

# Local Electoral Effects of Intergovernmental Fiscal Transfers: Quasi-Experimental Evidence from Brazil, 1982-1988\*

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## Abstract

A standard assumption in political economy models of public resource allocation is that voters reward politicians for providing economic benefits. Because politicians might target public resources towards swing or core support communities, empirical studies that try to test and quantify the electoral effect of resource transfers face a problem of simultaneous causality bias. This paper provides quasi-experimental evidence on electoral effects of resource transfers for local incumbent governments in Brazil. We estimate the transfers' effect on party re-election probabilities in the 1988 mayoral elections using regression-discontinuity analysis of a population-based revenue-sharing mechanism. Our results suggest that increasing fiscal transfers by 30% increased the probability that the incumbent party got re-elected by about 10 to 20 percentage points on average. The re-election probability of local non-aligned incumbent parties increased by about 35 percentage points while extra resources had no 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 to democracy made the transfers' electoral benefit unobservable.

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

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# 1 Introduction

A standard assumption in political economy models of public resource allocation is that voters reward politicians for providing economic benefits (Cox and McCubbins 1986; Lindbeck and Weibull 1987; Dixit and Londregan 1996, 1998). When economic benefits are financed by fiscal transfers from the central to sub-national governments, recent formal models of intergovernmental transfers emphasize that both the funding central (grantor) government and the implementing local or state (grantee) government are likely to take some credit for delivering benefits to voters (Solé-Ollé and Sorribas-Navarro 2008a; Khemani 2007; Arulampalam et al. 2009).<sup>1</sup> While the extent of voter responsiveness to material benefits is a key parameter in these models, empirical studies that test and quantify the strength of this relationship are relatively scant. As Díaz-Cayeros and his coauthors (2007: 14) put it: “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.”<sup>2</sup>

Assessing the validity of this claim is actually not straightforward because politicians might target public resources towards swing voters (those that are most susceptible to economic benefits) or core supporters (those with strong ideological attachment to a given party). Empirical studies that try to test voter responsiveness thus face a problem of simultaneous causality bias. For example, if regions that receive higher transfers from the central government are observed to vote more for the party in charge of the central government it may be because the resources were funneled to those regions precisely because of their loyalty. Endogeneity of resource transfers (to individuals or communities) is therefore a major hurdle that must be overcome in this line of research.

The key contribution of this paper is to provide quasi-experimental evidence on the political effects of fiscal transfers for local incumbent governments. We analyze the effect of transfers in the Brazilian municipal executive elections of 1988, the first elections in which

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<sup>1</sup>Intergovernmental transfers are empirically important. In many federations around the world they finance much if not most of the expenditures of sub-national levels of government (Rodden 2004; Ter-Minassian 1997).

<sup>2</sup>An exception is Remmer and Gelineau (2003).

all municipalities chose their mayor after decades of restrictions to political competition during the authoritarian regime. In Brazil during the 1980s (and continuing to the present day), a substantial part of national tax revenue is distributed to local governments strictly on the basis of population, via a formula based on thresholds. That is, if a municipality's population is over the first population threshold, it receives 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.<sup>3</sup> Because of the nature of 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 party re-election probabilities in the 1988 mayoral elections.

The estimation results suggest that increasing fiscal transfers by 30% increased the probability that the incumbent party was re-elected by about 10 to 20 percentage points on average. The re-election probability of local non-aligned incumbent parties increased by about 35 percentage points whereas extra fiscal transfers had no effect on the re-election probability of the PDS, the party of the authoritarian regime. Although the difference in electoral responses is not statistically significant at conventional levels, our finding is somewhat puzzling since the *a priori* expected heterogeneous electoral responses would be exactly opposite, i.e. a positive effect for aligned local incumbents and a smaller effect for non-aligned local incumbents. In our view the most convincing explanation of this puzzle lies in 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 electoral response receive no support in the data. The first is that voters in PDS run municipalities that received more resources rewarded not the PDS but its splinter party, the PFL. To test this explanation

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<sup>3</sup>See Litschig (2008b) for evidence that over the 1990s the transfer mechanism was manipulated to benefit aligned (right-wing) national deputies in electorally fragmented local political systems as well as aligned local executives.

we analyze a conception of “re-election” in which we code as 1 those PDS municipalities from the 1982-1988 period that voted in 1988 for the residual PDS *or* the PFL. We find no electoral effect under this coding either. The other explanation we examine is whether opposition parties used the extra resources to deliver improvements in local public services while the PDS did not. Our results suggest, however, that both PDS and opposition parties that got extra resources delivered higher benefits to voters, at least as measured by education outcomes (completed years of schooling and literacy).

Our paper most directly builds on a small but growing literature that attempts to overcome endogeneity issues in the empirical analysis of voters responsiveness to material benefits.<sup>4</sup> Levitt and Snyder (1997), for example, study how House of Representative elections are affected by federal spending, instrumenting for spending in one district with federal spending in neighboring districts. They find that non-transfer federal spending benefits incumbents, but transfers do not. The same instrumental variable approach is also used by Solé-Ollé and Sorribas-Navarro (2008b) 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 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. Most recently, Manacorda, et al. (2009) use a regression discontinuity approach to study the political effects of an Uruguayan anti-poverty program. They find that beneficiary households were significantly more likely to vote for the incumbent government.

Almost all of these works focus on electoral effects for the grantor government—that is, the benefit to the central government of granting these transfers. However, when public services are funded by the central government but implemented by some lower level of

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<sup>4</sup>While the focus here is on works that have attempted to deal with the endogeneity of transfers, there are of course other important papers on this topic in economics and political science.

government, it is reasonable to expect that the lower level incumbents derive some political benefit as well (Solé-Ollé and Sorribas-Navarro 2008a; Khemani 2007; Arulampalam et al. 2009). 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 more easily (as in Solé-Ollé and Sorribas-Navarro 2008b). With the exception of Solé-Ollé and Sorribas-Navarro (2008b) there has been no attention paid to electoral effects of fiscal transfers for local incumbent governments and it is 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.<sup>5</sup> 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 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, details our conceptual framework and gives a description of the revenue sharing mechanism we examine. 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 and a final section concludes with a discussion of extensions.

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<sup>5</sup>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.

## 2 Background

### 2.1 The Brazilian political 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).

## 2.2 Conceptual framework

The goal of this paper is to understand the effect that intergovernmental transfers had on the 1988 local executive elections. It should be noted that, for a variety of reasons, these elections represent a difficult environment in which to find an electoral effect. To begin with, to the extent that these transfers are expected to have an effect in democratic regimes, 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. Over our study period, 1982-1988, total government revenue in Brazil was about 25% of GDP, of which municipalities collected about 4%. At the same time, local governments managed about 17% of public resources (Shah 1990). Intergovernmental transfers to local governments thus represented about 3.25% of GDP. 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.<sup>6</sup> This grant accounted for about 50% of the revenue of the municipalities in our analysis.

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 non-excludable services, or public goods for short, and private goods (such as government jobs and other means of clientelism).<sup>7</sup> 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 of public good provision on electoral outcomes would be  $E_B$ , the partial derivative of  $E$  with respect to  $B$ . In contrast, 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.<sup>8</sup>

### 2.3 Mechanics of revenue sharing

In order to estimate the electoral response to fiscal transfers we exploit local variation in FPM transfers in a regression-discontinuity (RD) design. The critical feature of the FPM revenue-sharing mechanism for the purposes of this analysis is Decree 1881/81, which

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<sup>6</sup> The one condition is that municipalities must spend 25 percent of the transfers on education. This constraint is usually considered non-binding, in that municipalities typically spend about 20% of their total revenue on education. It is not clear how this provision was enforced in practice since there is no clear definition of education expenditures and accounting information provided by local governments was not systematically verified.

<sup>7</sup> 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.

<sup>8</sup> See Litschig (2008a) for an analysis of  $R_F$ .

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  $c(\cdot)$  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 the transfers were based on extrapolations produced by the national statistical agency, IBGE.

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 municipios 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 which 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, is less clear. 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 to contest the accuracy of the estimates in order to get more transfers. A related question is where the exact cutoffs come from, i.e. 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.<sup>9</sup> 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 are still equidistant from one another, the distance being 6792 for the first 7 cutoffs (except for the second cutoff which lies exactly halfway in between).

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.<sup>10</sup> 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.

These discontinuities in general 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.<sup>11</sup> Starting in 1988, official population estimates were updated annually, and more counties were reclassified in 1989 and 1990. By 1991, counties that were just below a threshold in 1982 received the same amount of transfers as those

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<sup>9</sup>Supplementary Law No. 35, 1967, Art. 1, Paragraphs 2 and 4.

<sup>10</sup>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).

<sup>11</sup>The methodology 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 Estatística 2002).

counties that were just above the threshold in 1982.<sup>12</sup>

### 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),  $\alpha$  an intercept,  $\tau$  the resource transfer effect,  $D$  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.

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

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<sup>12</sup>Supplementary Law n° 59/1988.

control over population seems plausible in our context. Moreover, even if local elites had perfect control over the number of residents in their county, 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. Put differently, local elites were unlikely to even know the exact locations of the new thresholds prior to 1980.

Still, one might worry that leaders in the central government had incentives to alter 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 leaders of a certain type.

### **3.2 Validity checks**

Since extensive manipulation would cast serious doubts on the internal validity of the design, 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 6 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 over 15 year olds, 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 dramatically 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.<sup>13</sup> 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. 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).

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<sup>13</sup>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.

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, the percentage of illiterate 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 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. We supplement the local linear estimates with higher order polynomial specifications using an extended support where we chose the order of the polynomial such that it best matches the local linear estimates in the smallest discontinuity sample.

In the analysis that follows, we focus particularly on the first three population thresholds (10188, 13584, and 16980). At subsequent cutoffs the variation in FPM transfers is too small to affect municipal overall budgets 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 results for the first three thresholds individually, we also pool the counties in these segments, in order to gain statistical power. The specification we use to test the null hypothesis of common resource effects across the first three cutoffs is as follows. Let  $s_k$  denote the 4 integers, 7500, 11800, 15100, 23772 that bound and partition the population support into 3 segments,  $\mathbf{z}_{cs}$  a set of pre-treatment covariates,  $c_s$  a fixed effect for each state, and  $u_{cs}$  an error term for each county. The testing specification is then:

$$\begin{aligned}
Y_{cs} = & [\tau_1 1[\text{pop}_{cs} > c_1] + \alpha_{10} \text{pop}_{cs} + \alpha_{11} (\text{pop}_{cs} - c_1) 1[\text{pop}_{cs} > c_1]] 1_{1p} \\
& + [\tau_2 1[\text{pop}_{cs} > c_2] + \alpha_{20} \text{pop}_{cs} + \alpha_{21} (\text{pop}_{cs} - c_2) 1[\text{pop}_{cs} > c_2]] 1_{2p} \\
& + [\tau_3 1[\text{pop}_{cs} > c_3] + \alpha_{30} \text{pop}_{cs} + \alpha_{31} (\text{pop}_{cs} - c_3) 1[\text{pop}_{cs} > c_3]] 1_{3p} \\
& + \sum_{k=1}^3 \beta_k 1[s_{k-1} < \text{pop}_{cs} \leq s_k] 1_{kp} + \boldsymbol{\gamma} \mathbf{z}_{cs} + a_s + u_{cs}
\end{aligned}$$

$$s_0 = 7500, s_1 = 11800, s_2 = 15100, s_3 = 23772$$

$$1_{kp} = 1[c_k(1-p) < \text{pop}_{cs} < c_k(1+p)], k = 1, 2, 3; p = 2, 3, 4\%$$

$$1_p = 1_{1p} + 1_{2p} + 1_{3p}$$

We fail to reject the null hypothesis  $\tau_1 = \tau_2 = \tau_3$  at conventional levels of significance irrespective of the bandwidth (results not shown), which provides some justification for pooling observations across segments. For the pooled analysis, we need to make observations comparable in terms of the distance from their respective threshold. 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:

$$\begin{aligned}
 x_{cs} &= \text{pop}_{cs} - 10188 \text{ if } s_0 < \text{pop}_{cs} \leq s_1 \\
 &\text{pop}_{cs} - 13564 \text{ if } s_1 < \text{pop}_{cs} \leq s_2 \\
 &\text{pop}_{cs} - 16980 \text{ if } s_2 < \text{pop}_{cs} \leq s_3
 \end{aligned}$$

$$\begin{aligned}
 Y_{cs} &= \tau 1[x_{cs} > 0]1_p + [\alpha_{10}x_{cs} + \alpha_{11}x_{cs}1[x_{cs} > 0]]1_{1p} \\
 &\quad + [\alpha_{20}x_{cs} + \alpha_{21}x_{cs}1[x_{cs} > 0]]1_{2p} \\
 &\quad + [\alpha_{30}x_{cs} + \alpha_{31}x_{cs}1[x_{cs} > 0]]1_{3p} \\
 &\quad + \sum_{k=1}^3 \beta_k 1[s_{k-1} < \text{pop}_{cs} \leq s_k]1_{kp} + \boldsymbol{\gamma}\mathbf{z}_{cs} + a_s + u_{cs}
 \end{aligned}$$

Essentially this equation allows for six different slopes, one each on either side of the three thresholds but imposes a common resource effect  $\tau$ . Under the continuity assumption above, the pooled treatment effect is given by  $\lim_{\Delta \downarrow 0} E[Y | x = \Delta] - E[Y | x = 0] = \tau$ . We follow the suggestions by Imbens and Lemieux (2008) and use a rectangular kernel (i.e. equal weight for all observations within  $p$  percent of the respective cutoff) and standard least squares theory for inference. Because our dependent variable is dichotomous, we also check whether 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 estimation results from the linear probability model. Estimates of the resource effect are almost all positive and most lie between 10 to 20 percentage points. This is true both for effects estimated within each segment and for the specifications that pool observations across cutoffs. The same pattern of results arises with the probit estimates

shown in table 6. While most of the estimates from individual cutoffs are not significantly different from 0, the pooling across the three cutoffs yields marginally significant (at 10%) estimates. Figure 5 presents the result graphically. Each dot represents the average residual from a regression of the re-election dummy on an indicator for whether the county was run by a PDS mayor from 1982-1988 (0) or an opposition party (1) within a bin of one percentage point of cutoff population. Since opposition-run municipalities were about 34 percentage points more likely to re-elect the same party into office than PDS-run municipalities, the residual from that regression is considerably less noisy than the re-election indicator itself. 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 3 population thresholds. The figure suggests that there is some evidence of a discontinuity at the threshold. It is worth noting that the regression function slopes downward both to the left and to the right of the cut-off, which is the same pattern followed by FPM transfers *per capita*.

Since the extra resources considered here are general budget support revenue-sharing transfers, credit-claiming for the central government is particularly hard, i.e. it seems likely that most of the political credit or goodwill should go to the local government (Solé-Ollé and Sorribas-Navarro 2008a; Khemani 2007; Arulampalam et al. 2009). In the extreme case the transfers would benefit exclusively the local incumbent party, irrespective of whether it is aligned with the central government or not. If, however, voters realize that any local public service or patronage benefits they experience must ultimately be financed by the central government, political credit would be shared and one would expect a positive resource effect for aligned local incumbents and a smaller or no effect for non-aligned local incumbents.

The results, however, are exactly opposite as presented in Table 7. While in the opposition municipalities the transfers have a significantly positive effect of about 35 percentage points, the sub-sample of PDS-governed municipalities shows systematically lower and less precisely estimated effects across specifications. As Table 8 shows, results from the

probit model are substantively similar to those from the linear probability model. The differences in point estimates between PDS and opposition municipalities can be seen graphically: Figure 6 shows no discontinuity in the re-election probability for municipalities that were run by the PDS from 1982 to 1988. Figure 7 on the other hand shows a sharp discontinuity for the opposition municipalities. The fact that the latter graph is considerably more noisy is at least partly due to smaller sample size for opposition municipalities which results in higher variability of the bin means.

We consider two hypotheses for the differential effect. The first is the possibility that voters in PDS run municipalities that received more resources rewarded not the PDS but its splinter party, the PFL. We first analyze a conception of “re-election” in which we code as 1 those PDS municipalities from the 1982-1988 period that voted in 1988 for the residual PDS *or* the PFL. The results are shown in Table 9, and there are no substantive differences with results in tables 7 or 8, i.e. the zero effect is not driven by coding of the dependent variable. 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 from 1982 to 1988. 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 opposition parties delivered improvements in local public services with the extra resources while the PDS did not. To evaluate this hypothesis, we examine the effect of extra transfers on both literacy and education outcomes as measures of real public service improvements. We are unable to find a difference between PDS and opposition municipalities along this public service dimension. As Table 10 shows, municipalities that received additional transfers perform better in literacy outcomes in both PDS and opposition municipalities. And as Table 11 shows, PDS municipalities are if anything better than opposition municipalities at turning transfers into schooling outcomes (although opposition municipalities 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 of these results is that the heterogeneous responses we find reflect the uniqueness of the 1988 election in Brazil, i.e. 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 is unobservable. This is consistent with the fact that the re-election probability for municipalities to the left of the cutoffs was about 10% for PDS while it was 35% for opposition municipalities (Table 7). The fact that the transfers improved 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 should have a positive local electoral effects in consolidated democratic systems as well.

## 7 Conclusion

This paper is one of the first attempts to study the effects of intergovernmental 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. Along with other recent papers that look at political resource effects for grantor governments we address endogeneity using a quasi-experimental research design.

Our results suggest that increasing fiscal transfers by 30% increased the probability that the incumbent party was re-elected by about 10 to 20 percentage points on average. The re-election probability of local non-aligned incumbent parties increased by about 35 percentage points whereas extra fiscal transfers had no effect on the re-election probability of the PDS, the party of the authoritarian regime. Compared to the one other study

that looks at local electoral effects that we are aware of (Sole-Olle and Sorribas-Navarro, 2008b), the heterogeneous response we find is quite different. They find for Spain that both local aligned and non-aligned governments benefit electorally from higher capital grants from the center. Our interpretation is that the zero effect we find for aligned local governments is very specific to the period and setting we study, i.e. the end of the authoritarian regime in Brazil and with it the collapse of the government party. Examining electoral responses in other contexts and across types of transfers is thus an obvious avenue for future research.

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 local public services. 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.

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Table 1: Mayor party affiliations in 1982 and 1988

Party	Party-type	1982		1988	
		N	%	N	%
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

<i>Percent of GDP</i>			
	1960	1980	1988
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
<i>Percent of total revenues</i>			
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

<i>Population bracket</i>				<i>Coefficient</i>
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

Sample Observations	Population range						
	7,500 - 44,148			8,500 - 18,700			
	Full	Full	PDS	Opposition	Rural	Urban	
	2306	1248	844	358	624	624	
<u>1980 county characteristics (IBGE)</u>							
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	
<u>1982 Financial data (Ministry of Finance)</u>							
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	
<u>1991 Real school resources (1991 school census)</u>							
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	
<u>1991 School outcomes (1991 census)</u>							
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	
<u>1988 Electoral outcomes (TSE)</u>							
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: PDS 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	Various
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
<u>Pooled Thresholds 1-3</u>							
I[x > 0]	0.188 (0.125)	0.224** (0.104)	0.112 (0.099)	0.127 (0.088)	0.099 (0.083)	0.114 (0.076)	0.222** (0.108)
Observations	195	195	282	282	374	374	1215
R-squared	0.27	0.42	0.28	0.38	0.28	0.37	0.40
<u>Pooled Thresholds 1-2</u>							
I[x > 0]	0.126 (0.164)	0.203 (0.158)	0.089 (0.124)	0.146 (0.117)	0.047 (0.110)	0.084 (0.103)	0.212 (0.141)
Observations	129	129	192	192	250	250	839
R-squared	0.28	0.42	0.23	0.31	0.31	0.32	0.42
<u>1<sup>st</sup> Threshold</u>							
I[pop > 10188]	0.105 (0.274)	0.232 (0.239)	0.102 (0.206)	0.215 (0.211)	0.021 (0.162)	0.132 (0.150)	0.203 (0.141)
Observations	65	65	100	100	134	134	463
R-squared	0.30	0.42	0.07	0.34	0.06	0.35	0.26
<u>2<sup>nd</sup> Threshold</u>							
I[pop > 13584]	-0.002 (0.183)	0.076 (0.190)	0.084 (0.153)	0.093 (0.153)	0.052 (0.133)	0.056 (0.133)	0.056 (0.153)
Observations	64	64	92	92	116	116	376
R-squared	0.06	0.14	0.08	0.10	0.09	0.09	0.20
<u>3<sup>rd</sup> Threshold</u>							
I[pop > 16980]	0.186 (0.228)	0.172 (0.223)	0.058 (0.180)	0.132 (0.140)	0.183 (0.144)	0.213 (0.109)	0.240** (0.121)
Observations	66	66	90	90	124	124	376
R-squared	0.24	0.46	0.20	0.42	0.16	0.38	0.262

Notes: Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates always include the indicator for whether the county was run by a PDS mayor from 1982-1988 (0 or an opposition party (1)). The last specification also includes average years of schooling for individuals 25 years and older and it's interaction with the PDS indicator. Other covariates such as county income per capita, 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 do not alter the estimate of interest and are jointly insignificant. All specifications allow for differential slopes and curvature by segment and relative to the thresholds.

Table 6:

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	Various
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment Covariates:	N	Y	N	Y	N	Y	Y
<u>Pooled Thresholds 1-3</u>							
I[x > 0]	0.150 (0.103)	0.207** (0.106)	0.102 (0.092)	0.153* (0.090)	0.093 (0.079)	0.116 (0.077)	0.212* (0.115)
Observations	195	195	282	282	374	374	1215
Pseudo R-squared	0.05	0.24	0.06	0.16	0.07	0.16	0.25
<u>Pooled Thresholds 1-3</u>							
I[x > 0]	0.081 (0.125)	0.191 (0.128)	0.100 (0.123)	0.161 (0.123)	0.049 (0.103)	0.092 (0.102)	0.226 (0.150)
Observations	129	129	192	192	250	250	839
Pseudo R-squared	0.08	0.21	0.06	0.15	0.08	0.15	0.22
<u>1<sup>st</sup> Threshold</u>							
I[pop > 10188]	0.124 (0.178)	0.233 (0.187)	0.086 (0.201)	0.208 (0.188)	0.024 (0.160)	0.120 (0.143)	0.204 (0.163)
Observations	61	61	100	100	134	134	463
Pseudo R-squared	0.20	0.34	0.07	0.21	0.06	0.25	0.25
<u>2<sup>nd</sup> Threshold</u>							
I[pop > 13584]	0.026 (0.193)	0.095 (0.207)	0.113 (0.168)	0.093 (0.153)	0.074 (0.141)	0.077 (0.141)	0.075 (0.172)
Observations	58	58	86	92	109	109	376
Pseudo R-squared	0.04	0.11	0.07	0.10	0.09	0.09	0.16
<u>3<sup>rd</sup> Threshold</u>							
I[pop > 16980]	0.247** (0.134)	0.253 (0.149)	0.043 (0.141)	0.129 (0.128)	0.190 (0.120)	0.225* (0.114)	0.275* (0.141)
Observations	65	64	89	89	124	124	376
Pseudo R-squared	0.13	0.33	0.08	0.27	0.09	0.25	0.22

The table gives marginal effects after Probit estimation. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. Pre-treatment covariates always include the indicator for whether the county was run by a PDS mayor from 1982-1988 (0) or an opposition party (1). The last specification also includes average years of schooling for individuals 25 years and older and it's interaction with the PDS indicator. Other covariates such as county income per capita, 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 do not alter the estimate of interest and are jointly insignificant. 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]	0.560 (0.385)	0.631* (0.368)	0.369 (0.287)	0.472* (0.249)	0.356 (0.216)	0.434** (0.199)	0.406*** (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 (0.488)	0.451 (0.533)	0.183 (0.359)	0.405 (0.314)	0.095 (0.298)	0.191 (0.270)	0.407** (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]	0.130 (0.106)	0.113 (0.105)	0.091 (0.087)	0.067 (0.087)	0.051 (0.071)	0.036 (0.073)	0.028 (0.085)
Observations	140	139	208	207	275	273	839
R-squared	0.11	0.19	0.25	0.26	0.20	0.22	0.14

Pooled Thresholds 1-2

I[x > 0]	0.160 (0.152)	0.155 (0.158)	0.145 (0.125)	0.085 (0.124)	0.043 (0.100)	0.002 (0.098)	0.024 (0.117)
Observations	89	89	136	136	181	180	581
R-squared	0.11	0.21	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 8:

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 (0.289)	0.415 (0.370)	0.325 (0.228)	0.584*** (0.179)	0.335* (0.193)	0.507*** (0.168)	0.353*** (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 (0.360)	0.594* (0.315)	0.225 (0.267)	0.446 (0.267)	0.244 (0.231)	0.354 (0.232)	0.312* (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 (0.034)	0.000 (0.001)	0.047 (0.079)	0.032 (0.070)	0.035 (0.071)	0.022 (0.065)	0.037 (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 (0.011)	0.000 (0.000)	0.088 (0.106)	0.058 (0.076)	0.034 (0.095)	0.007 (0.078)	0.060 (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 9:  
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 (0.168)	0.118 (0.168)	0.028 (0.142)	0.0187 (0.143)	-0.030 (0.122)	-0.032 (0.123)	0.108 (0.162)
Observations	140	139	208	207	275	273	839
Pseudo 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 (0.210)	0.260 (0.216)	0.217 (0.174)	0.156 (0.180)	0.122 (0.151)	0.0817 (0.155)	0.216 (0.201)
Observations	89	89	136	136	181	180	581
Pseudo 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 (0.168)	0.112 (0.171)	0.036 (0.144)	-0.035 (0.146)	-0.011 (0.121)	-0.076 (0.121)	0.097 (0.153)
Observations	140	139	208	207	275	273	839
R-squared	0.08	0.12	0.05	0.11	0.06	0.10	0.10

Pooled Thresholds 1-2

I[x > 0]	0.236 (0.210)	0.232 (0.229)	0.219 (0.177)	0.105 (0.190)	0.115 (0.150)	0.009 (0.155)	0.267 (0.204)
Observations	89	89	136	136	181	180	585
R-squared	0.11	0.14	0.06	0.12	0.07	0.14	0.12

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 10:

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** (0.036)	0.041* (0.023)	0.048** (0.020)	0.033** (0.0134)	0.034* (0.020)	0.027* (0.016)	0.039*** (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** (0.036)	0.033 (0.028)	0.048** (0.019)	0.013 (0.015)	0.019 (0.025)	0.008 (0.018)	0.033** (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* (0.041)	0.053** (0.025)	0.085*** (0.028)	0.057*** (0.018)	0.073*** (0.022)	0.041*** (0.015)	0.041** (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)	0.059* (0.032)	0.067* (0.037)	0.058** (0.024)	0.063** (0.029)	0.041** (0.020)	0.043** (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 11:  
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 (0.560)	0.221 (0.606)	0.665 (0.400)	0.381 (0.325)	0.458 (0.307)	0.235 (0.227)	0.384 (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 (0.627)	0.706 (0.777)	0.786* (0.437)	0.206 (0.401)	0.402 (0.405)	0.130 (0.298)	0.250 (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 (0.326)	0.197 (0.181)	0.617** (0.244)	0.297** (0.145)	0.623*** (0.213)	0.255** (0.126)	0.293* (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 (0.451)	0.256 (0.225)	0.511 (0.328)	0.305 (0.186)	0.635** (0.279)	0.323* (0.170)	0.335* (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)

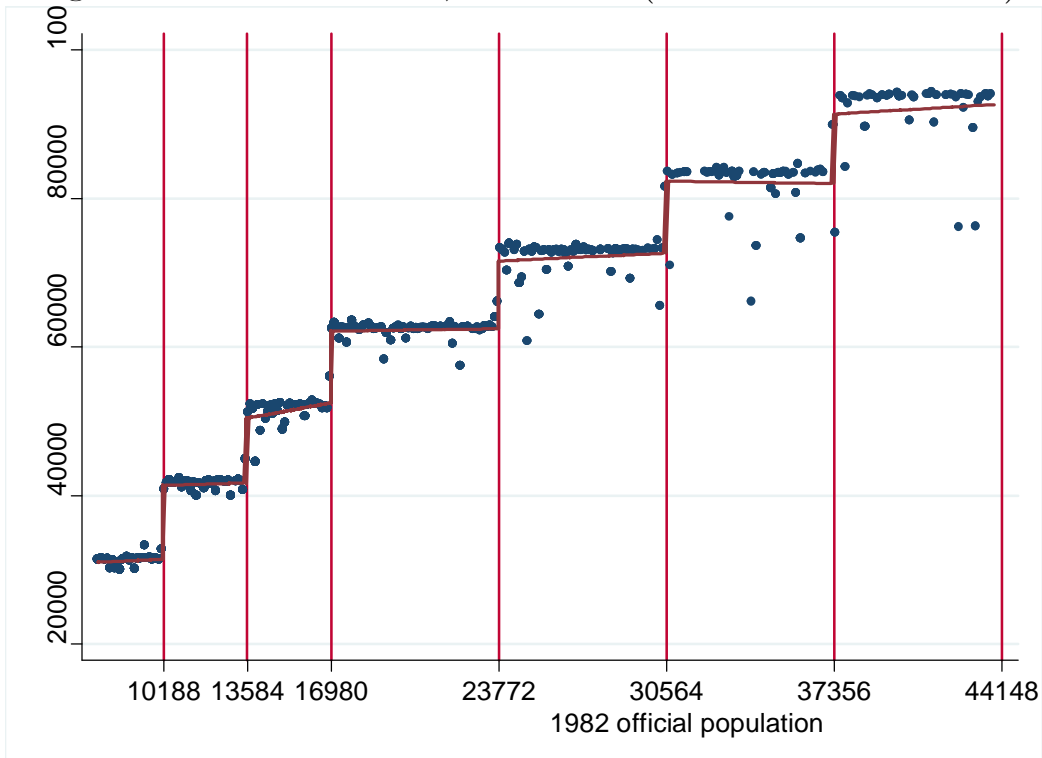


Figure 2: FPM transfers timeline

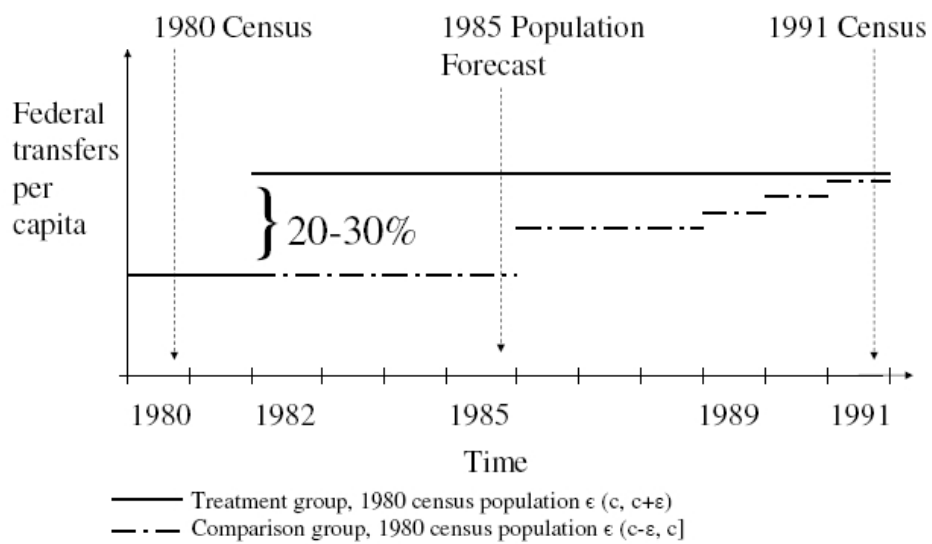


Figure 3: Histogram for 1982 official population

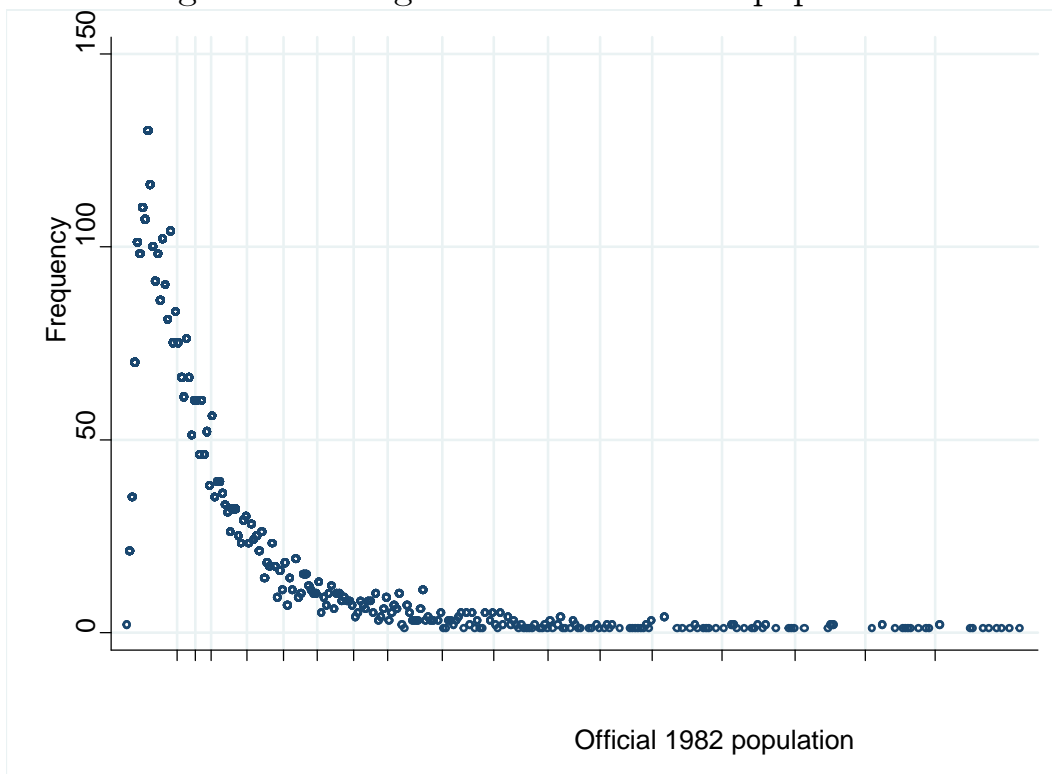


Figure 4: Histogram for 1982 official population, small to medium municipalities

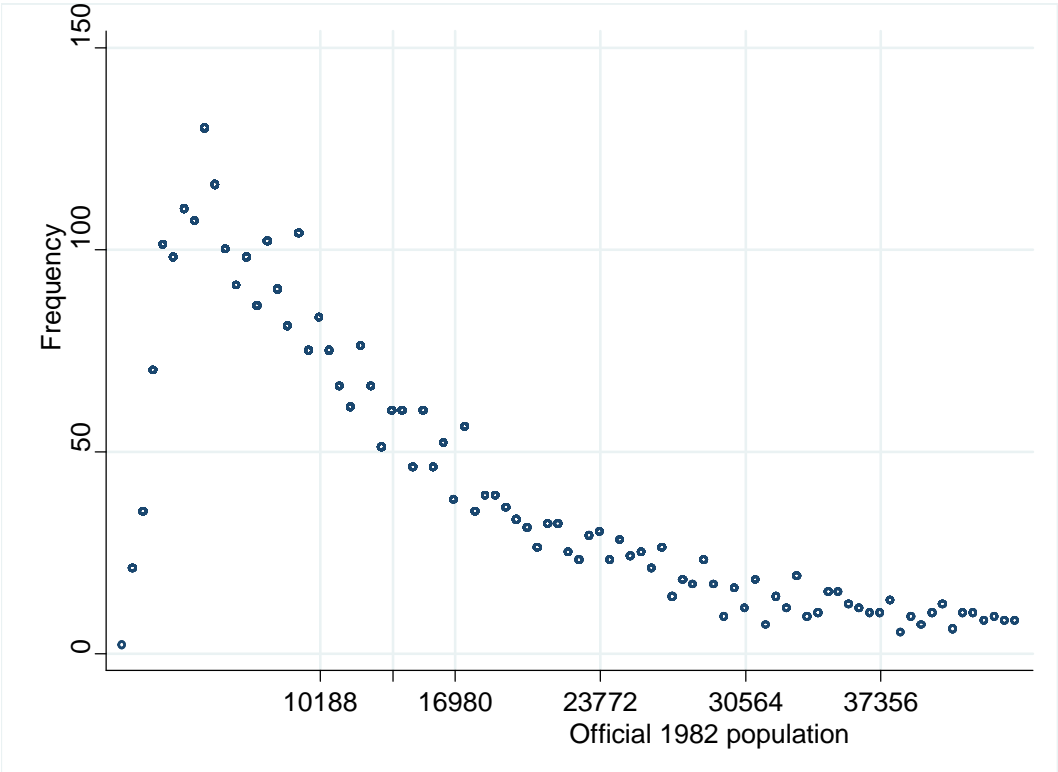


Figure 5: Re-election discontinuity plot for full sample of municipalities

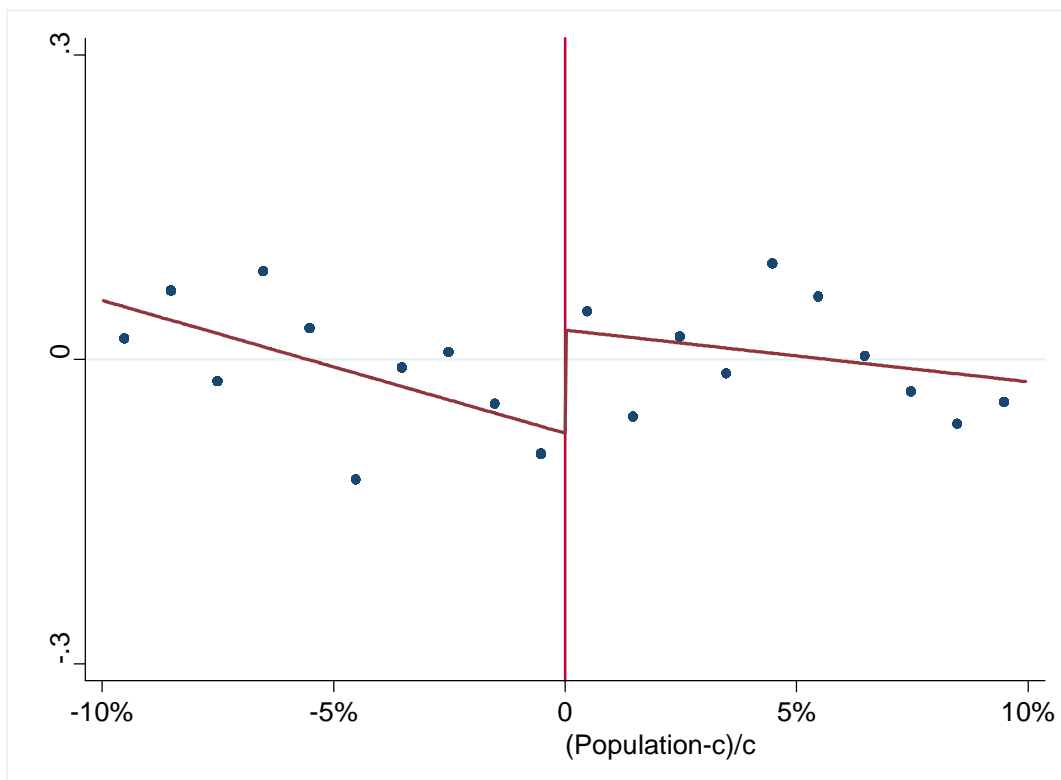


Figure 6: Re-election discontinuity plot for municipalities run by PDS mayors from 1982-1988

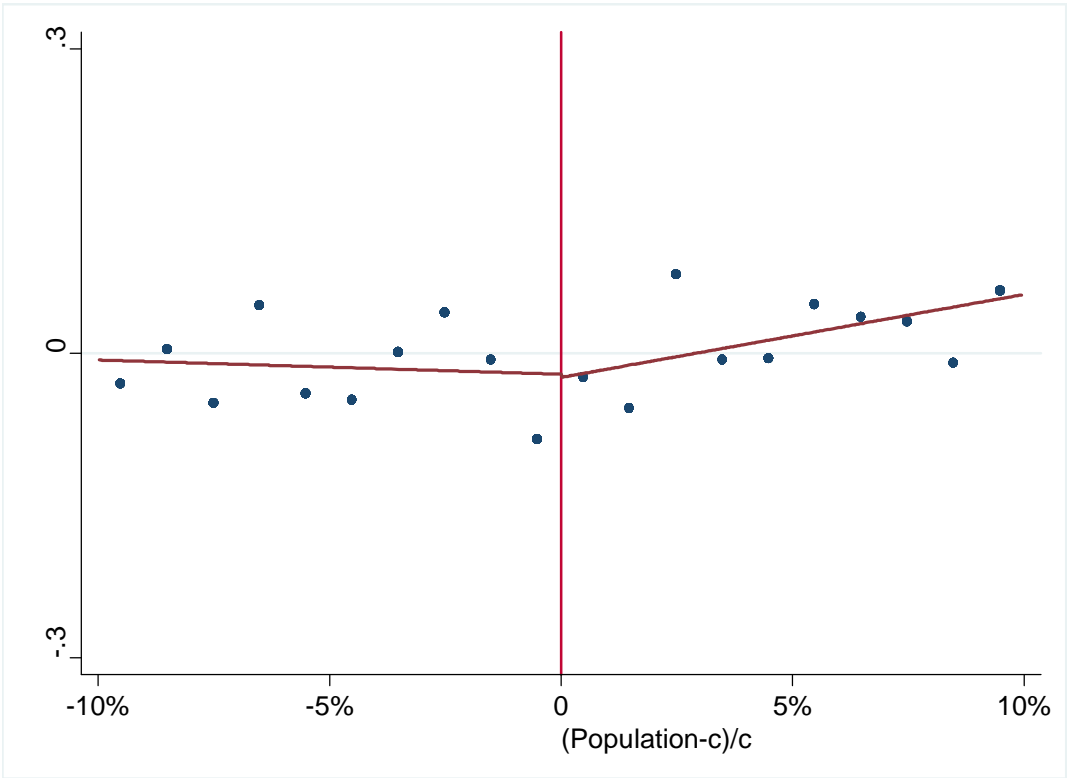


Figure 7: Re-election discontinuity plot for municipalities run by opposition mayors from 1982-1988

