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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 of political candidates with different abilities.

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

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 to be 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, despite 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 endogeneity 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 politicians of local governments. Each one of the above 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 features 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. Litschig and Morrison (2009) use the same approach and data for the municipal term 1984–88 to estimate the impact of federal transfers on the reelection probability of the incumbent party in mayoral elections, detecting a positive and significant effect. Using a tailored household survey, Vicente (2009) shows that the discovery of oil in the island of *São Tomé and Principe* was associated with a significant rise in perceived 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 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 effect of corruption disclosure on the election outcome and the effect 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 into 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]

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

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 market experience. 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 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

[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 on the absolute amount of rents as assumed here).

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

$$\begin{aligned} p^{JO} &= Pr[\theta \geq \frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O)] & (9) \\ &= \frac{1}{2} + \xi(1 + \sigma^J) - \xi \frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O) & (10) \end{aligned}$$

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

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

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

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:

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

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:

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

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 competence 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 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. Not only does a larger budget size

[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 ($d(r_t) = qr_t$). Intuitively, under the alternative assumption, a larger budget would reduce the probability of detection, inducing the incumbent mayor to grab even more rents.

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

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

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 of these forces 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 were 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 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:

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

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.

[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 would 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), Galasso and Nannicini (2009), and Persico, Rodriguez Pueblita, and Silverman (2009).

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 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. We highlight one such set of results, namely those relating to the effects of a windfall of government revenues. The remainder of the paper tests these implications on Brazilian municipal data, exploiting an institutional feature whereby federal transfers to municipal governments vary exogenously according to given population thresholds. The parameter τ in the model therefore corresponds to federal transfers received by municipal governments.

The theory generates predictions 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).

However, the empirical tests of these last two implications—unlike those of the first three—must rely on descriptive rather than quasi-experimental evidence, because the RDD setup only applies to transfers (τ) as a treatment.

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. Since 2000, the term limit for mayors has been 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 (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 our identification strategy (see next section).

[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%.

According to 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 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 with population below 50,940 (about 90% of Brazilian municipalities and 34% of the total 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 councillors 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), based on the population estimates calculated yearly by an independent 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,

[9] At the federal level, the resources earmarked for FPM transfers are 22.5% of total revenues from the federal income tax and 22.5% of revenues from 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.

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 practice 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 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

[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. Note that 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.

should expect to receive during the next term, in case they won the electoral race. The averaging across years within the same term also allows us to minimize measurement error.

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

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

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 of revenues, leaving the budget size unchanged, we also collected data on municipal finance, available from the Brazilian National Treasury website (*FIMBRA* dataset). However, these budget data—unlike the data on FPM transfers—are self-reported and therefore come from a different source.

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 or co-financed by the municipality unconstrained resources (therefore including FPM transfers) are also audited.[15] 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 abuses concerning the 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.[16] 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

[15] In particular, to obtain discretionary transfers (*covenio*, most of them for infrastructures), municipalities should contribute for a share of the project (*contrapartida*), whose amount is defined according to limits based on the municipal financial capacity as established by the *Lei de Diretrizes Orçamentárias*. Municipalities with population below 50,000 should finance from 2% to 4% of the total cost of the project.

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

check, we also consider an additional measure for each definition of corruption, namely 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 and are available upon request.

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 the corruption measures. Descriptive statistics on these variables—by population intervals—are reported in Table 3. According to our broad measure of corruption, 71% of mayors in the municipalities in our sample are found to be corrupt. This figure is decreasing with population size. For the more restrictive measure, 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.[17]

[17]Note that our definition of broad corruption is close to the measure used by Ferraz and Finan (2009a, Table 1), whose incidence is 78%.

Note that, among the 606 observations in the small sample, 229 (about 38%) refer to mayors who are in their first term and then decide to 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.[18] 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 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

[18]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%).

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.[19]. 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.[20]

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 is in the second term, or because he 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 municipalities. 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

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

[20]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.

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).[21]

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 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

[21]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.

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 (lower) theoretical transfers, that is, 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 frequency 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—both in the pooled thresholds used 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.[22]

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 (visual) evidence of a little sorting in the population estimates for 2001.[23]

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.

[22] Point estimates (standard errors) for the tests 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).

[23] Point estimates (standard errors) for the tests 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).

As the current FPM thresholds were 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 to the cell it belongs to (see Appendix I for more details). If manipulative sorting were at work in the actual Census 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 removing 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 (discretionary) 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 it 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 and robustness checks

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.

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, but not for broad corruption.[24]

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

[24]Note that using a treatment dummy rather than theoretical transfers, exactly as we have done for the balance tests in Tables 5 and 6, yields similar results with respect to the RDD estimations. In particular, for the pooled thresholds 1–7, we obtain the following estimates (standard errors). Broad corruption: 0.304 (0.068). Narrow corruption: 0.320 (0.076). Episodes of broad corruption: 0.382 (0.249). Episodes of narrow corruption: 0.442 (0.151). Results for the other thresholds are available upon request.

the number of episodes of narrow corruption by 0.19 (26%). Looking separately at the individual thresholds, we can see that moving from the average amount of transfers due to municipalities below the second threshold to the amount for those above it (an increase of approximately 6.1 hundred thousand *reais*) would increase broad corruption by about 51% and narrow corruption by about 52%. For the third threshold, the jumps would be of about 40% for broad corruption and 89% for narrow corruption.

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.

Finally, to further assess the validity of our identification strategy, in Table 11 we perform placebo tests by estimating the treatment effect at fake thresholds, where there should be no effect. In particular, mirroring the balance tests in Tables 5 and 6, we estimate whether there is any discontinuity in our corruption measures at the fake thresholds represented by the midpoints between the true FPM thresholds. With only one exception at the 10% level, the effects are never statistically different from zero.

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 additional revenues triggered by the thresholds.

Note that, to gain observations, the specifications in Tables 8-10 never include 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 constant the quality of the opponents) and the interaction effect (when the quality of the opponent is allowed to change with budget size)—see 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. The overall impact of transfers on corruption, however, remains statistically significant also in this small sample with 229 observations.

As a final remark, 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 (discretionary) 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. However, the assumption of no crowding-out seems to be met in our data, as discussed for Table 7 above.

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 12 refers to the first-stage and reduced-form regressions, while Table 13 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 IV estimates in Table 13, 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.[25] The overall impact on high-skilled occupation is not statistically significant, but there is evidence of a negative effect at some thresholds.[26]

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 councillors 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 councillors and not mayors, whose quality we measure here. And general equilibrium effects from the selection of councillors to the selection of mayors are implausible, because the wage policy was only introduced in 2000. Furthermore, Ferraz and Finan (2009b) show that around 10,000 there is a discontinuity in the characteristics of councillors, which may be due to either the wage cap or the FPM policy studied here. This discontinuity, however, is equivalent to an increase in the fraction of high (rather than low) quality politicians in the city council, the opposite of what we find for mayors. Nevertheless, below we report

[25]As the current 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).

[26]As for corruption, using a treatment dummy rather than theoretical transfers, exactly as we have done for the balance tests in Tables 5 and 6, yields similar results with respect to the RDD estimations. In particular, for the pooled thresholds 1–7, we obtain the following estimates (standard errors). Years of schooling: -0.069 (0.032). College: -0.490 (0.228). High-skilled occupation: -0.030 (0.023). Reelection: 0.057 (0.039). Results for the other thresholds are available upon request.

additional robustness exercises that address this issue.

Figure 7 provides a graphical representation of the discontinuities in the political variables induced by the FPM policy (the 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 14, 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 the confounding policy on the wage of city councillors 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 15, we include some additional robustness checks specific 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 (non-random) 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 of the results at some thresholds.

In Table 16, exactly as we have done for corruption, we implement placebo tests at fake thresholds (i.e., the midpoints between the true FPM thresholds) for the political selection variables. The estimated effects are never statistically different from zero, supporting once again the validity of the identification strategy.

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.[27]

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. However, 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 17 and 18 investigate the correlations. In both tables, corruption is the dependent variable (measured in different ways). Table 17 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, contradicting Proposition 2. Table 18 is instead motivated by Proposition 3. There, we report the effects of both FPM transfers and their interactions with different measures of the opponents' quality. Actual and interacted transfers are instrumented with theoretical transfers and their respective interaction with opponents' quality. The coefficient of interest is the interaction effect, which the theory predicts to be negative. Although in the larger sample that includes all thresholds there is no clear pattern, when the sample is restricted to the first three thresholds of more comparable municipalities,

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

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.

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 backwardness 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 opponents are now of lower quality, an incumbent can afford to grab even more rents while at the same time increasing his probability of reelection.

In light of these (theoretical) results, we have 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 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 opponents with a college degree by 7%.

These results are not inconsistent with higher transfers to municipalities increasing the quantity and 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. Nevertheless, 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 Brazilian 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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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.

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.* 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 auction. 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 owner, and that this person also owned the surrounding areas, hampering free access to the dam.

(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 *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.

Tables and Figures

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 3.1. The underlined thresholds are those studied in our empirical exercise.

Table 2 – Actual and Theoretical FPM Transfers

Population	Small sample			Large sample		
	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 3.2 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 3.3 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 (30). 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 (30)-(31). 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 (33). 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 (33) 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 – Placebo Tests: Corruption Measures

	Broad corruption	Narrow corruption	No. of broad corruption episodes	No. of narrow corruption episodes	Obs.
Thresholds 1–7	0.009	0.095	0.254	0.171	543
	(0.074)	(0.081)	(0.270)	(0.165)	
Thresholds 1–3	0.019	0.177	0.111	0.189	401
	(0.103)	(0.116)	(0.387)	(0.220)	
Threshold 1	0.049	0.116	-0.051	0.018	241
	(0.143)	(0.160)	(0.548)	(0.312)	
Threshold 2	-0.132	-0.202	0.230	-0.436	226
	(0.155)	(0.166)	(0.621)	(0.322)	
Threshold 3	-0.134	0.301*	-0.180	0.500	198
	(0.170)	(0.179)	(0.522)	(0.357)	

Notes. Discontinuity of corruption measures at fake thresholds (i.e., midpoints between the true FPM thresholds), estimated as the jump of a (split) third-order polynomial around the (fake) pooled thresholds or around the (fake) individual thresholds. Midpoints are: 1^{st}) 11,887; 2^{nd}) 15,283; 3^{rd}) 20,337; 4^{th}) 27,169; 5^{th}) 33,961; 6^{th}) 40,753; and 7^{th}) 47,545 . 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 – 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 (30)-(31). 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 – 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 (33). 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 – Robustness Checks: Opponents' Characteristics and Election Outcome

	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
			Spline (3rd-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)	
			2nd-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 (2nd-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)	
			4th-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 (4th-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 (3rd-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 (3rd-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 (33) 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 15 – 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 13 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 16 – Placebo Tests: Opponents' Characteristics and Election Outcome

	College	Years of schooling	High-skilled occupation	Incumbent reelection	Obs.
Thresholds 1–7	0.015	0.193	0.018	-0.048	2,430
	(0.033)	(0.232)	(0.033)	(0.040)	
Thresholds 1–3	0.021	0.127	0.010	0.004	1,775
	(0.047)	(0.335)	(0.047)	(0.057)	
Threshold 1	0.016	0.746	-0.007	-0.011	1,066
	(0.069)	(0.511)	(0.071)	(0.082)	
Threshold 2	-0.017	-0.167	-0.008	-0.062	953
	(0.066)	(0.460)	(0.067)	(0.082)	
Threshold 3	0.033	-0.018	0.108	0.008	864
	(0.069)	(0.503)	(0.069)	(0.086)	

Notes. Discontinuity of the political selection variables at fake thresholds (i.e., midpoints between the true FPM thresholds), estimated as the jump of a (split) third-order polynomial around the (fake) pooled thresholds or around the (fake) individual thresholds. Midpoints are: 1^{st}) 11,887; 2^{nd}) 15,283; 3^{rd}) 20,337; 4^{th}) 27,169; 5^{th}) 33,961; 6^{th}) 40,753; and 7^{th}) 47,545 . 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 17 – 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)

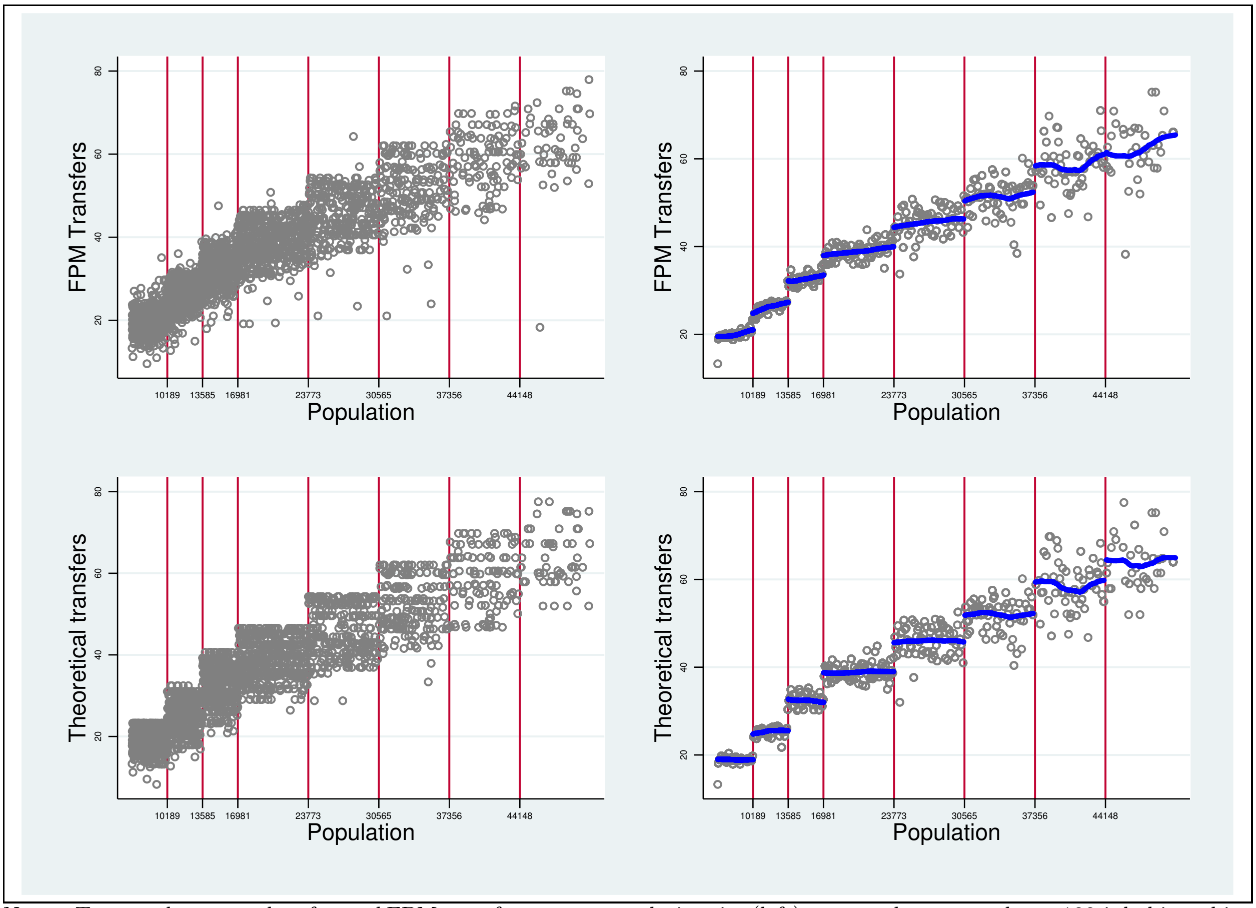
Notes. Probit (first and second corruption measures) and OLS (third and fourth corruption measures) estimations of the correlation between corruption and opponents' characteristics; marginal effects reported. Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality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

Table 18 – 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)

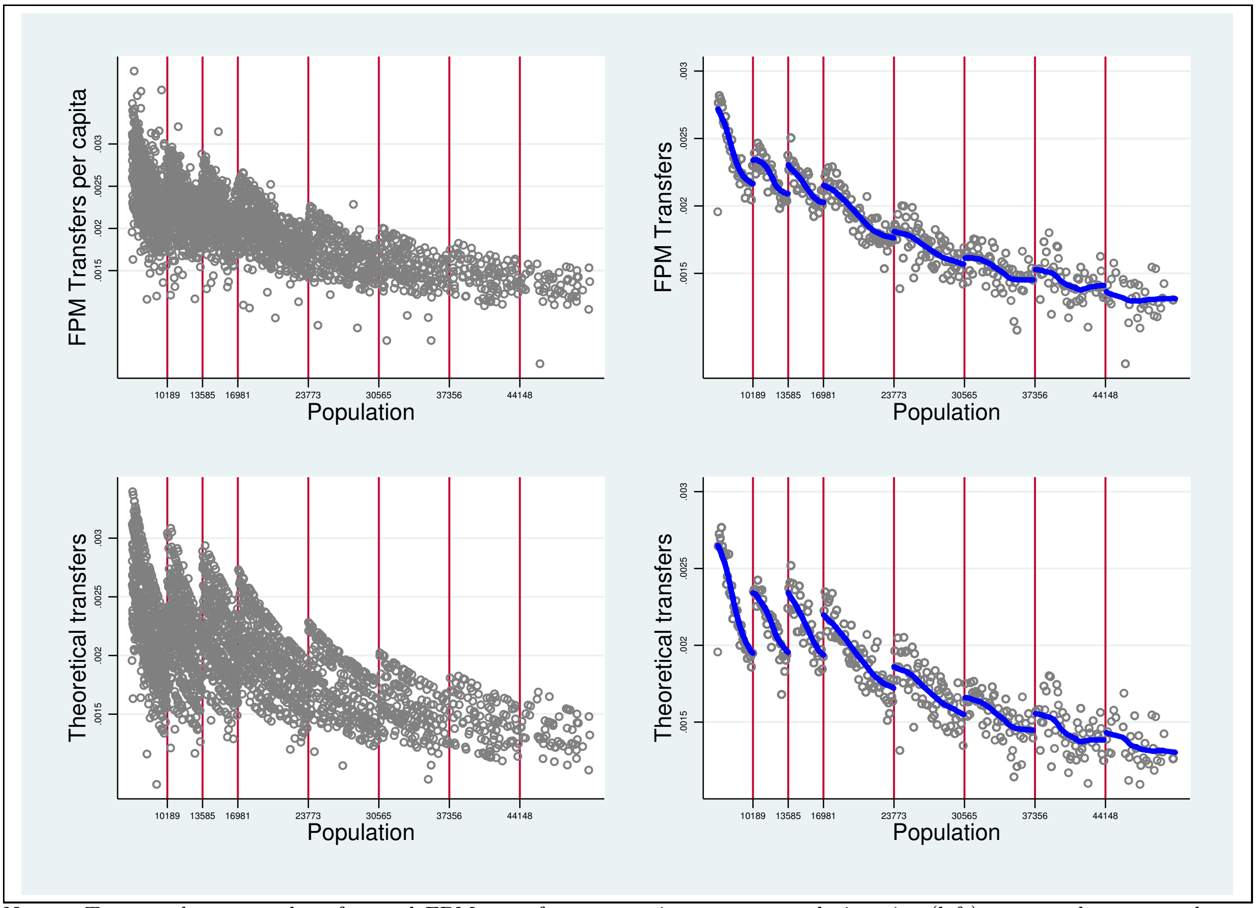
Notes. Effects of FPM transfers and their interaction with each opponents' characteristic (instrumented with theoretical transfers and their interaction with each opponents' characteristic). Mayoral terms 2001–2005 and 2005–2009. Robust standard errors clustered at the municipality level in parentheses. 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



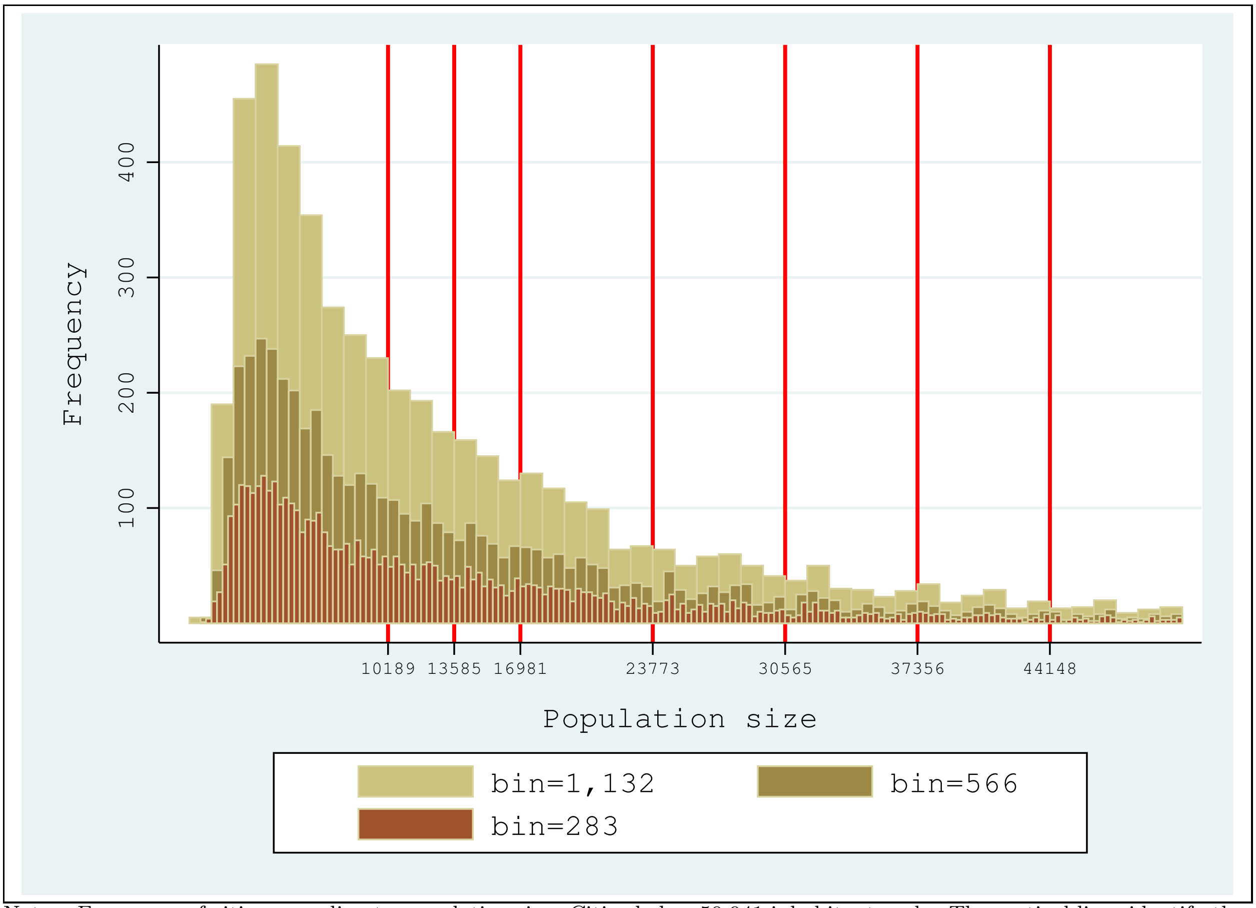
Notes. Top panel: scatterplot of actual FPM transfers versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers versus population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



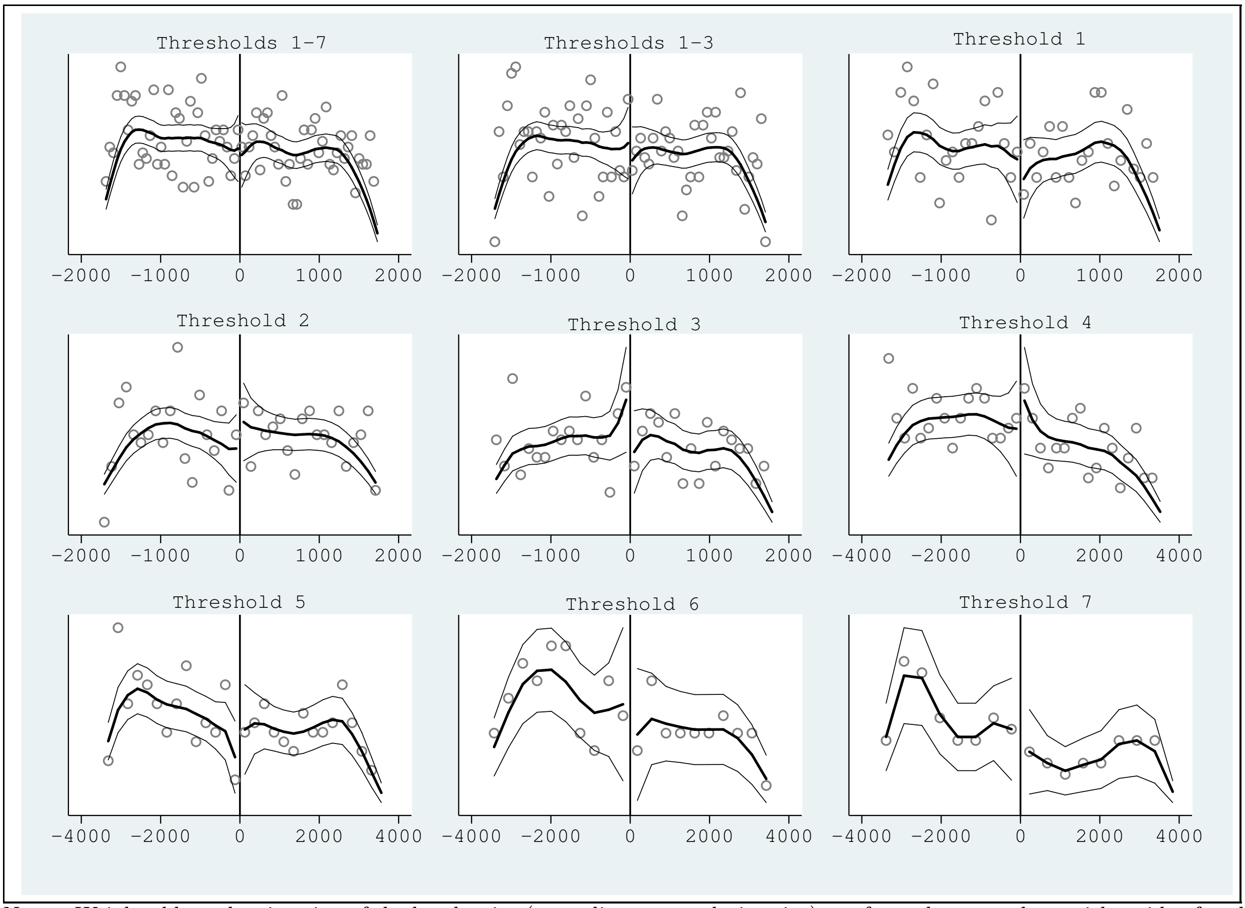
Notes. Top panel: scatterplot of actual FPM transfers per capita versus population size (left); scatterplot averaged over 100-inhabitant bins plus running-mean smoothing performed separately in each interval between two consecutive thresholds (right). Bottom panel: scatterplot of theoretical transfers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



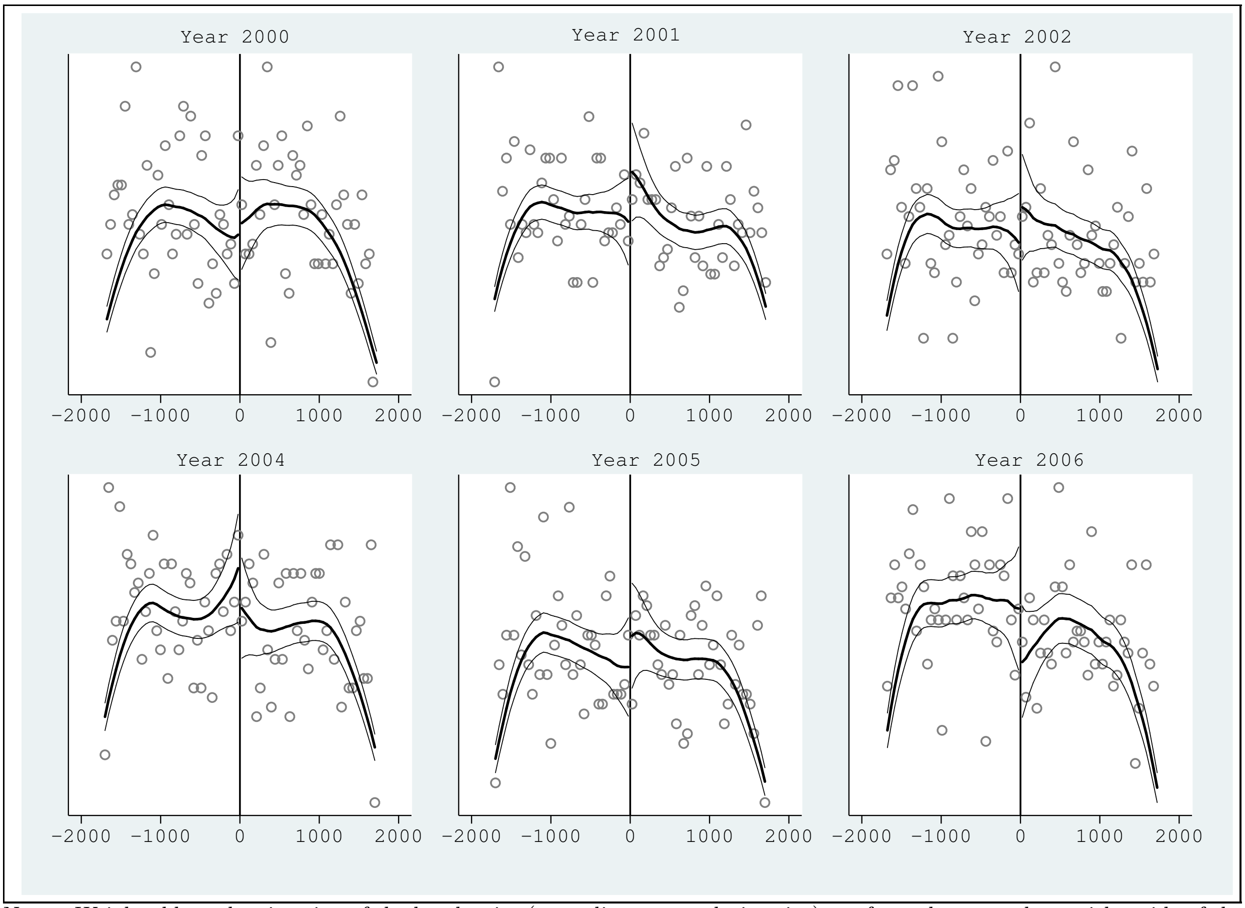
Notes. Frequency of cities according to population size. Cities below 50,941 inhabitants only. The vertical lines identify the first seven FPM revenue-sharing thresholds. Mayoral terms 2001–2005 and 2005–2009.

Figure 4 – McCrary Density Tests: Pooled and Individual Thresholds



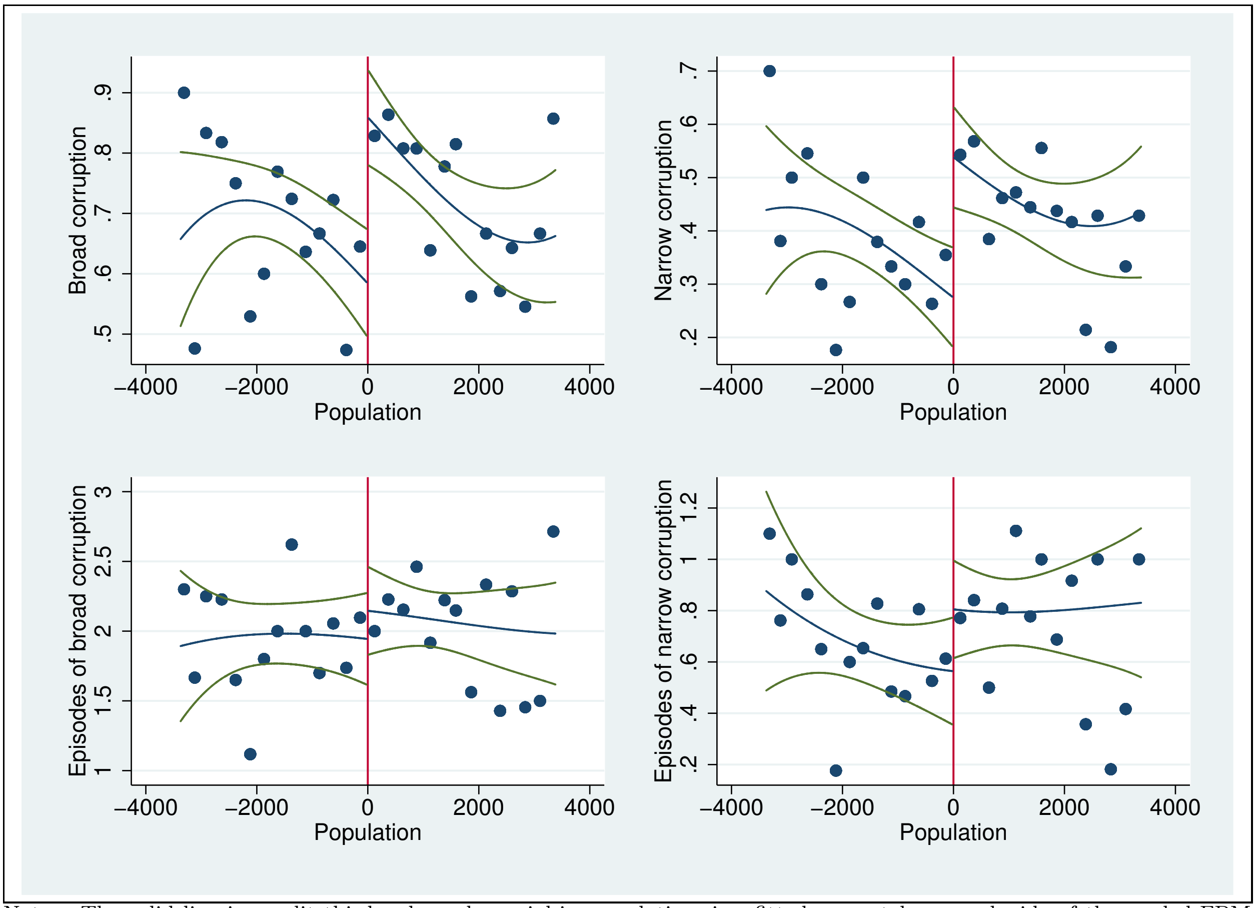
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of each pooled or individual FPM revenue-sharing threshold. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables. Mayoral terms 2001–2005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

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



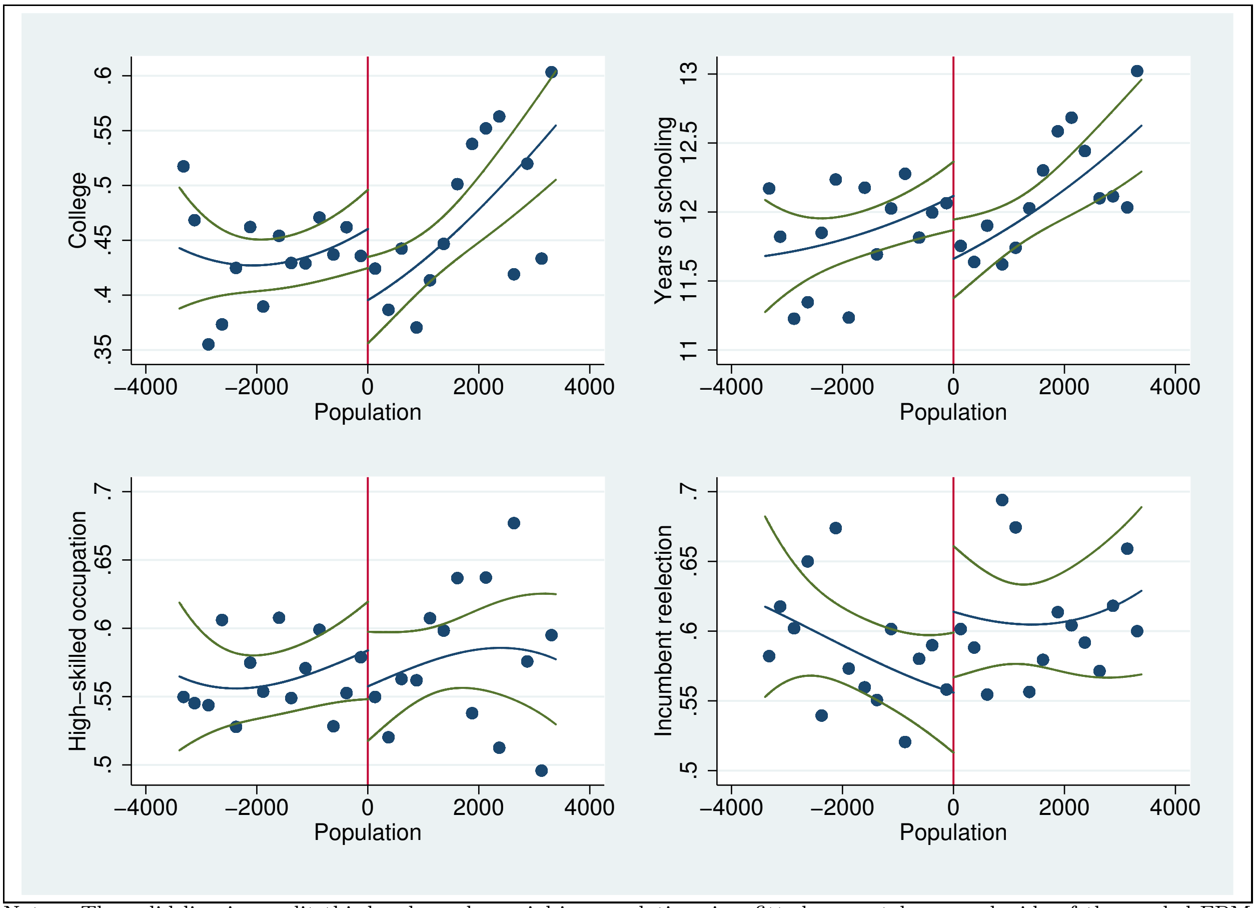
Notes. Weighted kernel estimation of the log density (according to population size), performed separately on either side of the pooled FPM revenue-sharing threshold (1–7) for each year in the sample period. Optimal binwidth and binsize as in McCrary (2008). Large sample with political selection variables.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