Financing Local Development: Quasi-Experimental Evidence from Municipalities in Brazil, 190-1991

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Financing Local Development: Quasi-Experimental Evidence from Municipalities in Brazil, 1980-1991*

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Abstract

This paper uses a regression discontinuity design to estimate the impact of additional unrestricted grant financing on local public spending, public service provision, schooling, literacy, and income at the community (*município*) level in Brazil. Additional transfers increased local public spending per capita by about 20% with no evidence of crowding out own revenue or other revenue sources. The additional local spending increased schooling per capita by about 7% and literacy rates by about 4 percentage points. The implied marginal cost of schooling—accounting for corruption and other leakages—amounts to about US$ 126, which turns out to be similar to the average cost of schooling in Brazil in the early 1980s. In line with the effect on human capital, the poverty rate was reduced by about 4 percentage points, while income per capita gains were positive but not statistically significant. Results also suggest that additional public spending had stronger effects on schooling and literacy in less developed parts of Brazil, while poverty reduction was evenly spread across the country.

Keywords: intergovernmental grants, decentralization, economic development
JEL: D70, H40, H72, O15

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1 Introduction

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] 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 actual service improvements might fail to have the intended impact. Given these facts and concerns about local government spending, it is not clear *ex ante* to what extent providing more financing improves public service delivery at the margin. However, due to high data requirements, there is very little research that looks at the impact of additional fiscal transfers on public services and development outcomes, such as human capital accumulation and earnings. 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.

This paper provides the first quasi-experimental evidence regarding the impact of intergovernmental transfers on local public services and living standards in a developing country. I analyze the effect of additional unrestricted grant financing on local public spending, public service provision, schooling, literacy, and income at the community (*município*) level in Brazil over the period 1980-1991.[2] Municipalities in Brazil elect their own local executives and legislators who are in charge of local spending, mainly on education, housing and urban infrastructure, and transportation. Brazil does not have a good reputation in terms of public governance in general[3] and there is recent objective evidence of corruption in the local delivery of centrally funded services from

[1] Shah 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] 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.

audit reports (Ferraz and Finan 2008). Moreover because about 40% 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.

In order to address the likely endogeneity of central government funding, my 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 which are "as good as" randomly assigned (under relatively weak, and to some extent testable, assumptions further discussed below).

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[4] and literacy rates) and income (lower poverty rates), measured in 1991.[5] Some of the channels through which these effects on living standards arose were as follows: Additional transfers increased local public spending per capita by about 20%, with no evidence of crowding out own revenue or other revenue sources. 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. Direct evidence on public service improvements is mixed: 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.

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. In order to deal with this issue, I also investigate whether the extra spending affected household income and municipal education outcomes, as measured by community average schooling and literacy rates. Education outcomes and earnings can

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

[5]I 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 2008).

be thought of as indirect summary measures of public services: extra public spending on education should improve the quality of local schools, thus increasing the marginal benefit of education for any given level of schooling (Behrman and Birdsall 1983). At the same time, other public inputs, such as spending on road quality, should reduce the marginal cost of schooling, thus increasing households' equilibrium schooling choice (Birdsall 1985; Behrman, Birdsall, and Kaplan 1996).

The results for education outcomes suggest that the relevant school-age cohorts acquired about 0.3 additional years of schooling per capita (a 7% increase), and literacy rates increased by about four percentage points on average (compared to a 76% literacy rate in the comparison communities). In order to interpret these results, 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. My back-of-the-envelope calculations suggest that the implied marginal cost of schooling—accounting for corruption and other leakages—amounts to about US$ 126, which turns out to be similar to the average cost of schooling in Brazil in the early 1980s. While these are rough estimates, the similarity of the marginal cost to the average cost indicate that the findings here are certainly plausible. Moreover these estimates suggest that—accounting for corruption and other leakages—providing more financing to local governments at the margin improved education outcomes at reasonable cost.

In turn, better and more widespread education and better local public service quality overall (better infrastructure and primary health care for example) are likely to increase household incomes. The evidence suggests that the extra public spending indeed had an effect on income, although only for the poor. Specifically, I find that the poverty rate (measured relative to the national income poverty line) was reduced by about 4 percentage points from a comparison group mean poverty rate of 67%. 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. My back-of-the-envelope calculations suggest that about 1 to 2 percentage points of the poverty reduction are plausibly accounted for by the education channel alone, leaving the remaining 2 to 3 percentage points to better local public service quality overall.

Brazil is a very diverse country and so it is instructive to evaluate whether the impacts of local public spending on schooling and income vary depending on existing levels of development in 1980. Assuming a decreasing marginal productivity of local spending, one would expect stronger effects in the less developed northern parts of the country, all else equal.[6] All else might not be equal, however. In particular, governance might be generally worse in the North and thus extra resources received might not be spent as productively as in the South. Moreover, asymmetries in political awareness and participation of the poor might be higher in the less developed North, leading to a public service provision that is less responsive to the needs of the poor (Bardhan and Mookherjee 2005). Results suggest that the same additional public spending had stronger effects on schooling and literacy in the North of Brazil, while the effect on poverty reduction was evenly spread across the country. In addition, I also find stronger effects on schooling in more rural compared to more urban municipalities, which would be consistent with the larger role municipal governments play in the provision of elementary education in rural areas.

In order to assess the internal validity of these results, I run a series of tests and robustness checks. First, there is no evidence of manipulation of the 1980 census municipality population figures. Second, I verify whether municipalities in the marginal (to the cutoff) treatment and comparison groups were ex ante comparable[7] by testing for discontinuities in pre-treatment 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 municipalities were already doing somewhat better than those in the comparison group as of 1980. Third, I show that all results are robust to both the inclusion of pre-treatment covariates (including pre-treatment education and earnings outcomes) and to the choice of bandwidth and functional form. Fourth, I show that the education gains are robust to a difference-in-differences approach that directly controls for pre-treatment schooling differences of elementary-school-age cohorts. In contrast, the corresponding difference-in-differences estimates

[6]Local inputs might also be cheaper in less developed parts of the country.

[7]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.

for cohorts that have largely completed their education—and for whom one would expect smaller or no impacts—are close to zero in magnitude and very far from statistical significance.[8] Finally, I find almost identical results when I restrict the sample 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.

It is worth emphasizing that the estimates reported here represent effects of local public spending increases for the subpopulation of municipalities with populations at or near the cutoffs specified in the revenue-sharing mechanism.[9] Because I find similar effects at these cutoffs, however, results are likely to generalize to small local communities in Brazil. Whether providing additional financing to local governments in other contexts would yield similar results is an open empirical question. The most closely related study investigates the effects of oil windfalls on local spending and living standards also at the local level in Brazil, albeit in a later period and using a different design (Caselli and Michaels 2009). Their results suggest that additional local public spending financed through royalties had little if any effect on local public services or household income per capita, although in some specifications they also find a reduction in the poverty rate.

Existing studies on the effects of unconditional grants have tended to focus on spending decisions by the local community without evaluating effects on public services or human capital and earnings outcomes. Specifically, the result obtained here 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 and local spending.[10] 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 deci-

[8] Strictly speaking this is not a placebo experiment. Although one would expect smaller effects on education outcomes for cohorts that were beyond regular elementary schooling age, the effect need not be zero since older cohorts might have attended adult literacy programs that were promoted by the military government, such as the MOBRAL (Movimento Brasileiro de Alfabetização), and offered through the local administration.

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

[10] 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. Whether such low own-revenue collection represents an optimal choice or whether it reflects an inability to raise more revenue locally I cannot say. See Hines and Thaler (1996) for a review of the flypaper literature and problems with the empirical work.

sions reflected preferences of voters (Bradford and Oates 1971).

While the effects on education and income presented here are best interpreted as local public spending or public service quality effects, it is useful to contrast these findings with those of the aggregate (state, district, or community) literature on school quality or school resource effects. In fact, the distinction between this study and most existing aggregate studies on school quality, education, and earnings might not be very significant in practice, since these aggregate studies typically use measures of school resources that are likely correlated with other dimensions of the public service environment.[11]

The positive effects on educational attainment (completed years of schooling) reported here are qualitatively consistent with aggregate studies in the U.S. and in developing countries.[12] 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).[13] 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 on earnings (Card and Krueger 1996). For developing countries, the aggregate evidence on school quality 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 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 does not imply that none of the extra funds were privately appropriated by the incumbent, wasted or used for political patronage. Indeed, some of the reported extra spending likely never translated into service improvements "on the ground" for precisely these rea-

[11] Behrman and Birdsall (1983) and Birdsall (1985) use average schooling of teachers and average teacher income across geographical areas in Brazil. Card and Krueger (1992) use teacher-student ratios, average teacher pay, and length of the school year across states in the U.S.

[12] 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, Lavy (1996) for Ghana, 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 (2000) for evidence on India. Glewwe, Kremer and Moulin (2009) provide evidence for Africa.

[13] 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.

sons and there is recent direct evidence to back this up. Specifically, Brollo, Nannicini, Perotti and Tabellini (2010) adopt the identification strategy of this paper and use the audit reports in Ferraz and Finan (2008), to show that municipalities that got a windfall of the same unrestricted funds analyzed here also experienced a roughly proportional increase in public management irregularities. They also provide evidence that the quality of candidates running for local office deteriorated in these municipalities. Increasing the accountability of both local politicians and service providers is therefore likely to improve public service quality, as discussed in Bjoerkman and Svensson (2009) for example. The results presented here 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 funds they received to expand public services to the general local population at reasonable cost.

The remainder of the paper is organized as follows: Section 2 documents the role of local governments in public service provision in Brazil and gives institutional background on revenue sharing. Section 3 provides the conceptual framework and discusses the identifying assumptions for a causal interpretation of the estimates presented in this paper. Section 4 describes the data. Section 5 discusses the estimation approach and Section 6 evaluates the internal validity of the study. Section 7 presents the main results. Section 8 provides further robustness checks. Section 9 discusses heterogeneity of impacts depending on the initial level of development of the municipality. The paper concludes with a discussion of limitations and extensions.

2 Background

2.1 Local public services and their financing in Brazil

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.[14] Because municipalities have never collected much in the way of own revenues, intergovernmental transfers were essential to their functioning. In the early 1980s, total government revenue in Brazil was about 25% of GDP, of which municipalities collected about 4%. At the same time, local govern-

[14]Local governments also provided some primary health care services (about 10% of local budgets). Local welfare assistance was close to negligible at the beginning of the 1980s.

ments managed about 17% of public resources (Shah 1991). In other words, intergovernmental transfers to local governments represented about 3.25% of GDP. 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.[15] This grant accounted for about 50% of the revenue of the municipalities used in this study.

In the empirical analysis below, I estimate the effect of additional FPM financing over the four year period 1982-1985 on local public spending, public service provision, schooling, literacy, and income. The public service indicators I consider are dictated by data availability. They are measured in 1991, the earliest post-treatment year for which comprehensive data on municipalities are available. The indicators are supposed to capture improvements in the main spending areas of education as well as housing and urban infrastructure. Unfortunately I do not have any indicators on local transportation services or infrastructure.

In the area of education, I use the teacher-student ratio in municipal elementary schools and the number of schools run by the municipal government. It is easy to see how extra spending over the period 1982-1985 might affect the number of schools six years later in 1991. Effects on teacher-student ratios in 1991 might arise if the extra spending on education was in fact smoothed over subsequent years or if additional teachers could not easily be dismissed once the funding differential stopped. Public service measures in the area of housing and urban infrastructure are the percentages of individuals in the municipality with access to water, sewer, electricity and living in substandard housing.

I also use education outcomes for the relevant school-age population, measured in 1991, as indirect summary measures of public service improvements. Public provision of elementary education in Brazil was for the most part a joint responsibility of state and local governments, while the federal government was primarily involved in financing and standard setting. Of total public elementary education spending in the early 1980's, local governments accounted for about 26%, while state governments accounted for about 65%, with the remainder accounted for by the federal government. About 21% of local government budgets were devoted to education, with the bulk

[15] 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.

(72%) going to fundamental education (grades 1-8) and the remainder to intermediary education (grades 9-12) (World Bank 1985).

In 1980, 55% of all elementary school students in Brazil were enrolled in state administered schools, 31% in municipality schools and the remaining 14% in private schools. In small and rural municipalities, such as those considered here, the proportion of students in schools managed by local governments was 0.74 while the proportions for state-run and private schools were 0.24 and 0.02, respectively. Elementary school was compulsory for 7-to 14-year-olds, but less than 14% of an age cohort in 1980 completed the 8 grades of compulsory schooling in 8 years. The average number of completed grades after 8 years in school was about 5. Individuals were eligible to attend regular elementary school until the age of 18 and regular secondary school until the age of 21. Beyond these age limits individuals had to enroll in special education classes (World Bank 1985).

2.2 Mechanics of revenue sharing in Brazil

In order to estimate the effect of additional grants on local living standards, I exploit variation in FPM funding at several population cutoffs using regression-discontinuity (RD) analysis. The critical feature of the FPM revenue-sharing mechanism for the purposes of this 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 1. 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 based on 1980 census population from 1982 (the first year the 1980 census figures were used) until 1985.[16] 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.[17] Likewise, from 1986 to 1988, the transfers were also based on such extrapolations, this time based on 1970 and 1980 census population figures.[18] 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.[19] 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 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 that 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, i.e. as a step function of population as in Decree 1881/81, is less clear. One explanation could be that compared to a linear schedule, for example, the bracket design mutes incentives for local officials at the interior of the bracket to tinker with their population figures or to contest the accuracy of the estimates in order to get more transfers. A related question is where the exact cutoffs come from—that is, why 10'188, 13'584, 16'980, and so forth? While I was unable to trace the origin of these cutoffs precisely, I know roughly how they came about. The initial legislation from 1967 created cutoffs at multiples of 2'000 up to 10'000,

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

[17]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).

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

[19]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. Results are omitted to save space and are available upon request.

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.[20] 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 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 this analysis is that over the study period, the transfers were in fact allocated as stipulated in Decree 1881/81.[21] Figure 1 plots cumulative FPM transfers over the period 1982 to 1985 against 1982 official population. Each dot in this figure corresponds to a municipality. The horizontal lines correspond to the modal levels of cumulative transfers for each bracket in the 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.[22] 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.[23] The cumulative transfer differential over the period 1982-1985 corresponds to about 2.5% of annual GDP in rural areas of the country and about 1.4% of annual GDP in urban areas for the counties in the estimation sample (Table 2).

Although the funding jump is the same in absolute terms at each cutoff, the jump declines in per capita terms the higher the cutoff. As is apparent from Figure 1, funding jumps by about R$ 130 (US$ 95) per capita at the first threshold, R$ 97 (US$ 70) at the second, R$ 78 (US$ 57) at the third, and declines monotonically for the following cutoffs (R$ 55, R$ 43 and R$ 35, respectively). Immediately to the left of the first three cutoffs, per capita FPM funding is about R$ 390 (286 US$), and this amount declines monotonically for the following cutoffs. For the first three cutoffs the funding increase per capita is therefore from the same baseline level and represents about 33% at the first, 25% at the second, and 20% at the third cutoff. Though the differences are not great,

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

[21] See Litschig (2008) for evidence that over the 1990s the transfer mechanism was manipulated to benefit aligned (right-wing) national deputies in electorally fragmented local political systems as well as aligned local executives.

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

[23] 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.

this means that the treatment in terms of additional per capita funding is not exactly the same across these cutoffs. However, since there are likely to be economies of scale in the provision of local public services—that is, unit costs decline with scale—the differences in treatment across cutoffs might be even smaller than what the per capita funding jumps would suggest. It thus seems reasonable to expect similar treatment effects around these cutoffs, as further discussed in Section 5 below.

3 Conceptual framework and identification

3.1 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 local public service quality (transportation infrastructure 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. 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 7.

3.2 Identification

The basic intuition behind the RD approach is that, in the absence of program manipulation, municipalities to the left of the treatment-determining population cutoff should provide valid counterfactual outcomes for municipalities on the right side of the cutoff (which received additional resources). More formally, let Y denote an outcome variable (e.g. public service levels, average schooling, poverty rate), τ the (constant) treatment effect, D the indicator function for treatment (additional resources), pop municipality population, c a particular cutoff, $f(pop)$ a polynomial function of population and u unobserved factors that affect outcomes. The model is as follows:

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

If the potential regression functions $E[Y|D = 1, pop]$ and $E[Y|D = 0, pop]$ are both continuous in population, or equivalently, if $E[u|pop]$ is continuous, then the difference in conditional expectations identifies the treatment effect at the threshold:[24]

$$\lim_{pop \downarrow c} E[Y|pop] - \lim_{pop \uparrow c} E[Y|pop] = \tau \tag{1}$$

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

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

where Y_R is the effect of R on Y, π represents the jump in spending that occurs at the cutoff and υ represents other factors that affect spending. Under the continuity assumption above, the difference in conditional means of Y at the cutoff is now

$$\lim_{pop \downarrow c} E[Y|pop] - \lim_{pop \uparrow c} E[Y|pop] = \left\{ \lim_{pop \downarrow c} E[R|pop] - \lim_{pop \uparrow c} E[R|pop] \right\} Y_R \tag{2}$$

If government spending is the only channel through which additional transfers operate (the exclusion restriction), the ratio of jumps in Y and R identifies Y_R, the impact of local public spending on outcome Y. Reductions in local taxes and corresponding increases in private spending would violate this exclusion restriction, for example. However, as shown in Section 7 below, local taxes do not seem to have responded to additional transfers. There is also no evidence that state or federal levels of government altered other governmental transfers around the cutoffs.

The most important assumption for this study concerns the continuity of the potential regression functions, or equivalently, the continuity of $E[u|pop]$, which gives the estimands in equations (1) and (2) above a causal interpretation. Intuitively, the continuity assumption requires that unobservables, u, vary smoothly as a function of population and, in particular, do not jump at the cutoff. As shown in Lee (2008) and Lee and Lemieux (2009), sufficient for the continuity of the regression functions (or the continuity of $E[u|pop]$) is the assumption that individual densities

[24] With heterogeneous treatment effects, the RD gap identifies the average treatment effect at the cutoff. See Lee (2008) for an alternative interpretation of the treatment effect identified in this case as a weighted average of individual treatment effects, where the weights reflect the ex ante probability that an individual´s score is realized close to the cutoff.

of the treatment-determining variable are smooth. In the case considered here, 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. 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 over the study period all eligible municipalities received more FPM transfers, and none of the ineligible ones did).

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

Still, one might worry that leaders in the central government had incentives to alter the cutoffs to benefit local leaders they favored. It is unlikely, however, that this kind of manipulation would have occurred. For example, in order for leaders at the central government level to have used the cutoffs to benefit leaders 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, I show in Section 6 below that local governments that were run by the PDS[25], the party of the authoritarian regime that was in control of the central government until 1985, were not over-represented to the right of the cutoffs during the study period.

[25]PDS stands for Partido Democrático Social

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

4 Data

The analysis in this paper draws on multiple data sources from Brazil. Population estimates determining transfer amounts from 1982 until 1991 were transcribed from successive reports issued by the federal court of accounts (TCU). Data on local public budgets, including FPM transfers and spending categories, were self-reported by municipality 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.

Data on 1980 municipality characteristics are based on the 25% sample of the census and have been calculated by the national statistical agency (only a shorter census survey was administered to 100% of the population). As pre-treatment covariates, I include 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. The 1991 poverty rate was calculated by the government research institute IPEA[26] 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.

Data on municipal elementary schools and primary school teacher-student ratios are from the 1991 school infrastructure survey. Primary school teachers are those working in grades 1-4 as opposed to grades 5-8. I use microdata from the 10% and 20% samples of the 1991 census and

[26] Instituto de Pesquisa Econômica Aplicada.

from the 25% sample of the 1980 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 old 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 during 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.

While I include cohorts up to and including age 18 in 1982, older cohorts might have been affected by the additional spending as well, although likely to a lesser extent. For example, older cohorts might have gone to local secondary schools (although there are relatively few of them) or to state secondary schools paid for by the local government (World Bank 1985). Even those over the age of 21 (cutoff age for secondary schooling) in 1982 might have enrolled in adult literacy programs that were promoted by the military government and offered through the local administration, such as the MOBRAL (Movimento Brasileiro de Alfabetização). Nevertheless, one would expect smaller effects on education outcomes for cohorts that were beyond regular schooling age. I provide evidence that this was in fact the case in Section 8 below.

I 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 pre-school 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 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 quality 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.

Table 2 shows descriptive statistics for the variables used in the statistical analysis, as well as other information regarding revenue and expenditures in the municipalities. The numbers show that FPM transfers are the most important source of revenue for the relatively small local governments considered here, amounting to about 50% on average and 56% in rural areas. Table 2 also shows that education spending accounts for about 20% of local budgets on average, with similar shares going to housing and urban infrastructure, and transportation spending. In addition, Table 2 documents a marked difference in development indicators between the relatively developed southern part of the country (South, Southeast and Center-West regions) and the less developed northern regions (North and Northeast region, see Table 2 for definitions). The contrast between rural and urban communities is similarly striking.

5 Estimation approach

Following Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2008), the 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. I 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, I 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 findings are not driven by functional form assumptions, I present most estimation results from linear specifications in the discontinuity samples, adding quadratic specifications as a robustness check. I supplement the local linear estimates with higher order polynomial specifications, using an extended support, and I choose the order of the polynomial such that it best matches the local linear estimates in the discontinuity samples. This 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, I focus particularly on the first three population cutoffs (c_1 =10'188, c_2 =13'584, and c_3 =16'980). At subsequent cutoffs the variation in FPM transfers is too small to affect municipal overall budgets, and hence there is no "first stage" in terms of overall resources available for the municipality, as shown in Section 7 below. While I present results for the first three cutoffs individually, I 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.[27] 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. With similar treatment intensity it seems reasonable to expect similar treatment effects at least around the first few cutoffs, a testable hypothesis for which I find support below.

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

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

is then:

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

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

Figure 2 illustrates the estimation approach. I 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, I need to make observations comparable in terms of the distance from their respective cutoff. To do this, I 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:

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

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

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

6 Internal validity checks

Since extensive manipulation of the population figures on which FPM allocations were based would cast serious doubts on the internal validity of the design, I check for any evidence of sorting, notably discontinuous population distributions. Figure 4 plots the histogram for 1982 official municipality population.[28] 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 anywhere near conventional significance levels for any of the first six cutoffs according to the density test suggested by McCrary (2008).[29]

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

In the larger samples that include municipalities within +/- 4 to 6 percentage points, some individual discontinuities in Table 3 are statistically significant. This happens mostly due to larger point estimates compared to the smaller bandwidths, rather than lower standard errors, which suggests that these significant results might reflect a specification error.[30] Table 3.1 in the Online

[28] The histogram for the full support is omitted to save space and available upon request.

[29] The estimates (and standard errors) are, for the first to sixth cutoffs respectively, -0.085 (0.098), -0.002 (0.112), 0.152 (0.135), 0.071 (0.167), -0.041 (0.253), 0.324 (0.344). Separate density plots for each cutoff are presented in Figure 4.1 in the Online Appendix.

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

Appendix shows results from quadratic specifications that confirm this view: virtually none of the pre-treatment differences found in the 4 and 5 percent samples in Table 3 are now statistically significant, both due to lower estimates and higher standard errors. Moreover, all F-tests in Tables 3 and 3.1 fail to reject the joint null hypotheses of no discontinuities in any pre-treatment covariate at conventional levels of significance (lowest p-value is 0.26).[31] In other words, from a statistical point of view, there is no evidence that treatment group municipalities were systematically different in terms of local development or overall public resources from municipalities in the marginal comparison group in the pre-treatment period.[32]

Nonetheless, the point estimates for education outcomes and public revenues are all positive. Moreover, some of these estimates are of the same order of magnitude as those found in the post-treatment period as further discussed below, suggesting that treatment group municipalities might already have been somewhat better off than those in the comparison group as of 1980. In Section 7 below I show that the estimated effects are robust to the inclusion of relevant pre-treatment covariates, including the four pre-treatment education and earnings outcomes shown in Table 3. In Section 8, results are shown to be robust to a difference-in-differences approach that directly controls for pre-treatment schooling differences of elementary-school-age cohorts.

7 Main estimation results

This section starts out by demonstrating that FPM transfers increased local total revenue and spending per capita by about 20%, with no evidence of crowding out own revenue or other revenue sources. 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. The second subsection presents direct evidence on public service improvements in these broad spending areas. The third subsection discusses 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 fourth subsection presents and discusses effects on income (lower poverty rates). The final subsection shows that the estimated impacts are

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

[32] Results for the first two cutoffs pooled are quantitatively similar and available upon request.

not only individually but jointly significant.

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

7.1 Effects on overall spending and spending shares

Table 4 gives estimates of the jump in total local public revenue per capita over the 1982-1985 period. The pooled estimates in the first two rows suggest that per capita revenues increased by about 20 percent at the thresholds. The magnitude of the jump is roughly consistent with the size of FPM transfers in local budgets (about 50%) and the jump in per capita FPM transfers at the cutoffs (about 33% for the 10'188 cutoff and less for subsequent cutoffs).[33] Figure 4 graphically represents the results for FPM transfers, total revenue, own revenue and other revenues, which are composed of other federal and state government transfers, all cumulative over the period 1982-1985. Each dot represents the residual from a regression of the dependent variable on state and segment dummies averaged for a particular bin. The state and segment effects are included to absorb some of the variation in the dependent variable and make the jump at the cutoff more easily visible. For example, the first dot to the left of zero in panel B of Figure 4 represents average residual total government revenue per capita for all municipalities within one percentage point (in terms of population) to the left of one of the first three population thresholds.[34]

[33] To see this, let R denote total revenue, F FPM funding and O other funding, such that $R = F + O$ and $\frac{\Delta R}{R} = \frac{\Delta F}{F}\frac{F}{R} + \frac{\Delta O}{O}\frac{O}{R}$. If $\Delta O = 0$, as shown below, and $\frac{F}{R} = 0.5$ on average, as shown in Table 2, then $\frac{\Delta R}{R} = 33\% \times 50\% = 16.5\%$. The estimates in Table 4 are somewhat larger, perhaps because municipalities with missing FPM information rely more heavily on FPM funding, in which case $\frac{F}{R}$ might be more like 0.6, or simply by chance. Note that proportional changes at the cutoff are identical whether or not the variable is scaled by population, P: $\Delta \ln\left(\frac{R}{P}\right) = \Delta \ln(R) \cong \frac{\Delta R}{R}$.

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

To demonstrate the correspondence between panel B of Figure 4 and the results in Table 4, if instead of fitting two straight regression lines through the ten dots on either side of the cutoff, this figure were to fit two lines through the first *two* dots on either side of the cutoff, the result would roughly illustrate the jump estimated in column 1 of Table 4 for pooled cutoffs 1-3 in the two percent neighborhood without covariates. With this in mind, the figure shows clear evidence of a discontinuity in total per capita revenue at the pooled cutoff, and it additionally shows that the discontinuity is visually robust irrespective of the width of the neighborhood examined. It is also worth noting that both the regression functions for total revenue per capita and FPM per capita (panel A) slope downward to the left and to the right of the cutoff, as expected given the FPM allocation mechanism.

At the same time, panels C and D of Figure 4 show that there are no discontinuities in either own revenue or other revenues. This suggests that the effects on education and poverty discussed below can be attributed to local spending on public services, rather than additional private spending associated with local tax breaks (that is, the exclusion restriction discussed in Section 3 seems to hold). Statistical analysis confirms this conclusion but is not presented here to save space (results are available on request). Table 5 shows that total spending increased by an almost identical percentage as total revenue. Because small local governments were running close to balanced budgets at the time, this implies that total spending increased essentially one-for-one with FPM transfers.[35] This result is also borne out when I 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 none of the estimates are statistically different from unity as shown in Table 5.1 in the Online Appendix.

Tables 4 and 5 also show 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 overall resources.[36] 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

[35] To see this, let R denote total revenue as before, Exp, total expenditures and B the budget balance, such that $R = Exp + B$ and $\frac{\Delta R}{R} = \frac{\Delta Exp}{Exp}\frac{Exp}{R} + \frac{\Delta B}{B}\frac{B}{R}$. If the budget is balanced, $B = 0$, then $\frac{\Delta R}{R} = \frac{\Delta Exp}{Exp}$ implies that every Real of extra revenue was spent. But if $B > 0$ for example, we could find $\frac{\Delta R}{R} = \frac{\Delta Exp}{Exp}$ and yet part of the extra revenue would have been saved or used to pay back debt.

[36] At the 5th cutoff the discontinuity estimates are much more variable and they are nowhere near statistical significance. Results are available on request.

a sensible argument, estimates around higher cutoffs are not pursued here for the sake of brevity and ease of interpretation of the estimated impacts (see Section 7.3 below). Another point worth noting is that the included pre-treatment covariates are significant predictors of municipality per capita revenue and 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.

Figure 5 documents effects on total spending per capita as well as on the main local expenditure categories: education, housing and urban infrastructure, and transportation. As with total revenue, there is clear evidence of a jump of about 20% at the cutoff in all of these variables, although the jumps in expenditure categories are now more sensitive to the width of the neighborhood examined. The regression lines also slope downward almost without exception, which is further evidence favoring the validity of the design. The spending category graphs are considerably noisier than the total spending graph because the sample size is smaller (due to missing values) and because the expenditure categories are only available for the years 1982 and 1983, whereas total spending is reported over the entire period 1982 to 1985. Nevertheless, the jumps in the expenditure categories are also statistically significant as shown in Table 6. This evidence thus suggests that local spending on education, housing and urban infrastructure, and transportation all increased by about 20% per capita, leaving local spending shares essentially unchanged.[37]

7.2 Effects on public services

Having established that additional FPM transfers were used to finance an expansion of public spending per capita of about 20% over the period 1982-1985, the remainder of this section proceeds to document impacts of this extra spending on public services.

Table 7 shows effects on the primary school teacher-student ratio. Although the extra FPM funding stopped by the end of 1985, effects on teacher-student ratios in 1991 might arise if the extra spending on education was in fact smoothed over subsequent years or if additional teachers could not easily be dismissed. Estimates are reasonably close across samples and suggest that the teacher-student ratio increased by about .01, or one teacher per hundred students. This compares

[37]To be precise, the null hypothesis of a proportional, 20 percent per capita increase cannot be rejected in any of the specifications.

to an average teacher-student ratio in the marginal comparison group of about .05. The implied average class-size reduction at the primary school level amounts to about 3 students per teacher. In contrast, results on municipal elementary schools (not shown) display no clear patterns and are imprecisely estimated, suggesting that transfers financed mostly more labor input as opposed to school infrastructure.

Housing infrastructure measures do not indicate much evidence of public service improvements although they are for the most part positive and also statistically significant in some specifications. Rather than showing separate tables with mostly insignificant results, I present the school and housing infrastructure estimates below when I test the joint significance of all the outcome variables. Figure 6 shows the results for the teacher-student ratio, elementary schools, and water and electricity access graphically (the graphs for sewer and inadequate housing look very similar to the electricity graph). Direct evidence on public service improvements is thus mixed at best: while there is evidence that student-teacher ratios in local primary school systems fell, there is little evidence that housing and urban development spending affected housing conditions.

7.3 Effects on education outcomes

This section presents estimates of education and income gains for the communities that received extra financing from the central government. Tables 8 and 9 present results for average years of schooling (completed grades) of individuals 19 to 28 years and 9 to 18 years of age in 1991, respectively. The pooled point estimates in rows 1 and 2 of Table 9 suggest that the older cohort accumulated about 0.3 additional years of schooling per capita (specifications with covariates). This schooling gain would be consistent with 3 out of 10 individuals from this cohort completing an additional year of schooling for example. The estimates at individual cutoffs are all positive but more variable, which likely reflects small sample biases. While most of the estimates from individual cutoffs are not significantly different from zero, the pooling across cutoffs c_1 and c_2, as well as c_1, c_2 and c_3, yields statistically significant estimates (at 1%) even within a relatively small neighborhood of +/- 3% around the cutoffs.

Corresponding results for the younger cohort shown in Table 9 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. Given that average years of schooling in marginal comparison group counties for the 19-28 aged cohort in 1991 was about 4.3 years with a standard deviation of 1.45 years, the schooling gains amount to about 7% or about 0.2 standard deviations. For the younger cohort, the marginal comparison group years of schooling were 2.7 years with a standard deviation of 1.08 years. The 0.15 schooling gain thus amounts to about 6% or 0.14 standard deviations.

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 compulsory schooling goes from 7 to 14 years. On average in Brazil at the time, a year in school led to about 0.625 completed grades—5 years of schooling 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 these results, 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$.[38] Assuming that about 20% of the additional FPM funds were spent on education (Table 6), and assuming further that only the 0- to 18-year-olds in 1982, about 50% of the total population,[39] 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 Tables 8 and 9, this marginal spending purchased about 0.3 years and 0.15 years of schooling (specifications with covariates), respectively. Taking an unweighted average of 0.225, the implied marginal cost of

[38]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.

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

an additional completed year of schooling is about $\$28.4 \times \frac{1}{0.225} = \$126$. This compares to average annual education expenditures per capita at the cutoffs in 1982 of about 44 R$ in 2008 prices, or 31 international US$. Assuming again that these funds were spent on the 0- to 18-year-olds, and that a year in school leads to about 0.625 completed grades on average (World Bank, 1985), the average cost of a completed additional year of schooling is about $\$31 \times 2 \times \frac{1}{0.625} = \$99$. While these are rough estimates, the similarity of the marginal cost to the average cost indicate that the findings here are certainly plausible. Moreover these estimates suggest that—accounting for corruption and other leakages—providing more financing to local governments at the margin improved education outcomes at reasonable cost.

Tables 10 and 11 show 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% in the comparison group. For the younger cohort the literacy differential is about 3 percentage points compared to an average literacy rate of about 74% in the comparison group. Panels A and B of Figure 7 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). In line with this graphical evidence, estimates of the discontinuity for neighborhoods not shown in Tables 8 through 12 are quantitatively similar to the estimates presented here and are available upon request.

7.4 Effects on poverty and income per capita

Both better and more widespread education and better local public service quality overall (better infrastructure and primary health care for example) are likely to increase household incomes. The evidence suggests that the extra public spending indeed had an effect on income, although only for the poor. Specifically, Table 12 shows impacts on the municipality poverty rate (measured relative to the national income poverty line). All pooled estimates shown in rows 1 and 2 are negative, with values around -4 to -5 percentage points, down from a mean poverty rate of 64% in the comparison group. The estimates at individual cutoffs are also all negative although they are more variable. While most of the estimates from individual cutoffs are not significantly different from zero, the pooling across cutoffs yields statistically significant estimates (at 1%) even in the discontinuity samples. While income per capita in 1991 is higher in the communities that got more funding, the difference is not statistically significant (results not shown). Panels C and D of Figure 7 show effects on the poverty rate and on household income per capita graphically.

In order to interpret the results on poverty reduction, it is again useful to do some back-of-the-envelope calculations. As emphasized in Section 3, impacts on poverty are likely to arise through better and more widespread education, as well as through better local public service quality overall (better infrastructure and primary health care for example). Regarding the education channel, Tables 8 and 9 show that the schooling gains for the 10- to 19-year-olds and the 0- to 9-year-olds in 1982 were 0.3 years and 0.15 years, respectively. A likely distribution of individual-level gains that would lead to this average impact is that 30 out of 100 individuals in the older cohort and 15 out of 100 in the younger cohort completed another year of schooling. Given the shares of these cohorts in the total population—27% and 23%, respectively, according to De Carvalho (1997)—we can thus estimate what percent of the overall population got an additional year of schooling, namely about $27\% \times 15\% + 23\% \times 30\% = 11\%$.

Now suppose that an extra year of schooling raises wages by 12% (Behrman and Birdsall 1983), that labor supply is constant, and that about 10% of the population earn per capita income that falls within a 12% range below the poverty line. Suppose further that about 64% of the total population would have been poor in the absence of the extra funding (this corresponds to the comparison group average poverty rate shown in Table 12) 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 $10\% \times 17\% = 1.72\%$ of the total population escaped poverty through the schooling channel alone. This number will be higher the higher 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 poverty reduction, leaving the remaining 2 to 3 percentage points to better local public service quality overall.

7.5 Testing joint significance

The analysis so far has examined the effects of additional financing on several intermediary and final outcomes, some of which were statistically significant, while others were not. Since examining a sufficient number of variables would always yield some that are statistically different from zero simply by chance, it is important to test the joint hypotheses of zero effects in all variables. Table 13 presents local linear estimates from the pooled specification across the first 3 cutoffs and the results of F-statistics, testing the null hypothesis of no discontinuities in any outcome variable. The tests clearly reject these joint hypotheses, suggesting that at least some of the effects are real.

Overall there is thus strong evidence that the additional public spending improved education outcomes at reasonable cost and reduced the number of poor people relative to the national income poverty line. It is worth remembering at this point that—under the continuity assumption and exclusion restriction discussed in Section 5 above—these effects can be attributed to extra local public spending (although not exclusively to education spending), rather than private spending, since there is no evidence of local tax breaks, and direct welfare spending by local governments was very limited.

8 Further robustness checks

This section provides further robustness checks regarding functional form of both the running variable (population) and of pre-treatment covariates, as well as difference-in-differences estimates that directly control for pre-treatment schooling differences of elementary-school-age cohorts. The corresponding difference-in-differences estimates are also presented for cohorts that have largely completed their education and for whom one would expect smaller or no impacts. A final robustness check uses only the sub-sample of individuals who were born in the municipality and never moved away. The section starts out with robustness checks for schooling (8.1), followed by literacy (8.2) and poverty (8.3). All the previously discussed results turn out to be robust to these additional tests. The corresponding tables are available in an online appendix.

8.1 Schooling

Table 8.1 presents pooled estimates across cutoffs c_1 and c_2, as well as c_1, c_2 and c_3 for the older cohorts of 19- to 28-year-olds in 1991 and for the three previously used bandwidths ($p = 2\%$, 3% and 4%). For each bandwidth Table 8.1 has 3 columns, corresponding to the following specifications: first, linear population polynomial with pre-treatment covariates as in Table 8 but now including average years of schooling of the 8- to 17-year-olds in 1980 (19- to 28-year-olds in 1991) based on the 1980 census micro-data as an additional control; second, quadratic population polynomial without covariates; and third, linear population polynomial with a quadratic specification of the pre-treatment covariates. The corresponding results for the younger cohort of 9- to 18-year-olds in 1991 are presented in Table 9.1.

All estimates in Table 8.1 are positive and most of them fall in the 0.2 to 0.3 range, the same result encountered in Table 8 for the specifications with covariates. And as before, these estimates become statistically significant (at 5%) even within a relatively small neighborhood of +/- 3% around the cutoffs. Table 8.1 also gives results of three hypothesis tests, one for each of the three specifications discussed above. The first is a t-test of the hypothesis that the coefficient on the pre-treatment outcome is equal to one, as imposed in the first-difference specification further discussed below. This null hypothesis is soundly rejected across bandwidths and cutoffs (p-values of 0.01 or lower). It turns out, however, that whether the coefficient on initial schooling is imposed or not matters little for the results. The second test investigates the joint hypotheses that the coefficients on the quadratic population terms on either side of the cutoff are zero, that is, whether linearity of the population polynomial can be rejected. As expected, there is no statistical evidence against linearity close to the cutoff ($p = 2\%$ and 3%) although for the $p = 4\%$ bandwidth linearity is clearly rejected. The third is an F-test for the joint hypotheses that the coefficients on the quadratics in covariates are all zero. It turns out that the statistical evidence against including covariates linearly is weak across bandwidths and cutoffs.

Estimates of the schooling gains for the 9- to 18-year-old cohort in 1991 based on the same specifications as above are presented in Table 9.1. The only difference is that pre-treatment average schooling for this cohort (0- to 7-year-olds in 1980) is not included since the census only collects schooling information for those aged 5 or above. As in Table 9, the discontinuity estimates fluctuate around 0.15 years per capita, again statistically significant even in the narrow samples around the cutoffs. Again there is no statistical evidence against linearity of the population polynomial close to the cutoff ($p = 2\%$ and 3%) and only weak statistical evidence against including covariates linearly.

Table 8.2 presents estimates of a difference-in-differences approach where the dependent variable is the difference in average years of schooling between 1991 and 1980 of the older cohorts (19- to 28-year-olds in 1991). This approach effectively controls for municipality-level unobserved time-invariant factors that might be correlated with extra funding but imposes a coefficient of one on initial schooling of this cohort, rather than allowing the coefficient to be estimated as in Table 8.1 above. For each bandwidth Table 8.2 has 3 columns, corresponding to the following specifications: first, linear population polynomial without covariates; second, quadratic population polynomial without covariates; and third, linear population polynomial with covariates. Again, all estimates in Table 8.2 are positive, most of them fall in the 0.2 to 0.3 range and they become statistically significant even within a relatively small neighborhood of +/- 3% around the cutoffs.

In contrast, the corresponding difference-in-differences estimates for those 25 years and older in 1980—typically considered to have completed most of their schooling—are close to zero in magnitude (sometimes negative) and very far from statistical significance as shown in Table 8.3. These estimates are for the exact same cohorts for which Table 3 shows a positive schooling differential before the extra funding had started. While it is reassuring that these older cohorts did not experience any schooling gains, strictly speaking this is not a placebo experiment. Although one would expect smaller effects on education outcomes for cohorts that were beyond regular elementary schooling age, the effect need not be zero since older cohorts might have attended adult literacy programs that were promoted by the military government and offered through the local administration, such as the MOBRAL (Movimento Brasileiro de Alfabetização). In fact the difference in average years of schooling of these cohorts in the comparison group is about 0.32, on average (Table 8.3). This would be consistent with roughly one out of three individuals among those 25 years and older getting an extra year of schooling over the eleven-year-period from 1980 to 1991.

As a final robustness check, I also estimate the impact on schooling for the 19- to 28-year-olds in 1991 on a restricted sample of individuals who were born in a given municipality and never moved away. The results are shown in Table 8.4 and are again quantitatively close to those from the unrestricted sample. This provides suggestive evidence that the schooling gains stem at least partly from existing residents, rather than being driven by in-migration of more highly educated individuals in response to public service improvements. The results are only suggestive, however, because there could be selective attrition among non-migrants across treatment and comparison communities. In particular, more educated individuals might be more likely to stay in the municipality in response to public service improvements.

8.2 Literacy

Tables 10.1 and 11.1 present robustness checks for literacy outcomes of the 19- to 28-year-olds and 9- to 18-year-olds in 1991, respectively, using the same specifications as in Tables 8.1 and 9.1 above. As in Table 10, the estimates in Table 10.1 suggest a literacy gain of about four percentage points throughout, significant even in the +/- 2% window around the cutoffs. As with schooling above, the hypothesis that the coefficient on the pre-treatment outcome is equal to one is soundly rejected across bandwidths and cutoffs (p-values of 0.00). And again as expected, there is no statistical evidence against linearity of the population polynomial close to the cutoff ($p = 2\%$ and 3%) although for the p=4% bandwidth linearity is again rejected. There is also strong statistical evidence across bandwidths and cutoffs against including covariates linearly. This turns out not to matter much since estimates with linear vs. quadratic covariates are very similar if not identical.

Estimates of the literacy gains for the 9- to 18-year-old cohort in 1991 are presented in Table 11.1. As in Table 11, estimated impacts are all around three percentage points, again statistically significant even in the discontinuity samples. As in Table 10.1 there is no statistical evidence against linearity of the population polynomial close to the cutoff and strong evidence against including covariates linearly across bandwidths and cutoffs.

Table 10.2 presents estimates where the dependent variable is the difference in literacy rates between 1991 and 1980 of the older cohorts (19- to 28-year-olds in 1991). Compared to the estimates of about four percentage points in Tables 10 and 10.1, those in Table 10.2 suggest a slightly lower literacy gain of about three percentage points, again statistically significant even within a relatively small neighborhood of +/- 3% around the cutoffs.

Table 10.3 presents the final robustness check for the literacy gains of the 19- to 28-year-olds in 1991 based on the sub-sample of individuals who were born in a given municipality and never moved away. The results are again quantitatively close to those from the unrestricted sample.

8.3 Poverty

Table 12.1 presents robustness checks for the poverty rate using the same specifications as in Table 8.1 above. As in Table 12, the estimates in Table 12.1 suggest a poverty reduction of about four to five percentage points, significant even in the +/- 2% window around the cutoffs. As with schooling and literacy above, the hypothesis that the coefficient on pre-treatment poverty is equal to one is soundly rejected across bandwidths and cutoffs (p-values of 0.00). There is no statistical evidence against linearity of the population polynomial for any bandwidth. In contrast, there is strong statistical evidence across bandwidths and cutoffs against including covariates linearly. Again, this turns out not to matter much since estimates with linear vs. quadratic covariates are always very similar if not identical.

9 Heterogeneous effects

In this section I show that additional resources had stronger effects on schooling and literacy in the North of Brazil, which is generally less developed than the South (see Table 2 for the definitions of North and South). In contrast, poverty reduction was evenly spread across the country. I also find stronger effects on schooling in rural compared to urban municipalities, which would be consistent with the larger role municipal governments play in the provision of elementary education in rural areas.

Table 14 shows the effects of additional FPM transfers on total public spending per capita and on the primary school teacher-student ratio in northern and southern states of Brazil. Spending increased by about 20% in both parts of the country and effects on primary school teacher-student ratios tend to be positive and statistically significant, especially in the South. Although all estimates tend to be larger in the South they are not statistically different from each other. None of the other public service indicators are statistically significant in either region (results not shown).

Table 15 shows that the average schooling and literacy gains reported earlier are for the most part accounted for by gains in the northern part of the country. The estimates with covariates put the schooling gains in the North at about 0.3 to 0.4 years. Literacy gains are also larger in the North than in the South. These regional differences in literacy and schooling gains are statistically significant.[40] The poverty reduction, in contrast, is larger in the South, although this difference is not statistically significant.

Tables 16 and 17 examine whether the notion that extra funds have stronger effects in less developed areas holds true not just between the northern and southern parts of Brazil but also across rural and urban areas as distinguished by the median percentage of urban residents in 1980. Table 16 shows that spending increased by about 20% in both urban and rural municipalities. The effect on primary school teacher-student ratios tends to be positive and statistically significant in rural areas, with no real difference in urban areas although the differential effect is not statistically significant.[41] Again, none of the other public service indicators are statistically significant in either region (results not shown).

The results in Table 17 suggest that almost the entire schooling gains come from rural municipalities (an additional 0.5 year of schooling per capita). Effects in urban communities are smaller, statistically insignificant and statistically different from the effects in rural communities.[42] The literacy gains are more evenly spread although they too are concentrated among rural municipalities and somewhat smaller in urban municipalities although the difference is not statistically significant.[43] The poverty reduction is evenly spread across urban and rural communities. Overall, these results suggest that additional public spending had stronger effects on schooling and literacy in less developed parts of Brazil, while poverty reduction was evenly spread across the country.[44]

An alternative explanation for these effects is that poor communities had stronger preferences for education than richer communities and hence spent a higher proportion of extra funds on education. A direct test of this alternative view is to examine the share of education expenditure in

[40] The coefficients and standard errors on the interaction of the treatment indicator with the region indicator (1 for North) in the pooled sample for schooling and literacy are, respectively: 0.208 (0.105) and 0.019 (0.011).

[41] The coefficient and standard error on the interaction term of the treatment indicator with the urban indicator (1 for urban) are -.003 and (.004).

[42] The coefficient and standard error on the interaction term are -.185 and (.106).

[43] The coefficient and standard error on the interaction term are -.005 and (.011).

[44] I also break the sample into high vs. low education and low vs. high initial poverty counties and find quantitatively similar results.

total spending subsequent to the increase in funding in poor vs. rich areas. Unfortunately, however, existing expenditure data do not allow such a disaggregation between 1984 and 1989. When I test for differential effects on education expenditure shares using data from 1982 and 1983, I find no significant effects (results not shown), suggesting that stronger preferences for education in poor communities are not the driving force behind the higher schooling and literacy gains found in less developed parts of Brazil.

10 Conclusion

Results presented in this paper suggest that communities that received extra financing from the central government benefited in terms of education outcomes, used as an indirect summary measure of public service improvements. The implied marginal cost of schooling—accounting for corruption and other leakages—amounts to about US$ 126, which turns out to be similar to the average cost of schooling in Brazil in the early 1980s. In turn, more education and better local public service quality overall increased household incomes, although only for the poor. These effects can be attributed to local public spending (although not exclusively to education spending), rather than private spending, since there is no evidence of local tax breaks, and direct welfare spending by local governments was very limited. It also seems that additional public spending had stronger effects on schooling and literacy in less developed parts of Brazil, while poverty reduction was evenly spread across the country.

As with any regression discontinuity analysis, the treatment effects presented in this paper apply only to municipalities with population levels at the respective cutoffs. However, because results are quantitatively similar across the first three 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% of Brazilian municipalities at the time. Whether providing additional financing to local governments in other contexts would yield similar results is an open empirical question. The most closely related study on the effects of oil windfalls on local spending and living standards in Brazilian municipalities finds little if any effect on local public services or household income per capita, although in some specifications they also find a reduction in the poverty rate (Caselli and Michaels 2009).

Needless to say, the results presented in this paper do not imply that all is well with the way public services are delivered in Brazil. The 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 funds they received to expand public services to the general local population at reasonable cost. Future research might 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.

11 References

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Table 1: 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 2: Descriptive statistics (sample means)

	Population range					
	7'500 - 44'148	8'500 - 18'700				
Sample	Full	Full	North	South	Rural	Urban
Observations	2306	1248	536	712	624	624
1980 county characteristics (IBGE)						
Average years of schooling (25 years and older)	1.96	1.90	1.04	2.56	1.52	2.29
Percentage of residents living in urban areas (%)	30.0	27.9	22.4	32.2	14.8	41.7
Net enrollment rate of 7- to 14-year-olds (%)	55.6	55.5	39.5	67.6	48.9	62.1
Illiteracy rate, 15 years and older (%)	39.0	39.1	56.0	26.3	44.4	33.7
Poverty headcount ratio (national poverty line, %)	58.6	59.3	78.0	45.3	67.9	50.7
Income per capita (% of minimum salary in 1991)	77.5	75.2	41.0	101.0	58.6	91.9
Infant mortality (per 1000 life births)	88.9	88.5	129.0	57.6	96.2	80.7
GDP ('000) 2008 Reais (IPEA)	108'587	64'214	33'023	87'728	46'827	81'741
1982 Financial data (Ministry of Finance)						
Total county revenue ('000) 2008 Reais	3'957	2'876	1'826	3'562	2'360	3'365
Total county revenue 1982/GDP 1980 (%)	5.3	5.6	7.3	4.5	6.2	5.0
FPM transfers/total revenue (%)	48.0	49.7	66.4	37.9	56.4	43.3
Own revenue/total revenue (%)	5.9	5.1	1.1	7.7	2.6	7.5
Other revenue/total revenue (%)	46.9	45.9	32.9	54.7	41.9	49.7
Administrative spending/total spending (%)	22.3	22.3	22.9	21.7	21.8	22.9
Education spending/total spending (%)	20.9	21.2	23.9	18.6	22.3	20.0
Housing spending/total spending (%)	19.5	17.9	19.9	16.0	15.9	20.2
Health spending/total spending (%)	9.9	10.4	14.3	6.3	11.1	9.6
Transportation spending/total spending (%)	20.9	21.8	12.2	30.0	23.2	20.2
Other spending/total spending (%)	8.5	8.5	8.5	8.5	8.2	8.6
1991 Real school resources (school census)						
Number of municipal elementary schools	37.8	30.2	41.0	20.0	37.5	21.4
Primary school student-teacher ratio	20.3	19.7	22.5	17.1	20.4	18.9
1991 Housing and urban services (IBGE)						
Individuals with access to electricity (%)	71.0	70.0	52.4	83.3	58.2	81.9
Individuals with access to drinking water (%)	70.0	69.4	50.0	83.8	62.2	76.5
Individuals with access to sewer (%)	41.3	41.6	19.7	54.2	30.9	50.2
Individuals living in inadequate housing (%)	0.61	0.53	0.62	0.47	0.46	0.62
1991 education outcomes (census)						
Average years of schooling (19- to 28-year-olds)	4.6	4.5	3.33	5.5	4.0	5.1
Literacy rate (19- to 28-year-olds) (%)	78.8	79.0	63.0	91.1	73.7	84.3
Average years of schooling (9- to 18-year-olds)	2.8	2.9	1.8	3.6	2.5	3.2
Literacy rate (9- to 18-year-olds) (%)	76.8	77.2	57.8	91.9	71.1	83.4
1991 Household income (IBGE)						
Poverty headcount ratio (R$140 poverty line) (%)	60.0	60.2	80.0	45.3	69.2	51.2
Household income per capita 2008 Reais	224	217	116	294	169	266

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 3: Test of discontinuities in pre-treatment covariates

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

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

Table 4: Impact on total revenue

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

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Various[1]
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.146*	0.192***	0.171***	0.167***	0.199***	0.179***	0.160***
	(0.077)	(0.071)	(0.061)	(0.053)	(0.056)	(0.050)	(0.037)
Observations	176	173	252	249	332	328	1041
R-squared	0.75	0.85	0.72	0.82	0.65	0.78	0.75
Pooled cutoffs 1-2							
I[X > 0]	0.169*	0.256***	0.207**	0.217***	0.202***	0.193***	0.210***
	(0.102)	(0.086)	(0.082)	(0.072)	(0.075)	(0.067)	(0.048)
Observations	116	115	173	172	222	220	711
R-squared	0.73	0.84	0.73	0.82	0.70	0.81	0.76
1st cutoff							
I[pop > 10188]	0.143	0.320**	0.255**	0.267**	0.233**	0.223**	0.256***
	(0.154)	(0.155)	(0.113)	(0.112)	(0.101)	(0.099)	(0.085)
Observations	58	57	88	87	117	115	386
R-squared	0.86	0.91	0.86	0.91	0.81	0.87	0.80
2nd cutoff							
I[pop > 13584]	0.148	0.195	0.205	0.291**	0.218*	0.282**	0.153**
	(0.182)	(0.180)	(0.135)	(0.142)	(0.123)	(0.124)	(0.060)
Observations	58	58	85	85	105	105	325
R-squared	0.67	0.82	0.67	0.80	0.67	0.81	0.75
3rd cutoff							
I[pop > 16980]	-0.045	-0.036	0.008	0.044	0.095	0.096	0.084*
	(0.133)	(0.102)	(0.114)	(0.080)	(0.113)	(0.069)	(0.047)
Observations	60	58	79	77	110	108	330
R-squared	0.86	0.95	0.82	0.92	0.63	0.81	0.76
4th cutoff							
I[pop > 23772]	0.023	0.142	0.088	0.093	0.202	0.102	0.096
	(0.258)	(0.131)	(0.203)	(0.164)	(0.147)	(0.118)	(0.081)
Observations	39	39	62	61	89	88	302
R-squared	0.90	0.96	0.81	0.88	0.77	0.86	0.82

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

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

Table 5: Impact on total public spending

Dependent variable: log total public spending per capita (1982-1985)							
Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Various[1]
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.173**	0.211***	0.172***	0.163***	0.206***	0.184***	0.158***
	(0.076)	(0.065)	(0.060)	(0.051)	(0.055)	(0.047)	(0.036)
Observations	191	188	278	275	368	364	1158
R-squared	0.76	0.85	0.73	0.83	0.68	0.80	0.76
Pooled cutoffs 1-2							
I[X > 0]	0.227***	0.280***	0.227***	0.218***	0.231***	0.207***	0.208***
	(0.098)	(0.082)	(0.078)	(0.070)	(0.071)	(0.062)	(0.046)
Observations	124	124	190	189	247	245	789
R-squared	0.75	0.85	0.74	0.82	0.73	0.82	0.77
1st cutoff							
I[pop > 10188]	0.199	0.379**	0.263**	0.267**	0.249***	0.234**	0.248***
	(0.161)	(0.159)	(0.113)	(0.112)	(0.094)	(0.093)	(0.057)
Observations	62	61	95	94	128	126	428
R-squared	0.84	0.90	0.85	0.89	0.81	0.86	0.80
2nd cutoff							
I[pop > 13584]	0.214	0.188	0.227*	0.258*	0.249**	0.262**	0.205**
	(0.172)	(0.166)	(0.127)	(0.135)	(0.114)	(0.111)	(0.095)
Observations	63	63	95	95	119	119	361
R-squared	0.70	0.84	0.71	0.82	0.71	0.83	0.77
3rd cutoff							
I[pop > 16980]	-0.038	-0.027	-0.008	0.023	0.073	0.091	0.094**
	(0.145)	(0.113)	(0.122)	(0.083)	(0.117)	(0.077)	(0.045)
Observations	66	64	88	86	121	119	369
R-squared	0.84	0.93	0.82	0.92	0.67	0.84	0.77
4th cutoff							
I[pop > 23772]	0.045	0.165	0.152	0.134	0.159	0.061	0.111
	(0.272)	(0.184)	(0.195)	(0.144)	(0.146)	(0.115)	(0.070)
Observations	44	44	69	68	96	95	353
R-squared	0.79	0.91	0.83	0.91	0.80	0.89	0.84

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

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

Table 6: Impacts on spending categories

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Various[1]
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Panel A: log education spending per capita (1982-1983)							
Pooled cutoffs 1-3							
I[X > 0]	0.283	0.235	0.224	0.128	0.293**	0.232*	0.190***
	(0.195)	(0.214)	(0.140)	(0.142)	(0.119)	(0.121)	(0.066)
Observations	140	137	205	202	273	269	832
R-squared	0.44	0.54	0.49	0.55	0.46	0.51	0.43
Pooled cutoffs 1-2							
I[X > 0]	0.442*	0.519**	0.390**	0.270	0.320**	0.322**	0.340***
	(0.230)	(0.253)	(0.176)	(0.181)	(0.143)	(0.144)	(0.110)
Observations	94	93	141	140	185	183	578
R-squared	0.57	0.69	0.59	0.63	0.52	0.57	0.45
Panel B: log housing and urban infrastructure spending per capita (1982-1983)							
Pooled cutoffs 1-3							
I[X > 0]	0.100	-0.050	0.152	-0.010	0.352*	0.277	0.312**
	(0.315)	(0.332)	(0.242)	(0.231)	(0.207)	(0.203)	(0.147)
Observations	136	133	198	195	263	259	810
R-squared	0.43	0.58	0.37	0.54	0.35	0.52	0.45
Pooled cutoffs 1-2							
I[X > 0]	0.396	0.135	0.435	0.141	0.550**	0.462**	0.451***
	(0.323)	(0.332)	(0.278)	(0.249)	(0.236)	(0.231)	(0.163)
Observations	92	91	136	135	180	178	564
R-squared	0.46	0.69	0.41	0.60	0.36	0.53	0.43
Panel C: log transportation spending per capita (1982-1983)							
Pooled cutoffs 1-3							
I[X > 0]	0.156	0.064	0.130	0.105	0.258	0.222	0.170*
	(0.232)	(0.267)	(0.177)	(0.198)	(0.161)	(0.163)	(0.100)
Observations	139	136	202	199	267	263	810
R-squared	0.80	0.82	0.78	0.80	0.73	0.74	0.70
Pooled cutoffs 1-2							
I[X > 0]	0.208	0.232	0.218	0.232	0.258	0.276	0.221**
	(0.288)	(0.364)	(0.226)	(0.264)	(0.199)	(0.205)	(0.113)
Observations	93	92	139	138	181	179	565
R-squared	0.83	0.85	0.82	0.83	0.78	0.79	0.73

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

[1]Moving down the table from the top of Panel A to the bottom of panel C, the specifications are linear, quadratic, quadratic, quadratic, linear, and linear, respectively.

Table 7: Impact on teacher-student ratio

Dependent variable: primary school teacher-student ratio in 1991, LHS mean: 0.054, sd: 0.02

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.012**	0.010**	0.009**	0.007*	0.008**	0.006*	0.012***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.005)
Observations	173	170	259	256	340	336	1098
R-squared	0.45	0.53	0.44	0.52	0.42	0.50	0.48
Pooled cutoffs 1-2							
I[X > 0]	0.012**	0.012**	0.008*	0.010**	0.006	0.006	0.007
	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
Observations	112	111	177	176	232	230	753
R-squared	0.45	0.56	0.45	0.53	0.41	0.48	0.47
1st cutoff							
I[pop > 10188]	0.009	0.011	0.015	0.014	0.011	0.011*	0.016**
	(0.013)	(0.014)	(0.010)	(0.008)	(0.008)	(0.006)	(0.008)
Observations	54	53	89	88	122	120	411
R-squared	0.37	0.71	0.36	0.56	0.36	0.50	0.46
2nd cutoff							
I[pop > 13584]	0.015	0.019	0.004	0.011	0.002	0.002	0.005
	(0.010)	(0.012)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)
Observations	58	58	88	88	110	110	342
R-squared	0.57	0.65	0.59	0.67	0.56	0.62	0.49
3rd cutoff							
I[pop > 16980]	0.019*	0.011	0.013*	0.007	0.013**	0.009	0.026***
	(0.010)	(0.012)	(0.008)	(0.008)	(0.007)	(0.006)	(0.008)
Observations	61	59	82	80	108	106	345
R-squared	0.72	0.77	0.66	0.75	0.58	0.66	0.53

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 8: Impact on schooling, 19- to 28-year-olds in 1991

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

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.330	0.231	0.527***	0.312***	0.551***	0.290***	0.356**
	(0.260)	(0.151)	(0.199)	(0.114)	(0.172)	(0.102)	(0.140)
Observations	200	197	293	290	386	382	1243
R-squared	0.72	0.89	0.71	0.89	0.69	0.89	0.88
Pooled cutoffs 1-2							
I[X > 0]	0.415	0.191	0.511**	0.309**	0.512**	0.304**	0.374**
	(0.324)	(0.180)	(0.243)	(0.140)	(0.215)	(0.129)	(0.179)
Observations	131	130	200	199	259	257	857
R-squared	0.74	0.90	0.74	0.89	0.71	0.88	0.87
1st cutoff							
I[pop > 10188]	0.286	0.557	0.445	0.439	0.403	0.424*	0.525*
	(0.500)	(0.484)	(0.352)	(0.302)	(0.340)	(0.242)	(0.313)
Observations	66	65	101	100	135	133	470
R-squared	0.79	0.91	0.78	0.89	0.75	0.89	0.87
2nd cutoff							
I[pop > 13584]	0.398	0.347*	0.497	0.338*	0.585*	0.257	0.215
	(0.530)	(0.204)	(0.373)	(0.172)	(0.305)	(0.158)	(0.193)
Observations	65	65	99	99	124	124	387
R-squared	0.77	0.96	0.76	0.93	0.73	0.90	0.88
3rd cutoff							
I[pop > 16980]	0.024	0.403	0.280	0.185	0.552	0.169	0.366
	(0.507)	(0.333)	(0.385)	(0.224)	(0.353)	(0.192)	(0.231)
Observations	69	67	93	91	127	125	386
R-squared	0.77	0.94	0.73	0.93	0.70	0.92	0.91

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

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

Dependent variable: average years of schooling, 9- to 18-year-olds in 1991, LHS mean: 2.7, sd: 1.08

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.211	0.158*	0.293**	0.173**	0.301***	0.144**	0.181**
	(0.157)	(0.095)	(0.117)	(0.071)	(0.099)	(0.062)	(0.082)
Observations	200	197	293	290	386	382	1243
R-squared	0.84	0.94	0.83	0.93	0.81	0.93	0.92
Pooled cutoffs 1-2							
I[X > 0]	0.269	0.175	0.287*	0.209**	0.285**	0.176**	0.218**
	(0.204)	(0.120)	(0.150)	(0.090)	(0.130)	(0.081)	(0.103)
Observations	131	130	200	199	259	257	857
R-squared	0.83	0.94	0.83	0.93	0.81	0.92	0.91
1st cutoff							
I[pop > 10188]	0.245	0.412	0.340*	0.352**	0.268	0.265*	0.370**
	(0.270)	(0.270)	(0.202)	(0.168)	(0.189)	(0.134)	(0.170)
Observations	66	65	101	100	135	133	470
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	0.307	0.102	0.115
	(0.311)	(0.157)	(0.224)	(0.116)	(0.191)	(0.110)	(0.082)
Observations	65	65	99	99	124	124	387
R-squared	0.84	0.96	0.84	0.95	0.81	0.93	0.91
3rd cutoff							
I[pop > 16980]	0.052	0.145	0.127	0.022	0.296	0.060	0.128
	(0.282)	(0.225)	(0.210)	(0.144)	(0.188)	(0.124)	(0.147)
Observations	69	67	93	91	127	125	386
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 (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 10: Impact on literacy, 19- to 28-year-olds in 1991

Dependent variable: literacy rate, 19- to 28-year-olds in 1991, LHS mean: 0.76, sd: 0.17

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.058**	0.048***	0.064***	0.050***	0.062***	0.042***	0.054***
	(0.027)	(0.016)	(0.019)	(0.012)	(0.016)	(0.011)	(0.014)
Observations	200	197	293	290	386	382	1243
R-squared	0.78	0.91	0.80	0.92	0.80	0.91	0.90
Pooled cutoffs 1-2							
I[X > 0]	0.055	0.044**	0.046**	0.047***	0.046**	0.037***	0.053***
	(0.035)	(0.019)	(0.023)	(0.015)	(0.021)	(0.014)	(0.018)
Observations	131	130	200	199	259	257	857
R-squared	0.79	0.93	0.83	0.93	0.81	0.91	0.90
1st cutoff							
I[pop > 10188]	0.059	0.066*	0.059	0.076***	0.042	0.048*	0.073**
	(0.059)	(0.037)	(0.040)	(0.026)	(0.039)	(0.024)	(0.031)
Observations	66	65	101	100	135	133	470
R-squared	0.83	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	0.052**	0.027*	0.029**
	(0.040)	(0.023)	(0.028)	(0.015)	(0.023)	(0.015)	(0.013)
Observations	65	65	99	99	124	124	387
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	0.058***
	(0.044)	(0.030)	(0.036)	(0.026)	(0.031)	(0.020)	(0.021)
Observations	69	67	93	91	127	125	386
R-squared	0.86	0.95	0.84	0.94	0.82	0.94	0.92

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 11: Impact on literarcy, 9- to 18-year-olds in 1991

Dependent variable: literacy rate, 9- to 18-year-olds in 1991, LHS mean: 0.74, sd: 0.20

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	0.038	0.028	0.043**	0.027*	0.048***	0.026**	0.032**
	(0.028)	(0.019)	(0.020)	(0.014)	(0.017)	(0.012)	(0.016)
Observations	200	197	293	290	386	382	1243
R-squared	0.82	0.93	0.83	0.91	0.82	0.91	0.90
Pooled cutoffs 1-2							
I[X > 0]	0.050	0.036	0.038	0.037**	0.040*	0.028*	0.043**
	(0.037)	(0.023)	(0.025)	(0.017)	(0.022)	(0.016)	(0.019)
Observations	131	130	200	199	259	257	857
R-squared	0.82	0.93	0.84	0.92	0.82	0.91	0.90
1st cutoff							
I[pop > 10188]	0.050	0.057	0.061	0.073**	0.054	0.059**	0.073**
	(0.058)	(0.048)	(0.041)	(0.032)	(0.040)	(0.026)	(0.031)
Observations	66	65	101	100	135	133	470
R-squared	0.85	0.94	0.84	0.92	0.81	0.91	0.90
2nd cutoff							
I[pop > 13584]	0.041	0.037	0.024	0.020	0.034	0.006	0.019
	(0.045)	(0.037)	(0.031)	(0.020)	(0.026)	(0.018)	(0.014)
Observations	65	65	99	99	124	124	387
R-squared	0.84	0.94	0.88	0.95	0.87	0.94	0.90
3rd cutoff							
I[pop > 16980]	-0.006	0.011	0.029	0.010	0.058*	0.015	0.015
	(0.050)	(0.045)	(0.039)	(0.034)	(0.034)	(0.026)	(0.031)
Observations	69	67	93	91	127	125	386
R-squared	0.87	0.94	0.85	0.93	0.84	0.93	0.92

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 12: Impact on the poverty rate in 1991

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

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Pooled cutoffs 1-3							
I[X > 0]	-0.038	-0.066***	-0.062**	-0.053***	-0.058**	-0.039***	-0.043***
	(0.039)	(0.022)	(0.029)	(0.017)	(0.024)	(0.015)	(0.018)
Observations	200	197	293	290	386	382	1243
R-squared	0.79	0.93	0.78	0.92	0.76	0.91	0.91
Pooled cutoffs 1-2							
I[X > 0]	-0.012	-0.052*	-0.043	-0.042**	-0.036	-0.023	-0.038
	(0.052)	(0.029)	(0.039)	(0.022)	(0.032)	(0.019)	(0.023)
Observations	131	130	200	199	259	257	857
R-squared	0.77	0.93	0.77	0.93	0.76	0.92	0.91
1st cutoff							
I[pop > 10188]	-0.019	-0.010**	-0.032	-0.048	-0.026	-0.027	-0.027
	(0.056)	(0.048)	(0.044)	(0.032)	(0.042)	(0.031)	(0.018)
Observations	66	65	101	100	135	133	470
R-squared	0.87	0.95	0.84	0.93	0.81	0.92	0.90
2nd cutoff							
I[pop > 13584]	-0.014	-0.048	-0.060	-0.055	-0.042	-0.026	-0.054
	(0.010)	(0.055)	(0.063)	(0.040)	(0.051)	(0.033)	(0.035)
Observations	65	65	99	99	124	124	387
R-squared	0.73	0.94	0.74	0.93	0.74	0.93	0.91
3rd cutoff							
I[pop > 16980]	-0.096	-0.010**	-0.096*	-0.070**	-0.088**	-0.061**	-0.071**
	(0.067)	(0.047)	(0.050)	(0.031)	(0.043)	(0.027)	(0.035)
Observations	69	67	93	91	127	125	386
R-squared	0.85	0.94	0.82	0.93	0.78	0.92	0.91

Notes: OLS estimations. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects. The pooled specifications include segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 13: Joint significance test of education, income and public service outcomes

Neighborhood (%):	2	2	3	3	4	4
Pre-treatment covariates	N	Y	N	Y	N	Y
Education Outcomes						
Literacy rate (19- to 28-year-olds)	0.058** (0.027)	0.048*** (0.016)	0.064*** (0.019)	0.050*** (0.012)	0.062*** (0.016)	0.042*** (0.011)
Literacy rate (9- to 18-year-olds)	0.038 (0.028)	0.028 (0.019)	0.043** (0.020)	0.027* (0.015)	0.048*** (0.018)	0.026** (0.012)
Average years of schooling (19- to 28-year-olds)	0.330 (0.261)	0.231 (0.151)	0.527*** (0.199)	0.312*** (0.114)	0.551*** (0.172)	0.290*** (0.103)
Average years of schooling (9- to 18-year-olds)	0.211 (0.158)	0.158* (0.096)	0.293** (0.118)	0.173** (0.071)	0.301*** (0.010)	0.144** (0.062)
Household income						
Poverty headcount ratio (National poverty line)	-0.038 (0.039)	-0.065*** (0.022)	-0.062** (0.028)	-0.053*** (0.017)	-0.058** (0.024)	-0.039*** (0.015)
Income per capita (R$ 2008)	8.170 (24.37)	17.42 (17.93)	16.19 (19.60)	5.731 (16.11)	20.10 (17.53)	6.107 (14.35)
School resources						
Number of municipal elementary schools	5.163 (5.770)	4.317 (5.141)	-0.807 (4.578)	2.190 (4.194)	-2.896 (4.077)	-0.243 (3.623)
Primary school Teacher-student ratio	0.012** (0.005)	0.010** (0.005)	0.009** (0.004)	0.007* (0.004)	0.008** (0.003)	0.006* (0.003)
Housing and urban services						
Individuals with access to electricity (%)	3.850 (4.538)	5.163* (2.986)	5.555 (3.687)	3.618 (2.551)	4.182 (3.140)	1.864 (2.113)
Individuals with access to water (%)	5.258 (3.620)	5.919* (3.527)	6.025** (3.055)	5.587* (2.894)	1.032 (2.841)	0.043 (2.741)
Individuals with access to sewer (%)	2.249 (7.361)	4.783 (6.956)	8.265 (5.726)	8.806 (5.483)	1.679 (5.027)	2.650 (4.736)
Individuals living in inadequate housing (%)	0.029 (0.666)	0.188 (0.635)	-0.000 (0.320)	-0.011 (0.359)	-0.167 (0.404)	-0.119 (0.392)
F-statistic (p-value)	1.47 (0.14)	2.79 (0.00)	1.84 (0.04)	3.47 (0.00)	1.94 (0.03)	2.33 (0.00)

Notes: All entries are local linear estimates from the pooled specification across the first 3 cutoffs. The F-statistic tests the joint null hypotheses of no discontinuities in any outcome variable. All outcome variables from the 1991 school or regular census. Clustered (at the municipality level) standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications allow for differential slopes by segment and on each side of the cutoff.

Table 14: Total spending and teacher-student ratio, North vs. South

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Panel A: South of Brazil (South, Southeast and Center-west regions)							
Dependent variable: log total public spending per capita (1982-1985)							
I[X > 0]	0.202*	0.228**	0.183**	0.201***	0.222***	0.215***	0.187***
	(0.106)	(0.091)	(0.087)	(0.074)	(0.080)	(0.071)	(0.052)
Observations	106	103	151	148	203	199	644
R-squared	0.60	0.75	0.59	0.76	0.55	0.73	0.65
Dependent variable: primary school teacher-student ratio in 1991, LHS mean: 0.062, sd: 0.03							
I[X > 0]	0.018**	0.018**	0.013*	0.012**	0.011*	0.010*	0.018**
	(0.010)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.008)
Observations	87	84	129	126	171	167	550
R-squared	0.32	0.46	0.30	0.45	0.24	0.38	0.40
Panel B: North of Brazil (North and Northeast regions)							
Dependent variable: log total public spending per capita (1982-1985)							
I[X > 0]	0.145	0.149*	0.191**	0.142**	0.207***	0.118**	0.142**
	(0.095)	(0.074)	(0.080)	(0.067)	(0.075)	(0.056)	(0.063)
Observations	85	85	127	127	165	165	514
R-squared	0.36	0.64	0.43	0.63	0.31	0.58	0.40
Dependent variable: primary school teacher-student ratio in 1991, LHS mean: 0.046, sd: 0.01							
I[X > 0]	0.006	0.001	0.004	0.001	0.006	0.004	0.007*
	(0.005)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
Observations	86	86	130	130	169	169	548
R-squared	0.38	0.46	0.40	0.48	0.58	0.50	0.40

Notes: All entries are local linear estimates (standard errors) from the pooled specification across the first 3 cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 15: Schooling, literacy and poverty, North vs. South

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Panel A: South of Brazil (South, Southeast and Center-west regions)							
Dependent variable: average years of schooling, 19- to 28-year-olds in 1991, LHS mean: 5.4, sd: 0.90							
I[X > 0]	0.162	0.148	0.361	0.198	0.293	0.138	0.137
	(0.347)	(0.171)	(0.272)	(0.125)	(0.229)	(0.122)	(0.157)
Observations	114	111	163	160	217	213	695
R-squared	0.32	0.75	0.32	0.78	0.24	0.76	0.76
Dependent variable: literacy rate, 19- to 28-year-olds in 1991, LHS mean: 0.90, sd: 0.06							
I[X > 0]	0.035	0.038***	0.033*	0.028***	0.023	0.020**	0.024***
	(0.023)	(0.010)	(0.019)	(0.008)	(0.016)	(0.008)	(0.008)
Observations	114	111	163	160	217	213	695
R-squared	0.39	0.87	0.36	0.85	0.28	0.81	0.80
Dependent variable: 1991 poverty rate, LHS mean: 0.47, sd: 0.17							
I[X > 0]	-0.038	-0.085***	-0.068	-0.070***	-0.072*	-0.062***	-0.054**
	(0.064)	(0.031)	(0.046)	(0.024)	(0.039)	(0.021)	(0.024)
Observations	114	111	163	160	217	213	695
R-squared	0.50	0.87	0.46	0.86	0.39	0.83	0.82
Panel B: North of Brazil (North and Northeast regions)							
Dependent variable: average years of schooling, 19- to 28-year-olds in 1991, LHS mean: 3.0, sd: 0.90							
I[X > 0]	0.613	0.250	0.764**	0.315	0.937***	0.341**	0.461**
	(0.415)	(0.272)	(0.306)	(0.217)	(0.257)	(0.171)	(0.218)
Observations	86	86	130	130	169	169	548
R-squared	0.35	0.76	0.31	0.70	0.26	0.70	0.68
Dependent variable: literacy rate, 19- to 28-year-olds in 1991, LHS mean: 0.60, sd: 0.12							
I[X > 0]	0.093	0.057	0.110***	0.074**	0.116***	0.058**	0.065**
	(0.059)	(0.038)	(0.039)	(0.030)	(0.031)	(0.023)	(0.028)
Observations	86	86	130	130	169	169	548
R-squared	0.33	0.72	0.36	0.70	0.36	0.70	0.65
Dependent variable: 1991 poverty rate, LHS mean: 0.82, sd: 0.06							
I[X > 0]	-0.041*	-0.023	-0.045**	-0.011	-0.046**	-0.006	-0.033
	(0.024)	(0.023)	(0.021)	(0.020)	(0.020)	(0.016)	(0.021)
Observations	86	86	130	130	169	169	548
R-squared	0.44	0.73	0.43	0.68	0.39	0.64	0.55

Notes: All entries are local linear estimates (standard errors) from the pooled specification across the first 3 cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

Table 16: Total spending and teacher-student ration, urban vs. rural municipalities

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Various[1]
Neighborhood (%)	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Panel A: urban municipalities (% urban residents in 1980 > 24.8)							
Dependent variable: log total public spending per capita (1982-1985)							
I[X > 0]	0.130	0.134	0.127	0.072	0.200**	0.141*	0.147**
	(0.127)	(0.098)	(0.105)	(0.092)	(0.089)	(0.080)	(0.061)
Observations	100	97	138	135	184	180	573
R-squared	0.73	0.84	0.72	0.80	0.62	0.74	0.75
Dependent variable: primary school teacher-student ratio in 1991, LHS mean: 0.058, sd: 0.023							
I[X > 0]	0.003	0.001	0.005	0.004	0.004	0.001	0.007
	(0.012)	(0.012)	(0.009)	(0.008)	(0.008)	(0.007)	(0.009)
Observations	80	77	116	113	152	148	485
R-squared	0.43	0.54	0.41	0.50	0.40	0.48	0.44
Panel B: rural municipalities (% urban residents in 1980 < 24.8)							
Dependent variable: log total public spending per capita (1982-1985)							
I[X > 0]	0.210***	0.271***	0.219***	0.209***	0.200***	0.189***	0.189***
	(0.076)	(0.079)	(0.062)	(0.064)	(0.053)	(0.052)	(0.052)
Observations	91	91	140	140	184	184	585
R-squared	0.85	0.90	0.85	0.90	0.82	0.88	0.83
Dependent variable: primary school teacher-student ratio in 1991, LHS mean: 0.050, sd: 0.013							
I[X > 0]	0.011***	0.011**	0.007**	0.007	0.008**	0.008***	0.012***
	(0.004)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
Observations	93	93	143	143	188	188	613
R-squared	0.62	0.63	0.60	0.63	0.57	0.60	0.57

Notes: All entries are local linear estimates (standard errors) from the pooled specification across the first 3 cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

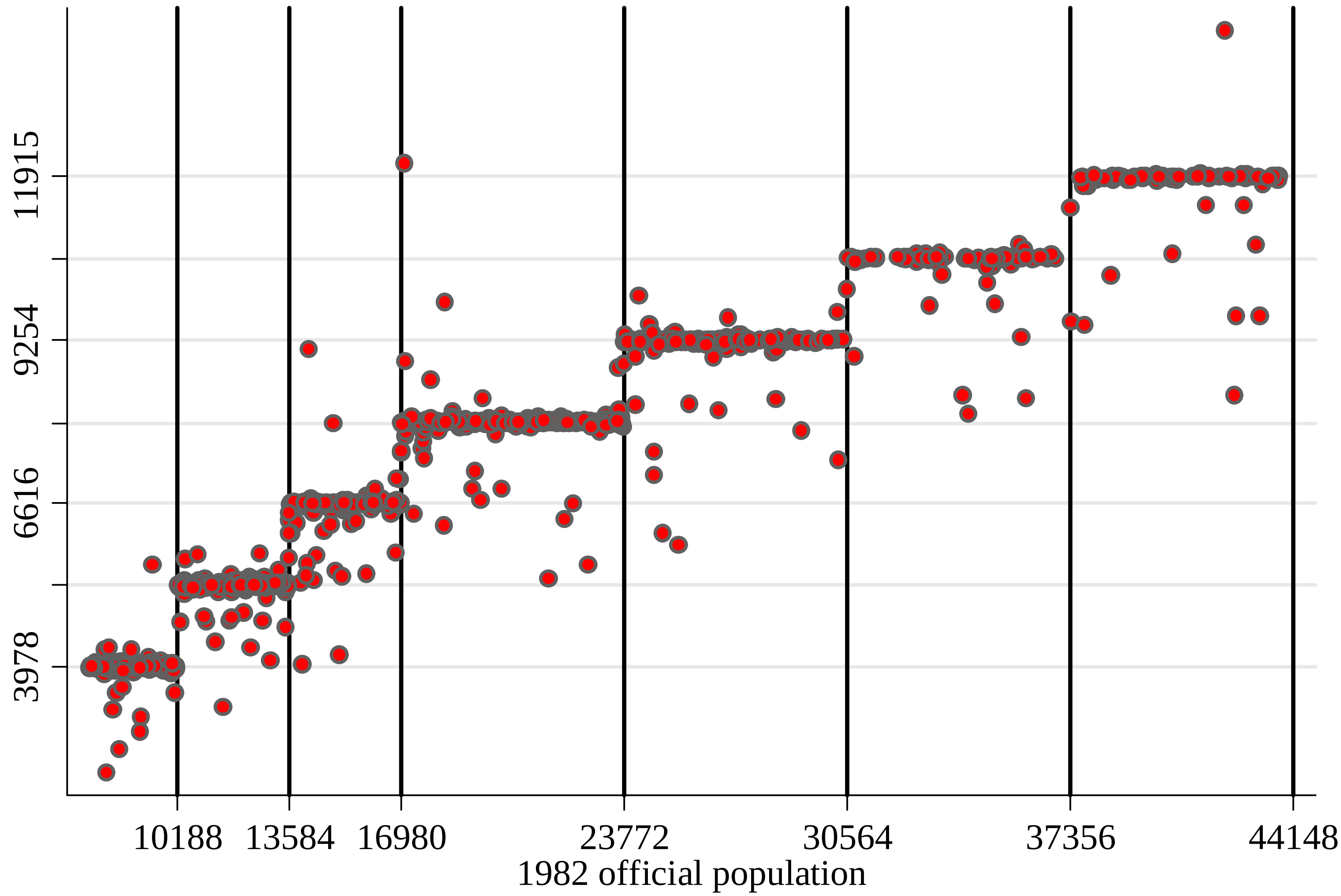
[1]Moving down the last column of the table from the top of Panel A to the bottom of panel B, the specifications are quadratic, quartic, cubic, and quartic, respectively.

Table 17: Schooling, literacy, poverty, urban vs. rural municipalities

Polynomial specification:	Linear	Linear	Linear	Linear	Linear	Linear	Quartic
Neighborhood (%):	2	2	3	3	4	4	15
Pre-treatment covariates:	N	Y	N	Y	N	Y	Y
Panel A: urban municipalities (% urban residents in 1980 > 24.8)							
Dependent variable: average years of schooling,19- to 28-year-olds in 1991, LHS mean: 5.1, sd: 1.18							
I[X > 0]	0.114	0.122	0.404	0.185	0.489**	0.163	0.128
	(0.316)	(0.183)	(0.266)	(0.156)	(0.223)	(0.139)	(0.167)
Observations	105	102	146	143	193	189	609
R-squared	0.68	0.88	0.71	0.87	0.69	0.86	0.86
Dependent variable: literacy rate, 19- to 28-year-olds in 1991, LHS mean: 0.84, sd: 0.14							
I[X > 0]	0.038*	0.038**	0.054***	0.042***	0.070***	0.043***	0.038***
	(0.022)	(0.014)	(0.019)	(0.012)	(0.017)	(0.012)	(0.013)
Observations	105	102	146	143	193	189	609
R-squared	0.87	0.94	0.87	0.94	0.85	0.93	0.91
Dependent variable: 1991 poverty rate, LHS mean: 0.52, sd: 0.22							
I[X > 0]	-0.051	-0.057**	-0.066*	-0.045**	-0.072**	-0.038**	-0.030***
	(-0.044)	(0.028)	(0.034)	(0.022)	(0.031)	(0.019)	(0.001)
Observations	105	102	146	143	193	189	609
R-squared	0.82	0.95	0.84	0.94	0.82	0.93	0.92
Panel B: Rural Municipalities (% urban residents in 1980 < 24.8)							
Dependent variable: average years of schooling, 19- to 28-year-olds old in 1991, LHS mean: 3.5, sd: 1.38							
I[X > 0]	0.351	0.237	0.589**	0.476	0.653***	0.494***	0.463**
	(0.412)	(0.276)	(0.297)	(0.203)	(0.243)	(0.167)	(0.211)
Observations	95	95	147	147	193	193	634
R-squared	0.72	0.89	0.75	0.88	0.73	0.87	0.87
Dependent variable: literacy rate, 19- to 28-year-olds in 1991, LHS mean: 0.69, sd: 0.17							
I[X > 0]	0.057	0.037	0.055	0.050*	0.056*	0.044**	0.043*
	(0.054)	(0.038)	(0.036)	(0.027)	(0.028)	(0.022)	(0.023)
Observations	95	95	147	147	193	193	634
R-squared	0.72	0.91	0.78	0.91	0.78	0.90	0.88
Dependent variable: 1991 poverty rate, LHS mean: 0.76, sd: 0.15							
I[X > 0]	-0.002	-0.041	-0.048	-0.050*	-0.047	-0.036	-0.050*
	(0.045)	(0.031)	(0.034)	(0.028)	(0.029)	(0.022)	(0.026)
Observations	95	95	147	147	193	193	634
R-squared	0.77	0.88	0.73	0.87	0.71	0.85	0.85

Notes: All entries are local linear estimates (standard errors) from the pooled specification across the first 3 cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (%) is % distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pre-treatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1%, 5% and 10% levels, respectively.

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



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

Figure 2: Estimation Approach

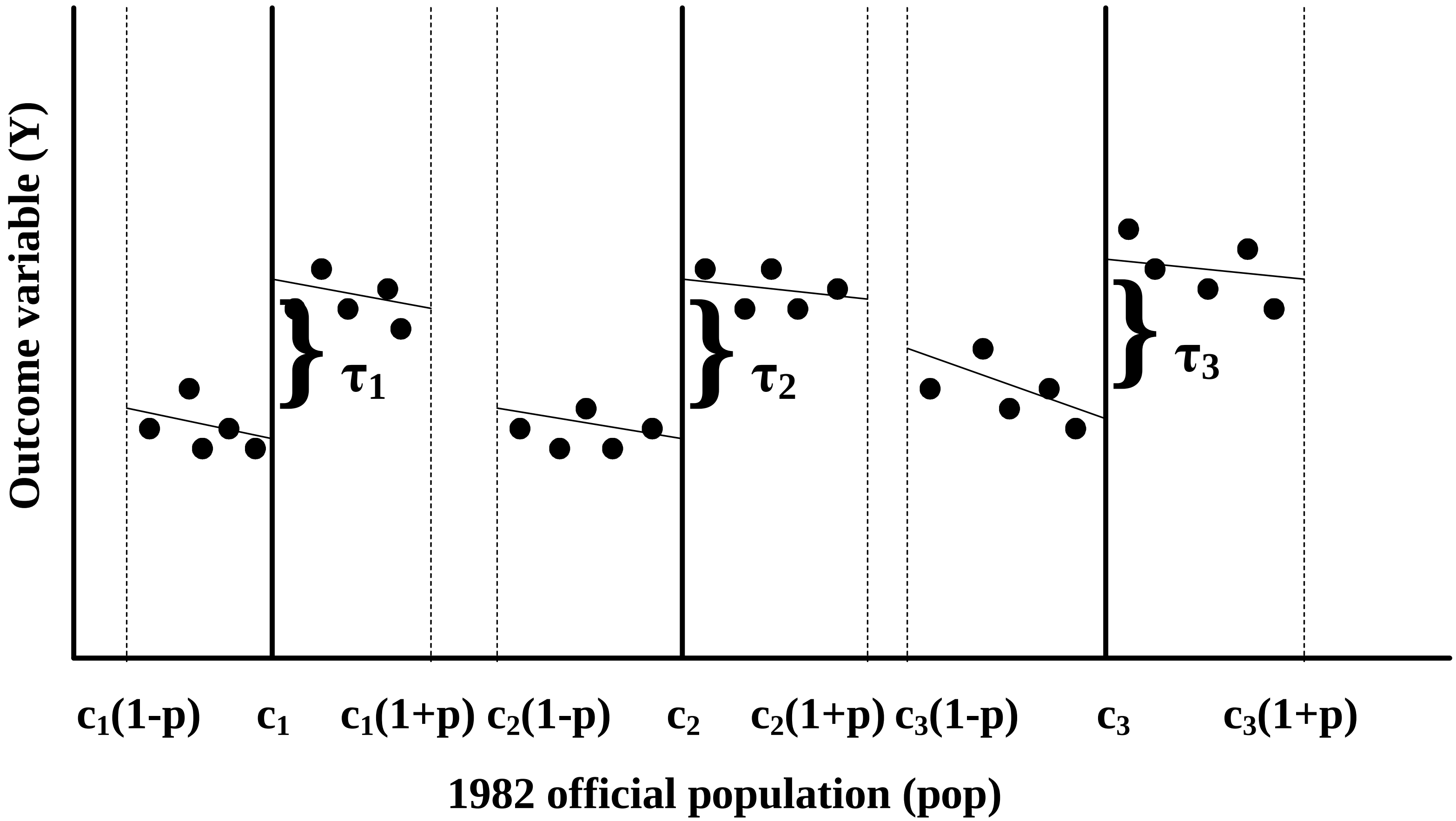
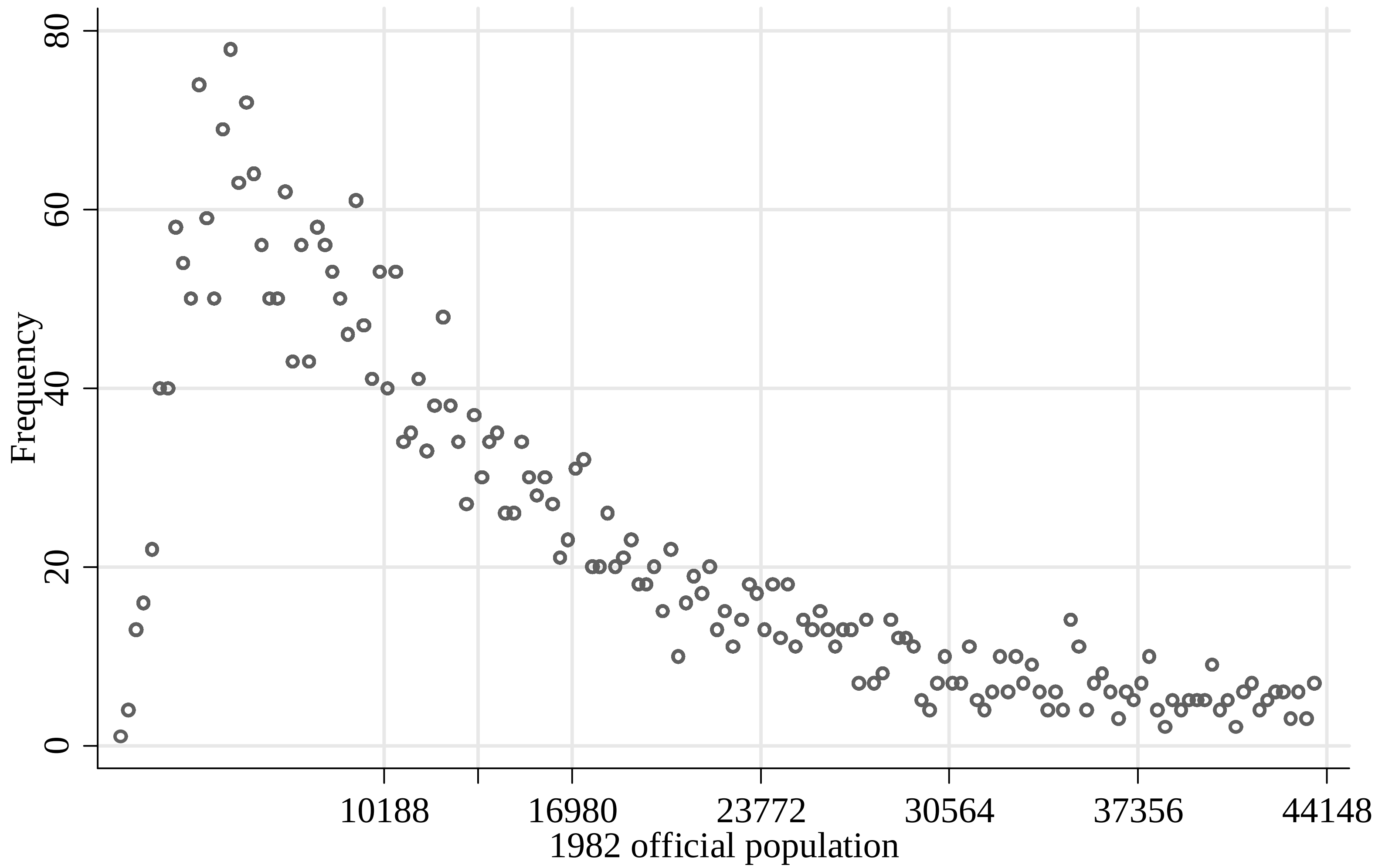
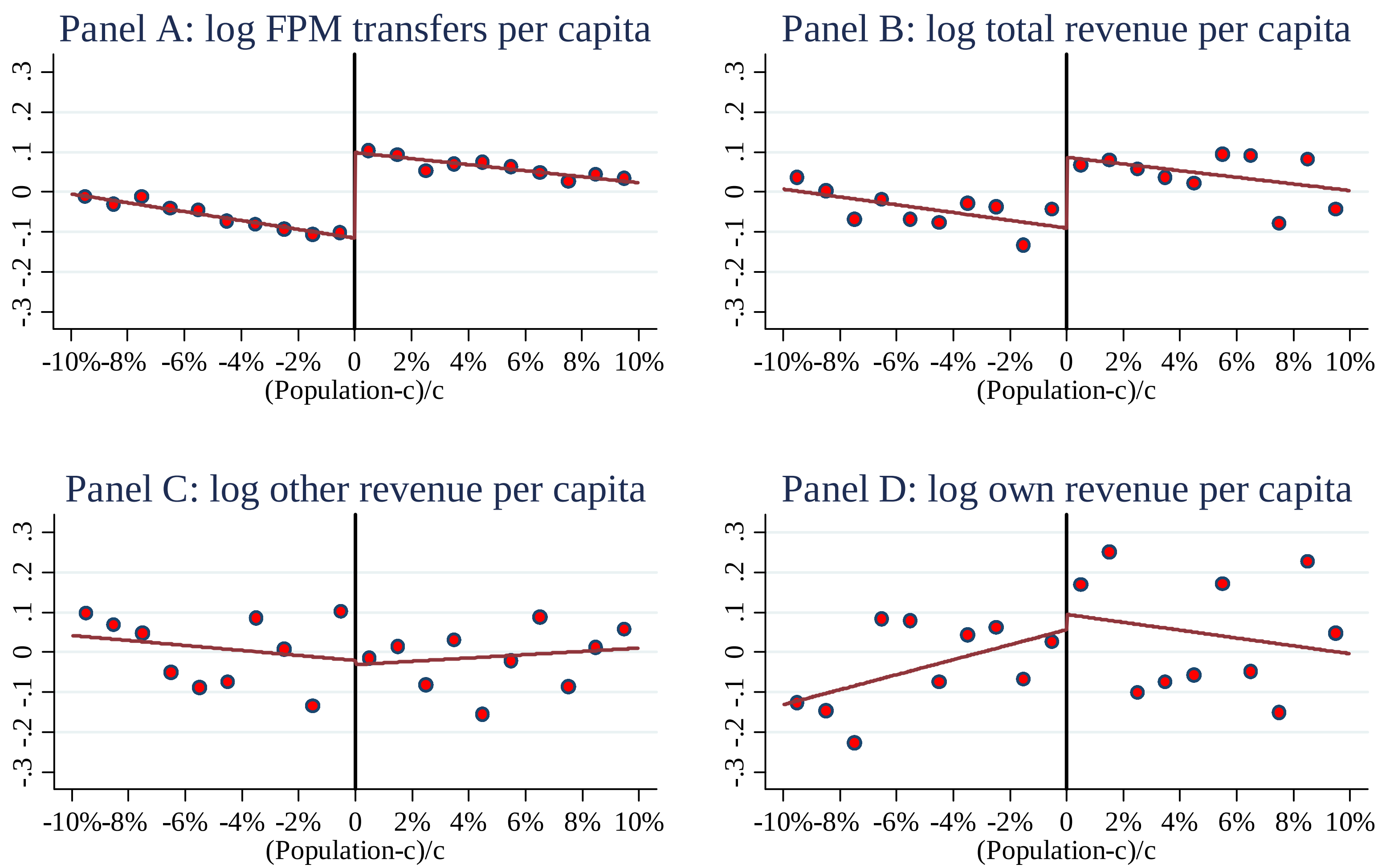


Figure 3: Histogram for 1982 official population, small to medium municipalities



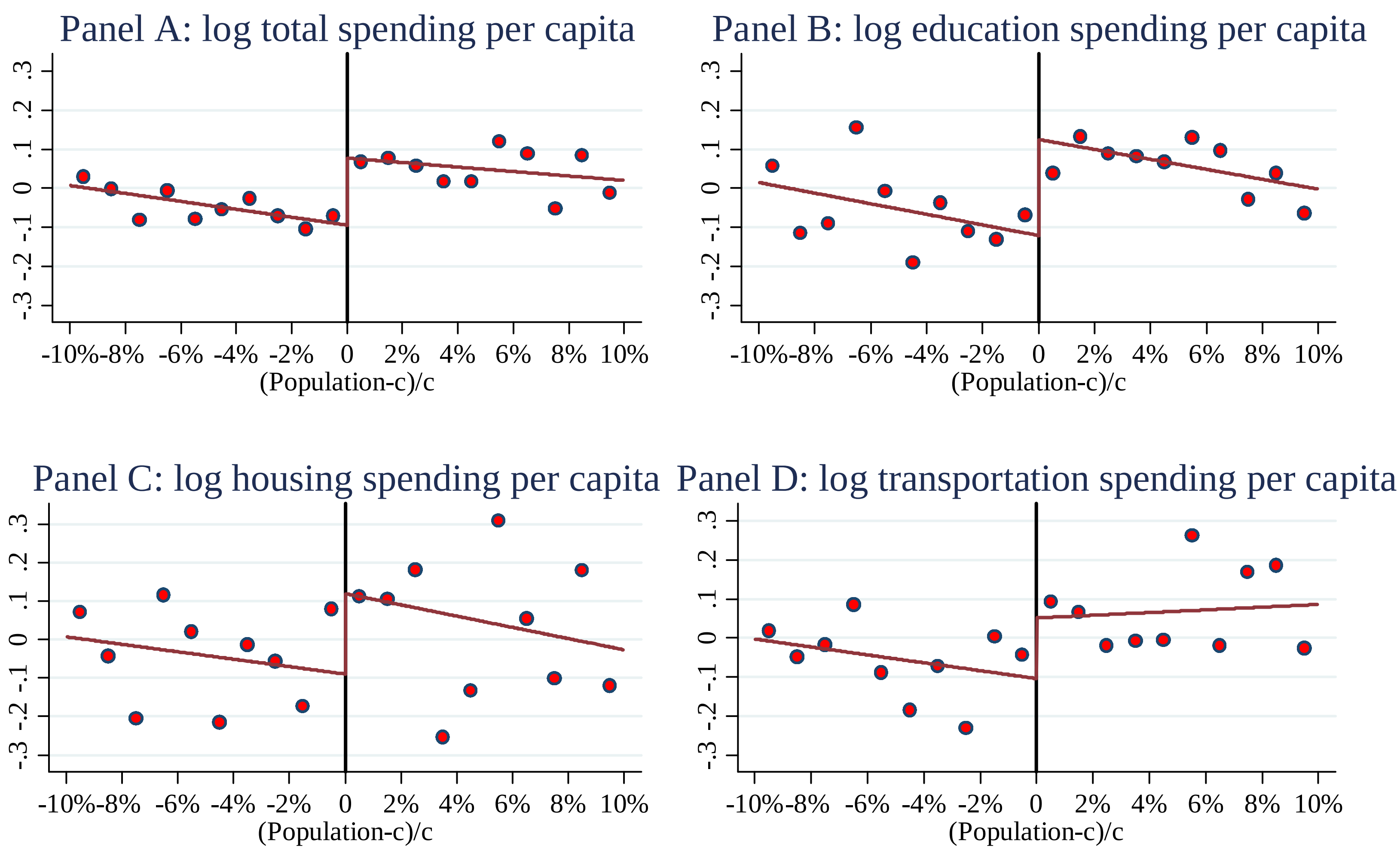
The bin-width is 283. 1982 official population is based on the 1980 census conducted by the national statistical agency, IBGE. McCrary density test fails to reject the null of no discontinuity in the density at conventional levels of significance for the first six cutoffs.

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



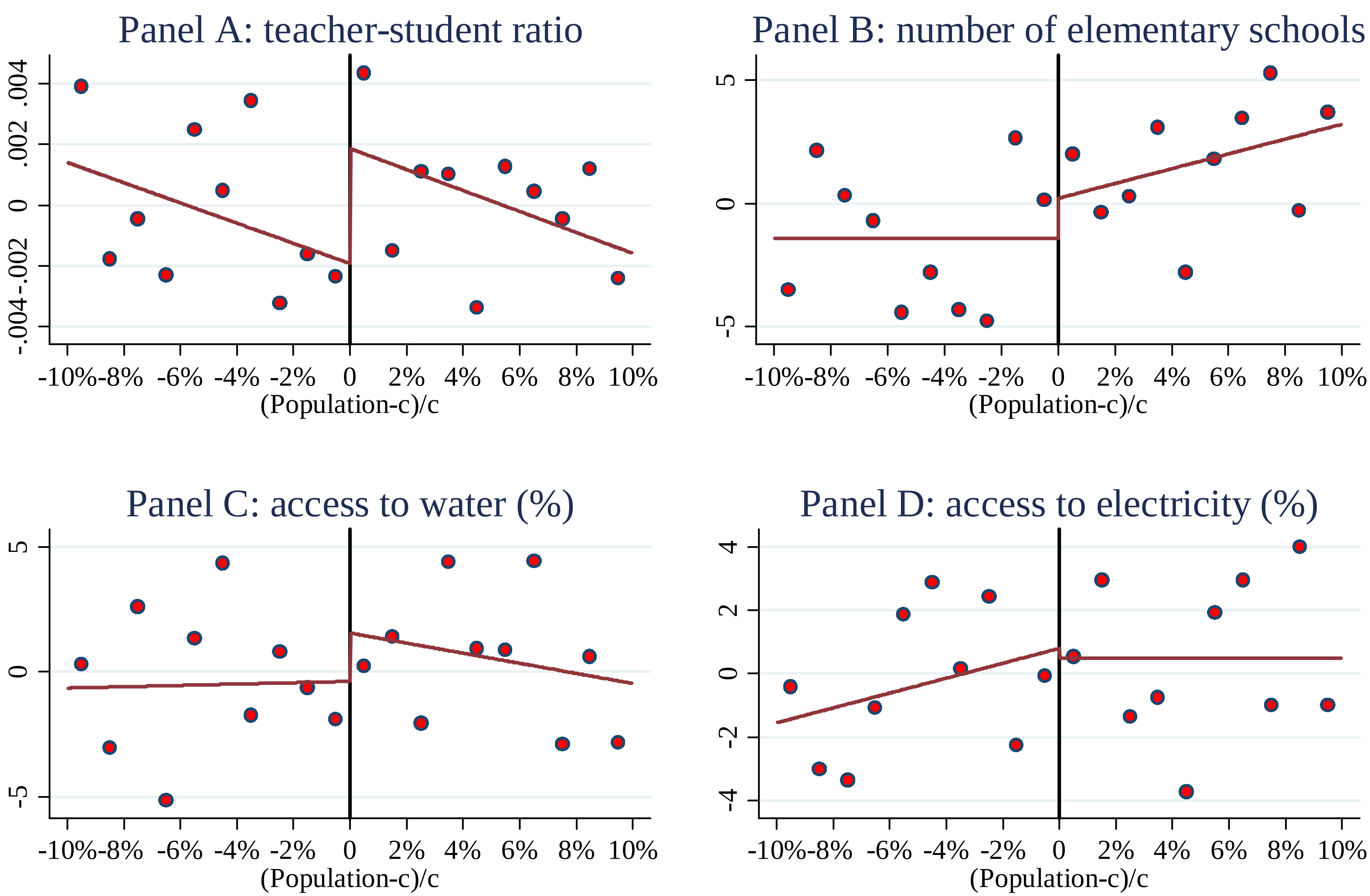
All variables are summed over the period 1982-1985 and scaled by 1980 municipality census population. Each dot represents the sample average of the (partialled out) dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 5: Effects on total spending and main spending categories



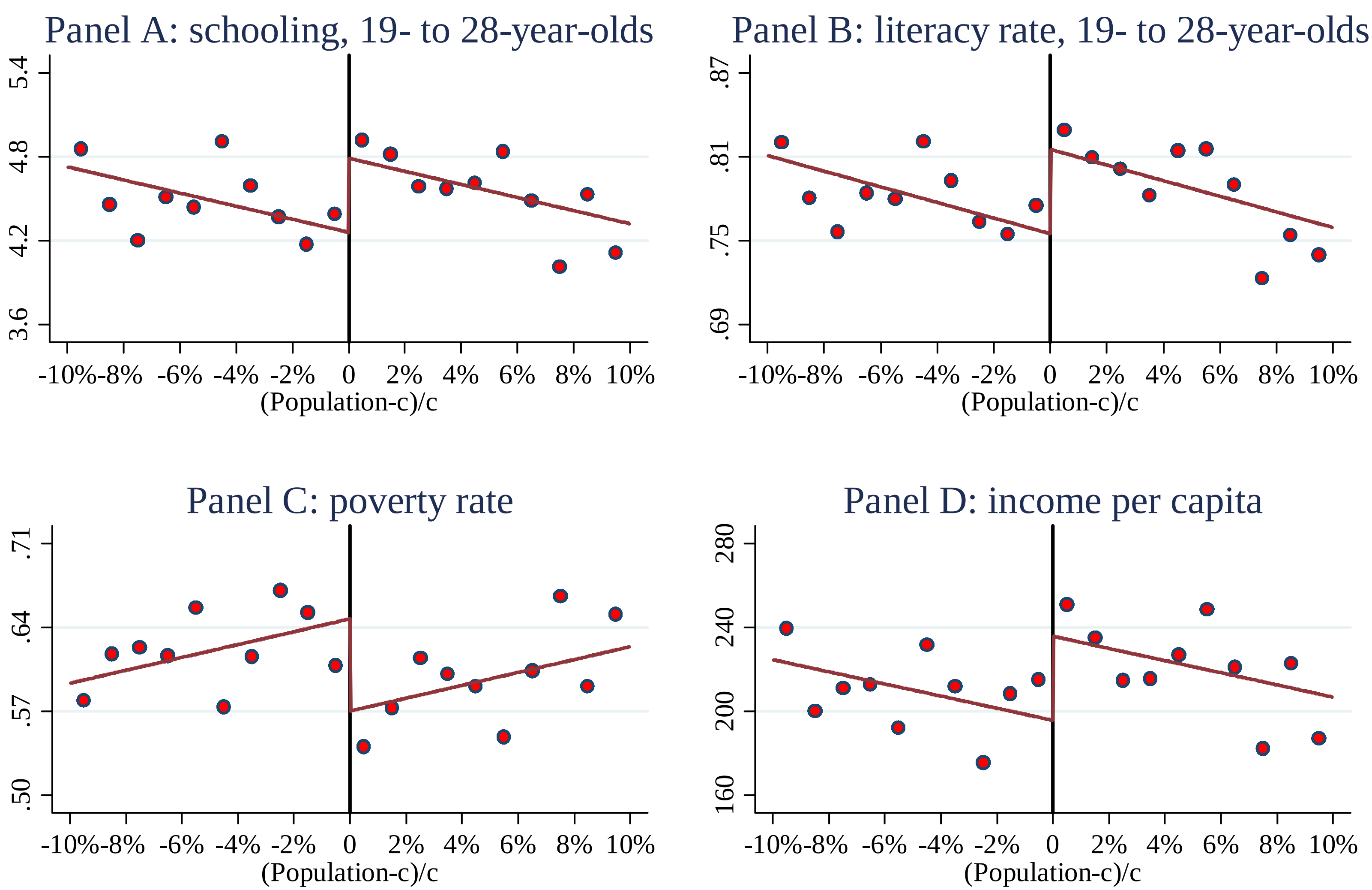
Total spending is summed over the period 1982-1985. The spending categories are summed over the period 1982-1983. All variables are scaled by 1980 municipality census population. Each dot represents the sample average of the (partialled out) dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 6: Effects on direct public service measures



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 7: Effects on schooling, literacy and household income



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