to municipal governments. Hence, to test the predictions of the model, we need to assume that the increase in FPM transfers that occurs at the population thresholds is not entirely offset by a corresponding reduction in other (discretional) federal or state transfers. The results are displayed in Table 7, where we implement equation (30) with the (log of) the budget indicators as dependent variables, and the (log of) theoretical transfers as the regressor of interest. All variables are reactive to the policy thresholds. In particular, the elasticity of total revenues is positive and significant, although slightly lower than would be expected if other sources of revenues remained invariant, keeping into account the FPM share (about 40%). This suggests that local governments react to the additional transfers by reducing local taxes, as indeed shown in column 2 of Table 7. Local expenditures also go up with larger federal transfers (see the remaining columns of the table), indicating that the reduction in local taxes does not entirely offset the extra federal revenues. Note that the sources of data on the budgetary items displayed in Table 7 are not the same as for the FPM transfers, so that these coefficients ought to be treated with caution.

5.2 Estimation results

In this section, we implement the (fuzzy) RDD estimations discussed in Section 4 and test the predictions of our model.

5.2.1 Transfers and corruption

We start by investigating the effect of federal transfers on corruption (Propositions 1 and 5 above). The results, consistently with the theory, point to a large and significant effect of fiscal windfalls on the frequency of corruption episodes.

Table 8 estimates the first stage and the reduced-form regressions—equations (30)-(31). Throughout, we control for a third-order polynomial in population size, as well as time and state dummies. The table reports the estimated coefficients of theoretical transfers, in a regression where the dependent variable corresponds to each column heading. The row "Thresholds 1–7" is obtained by estimating a single regression on the entire sample, and implicitly constraining the coefficient on theoretical transfers to be the same at all thresholds. Accordingly, the row "Thresholds 1–3" does the same over the first three thresholds. The remaining rows correspond to different subsamples, where observations are partitioned in symmetric intervals around each of the first three thresholds.

The first column reports the estimated first-stage coefficient, namely the effect of theoretical transfers on actual FPM transfers. The coefficient is positive and highly significant, but smaller than one. The finding that the impact of theoretical on actual transfers is less than one-for-one is not surprising: it might reflect manipulative sorting by the government body responsible for assigning an FPM coefficient to each municipality (i.e., some municipalities just below the threshold might be deliberately misclassified by TCU as being above the threshold); measurement error in our constructed variable—theoretical transfers—might also lead to a downward bias.[23]

The remaining columns in Table 8 report the reduced-form estimates for the different definitions of corruption. By the estimated coefficients in the second and third columns, an increase in theoretical transfers equal to one standard deviation (11.364 hundred thousand *reais* in this small sample on corruption) translates into a 34% overall increase in the incidence of our broad definition of corruption and a 49% increase in the incidence of the narrow measure. The impact on the number of violation episodes is significant for narrow corruption,

[23]Note that using a set of dummies based on population estimates by the IBGE, rather than theoretical transfers, yields similar results in the following RDD estimations (available upon request).

but not for broad corruption.

Figure 6 provides a graphical representation of the discontinuities in the corruption variables induced by the FPM policy (the intention-to-treat effects). We pool the seven thresholds together by normalizing population size according to the distance of each municipality from the above or below threshold; as above, intervals around each threshold are symmetric and constructed in such a way that no municipality appears in more than one interval. As expected, the scatterplots and the fitted third-order polynomials show relevant discontinuities at zero, especially for the two corruption dummies.

Table 9 estimates the baseline IV regressions—equation (33)—where theoretical transfers are used as instruments for the actual transfers. Consistently with the size of the first-stage coefficients, the IV point estimates in Table 9 are almost twice as large as the intention-to-treat effects. An increase in the amount of actual transfers equal to one standard deviation (11.275 hundred thousand *reais* in this small sample) translates into a 60% increase in broad corruption, 86% in narrow corruption, and 93% in the number of episodes of narrow corruption. Note that also a lower—but more plausible—increase in FPM transfers by 10% has a relevant impact, increasing broad corruption by 12 percentage points (i.e., by about 17%), narrow corruption by 10.1 percentage points (24%), and the number of episodes of narrow corruption by 0.19 (26%).

In Table 10, we implement a series of robustness checks to evaluate the sensitivity of our results with respect to the functional form of the control function in population size, $G(P_i)$, included in equation (33), or to the presence of a confounding policy on the wage of city councillors at 10,000 (see Section 3.1). As for the functional form, we specify $G(P_i)$ as either a spline third-order polynomial (with each interval going from a midpoint to the next), a second-order polynomial (spline or not), or a fourth-order polynomial (spline or not): in all of these cases, the results are very similar to those reported in Table 9 for the baseline specification with a third-order polynomial.

As for the wage policy at 10,000, we introduce two checks: we flexibly control for a (spline) third-order polynomial that also includes the 10,000 threshold, or we simply drop municipalities below 10,000 to focus on a sample without confounding policies. Both robustness checks confirm the baseline results.

On the whole, the quasi-experimental evidence confirms the theoretical prediction of a political resource curse in terms of increased corruption. As mentioned above, the corruption episodes documented in the audits are not strictly related to the FPM transfers. Hence, these estimates document a general deterioration in the quality of the policy-making environment induced by the additonal revenues triggered by the thresholds.

Note that, to gain observations, the specification in Tables 9 - 10 never includes regressors referring to the quality of the opponents. Hence, strictly speaking, these estimates correspond to a test of Proposition 5 - what in section 2 we called the "general equilibrium" effect of budget size on rents, namely the sum of the moral hazard effect (holding costant the quality of the opponents), and the interaction effect (when the quality of the opponent is allowed to change with budget size) - cf. equation (27). The estimates remain almost unchanged if we also control for the quality of the opponents, suggesting that the moral hazard effect is responsible for most if not all of the estimated effect of budget size on corruption. Nevertheless, this might reflect data limitations. When we merge the two samples (with the audited municipalities and with the municipalities where we have data on the features of the opponents) we are left with only 229 observations. Moreover, in this small sample, the characteristics of opponents are balanced around the thresholds, suggesting that there might not be enough variation to disentangle the moral hazard effect from the interaction effect.

Finally, recall that FPM transfers are only a fraction of the overall federal and state transfers received by municipal governments. Under our assumptions we consistently estimate the effect of FPM transfers on corruption. But to also estimate the effect of a windfall of revenues on corruption (as by Propositions 1 and 5) we need an additional hypothesis: namely, that other (discretional) federal or state transfers remain unchanged at each population threshold. In particular, if federal or state policymakers offset the changes in FPM transfers by cutting other sources of municipal revenues at the relevant population thresholds, then we estimate a lower bound on the effect of τ on corruption.

5.2.2 Transfers and political selection

Next, we study the effect of federal transfers on the quality of political opponents (Proposition 4) and on the incumbent's reelection (Proposition 6). As explained in Section 3.3, to stay close to our model's predictions, we first restrict the sample to municipalities where the first-term mayor decides to run for reelection, because only there we have a clear measure of the quality of the pool of opponents. This sample is larger than that on corruption, because it also includes municipalities that were not audited.

Table 11 refers to the first-stage and reduced-form regressions, while Table 12 reports the IV estimates. According to both tables, larger (actual or theoretical) federal transfers lead to a deterioration in the observed average quality of the opponents and to an increase in the probability that the incumbent is reelected. According to the the IV estimates in Table 12, an increase in FPM transfers equal to one standard deviation (12.631 hundred thousand *reais* in this large sample on political selection) translates into a 26% reduction in the fraction of opponents with a college degree, a 8% reduction in their average years of schooling, and a 26% increase in the incumbent's probability of reelection. Analogously, a 10% increase in actual transfers induces a 7% drop in college, 2% in years of schooling, and 7% increase in the reelection probability.[24] The overall impact on high-skilled occupation is not statistically significant, but there is evidence of a negative effect at some thresholds

The overall results on education are mostly driven by the first threshold, although in the other thresholds the estimated coefficients have the expected sign, contributing to the significance of the overall effect where accuracy is improved. As mentioned above, at a population of 10 000 the legislative cap on the salary of city councilors sharply increases. One might be concerned that this institutional variation close to the first FPM threshold might be responsible for our finding. The wage policy, however, involves councilors and not

[24]As the FPM revenue-sharing mechanism has been in place since 1981, one could be afraid of a general-equilibrium effect of transfers on politicians' education through the channel of citizens' education. Note that, the effect of transfers on schooling levels being positive (Litschig, 2008a), this would result in our estimates to be a lower bound of the direct effect of transfers on politicians' education. Furthermore, our estimates are not sensitive to the inclusion of the municipal literacy rate as an additional control in all specifications (results available upon request).

mayors, whose quality we measure here. And general equilibrium effects from the selection of councilors to the selection of mayors are implausible, because the wage policy was only introduced in 2000. Furthermore, Ferraz and Finan (2009b) has used the same data to show that a higher wage attracts higher (rather than lower) quality politicians in the city council, the opposite of what we find for mayors. Nevertheless, below we report additional results that address this issue.

Figure 7 provides a graphical representation of the discontinuities in the political variables induced by the FPM policy (intention-to-treat effects). Again, we pool the seven thresholds together to gain sample size. The two education variables show a clear tendency to grow both before and after the normalized threshold, but the discontinuity at zero is both clearly visible in the scatterplots and statistically significant as the jump in the (split) third-order polynomials.

In Table 13, we implement a series of robustness checks to evaluate the sensitivity of our results with respect to the functional form of $G(P_i)$, or to the presence of a confounding policy on the wage of city councilors at 10,000, as we did for the corruption results in Table 10 The results are strongly robust to any specification of the functional form of the control function in population size. As for the wage policy at 10,000, the results are robust to the inclusion of this additional threshold in a (spline) third-order polynomial, but we lose the significance of most estimates when we drop municipalities below 10,000.

In Table 14 to the political selection results. There, we replicate our baseline IV estimations in different samples. First, in panel A, we measure only the features of the opponent with the highest number of votes (in this case, restricting again to municipalities where the incumbent reruns). Second, in panels B and C, we check whether our results are driven by the (nonrandom) sample restriction to municipalities where the incumbents decide to stand for reelection. In particular, when the incumbent does not rerun, we look at the average quality of all new candidates (panel B) or at the average quality of the new candidates who do not belong to the incumbent's political party (panel C). All of these robustness checks are consistent with the baseline estimates, and the larger sample size even increases the statistical significance at some thresholds.

Finally, note that our results on political selection seem to be mostly driven by the first

three thresholds. Although this could simply be due to sample noise, it is tempting to speculate that the political arena changes along with local characteristics. In particular, the average presence of a local radio—which Ferraz and Finan (2009a) show to be associated with greater political accountability—is 0.13 around the first three thresholds versus 0.31 around the others. Therefore, the effects we find could partly interact with the degree of political accountability.[25]

5.2.3 Corruption and the quality of opponents

Besides the predictions tested above, our theoretical model has implications on the interplay between corruption and political selection: a political opposition of worse quality is predicted to increase corruption (Proposition 2), and to strengthen the positive impact of transfers on corruption (Proposition 3). Unfortunately, Brazilian institutions do not deliver a clean source of exogenous variation to test these propositions, but we can still control whether they are consistent with existing correlations in our sample. An additional difficulty arises from the fact that, as already noted, when we merge the small (corruption) sample and the large (political selection) sample, the remaining sample size is quite small (229 observations).

With these limitations in mind, Tables 15 and 16 investigate the correlations. In both tables, corruption is the dependent variable (measured in different ways). Table 15 , reports the estimated coefficients of different indicators of the quality of the political opposition, estimated by Probit (marginal effects) or by OLS. No clear correlation between corruption of the incumbent mayor and the quality of the pool of opponents arises from this exercise, contraddicting Proposition 2. Table 16 is instead motivated by Proposition 3. There, we report the effect of both FPM transfers and their interaction with different measures of the opponents' quality. Actual and interacted actual transfers are instrumented with theoretical transfers and their interaction with opponents' quality. The coefficient of interest is the interaction effect, which the theory predicts to be negative. Although in the larger sample

[25]Including the presence of a radio station as a control in all the RDD estimations for the corruption and political selection variables does not affect the results, as this variable is balanced around each threshold. Note also that the political party affiliation of the mayor is balanced around each threshold, therefore excluding additional partisan effect.

that includes all thresholds there is no clear pattern, when the sample is restricted to the first three thresholds of more comparable municipalities, the estimated interaction coefficients always have the (predicted) negative sign, and are generally statistically significant. Thus here the evidence is not inconsistent with the prediction that the adverse effect of fiscal windfalls is more pronounced if the political opposition is weak.[26]

6 Conclusion

Could a windfall of resources deteriorate the functioning of government institutions? And if so, how does this happen? These are important questions, because lagging regions or countries often receive additional funds from higher levels of government or from international organizations, to make up for their under-development. Since a common cause of economic backwardnes is precisely the poor functioning of government institutions, the risk that these additional resources could be counterproductive cannot be neglected.

Here we have focused on two mechanisms that are of fundamental importance in a variety of situations: the effects of additional resources on political corruption and on the incentives to participate in politics. At the margin, higher exogenous revenues induce more corruption, because incumbents have more rooms to grab rents without disappointing voters. Moreover, if the benefit of corrupt activities is more valuable to those with worse outside options, individuals of lower quality are attracted into politics. The interaction between these two effects gives rise to a complementarity: precisely because his/her opponents are now of lower quality, an incumbent can afford to grab even more rents while at the same time increasing his/her probability of reelection.

In light of these results, we investigated a specific Brazilian institution that provides an ideal quasi-experimental setting. We found considerable support for the implications of the theory. In particular, a 10% increase in the federal transfers to municipal governments

[26]To address a reverse causality issue between incumbent's corruption and opponents' quality, we also restricted these estimations to municipalities where the audit reports were released after the opponents' decision to challenge the incumbent. Despite the even lower sample size, results are similar to those in Table 16 (available upon request).

raises local corruption by 17% (broad definition, possibly including bad administration) or by 24% (narrow definition, with only severe violation episodes). Moreover, this fiscal windfall increases the incumbent's mayor probability of reelection by 7%, and shrinks the fraction of his/her opponents with a college degree by 7%.

These results are not inconsistent with higher transfers to municipalities increasing the quantity and/or quality of public services provided to the local population. For instance, Litschig (2008a), in the same quasi-experimental setting we use, finds that an exogenous increase in funds to Brazilian local governments raises spending on public education and improves literacy rates. Neverthless, our evidence suggests that these specific benefits are accompanied by a general deterioration in the functioning of local government institutions.

How general are these results, and in particular could they extend to other countries and situations? Only additional research can answer this question. Certainly the high frequency of abuses detected by the audits suggests that Brasilian municipalities are a fragile institutional environment where political agency problems are widespread. It could be that a windfall of resources would not have the same deleterious effects in societies with a long tradition of good government and with abundant social capital. Nevertheless, additional resources are often given precisely to regions or countries with weak institutions, like in the case of Structural Funds to lagging regions in the European Union, or of foreign aid to developing countries. As a result of these policies, these already weak institutions could become even weaker.

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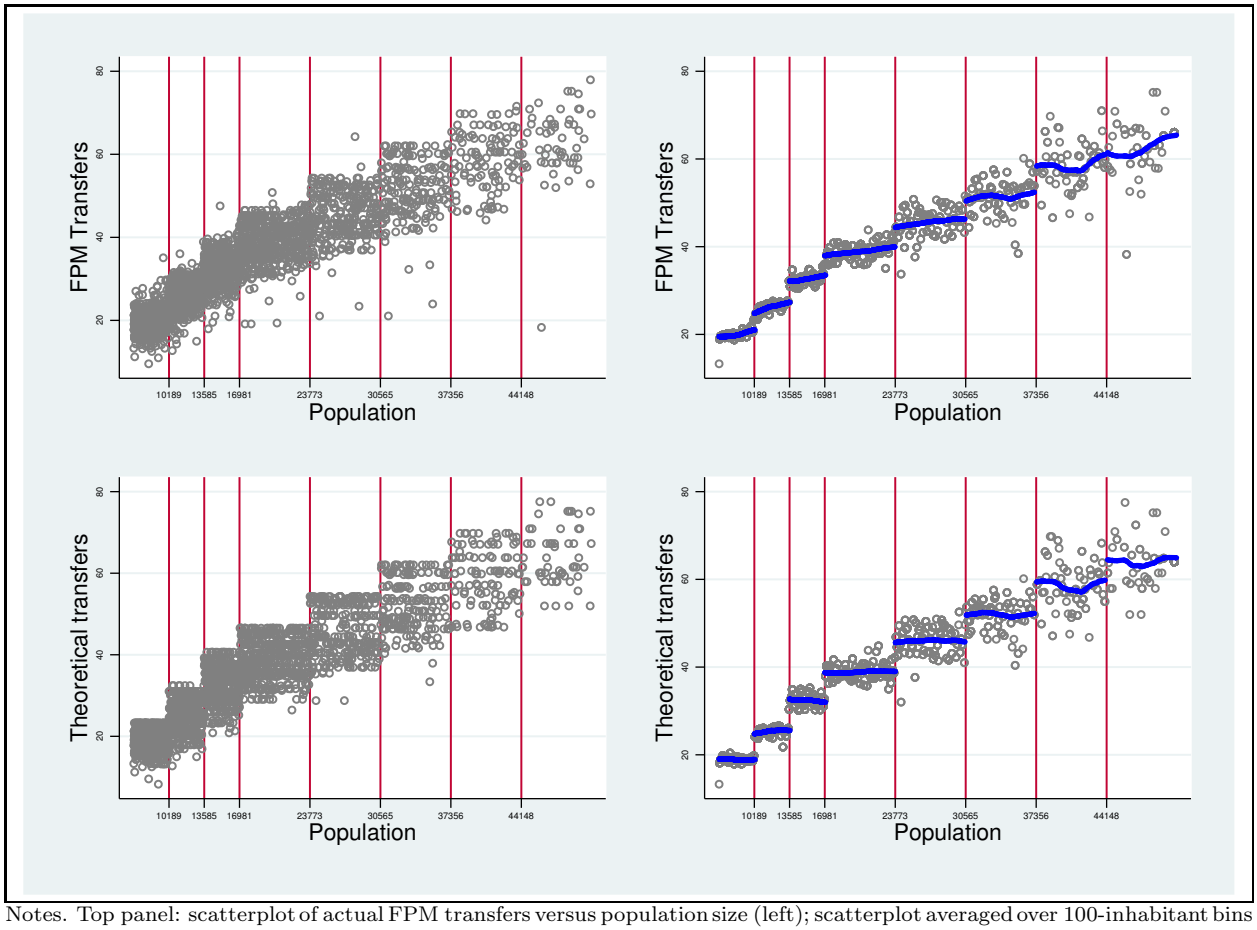
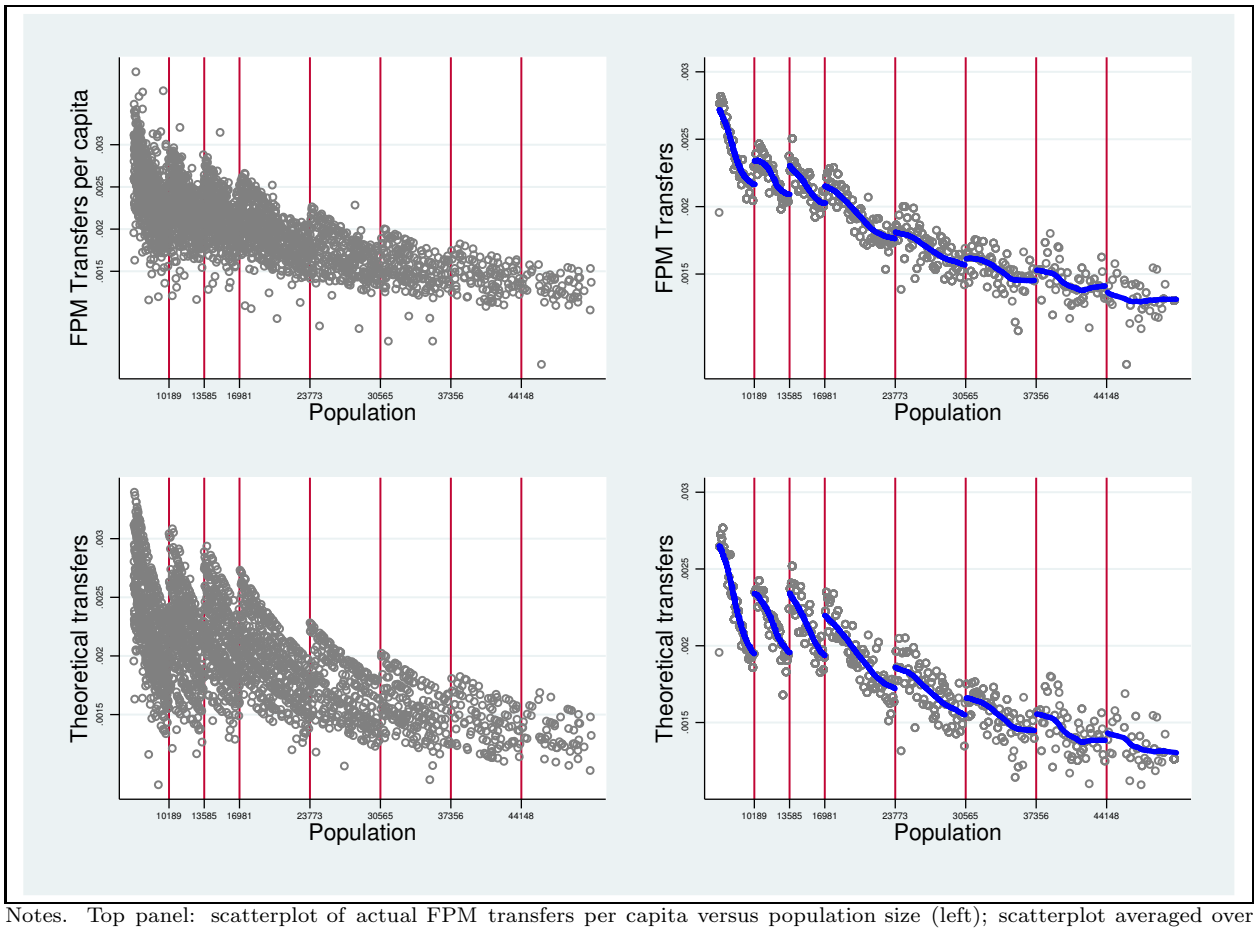
Figures and Tables

Figure 1 – Actual and Theoretical FPM Transfers



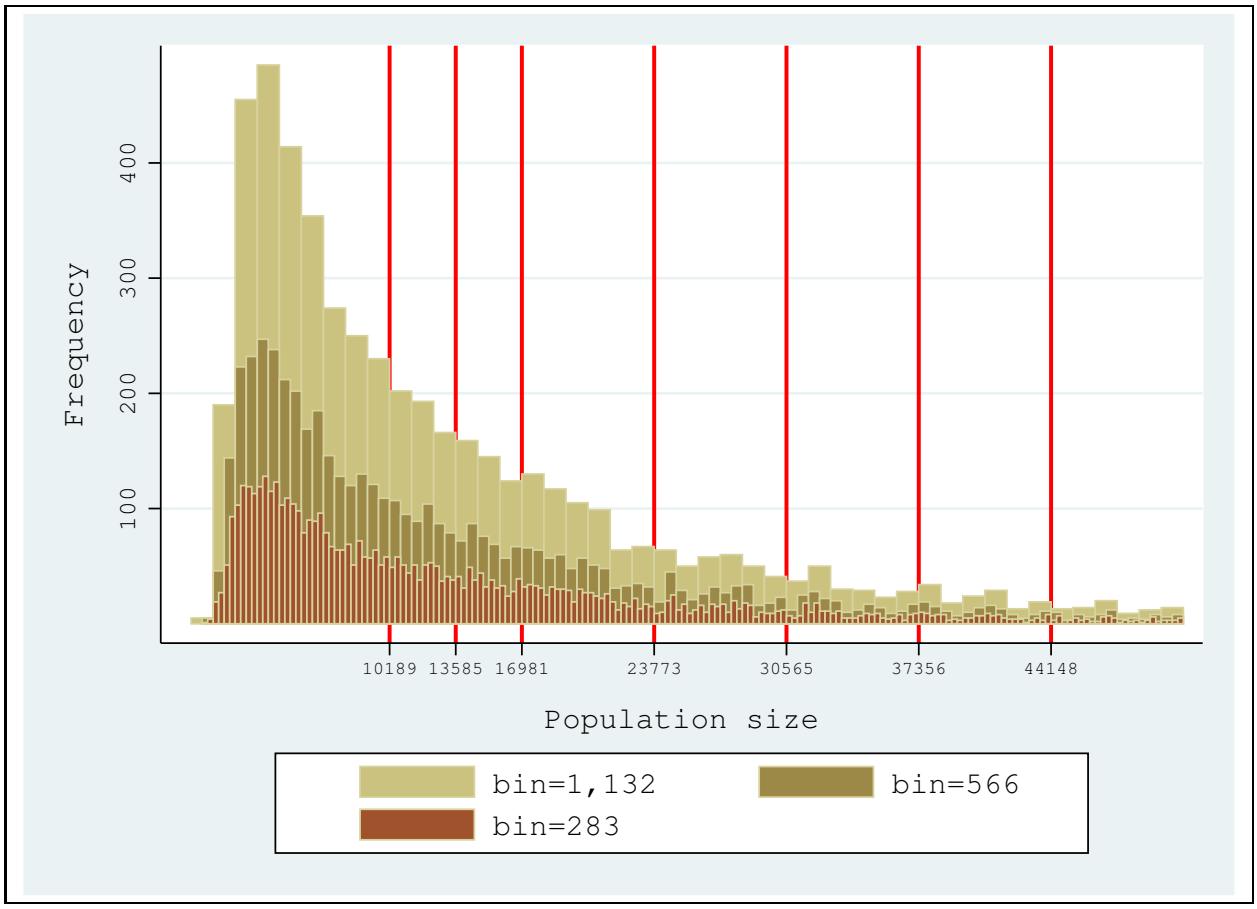
Notes. Top panel: scatterplot of actual FPM transfers versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Mayoral terms 2001–2005 and 2005–2009.

Figure 2 – Actual and Theoretical FPM Transfers (per capita)



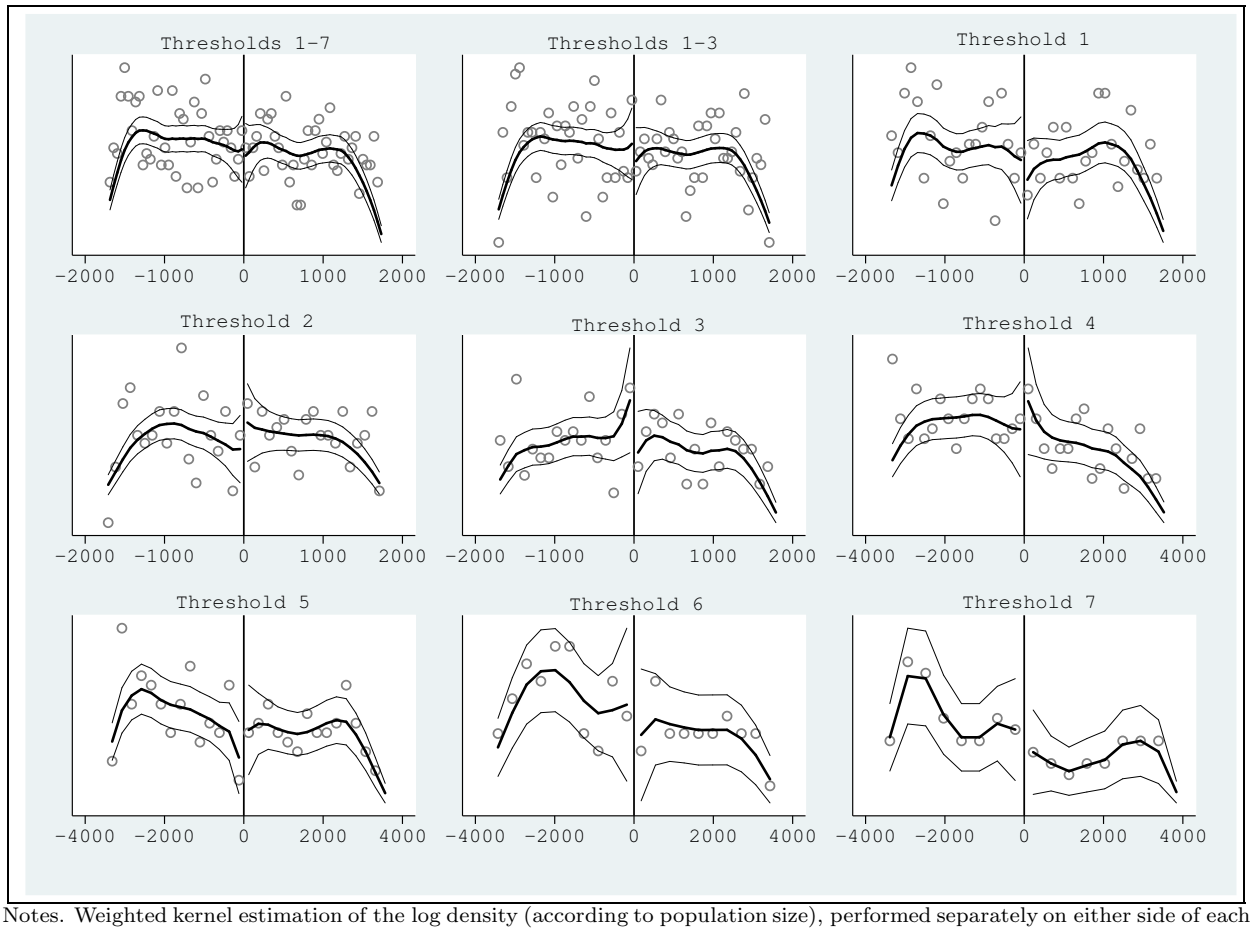
Notes. Top panel: scatterplot of actual FPM transfers per capita versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers per capita versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Mayoral terms 2001–2005 and 2005–2009.

Figure 3 – Population Distribution (<50,941)



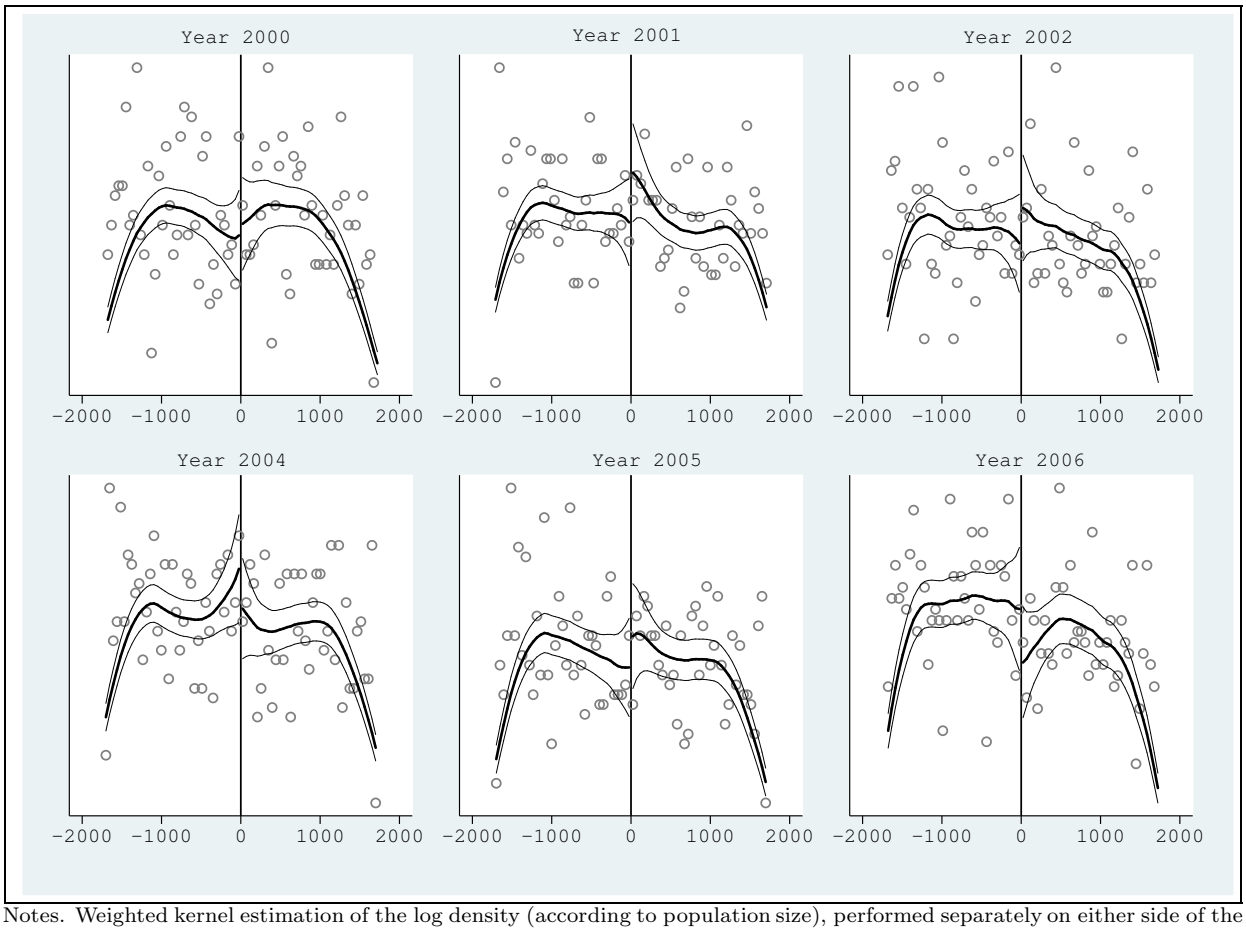
Notes. Frequency of cities according to population size. Cities below 50,941 inhabitants only. The vertical lines identify the first seven FPM revenue-sharing thresholds. Mayoral terms 2001–2005 and 2005–2009.

Figure 4 – McCrary Density Tests: Pooled and Individual Thresholds



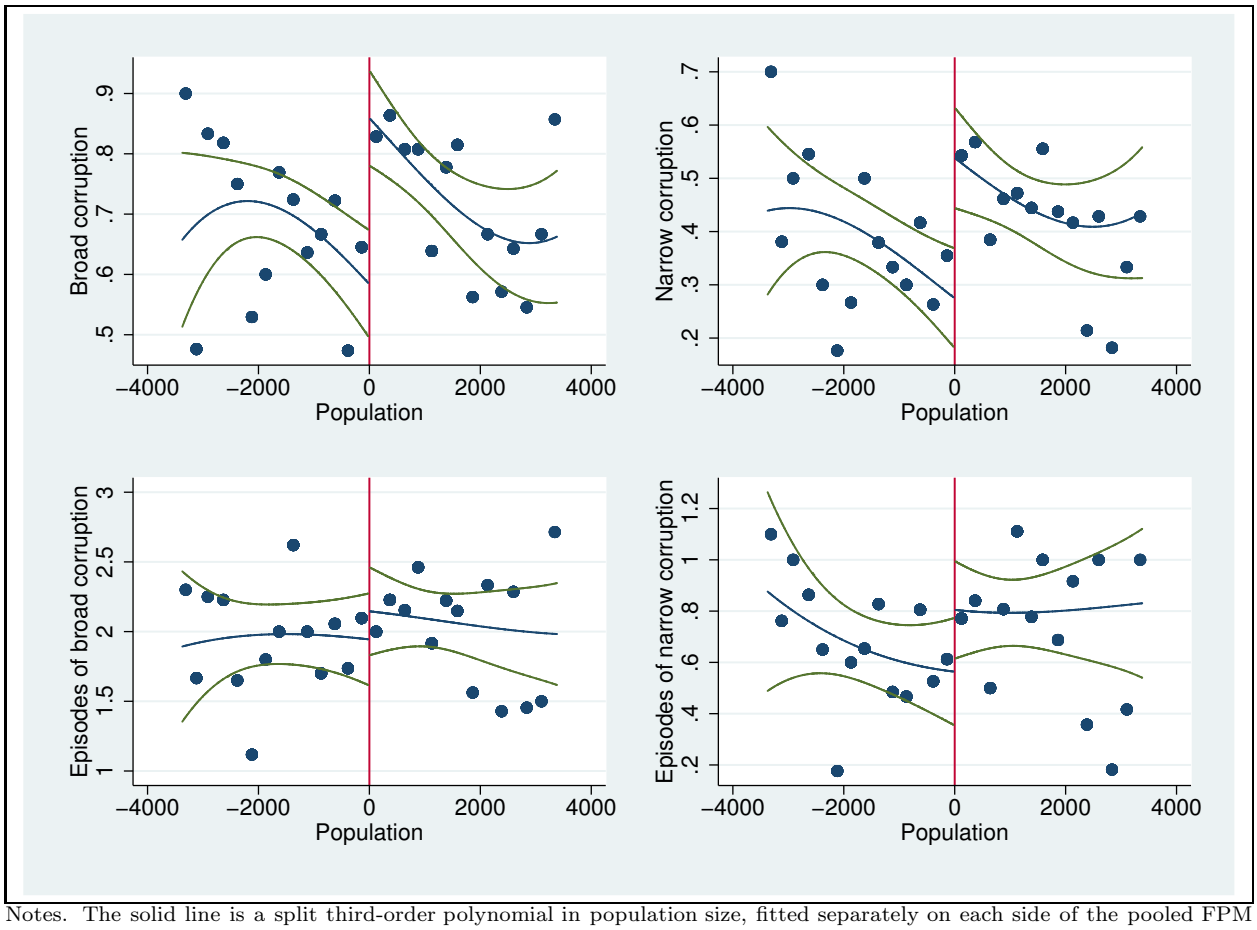
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of each pooled or individual FPM revenue-sharing threshold. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables. Mayoral terms 2001–2005 and 2005–2009.

Figure 5 – McCrary Density Tests: Pooled Threshold Year by Year



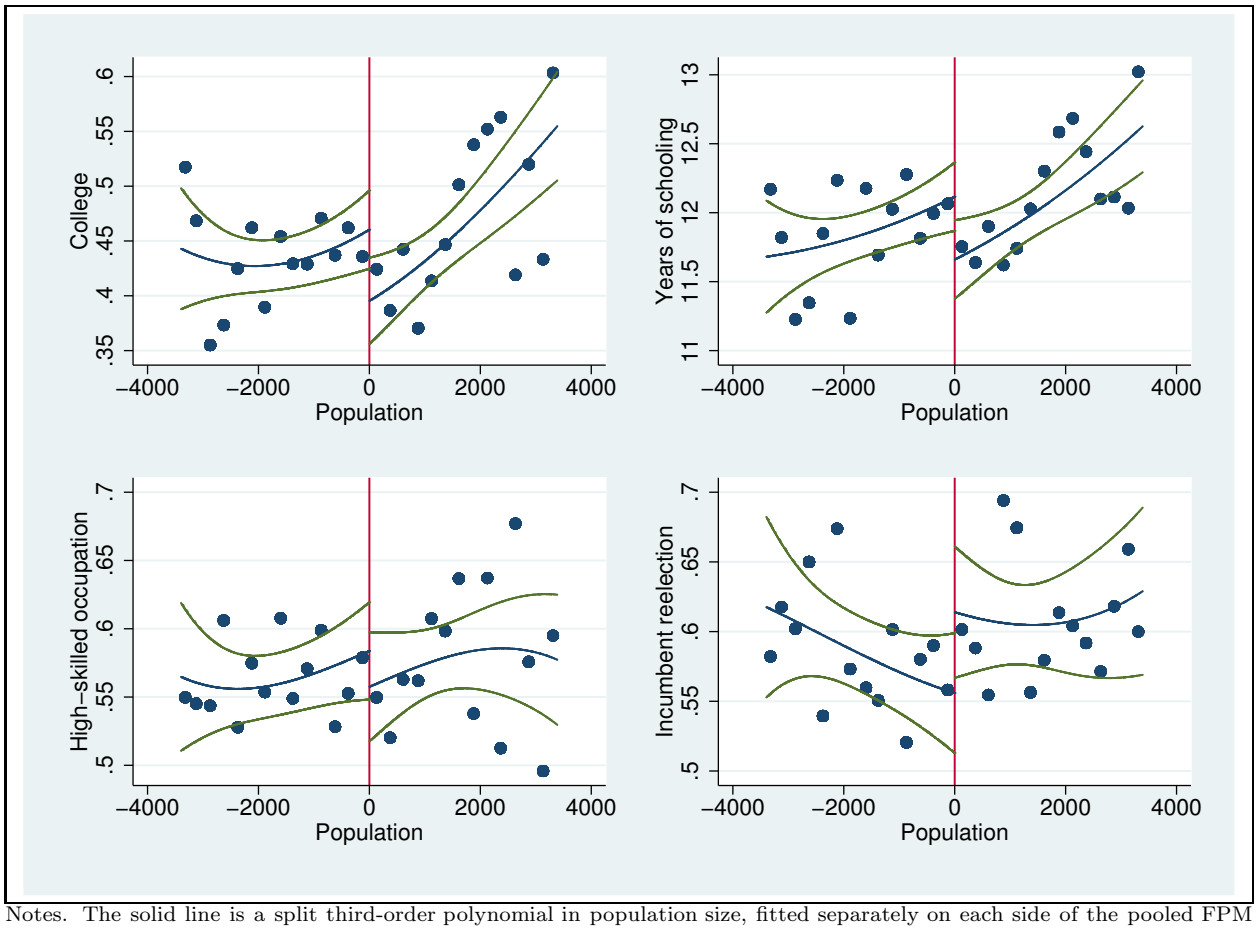
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of the pooled FPM revenue-sharing threshold (1–7) for each year in the sample period. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables. Mayoral terms 2001–2005 and 2005–2009.

Figure 6 – Intention-to-Treat Jumps: Corruption Measures



Notes. The solid line is a split third-order polynomial in population size, fitted separately on each side of the pooled FPM thresholds at zero (population size is normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval). The dashed lines are the 95% confidence interval of the polynomial. Scatter points are averaged over 250-unit intervals. Small sample with corruption variables (530 obs.). Terms 2001–2005 and 2005–2009.

Figure 7 – Intention-to-Treat Jumps: Opponents' Characteristics and Election Outcome



Notes. The solid line is a split third-order polynomial in population size, fitted separately on each side of the pooled FPM thresholds at zero (population size is normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval). The dashed lines are the 95% confidence interval of the polynomial. Scatter points are averaged over 250-unit intervals. Large sample with political selection variables (2,430 obs.). Terms 2001–2005 and 2005–2009.

Table 1 – FPM Coefficients

Population	FPM Coefficient
Below 10,189	0.6
10,189–13,584	0.8
13,585–16,980	1
16,981–23,772	1.2
23,773–30,564	1.4
30,564–37,356	1.6
37,356–44,148	1.8
44,148–50,940	2
Above 50,940	2–4

Notes. *FPM coefficient* is the coefficient used in the FPM revenue-sharing mechanism described in Section **??**. The underlined thresholds are those studied in our empirical exercise.

Table 2 – Actual and Theoretical FPM Transfers

	Small sample			Large sample		
Population	Actual transfers	Theoretical transfers	Obs.	Actual transfers	Theoretical transfers	Obs.
6,793–10,188	19.35	17.32	123	20.00	18.93	683
10,189–13,584	24.38	22.45	128	26.22	25.36	516
13,585–16,980	29.77	28.55	99	32.71	32.41	415
16,981–23,772	36.07	35.07	114	38.86	38.83	519
23,773–30,563	41.89	40.49	66	45.48	45.92	302
30,564–37,355	47.27	46.26	42	51.47	52.08	188
37,356–44,147	51.92	50.47	21	58.42	58.48	108
44,148–50,940	61.48	62.05	13	62.50	63.82	57
Total	31.68	30.22	606	33.79	33.44	2,788

Notes. *Population* is the number of resident inhabitants. The other columns report the average values of actual and theoretical FPM transfers (expressed in hundred thousand Brazilian *reais* at 2000 prices). *Small sample* refers to observations for which corruption measures are available (random audit reports). *Large sample* refers to observations for which political selection variables are available (i.e., where the incumbent runs for reelection). Mayoral terms 2001–2005 and 2005–2009.

Table 3 – Corruption Measures

Population	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes	Obs.
6,793–10,188	0.72	0.35	1.75	0.56	123
10,189–13,584	0.73	0.50	2.05	0.84	128
13,585–16,980	0.72	0.39	2.27	0.64	99
16,981–23,772	0.78	0.53	2.13	0.92	114
23,773–30,563	0.67	0.41	1.94	0.82	66
30,564–37,355	0.62	0.31	1.83	0.67	42
37,356–44,147	0.62	0.24	1.57	0.33	21
44,148–50,940	0.62	0.46	1.85	0.69	13
Total	0.71	0.42	1.99	0.73	606

Notes. *Population* is the number of resident inhabitants. The other columns report the average values of the corruption measures. The first and second measures are dummies; the third and fourth measures are the number of violation episodes. See Section **??** for the definition of broad versus narrow corruption. Mayoral terms 2001–2005 and 2005–2009.

Table 4 – Opponents' Characteristics and Election Outcome

Population	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
6,793–10,188	0.39	11.43	0.53	0.58	683
10,189–13,584	0.39	11.56	0.56	0.59	516
13,585–16,980	0.43	11.89	0.60	0.58	415
16,981–23,772	0.49	12.11	0.58	0.62	519
23,773–30,564	0.49	12.50	0.59	0.58	302
30,564–37,356	0.52	12.63	0.58	0.59	188
37,356–44,148	0.52	12.66	0.63	0.69	108
44,148–50,940	0.67	13.42	0.60	0.65	57
Total	0.44	11.93	0.57	0.59	2,788

Notes. *Population* is the number of resident inhabitants. The other columns report the average values of the characteristics of the pool of opponents or the reelection of the incumbent. All variables are dummies, except *Years of schooling*. See Section **??** for the definition of high-skilled occupation. Mayoral terms 2001–2005 and 2005–2009.

Table 5 – Balance Tests of Invariant Town Characteristics

	Area	North	Northeast	Center	South	Southeast	Obs.
Thresholds 1–7	3.981	0.020	-0.059	-0.014	0.055	-0.003	2,788
	(8.212)	(0.033)	(0.061)	(0.032)	(0.045)	(0.057)	
Thresholds 1–3	-4.411	0.035	-0.080	-0.028	0.039	0.034	2,133
	(3.610)	(0.034)	(0.068)	(0.037)	(0.050)	(0.062)	
Threshold 1	-2.537	-0.005	-0.131	-0.011	0.130	0.017	1,199
	(3.817)	(0.049)	(0.121)	(0.064)	(0.099)	(0.117)	
Threshold 2	2.701	-0.007	-0.012	-0.038	-0.055	0.112	931
	(6.885)	(0.073)	(0.127)	(0.079)	(0.096)	(0.118)	
Threshold 3	-1.342	0.088	-0.178	0.018	0.070	0.001	934
	(9.039)	(0.063)	(0.117)	(0.064)	(0.073)	(0.091)	

Notes. Discontinuity of invariant town characteristics (area size in km^2 and geographic location) at the FPM thresholds, estimated as the jump of a (split) third-order polynomial around pooled thresholds (i.e., with population normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval) or around individual thresholds. Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6 – Balance Tests of Pre-Treatment Town Characteristics

	Employed	Refrigerator	Radio	Car	Water and sewer	Obs.
Thresholds 1–7	-0.170	0.559	0.460	0.031	-0.118	2,217
	(0.700)	(0.717)	(0.594)	(0.307)	(0.685)	
Thresholds 1–3	-0.714	0.969	0.835	0.079	0.423	1,644
	(0.794)	(0.761)	(0.654)	(0.330)	(0.711)	
Threshold 1	-0.143	-0.068	0.133	-0.160	0.284	879
	(1.058)	(0.591)	(0.419)	(0.224)	(0.641)	
Threshold 2	0.048	-0.730	-0.230	-0.058	-0.967	742
	(0.927)	(0.562)	(0.451)	(0.250)	(0.652)	
Threshold 3	0.773	0.679	0.224	0.018	0.244	765
	(0.862)	(0.420)	(0.328)	(0.159)	(0.501)	

Notes. Discontinuity of pre-treatment town characteristics (from the 1980 Census) at the FPM thresholds, estimated as the jump of a (split) third-order polynomial around pooled thresholds (i.e., with population normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval) or around individual thresholds. All variables are per capita and measure average employment; refrigerator, radio, or car ownership; house access to water and sewer. Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7 – Budget Elasticities with respect to Theoretical Transfers

	Total revenues	Local taxes	Total expenditure	Infrastructure expenditure	Personnel expenditure	Obs.
Thresholds 1–7	0.527***	-0.700***	0.479***	0.708***	0.336***	2,788
	(0.108)	(0.253)	(0.108)	(0.218)	(0.120)	
Thresholds 1–3	0.627***	-0.711**	0.587***	1.017***	0.442***	2,133
	(0.130)	(0.299)	(0.132)	(0.278)	(0.146)	
Threshold 1	0.717***	-0.693*	0.637***	1.176***	0.391**	1,199
	(0.184)	(0.421)	(0.186)	(0.445)	(0.195)	
Threshold 2	0.511**	-1.321***	0.513**	1.233**	0.228	931
	(0.227)	(0.479)	(0.238)	(0.573)	(0.245)	
Threshold 3	0.866***	-1.072*	0.902***	1.589***	0.521	934
	(0.285)	(0.648)	(0.275)	(0.515)	(0.318)	

Notes. Elasticities of (self-reported) revenues and expenditure variables with respect to theoretical transfers, estimated as the log-version of equation (**??**). All variables are expressed in Brazilian *reais* at 2000 prices. Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 8 – Reduced-Form Effects: FPM Transfers and Corruption Measures

	FPM transfers	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes	Obs.
Thresholds 1–7	0.553***	0.021***	0.018**	0.012	0.033**	606
	(0.054)	(0.007)	(0.008)	(0.027)	(0.016)	
Thresholds 1–3	0.599***	0.030***	0.022**	0.024	0.037*	464
	(0.075)	(0.009)	(0.010)	(0.037)	(0.022)	
Threshold 1	0.564***	0.013	-0.001	0.092	0.021	251
	(0.118)	(0.019)	(0.018)	(0.057)	(0.037)	
Threshold 2	0.707***	0.043***	0.030**	0.132*	0.056	227
	(0.107)	(0.016)	(0.015)	(0.073)	(0.043)	
Threshold 3	0.703***	0.032**	0.039**	-0.008	0.061*	213
	(0.159)	(0.014)	(0.015)	(0.067)	(0.036)	

Notes. Reduced-form effects of theoretical transfers on actual FPM transfers and corruption measures, estimated as in equations (**??**)-(**??**). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 9 – IV Estimates: Corruption Measures

	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes	Obs.
Thresholds 1–7	0.038***	0.032**	0.022	0.060**	606
	(0.013)	(0.014)	(0.047)	(0.028)	
Thresholds 1–3	0.050***	0.037**	0.039	0.062*	464
	(0.016)	(0.017)	(0.060)	(0.036)	
Threshold 1	0.023	-0.001	0.163*	0.036	251
	(0.032)	(0.031)	(0.099)	(0.063)	
Threshold 2	0.061**	0.043**	0.187*	0.079	227
	(0.024)	(0.021)	(0.103)	(0.059)	
Threshold 3	0.044**	0.054**	-0.011	0.084*	213
	(0.020)	(0.022)	(0.088)	(0.047)	

Notes. Effects of FPM transfers (instrumented with theoretical transfers) on corruption measures, estimated as in equation (**??**). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 10 – Robustness Checks: Corruption Measures

	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes	Obs.
			Spline (3^{rd}-order) polynomial		
Thresholds 1–7	0.039**	0.028*	0.032	0.057*	606
	(0.016)	(0.016)	(0.057)	(0.033)	
Thresholds 1–3	0.049***	0.035*	0.056	0.052	464
	(0.018)	(0.018)	(0.066)	(0.037)	
			2^{nd}-order polynomial		
Thresholds 1–7	0.031***	0.033***	0.028	0.051**	606
	(0.012)	(0.012)	(0.039)	(0.024)	
Thresholds 1–3	0.043***	0.037**	0.039	0.062*	464
	(0.015)	(0.016)	(0.055)	(0.034)	
			Spline (2^{nd}-order) polynomial		
Thresholds 1–7	0.041***	0.040***	0.022	0.072**	606
	(0.015)	(0.015)	(0.053)	(0.031)	
Thresholds 1–3	0.048***	0.044**	0.036	0.065*	464
	(0.016)	(0.018)	(0.060)	(0.037)	
			4^{th}-order polynomial		
Thresholds 1–7	0.037***	0.028**	0.017	0.051*	606
	(0.013)	(0.014)	(0.049)	(0.029)	
Thresholds 1–3	0.050***	0.038**	0.038	0.063*	464
	(0.016)	(0.017)	(0.060)	(0.035)	
			Spline (4^{th}-order) polynomial		
Thresholds 1–7	0.033**	0.031**	0.029	0.064**	606
	(0.015)	(0.015)	(0.049)	(0.028)	
Thresholds 1–3	0.047***	0.026	0.054	0.042	464
	(0.018)	(0.019)	(0.066)	(0.038)	
			Spline (3^{rd}-order) polynomial including 10,000		
Thresholds 1–7	0.039**	0.029*	0.022	0.053*	606
	(0.016)	(0.016)	(0.057)	(0.032)	
Thresholds 1–3	0.024**	0.019*	-0.014	0.031	464
	(0.011)	(0.011)	(0.043)	(0.023)	
			Spline (3^{rd}-order) polynomial above 10,000		
Thresholds 1–7	0.055***	0.040**	0.038	0.066*	491
	(0.020)	(0.019)	(0.066)	(0.039)	
Thresholds 1–3	0.061***	0.039*	0.057	0.044	349
	(0.022)	(0.021)	(0.074)	(0.043)	

Notes. Effects of FPM transfers (instrumented with theoretical transfers) on corruption measures, estimated as in equation (**??**) adjusting the functional form of $G(P_i)$ as specified, or restricting the sample above 10,000 (wage threshold). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 11 – Reduced-Form Effects: FPM Transfers, Opponents' Characteristics, and Election Outcome

	FPM transfers	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
Thresholds 1–7	0.732***	-0.007**	-0.059***	-0.002	0.009***	2,788
	(0.025)	(0.003)	(0.019)	(0.003)	(0.003)	
Thresholds 1–3	0.667***	-0.011***	-0.100***	-0.007	0.010**	2,133
	(0.025)	(0.004)	(0.031)	(0.004)	(0.005)	
Threshold 1	0.566***	-0.021***	-0.169***	-0.013	0.020**	1,199
	(0.045)	(0.007)	(0.059)	(0.008)	(0.009)	
Threshold 2	0.674***	-0.005	-0.091	0.004	0.005	931
	(0.050)	(0.008)	(0.062)	(0.008)	(0.009)	
Threshold 3	0.694***	-0.003	-0.057	-0.012*	0.012	934
	(0.050)	(0.007)	(0.049)	(0.007)	(0.008)	

Notes. Reduced-form effects of theoretical transfers on actual FPM transfers, characteristics of the pool of opponents, and the incumbent mayor's reelection probability, estimated as in equations (**??**)-(**??**). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 12 – IV Estimates: Opponents' Characteristics and Election Outcome

	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
Thresholds 1–7	-0.009**	-0.080***	-0.002	0.012***	2,788
	(0.004)	(0.026)	(0.004)	(0.005)	
Thresholds 1–3	-0.017***	-0.150***	-0.010	0.015**	2,133
	(0.006)	(0.046)	(0.006)	(0.007)	
Threshold 1	-0.037***	-0.298***	-0.023*	0.036**	1,199
	(0.014)	(0.108)	(0.014)	(0.016)	
Threshold 2	-0.008	-0.134	0.006	0.007	931
	(0.011)	(0.091)	(0.011)	(0.014)	
Threshold 3	-0.004	-0.082	-0.018*	0.017	934
	(0.010)	(0.070)	(0.010)	(0.012)	

Notes. Effects of FPM transfers (instrumented with theoretical transfers) on the characteristics of the pool of opponents and the incumbent mayor's reelection probability, estimated as in equation (**??**). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 13 – Robustness Checks: Opponents' Characteristics and Election Outcome

	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
			Spline (3^{rd}-order) polynomial		
Thresholds 1–7	-0.011**	-0.088***	-0.003	0.009	2,788
	(0.004)	(0.030)	(0.004)	(0.005)	
Thresholds 1–3	-0.020***	-0.161***	-0.012*	0.015*	2,133
	(0.007)	(0.051)	(0.007)	(0.008)	
			2^{nd}-order polynomial		
Thresholds 1–7	-0.008**	-0.069***	-0.001	0.012***	2,788
	(0.004)	(0.025)	(0.004)	(0.004)	
Thresholds 1–3	-0.014**	-0.129***	-0.007	0.019***	2,133
	(0.006)	(0.040)	(0.005)	(0.006)	
			Spline (2^{nd}-order) polynomial		
Thresholds 1–7	-0.010**	-0.082***	-0.003	0.009*	2,788
	(0.004)	(0.029)	(0.004)	(0.005)	
Thresholds 1–3	-0.018***	-0.153***	-0.011*	0.016**	2,133
	(0.007)	(0.048)	(0.007)	(0.008)	
			4^{th}-order polynomial		
Thresholds 1–7	-0.010***	-0.083***	-0.002	0.012***	2,788
	(0.004)	(0.026)	(0.004)	(0.005)	
Thresholds 1–3	-0.017***	-0.154***	-0.011*	0.016**	2,133
	(0.006)	(0.047)	(0.006)	(0.007)	
			Spline (4^{th}-order) polynomial		
Thresholds 1–7	-0.009**	-0.079**	-0.006	0.009	2,788
	(0.005)	(0.032)	(0.005)	(0.006)	
Thresholds 1–3	-0.020***	-0.160***	-0.013*	0.017**	2,133
	(0.008)	(0.054)	(0.007)	(0.009)	
		Spline (3^{rd}-order) polynomial including 10,000			
Thresholds 1–7	-0.011**	-0.093***	-0.004	0.008	2,788
	(0.005)	(0.031)	(0.004)	(0.005)	
Thresholds 1–3	-0.018**	-0.174***	-0.014*	0.012	2,133
	(0.007)	(0.054)	(0.007)	(0.009)	
		Spline (3^{rd}-order) polynomial above 10,000			
Thresholds 1–7	-0.004	-0.041	-0.001	0.008	2,138
	(0.005)	(0.036)	(0.005)	(0.007)	
Thresholds 1–3	-0.008	-0.115*	-0.008	0.013	1,483
	(0.009)	(0.067)	(0.009)	(0.011)	

Notes. Effects of FPM transfers (instrumented with theoretical transfers) on the characteristics of the pool of opponents and the incumbent mayor's reelection probability, estimated as in equation (**??**) adjusting the functional form of $G(P_i)$ as specified, or restricting the sample above 10,000 (wage threshold). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 14 – IV Estimates: Politicians' Characteristics in All Municipalities

	College	Years of schooling	High-skilled occupation	Obs.
		Panel A		
Thresholds 1–7	-0.011**	-0.087***	-0.001	2,788
	(0.005)	(0.032)	(0.005)	
Thresholds 1–3	-0.015**	-0.151***	-0.010	2,133
	(0.008)	(0.055)	(0.007)	
Threshold 1	-0.037**	-0.430***	-0.016	1,199
	(0.016)	(0.131)	(0.016)	
Threshold 2	-0.015	-0.174	0.008	931
	(0.014)	(0.107)	(0.014)	
Threshold 3	-0.002	-0.049	-0.025**	934
	(0.013)	(0.084)	(0.013)	
		Panel B		
Thresholds 1–7	-0.004	-0.063***	0.003	5,452
	(0.003)	(0.019)	(0.003)	
Thresholds 1–3	-0.010**	-0.109***	0.000	4,177
	(0.004)	(0.030)	(0.004)	
Threshold 1	-0.017**	-0.171**	-0.004	2,360
	(0.009)	(0.067)	(0.009)	
Threshold 2	-0.004	-0.113*	0.012	1,799
	(0.008)	(0.058)	(0.008)	
Threshold 3	-0.004	-0.079*	-0.002	1,817
	(0.007)	(0.047)	(0.007)	
		Panel C		
Thresholds 1–7	-0.004	-0.059***	0.002	5,281
	(0.003)	(0.020)	(0.003)	
Thresholds 1–3	-0.010**	-0.107***	-0.002	4,027
	(0.004)	(0.031)	(0.004)	
Threshold 1	-0.016*	-0.181**	-0.007	2,267
	(0.009)	(0.071)	(0.009)	
Threshold 2	-0.006	-0.107*	0.009	1,745
	(0.008)	(0.061)	(0.008)	
Threshold 3	-0.004	-0.075	-0.004	1,760
	(0.007)	(0.050)	(0.007)	

Notes. Same estimations as in Table 12 but with different samples. *Panel A* considers only the opponents (of the incumbent who runs for reelection) with the highest number of votes. *Panel B* considers all candidates in municipalities where the incumbent does not run for reelection and all opponents in municipalities where the incumbent reruns. *Panel C* considers all the opponents of the political party of the incumbent mayor (irrespective of whether the incumbent reruns or not). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 15 – Opponents' Characteristics and Corruption Measures

	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes
		Thresholds 1–7 (Obs. 229)		
Years of schooling	-0.011	-0.016	0.011	0.033
	(0.017)	(0.016)	(0.053)	(0.029)
College	0.141	0.135	0.164	-0.072
	0.129)	(0.125)	(0.385)	(0.263)
High-skilled	-0.110	-0.072	-0.053	-0.063
	(0.075)	(0.080)	(0.222)	(0.138)
		Thresholds 1–3 (Obs. 179)		
Years of schooling	-0.028	-0.014	-0.006	0.045
	(0.017)	(0.018)	(0.059)	(0.032)
College	0.216*	0.106	0.017	-0.249
	(0.130)	(0.146)	(0.423)	(0.288)
High-skilled	-0.204**	-0.082	-0.149	-0.116
	(0.079)	(0.092)	(0.240)	(0.149)

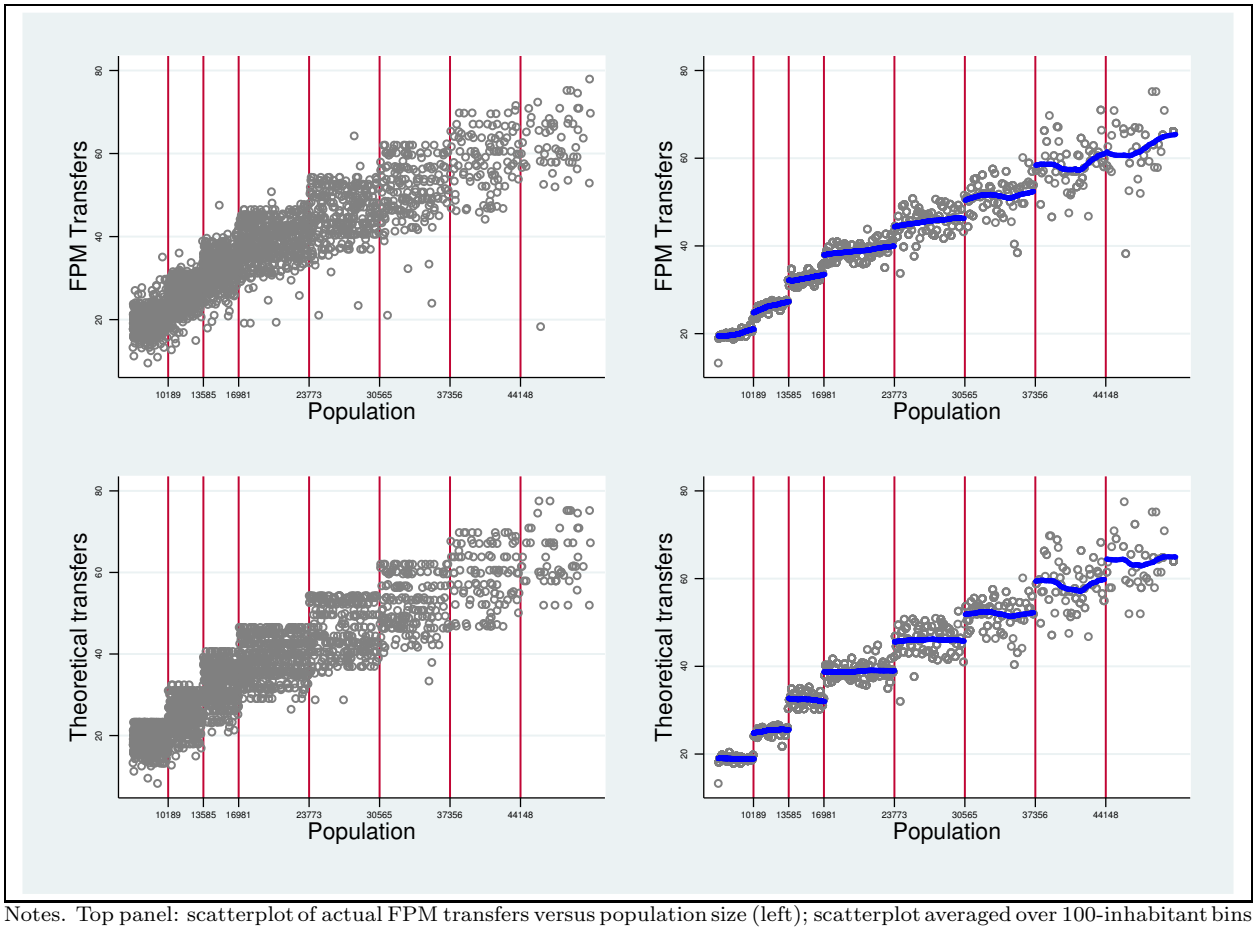
Notes. Probit (first and second corruption measures) and OLS (third and fourth corruption measures) estimations of the correlation between corruption and opponents' characteristics; marginal effects reported. Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 16 – Opponents' Characteristics and the Impact of Transfers on Corruption

	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes
		INTERACTION WITH COLLEGE:		
		Thresholds 1–7 (Obs. 229)		
Interaction	-0.002	-0.006	0.003	-0.009
	(0.008)	(0.007)	(0.026)	(0.016)
FPM	0.029*	0.030	0.021	0.058
	(0.017)	(0.018)	(0.060)	(0.037)
		Thresholds 1–3 (Obs. 179)		
Interaction	-0.021*	-0.025**	-0.095***	-0.064**
	(0.011)	(0.011)	(0.033)	(0.025)
FPM	0.051**	0.039	0.098	0.114**
	(0.024)	(0.027)	(0.089)	(0.058)
		INTERACTION WITH YEARS OF SCHOOLING:		
		Thresholds 1–7 (Obs. 229)		
Interaction	0.000	-0.000	0.000	-0.001
	(0.001)	(0.001)	(0.003)	(0.002)
FPM	0.024	0.032	0.018	0.065
	(0.023)	(0.022)	(0.077)	(0.045)
		Thresholds 1–3 (Obs. 179)		
Interaction	-0.002	-0.001	-0.011**	-0.004
	(0.002)	(0.002)	(0.006)	(0.004)
FPM	0.072*	0.049	0.198	0.148*
	(0.039)	(0.036)	(0.128)	(0.081)
		INTERACTION WITH HIGH-SKILLED OCCUPATION:		
		Thresholds 1–7 (Obs. 229)		
Interaction	0.006	-0.004	-0.005	-0.006
	(0.007)	(0.008)	(0.023)	(0.016)
FPM	0.026	0.029	0.026	0.059
	(0.017)	(0.018)	(0.056)	(0.036)
		Thresholds 1–3 (Obs. 179)		
Interaction	-0.020	-0.027**	-0.079**	-0.077***
	(0.012)	(0.011)	(0.033)	(0.025)
FPM	0.057**	0.048*	0.116	0.139**
	(0.025)	(0.027)	(0.091)	(0.059)

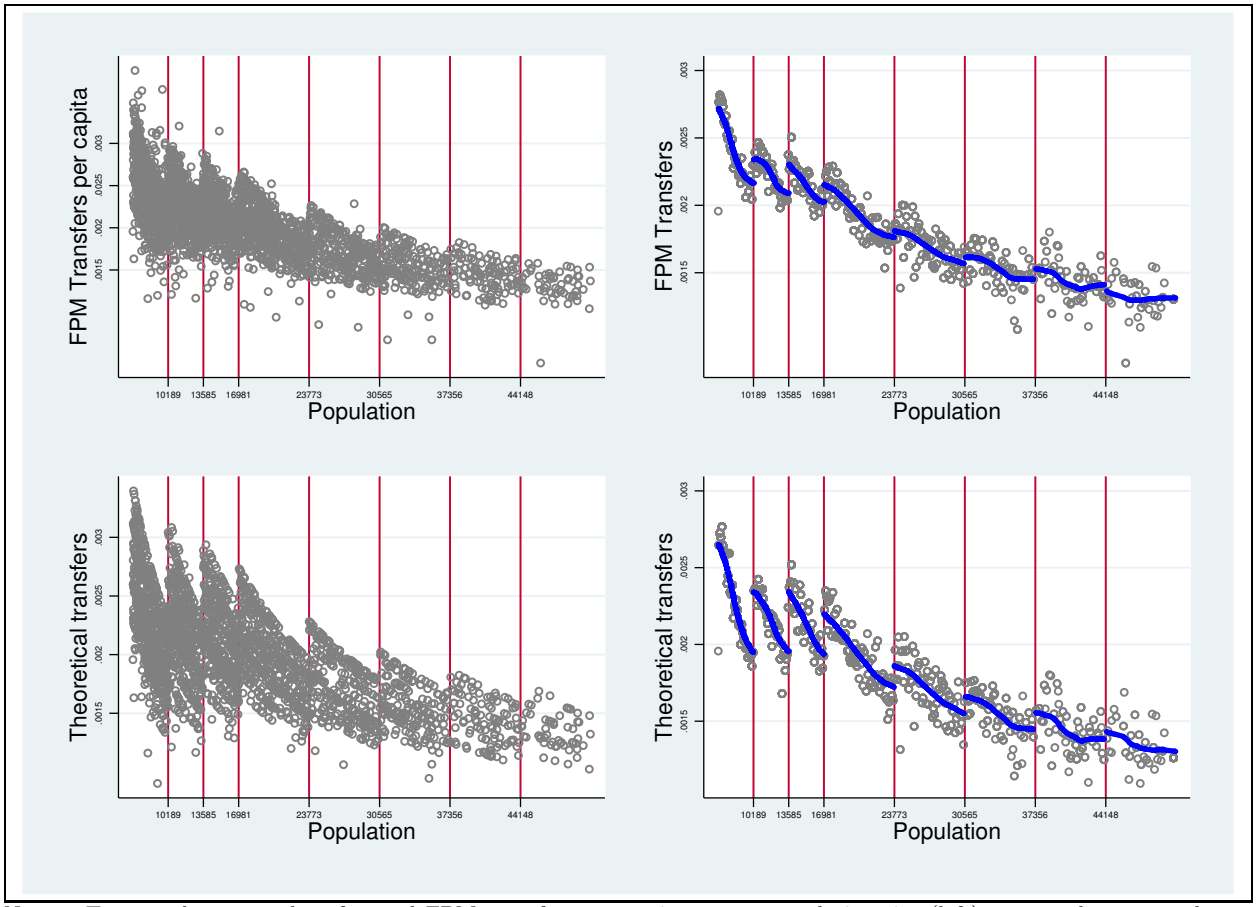
Notes. Effects of FPM transfers and their interaction with each opponents' characteristic (instrumented with theoretical transfers and their interaction with each opponents' characteristic). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure 1 – Actual and Theoretical FPM Transfers



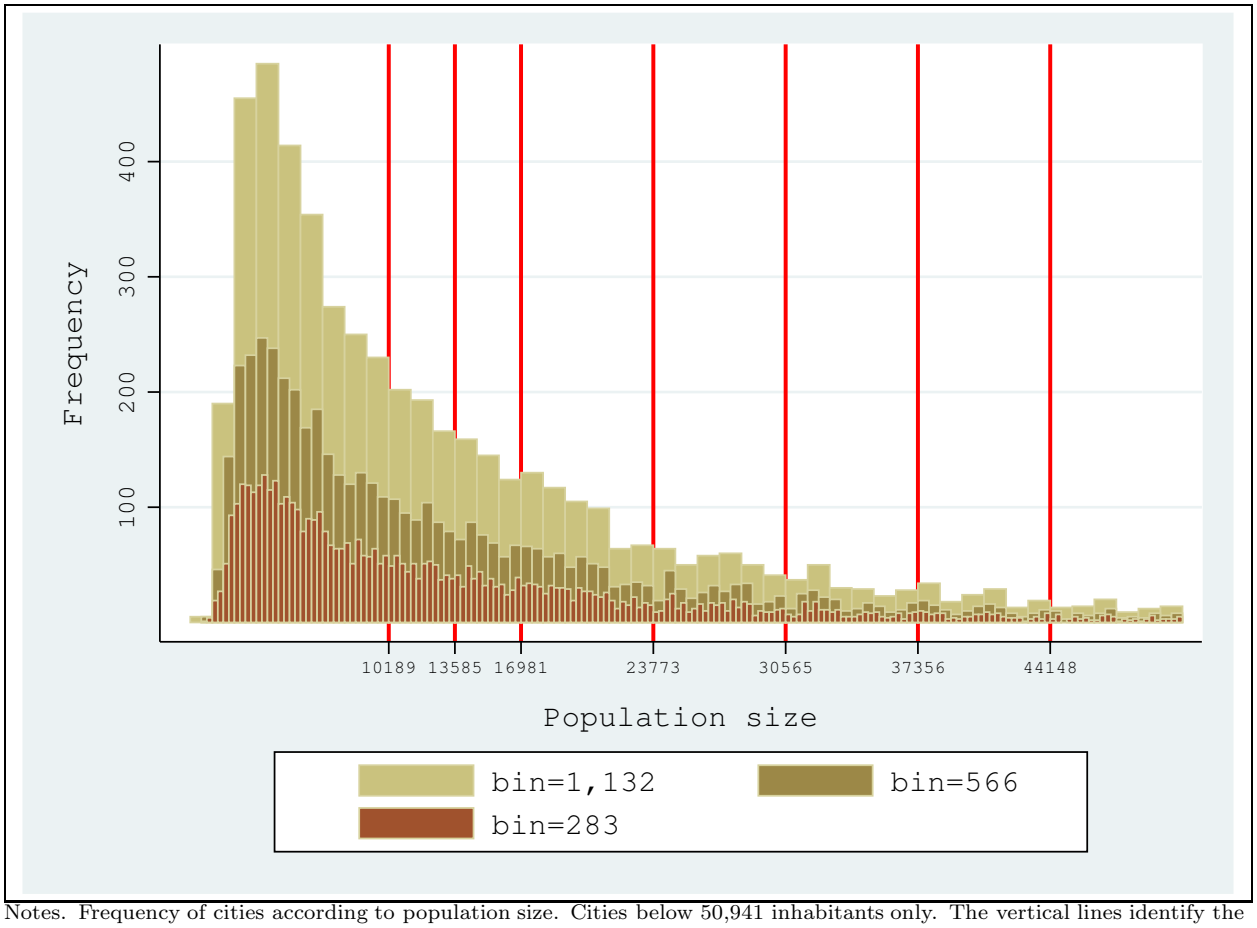
Notes. Top panel: scatterplot of actual FPM transfers versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Mayoral terms 2001–2005 and 2005–2009.

Figure 2 – Actual and Theoretical FPM Transfers (per capita)



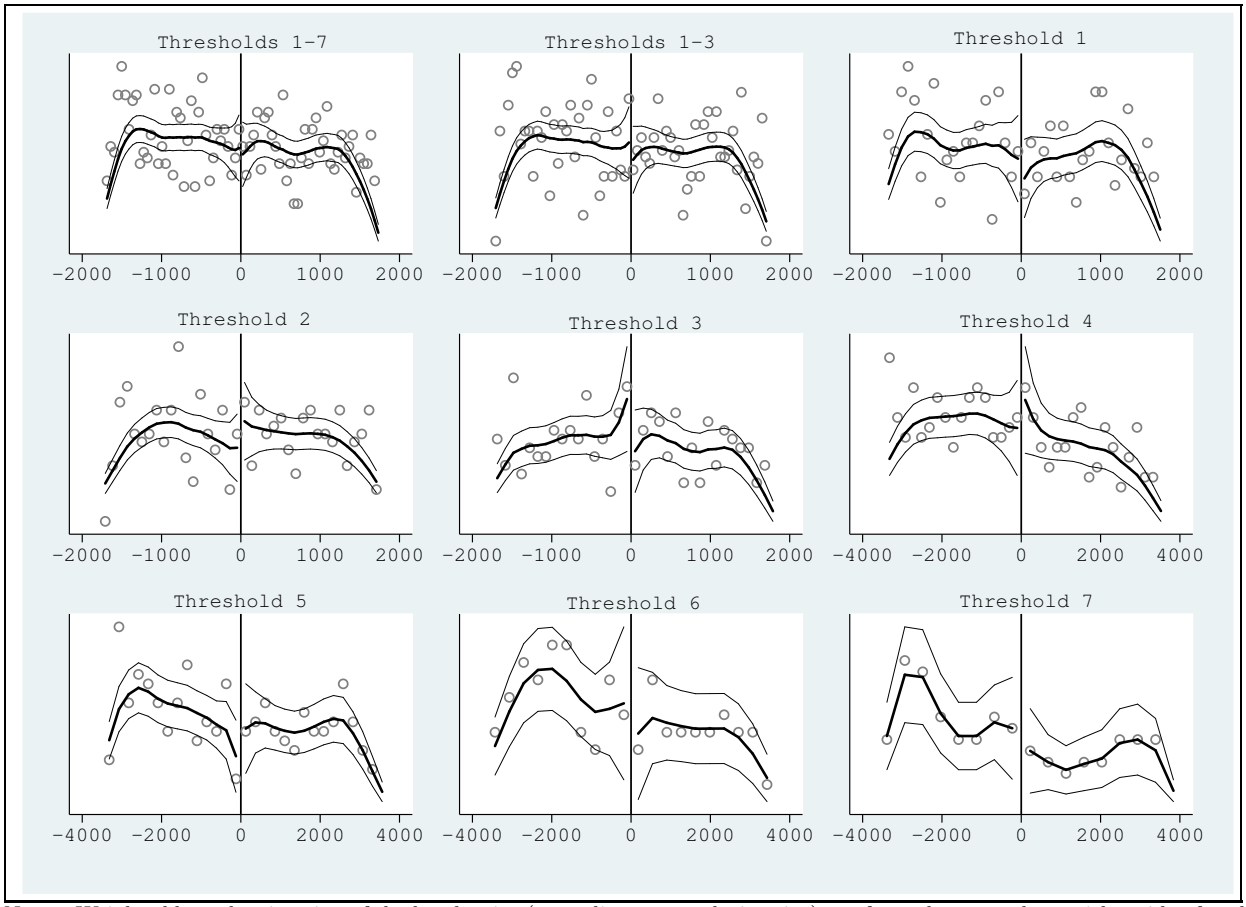
Notes. Top panel: scatterplot of actual FPM transfers per capita versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers per capita versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Mayoral terms 2001–2005 and 2005–2009.

Figure 3 – Population Distribution (<50,941)



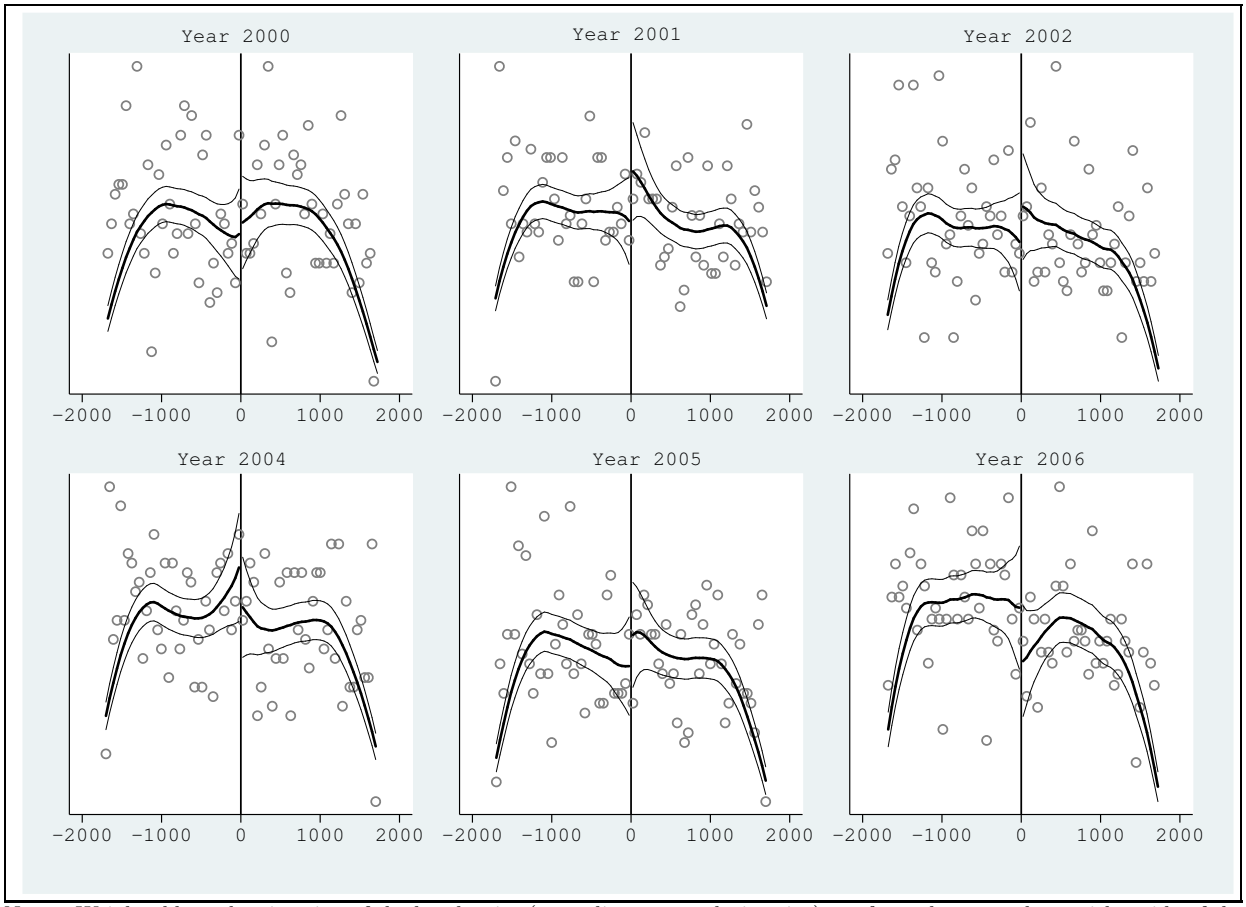
Notes. Frequency of cities according to population size. Cities below 50,941 inhabitants only. The vertical lines identify the first seven FPM revenue-sharing thresholds. Mayoral terms 2001–2005 and 2005–2009.

Figure 4 – McCrary Density Tests: Pooled and Individual Thresholds



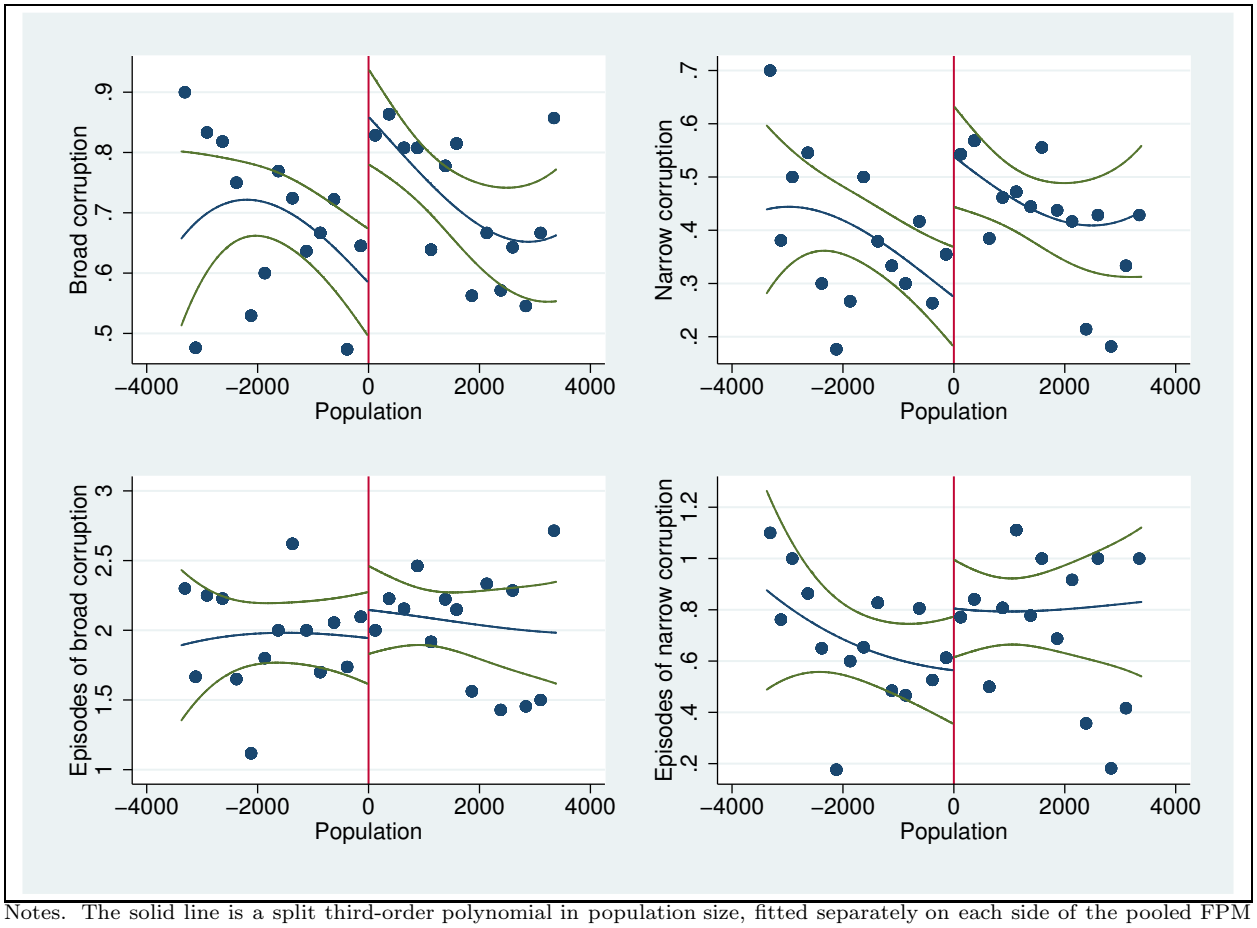
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of each pooled or individual FPM revenue-sharing threshold. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables. Mayoral terms 2001–2005 and 2005–2009.

Figure 5 – McCrary Density Tests: Pooled Threshold Year by Year



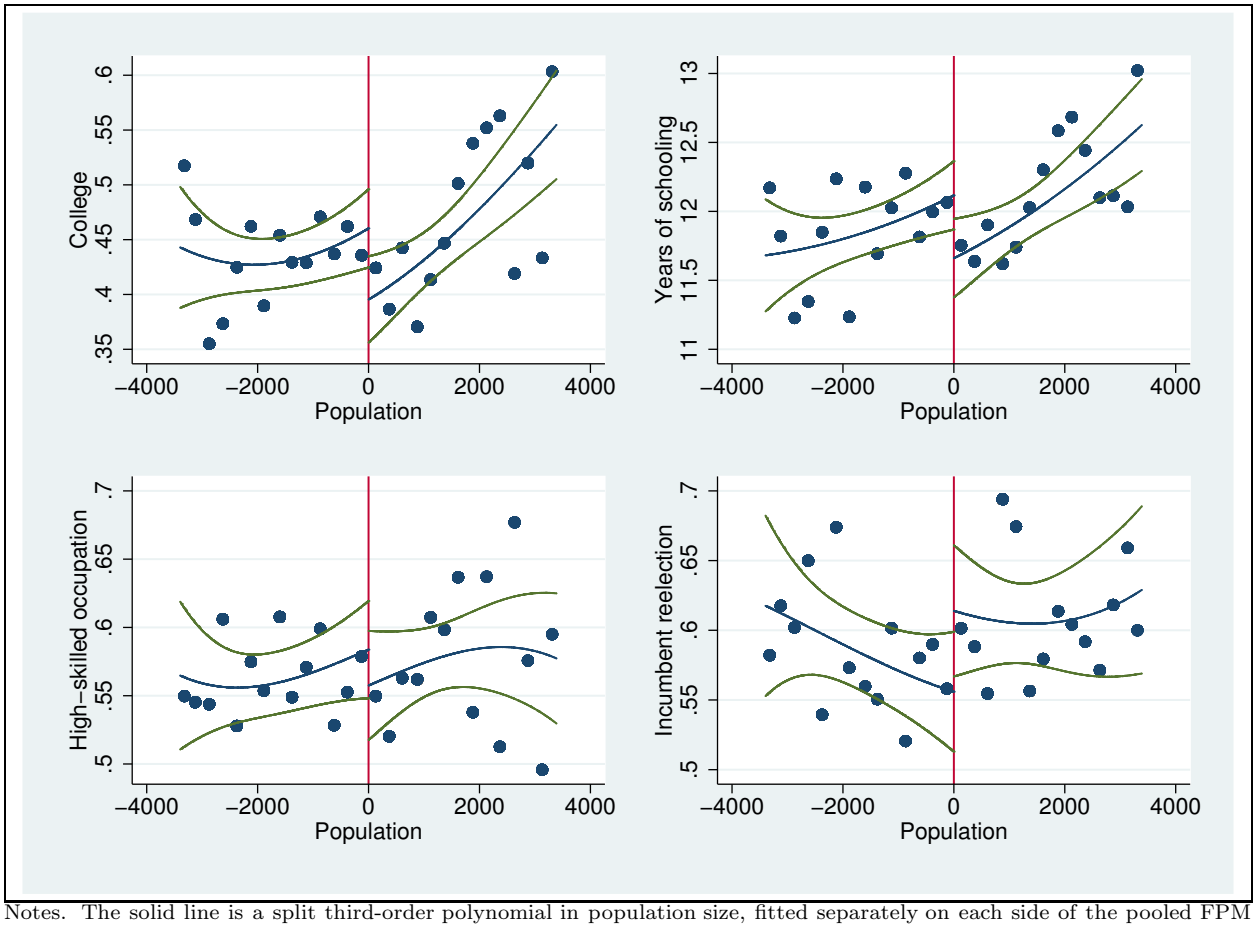
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of the pooled FPM revenue-sharing threshold (1–7) for each year in the sample period. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables. Mayoral terms 2001–2005 and 2005–2009.

Figure 6 – Intention-to-Treat Jumps: Corruption Measures



Notes. The solid line is a split third-order polynomial in population size, fitted separately on each side of the pooled FPM thresholds at zero (population size is normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval). The dashed lines are the 95% confidence interval of the polynomial. Scatter points are averaged over 250-unit intervals. Small sample with corruption variables (530 obs.). Terms 2001–2005 and 2005–2009.

Figure 7 – Intention-to-Treat Jumps: Opponents' Characteristics and Election Outcome



Notes. The solid line is a split third-order polynomial in population size, fitted separately on each side of the pooled FPM thresholds at zero (population size is normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval). The dashed lines are the 95% confidence interval of the polynomial. Scatter points are averaged over 250-unit intervals. Large sample with political selection variables (2,430 obs.). Terms 2001–2005 and 2005–2009.

Appendix I: IBGE Population Estimates

IBGE uses a top-down approach to consistently estimate population figures for the lower units partitioning the Brazilian territory. According to this methodology, IBGE first produces a population estimate for a larger area in the year t, called P_t. Then, this large area is split in N smaller areas P_{nt}, where $P_t = \sum_{n=1}^{N} P_{nt}$, with $n = 1, 2, ..., N$. For instance, assume that P_t is the population estimate for the entire Brazil, based on the estimated birth rates, mortality rates, and net migration. P_{nt} is instead the population estimate for a given state, and it is calculated in the following way:

$$P_{nt} = a_n P_t + b_n$$

where $a_n = (P_{nt_1} - P_{nt_0})/(P_{t_1} - P_{t_0})$; $b_n = P_{nt_0} - a_n P_{nt_0}$; t refers to the year of the estimate; t_0 refers to the 1991 Census; and t_1 refers to the 2000 Census.

Population estimates at the municipal level follow the same logic. Municipalities within a given state are grouped by quartiles of both last Census population size and past population growth between Censuses; moreover, growing municipalities between the last two Censuses are separated from shrinking municipalities. Each of these $q = 1, 2, ..., Q$ cells of municipalities is then assigned its share of the state population estimate, P_{qnt}, proportional to the last cell-specific Census population. Finally, each municipality within every cell is assigned its population estimate, P_{mqnt}, based on past Census information. The specific formula for the municipal population estimates is therefore as follows:

$$P_{mqnt} = a_{mqn} P_{qnt} + b_{mqn}$$

where $a_{mqn} = P_{mqnt_1} - P_{mqnt_0}/P_{qnt_1} - P_{qnt_0}$; $b_{mqn} = P_{mqnt_0} - a_{mqn} P_{mqnt_0}$; t refers to the year of the estimate; t_0 refers to the 1991 Census; and t_1 refers to the 2000 Census.

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Appendix II: Examples of Violation Episodes

(1) Illegal procurement practices

(a) Limited competition. In the municipality of *Buritis* (state of *Rondônia*), in a bidding process regarding the purchase of food, the city invited three companies, two of them from the municipality of *Porto Velho*, 210 kilometers far from *Buritis*. Auditors contested this fact because in *Buritis* there are companies that could have participated in the auction. More importantly, the company that won the bid for all 64 items (42,000 *reais*) was owned by the mayor's wife. The mayor's wife was also the accountant of another company that was invited to participate in the auction.

(b) Manipulation of the bid value. In the municipality of *Itapira* (state of *São Paulo*), auditors found evidence of manipulation of the bid value for the acquisition of materials in the construction of the water supply system. According to Law No. 8666/93, if the value of the project is below a certain threshold, no bid process is required. Auditors found evidence that the municipal administration had divided the project into three (fake) sub-projects in order to avoid the bid procedure.

(2) Fraud

In the municipality of *Santa Terezinha* (state of *Bahia*), auditors found evidence of a simulated auction for the purchase of computer equipment worth about 10,000 *reais*. The companies alleged to have participated in the procurement practice were: *LL Equipmentos Informática Ltda.* (winner), *MSGL Informática Ltda.*, *Núcleo Comércio*, and *Servicos de Informátiça Ltda.* Although it is required that all bidders attend the opening of the tender envelopes, the company *MSGL Informática Ltda.*—which had never been involved in computer equipments sales—never participated to the auction. The director of the winning company (*LL Equipmentos)* declared to the auditors that: "(...) I sold computer equipment worth 10,000 *reais* to the municipality of *Santa Teresinha*, represented by the mayor's husband, who showed me two different proposals by other companies and asked me to under-bid them."

In the municipality of *Salinas da Margarida* (state of *Bahia*), there was evidence of a simulated auction involving funds for education (FUNDEF): in three bidding processes for

a total amount of 142,600 *reais*, the alleged participants denied any involvement in the auctions. For example, the owners of the companies *Plantek* and *J.S. Construções Gerais* formally declared to the auditors that they had not been invited to this auction and that their signatures had been falsified.

(3) Favoritism in the good receipt

In the municipality of *General Sampaio* (state of *Ceará*), auditors found out that the land on which a dam was built had been previously donated by the city to the actual owner, and that this person also owned the surrounding areas, hampering free access to the dam by the population.

(4) Over-invoicing

In the municipality of *São Fransciso do Conde* (state of *Bahia*), the construction company *Mazda* was hired without a bidding process to carry out the construction of a road nine kilometers long. The road should have been budgeted at about 1 million *reais*, but the invoices presented by the company proved that there had been a disbursement of 5 million *reais*. The municipal administration did not present any document justifying the expenditure. *Mazda*, a company with no experience in road construction, sub-contracted another company to perform the job only paying 1,800,000 *reais*.

(5) Diversion of funds

The municipality of *Buritis* (state of *Rondônia*) received 50,000 *reais* from the federal government to purchase a school bus for transporting students. Auditors found that the vehicle was also used to transport professors from the urban area to schools in rural areas. Furthermore, the school bus performed trips outside the municipality without justification.

In the municipality of *São Pedro do Piauí* (state of *Piauí*), daily stays in hotel (worth 4,400 *reais*) for some officials of the Municipal Secretary of Education were paid with no evidence that these stays had anything to do with activities related to their duties.

In the municipality of *Cândido Mendes* (state of *Maranhão*), 91% of the resources that should have been spent for the salaries of professors were actually used to pay public employees performing different duties.

In the municipality of *Belém* (state of *Paraíba)*, auditors found out that 160,000 *reais*

that should have been spent on basic health services (i.e., medical consultations, basic dental care, vaccinations, educational activities, etc.) were used to pay meals for the staff of the health program and to cover debt services of the municipality.

(6) Paid but not proven

The municipality of *Cerro Branco* (state of *Rio Grande do Sul*) did not provide any documentation to justify the expenditure of 29,100 *reais* for health services.

CHAPTER 3: Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil

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Abstract

We apply a regression discontinuity design in close electoral races to identify the impact of (exogenous) partisan alignment on federal transfers to municipal governments in Brazil. According to our results, municipalities where the mayor is affiliated with the coalition of the President receive larger transfers by about 36%–43% in the last two years of the term. This effect is mainly driven by the fact that the federal government penalizes municipalities run by mayors from the opposition coalition who won by a narrow margin, thereby tying their hands for the next (close) electoral race. We also find that politically motivated transfers are larger for second-term mayors—who may have more political connections, weaker reelection incentives, or stronger incentives to run for higher offices—and for small municipalities without a radio station, where the mayor can more easily claim political credit for transfers.

*This chapter is a joint paper with Tommaso Nannicini

1 Introduction

In a federal state, transfers from the central government to lower-tier administrative units are a crucial ingredient both for the efficient provision of public goods and services, and for the political competition between parties (or coalitions) at different levels of government. A large body of research in public finance and political economy has investigated the (normative) implications and the (positive) determinants of intergovernmental transfers, respectively.[1] From a political economy perspective, it is hard to believe that the central government—based on either its own preferences or the presence of institutional and political constraints—ends up acting as a benevolent social planner. Indeed, although federations usually adopt allocation rules that shelter the distribution of intergovernmental grants from political distortions, incumbent politicians at various layers of government can still take advantage of a lot of discretionary policy instruments to tease voters.

In this paper, we apply a Regression Discontinuity Design (RDD) in close electoral races, in the spirit of Lee (2008), in order to identify the impact of partisan alignment on the amount of federal transfers to municipal governments in Brazil. Indeed, if random factors—for example, unexpected breaking news or rain on election day—played even a small role in deciding electoral outcomes, the victory of the political coalition aligned with the Brazilian President would mimic random assignment in those municipal elections decided by a narrow margin. The RDD setup therefore delivers a clean source of exogenous variation in partisan alignment. In the empirical analysis, we construct three different measures of partisan alignment according to the broad coalition, narrow coalition, or political party of the Brazilian President.

The theoretical literature has provided different explanations for politically motivated transfers, or tactical redistribution. On one hand, incumbent parties may use intergovernmental grants to increase their (or their allies') reelection probability at the central and local level, therefore allocating larger transfers to localities where swing voters are overrepresented (see Lindbeck and Weibull, 1987; Dixit and Londregan, 1998). On the other hand, incumbent

[1] See Oates (1972) and Musgrave and Musgrave (1989, chapter 28) for an overview of the efficiency and equity arguments motivating the use of intergovernmental transfers from a normative point of view.

parties may use transfers to reward their core supporters (see Cox and McCubbins, 1986).[2] Furthermore, as far as the local government can claim political credit for the money the central government is allocating, partisan alignment between the two levels of government—that is, the fact that they belong to the same political coalition—should increase the amount of transfers (see Arulampalam et al., 2009).

Testing the above predictions empirically has proven to be a difficult endeavour. In particular, without a credible source of exogenous variation in partisan alignment, the empirical correlation between alignment and larger transfers (if any) could be completely driven by (local) socio-economic conditions affecting both electoral outcomes and resource allocation. Most of the early studies have tackled the issue with a "selection on observables" assumption, controlling for different measures of the normative and political determinants of intergovernmental grants. For the US, Grossman (1994) finds that the similarity of party affiliation between federal and state politicians increases grants made to a state; Levitt and Snyder (1995) show that the share of democratic voters is an important predictor of federal transfers to a district, especially in years of democratic control in Congress. For Australia, Worthington and Dollery (1998) also detect tactical distribution of grants. This approach, however, is likely to suffer from a problem of omitted bias.

Solé-Ollé and Sorribas-Navarro (2008) improve upon the existing literature with a difference-in-differences strategy, both across time (exploiting the within-municipality variation in partisan alignment induced by subsequent elections) and across grantors (exploiting the within-municipality variation in partisan alignment with different layers of government). They use data on Spain and find that municipalities aligned with the two upper-tier governments receive over 40% more grants than the others. Arulampalam et al. (2009) also control for (time-invariant) confounding factors at the state level in India, and find that aligned states receive larger grants, especially if they are swing states. Finally, for Portugal, Veiga and Pinho (2007) use a fixed-effect specification and find evidence of distortions favoring munic-

[2]The "swing voters" hypothesis with respect to intergovernmental grants has received empirical support for Albania (Case, 2001) and Sweden (Johansson, 2003). Looking at different outcomes, the share of unaligned voters has also been shown to be positively correlated with presidential campaigning in the US (Stromberg, 2008) and the quality of political candidates in Italy (Galasso and Nannicini, 2009).

ipalities ruled by the Prime Minister's party during the early years of democracy, but not in the period of established democracy. Yet, unobservable confounders might be time-varying as well. For example, the occurrence of an international economic crisis could swing a large fraction of voters in export-oriented regions toward a given political party, and at the central level this party could decide to favor the same regions because of its policy preferences rather than tactical motivations.

From the above literature, we borrow the idea that higher revenues can be exploited by incumbent politicians to increase their chance of winning the election at the municipal or federal level, depending on which layer of government is able to obtain political credit for the transferred resources.[3] The RDD setup we use in this paper, however, improves internal validity by controlling for both time-invariant and time-varying confounders.

Our econometric strategy not only comes with the advantage of improving internal validity, but also addresses an additional question emerged in the literature: The interaction between the degree of political competition and partisan alignment in determining the tactical distribution of central-level transfers. Arulampalam et al. (2009) build a theoretical model where this interaction plays a crucial role. If the central government were able to obtain full political credit for the transferred resources, it would be indifferent between benefiting aligned or unaligned municipalities. On the contrary, if voters were unable to distinguish the source of the grants and there were some leakage in their goodwill toward the federal government, aligned municipalities should receive more transfers. Furthermore, among aligned municipalities, those where the incumbent won by a narrow margin should receive more. By the same token, among unaligned municipalities, those where the incumbent won by a nar-

[3]Indeed, for Brazil, Brollo et al. (2009) and Litschig and Morrison (2009) apply a regression discontinuity design in population size to estimate the impact of larger transfers on the reelection of the incumbent mayor: Both studies detect a positive effect in different sample periods. Brollo (2008) finds that the Brazilian federal government, after the disclosure of corruption episodes, reduces transfers only to unaligned municipalities. For Uruguay, Manacorda et al. (2009) apply a regression discontinuity design in the assignment criterion of an anti-poverty program and find a positive impact on the recipients' support for the government. For Spain, Solé-Ollé and Sorribas-Navarro (2008) find that grants allocated to local governments of the same party buy more political support than the others.

row margin should receive less, because the federal government wants to tie the hands of its (political) enemies in the next election. For the institutional and political reasons discussed in Section 2.2, Brazil is a setting where the above "goodwill leakage" is likely to be sizable. Focusing on close elections therefore allows us to shed light on the interplay between political competition and partisan alignment.

At the end of the day, the only limitation of the RDD setup is due to the fact that, in order to ensure internal validity, we must restrict the sample to electoral races where there are only two candidates and one of them is affiliated with the President's coalition, which amount to 33% of all electoral races. Although the sample restriction only affects external validity, we address this limitation by comparing two-man races with all the rest. We also present benchmark estimation results with OLS and difference-in-differences specifications both in the all sample and in two-man races. The descriptive statistics show that two-man races are mainly concentrated in municipalities with smaller population, lower per-capita income, and poorer infrastructures. As a result, these municipalities end up receiving more federal transfers over the mayoral mandate. According to OLS, the correlation between partisan alignment and federal transfers is identical in the two samples, while the difference-in-differences estimator, which accommodates for time-invariant confounders, detects a higher correlation in the sample of two-man races, where the marginal impact of public expenditure on voters' utility is also likely to be higher.

According to our RDD estimates, municipalities where the mayor is affiliated with the political coalition of the Brazilian president receive larger federal transfers by 36%–43% (depending on the estimation methods) in the *last* two years of the mayoral mandate, that is, when the next municipal elections are approaching. During the *first* two years of the mandate—when instead federal elections are approaching—we find no evidence of partisan distortions in the allocation of transfers. This is also consistent with the "goodwill leakage" hypothesis discussed above, as it seems that opportunistic grants take place in proximity of municipal rather than federal elections. Interestingly, the effect we find is driven by a sizable cut in transfers to unaligned municipalities close to the threshold of zero margin of victory. In other words, there is evidence that the federal government penalizes municipalities ruled by mayors belonging to the opposition coalition, especially if they won by a narrow margin,

thereby tying their hands in close electoral races.

We also detect significant heterogeneity results. The effect of partisan alignment on transfers is much higher for second-term mayors, in municipalities without a radio station, and in small towns. The larger (politically motivated) transfers received by second-term mayors might have a twofold explanation: On one hand, politicians with longer experience might count on stronger political connections; on the other hand, the central government might want to compensate for the lower effort of aligned mayors without reelection incentives. The absence of a radio station and the small population size—two features that are difficult to disentangle in the Brazilian data—might instead allow the mayor to claim more credit for the increased revenues, therefore raising the political reward from intergovernmental grants. In small municipalities, in fact, local institutions may well be the major source of political information for citizens, while the presence of a radio station may increase the ability of the President or congressmen to publicize their role in allocating federal resources to a given municipality.[4]

The remainder of the paper is organized as follows. In Section 2, we discuss the Brazilian institutional framework, mainly the political system and the allocation of federal transfers. In Section 3, we present our econometric strategy. In Section 4, we discuss the data sources, the sample selection procedure, and the variables of interest. In Section 5, we present the empirical results. We conclude with Section 6.

2 Institutional framework

2.1 The Brazilian federal system

Brazilian politics takes place in the framework of a federal presidential representative democracy and of a multi-party system. Legislative power is exercised by the bicameral National Congress, made up of the Federal Senate (or upper house, with 81 members) and of the

[4]Ferraz and Finan (2008) show that corruption disclosure in Brazilian municipalities—following independent audit reports—has a negative impact on the incumbent mayor's reelection, and that this effect is more pronounced where a local radio is present, making the electorate more informed.

Chamber of Deputies (or lower house, with 513 members). The Brazilian territory is divided in 26 states and 1 federal district (*Brasilia*), ruled by a governor and a legislative assembly. Municipalities represent the lowest layer of administrative division, and are ruled by an elected mayor (*Prefeito*) and an elected city council (*Camera dos Vereadores*). Municipal governments are in charge of a relevant share of the provision of public goods and services related to education, health, and infrastructure projects.

The federal President, governors, and mayors of municipalities above 200,000 voters are directly elected with (runoff) majority rule, while mayors of municipalities below 100,000 are elected with plurality rule. Deputies and senators are elected with open-list proportional representation, and the same holds for state deputies and city councillors. The elections of the President, governors, and members of Congress all take place at the same time every four years, while municipal elections are staggered by two years and also take place every four years.

The Brazilian party system is characterized by the presence of many political parties, and by coalition governments whose composition has constantly changed over time. In particular, the fragmentation of the party system increased in the late 1980s, because of a new legislation easing the requirements for party organization, and because of an exodus from the two largest parties—PMDB (*Partido do Movimento Democrático Brazileiro*) and PFL (*Partido da Frente Liberal*). Nowadays, there are 27 parties officially registered in Brazil. Besides PMDB and PFL, PT (*Partido dos Trabalhadores*) and PSDB (*Partido da Social-Democracia Brasileira*) are the most important of them. The current President is Luiz Inácio Lula da Silva, affiliated with PT. He was elected in October 2002, and reelected in October 2006 (oath of office taking place in January of the following year). PT also rules some states and major cities, but none of the major Brazilian states. The former President was Fernando Henrique Cardoso from PSDB. He was elected in October 1994, and reelected in October 1998. PSDB rules the two biggest Brazilian states, *Sao Paulo* and *Minas Gerais*. However, PMDB is the largest Brazilian party, measured by number of affiliates, number of deputies and senators, and number of mayors. Because of its relevance and median position in the ideological spectrum, PMDB usually supports the government; indeed, it was a member of the government coalition in both Cardoso's mandates and in the second of Lula's mandates. Among the major parties,

PFL is the more right-oriented and has always been concentrated in the Northeast of Brazil. Note that our sample encompasses part of both Cardoso's and Lula's tenure in office.

Brazil has a weakly institutionalized party system, with high electoral volatility, low levels of party identification in the electorate, high fragmentation, and lack of strong ideological platforms. At the federal level, party leaders hold the ability to appoint and substitute members of committees at any time, to add or withdraw proposals in the legislative agenda, to establish the position of the party regarding a bill on the Congress floor, and to negotiate directly with the executive. Following the Constitution of 1988, however, all elected Presidents have been able to build reasonably stable post-electoral government coalitions by means of several discipline devices, such as veto power on a vast arrays of issues, special prerogatives on the allocation of the budget—including federal transfers—and the strong power of the leaderships of the coalition parties (see Figueiredo and Limongi, 2000; Pereira and Mueller, 2002).

2.2 The allocation of federal transfers

The resources of Brazilian municipalities come from (i) local revenues, such as fines, exemptions, service taxes, and residential property taxes; and (ii) transfers from the federal, state, or other municipal governments. The most important source of municipal revenues is represented by federal transfers, which on average amount to 65% of the municipal budget. Basically, there are two types of federal transfers: (i) Constitutional automatic transfers mostly unrestricted (*Fundo de Participacao do Municipios*) or tied to education and health programs; and (ii) discretionary transfers (*CONVÊNIO*), most of them related to infrastructure projects. Excluding some big cities, such as Brazilian state capitals, municipal governments are strongly dependent on these transfers for their budget (as a matter of fact, tax revenues average to only 5.5% of municipal total revenues).

In this study, we focus on discretionary federal transfers devoted to infrastructure projects, which amount to about 15% of total municipal expenditure in infrastructures. These transfers are related to budget items that involve the construction of buildings and bridges, the paving of roads, the building of water systems and sewer linkage, the purchase of ambulances, and

so on. We focus on this type of federal transfers because the bulk of the other revenues are largely non-discretionary and hard to manipulate. Furthermore, such transfers are used to finance highly visible projects, that is, they are an ideal target for politicians willing to tease voters.

In order to analyze the allocation of discretionary federal transfers, it is crucial to understand how the legislative bargaining process works at the federal level. Because of the institutional and political context described above, the President usually faces the hard task of passing his legislative agenda in a Congress where there are about 18 parties, and where the President's party usually controls less than 20 percent of the seats. Political institutions, however, provide him with a wide array of instruments to unify his coalition in Congress, and discretionary transfers to targeted areas are one of them.

Basically, the annual budget law (*Lei Orçamentária Anual*, LOA) is first drafted by the executive and then subject to amendments added by legislators (both individually and collectively). In most cases, the municipalities that will receive the (discretionary) grants are chosen by the legislators, as the bulk of the proposed amendments include benefits to local areas, trying to bring the pork home. The Budget Committee is responsible for the authorization of the bill. After a period of discussion, Congress votes for the budget law, which is then sent to the President for the final decision. As the budget law is not mandatory in Brazil, however, the President has a major role in deciding the allocation of the discretionary transfers, and he can use them to make congressmen follow the guidelines of the government coalition. On the other hand, municipal governments should also exert some effort to apply for these transfers. A budgetary amendment can be executed only if an agreement (i.e., *CONVÊNIO*) between the municipal administration and the central government is signed.

At the end of the day, voters in a given municipality will receive discretionary transfers depending on three factors: (i) The effort of their municipal administration in applying for these transfers; (ii) the interests of a federal congressman in supporting the municipality; and (iii) the interest of the President in executing the budget amendment (that is, send the money exactly to that municipality). The political reward in terms of votes can accrue to any of these subjects. Yet, the mayor of the municipality will probably obtain the lion's share of the political credit for the higher municipal revenues, especially when voters are

not sufficiently informed about the source of the grant, or when there is low competition in claiming credit (e.g., in small municipalities with just one congressman).

3 Econometric strategy

We are interested in estimating the causal effect of the treatment "being politically aligned with the President" on the amount of (discretionary) federal transfers. Using Rubin's (1974) potential-outcome framework, define $T_{im}(1)$ as the potential transfers received by municipality i, during the administrative mandate m, in case the mayor is politically aligned with the President, and $T_{im}(0)$ as the potential transfers of the same municipality in case the mayor is not politically aligned with the President. The variable A_{im} defines the treatment status of i: $A_{im} = 1$ if there is partisan alignment, and $A_{im} = 0$ otherwise. The observed outcome is thus: $T_{im} = A_{im} \cdot T_{im}(1) + (1 - A_{im}) \cdot T_{im}(0)$. The estimand of interest is the average treatment effect: $E[T_{im}(1) - T_{im}(0)]$.

Define W_i as a set of relevant town-specific covariates (including state fixed effects), X_{im} as a set of (mandate-varying) mayoral characteristics, and δ_m as mandate fixed effects. In the OLS estimation

$$T_{im} = \alpha + \tau A_{im} + \beta W_i + \phi X_{im} + \delta_m + \varepsilon_{im}, \tag{1}$$

the estimated $\hat{\tau}$ is based on the conditional comparison of the observed transfers of aligned and unaligned municipalities, which does not generally provide an unbiased estimate of the average treatment effect, as long as towns with different unobservable characteristics affecting federal transfers self-select into partisan alignment by voting for different parties.

A difference-in-differences estimator can instead control for time-invariant confounding factors by means of municipality fixed effects γ_i:

$$T_{im} = \alpha + \tau A_{im} + \phi X_{im} + \gamma_i + \delta_m + \varepsilon_{im}. \tag{2}$$

Also in this estimation, however, $\hat{\tau}$ might fall short of providing an unbiased estimate of the average treatment effect. In particular, unobservable confounders might be time-varying as well. This would happen if, for example, following an international economic crisis, a large

fraction of the electorate in export-oriented regions decided to vote for a given political party and, at the federal level, this party decided to favor the same regions because of its policy preferences rather than tactical motivations.

In order to deal with the presence of both time-invariant and time-varying confounders, we therefore implement an RDD strategy in the spirit of Lee (2008) and compare municipalities where the politically aligned candidate barely won with municipalities where the politically aligned candidate barely lost.[5] Specifically, we calculate the margin of victory of the candidate for mayor politically aligned with the Brazilian President in each municipality i and mandate m (MVP_{im}).[6] This measure is thus positive in municipalities where the mayor belongs to the federal government coalition, and negative otherwise. At the threshold of zero margin of victory, $MVP_{im} = 0$, partisan alignment sharply changes from zero to one. This treatment assignment mechanism is an example of *sharp* RDD.

MVP_{im} can be seen as a random variable depending on observable and unobservable town characteristics, as well as on random shocks on election day. Lee (2008) shows that, in this setup, RDD identification requires that: (i) For each political candidate, the probability of winning is never equal to zero or one; (ii) for each political candidate, the probabilities of winning or losing the election by a narrow margin are identical.[7] In other words, electoral outcomes depend on both predictable elements and random chance, which is then crucial only for close races. For instance, heavy rain on election day may influence turnout and, as a result, the victory of one candidate over the other in marginal municipalities. The average treatment effect in close races can thus be identified as:

$$E[T_{im}(1) - T_{im}(0)|MVP_{im} = 0] = \lim_{\epsilon \downarrow 0} E[T_{im}|MVP_{im} = \epsilon] - \lim_{\epsilon \uparrow 0} E[T_{im}|MVP_{im} = \epsilon]. \quad (3)$$

Note that this is a local effect, which cannot be extrapolated to the whole population

[5] See also Lee, Moretti, and Butler (2004). Ferreira and Gyourko (2009) and Pettersson-Lidbom (2008) use the same identification strategy to estimate the impact of political parties with different ideologies on policy outcomes in the US and Sweden, respectively.

[6] We use three different measures of partisan alignment according to the broad coalition, narrow coalition, or political party of the federal President. See Section 4 for more details.

[7] These conditions are equivalent to the standard RDD assumption that potential outcomes must be a continuous function of the running variable at the threshold (Hahn, Todd, and Van der Klaauw, 2001).

without additional homogeneity assumptions. But this local effect, defined for close electoral races only, has first-order theoretical relevance in our case, because it sheds light on the tactical allocation of federal transfers in marginal municipalities. The only price we have to pay to obtain internal validity is a loss in external validity: To achieve an assignment mechanism to partisan alignment which is as good as random in close elections, we must restrict the sample to electoral races with only two political candidates, one of whom is politically affiliated with the Brazilian President.

Various methods can be used to estimate the above local average treatment effect. We first apply a split polynomial approximation, that is, we fit a p-order polynomial in MVP_{im} on either side of the threshold $MVP_{im} = 0$:

$$T_{im} = \sum_{k=0}^{p}(\rho_k MVP_{im}^k) + A_{im}\sum_{k=0}^{p}(\tau_k MVP_{im}^k) + \delta_m + v_s + \eta_{im}, \tag{4}$$

where δ_m are mandate fixed effects, and v_s state fixed effects. The coefficient $\hat{\tau}_0$ identifies the local treatment effect, that is, whether aligned municipalities in close races receive larger transfers from the central government. Standard OLS inference procedures can be applied; we also cluster standard errors at the town level, because the same municipality may be observed in repeated mayoral terms.

The above approach is attractive for many reasons, although a possible concern is that it may be sensitive to outcome values for observations far away from the threshold (see Imbens and Lemieux, 2008). To avoid this, the second method we use restricts the estimation to a compact support, and fits linear regression functions to the observations within a distance h on either side of the threshold. In other words, we restrict the sample to municipalities in the interval $MVP_{im} \in [-h, +h]$ and estimate the model:

$$T_{im} = \rho_0 + \rho_1 MVP_{im} + \tau_0 A_{im} + \tau_1 A_{im} \cdot MVP_{im} + \delta_m + v_s + \eta_{im}, \tag{5}$$

where δ_m are mandate fixed effects, v_s state fixed effects, standard errors are clustered at the town level, and the bandwidth h is selected applying the cross-validation method.[8] Again, $\hat{\tau}_0$ identifies the local treatment effect.

[8]In particular, we apply the following cross-validation method (see Imbens and Lemieux, 2008), which

In the empirical analysis, we also check for treatment effect heterogeneity by estimating the local average effect in separate subsamples. Assume D_{im} captures a given heterogeneity dimension (for instance, the presence of a radio station). We estimate:

$$\begin{aligned} T_{im} = \sum_{k=0}^{p}(\rho_k MVP_{im}^k) + A_{im}\sum_{k=0}^{p}(\tau_k MVP_{im}^k) + \\ + D_{im}\cdot\Big[\sum_{k=0}^{p}(\alpha_k MVP_{im}^k) + A_{im}\sum_{k=0}^{p}(\beta_k MVP_{im}^k)\Big] + \delta_m + v_s + \xi_{im}. \end{aligned} \tag{7}$$

As a result, $\hat{\tau_0}$ identifies the treatment effect in $D_{im} = 0$, $\hat{\tau_0} + \hat{\beta_0}$ in $D_{im} = 1$, and $\hat{\beta_0}$ the difference between the two.

4 Data sources and sample selection

Data on federal transfers are obtained from the Brazilian National Treasury (*Tesouro Nacional*) website, which provides information from municipal and state annual balance sheets about assets, liabilities, revenues, and expenditure for all Brazilian municipalities and states. Electoral data and information about mayoral characteristics and party affiliation are obtained from the National Electoral Office (*Tribunal Superior Eleitoral*). Control variables on town characteristics—such as population size, per-capita income, and so on—are retrieved from the 2000 Brazilian Census.

Basically, every two years there are elections in Brazil. However, federal elections and municipal elections take place at a different time. The exact timing of both federal and municipal elections over our sample period is illustrated in Figure 1. In fact, our study

consists in choosing h so as to minimize the loss function:

$$CV_{_T}(h) = \frac{1}{N}\sum_{i=1}^{N}(T_i - \hat{\mu}_{_h}(MVP_i))^2, \tag{6}$$

where the predictions $\hat{\mu}_{_h}(MVP_i)$ are retrieved as follows. For every MVP_i to the left (right) of the threshold, we predict the value of transfers as if the point were at the boundary of the estimation, using only observations in the interval $[MVP_i - h, MVP_i]$ ($[MVP_i, MVP_i + h]$). We then calculate the loss function discarding 50% of the observations on either side of the threshold $MVP_i = 0$.

encompasses federal governments in office during the three four-year mandates 1999–2002, 2003–2006, and 2007–2010; as well as municipal administrations in office during the three four-year mandates 1997–2000, 2001–2004, and 2005–2008.

Our baseline results consider federal transfers only in the last two years of each mayoral mandate, that is, the per-capita amount of infrastructure transfers for the periods 1999–2000, 2003–2004, and 2007–2008. The choice of this timing is motivated by the fact that we want to capture opportunistic transfers in proximity of upcoming municipal elections. For the years 1999–2000, the President was Fernando Henrique Cardoso from PSDB, while in the years since 2003 the President was Luis Inácio Lula da Silva from PT.

As for the treatment, we define three different variables indicating whether the municipality is politically aligned with the federal President or not: Two measures of the federal government coalition (broad versus narrow), and the President's party. Table 1 describes our three measures. The broad coalition measure considers all political parties that belonged to the government coalition at some point of each presidential mandate. On the contrary, narrow coalition, for instance, does not consider PMDB as part of the government coalition during the first mandate of Lula (2003–2006), because this party entered Lula's coalition only at the end of the mandate. Furthermore, the definition of narrow coalition excludes small parties that are not strictly necessary to ensure a parliamentary majority to the government in office.

As a second step, we also check whether opportunistic transfers take place in proximity of federal rather than municipal elections. We thus build an alternative measure of both transfers and President's coalition, referring to the first two years (last two years) of each municipal (federal) mandate. Table 2 reports the modified political coalition variables.

In order to implement our identification strategy in the Brazilian multi-party system, as discussed in the previous section, we must restrict our sample to municipalities where only two candidates run for mayor and one of them is politically aligned with the federal President. To evaluate the external validity of our results, it is thus important to understand how this sample differs from the rest of Brazilian municipalities.

Table 3 reports the summary statistics of the relevant variables for two different samples

(according to our three measures of partisan alignment): two-man races versus other races.[9] These variables are both the town-specific Census characteristics (i.e., the covariates W_i that we include in the OLS and difference-in-differences specifications) and the mayor-specific characteristics (i.e., the covariates X_{im}). The former group includes: population size; per-capita income; the over-20 literacy rate; the rate of urban population; the fraction of houses with access to water, sewer, or electricity; the presence of a radio station; and geographical location (North, Northeast, Center, South, Southeast). The latter group includes years of schooling, gender, and marital status of the mayor.

On average, municipalities in the two-man races receive more infrastructure transfers both in the first two years and in the last two years of each municipal electoral cycle. Furthermore, they are smaller, poorer, less literate, with poorer infrastructures (water supply, sewer access, electricity), and have a higher fraction of inhabitants living in rural areas. It is therefore plausible that federal transfers both make a greater difference and are easier to justify when they are targeted at these municipalities.

On the other hand, if we focus on the sample of two-man races, we can preliminary check if there are statistically significant differences between municipalities with a mayor who is aligned with the President and municipalities where the mayor is not aligned. According to the summary statistics reported in Table 4, municipalities aligned with the President's broad coalition, on average, receive larger per-capita transfers when municipal elections are approaching. There is no descriptive evidence of opportunistic transfers, however, according to the other measures of partisan alignment, or when federal elections are approaching. Looking at the town-specific or mayor-specific characteristics, there is no evidence of "selection on observables" in our sample. Indeed, there are no statistically significant differences between aligned and unaligned municipalities, except for three Brazilian regions in the narrow measure of partisan alignment. This means that municipalities that are politically aligned with the President do not seem to differ from the others in terms of level of development or political selection. This is not particularly relevant for our identification strategy—which accomodates for both selection on observables and unobservables—but it is an additional

[9] In all specifications, we consider only municipalities with less than 200,000 inhabitants, where elections are held under plurality rule.

piece of information on Brazilian politics.

5 Empirical results

5.1 Baseline estimates

Our main estimation results are presented in Table 5. The first, second, and third column report the results for broad coalition, narrow coalition, and party of the president, respectively. Panel A and panel B report the benchmark OLS and difference-in-differences results for both all municipalities and two-man electoral races, respectively. Panel C instead shows the RDD results according to three different estimation methods (split polynomial approximation with full and half bandwidth, and local linear regression with optimal bandwidth).

According to the benchmark cross-sectional and panel evidence, politically aligned municipalities receive more infrastructure transfers. The results of the OLS regressions in the all sample and in two-man races are similar for all three measures of partisan alignment. For all municipalities (panel A), transfers increase by 13.72%, 14.21%, and 11.56%, when the mayor belongs to the broad coalition, narrow coalition, or party of the President, respectively. For tow-man races, the OLS estimates are almost identical. Note, instead, that in the difference-in-differences specifications the estimates are almost the same as OLS when all municipalities are considered. In this case, the per-capita amount of transfers increases by 16.75%, 17.40%, and 11.16% according to our three measures of partisan alignment. On the contrary, the impact of political alignment on transfers is considerably higher when the sample is restricted to two-man races: Transfers increase by 40.20%, 44.77%, and 60.23% if the mayor is affiliated with the broad coalition, narrow coalition, or party of the President, respectively. The above discrepancy might be due to the fact that the difference-in-differences strategy is removing a downward bias in the OLS coefficient, which is present in the two-man but not in the all sample. Yet, also the diff-in-diff results in two-man races may suffer from omitted bias, as long as unobservable confounders vary across mayoral terms.

Panel C of Table 5 reports the main RDD results in (close) two-man races. According to the baseline estimation with split polynomial approximation and full bandwidth as in

equation (4), being affiliated with the broad coalition, narrow coalition, or party of the federal President increases the amount of per-capita infrastructure transfers by 35.93%, 39,57%, and 43,27%, respectively. As a robustness check, we also divide by half the bandwidth and estimate again equation (4), finding identical results. We then implement a local linear regression with optimal bandwidth as in equation (5): According to this estimation, being affiliated with the broad coalition, narrow coalition, or party of the federal President increases the amount of per-capita infrastructure transfers by 22,19%, 33,74%, and 40,28%, respectively. Note that the larger RDD effect of partisan alignment on transfers with respect to the benchmark OLS results can have a twofold explanation: On one hand, the RDD setup controls for unobservables removing the omitted bias of OLS; on the other hand, the RDD setup focuses on close electoral races only, where the effect of partisan alignment might be higher because of tactical motivations.

Figures 8 through 10 provide a way to tackle this last (interesting) issue. There, we show the estimated split polynomials in MVP to highlight not only the jump in transfers at $MVP = 0$, but also the shape of the relationship between transfers and MVP for aligned municipalities (to the right of the zero threshold) and unaligned municipalities (to the left of the zero threshold). Figure 8 clearly shows that the RDD estimate is driven by a sizable cut in transfers to unaligned municipalities close to the zero threshold. In other words, the federal government penalizes municipalities ruled by mayors belonging to the opposition coalition, especially if they won by a narrow margin, thereby tying the hands of its (political) enemies for the next (close) electoral races. Furthermore, there is some evidence that the federal government also gives more money to its strongholds (where MVP is positive and very high) and less to its enemies' strongholds (where MVP is negative and very low), although we do not know if this is driven by the local characteristics of these towns or by political motivations. In addition, note that the tails of the polynomial are not accurately estimated because of the small sample size.

Table 6 reports the same benchmark and RDD estimations for a different outcome variable, that is, per-capita infrastructure transfers from the federal government in the first two years of the municipal mandate (when the federal elections are approaching). The estimation results show that there is no evidence of opportunistic transfers in proximity of federal

elections. The simple comparison of these results with those in Table 5 therefore highlights a clear political budget cycle in federal transfers to municipal governments with respect to the timing of municipal elections. This also reinforces the idea that mayors can claim the lion's share of the political credit from larger transfers, because tactical redistribution takes place only in proximity of the elections for mayor and not of the elections for the Brazilian President.

5.2 Robustness checks

In order for our RDD econometric strategy to be internally valid, as discussed in Section 3, political parties must not be able to sort above the threshold of zero margin of victory. In other words, political parties, even when they control the federal government, should not be able to manipulate electoral outcomes in (very) close elections. To check for the absence of manipulative sorting, we perform: (i) Visual inspection of the histograms of MVP in Figures 2, 4, and 6; and (ii) formal tests of the continuity of the density at $MVP = 0$, in the spirit of McCrary (2008), in Figures 3, 5, and 7. None of these procedures shows any evidence of manipulative sorting around the zero threshold.

In Table 7, we further check for discontinuities of town and mayoral characteristics at $MVP = 0$ by performing a set of balance tests, which are performed estimating equation (4) with town-specific and mayoral characteristics as dependent variables. These robustness exercises have a twofold interpretation. On the one hand, for variables that are predetermined, we should observe no discontinuity, as long as there is no manipulative sorting around the zero threshold. This is the case of the 2000 Census variables and geographic location, which are indeed balanced around the thresholds, excluding a few exceptions (one for narrow and two for broad coalition). On the other hand, for variables that are endogenous to the treatment, a non-zero discontinuity would not cast a shadow on the internal validity of the results, but would suggest a possible channel of the treatment effect. This is the case of mayoral characteristics, as long as political parties are intrinsically different in the observable features of their political candidates in close races.[10] Also mayoral characteristics do

[10] On the competition between political parties based on the features of political candidates in close electoral

not display significant discontinuities at the threshold.

5.3 Heterogeneity results

Finally, in Table 8, we estimate equation (7) to detect heterogeneity (if any) in the treatment effect. We consider three dimensions: whether the mayor has been elected for his first or second mandate (note that there is a two-term limit in Brazil); whether the municipality has a radio station or not; whether population size is above or below the median population of Brazilian municipalities.[11] According to the estimation results, the effect of partisan alignment on transfers is much higher for second-term mayors, in municipalities without a radio station, and in small towns. The interaction effect is indeed always statistically significant for the broad and narrow definitions of President's coalition. For second-term mayors, being politically aligned with the President almost doubles the amount of received transfers according to both coalition definitions. In municipalities without radio station, transfers increase by about 64%, 71%, and 73% if the mayor is affiliated with the broad coalition, narrow coalition, or party of the President, respectively.

The larger (politically motivated) transfers received by second-term mayors might have a twofold explanation: On one hand, politicians with longer experience might count on stronger political connections; on the other hand, the central government might want to compensate for the lower effort of aligned mayors without reelection incentives. Furthermore, (well connected) second-term mayors might put extra effort in attracting resources to run for higher offices and build a political career. The absence of a radio station and the small population size—two features that are highly confounded in our data—might instead allow the mayor to claim more credit for the increased revenues, therefore raising the political reward from intergovernmental grants. In small municipalities, in fact, local institutions may well be the major source of political information for citizens, while the presence of a radio station may increase the ability of the President or congressmen to publicize their role in

races, see Galasso and Nannicini (2009).

[11] The results on population size are robust to the estimation of a continuous version of equation (7), with the exact number of resident inhabitants in the place of D_{im}.

allocating federal resources to a given municipality.

6 Conclusions and future research

In this paper, we document the existence of (sizable) tactical motivations in the allocation of federal transfers by the Brazilian federal government, aimed at penalizing unaligned municipalities where mayors belonging to the opposition coalition won by a narrow margin. Our RDD estimates—which accommodate for the presence of both time-invariant and time-varying confounding factors—show that mayors politically aligned with the Brazilian President receive larger federal transfers in close electoral races, by an amount that varies from 36% to 43% according to the used estimation method or measure of partisan alignment. The results are statistically significant only for federal transfers received in the last two years of the (four-year) mayoral term, therefore pointing to the existence of a relevant political budget cycle in Brazilian municipal revenues.

As future research, we are going to extend the above results in multiple directions. In particular, we plan:

- to further evaluate the robustness of the results presented in this paper by means of falsification tests and functional-form sensitivity checks;
- to repeat our estimations with data on additional types of (discretionary) federal transfers to Brazilian municipalities;
- to estimate the differential incumbency advantage of aligned versus unaligned mayors, applying again the RDD methodology proposed by Lee (2008).

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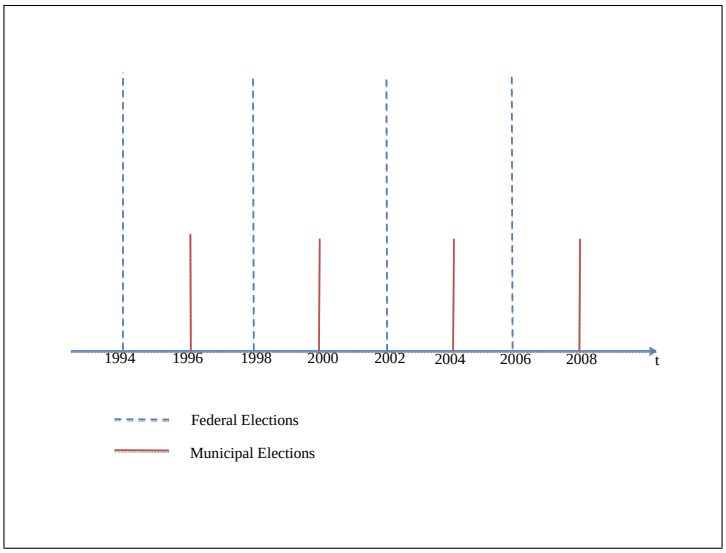
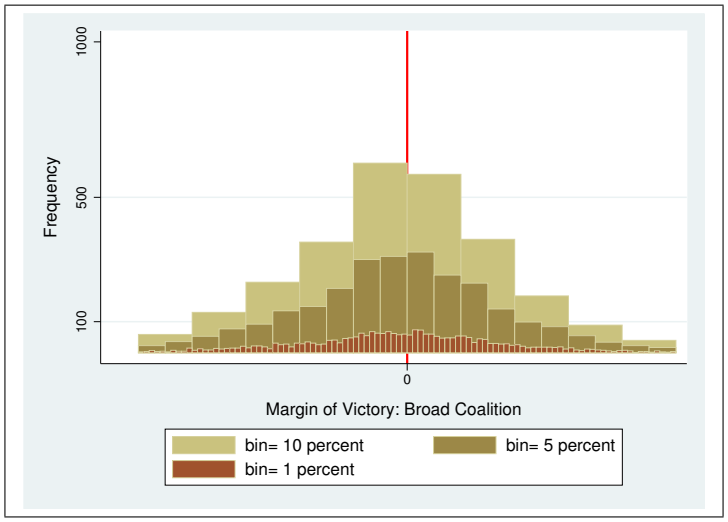
Figures and Tables

Figure 1 – Timing of elections



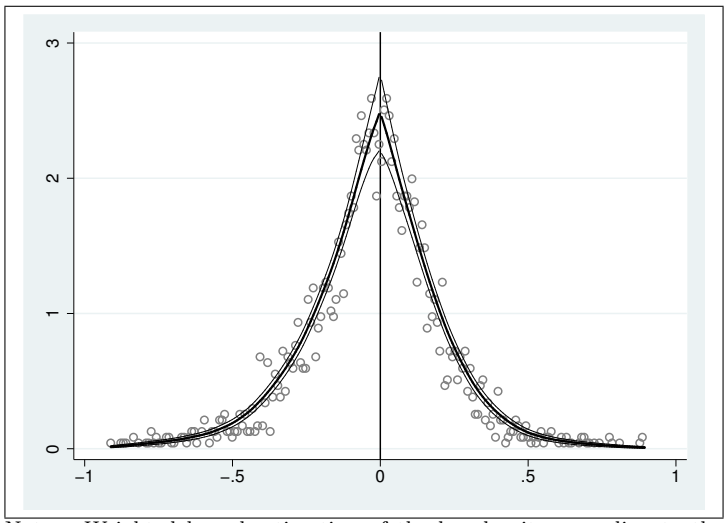
Notes. Timing of federal and municipal election in the sample period.

Figure 2 – Histogram of the margin of victory: Broad coalition



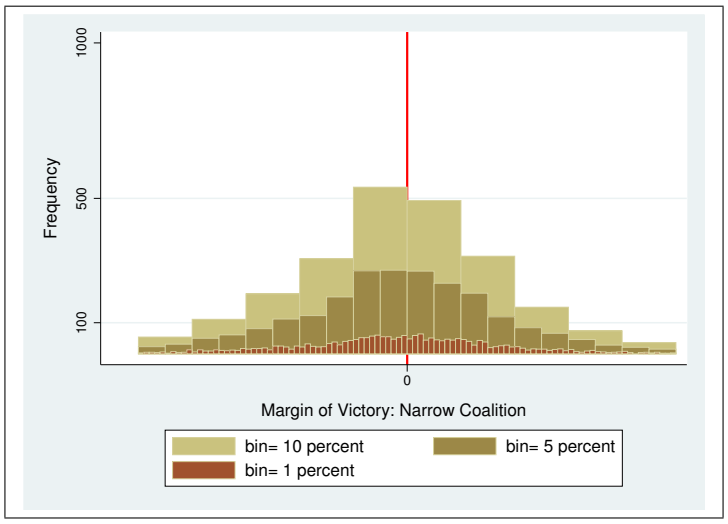
Notes. Frequency of municipalities according to the margin of victory of the president's broad coalition. Two-man races only.

Figure 3 – McCrary test on close races: Broad coalition



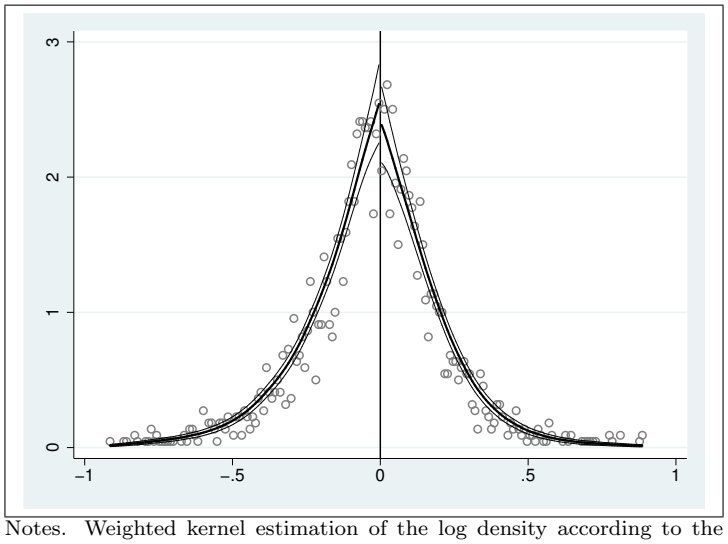
Notes. Weighted kernel estimation of the log density according to the margin of victory of the president's broad coalition, performed separately on either side of the zero threshold. Optimal binwidth and binsize as in McCrary (2008). Two-man races only.

Figure 4 – Histogram of the margin of victory: Narrow coalition



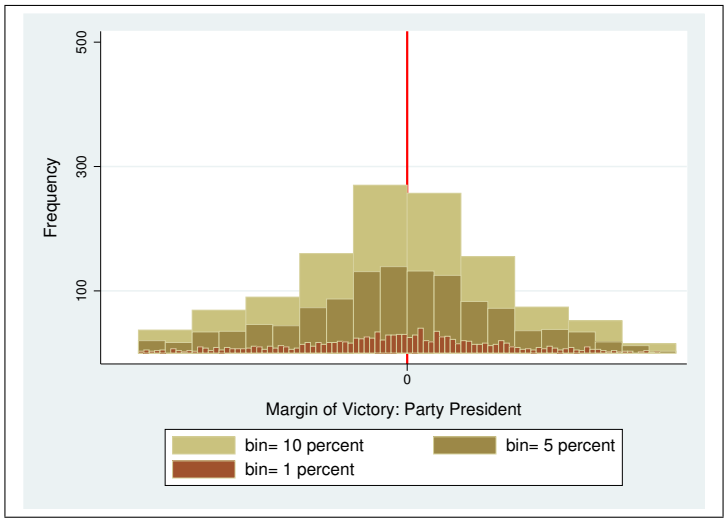
Notes. Frequency of municipalities according to the margin of victory of the president's narrow coalition. Two-man races only.

Figure 5 – McCrary test on close races: Narrow coalition



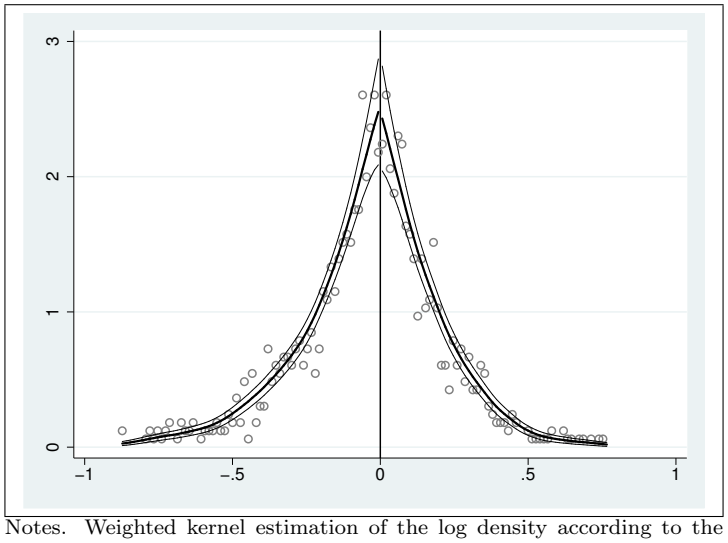
Notes. Weighted kernel estimation of the log density according to the margin of victory of the president's narrow coalition, performed separately on either side of the zero threshold. Optimal binwidth and binsize as in McCrary (2008). Two-man races only.

Figure 6 – Histogram of the margin of victory: President's party



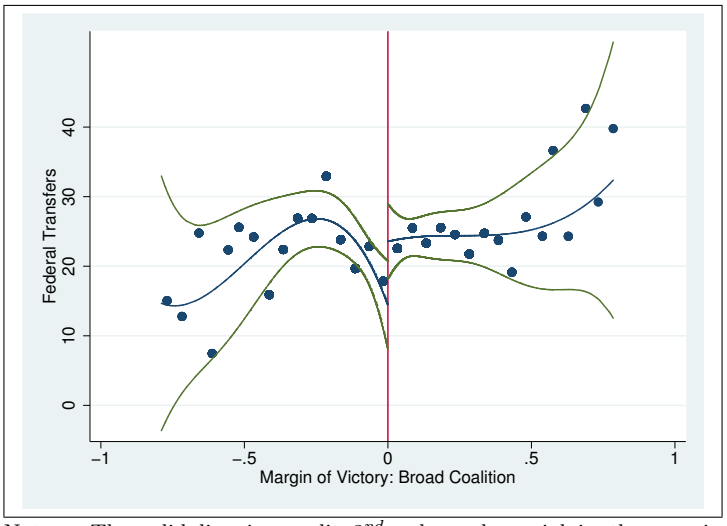
Notes. Frequency of municipalities according to the margin of victory of the president's political party. Two-man races only.

Figure 7 – McCrary test on close races: President's party



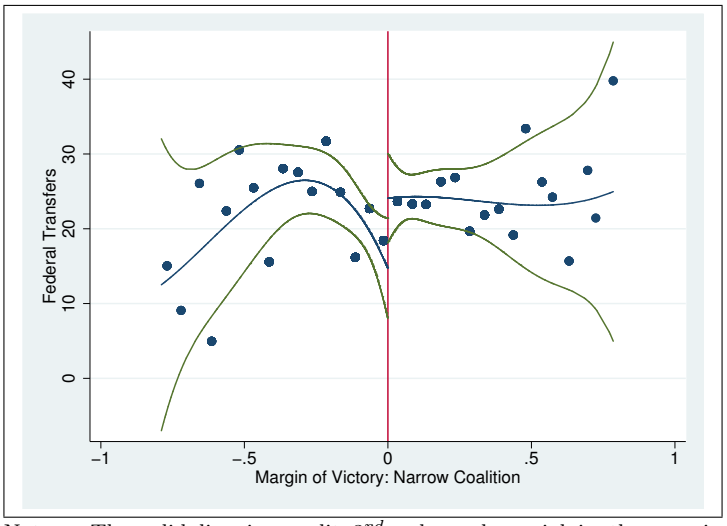
Notes. Weighted kernel estimation of the log density according to the margin of victory of the president's political party, performed separately on either side of the zero threshold. Optimal binwidth and binsize as in McCrary (2008). Two-man races only.

Figure 8 – Partisan alignment and federal transfers: Broad coalition



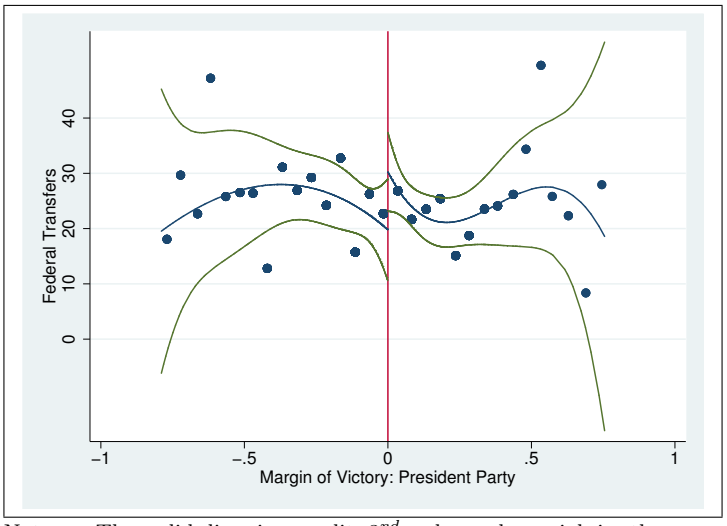
Notes. The solid line is a split 3^{rd}-order polynomial in the margin of victory of the president's broad coalition, fitted over the interval $[-0.80, +0.80]$; the dashed lines are the 95% confidence interval. Scatter points are averaged over 5-unit intervals. Two-man races only.

Figure 9 – Partisan alignment and federal transfers: Narrow coalition



Notes. The solid line is a split 3^{rd}-order polynomial in the margin of victory of the president's narrow coalition, fitted over the interval $[-0.80, +0.80]$; the dashed lines are the 95% confidence interval. Scatter points are averaged over 5-unit intervals. Two-man races only.

Figure 10 – Partisan alignment and federal transfers: President's party



Notes. The solid line is a split 3^{rd}-order polynomial in the margin of victory of the president's political party, fitted over the interval $[-0.80, +0.80]$; the dashed lines are the 95% confidence interval. Scatter points are averaged over 5-unit intervals. Two-man races only.

Table 1 – Definitions of President's coalitions for the last two years of municipal mandates

Political party	Chamber of Deputies seats			Broad coalition			Narrow coalition		
	1998 election	2002 election	2006 election	1999 & 2000	2003 & 2004	2007 & 2008	1998 & 1999	2003 & 2004	2007 & 2008
PMDB	83	76	89	YES	YES	YES	YES	NO	YES
PT	59	91	83	NO	YES	YES	NO	YES	YES
PSDB	99	70	65	YES	NO	NO	YES	NO	NO
PFL	105	84	65	YES	NO	NO	YES	NO	NO
PP	60	48	42	YES	NO	NO	YES	NO	NO
PSB	18	22	27	NO	YES	YES	NO	YES	YES
PDT	25	21	24	NO	YES	YES	NO	YES	YES
PL	12	26	23	NO	YES	YES	NO	YES	YES
PTB	31	26	22	YES	YES	YES	YES	NO	NO
PPS	3	15	21	YES	NO	NO	YES	NO	NO
PV	1	5	13	NO	YES	YES	NO	NO	NO
PC do B	7	12	13	NO	YES	YES	NO	NO	NO
PSC	2	0	9	NO	NO	NO	NO	NO	NO
PTC	0	0	4	NO	NO	NO	NO	NO	NO
PSL	0	0	3	NO	NO	NO	NO	NO	NO
PMN	2	1	3	NO	NO	NO	NO	NO	NO
PRONA	1	6	2	NO	NO	NO	NO	NO	NO
PHS	0	0	2	NO	NO	NO	NO	NO	NO
PT do B	0	0	1	NO	NO	NO	NO	NO	NO
PAN	0	0	1	NO	NO	NO	NO	NO	NO
PRB	0	0	1	NO	NO	NO	NO	NO	NO
PSDC	0	1	0	NO	NO	NO	NO	NO	NO
PSL	1	1	0	NO	NO	NO	NO	NO	NO
PSD	3	4	0	NO	NO	NO	NO	NO	NO
PST	1	3	0	NO	NO	NO	NO	NO	NO
Others	0	0	0	NO	NO	NO	NO	NO	NO

Notes. See Figure 1 for the exact timing of federal and municipal elections and mandates. Fernando Henrique Cardoso (PSDB) was elected as President in both 1994 and 1999. Luis Inácio Lula da Silva (PT) was elected as President in both 2002 and 2006.

Table 2 – Definitions of President's coalition for the first two years of municipal mandates

Political party	Chamber of Deputies seats		Broad coalition			Narrow coalition		
	1998 election	2002 election	1997 & 1998	2001 & 2002	2005 & 2006	1997 & 1998	2001 & 2002	2005 & 2006
PMDB	83	76	YES	YES	YES	YES	YES	YES
PT	59	91	NO	NO	YES	NO	NO	YES
PSDB	99	70	YES	YES	NO	YES	YES	NO
PFL	105	84	YES	YES	NO	YES	NO	NO
PP	60	48	YES	YES	NO	YES	YES	NO
PSB	18	22	NO	NO	YES	NO	NO	YES
PDT	25	21	NO	NO	YES	NO	NO	YES
PL	12	26	NO	NO	YES	NO	NO	YES
PTB	31	26	YES	NO	YES	NO	NO	YES
PC do B	7	12	NO	NO	YES	NO	NO	NO
PPS	3	15	YES	YES	NO	YES	YES	NO
PV	1	5	NO	NO	YES	NO	NO	NO
PSC	2	0	NO	NO	NO	NO	NO	NO
PTC	0	0	NO	NO	NO	NO	NO	NO
PSL	0	0	NO	NO	NO	NO	NO	NO
PMN	2	1	NO	NO	NO	NO	NO	NO
PRONA	1	6	NO	NO	NO	NO	NO	NO
PHS	0	0	NO	NO	NO	NO	NO	NO
PT do B	0	0	NO	NO	NO	NO	NO	NO
PAN	0	0	NO	NO	NO	NO	NO	NO
PRB	0	0	NO	NO	NO	NO	NO	NO
PSDC	0	1	NO	NO	NO	NO	NO	NO
PSL	1	1	NO	NO	NO	NO	NO	NO
PSD	3	4	NO	NO	NO	NO	NO	NO
PST	0	3	NO	NO	NO	NO	NO	NO
Others	0	0	NO	NO	NO	NO	NO	NO

Notes. See Figure 1 for the exact timing of federal and municipal elections and mandates. Fernando Henrique Cardoso (PSDB) was elected as President in both 1994 and 1999. Luis Inácio Lula da Silva (PT) was elected as President in both 2002 and 2006.

Table 3 – Descriptive statistics: Two-man vs. other races

	Broad coalition			Narrow coalition			President party		
	Two-man	Other	*p-value*	Two-man	Other	*p-value*	Two-man	Other	*p-value*
First two-year transfers	20.71	16.62	0.000	20.74	16.73	0.000	26.67	15.30	0.000
Last two-year transfers	23.20	16.24	0.000	23.85	17.83	0.000	25.29	18.55	0.000
Population	11,958	25,940	0.000	12,017	25,010	0.000	11,688	22,962	0.000
Per-capita income	154.23	178.46	0.000	154.48	176.79	0.000	161.59	171.92	0.000
Literacy rate	0.55	0.57	0.000	0.55	0.57	0.000	0.56	0.56	0.893
Urban population	0.53	0.64	0.000	0.52	0.63	0.000	0.55	0.61	0.000
Water access	0.55	0.62	0.000	0.55	0.62	0.000	0.57	0.60	0.000
Sewer	0.19	0.29	0.000	0.18	0.28	0.000	0.24	0.26	0.144
Electricity	0.87	0.89	0.000	0.87	0.88	0.000	0.88	0.88	0.778
Radio	0.14	0.27	0.000	0.14	0.26	0.000	0.05	0.07	0.001
Years of schooling	11.32	11.73	0.000	11.30	11.70	0.000	11.59	11.59	0.966
Male	0.94	0.93	0.764	0.94	0.93	0.090	0.94	0.93	0.003
Married	0.84	0.81	0.009	0.84	0.82	0.050	0.84	0.82	0.203
North	0.05	0.07	0.001	0.05	0.06	0.005	0.05	0.06	0.260
Northeast	0.31	0.27	0.000	0.30	0.27	0.026	0.21	0.29	0.000
Center	0.07	0.07	0.396	0.07	0.07	0.965	0.07	0.07	0.746
South	0.29	0.19	0.000	0.31	0.18	0.000	0.24	0.22	0.093
Southeast	0.26	0.39	0.000	0.25	0.39	0.000	0.40	0.34	0.000
Observations	2,750	5,493		2,369	5,874		1,239	7,004	

Notes. *Two-man races* are those where only two candidates run for mayor and one of them is affiliated with the broad coalition, narrow coalition, or party of the president, respectively. *Other races* are the other elections. All columns except those with *p-value* report the average values in the respective subsamples; *p-value* refers to the statistical significance of the difference between means. *First two-year transfers* are the average infrastructure transfers from the federal government to municipalities in the first two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). *Last two-year transfers* are the average infrastructure transfers from the federal government to municipalities in the last two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). *Population* is the number of resident inhabitants in 2000. *Per-capita income* refers to monthly income in 2000 and is measured in Brazilian *reais*. The following variables refer to the 2000 Census and are expressed in percentage terms: *Urban population* is the fraction of people living in urban areas; *Literacy rate* is the fraction of people above 20 who are literate; *Water access*, *Sewer*, and *Electricity* are the fraction of houses with access to water supply, sewer, and electricity, respectively; *Radio* captures whether there is at least one local radio station in the municipality. *Years of schooling*, *male*, and *married* refer to the characteristics of the mayor. *North*, *Northeast*, *Center*, *South*, and *Southeast* are macro-regions.

Table 4 – Descriptive statistics: Aligned vs. non aligned municipalities

	Broad coalition			Narrow coalition			President party		
	Aligned	Non aligned	*p-value*	Aligned	Non aligned	*p-value*	Aligned	Non aligned	*p-value*
First two-year transfers	22.24	18.38	0.005	22.54	18.82	0.139	25.80	27.69	0.562
Last two-year transfers	24.28	22.23	0.299	24.09	22.77	0.258	24.23	24.34	0.821
Population	11,647	12,234	0.319	11,643	12,344	0.279	11,825	11,570	0.771
Per-capita income	152.75	155.54	0.470	152.39	156.30	0.300	158.17	164.52	0.176
Literacy rate	0.55	0.55	0.778	0.55	0.55	0.856	0.56	0.57	0.437
Urban population	0.53	0.53	0.771	0.52	0.53	0.647	0.56	0.55	0.333
Water access	0.55	0.56	0.270	0.54	0.55	0.210	0.57	0.57	0.987
Sewer	0.19	0.18	0.181	0.19	0.17	0.288	0.27	0.22	0.004
Electricity	0.87	0.86	0.332	0.87	0.86	0.484	0.88	0.88	0.924
Radio	0.13	0.14	0.602	0.13	0.15	0.298	0.12	0.15	0.155
Years of schooling	11.40	11.26	0.391	11.49	11.26	0.082	11.88	11.43	0.146
Male	0.94	0.93	0.313	0.95	0.94	0.353	0.96	0.96	0.967
Married	0.83	0.84	0.406	0.83	0.83	0.897	0.80	0.86	0.057
North	0.04	0.05	0.211	0.04	0.05	0.243	0.06	0.05	0.424
Northeast	0.31	0.31	0.978	0.29	0.30	0.720	0.19	0.23	0.074
Center	0.07	0.06	0.681	0.07	0.06	0.319	0.06	0.07	0.653
South	0.29	0.30	0.403	0.31	0.32	0.548	0.20	0.28	0.000
Southeast	0.27	0.25	0.200	0.26	0.24	0.309	0.47	0.35	0.000
Observations	1,293	1,457		1,104	1,265		571	668	

Notes. Two-man races only. *Aligned* municipalities are those where the winner is affiliated with the broad coalition, narrow coalition, or party of the president, respectively. *Non aligned* municipalities are those where the winner is not affiliated with the broad coalition, narrow coalition, or party of the president, respectively. All columns except those with *p-value* report the average values in the respective subsamples; *p-value* refers to the statistical significance of the difference between means. *First two-year transfers* are the average infrastructure transfers from the federal government to municipalities in the first two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). *Last two-year transfers* are the average infrastructure transfers from the federal government to municipalities in the last two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). *Population* is the number of resident inhabitants in 2000. *Per-capita income* refers to monthly income in 2000 and is measured in Brazilian *reais*. The following variables refer to the 2000 Census and are expressed in percentage terms: *Urban population* is the fraction of people living in urban areas; *Literacy rate* is the fraction of people above 20 who are literate; *Water access*, *Sewer*, and *Electricity* are the fraction of houses with access to water supply, sewer, and electricity, respectively; *Radio* captures whether there is at least one local radio station in the municipality. *Years of schooling*, *male*, and *married* refer to the characteristics of the mayor. *North*, *Northeast*, *Center*, *South*, and *Southeast* are macro-regions.

Table 5 – The effects of partisan alignment on last two-year transfers

	Broad coalition	Narrow coalition	Party president
		Panel A: All sample	
OLS	3.050***	3.424***	2.819***
	(0.761)	(0.799)	(1.018)
Diff-in-diff	3.724***	4.191***	2.723*
	(1.094)	(1.168)	(1.569)
Observations	8,243	8,243	8,243
		Panel B: Two-man races	
OLS	2.961*	3.529**	3.308
	(1.601)	(1.714)	(2.343)
Diff-in-diff	9.093***	10.786***	14.687***
	(2.776)	(3.040)	(5.529)
Observations	2,750	2,369	1,239
		Panel C: Two-man races	
Split polynomial (full h)	7.987**	9.533**	10.553*
	(3.461)	(3.885)	(5.764)
Observations	2,750	2,369	1,239
Split polynomial (half h)	7.924**	7.994*	10.230
	(3.904)	(4.414)	(6.448)
Observations	2,645	2,264	1,177
Local linear regression	4.932*	8.128**	9.824*
	(2.859)	(3.771)	(5.840)
Optimal h	0.23	0.11	0.11
Observations	2,120	1,116	562

Notes. Dependent variable: *Last two-year transfers*, i.e., the average infrastructure transfers from the federal government to municipalities in the last two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). Estimation methods: OLS in the all sample and two-man races only; diff-in-diff in the all sample and two-man races only; RDD in two-man races with different estimation methods (split polynomial with full bandwidth h; split polynomial with half bandwidth h; local linear regression with optimal bandwidth h). Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6 – The effects of partisan alignment on first two-year transfers

	Broad coalition	Narrow coalition	Party president
		Panel A: All sample	
OLS	0.475	-5.340	2.132
	(1.368)	(3.975)	(1.532)
Diff-in-diff	0.475	-5.340	2.132
	(1.368)	(3.975)	(1.532)
Observations	7,618	7,618	7,618
		Panel B: Two-man races	
OLS	-2.246	-0.374	-2.996
	(5.506)	(1.332)	(5.706)
Diff-in-diff	-2.246	-0.374	-2.996
	(5.506)	(1.332)	(5.706)
Observations	2,492	2,341	1,784
		Panel C: Two-man races	
Split polynomial (full h)	-4.148	-3.218	-0.196
	(4.575)	(4.722)	(6.462)
Observations	2,492	2,341	1,784
Split polynomial (half h)	-5.521	-2.799	1.757
	(5.081)	(4.633)	(6.744)
Observations	2,363	2,216	1,693
Local linear regression	-3.478	0.476	1.262
	(4.250)	(3.969)	(5.798)
Optimal h	0.23	0.11	0.11
Observations	1,854	1,067	762

Notes. Dependent variable: *First two-year transfers*, i.e., the average infrastructure transfers from the federal government to municipalities in the first two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). Estimation methods: OLS in the all sample and two-man races only; diff-in-diff in the all sample and two-man races only; RDD in two-man races with different estimation methods (split polynomial with full bandwidth h; split polynomial with half bandwidth h; local linear regression with optimal bandwidth h). Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7 – Estimated discontinuities of town and mayoral characteristics

	Broad coalition	Narrow coalition	President party
Population	-134.538	195.094	789.508
	(1,262.501)	(1,388.196)	(1,925.881)
Per-capita income	1.535	-1.561	-5.083
	(8.200)	(8.851)	(11.550)
Urban population	0.004	0.001	-0.018
	(0.021)	(0.022)	(0.031)
Water access	-0.007	-0.019	-0.038
	(0.022)	(0.024)	(0.032)
Sewer	-0.015	-0.020	-0.035
	(0.025)	(0.026)	(0.041)
Electricity	0.005	0.005	0.007
	(0.015)	(0.015)	(0.020)
Literacy rate	-0.007	-0.010	-0.007
	(0.012)	(0.013)	(0.017)
Radio	0.044	0.053	-0.028
	(0.032)	(0.036)	(0.050)
Years of schooling	0.231	0.640	1.917***
	(0.423)	(0.465)	(0.692)
Male	0.019	0.023	0.005
	(0.025)	(0.023)	(0.035)
North	-0.031*	-0.007	0.029
	(0.018)	(0.020)	(0.029)
Northeast	0.080*	0.070	-0.034
	(0.043)	(0.046)	(0.059)
Center	0.017	0.007	0.024
	(0.022)	(0.026)	(0.037)
South	-0.016	0.014	0.057
	(0.043)	(0.047)	(0.060)
Southeast	-0.049	-0.085**	-0.075
	(0.040)	(0.042)	(0.065)
Observations	2,750	2,369	1,239

Notes. Estimated discontinuities of town and mayoral characteristics at the threshold of zero margin of victory for the broad coalition, narrow coalition, or party of the president, respectively. Two-man races only. Estimation method: split 3^{rd} order polynomial with full bandwidth. *Population* is the number of resident inhabitants in 2000. *Per-capita income* refers to monthly income in 2000 and is measured in Brazilian *reais*. The following variables refer to the 2000 Census and are expressed in percentage terms: *Urban population* is the fraction of people living in urban areas; *Literacy rate* is the fraction of people above 20 who are literate; *Water access*, *Sewer*, and *Electricity* are the fraction of houses with access to water supply, sewer, and electricity, respectively; *Radio* captures whether there is at least one local radio station in the municipality. *Years of schooling*, *male*, and *married* refer to the characteristics of the mayor. *North*, *Northeast*, *Center*, *South*, and *Southeast* are macro-regions. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 8 – Partisan alignment and last two-year transfers: Heterogeneity results

	Broad coalition	Narrow coalition	President party
	Panel A: First vs. second term		
First term	-1.111	-1.474	5.333
	(4.578)	(5.246)	(5.865)
Second term	15.643*	17.091*	34.509
	(8.800)	(10.362)	(23.702)
Difference	16.754*	18.565*	40.176*
	(9.617)	(11.220)	(24.468)
Observations	2,750	2,369	1,239
	Panel B: Radio vs. no radio		
No Radio	10.419***	12.664***	11.148*
	(3.995)	(4.511)	(6.575)
Radio	-5.225	-5.707	0.361
	(4.476)	(4.561)	(6.245)
Difference	-15.644**	-18.371***	-10.787
	(6.117)	(6.480)	(9.086)
Observations	2,750	2,369	1,239
	Panel C: Small vs. large towns		
Small town	11.759**	15.057***	12.182
	(5.015)	(5.365)	(7.694)
Large town	0.448	-0.643	6.561
	(3.348)	(3.529)	(7.158)
Difference	-11.311*	-15.700**	-5.621
	(6.089)	(6.603)	(10.347)
Observations	2,750	2,369	1,239

Notes. Dependent variable: *Last two-year transfers*, i.e., the average infrastructure transfers from the federal government to municipalities in the last two years of the mayoral term (per-capita real values in 2000 Brazilian *reais*). Estimation method: RDD in two-man races; split polynomial with full bandwidth; discontinuities estimated separately in different subsamples. *First vs. second term* compares first-term and second-term mayors. *Radio vs. no radio* compares municipalities with and without radio station. *Small vs. large towns* compares municipalities below and above the median population. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.