Electoral Effects of Fiscal Transfers: Quasi-Experimental Evidence from Local Executive Elections in Brazil, 1982-1988*

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Abstract

Do resource transfers from the central government to sub-national governments affect local electoral contests? Because politics usually matters for the allocation of public resources, empirical studies of this topic face a problem of simultaneous causality bias. This paper provides the first quasi-experimental evidence on electoral effects of resource transfers for local incumbent governments. Using regression-discontinuity analysis of a population-based revenue-sharing mechanism in Brazil, we estimate the transfers' effect on re-election probabilities in the 1988 mayoral elections. Our results suggest that increasing resource transfers by 30% increased the re-election probability of local non-aligned incumbent parties by about 35 percentage points. The same resource differential had no statistically significant effect on the re-election probability of the PDS, the party of the authoritarian regime. Evaluating hypotheses for these heterogeneous responses, we conclude that the PDS' profound unpopularity during the transition period made the transfers' electoral benefit unobservable.

Keywords: Intergovernmental transfers, redistributive politics, voting, regression discontinuity
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1 Introduction

Sub-national political entities around the world rely on intergovernmental transfers for many, if not most, of their resources (see, e.g., Rodden 2004; Ter-Minassian 1997). As such, these transfers are of great political interest, and there is a large literature studying the incentives behind their allocation (e.g. Arulampalam, et al. 2009; Ferejohn 1974; Levitt and Snyder 1995; Mayhew 1974; Pande 2003). Surprisingly, however, there is a relative dearth of literature on the political effects of these transfers. As Díaz-Cayeros and his coauthors (2007: 14) note, "Theories of distributive politics are premised on the assumption that voters react by supporting parties that deliver benefits. Yet political scientists and economists seldom assess the validity of this claim."[1]

Assessing the validity of the claim is actually not a straightforward task. The fact that there are usually political forces driving the allocation of fiscal transfers leads to a problem of simultaneous causality bias. For example, if regions that receive higher transfers are observed to vote more for the politicians who gave the transfers, it may be because the resources were funneled to those regions precisely because of their loyalty. Endogeneity is therefore a major hurdle that must be overcome in this line of research.

The key contribution of this paper is to provide the first quasi-experimental evidence on the political effects of fiscal transfers for local incumbent (grantee) governments. We analyze the effect of transfers in the Brazilian municipal executive elections of 1988, the first elections in which all municipalities chose their mayor after decades of restrictions on electoral competition under the authoritarian regime. In Brazil during the 1980s (and continuing to the present day), a substantial part of national tax revenue was distributed to local governments strictly on the basis of population, via a formula based on thresholds. That is, if a municipality's population was over the first population threshold, it received additional resources, over the second threshold a higher amount, and so forth. We present evidence below that, perhaps surprisingly, over the 1980s the transfers were actually allocated in this fashion, with no apparent political interference.[2] Because of the nature of

[1] An exception is Remmer and Gélineau (2003).

[2] See Litschig (2008b) for evidence that over the 1990s the transfer mechanism was manipulated to

the formula (i.e. the threshold method), there are discontinuities in the per capita receipts of counties whose populations are close to the thresholds. Our regression discontinuity design (RD) exploits these differences to analyze effects on re-election probabilities in the 1988 mayoral elections.

The estimation results suggest that increasing resource transfers by 30% increased the re-election probability of local non-aligned (opposition) incumbent parties by about 35 percentage points. The same resource differential had no statistically significant effect on the re-election probability of the PDS, the party of the authoritarian regime. While there is no *a priori* theoretical reason to expect this type of heterogeneous electoral response, we believe that the most convincing explanation lies in the voters' rejection of the PDS at all levels of government during the transition to democracy. That is, the PDS' unpopularity shock dwarfed the beneficial resource effects by an order of magnitude. We show that two other hypotheses for the differential effect receive no support: that the splintering of the PDS made voters unable to attribute its benefits, and that opposition parties delivered higher levels of local public goods with the extra resources than the PDS did.

Our paper most directly builds on a small but growing literature that attempts to overcome endogeneity issues in the empirical analysis of political resource effects. Levitt and Snyder (1997), for example, studied the political effects of U.S. federal spending by instrumenting for federal spending in one district with federal spending in neighboring districts. They found that non-transfer federal spending benefited incumbents, but transfers did not. The same instrumental variable approach has also been used by Solé-Ollé and Sorribas-Navarro (2008) for Spain. The authors test whether intergovernmental grants allocated to aligned local governments buy more political support than grants allocated to local government controlled by opposition parties and find evidence consistent with this hypothesis, suggesting that the grantee reaps as much political credit from intergovernmental grants as the grantor. Other scholars have used the random assignment of certain spending programs to identify electoral effects. For example, several works have used

benefit aligned (right-wing) national deputies in electorally fragmented local political systems as well as aligned local executives.

the random selection inherent in the Mexican anti-poverty program *Progresa* to study its political effects. They have come to different conclusions: while Green (2005) finds no effect, de la O (2006) and Díaz Cayeros and his co-authors (2007) find that *Progresa* benefited incumbents.[3] Most recently, Manacorda, et al. (2009) have used a regression discontinuity approach to study the political effects of a Uruguayan anti-poverty program, and they found that beneficiary households were significantly more likely to vote for the incumbent government.

Almost all of these works focus on political resource effects for the grantor government—that is, the benefit to the central government of granting these transfers. However, as shown by Arulampalam and co-authors (2009), when public services are funded by the central government but implemented by some lower level of government, it is reasonable to expect that the lower level incumbents derive some political benefit as well. This is especially true when the transfers provide general budget support (as in our case) rather than finance a specific project for which the central government can claim credit. There has been almost no attention paid to political resource effects on local incumbent governments (the exception we know of is Solé-Ollé and Sorribas-Navarro 2008), and they are the focus of this paper.

In addition, the paper builds on a recent literature exploring the rationality (or lack thereof) of voters. Because extra resources in the scenario we examine were released by crossing a population threshold, presumably independent of any politician's effort, one might expect that voters would not reward politicians for benefits received as a result. However, voters are unlikely to be perfectly informed about the source of funds.[4] And several recent studies have demonstrated that voters do in fact reward (and punish) politicians for events well outside their control. Wolfers (2007), for example, finds that U.S. governors in oil-producing states are more likely to be re-elected following a rise in oil prices (also see Goldberg, et al. 2008). Similarly, Achen and Bartels (2004) have found

[3] While the focus here is on works that have attempted to deal with the endogeneity of transfers, there are several other important works on this topic in economics and political science.

[4] Moreover, even perfectly informed voters might still reward politicians for actually using the extra funds to improve public services and/or provide clientelistic benefits rather than pocketing everything for themselves.

that voters punish incumbents following natural disasters.

The paper proceeds as follows. Section 2 provides background on the political context of the 1988 Brazilian elections, gives a description of the revenue sharing mechanism we examine, and details our conceptual framework. In section 3 we discuss the key identifying assumption for a causal interpretation of our estimates, which is that municipalities had only imprecise control over the number of local residents. The third section also evaluates the internal and external validity of our study. Section 4 describes our data, and section 5 discusses the estimation approach. Section 6 presents the principal results of our analysis, indicating that there were heterogeneous effects of transfers on the re-election prospects of incumbent parties at the local level depending on whether they were aligned with the central government or not. A final section concludes with a discussion of extensions.

2 Background and conceptual framework

2.1 The Brazilian political and economic context

The 1988 local elections in Brazil were held in a period of great political change in the country. Most importantly, the elections were one of the culminating events of Brazil's extended transition to democracy. The military had ruled the country since 1964, and over the course of the 1980s had gradually loosened and lost control. Though the military prevented voters from electing mayors in important cities, meaningful local elections had been held in small and medium cities in 1982. And in 1985, the party of the dictatorship, the PDS, had lost the presidency to the major opposition party PMDB (though this was not on the basis of a popular election). The 1988 elections would be the first in over two decades in which all municipalities elected their own mayors.

Change at the national level had been reflected at the local level. As Table 1 shows, the PDS had won in almost two-thirds of the municipalities in 1982, to go along with its control of the central government. However, when mayoral elections were held in the state capitals in 1985, the party essentially disappeared from major urban areas, the result of a major party split (in which the PFL was formed) and widespread rejection of conservative parties. Smith (1986) reports that the conservative PDS, PFL, and PTB only won 28.2 percent of the vote in the 1985 mayoral elections. That same year saw the negotiated ascendance to power in the central government of the PMDB, whose popularity was short-lived. By the election of 1988, inflation ran at 1000 percent and the government was seen to be widely corrupt (Shidlo 1998). The result was widespread dissatisfaction with the political system, and an explosion of new parties seeking disgruntled voters. While the period of the dictatorship had seen electoral "competition" limited to two parties, voters in 1988 chose from 31 political parties—nineteen of whom were winners somewhere in the country—to elect mayors in more than 4000 municipalities (Ames 1994: 97).

The 1988 local executive elections represent a difficult environment in which to find an electoral effect of resource transfers. To begin with, the democratic regime in Brazil was still being consolidated, and many voters had never participated in elections before. The newness of elections in the country may have affected how informed voters were, and how familiar they were with democratic practices. For example, a poll regarding the presidential election the following year (1989) indicated that 70 percent of voters were voting for their president for the first time, with about the same percentage having low levels of education (Moisés 1993).

In addition, because of term limit rules, no incumbent mayors could be individually re-elected. Citizens could re-elect the party of the mayor, but as just noted, satisfaction with parties was particularly low, and in fact, party identification in Brazil faces particularly strong challenges in general (Kinzo 1993; Shidlo 1998). The Constitution stipulates that parties must be organized nationally, a difficult prospect in a country as diverse as Brazil. As Moisés (1993: 577) puts it, "Brazilians don't vote for parties, they vote for people."

In this context, why might these transfers have mattered? Simply put, intergovernmental transfers in Brazil were essential to the functioning of municipal governments. As Table 2 shows, municipalities have never collected much in the way of tax revenues despite taking on more and more responsibilities, such as elementary education, preventive health care, public housing, and local public transportation. In fact, in 2001, federal and state government transfers made up 80% of local revenue for counties with a population below 50,000.[5] The most important among these transfers is the federal Fundo de Participacao dos Municípios (FPM), a largely unconditional revenue sharing grant funded by federal income taxes and industrial products taxes.[6] This grant accounted for about 50% of the total revenue of the municipalities in our analysis.[7]

2.2 Mechanics of revenue sharing

Our identification strategy for estimating the electoral response to fiscal transfers is to exploit local variation in FPM resources in a regression-discontinuity (RD) design. The critical feature of the FPM revenue-sharing mechanism for the purposes of our analysis is Decree 1881/81, which stipulates that transfer amounts depend on county population in a discontinuous fashion. More specifically, based on county population estimates, pop^e, counties are assigned a coefficient $c = c(pop^e)$, where c(.) is the step function shown in Table 3. For counties with up to 10,188 inhabitants, the coefficient is 0.6; from 10,189 to 13,584 inhabitants, the coefficient is 0.8; and so forth. The coefficient $c(pop^e)$ determines the share of total FPM resources, rev_t, which are distributed to county c in year t according to the following formula:

$$FPM_{ct} = \frac{c(pop_c^e)}{\sum c_c^e} rev_t$$

This equation makes it clear that local population estimates are the only determinant of cross-county variation in FPM funding. Exact county population estimates are only available for census years or years when a national population count is conducted. In our study period, which spans the two local executive elections in 1982 and 1988, transfers were allocated based on 1980 census population from 1982 until 1985. From 1986 to 1988

[5] Overall, in 2002, local governments were in charge of 16.6 % of total public revenue (Banco Nacional de Desenvolvimento Econômico e Social 2003).

[6] The one condition over our study period is that municipalities must spend 20 percent of the transfers on education and culture (Decree No. 83.556, Art. 5, I). This constraint is usually considered non-binding, in that municipalities typically spend about 20% of their *total* revenue on education anyways. It is also not clear how this provision was enforced in practice since there is no clear definition of education and cultural expenditures and audits were weak or non-existent.

[7] Its importance is highlighted by Litschig (2008a), who shows that municipalities who received more of these transfers increased spending on public service provision which led to higher schooling and literacy rates.

the transfers were based on extrapolations produced by the national statistical agency, IBGE.[8]

While this design of the revenue sharing mechanism is fortunate for our scientific purposes, it also represents somewhat of a puzzle: why would politicians allocate resources based on objective criteria, such as population, rather than use discretion? The answer to this question lies in the political agenda of the military dictatorship which came to power in 1964. As detailed by Hagopian (1996), one of the major objectives of the military was to wrest control over resources from the traditional political elite and at the same time to depoliticize public service provision. The creation of the revenue sharing fund for the municipalities based on an objective criterion of need, population, was part of this greater agenda. It reflected an attempt to break with the clientelistic practice of the traditional elite, who manipulated public resources to the benefit of narrowly defined constituencies.

The reason for allocating resources by brackets, i.e. as a step function of population as in Decree 1881/81, represents a further puzzle. One explanation could be that compared to a linear schedule, for example, the bracket design mutes incentives for local officials at the interior of the bracket to tinker with their population figures or contest the accuracy of the estimates in order to get more transfers. A related question is where the exact cutoffs come from (why 10188, 13584, 16980, etc.)? While we were unable to trace the origin of these cutoffs precisely, we know roughly how they came about. The initial legislation from 1967 created cutoffs at multiples of 2000 and stipulated that these should be updated proportionally with population growth in Brazil.[9] The cutoffs were thus presumably updated twice, once with the census of 1970 and then with the census of 1980, which explains the "odd" numbers. It is also noteworthy that the thresholds were still equidistant from one another in 1981, the distance being 6792 for the first 7 cutoffs (except for the second cutoff, which lies exactly halfway in between the first and third cutoffs).

[8] The methodology used by IBGE ensures that population estimates are consistent between counties, states, and the updated population estimate for the country as a whole (Instituto Brasileiro de Geografia e Estatistica 2002).

[9] Supplementary Law No. 35, 1967, Art. 1, Paragraphs 2 and 4.

Perhaps most important for our analysis is that over the period we study the transfers were in fact allocated as stipulated by decree 1881/81. Figure 1 uses data from 1982 until 1985 to show that FPM transfers jumped by about 10,000,000 Reais (2005 prices) at each threshold over this period. This transfer differential corresponds to about 25% of annual GDP in rural areas of the country and about 14% of annual GDP in urban areas for counties with a population in the range 8500 to 18,700.[10] In per capita terms, FPM transfers jump by about 30% at the first threshold and decline monotonically for the following cutoffs since the absolute increase in FPM transfers is constant while population is higher at each subsequent threshold. As shown in Litschig (2008a), the FPM differential amounts to jumps in total revenue of about 15%, a magnitude consistent with the fact that FPM transfers represent about 50% of total revenue for the relatively small counties considered here.

The discontinuities lessened after 1985, when county population estimates were updated and some counties changed groups because of falls or, more often, rises in their population relative to 1980. However, as Figure 2 illustrates, the gap between the marginal (to the threshold) treatment and comparison groups remained substantial. Starting in 1988, official population estimates were updated annually, and more counties were reclassified in 1989 and 1990.[11] By 1991, counties that were just below a threshold in 1982 received the same amount of transfers as those counties that were just above the threshold in 1982.

2.3 Conceptual framework

The goal of this paper is to understand the effect that intergovernmental transfers had on the 1988 local executive elections. Given that variation in resources occurs at the level of the total local public budget, effects on electoral outcomes may arise through a variety of channels. These can broadly be divided into the public provision of relatively

[10] During 2005, the average Real/$ exchange rate was 2.4348. Observations that appear below the vertical lines are due to measurement error because transfer data in this figure (and in our data) are self-reported by municipalities, rather than based on administrative records of the central government treasury (which are not available for the period considered).

[11] Supplementary Law n^{o} 59/1988.

non-excludable services, or public goods for short, and private goods (such as government jobs and other means of clientelism).[12] Assume that electoral outcomes E depend on the levels of both public goods B and private goods V provided by the government in the local community. Both B and V depend on the overall level of local government resources R, of which FPM transfers F represent an important share:

$$E = E(B(R(F)), V(R(F)))$$

The effect estimated here can be thought of as E_F, the total derivative of electoral outcomes with respect to financial resource transfers. That is, the effect captures the influences of multiple channels through which resources pass to affect political outcomes. In particular, E_F incorporates R_F, the marginal propensity to spend transfers received, and B_F and V_F, the marginal propensities to spend on public and clientelistic goods, respectively.[13] While our approach does not allow us to disentangle the effect of public goods provision on electoral outcomes E_B from the effect of patronage E_V, we present evidence below suggesting that in Brazil over the 1980s at least part of the electoral effect of resource transfers worked through the public goods provision channel.

3 Identification and internal validity checks

3.1 Identification

The basic intuition behind the RD approach is that, in the absence of program manipulation, observations to the left of the treatment-determining population threshold should provide valid counterfactual outcomes for counties on the other side of the cutoff (which received additional resources). More formally, let Y denote the observed electoral outcome in a county (party re-election), α an intercept, τ the causal parameter of interest,

[12] Our use of the term public goods differs from the conventional definition as goods that are non-excludable and non-rival. Many publicly provided goods such as education and healthcare are actually private goods, i.e. excludable and rival. From a clientelistic perspective what matters most is that once a private good is publicly provided it becomes less excludable, i.e. the government loses the ability to benefit some and not others. In contrast, private or clientelistic goods can be allocated entirely at the government's discretion.

[13] See Litschig (2008a) for an analysis of R_F.

D the indicator function for treatment (additional resources), pop county population, c a particular cutoff, $f(pop)$ a polynomial function of population, and u an error term. The regression model is as follows:

$$
\begin{aligned}
Y &= \alpha + \tau D + f(pop) + u \\
D &= 1[pop > c]
\end{aligned}
$$

If the potential regression functions $E[Y|D = 1, pop_{cs}]$ and $E[Y|D = 0, pop_{cs}]$ are both continuous in population, then the difference in conditional expectations identifies the treatment effect at the threshold:

$$
\lim_{pop \downarrow c} E[Y|pop] - \lim_{pop \uparrow c} E[Y|pop] = \tau
$$

As shown in Lee (2008), sufficient for the continuity of the regression functions above is the assumption that individual densities of population are smooth. This assumption thus allows for mayors or other agents in the municipality to have some control over their particular value of population. Lee shows that as long as this control is imprecise, treatment status (extra transfers) will be randomized around the cutoff.[14]

Although local elites in Brazil clearly had an incentive to manipulate population figures in order to get more resources from the federal government, the assumption of imprecise control over population seems plausible in our context. Moreover, even if local elites had perfect control over the number of residents in their county, they were unlikely to even know the exact locations of the new thresholds prior to 1980. This is because, as discussed above, the legislation specified that thresholds would be updated in accordance with population growth in the country as a whole, pursuant to the release of the 1980 census results.

Still, one might worry that leaders in the central government had incentives to alter

[14] Another potential concern is that other government policies are also related to the cutoffs specified in Decree no. 1881/81, which would confound estimating the effects of the transfers only. However, to our knowledge this is not the case.

the threshold to benefit local leaders they favored. It is unlikely, however, that this kind of manipulation would have occurred. For example, in order for leaders at the central government level to have used the thresholds to benefit leaders of their party, there would have had to be places along the population distribution where PDS municipalities had systematically higher population than other municipalities. It is noteworthy in this context that the thresholds are equidistant from one another, making it even less likely that the thresholds were set in order to benefit local leaders of a certain type.

3.2 Validity checks

In order to confirm that extensive manipulation did not take place, we check for any evidence of sorting, notably discontinuous population distributions. Figure 3 and Figure 4 plot histograms for the full support of 1982 population and the left-hand side of the distribution, respectively. Visual inspection reveals no glaring discontinuities for the majority of thresholds, except for a somewhat curious bump to the right of the third threshold. Similarly, as Litschig (2008a) shows, neither visual inspection nor statistical evidence reveals discontinuities in the 1981 values of county total revenue and current transfers (which include as main components FPM and state value-added tax transfers). In other words, there is no evidence that treatment group counties were systematically different in terms of overall resources from counties in the marginal comparison group prior to 1981.

Section 5 provides additional evidence regarding the internal validity of our approach by showing that the estimated electoral effects are robust to the inclusion of relevant pre-treatment covariates, including county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 15 years old, infant mortality, enrollment of 7 to 14 year olds, and percent of population living in urban areas. Inclusion of these potentially confounding factors does not significantly alter treatment effect estimates in the discontinuity sample, suggesting that none of these variables are strongly correlated with both treatment status and outcomes. Consistent with this result is that we find no systematic evidence of statistically or economically significant differences when we test for discontinuities in these variables directly (results not shown).

As with any RD analysis, the treatment effects presented in this paper apply only to counties with population levels at the respective cutoffs.[15] However, because results are quantitatively similar across the first three thresholds, as shown in detail below, it seems likely that the resource effects presented here generalize at least to the subpopulation of municipalities in the approximate population range 8500-18700, which represents about 30% of Brazilian municipalities at the time.

4 Data

Our analysis draws on multiple sources of information. Population estimates determining transfer amounts were taken from successive reports issued by the Federal Court of Accounts (TCU). Data on local public budgets, including FPM transfers, are self-reported by county officials and compiled into reports by the Secretariat of Economics and Finance inside the federal Ministry of Finance. The data from these reports were entered into spreadsheets using independent double-entry processing. All public finance data were converted into 2005 currency units using the GDP deflator for Brazil. Electoral data for the municipal executive 1982 and 1988 elections are from the Supreme Electoral Tribunal (TSE).

As pre-treatment covariates we include the 1980 levels of county income per capita, average years of schooling for individuals 25 years and older, the poverty headcount ratio, illiterate percentage of people over 15 years old, the infant mortality rate, the education enrollment rate of 7- to 14-year-olds, and the percent of the municipal population living in urban areas. Data on these county characteristics are based on a random sample of 25% of each county's population taken by the census and have been calculated by

[15] See Lee (2008) for an alternative interpretation of the treatment effect identified in an RD analysis as a weighted average of individual treatment effects where the weights reflect the ex ante probability that an individual´s score is realized close to the cutoff.

the national statistical agency (only a shorter census survey was administered to 100% of the population). Table 4 shows descriptive statistics for the variables used in the statistical analysis, as well as other information regarding revenue and expenditures in the municipalities. The numbers show that FPM transfers are the most important source of revenue for the relatively small local governments considered here, amounting to about 52% on average and 58% in rural areas.

5 Estimation approach

Following Hahn, Todd and Van der Klaauw (2001), Porter (2003) and Imbens and Lemieux (2008), our main estimation approach is to use local linear regression in samples around the discontinuity, which essentially allows for (different) slopes of the regression function in the neighborhood of the cutoff. This is particularly important in the present application because per capita transfers are declining as population approaches the threshold from below, and again declining after the threshold. Assuming that a similar pattern characterizes outcomes as a function of population, a simple comparison of means for counties above and below the cutoff would provide downward biased estimates of the treatment effect.

Because there are relatively few observations in a local neighborhood of each threshold, our RD analysis also makes use of observations further away from the thresholds. The disadvantage of this approach is that the specification of the function $f(pop)$, which determines the slopes and curvature of the regression line, becomes particularly important. To ensure that our findings are not driven by functional form assumptions, we present most estimation results from linear specifications in the discontinuity samples and then supplement them with flexible quartic polynomial specifications using an extended population support.

In the analysis that follows, we focus particularly on the first three population thresholds (10188, 13584, and 16980). The reason for pooling only across the first three thresholds is that for larger counties, the increase in FPM transfers at subsequent cutoffs is too small to affect their overall budget, and hence there is no "first stage" in terms of overall resources available for the county as further detailed in Litschig (2008a). While we present the results for the three thresholds individually, we also pool the counties from around these thresholds, in order to gain statistical power. For the pooled analysis, we need to make the groups comparable in terms of their distance from the threshold. Therefore we rescale population to equal 0 at the respective thresholds within each of the first three segments, and then use the scaled variable, x_{cs} (county c in state s) for estimation purposes:

$$
\begin{aligned}
x_{cs} &= pop_{cs} - 10188 \ if \ 7500 < pop_{cs} \leq 11800 \\
& pop_{cs} - 13564 \ if \ 11800 < pop_{cs} \leq 15100 \\
& pop_{cs} - 16980 \ if \ 15100 < pop_{cs} \leq 23772
\end{aligned}
$$

Letting s_k denote the 4 integers, 7500, 11800, 15100, 23772 that bound and partition the population support into 3 segments, z_{cs} a set of pre-treatment covariates, c_s a fixed effect for each state, and u_{cs} an error term for each county, the pooled linear specification is as follows:

$$
\begin{aligned}
Y_{cs} &= \tau 1[x_{cs} > 0] + \alpha_1 x_{cs} + \alpha_2 x_{cs} 1[x_{cs} > 0] \\
&+ \alpha_3 x_{cs} 1[s_1 < pop_{cs} \leq s_2] + \alpha_4 x_{cs} 1[s_1 < pop_{cs} \leq s_2] 1[x_{cs} > 0] \\
&+ \alpha_5 x_{cs} 1[s_2 < pop_{cs} \leq s_3] + \alpha_6 x_{cs} 1[s_2 < pop_{cs} \leq s_3] 1[x_{cs} > 0] \\
&+ \sum_{k=1}^{3} \beta_k 1[s_{k-1} < pop_{cs} \leq s_k] + \mathbf{z}_{cs} + a_s + u_{cs}
\end{aligned}
$$

Essentially this equation allows for six different slopes, one each on either side of the three thresholds. Under the continuity assumption above, the pooled treatment effect is given by $\lim_{\Delta \downarrow 0} E[Y \mid x = \Delta] - E[Y \mid x = 0] = \tau$. We also use the above specification to estimate individual effects for the first three thresholds and test the null hypothesis of common effects, which we fail to reject (results not shown). We follow the suggestions by Imbens and Lemieux (2008) and use a rectangular kernel and standard least square theory for inference. Because our dependent variable is dichotomous, we also ensure the results are robust to estimation with probit models. Both the pooled treatment effect and effects at individual thresholds are estimated using observations within successively larger neighborhoods around the threshold in order to assess the robustness of the results.

6 Estimation results

Table 5 presents the initial set of results of the effects of transfers on the re-election probabilities of incumbent parties. While the coefficients are mostly positive, in none of the specifications are they significant. This is true both in the regressions run for each threshold and in the pooled regressions. Figure 5 presents the results graphically. Each dot represents the average residual from a regression of re-election on state and segment dummies within a bin of one percentage point of cutoff population. For example, the first dot to the left of zero represents the residual re-election rate for all counties within one percentage point (in terms of population) of one of the first three population thresholds. As is clear from the figure, there is little evidence of a discontinuity at the threshold. It is worth remembering at this point the discussion in Section 2, particularly that individuals themselves could not be re-elected and that party identification was particularly low in Brazil at the time. Given these circumstances, this result—no significant effect of subnational transfers on the probability that an incumbent party is re-elected—is perhaps not surprising.

However, given that the major difference between 1982 and 1988 was the national stature of the PDS, it is worthwhile to investigate whether there were differing effects on incumbents of that party as opposed to opposition parties. The results, presented in Table 6, are striking. While the sub-sample of PDS-governed municipalities yields results similar to those in Table 5, in the opposition municipalities the transfers have a significantly positive effect across specifications. Inclusion of pre-treatment covariates does not alter the point estimates much but substantively reduces standard errors. As Table 7 shows, results from the probit model are substantively similar to those from the linear probability model. They suggest that increasing resource transfers by 30% increased the re-election probability of local non-aligned (opposition) incumbent parties by about 35 percentage points. The same resource differential had no statistically significant effect on the re-election probability of the PDS, the party of the authoritarian regime. The differences in point estimates across samples can be seen graphically: Figure 6 shows little discontinuity in the results for municipalities that voted for the PDS in 1982, while Figure 7 demonstrates a sharp discontinuity for the opposition municipalities.

We consider two hypotheses for the differential effect. The first is the possibility that the disintegration of the PDS —as shown in Table 1—made voters unable to attribute the benefits they had received from the PDS. If this were true, voters might reasonably attribute those benefits to one of the splinter PDS parties. We first analyze a conception of party "re-election", in which we count municipalities that voted in 1988 for the residual PDS *or* the PFL, the major party that formed as a result of the PDS split. The results are shown in Table 8, and there are no substantive differences with panels B of Tables 6 and 7. Next, we consider a re-election to be when *any* of the right-wing parties were elected in 1988 in a municipality that had been PDS in 1982. These results (not shown) again yield no significant effect. In sum, there is no evidence that the federal transfers benefited the PDS incumbents in any way.

The second hypothesis we consider is that the transfers had differing effects on public services in PDS and opposition municipalities. To evaluate this hypothesis, we examine the effect of extra transfers on both literacy and education outcomes. As Table 9 shows, we are unable to find systematic differences between PDS and opposition municipalities that could account for the observed heterogeneous electoral response, since municipalities that received additional transfers perform better in literacy outcomes in both PDS and opposition municipalities. And as Table 10 shows, PDS municipalities are if anything better than opposition municipalities at turning transfers into schooling outcomes (it should be noted, however, that opposition municipalities in general had higher levels of education to begin with). Despite utilizing the transfers in relatively similar ways (at least with regard to outcomes), PDS municipalities were thus unable to benefit electorally from the transfers.

Our interpretation is that these results reflect the uniqueness of the 1988 election in Brazil. Specifically, our best guess is that the differential effect is due to a large-scale voter rejection of the PDS, the party of the authoritarian regime, which made the PDS's chances of winning so low that any beneficial impact of the additional transfers was unobservable. The fact that the transfers benefited the re-election prospects of all other parties—even in the context of weak party identification, a new democratic regime, and term limits on incumbents preventing their re-election—suggests to us that such transfers have a positive effect on re-election prospects in most cases.

7 Conclusion

This paper is one of the first attempts to study the effects of sub-national transfers on the electoral fortunes of receiving (grantee) governments, as opposed to the central (grantor) government. As mentioned at the beginning of the paper, such transfers make up a critical amount of the revenue of local governments around the world, so the lack of scholarly attention to their political effects at the sub-national level is striking. To the extent that this lack of attention is not caused by data issues or simply that scholars take the political effects for granted, it may exist because studying the effects presents particularly difficult problems of simultaneous causality bias. To overcome these problems, this paper presents the first quasi-experimental evidence on electoral effects of fiscal transfers for grantee governments.

In particular, we exploit discontinuities in sub-national transfers in Brazil to estimate their effect on municipal electoral outcomes in 1988. Using a regression discontinuity approach, we are unable to find this relationship in our full sample using a variety of different operationalizations. However, taking note of the uniqueness of the 1988 Brazilian election—the first election in which all municipalities chose their mayor after decades of an authoritarian regime—we also examine whether the transfers had differing effects on municipalities run by the PDS, the dictatorship party that had splintered prior to the election, as opposed to other parties. In fact, they did. While we are never able to find significant political effects of the transfers in PDS municipalities, we consistently find significant effects in opposition municipalities. This is true despite the fact that we can find no important differences in the effects of transfers on public service outcomes in PDS and opposition municipalities. We interpret these results as evidence of large-scale voter rejection of the PDS, which made the beneficial effects of the transfers unobservable. That we find positive results for the other parties in such a challenging environment—including an incipient and fractured party system and term limits preventing incumbent mayors from running for re-election—suggests to us that these transfers likely improve re-election prospects in other settings.

The set of results presented here indicates the importance of further research on this topic. We believe the result that the transfers bolstered most incumbent parties in such a challenging political scenario (in many ways, a "least likely" scenario) suggests that such transfers strengthen all municipal incumbents generally. However, given the exceptional circumstances of the Brazilian 1988 election, this hypothesis is one that should be examined in other contexts, both in Brazil and in other countries.

An additional area of research implied by our findings concerns a particularly important group of democratic elections: first democratic elections. As democratic transitions have occurred around the world in the past several decades, several authoritarian parties have managed to transform themselves into democratically viable parties. This did not occur in Brazil, despite our evidence that PDS municipalities were no worse than opposition municipalities at turning transfers into public goods. More research is needed to understand why the PDS did not succeed electorally, while parties like the PRI in Mexico and the KMT in Taiwan did (Friedman and Wong 2008). The results here indicate that we should not take for granted that municipalities that have benefited from the authoritarian party will necessarily support it in a first democratic election. More work is needed to understand the reasoning in voters' minds behind this (lack of a) relationship.

Finally, this study of the political effects of federal transfers at the municipal level indicates a need to re-visit the question of the effects of these transfers at the national level. As mentioned in the introduction, several studies have now found that federal transfers yield political benefits to those in power in the central government. However, the overall effect of these transfers may be considerably more complex for a party in power if the transfers simultaneously yield political benefit to incumbents at sub-national levels of government. Parsing out these overall effects, and understanding how parties in power manage the trade-offs, is a particularly interesting area for future research (see Arulampalam, et al. 2009).

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Table 1: Mayor party affiliation in 1982 and 1988

Party	Party-type	1982 N	1982 %	1988 N	1988 %
PDS	Right	2,537	64.5	444	10.4
PFL	Right			1,054	24.7
PTB	Right	7	0.2	333	7.8
PMB	Right			58	1.4
PL	Right			237	5.5
PDC	Right			231	5.4
PRN	Right			4	0.1
PSC	Right			26	0.6
PRTB	Right			8	0.2
PSD	Right			2	0.1
PMDB	Left	1,366	34.7	1,593	37.3
PDT	Left	20	0.5	192	4.5
PT	Left	2	0.1	38	0.9
PSB	Left			37	0.9
PSDB	Left			18	0.4
PSTU	Left			1	0.0
Total		3,936	100	4,276	100.0

Table 2: Gross tax revenues collected by jurisdiction, 1960-1988

	1960	1980	1988
Percent of GDP			
Total	17.4	24.6	22.5
Federal government	11.1	18.5	15.8
States	5.5	5.4	6.0
Municipalities	0.8	0.7	0.7
Percent of total revenues			
Total	100	100	100
Federal government	64	75.1	70.6
States	31.2	22	26.5
Municipalities	4.8	2.9	2.9

Table 3: Population brackets and coefficients for the FPM

Population bracket				*Coefficient*
up to	10,188			0.6
from	10,189	to	13,584	0.8
from	13,585	to	16,980	1
from	16,981	to	23,772	1.2
from	23,773	to	30,564	1.4
from	30,565	to	37,356	1.6
from	37,357	to	44,148	1.8
from	44,149	to	50,940	2
from	50,941	to	61,128	2.2
from	61,129	to	71,316	2.4
from	71,317	to	81,504	2.6
from	81,505	to	91,692	2.8
from	91,693	to	101,880	3
from	101,881	to	115,464	3.2
from	115,465	to	129,048	3.4
from	129,049	to	142,632	3.6
from	142,633	to	156,216	3.8
above	156,216			4

Table 4: Descriptive Statistics

	Population range					
	7,500 - 44,148	8,500 - 18,700				
Sample	Full	Full	Incumbent	Opposition	Rural	Urban
Observations	2306	1248	844	358	624	624
1980 county characteristics (IBGE)						
Avg. years of schooling (25 years and older)	2.0	1.9	1.7	2.4	1.5	2.3
Percentage of residents living in urban areas (%)	30.0	27.9	25.8	32.8	14.8	41.7
Net enrollment rate of 7 to 14 year olds (%)	55.6	55.5	51.4	64.5	48.9	62.1
Illiteracy, 15 years and older (%)	39.0	39.1	43.5	30.0	44.4	33.7
Poverty headcount ratio (national poverty line, %)	58.6	59.3	64.8	47.4	67.9	50.7
Income per capita (% of minimum salary in 1991)	77.5	75.2	65.4	96.6	58.6	91.9
Infant mortality (per 1000 life births)	88.9	88.5	97.7	70.0	96	80.7
GDP ('000) 2005 Reais (IPEA)	93,101	55,056	46,005	70,619	40,149	70,084
1982 Financial data (Ministry of Finance)						
Total county revenue ('000) 2005 Reais	31,188	22,672	20,557	26,187	18,601	26,525
Total county revenue 1982/GDP 1980 (%)	48.6	51.6	56.3	42.2	57.5	46.0
FPM transfers/total revenue (%)	48.0	49.7	54.2	41.1	56.4	42.3
Own revenue/total revenue (%)	5.9	5.1	3.9	7.4	2.6	7.5
Other revenue/total revenue (%)	46.9	45.9	42.8	52.0	41.9	49.7
Administrative spending/total spending (%)	22.3	22.3	21.9	23.0	21.8	22.9
Education spending/total spending (%)	20.9	21.2	22.1	19.2	22.3	20.0
Housing spending/total spending (%)	19.5	17.9	18.9	16.2	15.9	20.2
Health spending/total spending (%)	9.9	10.4	11.6	7.9	11.1	9.6
Transportation spending/total spending (%)	20.9	21.8	20.0	26.0	23.2	20.2
Other spending/total spending (%)	8.5	8.5	8.2	9.3	8.2	8.6
1991 Real school resources (1991 school census)						
Number of municipal elementary schools	37.8	30.2	33.2	23.3	37.5	21.4
Primary school teacher-student ratio	0.054	0.056	0.054	0.061	0.054	0.059
1991 School outcomes (1991 census)						
Avg. years of completed schooling (19 to 28 olds)	4.6	4.5	4.2	5.3	4	5.1
Literacy rate (19 to 28 olds)	78.8	79.0	75.0	87.5	73.7	84.3
1988 Electoral outcomes (TSE)						
Re-election (party) (%)	43.7	42.5	11.4	45.2	44.9	40
Re-election (party, PFL88 as PDS88) (%)	56.5	56.9	41.4	45.2	59	54.7

Notes: Incumbent refers to municipalities run by PDS (or PFL after official party split in 1985) mayors from 1982 to 1988. Opposition refers to municipalities run by PMDB, PDT, PT or PTB mayors from 1982 to 1988. Rural sample: percentage of county residents living in urban areas < 24.8; Urban sample: percentage of county residents living in urban areas > 24.8.

Table 5

Dependent Variable: Incumbent party re-elected for mayor's office in 1988; LHS mean: 16%, SD: 0.37

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Pooled Thresholds 1-3							
I[x > 0]	0.166	0.152	0.112	0.111	0.0987	0.102	0.127
	(0.128)	(0.135)	(0.0991)	(0.103)	(0.0830)	(0.0863)	(0.110)
Observations	195	193	282	280	374	371	1199
R-squared	0.137	0.152	0.119	0.137	0.121	0.142	0.120
Pooled Thresholds 1-2							
I[x > 0]	0.0847	0.0734	0.0722	0.0661	0.0473	0.0409	0.0945
	(0.173)	(0.189)	(0.133)	(0.139)	(0.110)	(0.114)	(0.145)
Observations	129	129	192	192	250	249	828
R-squared	0.157	0.215	0.154	0.201	0.142	0.187	0.128
1st Threshold							
I[pop > 10188]	0.105	0.215	0.0713	0.0926	-0.00170	-0.0231	0.117
	(0.274)	(0.291)	(0.247)	(0.244)	(0.177)	(0.177)	(0.167)
Observations	65	65	100	100	134	133	458
R-squared	0.300	0.445	0.154	0.283	0.138	0.218	0.132
2nd Threshold							
I[pop > 13584]	-0.0665	-0.169	0.0618	-0.0164	0.0149	-0.0104	0.0665
	(0.229)	(0.264)	(0.169)	(0.177)	(0.146)	(0.157)	(0.156)
Observations	64	64	92	92	116	116	370
R-squared	0.362	0.520	0.347	0.438	0.285	0.305	0.155
3rd Threshold							
I[pop > 16980]	0.186	0.160	0.0584	0.0443	0.183	0.208	0.0744
	(0.228)	(0.305)	(0.180)	(0.203)	(0.144)	(0.152)	(0.157)
Observations	66	64	90	88	124	122	371
R-squared	0.242	0.313	0.199	0.252	0.164	0.200	0.175

Notes: Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 6

Dependent Variable (0/1): Incumbent party re-elected for mayor's office in 1988

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Panel A: Opposition (PMDB, PDT, PT, PTB) governed counties in 1982; LHS mean: 35%, SD: 0.48							
Pooled Thresholds 1-3							
I[x > 0]	0.560	0.631*	0.369	0.472*	0.356	0.434**	0.406***
	(0.385)	(0.368)	(0.287)	(0.249)	(0.216)	(0.199)	(0.152)
Observations	55	54	74	73	99	98	360
R-squared	0.46	0.69	0.33	0.52	0.29	0.37	0.19
Pooled Thresholds 1-2							
I[x > 0]	0.250	0.451	0.183	0.405	0.095	0.191	0.407**
	(0.488)	(0.533)	(0.359)	(0.314)	(0.298)	(0.270)	(0.165)
Observations	40	40	56	56	69	69	247
R-squared	0.40	0.68	0.30	0.51	0.35	0.46	0.23
Panel B: Center-incumbent (PDS) governed counties in 1982; LHS mean: 10%, SD: 0.30							
Pooled Thresholds 1-3							
I[x > 0]	0.197*	0.191	0.091	0.067	0.051	0.036	0.028
	(0.114)	(0.121)	(0.087	(0.087)	(0.071)	(0.073)	(0.085)
Observations	140	139	208	207	275	273	839
R-squared	0.33	0.35	0.25	0.26	0.20	0.22	0.14
Pooled Thresholds 1-2							
I[x > 0]	0.250	0.249	0.145	0.085	0.043	0.002	0.024
	(0.168)	(0.179)	(0.125)	(0.124)	(0.100)	(0.098)	(0.117)
Observations	89	89	136	136	181	180	581
R-squared	0.36	0.41	0.27	0.34	0.23	0.31	0.16

Notes: Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 7

Dependent Variable (0/1): Incumbent party re-elected for mayor's office in 1988

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Panel A: Opposition (PMDB, PDT, PT, PTB) governed counties in 1982; LHS mean: 35%, SD: 0.48							
Pooled Thresholds 1-3							
I[x > 0]	0.390	0.415	0.325	0.584***	0.335*	0.507***	0.353***
	(0.289)	(0.370)	(0.228)	(0.179)	(0.193)	(0.168)	(0.131)
Observations	54	53	73	72	98	97	360
Pseudo R-squared	0.19	0.59	0.08	0.25	0.16	0.27	0.05
Pooled Thresholds 1-2							
I[x > 0]	0.237	0.594*	0.225	0.446	0.244	0.354	0.312*
	(0.360)	(0.315)	(0.267)	(0.267)	(0.231)	(0.232)	(0.155)
Observations	40	40	56	56	69	69	247
Pseudo R-squared	0.17	0.43	0.06	0.23	0.17	0.31	0.05
Panel B: Center-incumbent (PDS) governed counties in 1982; LHS mean: 10%, SD: 0.30							
Pooled Thresholds 1-3							
I[x > 0]	0.012	0.000	0.047	0.032	0.035	0.022	0.037
	(0.034)	(0.001)	(0.079)	(0.070)	(0.071)	(0.065)	(0.092)
Observations	140	139	208	207	275	273	839
Pseudo R-squared	0.15	0.31	0.08	0.15	0.03	0.11	0.09
Pooled Thresholds 1-2							
I[x > 0]	0.002	0.000	0.088	0.0579	0.034	0.007	0.060
	(0.011)	(0.000)	(0.106)	(0.076)	(0.095)	(0.078)	(0.124)
Observations	89	89	136	136	181	180	581
Pseudo R-squared	0.17	0.36	0.11	0.24	0.04	0.21	0.11

Notes: Table gives marginal effects after Probit estimation. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 8

Dependent Variable: PDS or PFL re-elected for mayor's office in 1988 for PDS counties in 1982

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Panel A, Maximum Likelihood							
Pooled Thresholds 1-3							
I[x > 0]	0.101	0.118	0.028	0.0187	-0.030	-0.032	0.108
	(0.168)	(0.168)	(0.142)	(0.143)	(0.122)	(0.123)	(0.162)
Observations	140	139	208	207	275	273	839
R-squared	0.03	0.05	0.02	0.03	0.02	0.03	0.08
Pooled Thresholds 1-2							
I[x > 0]	0.239	0.260	0.217	0.156	0.122	0.0817	0.216
	(0.210)	(0.216)	(0.174)	(0.180)	(0.151)	(0.155)	(0.201)
Observations	89	89	136	136	181	180	581
R-squared	0.02	0.06	0.02	0.04	0.02	0.05	0.09
Panel B, OLS							
Pooled Thresholds 1-3							
I[x > 0]	0.114	0.112	0.036	-0.035	-0.011	-0.076	0.097
	(0.168)	(0.171)	(0.144)	(0.146)	(0.121)	(0.121)	(0.153)
Observations	140	139	208	207	275	273	839
R-squared	0.082	0.121	0.049	0.107	0.057	0.104	0.10
Pooled Thresholds 1-2							
I[x > 0]	0.236	0.232	0.219	0.105	0.115	0.00886	0.267
	(0.210)	(0.229)	(0.177)	(0.190)	(0.150)	(0.155)	(0.204)
Observations	89	89	136	136	181	180	585
R-squared	0.109	0.138	0.057	0.123	0.068	0.140	0.127

Notes: Panel A gives marginal effects after Probit estimation. PFL 1988 coded as PDS 1988 because of official party split in 1985. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 9

Dependent Variable: Literacy, 19-28 year olds in 1991

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Panel A: Opposition (PMDB, PDT, PT, PTB) governed counties in 1982; LHS mean: 0.86, SD: 0.13							
Pooled Thresholds 1-3							
I[x > 0]	0.082**	0.041*	0.048**	0.033**	0.034*	0.027*	0.039***
	(0.036)	(0.023)	(0.020)	(0.0134)	(0.020)	(0.016)	(0.013)
Observations	55	54	74	73	99	98	360
R-squared	0.934	0.977	0.926	0.972	0.905	0.945	0.940
Pooled Thresholds 1-2							
I[x > 0]	0.083**	0.033	0.048**	0.013	0.019	0.008	0.033**
	(0.036)	(0.028)	(0.019)	(0.015)	(0.025)	(0.018)	(0.016)
Observations	40	40	56	56	69	69	247
R-squared	0.932	0.974	0.925	0.976	0.915	0.956	0.940
Panel B: Center-incumbent (PDS) governed counties in 1982; LHS mean: 0.72, SD: 0.17							
Pooled Thresholds 1-3							
I[x > 0]	0.073*	0.053**	0.085***	0.057***	0.073***	0.041***	0.041**
	(0.041)	(0.025)	(0.028)	(0.018)	(0.022)	(0.015)	(0.017)
Observations	140	139	208	207	275	273	839
R-squared	0.751	0.901	0.771	0.899	0.769	0.898	0.882
Pooled Thresholds 1-2							
I[x > 0]	0.070	0.059*	0.067*	0.058**	0.063**	0.041**	0.043**
	(0.059)	(0.032)	(0.037)	(0.024)	(0.029)	(0.020)	(0.026)
Observations	89	89	136	136	181	180	581
R-squared	0.763	0.924	0.794	0.919	0.791	0.904	0.873

Notes: Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 10

Dependent Variable: Years of schooling, 19-28 year olds in 1991

Specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
Panel A: Opposition (PMDB, PDT, PT, PTB) governed counties in 1982; LHS mean: 5.15, SD: 1.2							
Pooled Thresholds 1-3							
I[x > 0]	0.645	0.221	0.665	0.381	0.458	0.235	0.384
	(0.560)	(0.606)	(0.400)	(0.325)	(0.307)	(0.227)	(0.300)
Observations	55	54	74	73	99	98	360
R-squared	0.779	0.876	0.796	0.875	0.724	0.883	0.870
Pooled Thresholds 1-2							
I[x > 0]	0.823	0.706	0.786*	0.206	0.402	0.130	0.250
	(0.627)	(0.777)	(0.437)	(0.401)	(0.405)	(0.298)	(0.403)
Observations	40	40	56	56	69	69	247
R-squared	0.744	0.903	0.789	0.872	0.736	0.888	0.849
Panel B: Center-incumbent (PDS) governed counties in 1982; LHS mean: 3.94, SD: 1.42							
Pooled Thresholds 1-3							
I[x > 0]	0.381	0.197	0.617**	0.297**	0.623***	0.255**	0.293*
	(0.326)	(0.181)	(0.244)	(0.145)	(0.213)	(0.126)	(0.153)
Observations	140	139	208	207	275	273	839
R-squared	0.720	0.896	0.686	0.886	0.670	0.888	0.871
Pooled Thresholds 1-2							
I[x > 0]	0.401	0.256	0.511	0.305	0.635**	0.323*	0.335*
	(0.451)	(0.225)	(0.328)	(0.186)	(0.279)	(0.170)	(0.183)
Observations	89	89	136	136	181	180	581
R-squared	0.754	0.922	0.722	0.906	0.701	0.892	0.857

Notes: Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, enrollment of 7 to 14 year olds and percent of population living in urban areas. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Figure 1: FPM Transfers, 1982-1985 (in ´000 of 2005 Reais)

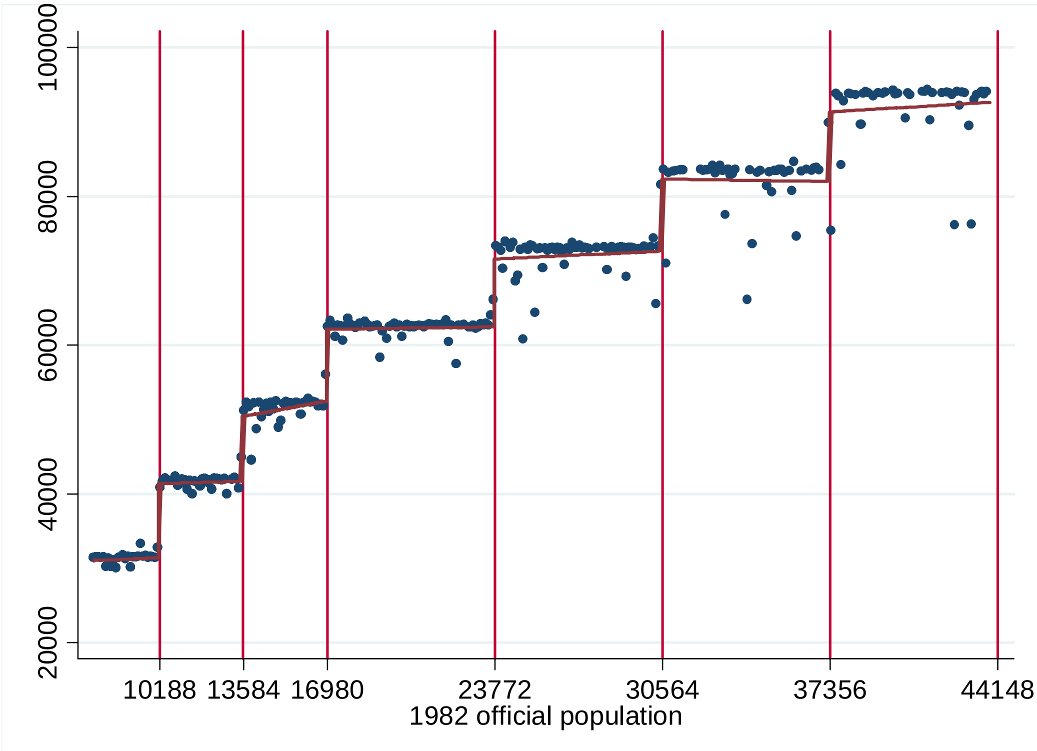

Figure 2: FPM transfers timeline

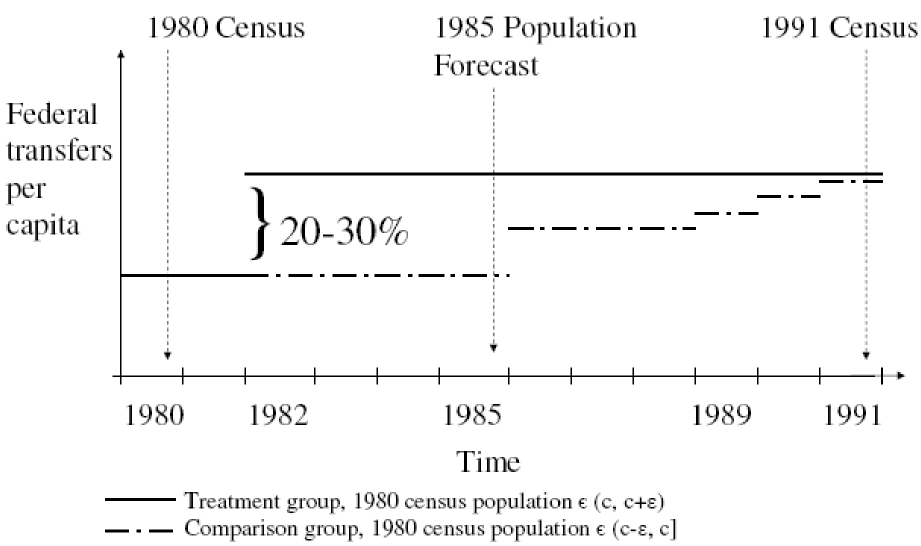

Figure 3: Histogram for 1982 official populationi

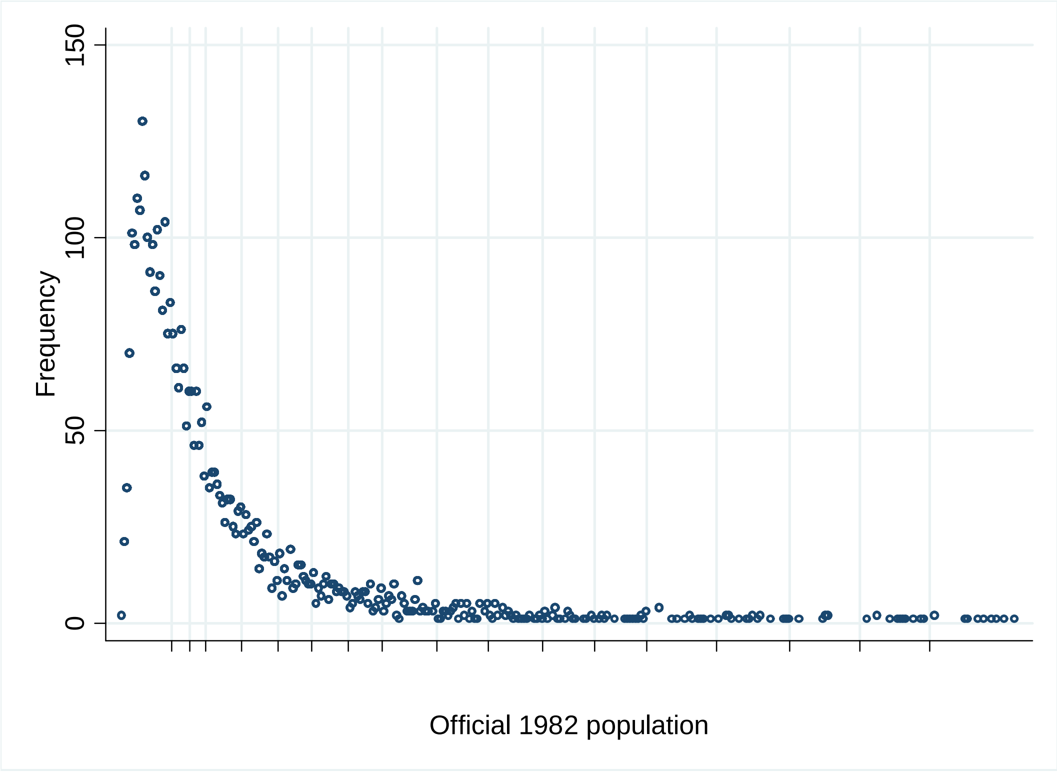

Figure 4: Histogram for 1982 official population, small to medium municipalities

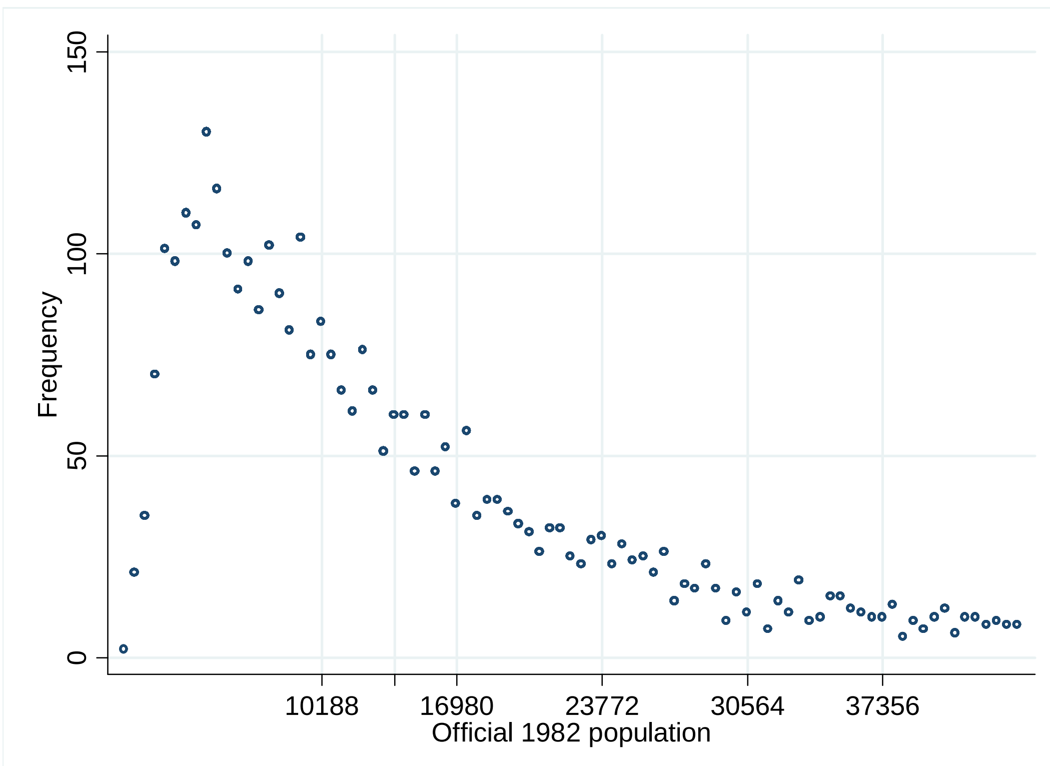

Figure 5: Discontinuity plot for full sample

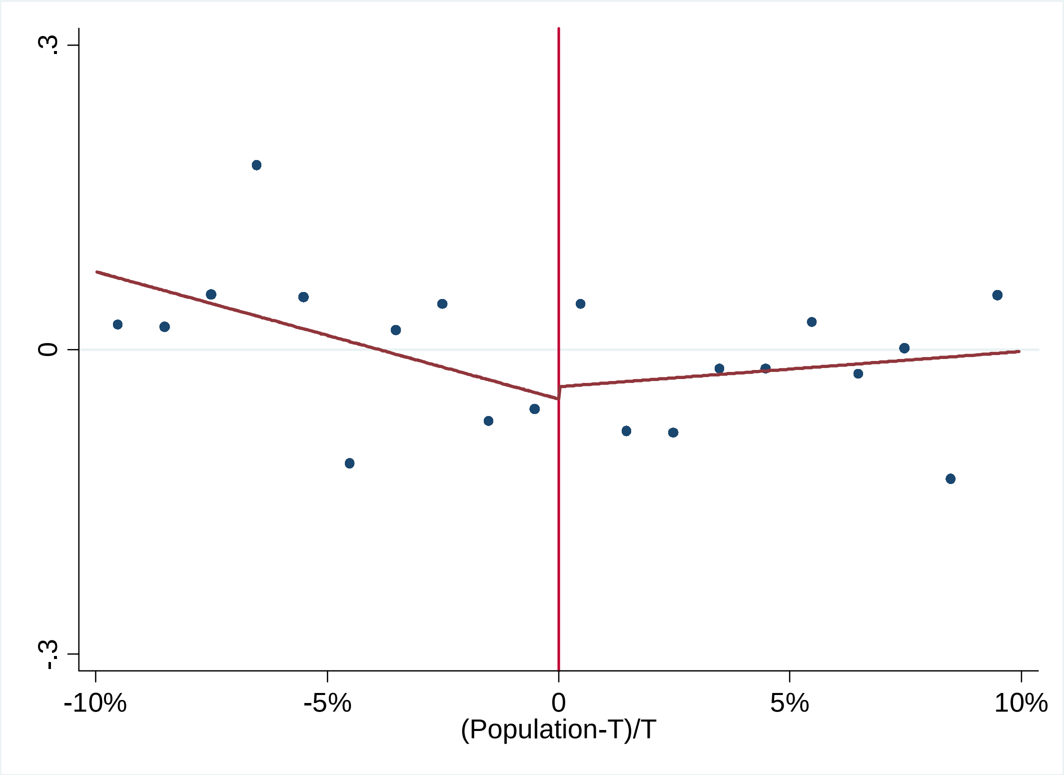

Figure 6: Discontinuity plot for municipalities with PDS incumbents in 1982

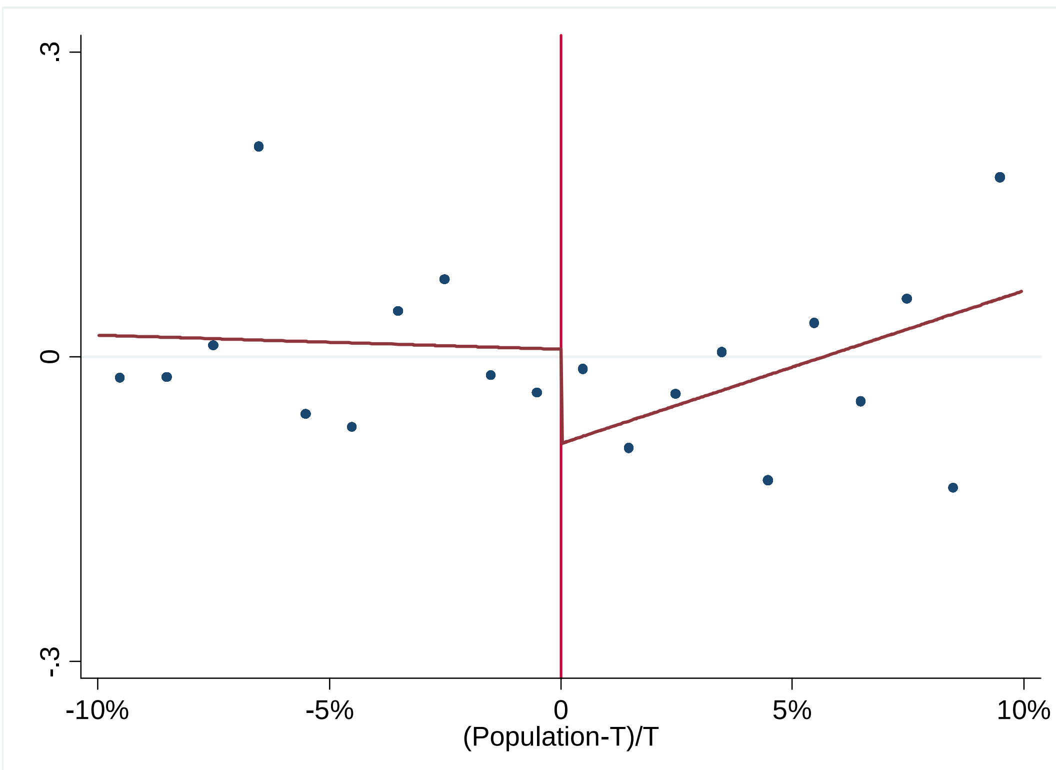

Figure 7: Discontinuity plot for municipalities with opposition (non-PDS) incumbents in 1982