The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction

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Abstract

This paper provides regression discontinuity (RD) evidence on development impacts of intergovernmental transfers. Extra transfers in Brazil increased local government spending per capita by about 20 percent over a four-year period with no evidence of crowding out own revenue or other revenue sources. Schooling per capita increased by about 7 percent and literacy rates by about 4 percentage points. In line with the effect on human capital, the poverty rate was reduced by about 4 percentage points. Somewhat noisier results also suggest that the re-election probability of local incumbent parties in the 1988 elections improved by about 10 percentage points.

Keywords: intergovernmental grants, decentralization, economic development, regression discontinuity, voting
JEL: D70, D72, H40, H72, O15

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Many economists are skeptical whether making more funds available to governments in poor countries leads to better development outcomes (Easterly 2006, 2008). Similar skepticism applies to intergovernmental transfers, and more specifically to whether providing additional financing to local governments in developing countries raises living standards of the local population (Shah 2006).[1] Since intergovernmental transfers finance a large share of decentralized public service provision in developing countries around the world (Rodden 2004, Shah 2006), it is important to know to what extent additional funding to local governments actually "trickles down" to the population. The reasons to worry are many, including corruption (Reinikka and Svensson 2004; Olken 2007; Ferraz and Finan 2008), simple waste in the provision of public services (Bandiera, Prat, and Valletti 2009), and capture of the political process by the local elite (Bardhan and Mookherjee 2005). Moreover, funds might be rationally crowded out by benevolent and efficient local governments, and even the money that ends up being spent on service improvements might fail to have the intended impact. Given these facts and concerns about local government spending, it is not clear *ex ante* whether providing more financing improves public service delivery at the margin. And due to high data requirements, there is very little empirical research that looks at the impact of additional fiscal transfers on public services and development outcomes, such as human capital accumulation and earnings.

This paper provides regression discontinuity (RD) evidence on development impacts of intergovernmental transfers at the municipality (*município*) level in Brazil over the period 1980-1991.[2] We analyze the effects of additional unrestricted grant financing on public spending and public service provision in the main spending areas of education, transportation, and housing and urban infrastructure.[3] In addition, we look at education outcomes—schooling and literacy—and income, because they represent indirect summary measures of public service provision in the municipality. Finally, we examine the impact of additional funding on the odds of reelection of incumbent parties.

Brazil seems a difficult environment in which to find impacts on public service delivery because

[1]Shah (2006:1) starts his review of the literature with the following (anonymous) quote: "The practice of intergovernmental fiscal transfers is the magical art of passing money from one government to another and seeing it vanish in thin air."

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

[3]The municipalities in our sample spent on average about 20% of their budgets in each of these areas, while spending about 10% on health. Local welfare spending was close to negligible in the early 1980s.

it does not have a good reputation in terms of public governance in general,[4] and there is recent objective evidence from audit reports of corruption in the local delivery of centrally funded services (Ferraz and Finan 2008). Moreover, because about 40 percent of the Brazilian population was illiterate and therefore did not have the right to vote until 1985, concerns about elite capture of the local political process are likely to apply. Recent evidence on oil-royalty financed local spending increases suggests little or no public service improvements (Monteiro and Ferraz 2010, Caselli and Michaels 2013), and negligible impacts on poverty reduction (Caselli and Michaels 2013), as further discussed in the conclusion.

In order to address the likely endogeneity of central government funding, our identification strategy exploits the fact that a substantial part of national tax revenue in Brazil is redistributed 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. Around the population cutoffs there are thus jumps in per capita central government funding and local public spending that are "as good as" randomly assigned (under relatively weak, and to some extent testable, assumptions discussed below).[5]

The main empirical result of the paper is that communities that received extra financing from the central government over the period 1982-1985 benefited in terms of education outcomes (higher schooling[6] and literacy rates) and income (lower poverty rates), measured in 1991.[7] The results for education outcomes suggest that the relevant school-age cohorts acquired about 0.3 additional years of schooling per capita (a 7 percent increase), and literacy rates increased by about four percentage points (compared to a 76 percent literacy rate in the comparison communities). According to our back-of-the-envelope calculations, the implied marginal cost of a year of schooling amounts to about US$ 126 (in 2008 prices), which turns out to be similar to the average cost of a year of schooling in Brazil in the early 1980s. While these are rough estimates, the similarity of the

[4]According to Transparency International's Corruption Perception Index for 1995 (the earliest available year), Brazil ranked as the fifth most corrupt out of 41 surveyed countries.

[5]Brollo, et al. (2012) build on our paper by using the same funding discontinuities in a later period, as further discussed below. Other studies of Brazilian local governments use discontinuity designs to study other phenomena. For example, Ferraz and Finan (2009) exploit discontinuities in legislators' wage caps at various population cutoffs that were introduced in the year 2000, to examine whether better pay attracts better quality politicians and improves political performance.

[6]Schooling refers to completed grades, not "years in school".

[7]We focus on the beginning of the 1980s because starting in 1988, official population estimates were updated annually, which meant that 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 2012).

marginal cost to the average cost indicates that the findings here are plausible. Moreover, these estimates suggest that—even accounting for potential corruption and other leakages—providing more financing to local governments at the margin improved education outcomes at reasonable cost.

Our results on income suggest that extra financing from the central government reduced the poverty rate (measured relative to the national income poverty line) by about 4 percentage points from a comparison group mean poverty rate of 64 percent. Income per capita gains were positive but not statistically significant. The income gains for the poor are unlikely to be driven by direct welfare transfers since these were negligible at the time, and also since income was measured in 1991 and the funding differential lasted only until the end of 1985. Our back-of-the-envelope calculations suggest that about 2 percentage points of the poverty reduction are plausibly accounted for by the education channel alone, leaving the remaining 2 percentage points to improved public service provision overall.

The education and earnings gains we find are consistent with a simple human capital model in which spending on education improves the quality of local schools, thus increasing the marginal benefit of education for any given level of schooling (Behrman and Birdsall 1983), while spending on other public inputs, such as road quantity and quality, reduces the marginal cost of schooling, leading households to increase their equilibrium schooling choice (Birdsall 1985; Behrman, Birdsall, and Kaplan 1996). In particular, additional transfers increased local public spending per capita by about 20 percent, with no evidence of crowding out own revenue or other revenue sources, implying that the education and earnings gains can be attributed to extra local public spending rather than private spending. Additionally, local spending shares remained essentially unchanged—that is, local spending on education, housing and urban infrastructure, and transportation all increased by about 20 percent per capita.

We should note that direct evidence on improved public service delivery is mixed at best: while there is some evidence that student-teacher ratios in local primary school systems fell, there is little evidence that housing and urban development spending affected housing conditions. However, an important limitation of looking at direct public service measures is that there are no data on what the money was actually spent on, and so it is difficult to know whether the available measures are the "right" ones. Quality improvements and repairs, for example, would be impossible to detect with simple quantity measures of public services.

The positive impacts on education and earnings we observe may seem surprising given existing evidence on corruption from developing countries in general, and Brazil in particular. Indeed, Brollo, et al. (2012) build on our paper by using the same discontinuities in a later period to show that extra funding led to a roughly proportional increase in measures of corruption based on audit reports. They also provide evidence that the quality of candidates running for local office deteriorated as a result of higher funding, and that the incumbent's chances of reelection improved.

However, as noted by Brollo et al., the existence of corruption does not rule out the possibility that mechanisms of accountability were at work as well. In fact, classical political agency models of accountability (Barro 1973; Ferejohn 1986; Persson and Tabellini 2000; Besley 2006) can generate predictions of corruption and public service provision increasing simultaneously as a result of increased financing. Consistent with this intuition, we provide tentative evidence that the reelection probability of local incumbent parties in the 1988 elections improved by about 10 percentage points in the municipalities just over the population thresholds. Together with the education and earnings gains, the reelection result suggests that expected electoral rewards encouraged incumbents to spend additional funds at least partly in ways that were valued by voters.[8]

In order to assess the internal validity of all our results, we run a series of tests and robustness checks. First, there is no evidence of manipulation of the 1980 census municipality population figures, which constitute the running variable in our RD design. Second, we verify whether municipalities in the marginal (to the cutoff) treatment and comparison groups were ex ante comparable[9] by testing for discontinuities in pretreatment covariates such as whether the municipality was aligned with the central government in 1982, municipality own and total revenues, income per capita, poverty, urbanization, elementary school enrollment, schooling, literacy, and infant mortality. The results show that there is no statistical evidence of discontinuities in these potentially confounding factors, although some of the point estimates suggest that treatment group municipal-

[8]Note that for the political selection mechanism proposed by Brollo et al. to explain the electoral effect we find, the receipt 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 funding differential 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.

[9]Municipalities in the marginal treatment (comparison) group are those whose 1980 census population falls in the interval $c, c+\varepsilon$ $(c-\varepsilon, c)$, where c is a cutoff and ε some small number relative to municipality population.

ities were already doing somewhat better than those in the comparison group as of 1980. Third, we show that all results are robust to both the inclusion of pretreatment covariates (including pretreatment education and earnings outcomes) and to the choice of bandwidth and functional form.

Further robustness checks show that the schooling and literacy gains of older cohorts are robust to using the difference in outcomes over time, rather than the 1991 levels. In contrast, the corresponding difference estimates for cohorts that had largely completed their education when the extra funding started in 1982—and for whom one would expect smaller or no impacts—are close to zero in magnitude and very far from statistical significance. We also find almost identical results when the sample is restricted to individuals who were born in the municipality and never moved away, which suggests that the schooling and literacy gains were not driven by selective migration. Finally, we test and reject the joint null hypotheses of no discontinuities in any of the outcome variables we consider, suggesting that at least some of the impacts we find are real.[10]

The remainder of the paper is organized as follows: Section I documents the role of local governments in public service provision in Brazil and gives institutional background on revenue sharing. Section II provides the conceptual framework and a discussion of identifying assumptions. Section III describes the data. Section IV discusses the estimation approach and Section V evaluates the internal validity of the study. Section VI presents estimation results. The paper concludes with a discussion of results, limitations and extensions.

I Background

A Local public services and their financing

Local government responsibilities at the beginning of the 1980s were mostly to provide elementary education, housing, and urban infrastructure, as well as local transportation services. The responsibility for delivery of elementary education was shared with state governments, while the federal government was primarily involved in financing and standard setting. In 1980, 55 percent of all elementary school students in Brazil were enrolled in state administered schools, 31 percent in municipality schools, and the remaining 14 percent in private schools. In small and rural municipalities, such as those considered here, the proportion of students in schools managed by

[10] All robustness checks in this paragraph are available in Section 3 of the Online Appendix.

local governments was 74 percent, while the proportions for state-run and private schools were 24 percent and 2 percent respectively (World Bank 1985).

In the 1980s local governments managed about 17 percent of public resources in Brazil (Shah 1991), about four percent of GDP, with 20 percent of local budgets going to education and similar shares to housing and urban infrastructure, and transportation spending, as shown in Table 1. 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 taxes.[11] Table 1 shows that FPM transfers were the most important source of revenue for the relatively small local governments considered here, amounting to about 50 percent on average and 56 percent in rural areas.

B Mechanics of revenue sharing in Brazil

In order to estimate the impact of intergovernmental transfers on outcomes, we exploit variation in FPM funding at several population cutoffs using regression discontinuity 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 municipality population in a discontinuous fashion. More specifically, based on municipality population estimates, pop^e, municipalities are assigned a coefficient $k = k(pop^e)$, where k(.) is the step function shown in Table 2. 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:

$$FPM_{mt} = \frac{k(pop^e_m)}{\sum_m k_m} rev_t$$

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

[11] 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.

based on 1980 census population from 1982 (the first year the 1980 census figures were used) until 1985.[12] 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.[13] Likewise, from 1986 to 1988, the transfers were also based on such extrapolations, this time based on 1970 and 1980 census population figures. Beginning in 1989, these extrapolations were updated on a yearly basis, which is still the practice today. 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 municipalities changed brackets due to decreases or, more often, increases in their population relative to 1980.[14] 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,

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

[13] 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).

[14] 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.

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.[15] 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).

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.[16] 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.[17] The cumulative transfer differential over the period 1982-1985 corresponds to about 2.5 percent of annual GDP in rural areas of the country and about 1.4 percent of annual GDP in urban areas for the counties in our estimation sample (Table 1).

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 percent at the first, 25 percent at the second, and 20 percent at the third cutoff. Though the differences are not great, this means

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

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

[17] 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.

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 IV below.

II Conceptual framework and identification

A Conceptual framework

Because the additional FPM transfers provide unrestricted budget support, effects on schooling and income may arise through a variety of channels in addition to education spending, such as improved local roads, for example. The following presents a framework for thinking about the causal effects estimated here and compares them to the micro and aggregate literatures on school resources, schooling, and earnings.

Assume that schooling S in the local community depends on public spending on education E, for example through class size C, and on another public input, say transportation infrastructure T, which in turn both depend on the overall level of local public spending or resources R of which FPM transfers F represent an important share. Also assume that household income I depends on schooling and public services (transportation for example). These relations can be summarized as follows:

$$\begin{aligned} S &= S(C(E(R(F))), T(R(F))) \\ I &= I(S(.), T(R(F))) \end{aligned}$$

Micro studies would typically estimate the effect of providing *real* resources to particular schools or classrooms, i.e. they would evaluate the *partial* derivatives S_C or I_C for example. In contrast, the effects estimated here can be thought of as S_F and I_F which represent *total* derivatives of schooling and income with respect to *financial* resource transfers, i.e. they capture effects arising through multiple spending channels, not just education spending. In particular, S_F and I_F both incorporate R_F, the marginal propensity to spend transfers received and E_R and T_R, the marginal propensities to spend on education and transportation infrastructure, respectively. These total derivatives may be higher or lower than those from specific education or infrastructure projects, depending on complementarities between these interventions.

The contribution of this paper is to provide the first quasi-experimental estimates of S_F and I_F, the effects of financial transfers on schooling and income, respectively. Existing aggregate studies on resource effectiveness in the education sector essentially evaluate S_E and I_E, as further discussed in the conclusion. The distinction between this study and most existing aggregate studies on school quality, education, and earnings might not be very significant in practice, however, since these aggregate studies typically use measures of school resources that are likely correlated with other dimensions of the public service environment as well.

If total spending is the only channel through which additional transfers operate (the exclusion restriction), the estimates presented here additionally identify S_R and I_R, the impacts of local public spending on schooling and income, respectively. Reductions in local taxes and corresponding increases in private consumption would violate the exclusion restriction for example. Empirically, local taxes do not seem to have responded to additional transfers as further detailed in Section VI.

B Identification

The key identifying assumption for this study is that unobservables vary smoothly as a function of population (if at all) and, in particular, do not jump at the cutoffs. As shown in Lee (2008) and Lee and Lemieux (2010), sufficient for the continuity of unobservables 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 with perfect control over local population numbers—such as through bribing surveyors—the problem for mayors was that they did not know the exact locations of the thresholds. 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. For this reason, it seems unlikely that sorting around the cutoffs would be a problem for our study.

It is useful in this context to compare this period with the period of the 1990s, during which Litschig (2012) has shown that population estimates on which transfers were based were manipulated. Population estimates are more easily manipulated than the census figures used in our study, but even the 1985 estimates (the last estimates made under the military government that had set up the rule) were not manipulated. There are probably three main reasons for this difference. The first is the change of political regime overseeing the transfers; the second is that by the 1990s the thresholds had been known for at least ten years; and a third is that the FPM started transferring larger amounts of money.

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 V 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 overrepresented to the right of the cutoffs in our study period.

A final potential concern is that other government policies are also related to the cutoffs specified in Decree 1881/81. If so, we would identify 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 in more recent years some government programs and policies do use other local population cutoffs for targeting. For example, the cutoffs determining wage caps for local legislators used in Ferraz and Finan (2009) were only introduced in the year 2000.[18]

III Data

Our analysis draws on multiple data sources from several time periods.[19] 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 pretreatment covariates the 1980 levels of municipality income per capita, average years of schooling for individuals 25 years and older, the poverty headcount 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 percent sample of the census and have been calculated by the national statistical agency (only a shorter census survey was administered to 100 percent of the population). The 1991 poverty rate was calculated

[18] The cutoffs for the wage cap legislation are 10'000, 50'000, 100'000, 300'000, and 500'000.

[19] Some of the municipalities in our estimation samples lost territory and population to newly created municipalities between 1980 and 1991. Specifically, 29 of the 386 municipalities in our largest local sample (p=4%) were such origin municipalities, 16 in the treatment group, and 13 in the comparison group. In the largest sample (N=1243, p=15%), the total of such origin municipalities is 107, 53 in the treatment, and 54 in the comparison group.

Ideally, we would want to add 1991 outcomes for individuals from the break-away parts of the origin municipalities. Unfortunately, however, our census microdata does not come with census-tract identifiers. However, since all our outcome variables are in per capita terms we think that the resulting measurement error is likely small. Moreover, the fact that municipalities that lost part of their territory are represented in almost identical proportions in treatment and comparison groups suggests that the measurement error is likely uncorrelated with treatment status. Consistent with this argument, our results are quantitatively unchanged if we exclude municipalities that lost territory and population due to municipal splits between 1980 and 1991.

by the government research institute IPEA[20] 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 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.

We also compute average years of schooling and the literacy rate for the 9- to 18-years-old cohort in 1991 (0-9 in 1982) because local governments in Brazil also provided preschool education and day-care services which could have benefited even the newborn cohort in 1982. One would expect this younger age group to exhibit a smaller treatment effect (at least in absolute terms) because most of them were not of elementary schooling age when spending increased in 1982. Moreover, most of this cohort had not completed elementary school in 1991 and so part of the impact on their level of schooling might be missed if the spending increase produced school supply improvements that had not faded completely by 1991. The 19- to 28-year-olds in contrast likely completed elementary and even intermediary education by 1991.

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

[20] Instituto de Pesquisa Econômica Aplicada.

schooling age. We show below that this was indeed the case.

Table 1 shows descriptive statistics for the variables used in the statistical analysis, as well as other information regarding revenue and expenditures in the municipalities.

IV 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 VI 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.[21] 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 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 percent 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 pretreatment 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

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

from the cutoffs is then:

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

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

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:

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

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

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.

As a specification check we also use equation (1) in a Two-Stage-Least-Squares (TSLS) approach with theoretical FPM transfers per capita (using modal FPM levels from Figure 1) as the endogenous variable and the three indicators, $1[pop_{ms} > c_1]$, $1[pop_{ms} > c_2]$, and $1[pop_{ms} > c_3]$, as multiple instruments. Finally, we use Angrist and Lavy's (1999) IV approach, putting theoretical FPM transfers per capita as the instrument for actual FPM transfers per capita, controlling for population.[22] The results of these alternative estimation approaches are quantitatively similar to those we get when we divide our reduced form estimates based on equation (2) by the average jump in FPM per capita funding (100R$ per capita across cutoffs c_1, c_2, and c_3, and 110R$ across c_1 and c_2). These additional results are omitted to save space and are available on request.

V 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).[23]

In Table 3, we estimate equation (2) pooled across the first three cutoffs for a host of pretreatment 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 pretreatment covariates mentioned above. Nor is there statistical evidence of pretreat-

[22] For this approach we impute missing actual FPM amounts with the modal levels shown in Figure 1.

[23] The estimates (and standard errors) are, for the first to sixth cutoffs respectively, -0.072 (0.095), 0.011 (0.111), 0.180 (0.136), 0.054 (0.174), -0.011 (0.269), 0.350 (0.357). Separate density plots for each cutoff are available in Figure 1 of the Online Appendix.

ment 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 pretreatment 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.[24] 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.[25] Results from quadratic specifications (available in the Online Appendix, Table 1) confirm this view: virtually none of the pretreatment 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 pretreatment covariate at conventional levels of significance (lowest p-value is 0.23).[26] 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 pretreatment 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 VI below we show that the estimated effects are robust to the inclusion of relevant pretreatment covariates, including the four pretreatment education and earnings outcomes shown in Table 3. Results are also robust to using the difference in average schooling and literacy outcomes over time, rather than the 1991 levels, as further discussed in the Online Appendix.

[24]To be precise, out of the 60 estimates in Table 3, 5 are significant at the 10 percent level and 4 at the 5 percent level. This is close to what one would expect if all parameters were indeed zero, namely 6 at 10 percent and 3 at 5 percent significance. Among the 60 estimates based on quadratic population polynomials in Table 1 in the Online Appendix, there are 4 significant at 10 percent and 3 at 5 percent.

[25]See for example Lee and Lemieux (2010) for more discussion on this point.

[26]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 2010).

VI Estimation results

This section presents the main estimation results, while robustness checks and additional results are relegated to the Online Appendix.[27] The section starts out by demonstrating that FPM transfers increased local public spending per capita by about 20 percent, with no evidence of crowding out own revenue or other revenue sources. The second subsection presents the main empirical result of the paper, which is that communities that received extra financing from the central government benefited in terms of education outcomes (higher schooling and literacy rates). The third subsection presents and discusses impacts on income (lower poverty rates). The last subsection shows that the probability of reelection increased by about 10 percentage points.

All the tables below show results for the first two cutoffs pooled and the first three cutoffs pooled, as well as for the cutoffs individually. The tables present results for successively larger samples around the cutoffs (typically $p = 2, 3, 4,$and 15 percent) 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 pretreatment 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.

A Impacts 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 percent) and the jump in per capita FPM transfers at

[27]Section 1 in the Online Appendix shows that local spending shares remained essentially unchanged—that is, local spending on education, housing and urban infrastructure, and transportation all increased by about 20% per capita. Section 2 in the Online Appendix shows that direct evidence on public service improvements is mixed at best: while there is some indication that student-teacher ratios in local primary school systems fell, there is little evidence that housing and urban development spending affected housing conditions. Section 3 in the Online Appendix discusses further robustness checks for the education and earnings gains. Section 4 of the Online Appendix shows that additional resources had stronger effects on schooling and literacy in less developed parts of Brazil, while poverty reduction was evenly spread across the country.

the cutoffs (about 33 percent for the 10'188 cutoff and less for subsequent cutoffs).[28] This result is also 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 are almost always at 1 or above, statistically different from zero, and virtually never statistically different from unity (results are available on request).

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.[29] 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 VI.B below). Another point worth noting is that the included pretreatment 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 graphical evidence of the discontinuity in public spending. 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. There are about 50 municipalities per bin. 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.[30] 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

[28] 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 $\Delta R/R = \Delta F/F \times F/R + \Delta O/O \times O/R$. If $\Delta O = 0$, as shown below, and $F/R = 0.5$ on average, as shown in Table 1, then $\Delta R/R = 33\% \times 50\% = 16.5\%$. The estimates in Table 4 are somewhat larger, perhaps because municipalities with missing FPM information rely more heavily on FPM funding, in which case 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 variable is scaled by population, P: $\Delta \ln(R/P) = \Delta \ln(R) \cong \Delta R/R$.

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

[30] 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 2010).

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 (panel D), all cumulative over the period 1982-1985. It is worth noting that both the regression functions for public spending per capita and FPM per capita exhibit a jump at the cutoff and slope downward to the left and to the right of the cutoff. At the same time, Figure 4 shows that there is no discontinuity in other revenues and that, if anything, own revenues have actually increased during the period of extra FPM funding. This suggests that the effects on education, poverty, and party reelection 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 II seems to hold. Statistical analysis confirms this conclusion but is not presented here to save space.

B Impacts on education outcomes

Table 5 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 percent) even within a relatively small neighborhood of +/- 3 percent 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 percent or about 0.2 standard deviation.

Since local governments in Brazil provided preschool 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 in absolute terms. Estimates for the younger cohort of

9- to 18-year-olds in 1991 (0-9 in 1982) shown in Table 6 suggest a schooling gain of about 0.15 years per capita. Pooled estimates are again mostly significantly different from zero even in the discontinuity samples. For the younger cohort, the marginal comparison group years of schooling were 2.6 years with a standard deviation of 1.08 years. The 0.15 schooling gain thus amounts to about 6 percent or 0.14 standard deviation.

It is important to note that the 4.3 average years of schooling for the older cohort 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.

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$.[31] Assuming that about 20 percent of the additional FPM funds were spent on education, and assuming further that only the 0- to 18-year-olds in 1982, about 50 percent of the total population,[32] were at least marginally affected by the spending boom, marginal education spending *per student* was about $\$71 \times 0.2 \times 2 = \$28.4$. 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

[31]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.

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

average of 0.225, the implied marginal cost of an additional completed year of schooling is about $\$28.4/0.225 = \$126$. 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 $\$31 \times 2/0.625 = \$99$. While these are rough estimates, the similarity of the marginal cost to the average cost indicates that the findings here are plausible.

Table 7 shows that students not only completed more grades in municipalities that received extra funds but that for some of them it made the difference between being able to read and write or not. For the older cohort the effect on literacy amounts to about 4 percentage points, compared to an average literacy rate of about 76 percent in the comparison group. Pooled estimates are again mostly significantly different from zero even within a relatively small neighborhood of +/- 3 percent around the cutoffs. For the younger cohort the literacy gain is about 3 percentage points compared to an average literacy rate of about 74 percent in the comparison group (Online Appendix Table 7.2). Panels A and B of Figure 5 show the schooling and literacy results for the older cohort graphically (the graphs for the younger cohort are omitted to save space and are available on request).[33] In line with this graphical evidence, estimates of the discontinuity for neighborhoods not shown in Tables 5, 6, and 7 are quantitatively similar to the estimates presented here and are available upon request.

C Impacts on poverty and income per capita

Both better and more widespread education and improved public service provision (better infrastructure for example) are likely to increase household incomes. Our results suggest that the extra public spending indeed had an effect on income, although only for the poor. Specifically, Table 8 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 percent in the comparison group.

[33] 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.

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 percent) 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 available on request).

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 provision 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—23 percent and 27 percent, respectively, according to De Carvalho (1997)—we can thus estimate what percent of the overall population got an additional year of schooling, namely about $23\% \times 30\% + 27\% \times 15\% = 11\%$.

Now suppose that an extra year of schooling raises wages by 12 percent (Behrman and Birdsall 1983), that labor supply is constant, and that about 10 percent of the population earn per capita income that falls within a 12 percent range below the poverty line. Suppose further that about 64 percent 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 8) 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 provision overall.

D Impact on the probability of reelection

As discussed in the introduction, one reason that municipal mayors may have allocated extra funds to public service improvements is electoral accountability. Because of weak term limit rules, incumbent mayors could not be individually reelected to serve consecutive terms but could be reelected in a later term. If public service improvements while in office had any positive benefit for future reelection prospects, mayors would have had an incentive to turn extra funds into marginal public service improvements. To explore this possibility, we analyze whether citizens were more likely in the 1988 local elections to reelect the party of the mayor elected in the previous elections in 1982 (1 for reelection, 0 otherwise). Voters in 1988 chose from 31 political parties—sixteen of which were winners somewhere in the country—to elect mayors in about 4000 municipalities.

Table 9 presents estimation results for the party reelection indicator based on the linear probability model. Most of the pooled estimates shown in rows 1 and 2 fall in the range 0.7 to 0.13, with a modal value of around 10 percentage points. The estimates at individual cutoffs are positive almost without exception. Although they seem to vary considerably, almost all of these estimates fall within the 95 percent confidence interval around the most precise estimate in row 1 column 7 of Table 9 (0.002, 0.167). 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 percent) 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 of at least 50 percent compared to the narrowest neighborhoods. The same pattern of results arises when state dummies are included or with probit estimates (results available on request).

We also estimate the impact on reelection using quadratic and cubic polynomial specifications of the running variable as further robustness checks. These estimates (available in the Online Appendix, Table 9.1) fall in the same range as those in Table 9. Most of the nonlinear estimates are not statistically significant because standard errors increase substantially compared to the linear model (the standard error is 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. There is virtually no statistical evidence against the null hypothesis of a linear model in any of our specifications. In addition to the *a priori* case for a linear specification based on the relationship between population and spending per capita shown in panel B of Figure 4, these statistical test results further corroborate our focus on the linear estimates and standard errors.

Panel D of Figure 5 presents graphical evidence of the discontinuity in reelection rates. The jump in solid lines at the cutoff approximately corresponds to the estimate of the discontinuity in the first row and fifth column of Table 9. As with the jump in FPM funding and total spending per capita above, Figure 5 shows that the jump in the reelection probability is visually robust irrespective of the width of the neighborhood examined, although there is visibly more variance in this figure than in FPM funding or total per capita spending (Panels A and B of Figure 4). In line with this graphical evidence, estimates of the discontinuity for neighborhoods not shown in Table 9 are quantitatively similar to the estimates presented here and are available on request.

We conclude from these results that additional local government spending per capita of about 20 percent over a four-year period improved the reelection probability of local incumbent parties in the 1988 elections by about 10 percentage points. Equivalently, we estimate a semielasticity $\frac{\partial p^*}{\partial g/g}$ of about 0.5.

VII Conclusion

Results presented in this paper suggest that communities that received extra financing from the central government benefited in terms of education outcomes and poverty reduction. It is worth remembering that—under the standard continuity assumption—all of these effects were triggered by 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 at the time.

As with any regression discontinuity analysis, the impacts presented in this paper apply only to municipalities with population levels at the respective cutoffs. However, because our results are quantitatively similar across thresholds 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 percent of Brazilian municipalities at the time.

Whether providing additional resources to local governments in other contexts would also improve education outcomes and reduce poverty is not clear. In fact, related studies of the effects of natural resource-based revenue windfalls on government behavior at the local level in Brazil yield mixed results. Caselli and Michaels (2013) look at impacts of fiscal windfalls from oil on local spending and living standards, finding little if any effect on local public services or household income per capita. They conclude that the additional oil revenue was mostly embezzled or used for patronage but that general revenue might have been put to more productive uses. Monteiro and Ferraz (2010) also look at oil royalties in Brazil and consider political outcomes in addition to public service provision. They find that the extra oil revenues were turned into government jobs, but find no effects on infrastructure, education, or health supply. They also find that the extra oil revenue increased the probability of reelection in the short run and, similar to us, conclude that accountability mechanisms led politicians to spend revenues in ways their citizens valued.

In sum, none of these studies—including ours—find direct evidence of improved public service provision. However, our study finds education gains while the other two studies do not consider education outcomes. In addition, we find income gains for the poor, while Caselli and Michaels (2013) find negligible impacts on the poverty rate (Monteiro and Ferraz do not look at poverty). An important reason for the difference in results on poverty may be the quality of information populations have about revenues. As Caselli and Michaels suggest, there may be less voter information regarding oil royalties compared to financing from other sources, such as federal transfers.[34] Moreover, keeping track of government finances may be harder in larger cities, and the average population size of the municipalities in our studies differs by an order of magnitude: about 150'000 in Caselli and Michaels and 13'000 in ours.

It is also useful to note that the findings reported here are generally in line with much existing work from two literatures. The result that additional transfers to local governments increased local public spending one-for-one, with no evidence of crowding out own revenue or other revenue sources, has been found in many previous studies in the literature on intergovernmental grants

[34]Cross-country evidence suggests that producers of oil and gas tend to have less accessible and comprehensive public budgets than countries that are not resource dependent (Heuty and Carlitz 2008).

and local spending.[35] This empirical regularity is referred to as the "flypaper effect", since the grant money sticks where it hits (the public budget) rather than finding its way into private budgets (through tax breaks or direct transfers), which is what theory would predict if transfer income and private income were perfectly fungible and local government spending decisions reflected preferences of voters (Bradford and Oates 1971).

Additionally, the positive effects on schooling reported here are qualitatively consistent with aggregate studies in the U.S. and in developing countries that look at the links between school resources, educational attainment, and earnings.[36] The positive effects on educational achievement (literacy) are also in line with most of the estimates in the aggregate literature that evaluates effects on test scores, which is summarized by Hanushek (2006).[37] However, in contrast to most of these studies, the results presented here are comfortably significant at conventional levels. The poverty reduction estimated here is also in line with most of the aggregate literature for the US, which tends to find positive and statistically significant effects of school resources on earnings (Card and Krueger 1996). For developing countries, the aggregate evidence on school availability, school resources and earnings is scant, except for Duflo's (2001) study on school construction in Indonesia, which shows positive effects on earnings (in addition to positive effects on schooling), and Behrman and Birdsall (1983), and Behrman, Birdsall and Kaplan (1996), who also estimate positive returns to measures of school quality in Brazil.

Needless to say, the results that transfers were spent one-for-one and that they had an impact on education and earnings outcomes do not imply that none of the extra funds were privately appropriated by the incumbent or wasted. Indeed, as mentioned in the introduction, classical agency models of political accountability can generate predictions of corruption and public service provision increasing simultaneously as a result of increased financing. Increasing the accountability of both local politicians and service providers is therefore likely to improve public service provision,

[35] The result is less 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. See Hines and Thaler (1996) for a review of the flypaper literature and problems with the empirical work.

[36] For aggregate evidence for the U.S. see Card and Krueger (1992) and Heckman, Layne-Farrar, Todd (1996). For developing countries see Birdsall (1985) for Brazil, Case and Deaton (1999) for South Africa and Duflo (2001) for Indonesia. For micro studies see Chin (2005) and Banerjee, Jacob, Kremer, Lanjouw and Lanjouw (2002) for evidence on India. Glewwe, Kremer and Moulin (2009) provide evidence for Africa.

[37] See Hoxby (2000) and Hanushek (2006) for a skeptical reading of the evidence on resource effects in education, both in the US and in developing countries. See Krueger (2003) and Krueger and Whitmore (2001) for the view that additional education resources, class size reductions in particular, do matter in the US.

as discussed in Björkman and Svensson (2009) for example.

Our results do suggest, however, that even in the absence of reforms that strengthen local accountability, and despite well founded worries about corruption, other leakages, and local capture, local governments in Brazil did use the additional federal transfers they received to expand public services to the general local population at reasonable cost. Given the impacts on education outcomes we find, investigating impacts on health outcomes seems like a natural next step. Future research might also attempt to assess the relative magnitudes of leakage and service provision in (marginal) government spending by looking at the exact services the money was spent on. Another important question left unanswered here is what the effects of extra funding would be in a centralized system without locally elected politicians. Given the scarcity of studies that trace the effects of funds on spending to public services and into development outcomes, there is thus a lot of room for future research.

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Table 1: Descriptive statistics (sample means)

	Population range					
	7'500 - 44'148	8'500 - 18'700				
Sample	Full	Full	North	South	Rural	Urban
Observations	2'353	1'277	564	713	627	628
1980 county characteristics (IBGE)						
Average years of schooling (25 years and older)	1.95	1.89	1.06	2.56	1.51	2.28
Percentage of residents living in urban areas (percent)	29.95	27.85	22.34	32.31	14.53	41.14
Net enrollment rate of 7- to 14-year-olds (percent)	55.45	55.27	39.64	67.65	48.59	62.26
Illiteracy rate, 15 years and older (percent)	39.18	39.23	55.46	26.38	44.44	33.95
Poverty headcount ratio (national poverty line, percent)	58.81	59.56	77.57	45.29	68.07	51.27
Income per capita (percent of minimum salary in 1991)	77.20	74.85	41.70	101.12	58.39	91.18
Infant mortality (per 1000 life births)	88.60	88.06	126.52	57.60	94.96	81.97
GDP ('000'000) 2008 Reais (IPEA)	107.38	63.54	33.01	87.72	46.63	79.72
1982 Financial data (Ministry of Finance)						
Total revenue ('000) 2008 Reais	3'957	2'876	1'825	3'562	2'370	3'335
Total revenue 1982/GDP 1980 (percent)	5.30	5.62	7.30	4.52	6.24	5.07
FPM transfers/total revenue (percent)	48.02	49.76	66.45	37.95	56.23	43.87
Own revenue/total revenue (percent)	5.91	5.19	1.11	7.71	2.64	7.55
Other revenue/total revenue (percent)	46.95	45.95	32.95	54.68	42.11	49.20
Administrative spending/total spending (percent)	22.32	22.32	22.94	21.71	21.40	23.12
Education spending/total spending (percent)	20.96	21.27	23.91	18.66	22.32	20.16
Housing and urban spending/total spending (percent)	19.52	17.99	19.91	16.04	15.96	20.29
Health spending/total spending (percent)	9.87	10.42	14.39	6.32	11.17	9.79
Transportation spending/total spending (percent)	20.92	21.85	12.30	30.37	23.59	19.54
Other spending/total spending (percent)	8.54	8.57	8.59	8.54	8.18	9.10
1991 Real school resources (school census)						
Number of municipal elementary schools	37.81	30.38	41.00	19.81	37.57	21.86
Primary school student-teacher ratio	20.41	19.81	22.48	17.14	20.31	19.19
1991 Housing and urban services (IBGE)						
Individuals with access to electricity (percent)	70.63	69.52	52.12	83.29	57.31	81.77
Individuals with access to drinking water (percent)	69.44	68.73	49.41	83.83	61.54	76.08
Individuals with access to sewer (percent)	41.17	41.44	19.66	54.25	30.58	50.59
Individuals living in inadequate housing (percent)	0.63	0.55	0.65	0.47	0.48	0.63
1991 education outcomes (census)						
Average years of schooling (19- to 28-year-olds)	4.63	4.56	3.37	5.51	3.99	5.13
Literacy rate (19- to 28-year-olds) (percent)	78.80	78.97	63.68	91.07	73.70	84.07
Average years of schooling (9- to 18-year-olds)	2.84	2.84	1.85	3.62	2.49	3.18
Literacy rate (9- to 18-year-olds) (percent)	76.76	77.09	58.43	91.87	70.85	83.03
1991 Household income (IBGE)						
Poverty headcount ratio (R$140 poverty line, percent)	59.66	60.37	79.38	45.33	69.36	51.76
Household income per capita, 2008 Reais	223	217	119	294	168	264
1982 and 1988 Electoral outcomes (TSE)						
Opposition (PDT, PMDB, PT, or PTB) in 1982 (0/1)	0.34	0.30	0.12	0.45	0.24	0.37
Party reelection in 1988 (0/1)	0.23	0.21	0.12	0.29	0.20	0.22

Notes: North includes North and Northeast regions, South includes Center-West, Southeast and South regions. North region: Acre, Amazonas, Para, Amapa, Rondonia, Roraima states; Northeast region: Maranhao, Piaui, Ceara, Rio Grande do Norte, Paraiba, Pernambuco, Alagoas, Sergipe, Bahia states; Center-West region: Mato Grosso, Mato Grosso do Sul, Goias states; Southeast region: Minas Gerais, Espirito Santo, Rio de Janeiro, Sao Paulo states; South region: Parana, Santa Catarina, Rio Grande do Sul states. 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 2: Brackets and coefficients for the FPM transfer

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

Source: Decree 1881/81

Table 3: Test of discontinuities in pretreatment covariates

Polynomial specification:	Linear	Linear	Linear	Linear	Linear
Neighborhood (percent):	2	3	4	5	6
Opposition party (0/1)	-0.131	-0.066	-0.044	-0.053	-0.067
	(0.108)	(0.092)	(0.081)	(0.072)	(0.066)
Average years of schooling	0.052	0.167	0.190	0.219**	0.145
(25 years and older)	(0.173)	(0.136)	(0.116)	(0.107)	(0.093)
Urban residents (percent)	0.348	0.542	-0.544	0.280	-1.843
	(4.538)	(3.607)	(3.069)	(2.871)	(2.538)
Net enrollment rate (percent)	2.132	3.692	4.705**	4.365**	2.043
(7- to 14-year-olds)	(3.810)	(2.879)	(2.386)	(2.120)	(1.877)
Literacy rate (percent)	1.053	1.335	2.330	2.627	1.582
(15 years and older)	(3.143)	(2.267)	(1.940)	(1.769)	(1.574)
Poverty headcount ratio (percent)	3.940	-0.512	-1.308	-1.739	-0.017
(national poverty line)	(3.718)	(2.845)	(2.416)	(2.206)	(1.932)
Income per capita (percent)	-3.150	2.856	4.202	5.840	2.687
(percent of minimum salary)	(8.213)	(5.905)	(4.904)	(4.510)	(3.979)
Infant mortality	-2.245	-3.667	-6.387	-3.810	-3.531
(per 1000 life births)	(5.386)	(4.469)	(4.078)	(3.455)	(3.189)
Log current transfers 1981	0.090	0.067	0.081	0.068	0.007
(per capita)	(0.093)	(0.071)	(0.066)	(0.061)	(0.056)
Log capital transfers 1981	0.027	0.097	0.097	0.062	0.064
(per capita)	(0.163)	(0.129)	(0.127)	(0.109)	(0.099)
Log total revenue 1981	0.085	0.080	0.130**	0.109*	0.050
(per capita)	(0.090)	(0.072)	(0.062)	(0.057)	(0.052)
Log own revenue 1981	0.498	0.464	0.411	0.348	0.299
(per capita)	(0.421)	(0.319)	(0.260)	(0.234)	(0.216)
Municipalities	202	297	391	479	570
F-statistic	0.85	0.82	1.27	1.20	1.27
[p-value]	[0.60]	[0.63]	[0.24]	[0.28]	[0.23]

Notes: Table entries are OLS estimates (standard errors) of discontinuities in pretreatment covariates using the pooled specification across the first three cutoffs described in Section IV, equation (2) 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 (percent) is percent 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 percent, 5 percent and 10 percent 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 (percent):	2	2	3	3	4	4	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.158**	0.191***	0.161***	0.145***	0.197***	0.167***	0.156***
	(0.076)	(0.063)	(0.060)	(0.050)	(0.054)	(0.046)	(0.036)
Observations	198	195	291	288	384	380	1235
R-squared	0.75	0.85	0.71	0.81	0.66	0.77	0.75
Pooled cutoffs 1-2							
I[X > 0]	0.181*	0.252***	0.187**	0.185***	0.212***	0.192***	0.191***
	(0.100)	(0.080)	(0.077)	(0.067)	(0.071)	(0.061)	(0.047)
Observations	129	128	198	197	257	255	850
R-squared	0.73	0.84	0.73	0.81	0.69	0.78	0.75
1st cutoff							
I[pop > 10188]	0.140	0.365**	0.235**	0.250**	0.224**	0.229**	0.233***
	(0.162)	(0.152)	(0.109)	(0.107)	(0.092)	(0.088)	(0.055)
Observations	65	64	100	99	134	132	464
R-squared	0.83	0.90	0.84	0.89	0.78	0.84	0.78
2nd cutoff							
I[pop > 13584]	0.187	0.147	0.181	0.234*	0.225**	0.247**	0.165*
	(0.171)	(0.168)	(0.120)	(0.125)	(0.108)	(0.106)	(0.094)
Observations	64	64	98	98	123	123	386
R-squared	0.71	0.83	0.70	0.81	0.69	0.80	0.74
3rd cutoff							
I[pop > 16980]	0.057	0.026	0.048	0.014	0.102	0.072	0.108**
	(0.149)	(0.101)	(0.123)	(0.075)	(0.113)	(0.070)	(0.045)
Observations	69	67	93	91	127	125	385
R-squared	0.83	0.93	0.79	0.92	0.66	0.83	0.76
4th cutoff							
I[pop > 23772]	0.091	0.143	0.105	0.079	0.107	0.001	0.062
	(0.232)	(0.133)	(0.163)	(0.128)	(0.129)	(0.109)	(0.072)
Observations	50	50	77	76	107	106	369
R-squared	0.80	0.90	0.78	0.86	0.76	0.84	0.81

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

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

Table 5: Impact on schooling, 19- to 28-year-olds in 1991

Dependent variable: average years of schooling, 19- to 28-year-olds in 1991; comparison mean: 4.26

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (percent):	2	2	3	3	4	4	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.322	0.225	0.516***	0.301***	0.528***	0.275***	0.351**
	(0.260)	(0.151)	(0.198)	(0.114)	(0.171)	(0.102)	(0.139)
Observations	202	199	297	294	391	387	1271
R-squared	0.71	0.89	0.71	0.89	0.69	0.88	0.88
Pooled cutoffs 1-2							
I[X > 0]	0.401	0.182	0.499**	0.299**	0.476**	0.283**	0.364**
	(0.325)	(0.180)	(0.241)	(0.139)	(0.214)	(0.129)	(0.178)
Observations	133	132	203	202	263	261	874
R-squared	0.73	0.90	0.73	0.89	0.70	0.88	0.87
1st cutoff							
I[pop > 10188]	0.212	0.504	0.365	0.395	0.327	0.373	0.478
	(0.503)	(0.475)	(0.350)	(0.298)	(0.337)	(0.239)	(0.311)
Observations	68	67	103	102	137	135	479
R-squared	0.78	0.90	0.77	0.89	0.74	0.89	0.87
2nd cutoff							
I[pop > 13584]	0.398	0.347*	0.497	0.338*	0.570*	0.257	0.225
	(0.530)	(0.204)	(0.373)	(0.173)	(0.305)	(0.158)	(0.193)
Observations	65	65	100	100	126	126	395
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	0.375
	(0.507)	(0.333)	(0.387)	(0.225)	(0.354)	(0.192)	(0.231)
Observations	69	67	94	92	128	126	397
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 (percent) is percent distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

Table 6: Impact on schooling, 9- to 18-year-olds in 1991

Dependent variable: average years of schooling, 9- to 18-year-olds in 1991; comparison mean: 2.61

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (percent):	2	2	3	3	4	4	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.207	0.155	0.287**	0.166**	0.288***	0.136**	0.177**
	(0.157)	(0.095)	(0.117)	(0.071)	(0.099)	(0.062)	(0.082)
Observations	202	199	297	294	391	387	1271
R-squared	0.84	0.94	0.83	0.93	0.81	0.93	0.92
Pooled cutoffs 1-2							
I[X > 0]	0.263	0.171	0.283*	0.205**	0.265**	0.164**	0.214**
	(0.205)	(0.121)	(0.148)	(0.089)	(0.129)	(0.080)	(0.102)
Observations	133	132	203	202	263	261	874
R-squared	0.83	0.94	0.83	0.93	0.81	0.92	0.91
1st cutoff							
I[pop > 10188]	0.209	0.385	0.302	0.330**	0.231	0.240*	0.350**
	(0.272)	(0.266)	(0.200)	(0.166)	(0.187)	(0.132)	(0.169)
Observations	68	67	103	102	137	135	479
R-squared	0.88	0.95	0.87	0.93	0.84	0.93	0.91
2nd cutoff							
I[pop > 13584]	0.298	0.264	0.255	0.191	0.295	0.102	0.098
	(0.311)	(0.157)	(0.225)	(0.117)	(0.191)	(0.110)	(0.129)
Observations	65	65	100	100	126	126	395
R-squared	0.84	0.96	0.84	0.95	0.81	0.93	0.92
3rd cutoff							
I[pop > 16980]	0.052	0.145	0.127	0.022	0.296	0.060	0.128
	(0.282)	(0.225)	(0.212)	(0.145)	(0.189)	(0.125)	(0.147)
Observations	69	67	94	92	128	126	397
R-squared	0.88	0.96	0.85	0.95	0.83	0.94	0.94

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

Table 7: Impact on literacy, 19- to 28-year-olds in 1991

Dependent variable: literacy rate, 19- to 28-year-olds in 1991; comparison mean: 0.76

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (percent):	2	2	3	3	4	4	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.057**	0.047***	0.062***	0.049***	0.059***	0.041***	0.053***
	(0.027)	(0.016)	(0.019)	(0.012)	(0.016)	(0.011)	(0.014)
Observations	202	199	297	294	391	387	1271
R-squared	0.78	0.91	0.80	0.91	0.80	0.91	0.90
Pooled cutoffs 1-2							
I[X > 0]	0.054	0.043**	0.044**	0.046***	0.042**	0.035***	0.052***
	(0.035)	(0.019)	(0.023)	(0.015)	(0.020)	(0.014)	(0.018)
Observations	133	132	203	202	263	261	874
R-squared	0.79	0.93	0.82	0.93	0.81	0.91	0.90
1st cutoff							
I[pop > 10188]	0.051	0.062	0.050	0.072***	0.034	0.042*	0.068**
	(0.060)	(0.037)	(0.040)	(0.026)	(0.039)	(0.025)	(0.029)
Observations	68	67	103	102	137	135	479
R-squared	0.82	0.95	0.84	0.94	0.82	0.93	0.90
2nd cutoff							
I[pop > 13584]	0.044	0.031	0.036	0.022	0.051**	0.027*	0.035*
	(0.040)	(0.023)	(0.028)	(0.015)	(0.023)	(0.015)	(0.020)
Observations	65	65	100	100	126	126	395
R-squared	0.82	0.95	0.85	0.93	0.86	0.93	0.90
3rd cutoff							
I[pop > 16980]	0.044	0.042	0.065*	0.042	0.073**	0.030	0.054**
	(0.044)	(0.030)	(0.036)	(0.026)	(0.031)	(0.020)	(0.025)
Observations	69	67	94	92	128	126	397
R-squared	0.86	0.95	0.84	0.94	0.82	0.94	0.91

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

Table 8: Impact on the poverty rate in 1991

Dependent variable: poverty rate in 1991; comparison mean: 0.64

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (percent):	2	2	3	3	4	4	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	-0.037	-0.064***	-0.060**	-0.051***	-0.054**	-0.037**	-0.050**
	(0.039)	(0.022)	(0.029)	(0.017)	(0.024)	(0.015)	(0.020)
Observations	202	199	297	294	391	387	1271
R-squared	0.79	0.93	0.78	0.92	0.76	0.91	0.90
Pooled cutoffs 1-2							
I[X > 0]	-0.010	-0.051*	-0.040	-0.039*	-0.030	-0.020	-0.047*
	(0.053)	(0.029)	(0.039)	(0.023)	(0.032)	(0.020)	(0.025)
Observations	133	132	203	202	263	261	874
R-squared	0.77	0.94	0.77	0.93	0.75	0.92	0.90
1st cutoff							
I[pop > 10188]	-0.007	-0.091*	-0.019	-0.040	-0.014	-0.018	-0.055
	(0.057)	(0.049)	(0.044)	(0.033)	(0.042)	(0.031)	(0.037)
Observations	68	67	103	102	137	135	479
R-squared	0.86	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	-0.040	-0.026	-0.050
	(0.102)	(0.055)	(0.063)	(0.040)	(0.052)	(0.034)	(0.036)
Observations	65	65	100	100	126	126	395
R-squared	0.73	0.94	0.74	0.93	0.74	0.93	0.91
3rd cutoff							
I[pop > 16980]	-0.097	-0.105**	-0.097*	-0.071**	-0.088**	-0.062**	-0.065**
	(0.067)	(0.048)	(0.051)	(0.032)	(0.044)	(0.027)	(0.034)
Observations	69	67	94	92	128	126	397
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 (percent) is percent distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

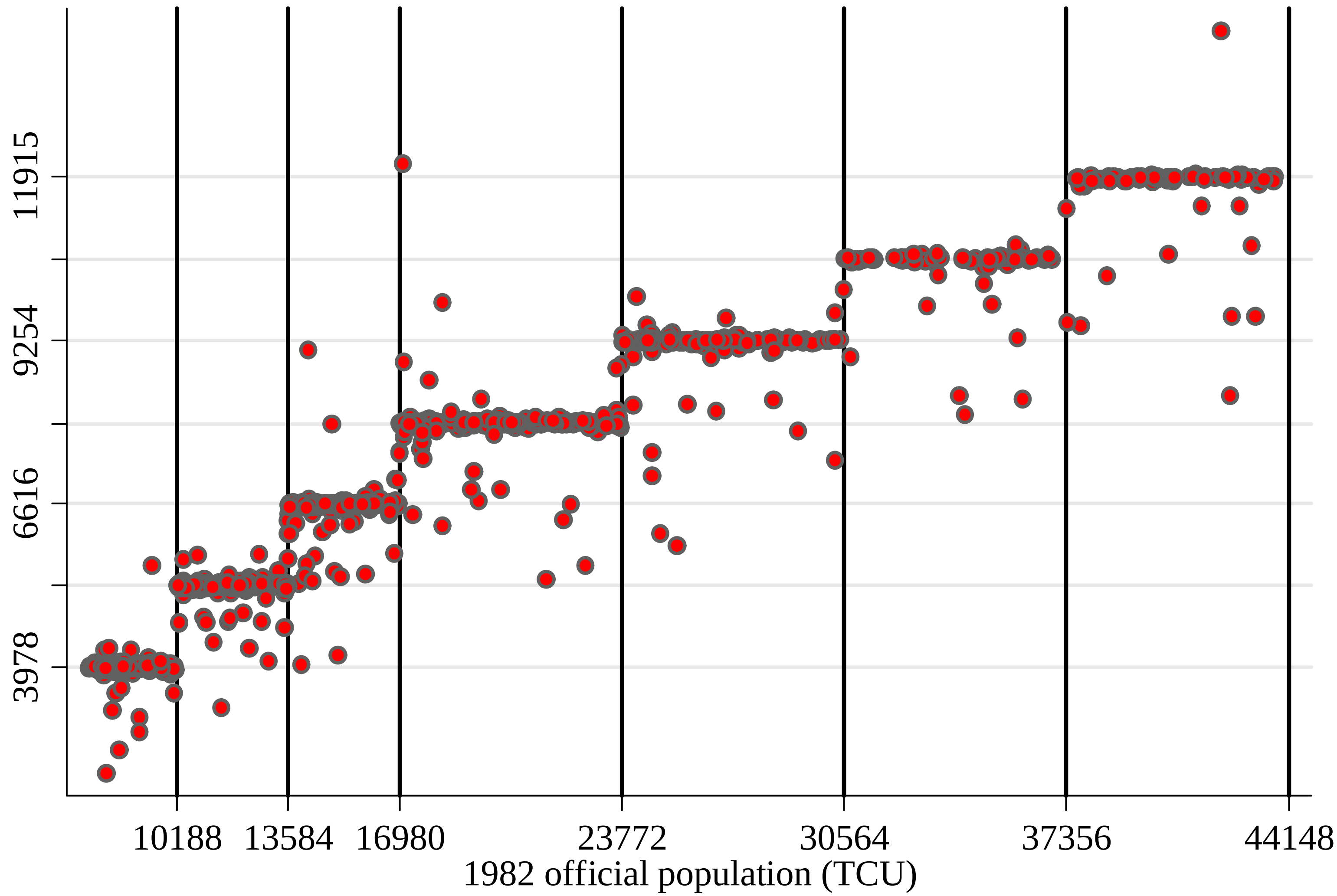
Table 9: Impact on the probability of reelection in 1988

Dependent variable: incumbent party reelected for mayor's office in 1988; comparison mean: 0.17

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Neighborhood (percent):	2	2	4	4	10	10	15
Pretreatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X> 0]	0.119	0.186*	0.086	0.106	0.082	0.103**	0.084**
	(0.112)	(0.107)	(0.079)	(0.073)	(0.051)	(0.048)	(0.042)
Observations	197	197	379	379	940	940	1242
R-squared	0.04	0.20	0.01	0.14	0.02	0.16	0.18
Pooled cutoffs 1-2							
I[X > 0]	0.024	0.135	0.022	0.074	0.100	0.125**	0.100*
	(0.147)	(0.144)	(0.101)	(0.095)	(0.063)	(0.060)	(0.052)
Observations	131	131	254	254	633	633	856
R-squared	0.05	0.16	0.01	0.12	0.02	0.16	0.18
1st cutoff							
I[pop > 10188]	0.089	0.236	0.023	0.129	0.147	0.207***	0.139**
	(0.238)	(0.211)	(0.154)	(0.130)	(0.090)	(0.080)	(0.068)
Observations	67	67	136	136	321	321	472
R-squared	0.09	0.24	0.01	0.24	0.02	0.22	0.21
2nd cutoff							
I[pop > 13584]	-0.018	0.071	0.021	0.034	0.051	0.049	0.060
	(0.187)	(0.192)	(0.134)	(0.132)	(0.088)	(0.088)	(0.081)
Observations	64	64	118	118	312	312	384
R-squared	0.01	0.10	0.00	0.02	0.01	0.11	0.13
3rd cutoff							
I[pop > 16980]	0.305*	0.233	0.220*	0.161	0.045	0.056	0.047
	(0.165)	(0.148)	(0.123)	(0.109)	(0.086)	(0.079)	(0.070)
Observations	66	66	125	125	307	307	386
R-squared	0.06	0.28	0.03	0.18	0.02	0.15	0.17

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. Pretreatment 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 percent, 5 percent and 10 percent levels, respectively.

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



Notes: 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

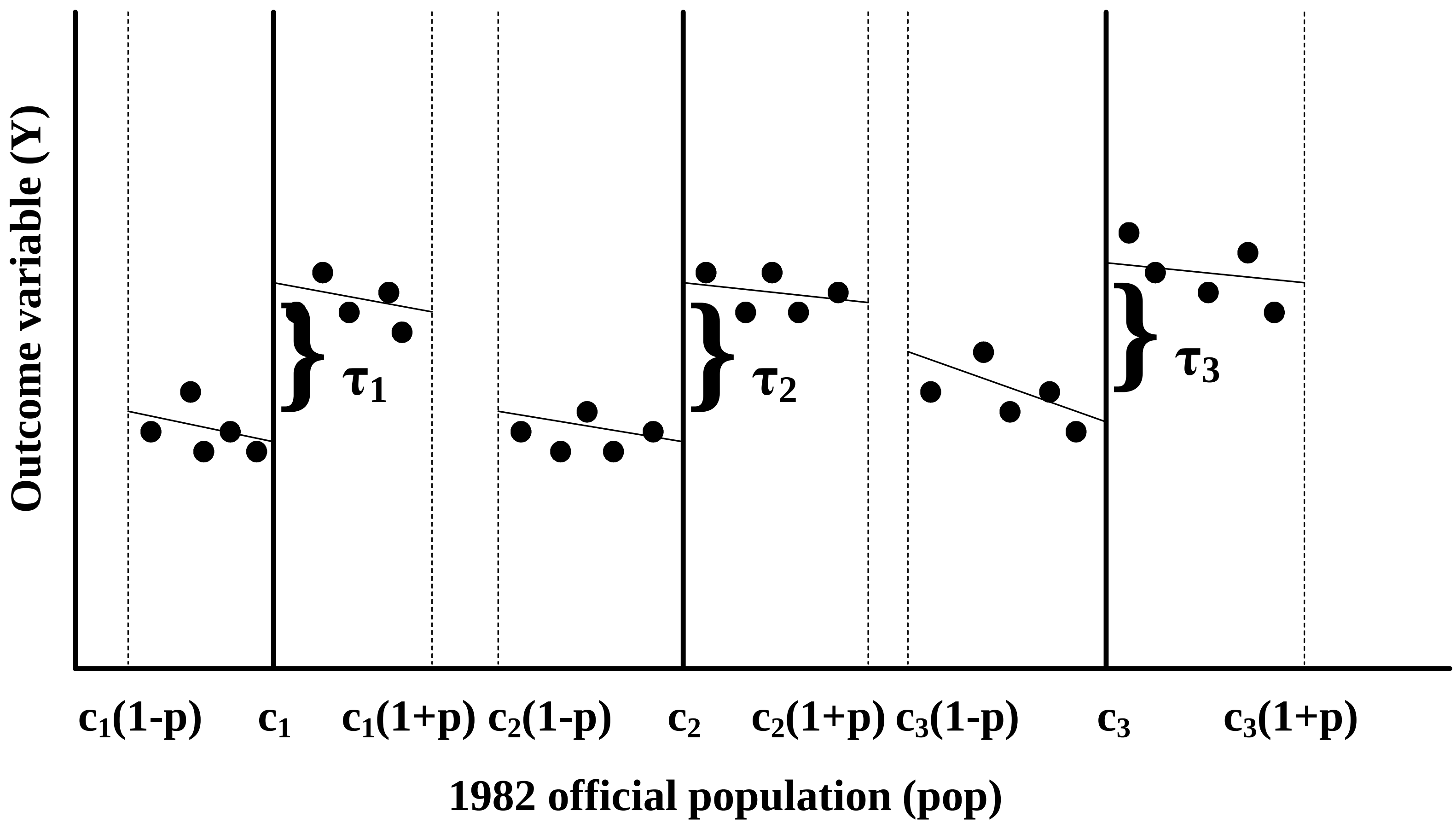
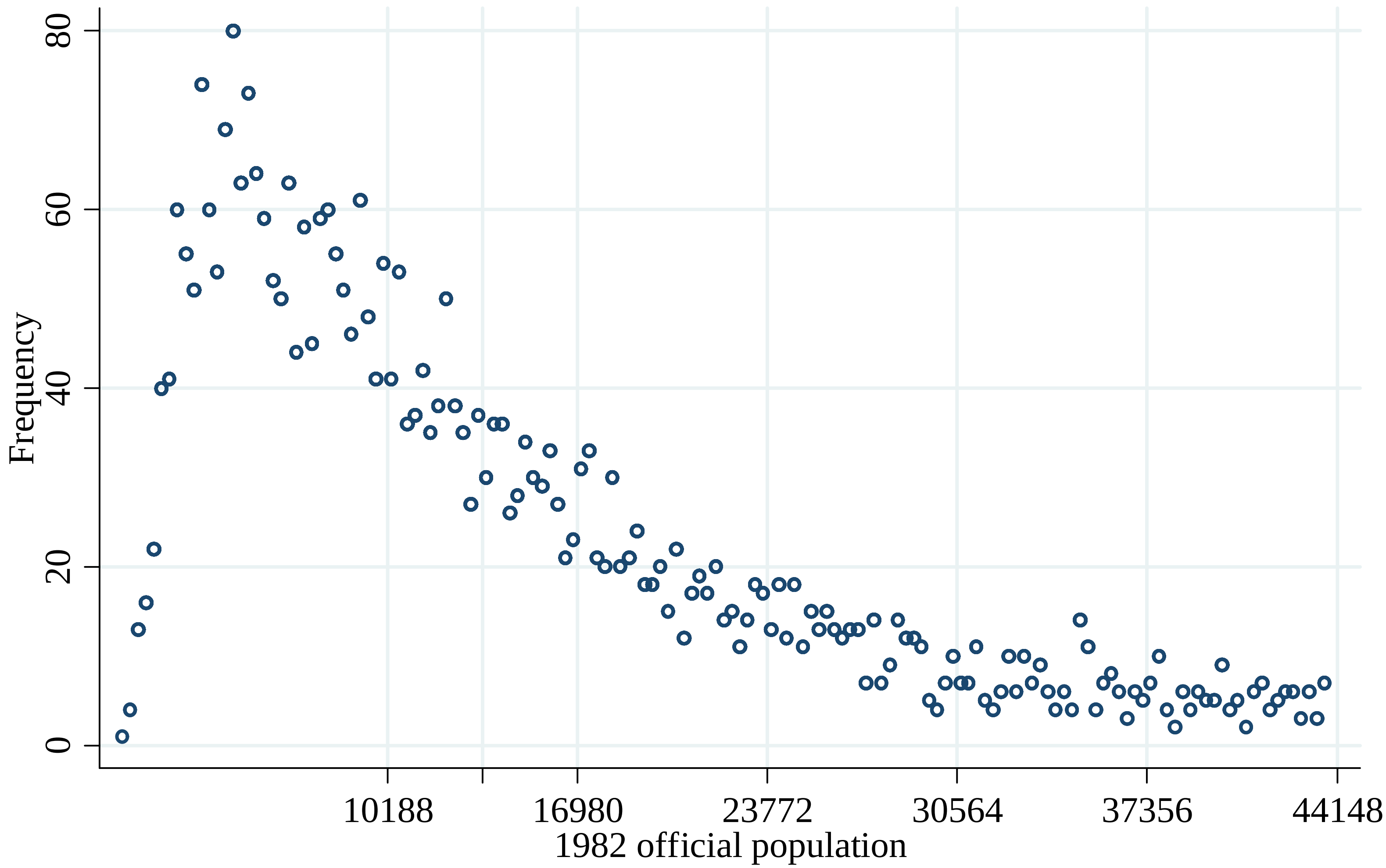
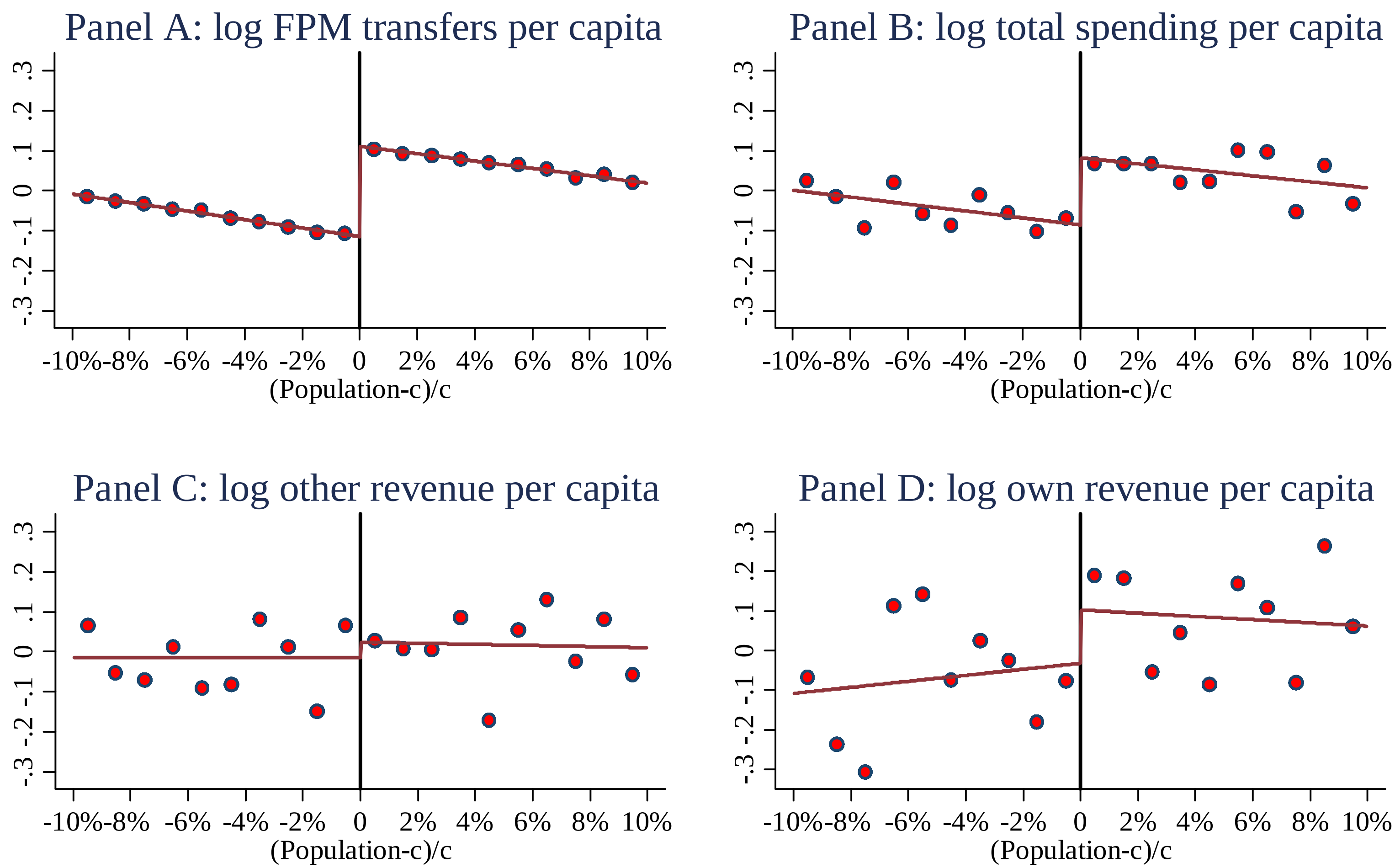


Figure 3: Histogram for 1982 official population, small to medium municipalities



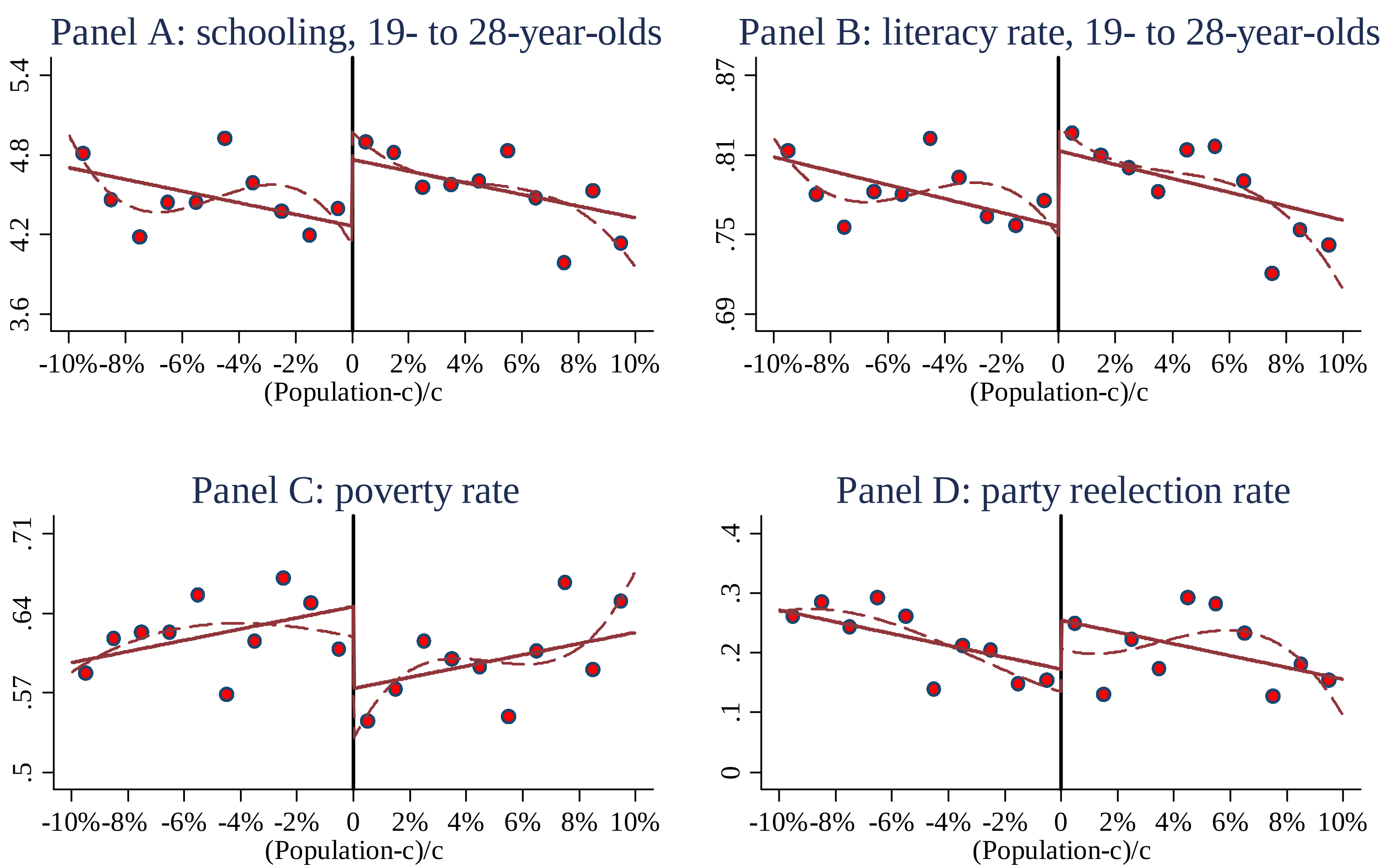
Notes: The bin-width is 283. 1982 official population is based on the 1980 census conducted by the national statistical agency, IBGE. McCrary density test fails to reject the null of no discontinuity in the density at conventional levels of significance for the first six cutoffs.

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



Notes: All variables are summed over the period 1982-1985 and scaled by 1982 official 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: Impacts on schooling, literacy, poverty, and party reelection



Notes: Each dot represents the sample average of the dependent variable in a given bin. Schooling, literacy, and poverty are measure in 1991 (census). Party re-election is measured in 1988. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.