The Political Resource Curse*
Online Appendix

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Abstract

This Appendix provides additional materials that are also discussed in the paper. In particular, Section A1 presents the theoretical derivations of some of the model's results. Section A2 presents validity tests on the lack of manipulative sorting in the IBGE population estimates, that is, the running variable of the fuzzy RD design implemented in the paper. Section A3 reports examples of violation episodes from the audit reports, in order to illustrate the coding of our measures of political corruption. Section A4 provides further robustness checks, in order to show that the baseline estimation results are not sensitive to the definition of the outcome variables, to functional form assumptions, or to sample restrictions.

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A1 Theoretical derivations

Equilibrium rents, holding constant the quality composition of the opponents

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:

$$E(\theta|g_1, J) \geq 1 + \sigma^O \qquad J, O = H, L \tag{A1}$$

where the left hand side of (A1) 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 equation (1) in the paper, it is easy to see that (see also Persson and Tabellini, 2000):

$$E(\theta|g_1, J) = \frac{g_1}{(\tau - r_1^{eJ})} \tag{A2}$$

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

$$E(\theta|g_1, J) = \theta\frac{\tau - r_1^J}{\tau - r_1^{eJ}} \tag{A3}$$

where r_1^J denotes the rents actually set by a type J incumbent. Thus, by (A1)-(A3), an incumbent of type J running against an opponent of type O wins the election with a perceived probability

$$p^{JO} = Pr[\theta \geq \frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O)] \tag{A4}$$

$$= \frac{1}{2} + \xi(1 + \sigma^J) - \xi\frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1 + \sigma^O) \tag{A5}$$

where the first equation follows from (A1)-(A3), and the second equation from the assumption about the distribution of θ.[1] More precisely, the above expression refers to the probability of

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

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

being reappointed, as perceived by the incumbent when setting rents, conditional on knowing the identity of the opponent.

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:

$$p^J = \frac{1}{2} + \xi(1+\sigma^J) - \xi\frac{\tau - r_1^{eJ}}{\tau - r_1^J}(1+\hat{\sigma}) \tag{A6}$$

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

$$1+\hat{\sigma} \equiv 1+\sigma(1-2\pi) \tag{A7}$$

We are now ready to discuss the determination of public policy in period 1. The incumbent maximizes equation (3) in the paper with respect to r_1, subject to (A6) 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:

$$\frac{\partial V_1^J}{\partial r_1} = \alpha^J + \frac{\partial p^J}{\partial r_1}V_2^J = 0 \tag{A8}$$

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

$$V_2^J = \alpha^J\overline{r} + R \equiv \alpha^J\psi\tau + R \tag{A9}$$

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 (A6) we have that in equilibrium:

$$\frac{\partial p^J}{\partial r_1^J} = -\frac{\xi(1+\hat{\sigma})}{\tau - r_1^J} < 0 \tag{A10}$$

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 (A10) the "electoral punishment" of the marginal rent. Note that (A10) immediately implies the first part of Prediction 1, i.e., the electoral punishment drops in absolute value as τ increases.

Combining (A8)-(A10), the equilibrium rent set in period 1 by an incumbent of type J is:

$$r_1^J = \tau - \xi(1+\hat{\sigma})(\psi\tau + R/\alpha^J) \tag{A11}$$

where, to have an interior optimum, we implicitly assume that the right hand side of (A11) 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 $\hat{\sigma}$. 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 (A10), it increases the electoral punishment of the marginal rent.

Differentiating the RHS of (A11) with respect to τ and $\hat{\sigma}$, we obtain the second parts of Predictions 1 and 2, as well as Prediction 3. Equation (A11) also implies the first part of Prediction 2: since $\alpha^H < \alpha^L$, equation (A11) implies that $r_1^H < r_1^L$.

Finally, and for use in the next subsection, note that the equilibrium probability that an incumbent of type J defeats an opponent of type O is:

$$p^{*J,O} = \frac{1}{2} + \xi(\sigma^J - \sigma^O) \tag{A12}$$

where we have used (A5) and we have imposed the equilibrium condition that actual and expected rents coincide; the "*" superscript denotes equilibrium. Correspondingly, the equilibrium probability of reappointment, based on the information available to the incumbent, is:

$$p^{*J} = \frac{1}{2} + \xi(\sigma^J - \hat{\sigma}) \tag{A13}$$

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.

Equilibrium when the quality of the opponents is endogenous

We now turn to the Proof of Predictions 4-6, when the quality of the opponents is determined endogenously in equilibrium.

By (A12) and the assumption that the incumbent type is unknown (i.e., that $\hat{\sigma} = 0$), the expected probability that an opponent of type J wins the election is $p^{*J} = \left[\frac{1}{2} + \xi\sigma^J\right]$. We can then rewrite the condition that induces the i-th individual in group J to enter politics (condition (4) in the paper) as:

$$iy^J \leq \frac{\left[\frac{1}{2} + \xi\sigma^J\right]}{n} V_2^J \tag{A14}$$

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

$$y^J n^J = \frac{\left[\frac{1}{2} + \xi\sigma^J\right]}{n} V_2^J \tag{A15}$$

Using (A15) we can solve for n:

$$n = \sqrt{\frac{V_2^H}{y^H}(\frac{1}{2} + \xi\sigma) + \frac{V_2^L}{y^L}(\frac{1}{2} - \xi\sigma)} \tag{A16}$$

Then from (A15) we have

$$n^J = \frac{V_2^J}{y^J}\frac{\left[\frac{1}{2} + \xi\sigma^J\right]}{n}, \quad J = H, L \tag{A17}$$

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

$$\pi = \frac{n^L}{n^H + n^L} = \frac{1}{1 + x} \tag{A18}$$

where

$$x \equiv \frac{V_2^H}{V_2^L}\frac{y^L}{y^H}\frac{\frac{1}{2} + \xi\sigma}{\frac{1}{2} - \xi\sigma} \gtrless 1 \tag{A19}$$

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.

To prove Prediction 4, 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:

$$\partial\frac{V_2^H}{V_2^L}/\partial\tau = \frac{\psi R}{(V_2^L)^2}(\alpha^H - \alpha^L) < 0 \tag{A20}$$

which in turn implies that $\partial\pi/\partial\tau > 0$—see (A18)-(A19)—and proves Prediction 4.

Combining (A11) with the definition of $\hat{\sigma}$ (A7) and with (A18), we get

$$r_1^J = \tau - \xi\left[1 - \sigma\left(\frac{1 - x}{1 + x}\right)\right](\psi\tau + R/\alpha^J) \tag{A21}$$

which we call the "general equilibrium" rent to distinguish it from the "partial equilibrium" rent (A11). It is easy to see that the equivalent of Prediction 1 holds also for the general equilibrium rent (A21). Prediction 6 follows from differencing r_1^J with regard to τ.

Finally, to prove Prediction 5, consider expression (A13), 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. Differentiating the RHS of (A13) and exploiting Prediction 4, we obtain Prediction 5.

A2 IBGE population estimates and manipulation tests

The IBGE procedure to construct 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.

Testing for manipulative sorting in the IBGE population estimates

For the validity of the (fuzzy) RD design implemented in the paper, it is crucial that the IBGE population numbers are not manipulated by local governments around the FPM cutoffs. We check for the lack of manipulative sorting in the following figures and tables.

Figure A1 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. Visual inspection does not reveal any frequency discontinuity at the seven FPM thresholds under investigation. We formally test for the presence of density discontinuities in Figure A2, 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). We run these tests using our population measure—averaged over each mayoral term—both in the pooled thresholds used in the main estimations (1–7, 1–3, and 4–7) and separately for each of the seven thresholds. We implement the pooling of thresholds 1–7 (overall effect), 1–3, and 4–7 by merging all of the relevant 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 each figure, the log-difference between the frequency to the right and to the left of each threshold is never statistically significant.[2] This confirms the lack of manipulation of the IBGE estimates averaged over the mayoral terms covered in our study.[3]

In Table A1, we further check for manipulative sorting by performing balance tests of the available invariant town characteristics and pre-treatment Census variables. If there were non-random sorting, we should expect some of these variables to differ systematically between treated and untreated municipalities around each FPM cutoff. 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). The balance tests are performed by estimating discontinuities in the invariant characteristics at every pooled threshold (1–7, 1–3, and 4–7), while controlling for a third-order polynomial as in our baseline specification for the main estimations reported in the paper. Panel A in Table A1 shows that no pre-treatment variables display a significant discontinuity, with the exclusion of Northeast in just one case and at a 10% level of statistical significance.

As the current FPM thresholds were established in 1981, in Panel B of Table A1, we use information from the 1980 Brazilian Census to check whether measures of 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 municipality 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 2,877 municipalities in our large sample, we thus end up with 2,276 observations. Importantly, no pre-treatment employment or wealth variables show a significant discontinuity in this sample.[4]

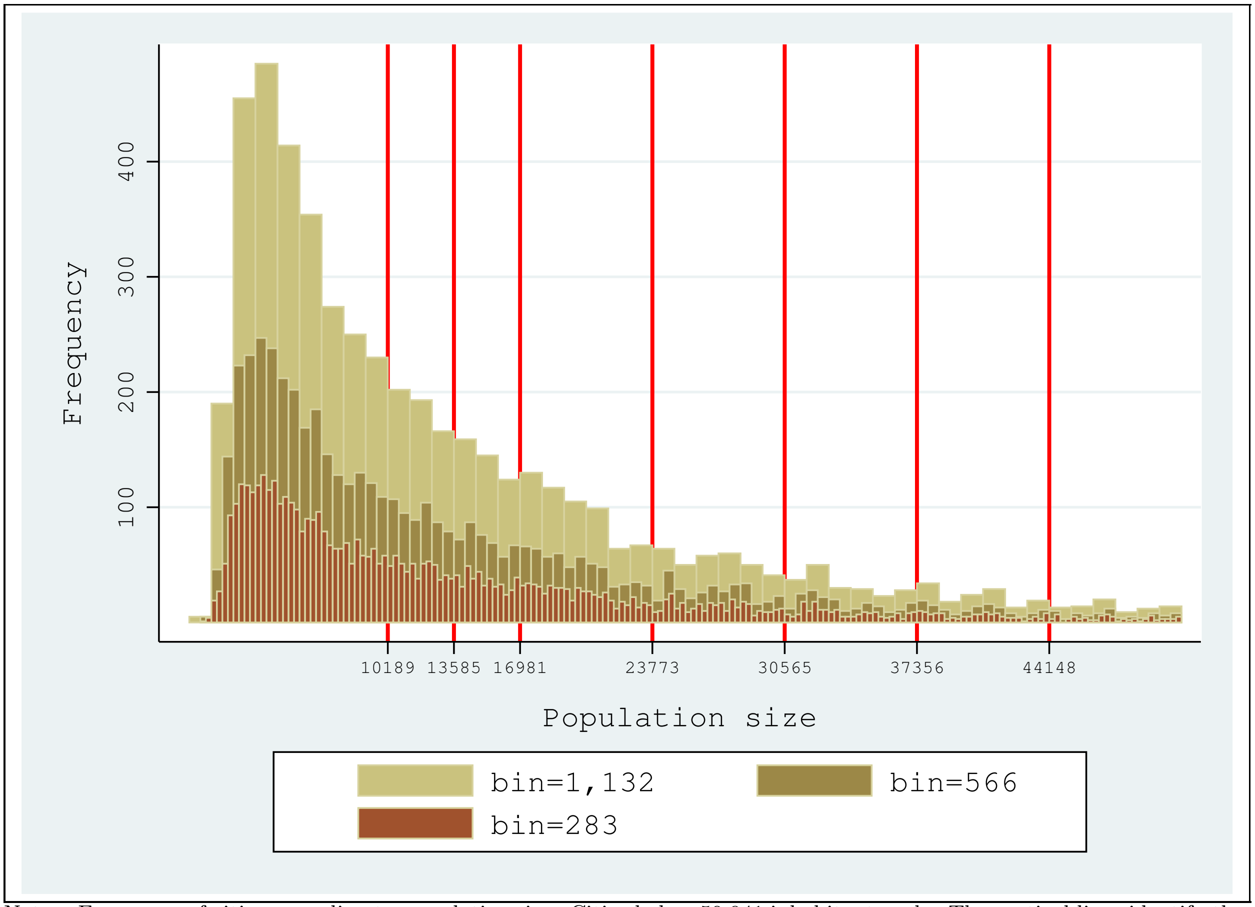
[2] Point estimates (standard errors) for the McCrary tests in Figure A2 are as follows. Overall effect: -0.050 (0.189); thresholds 1–3: -0.036 (0.144); thresholds 4–7: 0.391 (0.473); threshold 1: -0.213 (0.335); threshold 2: 0.444 (0.375); threshold 3: -0.192 (0.270); threshold 4: 0.159 (0.328); threshold 5: 0.578 (0.609); threshold 6: -0.546 (0.719); threshold 7: -0.511 (0.924). Optimal bandwidth and binsize as in McCrary (2008).

[3] In a previous working-paper version of the paper, we show the lack of manipulative sorting also in the yearly IBGE estimates for the years covered by our sample period (see NBER working paper No. 15705).

[4] In line with the results in Table A1, when we add the invariant and pre-treatment Census characteristics as control variables in our baseline RD specifications to implement a further robustness check, point estimates are substantially unchanged and statistical accuracy is improved (results available upon request).

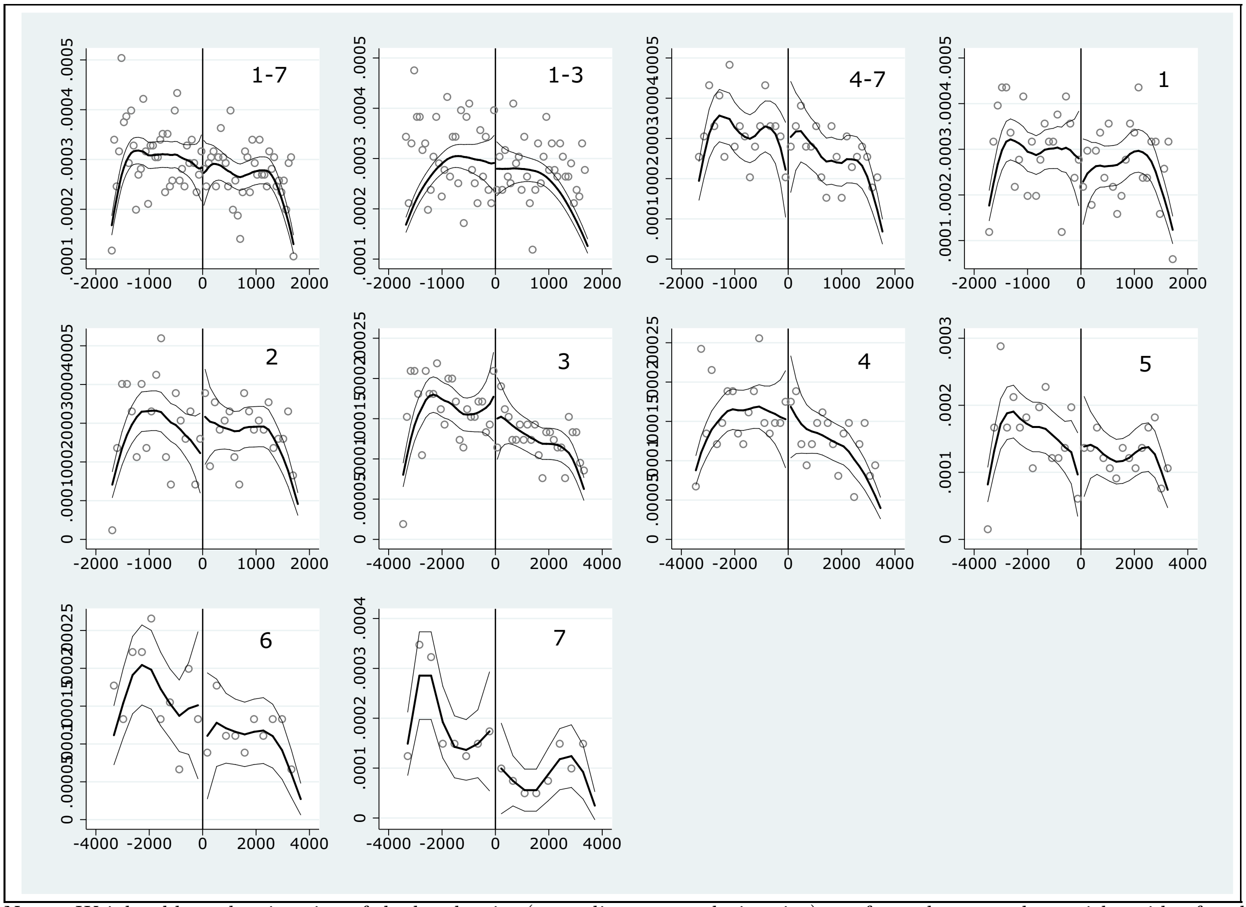
Finally, in Table A2, using the same specifications of Table A1, we run balance tests of potentially endogenous variables at the FPM cutoffs. Specifically, we check whether the political party of the mayor or his/her campaign funds (divided according to their source) display any discontinuity. This is not a pre-requisite for the validity of our RD setup, but showing that political orientation and campaign financing do not depend on FPM would rule out alternative political mechanisms behind the baseline results. This is indeed the case, as no variable in Table A2 shows any significant discontinuity. These findings are also discussed in the paper.

Figure A1 – Population distribution (below 50,941)



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

Figure A2 – 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 (1–7, 1–3, and 4–7) 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.

Table A1 – Balance tests of invariant and pre-treatment characteristics

	Overall effect	Thresholds 1–3	Thresholds 4–7
	A. Invariant town characteristics		
Area size	2.367	1.177	8.983
	(4.605)	(3.181)	(15.164)
North	-0.014	0.027	-0.060
	(0.022)	(0.029)	(0.044)
Northeast	-0.009	-0.102*	0.096
	(0.039)	(0.052)	(0.075)
Center	0.004	0.007	-0.022
	(0.020)	(0.028)	(0.037)
Southeast	-0.008	0.037	-0.105
	(0.036)	(0.048)	(0.070)
South	0.027	0.031	0.090
	(0.029)	(0.040)	(0.055)
Obs.	2,877	1,979	898
	B. Pre-treatment Census characteristics		
Employed	0.195	-0.054	0.004
	(0.531)	(0.711)	(0.924)
Refrigerator	0.015	0.055	-0.337
	(0.463)	(0.590)	(0.992)
Radio	0.207	0.491	-0.224
	(0.362)	(0.482)	(0.769)
Car	-0.032	-0.006	-0.209
	(0.195)	(0.248)	(0.430)
Water & sewer	-0.265	-0.231	-0.753
	(0.441)	(0.546)	(1.000)
Obs.	2,276	1,682	594

Notes. Estimated discontinuity of invariant town characteristics (panel A) and pre-treatment Census characteristics (panel B) at the FPM thresholds. Each cell reports the estimated coefficient of a dummy equal to one (zero) if population size is above (below) the closest threshold—controlling for a third-order polynomial in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each row heading. Robust standard errors clustered at the municipality level are in parentheses. The column "Overall effect" is obtained by estimations in the entire sample (i.e., thresholds 1–7); the columns "Thresholds 1-3" and "Thresholds 4-7" are obtained by estimations in the respective subsamples. Invariant town characteristics: *area size* is expressed in km^2; *North*, *Northeast*, *Center*, *Southeast*, and *South* are geographic location dummies. Pre-treatment Census characteristics: all variables come from the 1980 Census, are per capita, and refer to average employment; refrigerator, radio, or car ownership; house access to water and sewer. Mayoral terms 2001–2005 and 2005–2009. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A2 – Balance tests of potentially endogenous variables

	Overall effect	Thresholds 1–3	Thresholds 4–7
	A. Political orientation of the mayor		
PT	-0.015	-0.030	0.012
	(0.020)	(0.026)	(0.041)
PSDB	0.010	0.055	-0.011
	(0.026)	(0.034)	(0.054)
PMDB	-0.002	0.027	-0.014
	(0.030)	(0.042)	(0.056)
DEM	0.034	0.026	0.048
	(0.027)	(0.039)	(0.051)
Other parties	-0.027	-0.078	-0.035
	(0.039)	(0.052)	(0.079)
Obs.	2,877	1,979	898
	B. Campaign funds of the mayor		
Total funds	-2.782	6.996	-8.662
	(7.073)	(7.510)	(16.863)
Funds from firms	-4.335	1.710	-8.059
	(3.549)	(4.055)	(8.017)
Own funds	0.583	0.372	0.323
	(3.474)	(4.049)	(7.153)
Other donations	0.971	4.914	-0.927
	(4.509)	(4.355)	(12.049)
Obs.	1,082	708	374

Notes. Estimated discontinuity of potentially endogenous variables (i.e., political orientation and campaign funds of the mayor) at the FPM thresholds. Each cell reports the estimated coefficient of a dummy equal to one (zero) if the party dummy is above (below) the closest threshold—controlling for a third-order polynomial in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each row heading. Robust standard errors clustered at the municipality level are in parentheses. The column "Overall effect" is obtained by estimations in the entire sample (i.e., thresholds 1–7); the columns "Thresholds 1-3" and "Thresholds 4-7" are obtained by estimations in the respective subsamples. Panel A considers the four most important political parties in Brazil (as well as a residual category for small parties): PT (*Partido dos Trabalhadores*); PSDB (*Partido Social-Democracia Brasileiro*); PMDB (*Partido do Movimento Democrático Brasileiro*); DEM (*Democratas*), which is the old PFL. Mayoral terms 2001–2005 and 2005–2009. Panel B considers the total amount of the declared campaign funds received by the mayor (in Brazilian *reais*, 2004 prices); the amount of funds received by firms; the mayor's own campaign funds; and other donations (by individuals, political parties, etc.). Note that in panel B we are considering only mayoral term 2005–2009, because there are no data available on campaign funds for municipal elections before 2004. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

A3 Examples of violation episodes from the audit reports

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.

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 called to the auction and their signatures had been falsified.

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.

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*.

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.

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.

A4 Robustness checks

Checking for crowding-out in other budget variables

To check whether the increase in FPM transfers completely crowd-out other types of revenues, leaving the budget size unchanged, we 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 with respect to those we use in the paper.

To verify that indeed our seven FPM thresholds correspond to relevant changes in municipal fiscal policy, we regress some observed budgetary items against actual FPM transfers, instrumented with theoretical FPM transfers as in the (fuzzy) RD specifications used in the paper for the main outcomes. By doing so, we test whether 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 A3, where all variables (including FPM transfers) are expressed in logs, so that the coefficients can be interpreted as elasticities. All budget 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 of the budget (about 40%). This suggests that local governments react to the additional transfers by reducing local taxes, as indeed shown in column 2 of Table A3. Local expenditures also go up with larger federal transfers (see the remaining columns), indicating that the reduction in local taxes does not entirely offset the extra federal revenues.[5]

Robustness checks on the corruption results

In Table A4, we check the robustness of the baseline results with respect to the way we construct our measures of political corruption. For both definitions of corruption—broad and narrow—we construct a discrete indicator capturing the number of violation episodes detected in the audit reports. For the number of broad and narrow corruption episodes, respectively, the average values are 5.17 and 1.18. Table A4 reports the IV estimates in the (fuzzy) RD specifications and shows that, around the population thresholds under investigation, FPM transfers have a positive and significant impact also on these additional measures of corruption.[6]

In Table A5, instead, we implement a series of robustness checks aimed at evaluating the sensitivity of our corruption results with respect to the functional form of the control function in population size, $G(P_i)$, included in the (fuzzy) RD specifications used in the paper for the

[5] Again, note that the source of the data on the budgetary items displayed in Table A3 is not the same as for the FPM transfers, so that these coefficients ought to be treated with caution.

[6] In a previous working-paper version of the paper, we show additional results (including graphical evidence) for these two measures of political corruption (see NBER working paper No. 15705).

main outcomes. In particular, 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 found in the baseline specification with a third-order polynomial (see Table 5 in the paper).

Robustness checks on the political selection results

In Table A6, we implement a series of robustness checks to evaluate the sensitivity of our political selection results with respect to the functional form of $G(P_i)$, as we have done for the corruption results in Table A5. Again, the results are strongly robust to any specification of the functional form of the control function in population size.

Finally, in Table A7, we check whether the political selection results depend on the fact that—in order to stay close to our theoretical framework—we restrict the sample to municipalities where the incumbent runs for reelection. This is not the case, because the same negative impact of FPM transfers on the educational level of politicians is detected when considering all mayoral candidates in Brazilian municipalities over the terms considered in the paper (sample A); only the opponents (of the incumbent who runs for reelection) with the highest number of votes (sample B); all political candidates in municipalities where the incumbent does not run for reelection and all opponents in municipalities where the incumbent reruns (sample C); and all the opponents of the political party of the incumbent mayor, irrespective of whether the incumbent reruns or not (sample D). On the whole, the results on the educational attainments of politicians are strongly robust to these alternative sample selection choices.[7]

[7]As discussed in the paper, mayors are the crucial players in Brazilian municipal governments and corruption episodes detected by the audit reports refer to the municipal government, not to the municipal legislature (i.e., City Council). However, the same negative effect of FPM transfers on the quality of politicians is detected when looking at the educational level of city councillors. In particular, the impact of transfers on the college dummy is not statistically different from zero, but the impact on years of schooling is positive and statistically significant: point estimates (standard errors) are as follows. Overall effect: -0.023 (0.010); thresholds 1–3: -0.034 (0.015); thresholds 4–7: -0.015 (0.010); threshold 1: -0.048 (0.028); threshold 2: -0.084 (0.024); threshold 3: -0.038 (0.018); threshold 4: -0.035 (0.016); threshold 5: -0.026 (0.015); threshold 6: -0.013 (0.014); threshold 7: -0.022 (0.017).

Table A3 – IV estimates: budget variables

	Total revenues	Local taxes	Total expenditure	Infrastructure expenditure	Personnel expenditure
Overall effect	0.527***	-0.623***	0.473***	0.618***	0.362***
	(0.107)	(0.238)	(0.109)	(0.237)	(0.114)
Thresholds 1–3	0.019***	-0.023**	0.018***	0.028***	0.012***
	(0.004)	(0.010)	(0.004)	(0.008)	(0.005)
Thresholds 4–7	0.004	-0.020***	0.002	0.012**	-0.004
	(0.003)	(0.007)	(0.003)	(0.006)	(0.003)
Threshold 1	0.037***	-0.029*	0.035***	0.052***	0.030***
	(0.007)	(0.016)	(0.007)	(0.016)	(0.008)
Threshold 2	0.024***	-0.033**	0.022***	0.033**	0.015**
	(0.007)	(0.014)	(0.007)	(0.013)	(0.008)
Threshold 3	0.020***	-0.013	0.019***	0.034***	0.012**
	(0.005)	(0.012)	(0.005)	(0.010)	(0.006)
Threshold 4	0.010**	-0.020*	0.008*	0.017*	-0.003
	(0.005)	(0.011)	(0.005)	(0.009)	(0.006)
Threshold 5	0.014**	-0.011	0.011*	0.034***	0.004
	(0.006)	(0.013)	(0.006)	(0.011)	(0.007)
Threshold 6	0.002	-0.022**	0.002	0.008	-0.001
	(0.004)	(0.011)	(0.004)	(0.008)	(0.004)
Threshold 7	0.002	-0.033**	0.000	0.005	-0.006
	(0.006)	(0.013)	(0.006)	(0.011)	(0.007)
Obs.	2,877	2,877	2,877	2,877	2,877

Notes. Effects of FPM transfers on other budget variables. Each cell reports the estimated coefficient of actual FPM transfers (instrumented with theoretical FPM transfers)—controlling for a third-order polynomial in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each column heading. Robust standard errors clustered at the municipality level are in parentheses. The coefficients in the row "Thresholds 1-7" are obtained by estimating the regression in the entire sample; the heterogeneity coefficients in the other rows are obtained by interacting the regression with population-interval dummies (from the midpoint below to the midpoint above FPM thresholds) for "Thresholds 1-3," "Thresholds 4-7," and each individual threshold, respectively. All variables are in logs; the original budget variables are expressed in Brazilian *reais* at 2000 prices. Mayoral terms 2001–2005 and 2005–2009. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A4 – IV estimates: additional corruption measures

	Number of broad corruption episodes	Number of narrow corruption episodes
Overall effect	0.311**	0.108***
	(0.125)	(0.034)
Thresholds 1–3	0.313**	0.145***
	(0.148)	(0.047)
Thresholds 4–7	0.383**	0.100**
	(0.159)	(0.041)
Threshold 1	0.036	0.060
	(0.227)	(0.067)
Threshold 2	0.327	0.119**
	(0.202)	(0.060)
Threshold 3	0.371*	0.149**
	(0.198)	(0.063)
Threshold 4	0.223	0.066
	(0.218)	(0.053)
Threshold 5	0.774**	0.141*
	(0.309)	(0.075)
Threshold 6	0.286*	0.095*
	(0.173)	(0.053)
Threshold 7	0.049	0.010
	(0.226)	(0.054)
Obs.	1,202	1,202

Notes. Effects of FPM transfers on additional corruption measures. Each cell reports the estimated coefficient of actual FPM transfers (instrumented with theoretical FPM transfers)—controlling for a third-order polynomial in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each column heading. Robust standard errors clustered at the municipality level are in parentheses. The coefficients in the row "Thresholds 1-7" are obtained by estimating the regression in the entire sample; the heterogeneity coefficients in the other rows are obtained by interacting the regression with population-interval dummies (from the midpoint below to the midpoint above FPM thresholds) for "Thresholds 1-3," "Thresholds 4-7," and each individual threshold, respectively. The additional measures of corruption are only available for the small sample (random audit reports): *number of broad corruption episodes* and *number of narrow corruption episodes* measure the number of episodes linked to general or serious violations, respectively. Mayoral terms 2001–2005 and 2005–2009. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A5 – Robustness checks: corruption measures

	Broad corruption	Narrow corruption	Broad fraction of the amount	Narrow fraction of the amount
		A. Spline (3^{rd}-order) polynomial		
Overall effect	0.013*	0.020**	0.355	0.237*
	(0.007)	(0.008)	(0.232)	(0.125)
Thresholds 1–3	0.013	0.023**	0.462**	0.271**
	(0.009)	(0.011)	(0.227)	(0.119)
Thresholds 4–7	0.012*	0.020**	0.320	0.226*
	(0.007)	(0.008)	(0.253)	(0.136)
		B. 2^{nd}-order polynomial		
Overall effect	0.013**	0.023***	0.115	0.210*
	(0.006)	(0.007)	(0.193)	(0.110)
Thresholds 1–3	0.017**	0.029***	0.365*	0.331***
	(0.008)	(0.010)	(0.199)	(0.117)
Thresholds 4–7	0.013*	0.023***	0.225	0.245*
	(0.007)	(0.008)	(0.231)	(0.130)
		C. Spline (2^{nd}-order) polynomial		
Overall effect	0.012*	0.025***	0.320	0.282**
	(0.007)	(0.008)	(0.216)	(0.121)
Thresholds 1–3	0.014*	0.030***	0.417**	0.336***
	(0.008)	(0.010)	(0.212)	(0.122)
Thresholds 4–7	0.012*	0.023***	0.277	0.257*
	(0.007)	(0.008)	(0.244)	(0.133)
		D. 4^{th}-order polynomial		
Overall effect	0.014**	0.022***	0.213	0.259**
	(0.006)	(0.008)	(0.211)	(0.121)
Thresholds 1–3	0.018**	0.031***	0.449**	0.338***
	(0.008)	(0.010)	(0.213)	(0.118)
Thresholds 4–7	0.014**	0.023***	0.273	0.257*
	(0.007)	(0.008)	(0.246)	(0.138)
		E. Spline (4^{th}-order) polynomial		
Overall effect	0.011	0.018**	0.235	0.209*
	(0.007)	(0.009)	(0.217)	(0.110)
Thresholds 1–3	0.013	0.020*	0.327	0.239**
	(0.009)	(0.011)	(0.223)	(0.113)
Thresholds 4–7	0.011	0.018**	0.206	0.200*
	(0.007)	(0.008)	(0.233)	(0.117)
Obs.	1,202	1,202	1,140	1,140

Notes. Effects of FPM transfers on corruption measures. Each cell reports the estimated coefficient of actual FPM transfers (instrumented with theoretical FPM transfers)—controlling for a control function $G(P_i)$ in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each column heading. Robust standard errors clustered at the municipality level are in parentheses. The control function $G(P_i)$ is adjusted as specified in the different panels of the table. Mayoral terms 2001–2005 and 2005–2009. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A6 – Robustness checks: opponents' education and election outcome

	College	Years of schooling	Incumbent reelection
		A. Spline (3rd-order) polynomial	
Overall effect	-0.009**	-0.078***	0.009
	(0.004)	(0.030)	(0.005)
Thresholds 1–3	-0.019***	-0.165***	0.013*
	(0.007)	(0.047)	(0.008)
Thresholds 4–7	-0.007*	-0.060**	0.008
	(0.004)	(0.029)	(0.005)
		B. 2nd-order polynomial	
Overall effect	-0.006*	-0.065***	0.012***
	(0.004)	(0.025)	(0.004)
Thresholds 1–3	-0.017***	-0.150***	0.014**
	(0.006)	(0.040)	(0.007)
Thresholds 4–7	-0.006*	-0.052**	0.009*
	(0.004)	(0.026)	(0.005)
		C. Spline (2nd-order) polynomial	
Overall effect	-0.009**	-0.073**	0.009*
	(0.004)	(0.029)	(0.005)
Thresholds 1–3	-0.016***	-0.146***	0.013*
	(0.006)	(0.043)	(0.007)
Thresholds 4–7	-0.006	-0.052*	0.008
	(0.004)	(0.027)	(0.005)
		D. 4th-order polynomial	
Overall effect	-0.009**	-0.078***	0.012***
	(0.004)	(0.026)	(0.005)
Thresholds 1–3	-0.018***	-0.158***	0.014**
	(0.006)	(0.042)	(0.007)
Thresholds 4–7	-0.007*	-0.059**	0.009*
	(0.004)	(0.027)	(0.005)
		E. Spline (4th-order) polynomial	
Overall effect	-0.007	-0.068**	0.009
	(0.005)	(0.031)	(0.006)
Thresholds 1–3	-0.015**	-0.146***	0.013
	(0.007)	(0.049)	(0.008)
Thresholds 4–7	-0.006	-0.057*	0.008
	(0.005)	(0.030)	(0.005)
Obs.	2,877	2,877	2,877

Notes. Effects of FPM transfers on the opponents' education and election outcome. Each cell reports the estimated coefficient of actual FPM transfers (instrumented with theoretical FPM transfers)—controlling for a control function $G(P_i)$ in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each column heading. Robust standard errors clustered at the municipality level are in parentheses. The control function $G(P_i)$ is adjusted as specified in the different panels of the table. Mayoral terms 2001–2005 and 2005–2009. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A7 – IV estimates: politicians' education in different samples

	College	Years of schooling	College	Years of schooling	College	Years of schooling	College	Years of schooling
	Sample A		*Sample B*		*Sample C*		*Sample D*	
Overall effect	-0.003	-0.059***	-0.011**	-0.089***	-0.004	-0.063***	-0.004	-0.058***
	(0.003)	(0.018)	(0.005)	(0.032)	(0.003)	(0.019)	(0.003)	(0.020)
Thresholds 1–3	-0.010**	-0.106***	-0.020***	-0.175***	-0.011***	-0.115***	-0.011***	-0.118***
	(0.004)	(0.028)	(0.007)	(0.0052)	(0.004)	(0.029)	(0.004)	(0.030)
Thresholds 4–7	-0.003	-0.045**	-0.011**	-0.076**	-0.004	-0.051***	-0.004	-0.049**
	(0.003)	(0.019)	(0.007)	(0.034)	(0.003)	(0.019)	(0.003)	(0.020)
Threshold 1	-0.005	-0.086*	-0.022*	-0.221**	-0.005	-0.089*	-0.004	-0.093*
	(0.007)	(0.052)	(0.012)	(0.096)	(0.007)	(0.053)	(0.007)	(0.055)
Threshold 2	-0.013**	-0.163***	-0.027**	-0.244***	-0.015**	-0.179***	-0.016**	-0.192***
	(0.006)	(0.043)	(0.011)	(0.080)	(0.006)	(0.045)	(0.007)	(0.047)
Threshold 3	-0.010**	-0.082***	-0.022**	-0.163***	-0.011**	-0.093***	-0.011**	-0.095***
	(0.005)	(0.032)	(0.009)	(0.059)	(0.005)	(0.032)	(0.005)	(0.033)
Threshold 4	-0.004	-0.053*	-0.011	-0.064	-0.005	-0.063**	-0.005	-0.059*
	(0.004)	(0.029)	(0.008)	(0.059)	(0.004)	(0.030)	(0.005)	(0.032)
Threshold 5	-0.001	-0.042	-0.012	-0.103*	-0.002	-0.046	-0.000	-0.046
	(0.004)	(0.030)	(0.008)	(0.053)	(0.005)	(0.030)	(0.005)	(0.031)
Threshold 6	-0.005	-0.064**	-0.012	-0.089**	-0.005	-0.066**	-0.008*	-0.080***
	(0.004)	(0.026)	(0.008)	(0.043)	(0.004)	(0.026)	(0.004)	(0.028)
Threshold 7	0.004	0.003	-0.010	-0.089*	0.003	-0.004	0.005	0.004
	(0.004)	(0.028)	(0.008)	(0.052)	(0.004)	(0.029)	(0.004)	(0.029)
Obs.	5,648	5,648	2,788	2,788	5,452	5,452	5,281	5,281

Notes. Effects of FPM transfers on the education of political candidates in different samples. Each cell reports the estimated coefficient of actual FPM transfers (instrumented with theoretical FPM transfers)—controlling for a third-order polynomial in normalized population size, term dummies, and macro-region dummies—in a regression where the dependent variable corresponds to each column heading. Robust standard errors clustered at the municipality level are in parentheses. The coefficients in the row "Overall effect" are obtained by estimating the regression in the entire sample (i.e., thresholds 1-7); the heterogeneity coefficients in the other rows are obtained by interacting the regression with population-interval dummies (from the midpoint below to the midpoint above FPM thresholds) for "Thresholds 1-3," "Thresholds 4-7," and each individual threshold, respectively. *Sample A* considers all political candidates in all municipalities. *Sample B* considers only the opponents (of the incumbent who runs for reelection) with the highest number of votes. *Sample C* considers all political candidates in municipalities where the incumbent does not run for reelection and all opponents in municipalities where the incumbent reruns. *Sample D* 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. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.