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Stephan Litschig
Kevin Morrison

This version: February 2012
November 2010

Barcelona GSE Working Paper Series

Working Paper nº 515

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Stephan Litschig† Kevin Morrison‡

February 11, 2012

Abstract

Does additional government spending improve the electoral chances of incumbent political parties, and if so, through what channels? This paper provides the first quasi-experimental evidence on these questions. Our research design exploits discontinuities in federal funding to local governments in Brazil around several population cutoffs over the period 1982-1985. We find that extra fiscal transfers resulted in a 20% increase in local government spending per capita, and an increase of about 10 percentage points in the re-election probability of local incumbent parties. We also find positive effects of the government spending on education outcomes and earnings, which we interpret as indirect evidence of public service improvements. Together, our results suggest that expected electoral rewards encouraged incumbents to spend part of additional revenues on public services valued by voters, a finding in line with agency models of electoral accountability.

Keywords: Government spending, voting, regression discontinuity

JEL: H40, H72, D72

*A 2008 version of this paper using the same research design was entitled “Intergovernmental Transfers and Electoral Outcomes: Quasi-Experimental Evidence from Brazilian Municipalities, 1982-1988”. We are grateful for comments and suggestions from Daniel Benjamin, Francesco Caselli, Antonio Ciccone, Steve Coate, Marcel Fafchamps, Brian Fried, Philip Keefer, Fernanda Leite Lopez de Leon, Dina Pomeranz, Giacomo Ponzetto, Albert Solé-Ollé, Pilar Sorribas-Navarro, Joseph Stiglitz, and seminar participants at the 2010 CEPR DE workshop in Barcelona, NEUDC 2009 Tufts, the Institut d’ Economia de Barcelona at Universitat de Barcelona, Universitat Pompeu Fabra, the Leitner Political Economy Seminar at Yale University, Cornell University, and the 2008 American Political Science Association Meetings. David Samuels graciously shared his electoral data with us. All errors are our own.

†Universitat Pompeu Fabra and Barcelona GSE, stephan.litschig@upf.edu.

‡Cornell University, Department of Government, morrison@cornell.edu.

1 Introduction

Does additional government spending improve the electoral chances of incumbent political parties, and if so, what are the mechanisms? Existing empirical studies shed little light on either of these questions. In addition to coming to a variety of conclusions regarding the relationship between spending and electoral support—both positive and negative correlations have been found—the source of variation in government spending is never identified, leading to two main concerns regarding causal interpretation of existing estimates.[1] The first problem, often acknowledged in the literature, is unobserved heterogeneity of incumbent politicians, which might lead to omitted variable bias.[2] The second problem, which has been less appreciated in the literature, is reverse causality. This would arise, for example, if a strong electoral challenge induced the incumbent to raise spending in the hopes of gaining electoral support, leading to a downward biased estimate of the electoral effect of government spending.[3] Moreover, the empirical literature on government spending and electoral support fails to investigate the channels through which higher spending operates, making the findings difficult to interpret even if existing estimates were regarded as causal.

This paper is the first to address the identification challenge using a quasi-experimental research design. While the ideal design would be one in which extra spending is randomized across governments, such an experiment is unlikely to happen. Instead, our study attempts to approximate experimental conditions by exploiting variation in spending that is "as good as" randomized locally around a population threshold (under relatively weak, and to some extent testable, assumptions). Specifically, we analyze the effect of additional local government spending (mainly on education, housing and urban infrastructure, and transportation) on the re-election probability of local incumbent parties in the Brazilian municipal mayoral elections of 1988.[4] Our research design takes advantage of the fact that a substantial part of national tax revenue in Brazil is distributed to local

[1]Niskanen (1975), Peltzman (1992), Levitt and Snyder (1997), Matsusaka (2004), Akhmedov and Zhuravskaya (2004), Sakurai and Menezes-Filho (2008), Solé-Ollé and Sorribas-Navarro (2008), Jones, Meloni, and Tommasi (2009).

[2]For example, higher spending in certain jurisdictions may be the result of more greedy politicians extracting higher taxes and "spending" more, but siphoning off most of that spending into their own pockets. The observed correlation between government spending and electoral outcomes would then be biased downwards, since greedy politicians will provide fewer public services per dollar extracted and hence face a lower equilibrium re-election probability (Jones, Meloni, and Tommasi 2009).

[3]A similar downward bias would result if local jurisdictions received greater resources when they are swing constituencies (jurisdictions that are most susceptible to economic benefits), particularly if the allocating government equates swing constituencies with close elections.

[4]Municipalities are the lowest level of government in Brazil (below the federal and state governments). The discussion refers to counties, communities, and municipalities interchangeably.

governments strictly on the basis of population, via a formula based on cutoffs. That is, if a municipality's population is over the first population cutoff, it receives additional resources, over the second threshold a higher amount, and so forth. The transfer mechanism results in discontinuities in per capita central government funding and local spending around the population cutoffs over the period 1982-1985.[5] We exploit these jumps to estimate electoral effects using regression discontinuity analysis. The paper therefore provides the most credible estimates to date of the causal link between government spending and electoral outcomes,

Our main empirical result is that additional local government spending per capita of 20% improved the re-election probability of local incumbent parties in the 1988 elections by about 10 percentage points. The validity of this result, and our analysis in general, hinges on the identifying assumption that municipalities had (at most) only imprecise control over the number of local residents. We discuss in detail the plausibility of this untestable assumption in Section 3. The validity of our analysis also requires an exclusion restriction, which is that additional funding affects the probability of re-election only through local public spending and not through other channels, such as local tax breaks.[6] We show below that additional transfers increased local public spending essentially one-for-one. That is, local own revenue did not respond to extra transfers at all, so this particular exclusion restriction seems to hold.[7]

Our paper most directly builds on the empirical literature analyzing the electoral effects of government spending. As mentioned above, existing empirical evidence on electoral effects of government spending is mixed, with several studies even finding negative correlations. Such a negative correlation between government spending and electoral outcomes was originally found by Niskanen (1975) and Peltzman (1992) at the state level in the U.S. and confirmed in subsequent work by Matsusaka (2004). In contrast, several other recent studies have found a positive corre-

[5]We use the 1982-1985 period because, starting in 1988, official population estimates were updated annually, and so the magnitude of the variation in funding at the cutoffs was significantly reduced (Supplementary Law n^o 59/1988). In addition, there is strong evidence of manipulation of the 1991 estimates, which determined transfers through the entire decade of the 1990s and beyond (Litschig 2008b).

[6]Local governments were running essentially balanced budgets at the time so the extra transfers were neither saved nor used to pay back existing liabilities.

[7]The result that spending increases essentially one-for-one with extra transfers is referred to as the "flypaper effect" and has been found in many previous studies on intergovernmental grants and local spending, as reviewed in Hines and Thaler (1996) for example. The result is perhaps not very surprising for the relatively small local governments considered in this study, since they collect only about 6% of total revenue from their own residents and therefore have only little room to give tax reductions. We cannot say whether such low own-revenue collection represents an optimal choice or whether it reflects an inability to raise more revenue locally.

lation between government spending and electoral outcomes (Akhmedov and Zhuravskaya 2004; Sakurai and Menezes-Filho 2008; Jones, Meloni, and Tommasi 2009).[8]

Two papers, of which we are aware, deal with endogeneity of government spending using an IV approach, instrumenting for spending in a given district with spending outside the district (but inside the state or region containing the district). The first is Levitt and Snyder's (1997) pioneering work, which finds that federal spending benefits U.S. House of Representative incumbents. The other paper using essentially the same research design is by Solé-Ollé and Sorribas-Navarro (2008), who investigate electoral effects of capital grants in Spain. They find that incumbent parties in both grantor and grantee (recipient) governments benefit electorally from capital grants, although only when they are politically aligned. None of these papers provide any tests of their identifying assumptions, nor do they pay much attention to causal mechanisms.

There are indeed several causal mechanisms that might explain the electoral effect we find. One is proposed in a recent paper by Brollo, Nannicini, Perotti, and Tabellini (2010), who build on our work by exploiting the same funding discontinuities for identification as we do, albeit in a later period. They use the audit reports in Ferraz and Finan (2008, 2010), to show that municipalities that got a windfall of the same unrestricted funds analyzed here also experienced a roughly proportional increase in public management irregularities.[9] They argue that the expected extra transfers led inferior candidates to run for office, which in turn improved re-election prospects of incumbents.[10] For this political selection mechanism to explain the electoral effect we find, the treatment of extra transfers (and associated rents) would have to have occurred after the election, with the candidates being fully aware of which municipalities would receive the extra transfers. But this was not the case in our study. As discussed in more detail below, our treatment occurred in 1982-85; by the time of the election in 1988, the funding discontinuities between treatment and comparison groups had long disappeared and would not reappear.

A different possible causal mechanism underlying our results is that much of this spending went to fund political patronage, benefiting only certain groups of society. We find this eminently plau-

[8]In addition, positive correlations between certain budget categories, such as investment expenditures, and electoral outcomes have been found by Brender (2003), Veiga and Veiga (2007), and Drazen and Eslava (2010).

[9]In a similar vein, Caselli and Michaels (2009) argue that additional local public spending financed through oil royalties had disproportionately small effects on public services and household income per capita, which they interpret as indirect evidence of rent extraction by incumbents.

[10]Brollo et al. (2010) make it relatively clear that our paper, not theirs, is the first to use the funding discontinuities to look at electoral effects. See the last paragraph on page 3 in the 2010 version of their paper.

sible. Caselli and Michaels (2009), for example, study local public spending financed through oil royalties in Brazil, and some of the evidence they find is consistent with an expansion of patronage spending. Moreover, much other work on the effects of windfall revenues emphasizes how they expand the ability of governments to spend on certain groups in order to stay in power (Ahmed Forthcoming, Ross 2001). While much of this work has focused on authoritarian regimes, recent work has looked at such effects in the contexts of democracies, finding similar effects (Gervasoni 2010, Goldberg, et al. 2008).

While we think that patronage almost certainly played a role in spending's effect on re-election in the case considered here, in this paper we suggest a complementary causal mechanism that has been largely ignored in the literature. As we discuss in more detail in Section 3, the positive electoral effect of government spending we find is consistent with a political agency model in which voters are imperfectly informed about the state of the budget—that is, what side of the cutoff they are on (Persson and Tabellini 2000). In this model, windfall revenues are associated with increased corruption, consistent with prior literature. However, the re-election effect is driven not by corruption (or patronage) but rather by part of the increased revenue being spent on public services (Barro 1973; Ferejohn 1986; Persson and Tabellini 2000; Besley 2006). We investigate this additional causal mechanism by also examining the effect of government spending on public services. Of course, finding evidence of public service effects by themselves would not be proof that this was the only mechanism at work behind the electoral effect. Nevertheless, it would suggest an importantly different dynamic than the ones usually emphasized in the literature. If all of these revenues were being pocketed or spent on patronage, one would not expect to see significant public service improvements. In contrast, evidence of public service and electoral effects together would suggest that expected electoral rewards encouraged incumbents to spend at least part of additional revenues on public services valued by voters.

In order to gain some insight into whether public services improved, we build on earlier work by Litschig (2008a, 2011) who investigates whether extra government spending affects household income and municipal education outcomes, as measured by community average schooling and literacy rates.[11] We think of education outcomes and earnings as indirect measures of public services:

[11] We look at measures of behavioral responses instead of using public service measures because it is difficult to know what public services the money funded—that is, it is difficult to know what the "right" services would be to examine.

extra public spending on education might improve the quality of local schools, thus increasing the marginal benefit of education for any given level of schooling (Behrman and Birdsall 1983). At the same time, other public inputs, such as transportation, might reduce the marginal cost of schooling, thus increasing households' equilibrium schooling choice (Birdsall 1985; Behrman, Birdsall, and Kaplan 1996). Our results suggest that the relevant school-age cohorts acquired about 0.3 additional years of schooling per capita (a 7% increase), and literacy rates increased by about four percentage points on average (compared to a 76% literacy rate in the comparison communities). In line with the effect on human capital, the poverty rate (measured relative to the national income poverty line) was reduced by about four percentage points from a comparison group mean poverty rate of 64%. Income per capita gains were positive but not statistically significant. As discussed in Section 7 below, the magnitude of these education and earnings gains seems inconsistent with the hypothesis that only a small minority in the community benefited from extra spending.

In sum, in addition to providing the most credible estimates to date of the causal link between government spending and electoral outcomes, the paper advances a causal mechanism that has been underappreciated in the literature. While increasing government spending may lead to re-election through patronage or other disreputable means, under conditions of electoral accountability it may also do so through the improvement of public good provision. The implications of this mechanism are important particularly in light of much literature on the "resource curse," which has largely emphasized the negative aspects of windfall government spending.

The paper proceeds as follows. Section 2 provides background on the political context of the 1988 Brazilian elections, the public services provided by local governments, as well as their financing. Section 2 also gives a description of the revenue sharing mechanism we examine. In Section 3, we present a simple retrospective voting model to frame our work, and we discuss the identifying assumptions for a causal interpretation of our estimates. Section 4 describes the data. Section 5 discusses the estimation approach and Section 6 evaluates the internal validity of the study. Section 7 presents the empirical results. We conclude with a discussion of limitations and extensions.

2 Background

2.1 Political context and party re-election

As discussed above, our first goal is to estimate the effect of additional local public spending on the re-election probability of local incumbent parties in the Brazilian municipal mayoral elections of 1988. For a variety of reasons, the 1988 local executive elections represent a difficult environment in which to find an electoral effect, so that this might be considered a "least likely" case (Eckstein 1975).[12] To begin with, because of weak term limit rules, incumbent mayors could not be individually re-elected to serve consecutive terms, although they could be elected again after skipping one term. In a consecutive term, citizens could only re-elect the party of the mayor, which is how we code our dependent variable (1 for re-election, 0 otherwise). Satisfaction with parties was particularly low, however, and party identification in Brazil faces particularly strong challenges in general (Kinzo 1993; Shidlo 1998). As Moisés (1993: 577) puts it, “Brazilians don’t vote for parties, they vote for people.” In fact, public opinion surveys show that the percentage of the population agreeing that in its own vote choice, ’the candidate’s party is the decisive factor’ had declined from 43% in 1982 to 24% in 1986, and to 18% in 1988 (Muszynski and Teixeria Mendes 1990: 64, cited in Ames 1994: 95). Perhaps not surprisingly, party switching by politicians in Brazil was rampant around this time: Mainwaring (1991) reports that during the 1987-1990 Congress, about one-third of the 559 representatives switched parties.

Another complication is that 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. 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 thus be the first in over two decades in which the PDS was not in control of the central government.

Change at the national level had been reflected at the local level. As Table 1 shows, the PDS had won mayoral elections 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

[12] Footnote 5 explains why we do not explore electoral effects of these transfers in later years.

and other select municipalities in 1985, the party essentially disappeared from major urban areas, the result of a 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. This decline would continue in 1988, when PDS candidates would be elected to the mayor's office in a mere 10% of municipalities (see Table 1), leaving a void that was filled by an explosion of new parties. While the period of the dictatorship had seen electoral "competition" limited to only a few parties, voters in 1988 chose from 31 political parties—sixteen of which were winners somewhere in the country—to elect mayors in about 4000 municipalities.

2.2 Local public services and their financing

These local elections were important to voters because municipal budgets in Brazil are essential to many services valued by voters. For example, public provision of elementary education in the early 1980s was for the most part a joint responsibility of state and local governments, while the federal government was primarily involved in financing and standard setting. In 1980, 55% of all elementary school students in Brazil were enrolled in state administered schools, 31% in municipality schools, and the remaining 14% in private schools. In small and rural municipalities, such as those considered here, the proportion of students in schools managed by local governments was 74%, while the proportions for state-run and private schools were 24% and 2% respectively (World Bank 1985).

In all, over our study period of 1982-1985, local governments managed about 17% of public resources in Brazil (Shah 1991), about four percent of GDP, with 20% of local budgets going to education and similar shares to housing and urban infrastructure, and transportation spending, as shown in Table 2.[13] Most of these resources accrued to the local governments through intergovernmental transfers, since municipalities have never collected much in the way of taxes. The most important among these transfers was the federal Fundo de Participação dos Municípios (FPM), a largely unconditional revenue sharing grant funded by federal income and industrial products

[13]Local governments also provided some primary health care services (about 10% of local budgets). Local welfare assistance was close to negligible.

taxes.[14] This grant accounted for about 50% of the revenue of the municipalities in our analysis, as shown in Table 2.

2.3 Mechanics of revenue sharing

In order to estimate the electoral and public service responses to public spending increases, we exploit variation in FPM funding at several population cutoffs using regression-discontinuity (RD) analysis. 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 $k = k(pop^e)$, where k(.) is a step function of population. 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 $k(pop^e)$ determines the share of total FPM resources, rev_t, distributed to municipality m in year t according to the following formula:

$$FPM_{mt} = \frac{k(pop_m^e)}{\sum_m k_m} rev_t$$

This equation makes it clear that local population estimates should be the only determinant of cross-municipality variation in FPM funding. Exact county population estimates are only available for census years or years when a national population count is conducted. Transfers were allocated based on 1980 census population from 1982 (the first year the 1980 census figures were used) until 1985.[15] Previously, from 1976 to 1981, the transfers had been based on extrapolations from the 1960 and 1970 censuses, produced by the national statistical agency, IBGE.[16] Likewise, from 1986 to 1988, the transfers were also based on such extrapolations, this time based on 1970 and 1980 census population figures.[17] As a result of the update in 1986, the funding discontinuities for those municipalities around the cutoffs based on the 1980 census disappeared because many

[14] 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.

[15] The 1985 official estimates were already based on extrapolations which resulted in minor changes compared to the 1980 census numbers.

[16] The methodology used by the statistical agency in principle ensures that population estimates are consistent between municipalities, states, and the updated population estimate for the country as a whole (Instituto Brasileiro de Geografia e Estatística 2002).

[17] Beginning in 1989 the population estimates were updated on a yearly basis.

municipalities changed brackets due to decreases or, more often, increases in their population relative to 1980.[18] The "treatment" therefore consists of a (presumably) unexpected temporary funding windfall to the municipal budget, which lasted for four years from the beginning of 1982 through the end of 1985.

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 a revenue sharing fund for the *municípios* 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—that is, 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—that is, why 10’188, 13’584, 16’980, and so forth? 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 2’000 up to 10’000, then every 4’000 up to 30’000 and so forth. The legislation also stipulated that these cutoffs should be updated proportionally with population growth in Brazil.[19] 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 noteworthy that the thresholds during our study period are still equidistant from one another, the distance being 6’792 for the first seven cutoffs (except for the second cutoff, which lies exactly halfway in between the first and the third cutoffs).

[18] To be clear, there are no economically or statistically significant differences in FPM transfers between the treatment and comparison group (those around the first three cutoffs based on the 1980 census) from 1986 onwards.

[19] 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 in Decree 1881/81. Figure 1 plots cumulative FPM transfers over the period 1982 to 1985 against 1982 official population. The horizontal lines correspond to the modal levels of cumulative transfers for each bracket in our data. The figure shows that funding jumps by about 1’320’000 Reais (2008 prices) or about 1’000’000 international US$ at each threshold over this period.[20] Observations that appear above or below the horizontal lines are most likely due to measurement error, because transfer data in this figure are self-reported by municipalities, rather than based on administrative records of the Ministry of Finance, which are not available for the period considered.[21] The cumulative transfer differential over the period 1982-1985 corresponds to about 2.5% of annual GDP in rural areas of the country and about 1.4% of annual GDP in urban areas for the counties in our estimation sample (Table 2).

Although the funding jump is the same in absolute terms at each cutoff, the jump declines in per capita terms the higher the cutoff. As is apparent from Figure 1, funding jumps by about R$ 130 (US$ 95) per capita at the first threshold, R$ 97 (US$ 70) at the second, R$ 78 (US$ 57) at the third, and declines monotonically for the following cutoffs. Immediately to the left of the first three cutoffs, per capita FPM funding is about R$ 390 (286 US$), and this amount declines monotonically for the following cutoffs. For the first three cutoffs the funding increase per capita is therefore from the same baseline level and represents about 33% at the first, 25% at the second, and 20% at the third cutoff. Though the differences are not great, this means that the treatment in terms of additional per capita funding is not exactly the same across these cutoffs. However, since there are likely to be economies of scale in the provision of local public services—that is, unit costs decline with scale—the differences in treatment across cutoffs might be even smaller than what the per capita funding jumps would suggest. It thus seems reasonable to expect similar treatment effects around these cutoffs, as further discussed in Section 5 below.

[20]The 2005 Real/$ PPP exchange rate was about 1.36 (World Bank 2008).

[21]For later periods the data is available from the Ministry of Finance, and in these data there is essentially no variation in FPM transfers for a given state and population bracket.

3 Theoretical framework and identification

3.1 Theoretical framework

In order to frame our analysis, this section presents a simple rational retrospective voting model in the spirit of Barro (1973), Ferejohn (1986), and Persson and Tabellini (2000).[22] While our model captures essential elements and predictions of these classic agency models of electoral accountability—highlighting the implications of electoral incentives for government spending and public service provision—we develop the model in a way that facilitates comparison with our research design and allows us to illustrate how our work relates to existing empirical studies. We particularly draw on the model of Jones, Meloni, and Tommasi (2009), which we slightly adapt for our purposes.

We consider an incumbent mayor who values current political rents from holding office r (purely private consumption), and whose selfishness is parameterized by γ, ranging from 0 (unselfish or benevolent type) to infinity (extremely selfish type). We assume that γ is known to voters and might vary across municipalities. The incumbent also cares about future rents R, which become available through rent-sharing within the party if and only if the party is re-elected (R might also include an ego rent such as prestige for keeping the mayor's office in the hands of the party).[23] Party re-election happens with probability p. The incumbent's welfare is therefore given by:

$$W = \gamma\, w(r) + pR \tag{1}$$

The incumbent spends revenue g on public services b, valued by the representative voter, and rents r. We assume that the level of revenue is exogenous, to focus on the incumbent's allocation decision (between b and r) rather than extraction (from the voter's private income). This assumption, namely that government spending is financed exclusively through intergovernmental transfers (or other windfall revenue), approximates reality at the local level of government for many coun-

[22] We focus on an agency model because we believe it captures the underlying dynamic of our research design, which examines the effects of transfers over a certain period on an election in a later year. As discussed in the introduction, the political selection model proposed by Brollo et al. (2010) cannot account for the electoral effect we find.

[23] If R becomes available only in the event of individual re-election in a later period (politicians could be re-elected after skipping one term), the incumbent's welfare is given by $W = \gamma\, w(r) + E(R) = \gamma\, w(r) + p\lambda R + (1-p)\delta R$, where λ denotes the probability of individual re-election if the party was re-elected and δ is the probability of re-election if the party was not re-elected. As long as $\lambda > \delta$—meaning that it is easier to get re-elected later if your party was re-elected than if it was not re-elected—it pays for incumbents to provide public services while in office.

tries, including Brazil (Rodden 2004). The budget constraint is therefore:

$$g = b + r \tag{2}$$

The re-election probability p depends on the voter's satisfaction with the incumbent's performance. Voter satisfaction is increasing in b. Voter satisfaction also has a random component α to it, capturing uncertainty about the mapping from policy choices to electoral outcomes for the incumbent. For simplicity, we assume that α is distributed uniformly on the unit interval. Utility of the voter is then given by:

$$U = u(b) + \alpha$$

The agency models of electoral accountability cited above assume that re-election depends on whether or not the voter's utility is above her "reservation utility". This, in turn, can depend on whether the conditions for public good provision are good or bad. In Persson and Tabellini (2000, chapter 4.4), for example, the focus is on exogenous conditions that lower or raise the cost of public service provision. In our model, we focus on whether exogenous government revenue g is high or low. This captures the essential element of our research design, which examines municipalities around cutoffs where per capita financing jumps substantially. The parallel between the models is straightforward: both low costs of service provision and high exogenous funding expand potential service levels.

Whether or not the voter's reservation utility takes into account the conditions for public service provision depends on whether or not these conditions are known by the voter—that is, by the information environment of the model. If voters are perfectly informed about the conditions, they adjust their reservation utility to take account of more or less favorable circumstances for the incumbent. Alternatively, when voters do not know the conditions for public good provision (that is, they are imperfectly informed), "the best they can do is to choose a non-state-contingent cutoff level for their utility" (Persson and Tabellini 2000: 79). In other words, the reservation utility does not depend on the state of the budget.

We believe it more plausible that at least a substantial fraction of voters, if not most, in municipalities close to the cutoffs in Brazil were not sure what side of the cutoff they were on and hence whether funding was high or low. It is useful to know in this context that illiteracy rates were about 40% on average in the relatively small municipalities considered here (Table 2). Moreover, access to exact population numbers and updated cutoffs was difficult at the time due to limited availability of information technology and there was no requirement for the local government to make the level of FPM-funding public—in contrast to project-specific transfers received from the central government. As such, we model voters' reservation utility as not depending on g, and call that reservation utility $\overline{U}$. Under this assumption, the re-election probability of the incumbent is given by:

$$p = \Pr\left[u(b) + \alpha \geq \overline{U}\right] \tag{3}$$

In this type of model, when the reservation utility is not state-contingent, an incumbent's re-election probability and level of public service provision depend on the state in which he finds himself. In good states (if the cost for public service provision is low, or exogenous government funding is high), the incumbent can provide enough public goods to meet the reservation utility of the voter and get re-elected, as well as siphon off any remaining revenue for himself. In bad states, however, providing the level of public goods necessary to meet the reservation utility is not possible, so the incumbent allocates all revenue to rents and accepts defeat in the election. This mechanism therefore generates positive correlations between the state variable, public service provision, and re-election (Persson and Tabellini 2000, chapter 4.4).[24]

This same prediction is generated by our model, as can be seen by solving the incumbent's problem of choosing r and b to maximize (1) subject to (2) and (3). In order to obtain simple closed form solutions we assume logarithmic functional forms for both $w(r)$ and $u(b)$. These solutions are:

$$r^* = \frac{\gamma}{R+\gamma} g$$

$$b^* = \frac{R}{R+\gamma} g$$

$$p^* = 1 + \ln\left(\frac{R}{R+\gamma} g\right) - \overline{U}$$

[24] These positive correlations are partially robust to the information environment of the model. If voters are assumed to have perfect information about the conditions for public service delivery (in contrast to our model), there is still a positive correlation between the state variable and public service provision, but not between the state and re-election (Persson and Tabellini 2000). A positive correlation between the state and re-election can, however, be generated even with voters' perfect information about the state variable in a model of political selection, whereby higher rents attract lower quality challengers who make it easier for the incumbent to retain office (Brollo, Nannicini, Perotti, and Tabellini 2010). As discussed in the introduction, however, this mechansims cannot account for the electoral effect we observe in this paper.

The equations above reflect the two goals of our paper. First, we seek to test whether $\frac{\partial p^*}{\partial g}$ is positive as predicted by our model. Second, we seek to test an additional empirical implication of the model, which is that $\frac{\partial b^*}{\partial g}$ should also be positive. The existing literature exclusively focuses on the electoral effect of government spending, but without a simultaneous examination of public service effects, we cannot know what is causing whatever electoral effect is found.

Our simple framework also helps illustrate the advantages of our research design over other existing studies, which are likely plagued by bias due to omitted variables and reverse causality. For example, the omitted variable bias problem can be understood by considering a variant of the model above with tax-financed public spending—realistic for state or national governments. In such a model, higher spending might be the result of greedy politicians (higher γ types), who extract higher taxes and political rents but provide less public service per dollar extracted, thus having a lower equilibrium re-election probability (Jones, Meloni, and Tommasi 2009). The reverse causality problem, in turn, can be seen by examining the effect of $\overline{U}$. A strong electoral challenge could raise $\overline{U}$ and lower p, inducing the incumbent to raise spending in the hopes of gaining electoral support.[25]

Heterogeneity in γ or $\overline{U}$ across jurisdictions would therefore likely lead to a downward biased estimate of $\frac{\partial p^*}{\partial g}$, and possibly even to the negative correlation between observed spending and electoral outcomes found by Peltzman (1992) and other studies mentioned above. For studies at the subnational level, the likely bias is upwards. Local jurisdictions that manage to expand public spending—essentially by extracting resources from the center—might be those that are better managed overall, leading to a spurious positive correlation of government spending with electoral outcomes. With our research design, in contrast, unobservables related to the type of incumbent or to the strength of the electoral challenge are unlikely to be problematic, because g is "as good as" randomly assigned around the population cutoffs if municipalities had (at most) only imprecise control over the number of local residents, as discussed in the next section.

[25] This is, admittedly, an incomplete argument since our model does not capture the effort and spending responses of incumbents to more fierce electoral competition. Nevertheless, we believe the point is valid from an empirical perspective. See Levitt and Snyder (1997) for a more extensive discussion.

3.2 Identification

The basic intuition behind the regression discontinuity approach is that, in the absence of program manipulation, municipalities to the left of the treatment-determining population cutoff should provide valid counterfactual outcomes for counties on the right side of the cutoff (which received additional resources). More formally, let Y denote an outcome variable at the municipality level (party re-election, average schooling, or poverty rate), τ the (constant) treatment 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 unobserved factors that affect outcomes. The model is as follows:

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

If the potential regression functions $E[Y|D = 1, pop]$ and $E[Y|D = 0, pop]$ are both continuous in population, or equivalently, if $E[u|pop]$ is continuous, then the difference in conditional expectations identifies the treatment effect at the threshold:[26]

$$\lim_{pop \downarrow c} E[Y|pop] - \lim_{pop \uparrow c} E[Y|pop] = \tau \tag{4}$$

With a continuous endogenous variable, such as local public spending g, the model is as follows:

$$\begin{aligned} Y &= \frac{\partial Y}{\partial g} g + f(pop) + u \\ g &= \pi D + \upsilon \\ D &= 1[pop > c] \end{aligned}$$

where $\frac{\partial Y}{\partial g}$ represents the causal effect of government spending g on Y, π represents the jump in spending that occurs at the cutoff, and υ represents other factors that affect g. Under the continuity

[26]With heterogeneous treatment effects, the RD gap identifies the average treatment effect at the cutoff. See Lee (2008) for an alternative interpretation of the treatment effect identified in this case 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.

assumption above, the difference in conditional means of Y at the cutoff is now

$$\lim_{pop\downarrow c} E[Y|pop] - \lim_{pop\uparrow c} E[Y|pop] = \left\{ \lim_{pop\downarrow c} E[g|pop] - \lim_{pop\uparrow c} E[g|pop] \right\} \frac{\partial Y}{\partial g} \quad (5)$$

If government spending is the only channel through which additional transfers operate (the exclusion restriction), the ratio of jumps in Y and g identifies $\frac{\partial p^*}{\partial g}$, the impact of local public spending on the re-election probability, and $\frac{\partial b^*}{\partial g}$, the impact on public services. Reductions in local taxes and corresponding increases in private spending would violate this exclusion restriction, for example. However, as shown in Section 7 below, local taxes do not seem to have responded to additional transfers. There is also no evidence that state or federal levels of government altered other governmental transfers around the cutoffs.

As emphasized in the introduction, we do *not* make the stronger—and indeed implausible—assumption that the extra local public spending affected electoral prospects exclusively through a particular channel, such as public service improvements or political patronage. Indeed, it is very plausible that some of the extra spending served to expand political patronage—thus helping the incumbent political party get re-elected—and some of the extra cash might also have helped to keep some kids in school. But the magnitude of the impacts on education and poverty discussed below seem inconsistent with the hypothesis that only a small minority in the community benefited from the extra government spending. What our results suggest, therefore, is that expected electoral rewards encouraged incumbents to spend *at least part of* additional revenues on public services valued by voters at large.

The most important assumption for this study concerns the continuity of the potential regression functions, or equivalently, the continuity of $E[u|pop]$, which gives the estimands in equations (4) and (5) above a causal interpretation. Intuitively, the continuity assumption requires that unobservables, such as γ or $\overline{U}$, vary smoothly as a function of population and, in particular, do not jump at the cutoff. As shown in Lee (2008) and Lee and Lemieux (2009), sufficient for the continuity of the regression functions (or the continuity of $E[u|pop]$) is the assumption that individual densities of the treatment-determining variable are smooth. In our case, this assumption explicitly allows for mayors or other agents in the municipality to have some control over their particular value of population. As long as this control is imprecise, treatment assignment is randomized around the cutoff.

In our case, the continuity of individual population density functions also directly ensures that treatment status (extra transfers) is randomized close to the cutoff. (An additional concern would be imperfect compliance with the treatment rule, but in our study period all eligible municipalities received more FPM transfers, and none of the ineligible ones did.)

How reasonable is the continuity assumption in our context? Local elites in Brazil clearly had an incentive to manipulate, and presumably also some control over, the number of their local residents. It seems implausible, however, that this control was perfect, so the key identifying assumption is likely to hold here. It is also worth considering that under imperfect control, bringing people into the municipality is risky because there is always the chance that on census day the counted number falls just short of the cutoff and hence per capita funding actually falls. Moreover, even if local elites had perfect control over the number of residents in their municipality, the legislation specified that thresholds would be updated in accordance with population growth in the country as a whole *after* the release of the 1980 census results. Put differently, local elites were unlikely to know the exact locations of the new thresholds even if they wanted to manipulate their population count.

Still, one might worry that leaders in the central government had incentives to alter the cutoffs 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 cutoffs to benefit mayors of their party, there would have had to be places on the support of the municipality population distribution where aligned municipalities had a systematically higher density 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. In support of this contention, we show in Section 6 below that local governments that were run by the PDS, the party of the authoritarian regime that was in control of the central government until 1985, were not over-represented to the right of the cutoffs in our study period.[27]

A final potential concern is that other government policies are also related to the cutoffs specified in Decree 1881/81. If so, τ and $\frac{\partial Y}{\partial g}$ would reflect the combined causal effect of extra funding and other policies. To our knowledge, however, there are no other programs that use the same cutoffs, although some government programs and policies do use other local population cutoffs for

[27]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.

targeting.

4 Data

Our analysis draws on multiple data sources from Brazil. Population estimates determining transfer amounts over the period 1982-1988 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 2008 currency units using the GDP deflator for Brazil and taking account of the various monetary reforms that occurred in the country since 1980. Electoral data for the municipal executive 1982 and 1988 elections are from the Supreme Electoral Tribunal.

As discussed below, we include as pre-treatment covariates the 1980 levels of municipality income per capita, average years of schooling for individuals 25 years and older, the poverty head-count ratio, the illiterate percentage of people over 14 years old, the infant mortality rate, the school enrollment rate of 7- to 14-year-olds, and the percent of the municipal population living in urban areas. Data on these 1980 municipality characteristics are based on the 25% sample of the census and have been calculated by the national statistical agency (only a shorter census survey was administered to 100% of the population). The 1991 poverty rate was calculated by the government research institute IPEA[28] based on the 1991 census, using a poverty line of half the minimum wage in August 2000 (75.5 R$ at the time and about 140 R$ in 2008 prices) and household income per capita as the measure of individual-level income.

In addition, in analyzing the effects of additional spending on education outcomes, we use microdata from the 1991 census to compute municipality-level average years of schooling (that is, grades completed, not just "years in school") and the percent literate for the cohorts aged 19-28 years on census day (September 1st) in 1991. This was the cohort most likely affected by the public spending increase from 1982 to 1985, since the 19-year-olds in 1991 were about 10 years old in 1982 and hence in the middle of elementary schooling age (7-14), while the 29-year-olds

[28] Instituto de Pesquisa Econômica Aplicada.

were at least 19 years old (age 20 on September 1st 1982 but 19 at some point during the year 1982 for some) and hence ineligible to attend regular elementary school, which has a cutoff age at 18.

While we only include cohorts up to and including age 18 in 1982, older cohorts might have been affected by the additional spending as well, although likely to a lesser extent. For example, older cohorts might have gone to local secondary schools (although there are relatively few of them) or to state secondary schools paid for by the local government (World Bank 1985). Even those over the age of 21 (cutoff age for secondary schooling) in 1982 might have enrolled in adult literacy programs that were promoted by the military government and offered through the local administration, such as the MOBRAL (Movimento Brasileiro de Alfabetização). Nevertheless, one would expect smaller effects on education outcomes for cohorts that were beyond regular schooling age.

Table 2 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 50% on average and 56% in rural areas. Table 2 also shows that education spending accounts for about 20% of local budgets on average, with similar shares going to housing and urban infrastructure, and transportation spending.

5 Estimation approach

Following Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2008), our main estimation approach is to use local linear regression in samples around the discontinuity, which amounts to running simple linear regressions allowing for different slopes of the regression function in the neighborhood of the cutoff. Allowing for slope 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. We follow the suggestions by Imbens and Lemieux (2008) and use a rectangular kernel (i.e. equal weight for all observations in the estimation sample).

Because there are relatively few observations in a local neighborhood of each threshold, our RD analysis also makes use of more distant municipalities. 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, adding nonlinear specifications as a robustness check. We supplement the local linear estimates with higher order polynomial specifications, using an extended support, and we choose the order of the polynomial such that it best matches the local linear estimates in the discontinuity samples. Our main approach thus combines the advantage of local linear regression—comparing municipalities close to the cutoff, where local randomization of the treatment is most likely to hold but the variance of the estimates is relatively high—with the main advantage of using an extended support, namely sample size, which helps to reduce standard errors.

In the analysis that follows, we focus particularly on the first three population cutoffs (c_1 =10'188, c_2 =13'584, and c_3 =16'980). 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 municipality, as shown in Section 7 below. While we present results for the first three cutoffs individually, we also pool the municipalities across these cutoffs in order to gain statistical power.

Pooling requires the treatment intensity to be of comparable magnitude in order to interpret the size of estimated impacts.[29] As discussed above, although the funding jump is about 1'320'000 Reais (2008 prices) or about 1'000'000 international US$ at each cutoff, the treatment in terms of additional *per capita* funding is not the same across cutoffs. However, the differences across the first three cutoffs are not that large, and since there are likely to be economies of scale in the provision of local public services—that is, unit costs decline with scale—the differences in treatment across cutoffs are likely even smaller than what the differences in per capita funding jumps suggest.

The effects presented in this paper (as in any regression discontinuity analysis) apply only to the units near the relevant cutoffs—in our case, the municipalities near the population cutoffs. With

[29]Treatment effects need not be the same across cutoffs. If treatment effects are heterogeneous, the pooled estimates identify an average treatment effect across cutoffs.

similar treatment intensity it seems reasonable to expect similar treatment effects at least around the first few cutoffs, a testable hypothesis for which we find support below. Because our results are qualitatively similar across the first three cutoffs, it seems likely that the effects presented here generalize at least to the subpopulation of municipalities in the approximate population range 8’500-18’700, which represents about 30% of Brazilian municipalities at the time.

The specification we use to test the null hypothesis of common (average) effects across the first three cutoffs is as follows. Let seg_j denote the four integers (7’500, 11’800, 15’100, and 23’772) that bound and partition the population support into three segments; Y_{ms} an outcome in municipality m, state s; $\mathbf{z}_{ms}$ a set of pre-treatment covariates; a_s a fixed effect for each state; and u_{ms} an error term for each county. Neither covariates nor state fixed effects are needed for identification. We include them to guard against chance correlations with treatment status and to increase the precision of the estimates. The testing specification for a given percentage distance p from the cutoffs is then:

$$\begin{aligned} Y_{ms} = {} & [\tau_1 1[pop_{ms} > c_1] + \alpha_{10} pop_{ms} + \alpha_{11}(pop_{ms} - c_1) 1[pop_{ms} > c_1]]\, 1_{1p} \\ & + [\tau_2 1[pop_{ms} > c_2] + \alpha_{20} pop_{ms} + \alpha_{21}(pop_{ms} - c_2) 1[pop_{ms} > c_2]]\, 1_{2p} \\ & + [\tau_3 1[pop_{ms} > c_3] + \alpha_{30} pop_{ms} + \alpha_{31}(pop_{ms} - c_3) 1[pop_{ms} > c_3]]\, 1_{3p} \\ & + \sum_{j=1}^{3} \beta_j 1[seg_{j-1} < pop_{ms} \leq seg_j] 1_{jp} + \boldsymbol{\gamma} \mathbf{z}_{ms} + a_s + u_{ms} \end{aligned} \tag{6}$$

$$\begin{aligned} seg_0 &= 7500, seg_1 = 11800, seg_2 = 15100, seg_3 = 23772 \\ 1_{jp} &= 1[c_j(1-p) < pop_{ms} < c_j(1+p)], j = 1, 2, 3;\ p = 2, 3, 4\% \end{aligned}$$

Figure 2 illustrates the estimation approach. We fail to reject the null hypotheses $\tau_1 = \tau_2 = \tau_3$ at conventional levels of significance for all outcomes and in all specifications.

For the pooled analysis, we need to make observations comparable in terms of the distance from their respective cutoff. To do this, we rescale population to equal zero at the respective thresholds within each of the first three segments, and then use the scaled variable, X_{ms} (municipality m in state s), for estimation purposes:

$$X_{ms} = pop_{ms} - 10188 \text{ if } seg_0 < pop_{ms} \leq seg_1$$
$$pop_{ms} - 13564 \text{ if } seg_1 < pop_{ms} \leq seg_2$$
$$pop_{ms} - 16980 \text{ if } seg_2 < pop_{ms} \leq seg_3$$

$$\begin{aligned} Y_{ms} = \tau 1[X_{ms} > 0]1_p &+ [\alpha_{10}X_{ms} + \alpha_{11}X_{ms}1[X_{ms} > 0]]1_{1p} \\ &+ [\alpha_{20}X_{ms} + \alpha_{21}X_{ms}1[X_{ms} > 0]]1_{2p} \\ &+ [\alpha_{30}X_{ms} + \alpha_{31}X_{ms}1[X_{ms} > 0]]1_{3p} \\ + \sum_{j=1}^{3} \beta_j 1[seg_{j-1} &< pop_{ms} \leq seg_j]1_{jp} + \gamma \mathbf{z}_{ms} + a_s + u_{ms} \\ 1_p &= 1_{1p} + 1_{2p} + 1_{3p} \end{aligned} \tag{7}$$

Essentially this equation allows for six different slopes, one each on either side of the three cutoffs, but imposes a common effect τ. Under the continuity assumption above, the pooled treatment effect is given by $\lim_{\Delta \downarrow 0} E[Y|X = \Delta] - E[Y|X = 0] = \tau$. Both the pooled treatment effect and effects at individual cutoffs are estimated using observations within successively larger neighborhoods (larger p) around the cutoff in order to assess the robustness of the results.

6 Internal validity checks

Since extensive manipulation of the population estimates on which FPM allocations were based would cast serious doubts on the internal validity of the research design, we check for any evidence of sorting, notably discontinuous population distributions. Figure 3 plots the histogram for 1982 official municipality population up to the seventh cutoff. The bin-width in this histogram (283), is set to ensure that the various cutoffs coincide with bin limits. That is, no bin counts observations from both sides of any cutoff. Visual inspection reveals no discontinuities and the null hypothesis of a smooth density cannot be rejected at conventional significance levels for any of the first six cutoffs according to the density test suggested by McCrary (2008).[30]

[30] The estimates (and standard errors) are, for the first to sixth cutoffs respectively, -0.085 (0.098), -0.002 (0.112), 0.152 (0.135), 0.071 (0.167), -0.041 (0.253), 0.324 (0.344). Separate density plots for each cutoff are available on request.

In Table 3, we estimate equation (7) pooled across the first three cutoffs for a host of pre-treatment outcomes and other covariates. The results show that, in the samples with population of +/- 2 or 3 percentage points around the cutoffs, there is no statistical evidence of discontinuities in the 1980 pre-treatment covariates mentioned above. Nor is there statistical evidence of pre-treatment differences in the total public budget or its main components. While the 1981 public finance reports do not disaggregate transfers into FPM transfers and other categories, FPM transfers represent the bulk of current transfers, and so any discontinuities in pre-treatment FPM transfers should show up in 1981 current or capital transfers. Table 3 shows that such is not the case.

In the larger samples that include municipalities within +/- 4 to 6 percentage points, some individual discontinuities in Table 3 are statistically significant. This happens mostly due to larger point estimates compared to the smaller bandwidths, rather than lower standard errors, which suggests that these significant results might reflect a specification error.[31] Results from quadratic specifications (not shown but available on request) confirm this view: virtually none of the pre-treatment differences found in the 4 and 5 percent samples in Table 3 remain statistically significant, due to both lower estimates and higher standard errors. Moreover, all F-tests shown in Table 3 fail to reject the joint null hypotheses of no discontinuities in any pre-treatment covariate at conventional levels of significance (lowest p-value is 0.26).[32] In other words, from a statistical point of view, there is no evidence that treatment group municipalities were systematically different in terms of local development or overall public resources from municipalities in the marginal comparison group in the pre-treatment period.

Nonetheless, the point estimates for education outcomes and public revenues are all positive. Moreover, some of these estimates are of the same order of magnitude as those found in the post-treatment period as further discussed below, suggesting that treatment group municipalities might already have been somewhat better off than those in the comparison group as of 1980. In Section 7 below we show that the estimated effects are robust to the inclusion of relevant pre-treatment covariates, including the four pre-treatment education and earnings outcomes shown in Table 3. In addition, results are robust to a difference-in-differences approach that directly controls for pre-

[31] See for example Lee and Lemieux (2009) for more discussion on this point.

[32] The test of the joint null hypotheses of no jumps in pre-treatment covariates is done by stacking these variables and running a joint estimation of individual discontinuities (Lee and Lemieux, 2009).

treatment schooling differences of elementary-school-age cohorts.

7 Estimation results

This section starts out by demonstrating that FPM transfers increased local public spending per capita by about 20%, with no evidence of crowding out own revenue or other revenue sources. The second subsection presents the main result of the paper, which is that the probability of re-election increased by about 10 percentage points. The third subsection shows impacts on education outcomes (higher schooling and literacy rates). The fourth subsection presents and discusses effects on income (lower poverty rates). The last subsection discusses further robustness checks for the education and earnings gains that are not shown here but that are available on request.

All the tables below show results for the first two cutoffs pooled and the first three cutoffs pooled. The tables present results for successively larger samples around the cutoffs (typically 2, 3, 4, and 15%) and for each sample with and without covariates. The discussion will focus on the pooled estimates, because F-tests fail to reject the null hypothesis of homogenous effects at the three cutoffs at conventional levels of significance for all outcomes and in all specifications. Among the pooled estimates, those that control for covariates (including pre-treatment outcomes) are the most reliable because they control for chance correlations with treatment status. They are also the most precisely estimated, because the covariates absorb some of the variation in the outcome measures.

7.1 Effect on total spending, own revenue and other revenues

Table 4 gives estimates of the jump in total local public spending per capita over the 1982-1985 period. The pooled estimates in the first two rows suggest that per capita public spending increased by about 20 percent at the thresholds. The magnitude of the jump is roughly consistent with the size of FPM transfers in local budgets (about 50%) and the jump in per capita FPM transfers at the cutoffs (about 33% for the 10'188 cutoff and less for subsequent cutoffs).[33] This result is also

[33] To see this, let g denote total spending, R total revenue, F FPM funding and O other funding. Since municipalities were running essentially balanced budgets we have $g = R = F + O$ and $\frac{\Delta R}{R} = \frac{\Delta F}{F}\frac{F}{R} + \frac{\Delta O}{O}\frac{O}{R}$. If $\Delta O = 0$, as shown below, and $\frac{F}{R} = 0.5$ on average, as shown in Table 2, then $\frac{\Delta R}{R} = 33\% \times 50\% = 16.5\%$. The estimates in Table 4 are somewhat larger, perhaps because municipalities with missing FPM information rely more heavily on FPM funding, in which case $\frac{F}{R}$ might be more like 0.6, or simply by chance. Note that proportional changes at the cutoff are identical whether or not the

borne out when we estimate the effect of FPM funding per capita on total per capita spending directly, using the treatment indicator $I[X > 0]$ as the instrument. Estimates (not shown but available on request) are almost always at 1 or above, statistically different from zero, and virtually never statistically different from unity.

Table 4 also shows that for larger municipalities around the 4th cutoff, the increase in FPM transfers was too small to affect their overall budget, and hence there was no "first stage" in terms of total spending.[34] One could argue that the 4th cutoff could be used as well because, although not significant, the point estimates are similar to those at preceding cutoffs. While this is a sensible argument, estimates around higher cutoffs are not pursued here for the sake of brevity and ease of interpretation of the estimated impacts (see Section 7.3 below). Another point worth noting is that the included pre-treatment covariates are significant predictors of per capita spending, thus lowering standard errors. Pretreatment covariates also seem to be weakly related to the treatment indicators although the change in point estimates is relatively minor.

Panel B of Figure 4 presents the result graphically. Each dot represents the average residual from a regression of per capita spending on state and segment dummies. These are included to absorb some of the variation in per capita spending and make the jump at the cutoff more easily visible. For example, the first dot to the left of zero represents the residual spending per capita for all municipalities within one percentage point (in terms of population) to the left of one of the first three population thresholds.[35] To demonstrate the correspondence between panel B of Figure 4 and the results in Table 4, if instead of fitting two straight regression lines through the ten dots on either side of the cutoff, this figure were to fit two lines through the first *two* dots on either side of the cutoff, the result would roughly illustrate the jump estimated in column 1 of Table 4 for pooled cutoffs 1-3 in the two percent neighborhood without covariates. With this in mind, the figure shows clear evidence of a discontinuity in total spending at the pooled cutoff, and it additionally shows that the discontinuity is visually robust irrespective of the width of the neighborhood examined.

Figure 4 also graphically represents the results for FPM transfers (panel A), other revenue, which are composed of other federal and state government transfer (panel C), and own revenues

variable is scaled by population, P: $\Delta \ln\left(\frac{R}{P}\right) = \Delta \ln(R) \cong \frac{\Delta R}{R}$.

[34] At the 5th cutoff the discontinuity estimates are much more variable and they are nowhere near statistical significance.

[35] The null hypothesis that population means are equal for two sub-bins within each bin cannot be rejected, suggesting that the graph does not oversmooth the data (Lee and Lemieux 2009).

(panel D), all cumulative over the period 1982-1985. The figure shows clear evidence of a discontinuity at the pooled cutoff for FPM transfers and total revenue per capita. It is also worth noting that both the regression functions for total revenue per capita and FPM per capita slope downward, to the left and to the right of the cutoff. At the same time, Figure 4 shows that there are no discontinuities in either own revenue or other revenues. This suggests that the effects on party re-election, education, and poverty discussed below can be attributed to local spending on public services, rather than additional private spending associated with local tax breaks (that is, the exclusion restriction discussed in Section 3 seems to hold). Statistical analysis confirms this conclusion but is not presented here to save space.

7.2 Effect on the probability of re-election

Table 5 presents estimation results for the party re-election dummy from the linear probability model. All pooled estimates shown in rows 1 and 2 are positive, with values around 10 percentage points. The estimates at individual cutoffs are also positive almost without exception, although they are more variable, which likely reflects smaller sample size. While most of the estimates from individual cutoffs are not significantly different from zero, the pooling across cutoffs c_1 and c_2, as well as c_1, c_2 and c_3, yields statistically significant estimates (at 5%) when we use an extended support ($p = 10\%, 15\%$). Importantly, we reach statistical significance not through higher point estimates, but through a monotonic reduction in standard errors by at least 50 percent compared to the narrowest neighborhoods. The same pattern of results arises with probit estimates or when state dummies are included (results available upon request).

We also estimate the re-election effect based on quadratic and cubic polynomial specifications as further robustness checks. The estimates (available on request) fall in the same range as those presented in Table 5. Most of these nonlinear estimates are not statistically significant because standard errors increase substantially compared to the linear model (standard errors are sometimes twice the size of the corresponding linear specification). We use an F-test of the joint hypotheses that the coefficients on the quadratic and cubic population terms on either side of the cutoff are zero, that is, whether linearity of the population polynomial can be rejected. For neighborhoods up to and including 10% there is virtually no statistical evidence against the null hypothesis of a linear model (p-value of 0.15 at most), which corroborates our focus on the linear estimates and standard errors.

Figure 5 presents graphical evidence of the discontinuity, using the raw (not partialled-out) data. Each dot represents the proportion of municipalities that re-elected the same party into the mayor's office in a given bin. The jump at the cutoff corresponds exactly to the estimate of the discontinuity in the first row and fifth column of Table 5. As with the first stage (jump in total spending per capita) above, the figure shows that the jump in the re-election probability is visually robust irrespective of the width of the neighborhood examined, although there is visibly more variance in Figure 5 than in total per capita spending (Figure 4). In line with this graphical evidence, estimates of the discontinuity for neighborhoods not shown in Table 5 are quantitatively similar to the estimates presented here and are available upon request.[36]

We conclude from the results shown so far that additional local government spending per capita of about 20% over a four-year period improved the re-election probability of local incumbent parties in the 1988 elections by about 10 percentage points. Equivalently, we estimate a semi-elasticity $\frac{\partial p^*}{\partial g/g}$ of about 0.5. This is the most credible estimate to date of the effect of government spending on re-election. We now turn to examining whether public services also improved as predicted by models of electoral accountability.

7.3 Effects on education outcomes

Table 6 shows the results for years of schooling (completed grades) for individuals 19 to 28 years of age in 1991. The pooled point estimates suggest that this cohort accumulated about 0.3 additional years of schooling per capita (specifications with covariates). This schooling gain would be consistent with 3 out of 10 individuals from this cohort completing an additional year of schooling for example. The estimates at individual cutoffs are all positive but more variable, which likely reflects small sample biases. While most of the estimates from individual cutoffs are not significantly different from zero, the pooling across cutoffs c_1 and c_2, as well as c_1, c_2 and c_3, yields statistically significant estimates (at 1%) even within a relatively small neighborhood of +/- 3%

[36] In addition to the jump at the cutoff, there are also other jumps of similar magnitude visible in the graph. These might reflect other policies that use other cutoffs for targeting, or they might occur simply by chance. In either case, what matters most is that there is in fact a jump exactly where we would expect to see one based on the research design.

around the cutoffs. Given that average years of schooling in marginal comparison group counties for the cohort aged 19-28 years old in 1991 was about 4.3 years, with a standard deviation of 1.45 years, the schooling gains amount to about 7% or about 0.2 standard deviations.

It is important to note that the 4.3 average years of schooling represents grades completed, not "years in school". We do not know how many years the cohort 19-28 in 1991 (10-19 in 1982) spent in school, but it should be at least 8 because schooling is compulsory for children aged 7-14 years. On average in Brazil at the time, a year in school led to about 0.625 completed grades—5 years of completed grades for 8 years in school—which is consistent with the 4.3 years of schooling we find in the comparison municipalities (World Bank, 1985). In addition to the 10- to 14-year-olds in 1982, years of schooling might also have increased because of the cohorts aged 15 through 18 who were still eligible for elementary school. Even most of the 19-year-olds on September 1st in 1982 (28 in 1991), the last cohort included in the analysis, were 18 years old at some point during 1982 and hence could have benefited from improvements in the elementary school system.

Since local governments in Brazil provided pre-school education and day-care services that could have benefited even the newborn cohort in 1982, one would expect younger cohorts to exhibit positive but smaller treatment effects. Although not presented here, we find that there were indeed schooling gains of about 0.15 years per capita for the 9- to 18-years-old cohort in 1991 (0-9 in 1982). These additional results are available on request.

In order to interpret the magnitude of these effects, it is useful to consider the marginal cost of a year of schooling implied by these estimates and compare it to the average cost of schooling in Brazil at the time. This requires some assumptions, but a rough comparison can be made. The cumulative (1982-1985) jump in per capita funding averaged across the first three cutoffs is about 100 R$ expressed in 2008 prices, or 71 international US$.[37] Assuming that about 20% of the additional FPM funds were spent on education, and assuming further that only the 0- to 18-year-olds in 1982, about 50% of the total population,[38] were at least marginally affected by the spending boom, marginal education spending *per student* was about $\$71 \times 0.2 \times 2 = \$28.4$.

[37] Note that the 100R$ jump is averaged over three treatment intensities, namely 78R$, 97R$ and 130R$ per capita. The calculations below use this "average extra funding" which roughly corresponds to funding received by municipalities at the second cutoff. Adding more dissimilar funding jumps would further complicate the interpretation of estimated impacts based on pooled specifications.

[38] Census tabulations in De Carvalho (1997).

According to the estimates discussed above, this marginal spending purchased about 0.3 years and 0.15 years of schooling (specifications with covariates), respectively. Taking an unweighted average of 0.225, the implied marginal cost of an additional completed year of schooling is about $\$28.4 \times \frac{1}{0.225} = \$126$. This compares to average annual education expenditures per capita at the cutoffs in 1982 of about 44 R$ in 2008 prices, or 31 international US$. Assuming again that these funds were spent on the 0- to 18-year-olds, and that a year in school leads to about 0.625 completed grades on average (World Bank, 1985), the average cost of a completed additional year of schooling is about $\$31 \times 2 \times \frac{1}{0.625} = \$99$. While these are rough estimates, the similarity of the marginal cost to the average cost indicate that the findings here are certainly plausible.

Students not only completed more grades in municipalities that received extra funds, but for some of them it made the difference between being able to read and write or not. The effect on literacy (not shown but available on request) amounts to about four or five percentage points, compared to an average literacy rate of about 76% in the comparison group. The estimated literacy gain implies that four more individuals per one hundred learned to read and write, which strikes us as reasonable. Pooled estimates are again mostly significantly different from zero even within a relatively small neighborhood of +/- 3% around the cutoffs.

7.4 Effects on poverty and income per capita

Both better and more widespread education and better local public service quality overall (better infrastructure and primary health care for example) are likely to increase household incomes. The evidence suggests that the extra public spending indeed had an effect on income, although only for the poor. Specifically, Table 7 shows impacts on the municipality poverty rate (measured relative to the national income poverty line). All pooled estimates shown in rows 1 and 2 are negative, with values around -4 to -5 percentage points, down from a mean poverty rate of 64% in the comparison group. The estimates at individual cutoffs are also all negative although they are more variable. While most of the estimates from individual cutoffs are not significantly different from zero, the pooling across cutoffs yields statistically significant estimates (at 1%) even in the discontinuity samples. While income per capita in 1991 is higher in the communities that got more funding, the difference is not statistically significant (results not shown). Figure 6 shows the effects on education outcomes, poverty and income per capita graphically.

In order to interpret the results on poverty reduction, it is again useful to do some back-of-the-envelope calculations. Impacts on poverty are likely to arise through better and more widespread education, as well as through better local public service quality overall. Regarding the education channel, the estimates discussed above suggest schooling gains for the 10- to 19-year-olds and the 0- to 9-year-olds in 1982 of 0.3 years and 0.15 years, respectively. A likely distribution of individual-level gains that would lead to this average impact is that 30 out of 100 individuals in the older cohort and 15 out of 100 in the younger cohort completed another year of schooling. Given the shares of these cohorts in the total population—27% and 23%, respectively, according to De Carvalho (1997)—we can thus estimate what percent of the overall population got an additional year of schooling, namely about $27\% \times 15\% + 23\% \times 30\% = 11\%$.

Now suppose that an extra year of schooling raises wages by 12% (Behrman and Birdsall 1983), that labor supply is constant, and that about 10% of the population earn per capita income that falls within a 12% range below the poverty line. Suppose further that about 64% of the total population would have been poor in the absence of the extra funding (this corresponds to the comparison group average poverty rate shown in Table 7) and that schooling only increased among the poor, so that $0.11/0.64 = 17.2\%$ of the poor got an additional year of education. If the schooling gains are independent from the distance to the poverty line, then $10\% \times 17.2\% = 1.72\%$ of the total population escaped poverty through the schooling channel alone. This number will be higher the larger the (average) returns to schooling, the larger the share of the population within range to cross the poverty line given returns to schooling, and the higher the share of the poor within that range that do get an additional year of schooling (those closer to the poverty line might be more likely to get more schooling than those that are extremely poor). The education channel alone can thus account for about 2 percentage points of the estimated total 4-5 percentage points of poverty reduction, leaving the remaining 2 to 3 percentage points to better local public service quality overall.

It is worth remembering that—under the continuity assumption and exclusion restriction discussed in Section 5—all of these effects can be attributed to extra local public spending, rather than private spending, since there is no evidence of local tax breaks, and direct welfare spending by local governments was very limited. However, we need to emphasize that we are *not* claiming that these effects arise exclusively through public service improvements. Indeed, it is very plausible that some of the extra spending served to expand political patronage—thus helping the incumbent political party get re-elected—and some of the extra cash might also have helped to keep some kids in school. But the back-of-the-envelope calculations above seem inconsistent with the hypothesis that only a small minority in the community benefited from extra spending. What our results suggest, therefore, is that increased public service provision was an important causal mechanism underlying the link between government spending and re-election in Brazil during this time.

7.5 Further robustness checks on education and earnings

We have performed a number of further robustness checks for the education and earnings gains discussed above that are not shown here but that are available on request. First, we find that these results are robust to alternative specifications of functional form of both the running variable (population) and of pre-treatment covariates, as well as to a difference-in-differences approach that directly controls for pre-treatment schooling differences of elementary-school-age cohorts. Second, the corresponding difference-in-differences estimates for cohorts that have largely completed their education—and for whom one would expect smaller or no impacts—are indeed close to zero in magnitude (sometimes negative) and very far from statistical significance. As a final robustness check we use only the sub-sample of individuals who were born in the municipality and never moved away and find quantitatively similar results, suggesting that the schooling gains stem at least partly from existing residents, rather than being driven by immigration of more highly educated individuals in response to public service improvements.

8 Conclusion

This paper provides the most credible estimates to date of the causal link between government spending and electoral outcomes, as well as indirect evidence on one of the channels through which this link operates, namely public service improvements. Specifically, we find that extra local public spending per capita of about 20% over a four year period increased the re-election probability of the incumbent party by about 10 percentage points. The extra government spending also improved municipality education outcomes and household income for the poor, which we interpret as indirect evidence of public service improvements. Together, our results provide evidence that electoral rewards encouraged incumbents to spend part of additional revenues on public services valued by voters, a finding in line with agency models of electoral accountability.

As with any empirical study, an important question regards the generalizability of these findings. The effects presented in this paper (as in any regression discontinuity analysis) apply only to the units near the relevant cutoffs—in our case, the municipalities near the population cutoffs. Because our results are qualitatively similar across the first three cutoffs, it seems likely that the effects presented here generalize at least to the subpopulation of municipalities in the approximate population range 8'500-18'700, which represents about 30% of Brazilian municipalities at the time. However, examining electoral responses in other contexts is an obvious avenue for future research. In that light, it should be noted that our focus on government spending, rather than taxation, is less restrictive than it might seem because political decentralization around the world has typically not been accompanied by decentralization of revenue-raising, with the result that most of local spending is financed by grants from the central or state governments (Rodden 2004).[39]

Our advancement of the public services causal mechanism as a complement to the other main mechanism in the literature—namely patronage—naturally leads to another important line of research, investigating the relative effects these mechanisms have in re-election. These relative effects would almost certainly vary with the institutional environment of the election, such as the extent to which patronage spending is constrained and so forth. Our study cannot answer this question, but in demonstrating the potential importance of the public services mechanism, it will hopefully spur future research in this area.

[39]For upper levels of government the positive electoral effect of government spending we estimate would have to be balanced against the (presumably negative) electoral effect of raising revenue, which we cannot estimate with our data.

9 References

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Table 1: Mayor party affiliations in 1982 and 1988

		1982		1988	
Party	Party-type	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

Source: Tribunal Superior Eleitoral

Table 2: Descriptive statistics (sample means)

	Population range					
	7'500 - 44'148	8'500 - 18'700				
Sample	Full	Full	PDS	Opp.	Rural	Urban
Observations	2306	1248	844	358	624	624
1980 county characteristics (IBGE)						
Average years of schooling (25 years and older)	1.96	1.90	1.68	2.39	1.52	2.29
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 rate, 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	96.2	80.7
GDP ('000) 2008 Reais (IPEA)	108'587	64'214	54'845	82'480	46'827	81'741
1982 Financial data (Ministry of Finance)						
Total county revenue ('000) 2008 Reais	3'597	2'876	2'620	3'311	2'360	3'365
Total county revenue 1982/GDP 1980 (%)	5.3	5.6	6.1	4.6	6.2	5.0
FPM transfers/total revenue (%)	48.0	49.7	54.2	41.1	56.4	43.3
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 education outcomes (1991 census)						
Average years of schooling (19- to 28-year-olds)	4.6	4.5	4.2	5.3	4.0	5.1
Literacy rate (19- to 28-year-olds) (%)	78.8	79.0	75.0	87.5	73.7	84.3
1991 Household income (IBGE)						
Poverty headcount ratio (R$140 poverty line) (%)	60.0	60.2	65.7	47.5	69.2	51.2
Household income per capita 2008 Reais	223.6	217.4	188.7	282.9	168.7	266.3
1988 Electoral outcomes (TSE)						
Re-election (party) (%)	23.0	21.6	10.8	47.8	20.4	22.9
Re-election (party, PFL88 as PDS88) (%)	42.7	42.5	40.3	47.8	44.9	40.0

Notes: Opposition indicates that the county was run by a mayor from an opposition party (PMDB, PDT, PT or PTB). Rural sample: percentage of municipality residents living in urban areas < 24.8; Urban sample: percentage of municipality residents living in urban areas > 24.8.

Table 3: Test of discontinuities in pre-treatment covariates

Polynomial specification:	Linear	Linear	Linear	Linear	Linear
Neighborhood (%):	2	3	4	5	6
Opposition party (0/1)	-0.131	-0.078	-0.049	-0.056	-0.061
	(0.108)	(0.092)	(0.082)	(0.072)	(0.066)
Average years of schooling	0.057	0.173	0.202*	0.231**	0.159*
(25 years and older)	(0.174)	(0.137)	(0.117)	(0.108)	(0.094)
Urban residents (%)	0.005	0.007	-0.004	0.004	-0.015
	(0.045)	(0.036)	(0.031)	(0.029)	(0.025)
Net enrollment rate (%)	2.060	3.382	4.595*	4.260**	2.076
(7- to 14-year-olds)	(3.821)	(2.891)	(2.403)	(2.133)	(1.890)
Illiteracy rate (%)	-1.146	-1.511	-2.638	-2.886	-1.794
(15 years and older)	(3.157)	(2.286)	(1.951)	(1.782)	(1.587)
Poverty headcount ratio (%)	3.895	-0.563	-1.523	-2.077	-0.186
(National poverty line)	(3.733)	(2.868)	(2.439)	(2.227)	(1.948)
Income per capita (%)	-0.031	0.029	0.045	0.062	0.030
(percent of minimum salary)	(0.082)	(0.059)	(0.049)	(0.045)	(0.040)
Infant mortality	-2.263	-3.776	-6.490	-3.910	-3.530
(per 1000 life births)	(5.406)	(4.506)	(4.111)	(3.493)	(3.221)
Log current transfers 1981	0.090	0.067	0.081	0.068	0.007
(per capita)	(0.093)	(0.071)	(0.065)	(0.061)	(0.056)
Log capital transfers 1981	0.027	0.097	0.097	0.062	0.064
(per capita)	(0.163)	(0.130)	(0.127)	(0.109)	(0.099)
Log total revenue 1981	0.085	0.080	0.130**	0.109*	0.050
(per capita)	(0.089)	(0.072)	(0.062)	(0.057)	(0.052)
Log own revenue 1981	0.498	0.464	0.411	0.348	0.299
(per capita)	(0.414)	(0.315)	(0.258)	(0.232)	(0.215)
Municipalities	200	293	386	471	561
F-statistic	0.85	0.80	1.22	1.16	1.23
(p-value)	(0.60)	(0.65)	(0.26)	(0.31)	(0.26)

Notes: Table entries are OLS estimates (standard errors) of discontinuities in pre-treatment covariates using the pooled specification across the first three cutoffs described in Section 5, equation (3) in the main text. F-statistic tests the joint null hypotheses of no discontinuities in any pre-treatment covariate. Clustered (at the municipality level) standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. All specifications allow for differential slopes by segment and on each side of the cutoff. Opposition party is an indicator for whether the county was run by a PDS mayor from 1982-1988 (0) or a mayor from an opposition party (PMDB, PDT, PT or PTB) (1). (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 4: Impact on total public spending

Dependent variable: log total public spending per capita (1982-1985)

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Various[1]
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.173**	0.211***	0.172***	0.163***	0.206***	0.184***	0.158***
	(0.076)	(0.065)	(0.060)	(0.051)	(0.055)	(0.047)	(0.036)
Observations	191	188	278	275	368	364	1158
R-squared	0.76	0.85	0.73	0.83	0.68	0.80	0.76
Pooled cutoffs 1-2							
I[X > 0]	0.227***	0.280***	0.227***	0.218***	0.231***	0.207***	0.208***
	(0.098)	(0.082)	(0.078)	(0.070)	(0.071)	(0.062)	(0.046)
Observations	124	124	190	189	247	245	789
R-squared	0.75	0.85	0.74	0.82	0.73	0.82	0.77
1^{st} cutoff							
I[pop > 10188]	0.199	0.379**	0.263**	0.267**	0.249***	0.234**	0.248***
	(0.161)	(0.159)	(0.113)	(0.112)	(0.094)	(0.093)	(0.057)
Observations	62	61	95	94	128	126	428
R-squared	0.84	0.90	0.85	0.89	0.81	0.86	0.80
2^{nd} cutoff							
I[pop > 13584]	0.214	0.188	0.227*	0.258*	0.249**	0.262**	0.205**
	(0.172)	(0.166)	(0.127)	(0.135)	(0.114)	(0.111)	(0.095)
Observations	63	63	95	95	119	119	361
R-squared	0.70	0.84	0.71	0.82	0.71	0.83	0.77
3^{rd} cutoff							
I[pop > 16980]	-0.038	-0.027	-0.008	0.023	0.073	0.091	0.094**
	(0.145)	(0.113)	(0.122)	(0.083)	(0.117)	(0.077)	(0.045)
Observations	66	64	88	86	121	119	369
R-squared	0.84	0.93	0.82	0.92	0.67	0.84	0.77
4^{th} cutoff							
I[pop > 23772]	0.045	0.165	0.152	0.134	0.159	0.061	0.111
	(0.272)	(0.184)	(0.195)	(0.144)	(0.146)	(0.115)	(0.070)
Observations	44	44	69	68	96	95	353
R-squared	0.79	0.91	0.83	0.91	0.80	0.89	0.84

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. 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 people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

[1]Moving down the table from the pooled 1-3 cutoffs to the single 4^{th} cutoff, the specifications are quadratic, quadratic, quadratic, cubic, linear, and quadratic, respectively.

Table 5: Impact on the probability of re-election

Dependent Variable: Incumbent party re-elected for mayor's office in 1988; LHS mean: 0.16, sd: 0.37

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Neighborhood (%):	2	2	4	4	10	10	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X> 0]	0.128	0.167*	0.083	0.098	0.076	0.098**	0.094**
	(0.109)	(0.099)	(0.079)	(0.072)	(0.051)	(0.047)	(0.041)
Observations	195	195	374	374	919	919	1215
R-squared	0.01	0.18	0.01	0.13	0.01	0.16	0.18
Pooled cutoffs 1-2							
I[X > 0]	0.058	0.131	0.023	0.068	0.102	0.126**	0.112**
	(0.140)	(0.130)	(0.101)	(0.093)	(0.064)	(0.059)	(0.053)
Observations	129	129	250	250	621	621	839
R-squared	0.02	0.15	0.01	0.11	0.01	0.16	0.18
1st cutoff							
I[pop > 10188]	0.125	0.258	0.025	0.140	0.147	0.216***	0.153**
	(0.273)	(0.210)	(0.157)	(0.129)	(0.091)	(0.079)	(0.068)
Observations	65	65	134	134	315	315	463
R-squared	0.24	0.27	0.01	0.25	0.02	0.23	0.22
2nd cutoff							
I[pop > 13584]	-0.017	0.071	0.017	0.030	0.063	0.056	0.070
	(0.187)	(0.192)	(0.134)	(0.133)	(0.089)	(0.089)	(0.082)
Observations	64	64	116	116	306	306	376
R-squared	0.01	0.01	0.01	0.02	0.01	0.12	0.14
3rd cutoff							
I[pop > 16980]	0.143	0.233	0.223*	0.167	0.032	0.049	0.043
	(0.241)	(0.148)	(0.123)	(0.109)	(0.085)	(0.078)	(0.070)
Observations	66	66	124	124	298	298	376
R-squared	0.23	0.28	0.04	0.17	0.02	0.15	0.18

Notes: OLS estimations. 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). Other covariates such as average years of schooling for individuals 25 years and older, county income per capita, poverty headcount ratio, illiterate percentage of over 15 year olds, infant mortality, school enrollment of 7 to 14 year olds and percent of population living in urban areas do not alter the estimate of interest nor do they help reduce its standard error. All specifications allow for differential slopes by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 6: Impact on schooling, 19- to 28-year-olds in 1991

Dependent variable: average years of schooling, 19- to 28-year-olds in 1991, LHS mean: 4.3, sd: 1.45

Polynomial 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 cutoffs 1-3							
I[X > 0]	0.330	0.231	0.527***	0.312***	0.551***	0.290***	0.356**
	(0.260)	(0.151)	(0.199)	(0.114)	(0.172)	(0.102)	(0.140)
Observations	200	197	293	290	386	382	1243
R-squared	0.72	0.89	0.71	0.89	0.69	0.89	0.88
Pooled cutoffs 1-2							
I[X > 0]	0.415	0.191	0.511**	0.309**	0.512**	0.304**	0.374**
	(0.324)	(0.180)	(0.243)	(0.140)	(0.215)	(0.129)	(0.179)
Observations	131	130	200	199	259	257	857
R-squared	0.74	0.90	0.74	0.89	0.71	0.88	0.87
1st cutoff							
I[pop > 10188]	0.286	0.557	0.445	0.439	0.403	0.424*	0.525*
	(0.500)	(0.484)	(0.352)	(0.302)	(0.340)	(0.242)	(0.313)
Observations	66	65	101	100	135	133	470
R-squared	0.79	0.91	0.78	0.89	0.75	0.89	0.87
2nd cutoff							
I[pop > 13584]	0.398	0.347*	0.497	0.338*	0.585*	0.257	0.215
	(0.530)	(0.204)	(0.373)	(0.172)	(0.305)	(0.158)	(0.193)
Observations	65	65	99	99	124	124	387
R-squared	0.77	0.96	0.76	0.93	0.73	0.90	0.88
3rd cutoff							
I[pop > 16980]	0.024	0.403	0.280	0.185	0.552	0.169	0.366
	(0.507)	(0.333)	(0.385)	(0.224)	(0.353)	(0.192)	(0.231)
Observations	69	67	93	91	127	125	386
R-squared	0.77	0.94	0.73	0.93	0.70	0.92	0.91

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. 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 people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

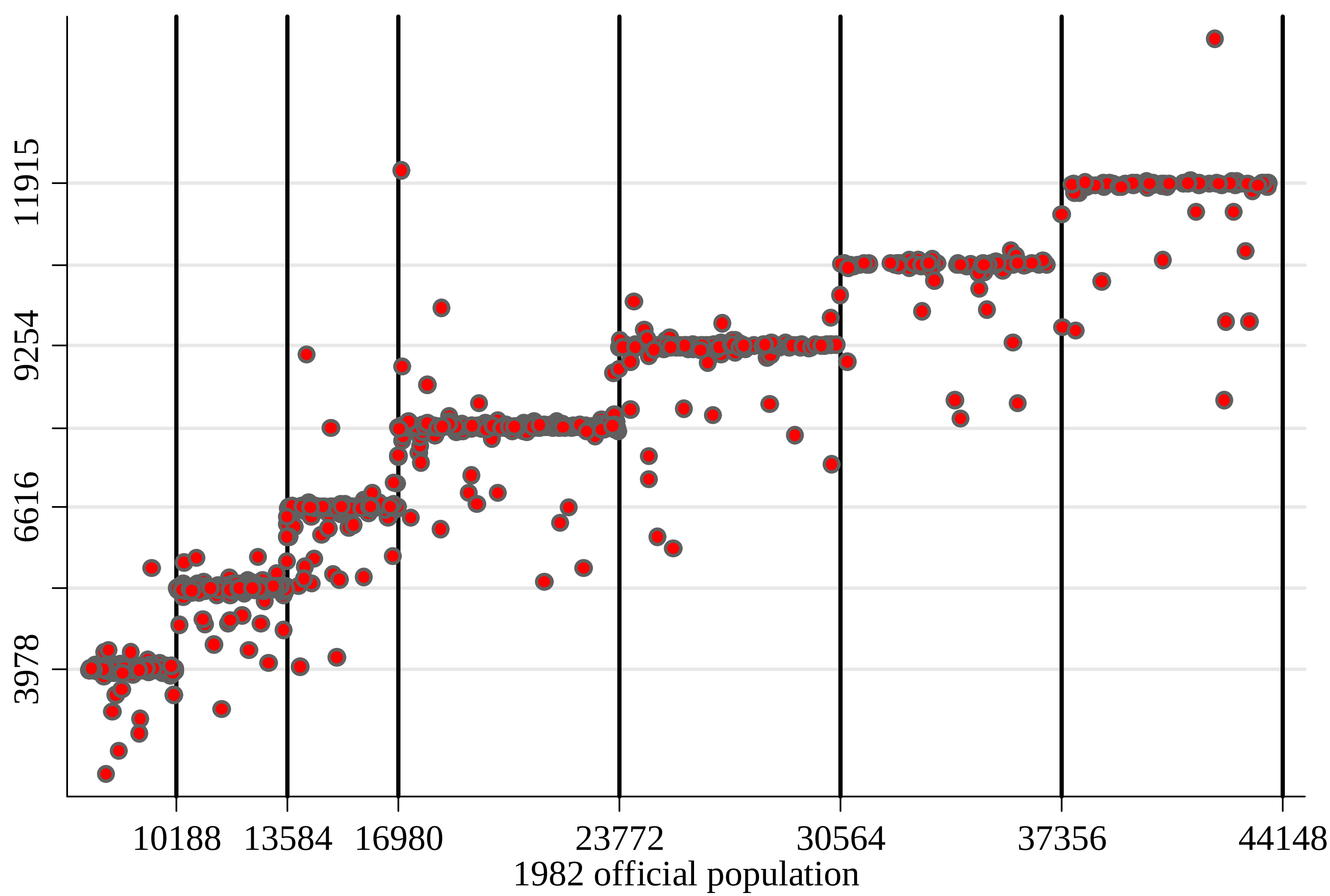
Table 7: Impact on the poverty rate in 1991

Dependent variable: 1991 poverty rate, LHS mean: 0.64, sd: 0.22

Polynomial 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 cutoffs 1-3							
I[X > 0]	-0.038	-0.066***	-0.062**	-0.053***	-0.058**	-0.039***	-0.043***
	(0.039)	(0.022)	(0.029)	(0.017)	(0.024)	(0.015)	(0.018)
Observations	200	197	293	290	386	382	1243
R-squared	0.79	0.93	0.78	0.92	0.76	0.91	0.91
Pooled cutoffs 1-2							
I[X > 0]	-0.012	-0.052*	-0.043	-0.042**	-0.036	-0.023	-0.038
	(0.052)	(0.029)	(0.039)	(0.022)	(0.032)	(0.019)	(0.023)
Observations	131	130	200	199	259	257	857
R-squared	0.77	0.93	0.77	0.93	0.76	0.92	0.91
1st cutoff							
I[pop > 10188]	-0.019	-0.100**	-0.032	-0.048	-0.026	-0.027	-0.027
	(0.056)	(0.048)	(0.044)	(0.032)	(0.042)	(0.031)	(0.018)
Observations	66	65	101	100	135	133	470
R-squared	0.87	0.95	0.84	0.93	0.81	0.92	0.90
2nd cutoff							
I[pop > 13584]	-0.014	-0.048	-0.060	-0.055	-0.042	-0.026	-0.054
	(0.010)	(0.055)	(0.063)	(0.040)	(0.051)	(0.033)	(0.035)
Observations	65	65	99	99	124	124	387
R-squared	0.73	0.94	0.74	0.93	0.74	0.93	0.91
3rd cutoff							
I[pop > 16980]	-0.096	-0.100**	-0.096*	-0.070**	-0.088**	-0.061**	-0.071**
	(0.067)	(0.047)	(0.050)	(0.031)	(0.043)	(0.027)	(0.035)
Observations	69	67	93	91	127	125	386
R-squared	0.85	0.94	0.82	0.93	0.78	0.92	0.91

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. 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 people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Figure 1: FPM Transfers, 1982-1985 (in ’000 of 2008 Reais)



Each dot represents a municipality. FPM transfers are self-reported by municipalities. 1982 official population is based on the 1980 census conducted by the national statistical agency, IBGE.

Figure 2: Estimation Approach

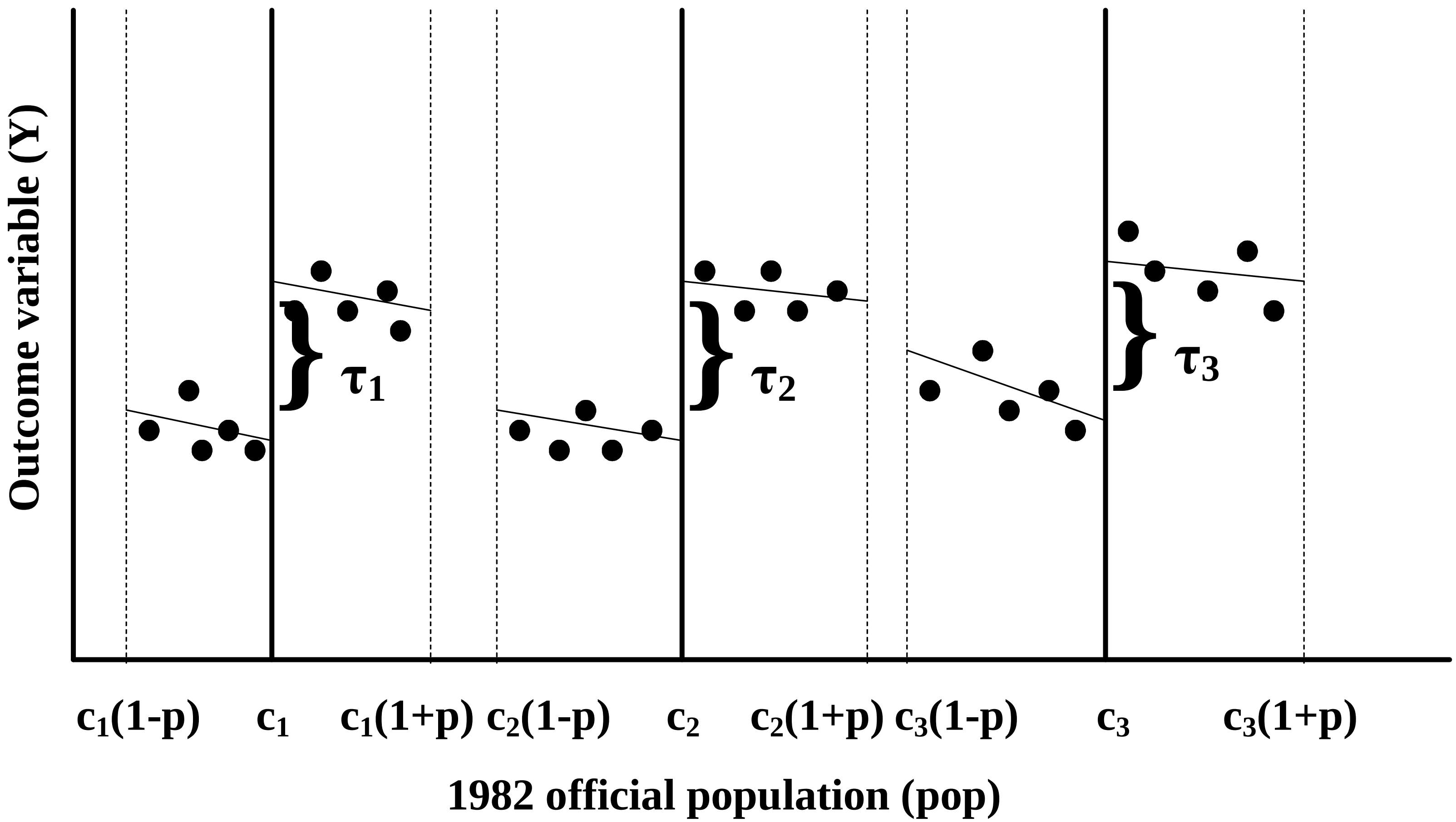
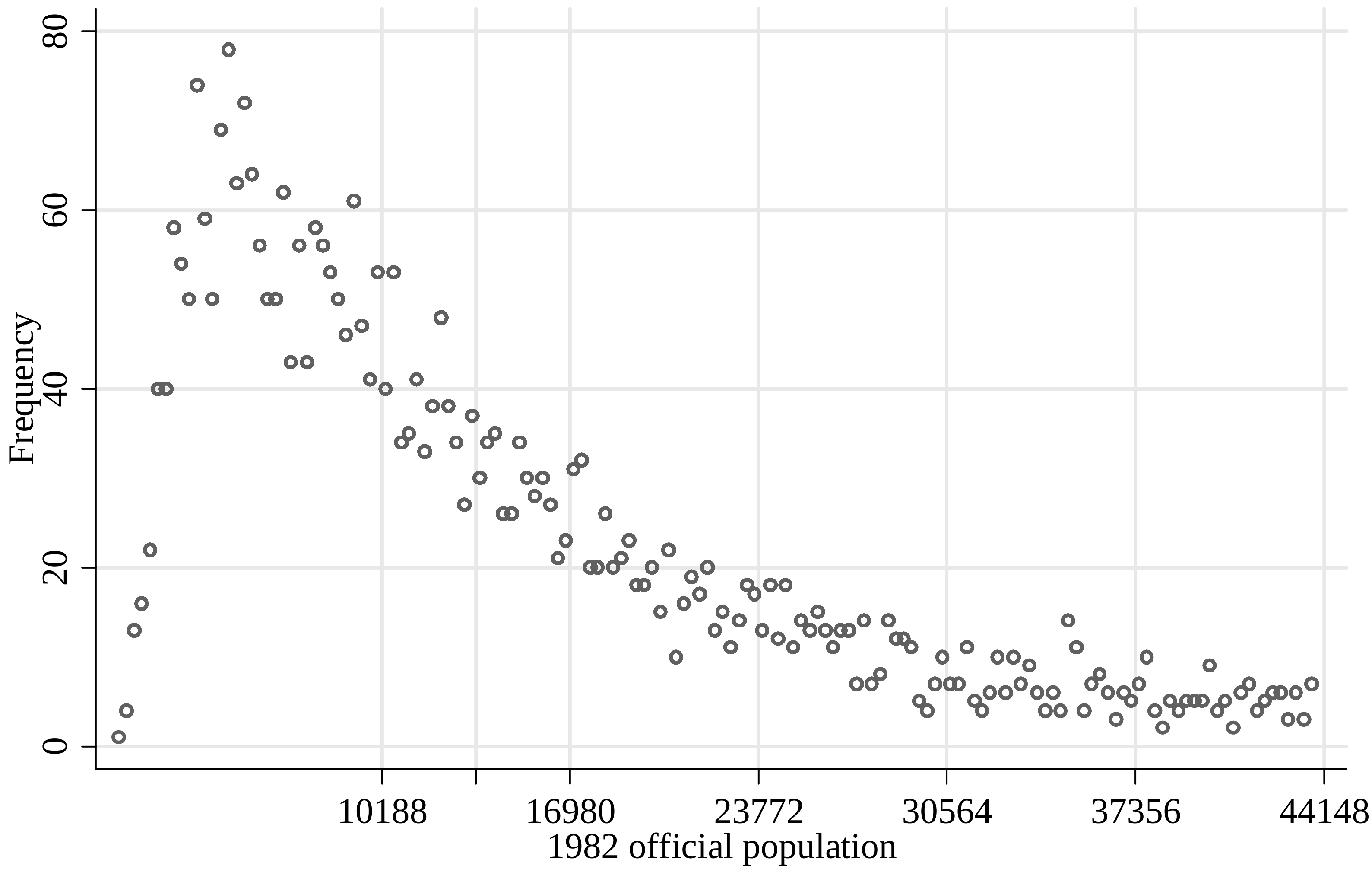
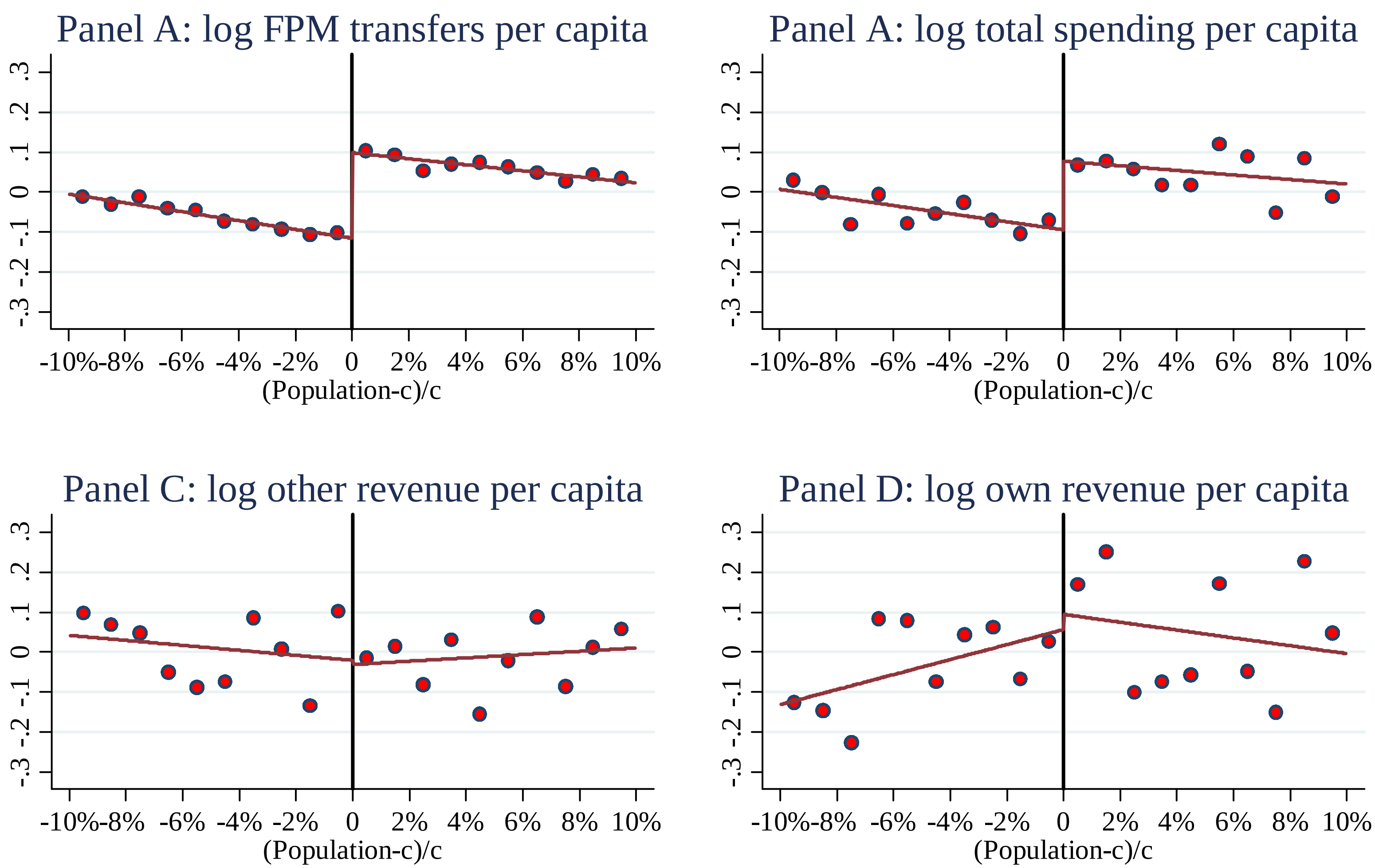


Figure 3: Histogram for 1982 official population, small to medium municipalities



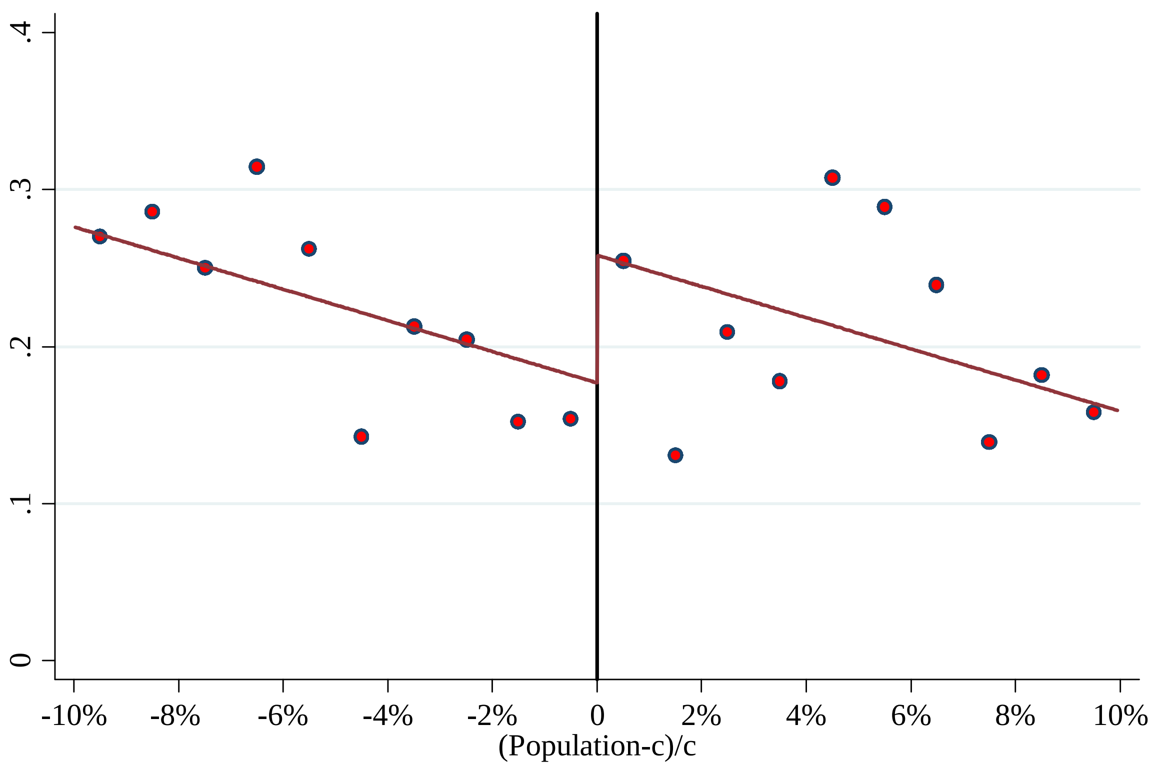
The bin-width is 283. 1982 official population is based on the 1980 census conducted by the national statistical agency, IBGE. McCrary density test fails to reject the null of no discontinuity in the density at conventional levels of significance for the first six cutoffs.

Figure 4: First stage and impacts on total spending, other revenue and own revenue



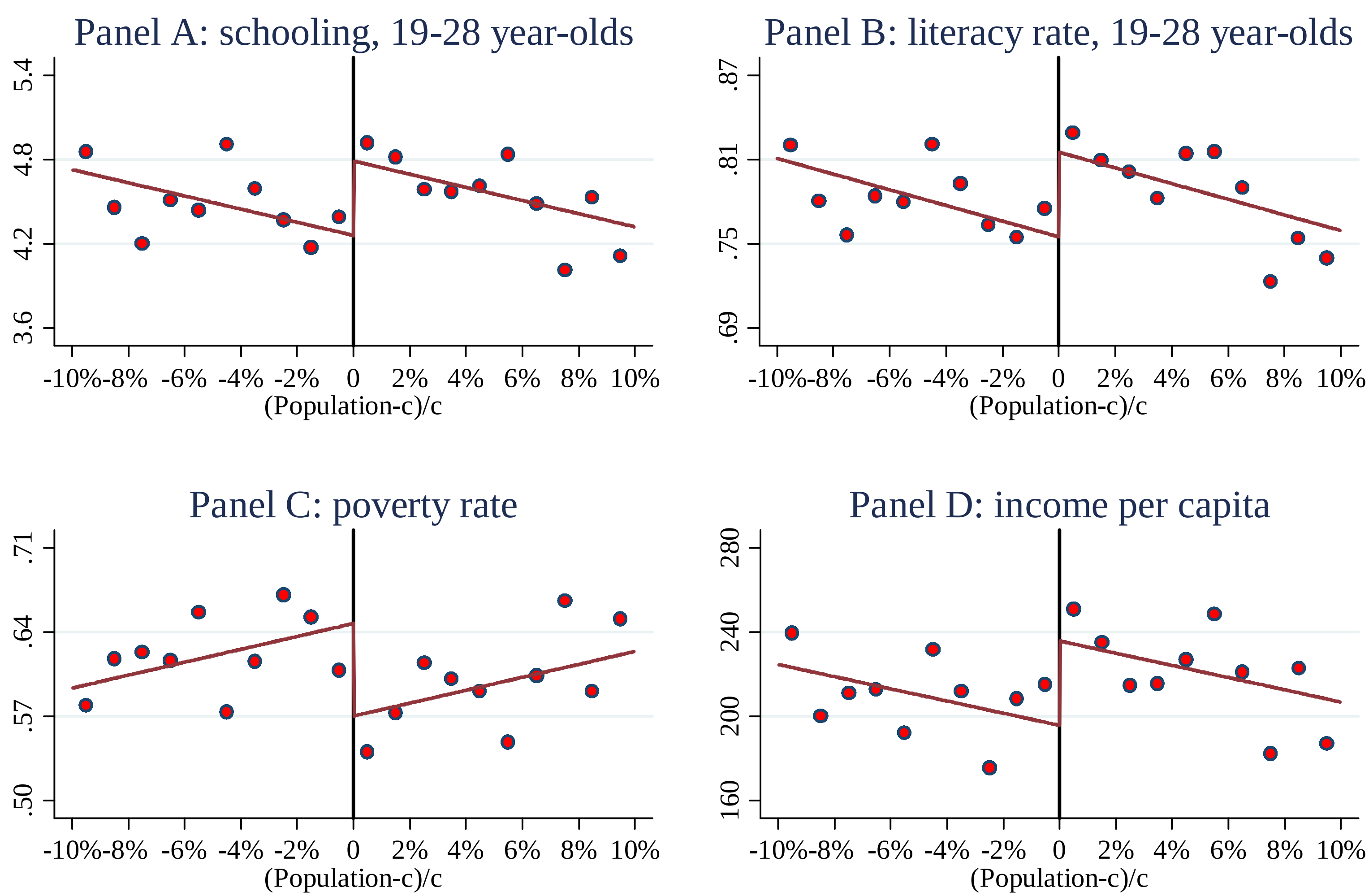
All variables are summed over the period 1982-1985 and scaled by 1980 municipality census population. Each dot represents the sample average of the (partialled out) dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 5: Re-election discontinuity plot



Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 6: Effects on schooling, literarcy and earnings



All variables based on the 1991 census. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.