The Long-run and Intergenerational Education Impacts of Intergovernmental Transfers*

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Abstract

This paper provides regression discontinuity evidence on long-run and intergenerational education impacts of a temporary increase in federal transfers to local governments in Brazil. Revenues and expenditures of the communities benefiting from extra transfers temporarily increased by about 20% during the 4 year period from 1982 to the end of 1985. Schooling and literacy gains for directly exposed cohorts established in previous work that used the 1991 census are attenuated but persist in the 2000 and 2010 censuses. Children and adolescents of the next generation—born after the extra funding had disappeared—show gains of about 0.08 standard deviation across the entire score distribution of two nationwide exams at the end of the 2000s. While we find no evidence of persistent improvements in school resources, we document discontinuities in education levels, literacy rates and incomes of test takers' parents that are consistent with intergenerational human capital spillovers.

Keywords: intergovernmental grants, human capital, test scores, regression discontinuity

JEL: H40, H72, I21, O15

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1 Introduction

It is well established that additional school resources can have positive impacts on students' educational attainment and achievement in both developed (Hanushek 2006) and developing countries (Glewwe and Kremer 2006). Moreover, for the U.S. (Oreopoulos, Page and Huff Stevens 2006), Norway (Black, Devereux and Salvanes 2005) and Sweden (Holmlund, Lindahl and Plug 2011) there is evidence that compulsory schooling reform—typically accompanied by higher educational resources—increases schooling not only in the present generation but also in the next generation, even if the estimated intergenerational spillover effects are small in magnitude.[1] However, none of these studies investigate intergenerational spillovers on children's cognitive skills for a given level of schooling. We are also not aware of any study that looks at the persistence of attainment or achievement gains and their intergenerational transmission in a developing country context, where public service levels are typically lower, inequality higher, and intergenerational spillovers therefore likely larger than in developed countries.

This paper provides evidence on long-run and intergenerational education impacts of a temporary increase in intergovernmental transfers in Brazil. We use the same regression discontinuity design as Litschig and Morrison (2013), which exploits that a substantial part of national tax revenue in the 1980s was redistributed to local governments only on the basis of population, via a formula based on cutoffs. Litschig and Morrison show that for relatively small communities the extra funding at the cutoffs translated into public spending increases on education, transportation, and housing and urban infrastructure of about 20 percent during the four-year-period from 1982 to the end of 1985. They also show that this public spending increase generated significant improvements in completed grades and literacy rates of school age cohorts, measured in the census of 1991. The present paper examines whether these education gains persisted in 2000 and 2010, and whether the temporary funding boost had long-term consequences for the educational achievement of the next generation, defined here as those born in 1990 or later. Since the funding discontinuities between treatment and comparison groups disappeared in 1986 and did not reappear, we are able to examine long-run and intergenerational impacts of a truly temporary funding shock.

[1] Holmlund, Lindahl and Plug (2011) reach the same conclusion based on a comprehensive review of twin- adoption- and IV- studies of intergenerational schooling effects.

Our first question is whether the education gains of cohorts directly exposed to higher federal transfers in the early 1980s were durable or instead faded with time, either because completed grades and literacy in the comparison cohorts caught up or because literacy in the treatment cohorts subsequently deteriorated. We use census data to show that the schooling and literacy gains for school age cohorts during the boost period previously established for 1991 are attenuated but persist in 2000 and even in 2010. The results hold for cohorts who had largely completed their schooling by 1991 and also for those who were still of schooling age in 1991 (at ages 9 to 18) and for whom gains could have easily faded with time.

Our second question is whether there are gains in cognitive skills of the next generation for a given level of schooling. We explore data from two nationwide standardized tests that were administered in the late 2000s, more than 20 years after the extra funding had stopped. The Prova Brasil measures mathematics and Portuguese language proficiency for students in 5th and 9th grade (that is, approximately at the end of primary- and middle-school in the U.S. education system). The target population for Prova Brasil are public schools with a minimum of 20 students per grade, and coverage is nearly universal.[2] The ENEM (*Exame Nacional do Ensino Médio*) measures general proficiency for students in the process of completing or having completed 12th grade (the equivalent of high school graduates in the US education system), and participation is voluntary.

We find that 9th graders and high school graduating cohorts—who attended school during the early 1990s and the decade of the 2000s—show gains of about 0.08 standard deviation across the entire test score distribution at the end of the 2000s. In contrast, 5th graders—who started school in the mid-2000s—do not show any test score gains at the end of the 2000s. We argue that this difference in results across cohorts might be due to improved public service provision over time—which likely reduces the importance of parental education for their children's academic performance—or increased importance of parental education at higher grades, as further discussed below.

We then perform a forensic analysis to investigate possible channels for the gains in cognitive skills of older children and adolescents at the end of the 2000s. A first mechanism we examine is stickiness in public service improvements. Existing studies find little or no evidence of improved

[2]In our sample, net enrollment rates in primary and middle school are about 94% in 2010.

public service delivery as a result of higher public spending either from fiscal transfers (Litschig and Morrison 2013) or oil revenue windfalls (Monteiro and Ferraz 2010; Caselli and Michaels 2013). However, 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. We focus on education inputs and draw information from established and new sources, dating from the mid-1990s to the late 2000s. From the Brazilian census of schools (*Censo Escolar*), we obtain information on school inputs such as existence of libraries, IT and science labs, and access to internet in municipal schools, as well as the teacher-student ratio and teacher education measures. From the teachers' and school directors' questionnaires from Prova Brasil 2007, we obtain information about the perception of problems facing municipal school systems.[3] We find no evidence of discontinuities in any of these education input measures (results available on request).

To investigate the existence of intergenerational education spillovers, we rely on a socioeconomic questionnaire that was administered jointly with the ENEM and Prova Brasil tests, allowing us to measure parental education levels in the late 2000s. Our results are consistent with some role for parental education, as we find that students from communities that benefited from extra federal transfers in the early 1980s are more likely to have parents with some middle or high school and less likely to have parents with only a primary school education. The socioeconomic questionnaire for Prova Brasil also investigates parental literacy, and the ENEM survey asks about household income. Results are consistent with those found for parental education. In beneficiary communities, Prova Brasil 9th grade test-takers are more likely to say that their parents are able to read, and ENEM test-takers tend to report higher household income (results available in the online Appendix, Table 10).

Whether parental education alone might account for children's test score gains is difficult to tell because existing evidence on causal intergenerational education effects has focused almost exclusively on completed schooling of children, rather than their test performance while in school or thereafter, and is limited to developed countries (the U.S. and Scandinavia in particular), where average schooling levels are about twice what they are in Brazil (about 12 vs. 6 years). The one study we are aware of finds that an additional year of maternal schooling increases math

[3]Teachers and school directors were asked whether their schools faced severe insufficiency of resources, insufficient teaching supplies, lack of teachers and disciplinary problems among students.

and reading test scores of 7-8 and 12-14 year-old children in the U.S. by about 0.1 of a standard deviation (Carneiro, Meghir and Parey 2013). Assuming that about one fifth of the children in our sample have a parent with an additional year of education (consistent with our results for directly exposed cohorts) and an effect size of parental schooling on child test performance twice as large as in the U.S. (due to the higher importance of parental schooling in areas of lower public service provision), we would expect a $0.20 \times 0.20 = 0.04$ standard deviation increase when we look at all test takers, short of the 0.08 impact we find in this study.

A third potential mechanism for the test score gains we find is higher income in the beneficiary communities. As additional federal funding led to increased public spending not only on education but also on transportation and housing and urban infrastructure, impacts on test score performance in the late 2000s may arise through channels other than parental schooling of directly exposed cohorts, such as higher incomes of those parents whose schooling levels were not affected by the funding boost. Indeed, Litschig and Morrison (2013) calculate that only about 2 percentage points of the 4 percentage point poverty reduction they find in 1991 is plausibly accounted for by the education channel alone, leaving the remaining 2 percentage points to improved public service provision overall. Our own analysis suggests that the poverty reduction of about 4 percentage points persists in 2000 and attenuated even in 2010 (results available in the online Appendix, Table 11). Our investigation of causal mechanisms therefore indicates that parental schooling played an important but not exclusive role in raising children's cognitive skills.[4]

The internal validity of our research design is assessed in Litschig and Morrison (2013, Section V). We reproduce the summary of their tests and robustness checks here for convenience. First, there is no evidence of manipulation of the 1980 census municipality population figures, which constitute the running variable in our RD design. Second, Litschig and Morrison verify whether municipalities in the marginal (to the cutoff) treatment and comparison groups were ex ante comparable by testing for discontinuities in pretreatment covariates such as whether the municipality was aligned with the central government in 1982, municipality own and total revenues, income per capita, poverty, urbanization, elementary school enrollment, schooling, literacy, and infant mor-

[4] An additional channel we explore is fertility. We find that in the census of 1991 the average number of live births among women 15 years old and above falls by about 0.15. The fertility reduction is attenuated but persists in the censuses of 2000 and 2010.

tality. 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, Litschig and Morrison show that all results are robust to both the inclusion of pretreatment covariates (including pretreatment education and earnings outcomes) and to the choice of bandwidth and functional form.

Further robustness checks in their online Appendix, Section 3, show that the schooling and literacy gains of directly exposed cohorts are robust to using the difference in outcomes over time, rather than the 1991 levels. In contrast, the corresponding difference estimates for cohorts that had largely completed their education when the extra funding started in 1982—and for whom one would expect smaller or no impacts—are close to zero in magnitude and very far from statistical significance. They also find almost identical results when the sample is restricted to individuals who were born in the municipality and never moved away, which suggests that the schooling and literacy gains were not driven by selective migration. Finally, Litschig and Morrison test and reject the joint null hypotheses of no discontinuities in any of the outcome variables they consider, suggesting that at least some of the impacts are real.

In the online Appendix for this paper we show key robustness checks for the new impacts presented here: education gains of directly exposed cohorts in 2000 and 2010 for non-migrants (Tables 2.1 and 3.1, respectively) as well as test score gains of ENEM and PB test-takers using nonlinear functional form specifications (Tables 4.2 and 5.2, respectively). An additional robustness check we perform in the online Appendix accounts for the fact that some municipalities lost territory and population to newly-created municipalities over the subsequent three decades by aggregating individual-level PB and ENEM test scores and survey responses to 1980 municipality borders whenever possible (Tables 4.1 to 9.1). Results of these robustness checks are quantitatively very similar to those shown in the paper.

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 discusses our identifying assumptions. Section 4 describes the data. Section 5 discusses the estimation approach. Section 6 presents estimation results. Section 7 concludes.

2 Background

2.1 Local public services and their financing

Local government responsibilities at the beginning of the 1980s were mostly to provide elementary education, housing, and urban infrastructure, as well as local transportation services. The responsibility for delivery of elementary education was shared with state governments, while the federal government was primarily involved in financing and standard setting. In 1980, 55 percent of all elementary school students in Brazil were enrolled in state administered schools, 31 percent in municipality schools, and the remaining 14 percent in private schools. In small and rural municipalities, such as those considered here, the proportion of students in schools managed by local governments was 74 percent, while the proportions for state-run and private schools were 24 percent and 2 percent respectively (World Bank 1985).

In the 1980s local governments managed about 17 percent of public resources in Brazil (Shah 1991), about four percent of GDP, with 20 percent of local budgets going to education and similar shares to housing and urban infrastructure, and transportation spending, as shown in Litschig and Morrison (2013), Table 1. Most of these resources accrued to the local governments through intergovernmental transfers, since municipalities have never collected much in the way of taxes. The most important among these transfers was the federal Fundo de Participação dos Municípios (FPM), a largely unconditional revenue sharing grant funded by federal income and industrial products taxes.[5] FPM transfers were the most important source of revenue for the relatively small local governments considered here, amounting to about 50 percent on average and 56 percent in rural areas.

2.2 Mechanics of revenue sharing in Brazil

In order to estimate the impact of intergovernmental transfers on outcomes, we exploit variation in FPM funding at several population cutoffs using regression discontinuity analysis. The critical feature of the FPM revenue-sharing mechanism for the purposes of our analysis is Decree

[5]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.

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:

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

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.[6] 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.[7] Likewise, from 1986 to 1988, the transfers were also based on such extrapolations, this time based on 1970 and 1980 census population figures. Beginning in 1989, these extrapolations were updated on a yearly basis, which is still the practice today. As a result of the update in 1986, the funding discontinuities for those municipalities around the cutoffs based on the 1980 census disappeared because many municipalities changed brackets due to decreases or, more often, increases in their population relative to 1980.[8] 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.

Figure 1 plots cumulative FPM transfers over the period 1982 to 1985 against 1982 official population. The horizontal lines correspond to the modal levels of cumulative transfers for each bracket in our data. The figure shows that funding jumps by about 1'320'000 Reais (2008 prices)

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

[7] 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).

[8] 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.

or about 1’000’000 international US$ at each threshold over this period.[9] 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.[10] The cumulative transfer differential over the period 1982-1985 corresponds to about 2.5 percent of annual GDP in rural areas of the country and about 1.4 percent of annual GDP in urban areas for the counties in our estimation sample. Figure 2 shows the distribution of municipalities within 5 percentage points of one of the first three cutoffs, 10’188, 13’584, and 16’980, across space.

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

3 Identification

The key identifying assumption for this study is that unobservables vary smoothly as a function of population (if at all) and, in particular, do not jump at the cutoffs. As shown in Lee (2008) and Lee and Lemieux (2010), sufficient for the continuity of unobservables is the assumption that individual densities of the treatment-determining variable are smooth. In our case, this assumption

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

[10]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.

explicitly allows for mayors or other agents in the municipality to have some control over their particular value of population. As long as this control is imprecise, treatment assignment is randomized around the cutoff. In our case, the continuity of individual population density functions also directly ensures that treatment status (extra transfers) is randomized close to the cutoff. (An additional concern would be imperfect compliance with the treatment rule, but in our study period all eligible municipalities received more FPM transfers, and none of the ineligible ones did.)

How reasonable is the continuity assumption in our context? As discussed in more detail in Litschig and Morrison (2013), the key identifying assumption is likely to hold here because mayors did not know the exact locations of the thresholds until after the release of the 1980 census results. Another potential concern is that other government policies are also related to the cutoffs specified in Decree 1881/81. If so, we would identify the combined causal effect of extra funding and other policies. To our knowledge, however, there are no other programs that used the same cutoffs in the early 1980s.

4 Data

4.1 Official population, FPM transfers, and covariates

Our analysis draws on multiple data sources from several time periods. Population estimates determining transfer amounts over the period 1982-1988 were taken from successive reports issued by the Federal Court of Accounts. Data on FPM transfers are self-reported by county officials and compiled into reports by the Secretariat of Economics and Finance inside the federal Ministry of Finance. The data from these reports were entered into spreadsheets using independent double-entry processing. Data on FPM transfers 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.

We include as pre-treatment covariates the 1980 levels of municipality income per capita, average years of schooling for individuals 25 years and older, the poverty headcount ratio, the illiterate percentage of people over 14 years old, the infant mortality rate, the school enrollment rate of 7- to 14-year-olds, and the percent of the municipal population living in urban areas. Data on these

1980 municipality characteristics are based on the 25 percent sample of the census and have been calculated by the national statistical agency (only a shorter census survey was administered to 100 percent of the population).

4.2 Schooling and literacy of directly exposed cohorts

For education outcomes of those directly affected by the increase in federal transfers, we use the microdata from the 1991, 2000 and 2010 population censuses to compute municipal-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 in 1991 , 28-37 in 2000 and 38-47 in 2010. This was the cohort most likely affected by the public spending increase from 1982 to 1985, since the 19-year-olds in 1991 were about 10 years old in 1982 and hence in the middle of elementary schooling age (7-14), while the 29-year-olds were at least 19 years old (age 20 on September 1st 1982 but 19 at some point during the year 1982 for some) and hence ineligible to attend regular elementary school, which has a cutoff age at 18.

We also compute average years of schooling and the literacy rate for the cohort that was 9- to 18-years-old in 1991, 18-27 in 2000 and 28-37 in 2010 (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 (at least in absolute terms) because most of them were not of elementary schooling age when spending increased in 1982. Moreover, most of this cohort had not completed elementary school in 1991 and so part of the impact on their level of schooling might be missed if the spending increase produced school supply improvements that had not faded completely by 1991. The 19- to 28-year-olds in contrast likely completed primary and even secondary education by 1991.

4.3 Test scores of the next generation

For education outcomes of the next generation, we rely on two standardized nationwide tests, Prova Brasil and ENEM (*Exame Nacional do Ensino Médio*). Prova Brasil is a universal test taken by students at the end of the basic cycles of fundamental education, that is, at the end of 5th and 9th grade. Student performance is measured in two disciplines: Portuguese language (reading) and mathematics (problem solving), with coverage of essentially all public schools with more than 20 students per grade. Prova Brasil microdata are available for 2007, 2009 and 2011, each one with more than 2 million observations per grade. We pool the scores for those three years for a given grade and compute the mean, median, 10th, 25th, 75th, and 90th percentile of the individual-level test score distribution for each municipality.[11]

ENEM is an annual exam designed for those students concluding high school or those who already concluded it. Its original stated goal, up to 2008, was to provide a reference for self-evaluation of the student's capabilities, and it was used as an input to the selection process of a few universities. From 2009 onwards, ENEM gained in importance as it became the unified entrance exam for the federal universities system. In our analysis, we select only test takers graduating from high school the year the test was taken. We pool together five test years, from 2007 to 2011, and again compute the mean, median, 10th, 25th, 75th, and 90th percentile of the individual-level test score distribution for each municipality.

In order to compare Prova Brasil and ENEM across different years, we calculate z-scores year by year on the full sample of test takers. When we do not have a headline test score (for instance, in Prova Brasil we have test scores for language and mathematics), we calculate z-scores for each one of the disciplines, add them up, and calculate z-scores for the total score.

4.4 Parental education of ENEM and Prova Brasil test takers

To investigate the existence of intergenerational education spillovers we rely on a socioeconomic questionnaire that was administered jointly with the ENEM and Prova Brasil tests, allowing us to measure parental education levels in the late 2000s. We aggregate responses into four categories, depending on the highest education level reached by the most educated parent. For Prova Brasil the categories are: "no more than primary school" (completed grades 0 through 4, including respondents who did not know the education level of their parents), "some middle school" (completed grades 5 through 7), "some high school" (completed grades 8 through 11), and "completed at least high school" (completed grades 12 or above). For ENEM the first two categories are the same, but due to differences in potential survey responses the "some high school" category includes high

[11] Prova Brasil 2007 was applied to children at the 4th and 8th grades and the sample covered only urban schools.

school graduates (completed grades 8 through 12) while the highest category is "some college" (completed grades 13 and above). For Prova Brasil there are sometimes substantial numbers of test-takers who did not fill out the socioeconomic survey. We verify that the nonresponse share exhibits no jump at the cutoff and disregard these individuals in the computation of parental education levels.

4.5 School quality measures

We also look into some measures of local education inputs. From the teacher's and school director's questionnaires from Prova Brasil 2007, we draw responses to questions about the incidence and severity of four different classes of problems: lack of financial resources, lack of teachers, lack of teaching supplies and disciplinary issues, and calculate averages by municipality. From Censo Escolar 1996 through 2008—which respectively cover the academic years 1995 through 2007—we draw the teacher-student ratio and the proportions of teachers in grades 1-4 with some college and those with some high school education for each municipality, aggregated at both the municipal school system and in general (i.e. including federal, state and private schools). From Censo Escolar 2008, we draw information on the proportion of schools with internet access, IT and science labs and libraries for each municipality, at both the municipal school system and in general.

5 Estimation approach

Following Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2008), our main estimation approach is to use local linear regression in samples around the discontinuity, which amounts to running simple linear regressions allowing for different slopes of the regression function in the neighborhood of the cutoff. Allowing for slope is particularly important in the present application because per capita transfers are declining as population approaches the threshold from below, and again declining after the threshold. Assuming that a similar pattern characterizes outcomes as a function of population, a simple comparison of means for counties above and below the cutoff would provide downward biased estimates of the treatment effect. We follow the suggestions by Imbens and Lemieux (2008) and use a rectangular kernel (i.e. equal weight for all observations in the estimation sample).

In the analysis that follows, we focus particularly on the first three population cutoffs (c_1 = 10'188, c_2 = 13'584, and c_3 = 16'980). At subsequent cutoffs the variation in FPM transfers is too small to affect municipal overall budgets, and hence there is no "first stage" in terms of overall resources available for the municipality (see Section VI in Litschig and Morrison, 2013). For our pooled analysis, we need to make observations comparable in terms of the distance from their respective cutoff. To do this, we rescale population to equal zero at the respective thresholds within each of the first three segments, and then use the scaled variable, X_{ms} (municipality m in state s), for estimation purposes:

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

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

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 $\lim_{\Delta \downarrow 0} E[Y|X = \Delta] - E[Y|X = 0] = \tau$. We use successively larger neighborhoods (larger p) around the cutoff in order to assess the robustness of the results.

6 Estimation results

This section starts out by demonstrating that the schooling and literacy gains for school age cohorts during the early 1980s established for the year 1991 in Litschig and Morrison (2013) are attenuated but persist in 2000 and 2010. In the second subsection we document achievement gains of about 0.08 standard deviation across the entire score distribution for ENEM 2007-2011 test-takers from high school graduating cohorts. These cohorts attended school during the early 1990s and the 2000s. The third subsection shows achievement gains of about 0.08 standard deviation, again across the entire test score distribution but for Prova Brasil 8th or 9th grade test-takers in 2007, 2008, or 2011, who attended school during the early 2000s. In the fourth subsection we show that there are no impacts on test scores for Prova Brasil 4th or 5th graders, who started school in the mid 2000s. The last subsection provides evidence on intergenerational transmission of human capital by showing discontinuities in the education levels of ENEM and Prova Brasil test-takers' parents.

All the tables below show results for the first three cutoffs pooled and for successively larger samples around the cutoffs ($p = 2, 3, 4$, and 5 percent), for each sample with and without covariates. Those estimates that control for covariates are the most reliable because they control for chance correlations with treatment status. They are also typically the most precisely estimated, because the covariates absorb some of the variation in the outcome measures.

6.1 Impacts on schooling and literacy for two directly exposed cohorts in 1991, 2000 and 2010

Table 2 shows the results for years of schooling (completed grades) for individuals 9 to 18 and 19 to 28 years of age in 1991, and for the same two cohorts in 2000 and 2010. The point estimates suggest that the older cohort (19-28 in 1991) accumulated about 0.3 additional years of schooling per capita by 1991. Reduced schooling gains of about 0.2 years and 0.1 years persist in 2000 and even in 2010, respectively. The initial (1991) schooling gains would be consistent with 3 out of 10 individuals from this cohort completing an additional year of schooling for example. While the estimates for 1991 are statistically significant (at 1 percent) even within a relatively small neighborhood of +/- 3 percent around the cutoffs, those for 2000 and 2010 are less precisely estimated.

Estimates for the younger cohort of 9- to 18-year-olds in 1991 (0-9 in 1982) shown in Table 2 suggest a schooling gain of about 0.15 years per capita. For this younger cohort, these gains also persist and tend to be even slightly larger in 2000 and 2010 compared to 1991, despite the fact that average schooling in marginal comparison municipalities more than doubled between 1991 and 2000 (from about 2.6 years of schooling on average in 1991 to about 5.8 in 2000 as shown in Table 2). The estimates for 1991 are again highly significant, while those for 2000 and 2010 are again less precisely estimated. A likely distribution of individual-level gains that would lead to the average impacts in 1991 is that 15 out of 100 individuals in the younger cohort completed another year of schooling. Given the shares of the older and younger cohorts in the total population—23 percent and 27 percent, respectively, according to de Carvalho (1997)—we can thus estimate what percent of the overall population got an additional year of schooling by 1991, namely about $23\% \times 30\% + 27\% \times 15\% = 11\%$.

Figure 3 presents graphical evidence of the discontinuities in schooling for the two cohorts in 1991 and 2000 (2010 results available on request). Each dot represents average years of schooling for a given cohort, year, and bin. There are about 50 municipalities per bin. To demonstrate the correspondence between panel A of Figure 3 and the results in Table 2, 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 2 in the two percent neighborhood without covariates. With this in mind, the figure shows clear evidence of discontinuities in schooling at the cutoff, and it additionally shows that the discontinuities are visually robust irrespective of the width of the neighborhood examined.

Table 3 shows that students not only completed more grades in municipalities that received extra funds but that for some of them it made the difference between being able to read and write or not. For the older cohort the effect on literacy amounts to about 4 percentage points in 1991, compared to an average literacy rate of about 76 percent in the comparison group. The literacy gains in 2000 and 2010 are reduced to about 2 percentage points. For 1991 all estimates are highly significant (at 1 percent) and they remain significant at 5% even in 2000 and 2010. For the younger cohort, the literacy gain in 1991 is about 3 percentage points compared to an average literacy rate of about 74 percent in the comparison group. This gain is reduced to about 1.5 percentage points in 2000 and 2010 and statistical significance is mostly at 5 percent throughout. Figure 4 shows the literacy gains for both cohorts in 1991 and 2010 graphically (2010 results available on request). In line with this graphical evidence, discontinuity estimates for neighborhoods not shown in Tables 2 and 3 are quantitatively similar to the estimates presented here and are available upon request.

6.2 Impact on ENEM test scores, high school graduating cohorts

Table 4 gives estimates of the jump in the municipality-level mean, 10th, 25th, 50th, 75th, and 90th percentiles of standardized ENEM test scores for high school graduating cohorts from 2007 to 2011. Overall, we find gains across the entire test score distribution of about 0.08 standard deviation. Statistical significance is reached at 5 percent for all test score statistics and even at 1 percent for the 10th percentile estimates. Figure 5 shows clear evidence of discontinuities in ENEM test score statistics at the cutoff, and it additionally shows that the discontinuities are visually robust irrespective of the width of the neighborhood examined. It is also clear that the ENEM test score statistics are decreasing as population approaches the cutoff point from below, and again declining after the threshold. Figures for the 25th and 75th percentiles are similar and available on request.

6.3 Impact on Prova Brasil test scores, 8th or 9th graders

Table 5 gives estimates of the jump in the municipality-level mean, 10th, 25th, 50th, 75th, and 90th percentiles of standardized Prova Brazil test scores for students in 8th or 9th grade in 2007, 2009, and 2011 for municipalities that benefited from extra per capita FPM transfers during the early 1980s. Overall, we again find gains across the test score distribution of about 0.08 standard deviation. Estimates for neighborhoods 3, 4 and 5 percent around the cutoffs are statistically significant at 10% or less in most specifications. Estimates for the 2 percent neighborhood are smaller and not statistically significant.

Figure 6 presents graphical evidence of the discontinuities in Prova Brasil mean, median, 10th and 90th percentile test score statistics for 8th or 9th graders. Figures for the 25th and 75th percentiles are similar and available on request. While it is clear that the test score statistics are decreasing as population approaches the cutoff point from below, and again declining after the threshold, it is not entirely clear based on the bin averages that there is a discontinuity—rather than a non-linearity—at the cutoff point. However, as is evident from the cubic fits—shown as dashed lines in all the figures—there is indeed a discontinuity, although of smaller magnitude than the linear fits would indicate.

To investigate the possibility of nonlinearities—rather than discontinuities—in test score statistics further, we estimate equation 1 using a cubic polynomial specification of the running variable for ENEM and PB 8th/9th grade test scores as further robustness checks (available in the online Appendix, Tables 4.2 and 5.2, respectively). These estimates fall in the same range as those in Tables 4 and 5 although they are more variable. Most of the nonlinear estimates are not statistically significant because standard errors increase substantially compared to the linear model (the standard error is sometimes twice the size of the corresponding linear specification). We use an F-test of the joint hypotheses that the coefficients on the quadratic and cubic population terms on either side of the cutoff are zero—that is, whether linearity of the population polynomial can be rejected. There is virtually no statistical evidence against the null hypothesis of a linear model. In addition to the *a priori* case for a linear specification based on the relationship between population and FPM transfers per capita, these statistical test results further corroborate our focus on the linear estimates and standard errors.

6.4 Impact on Prova Brasil test scores, 4th or 5th graders

Table 6 presents estimates of the jump in the municipality-level mean, 10th, 25th, 50th, 75th, and 90th percentiles of standardized Prova Brazil test scores for students in 4th or 5th grade in 2007, 2009, and 2011 for municipalities that benefited from extra per capita FPM transfers during the early 1980s. Overall, we find no evidence of gains anywhere in the test score distribution for these cohorts who started school in the mid-2000s. Figure 7 presents graphical evidence. Although the linear fits suggest jumps at the cutoff, it is clear from the bin averages and the cubic fits that discontinuities cannot be distinguished from nonlinearities in the distribution of Prova Brasil test score statistics for 4th or 5th graders. Again, figures for the 25th and 75th percentiles are similar and available on request.

To sum up the results for Prova Brasil, we find that 8th or 9th graders—who started school in the early 2000s—show gains of about 0.08 standard deviation across the entire score distribution at the end of the 2000s, while 4th or 5th graders—who started school in the mid-2000s—do not show any test score gains. There are two main explanations that could account for this difference in results. The first is that overall public service provision—and education service provision in particular—has improved over time, potentially reducing the impact of higher parental schooling levels on their children's academic performance. As an example, Figure 8 illustrates the increase in municipal teachers' qualifications between 1996 and 2008. While back in 1996 the shares of teachers with some college, some high school, and no more than middle school education (the omitted category) were about 0.10, 0.60, and 0.30, respectively, in 2008 about 60 percent had some college education and 40 percent some high school education. Figure 8 also shows that teacher qualifications show no discontinuities at the cutoff in either period, suggesting that higher teacher qualifications are not responsible for the test score gains of the older cohorts in the late 2000s.

A second explanation for the absence of test score gains for the younger cohorts is that higher parental education might matter more for their children's academic performance in middle or high school (grades 5 through 12) compared to primary school, perhaps because the test material becomes more difficult. Both explanations rely on parental schooling and are consistent with the persistent schooling and literacy gains of the previous generation—the cohorts directly exposed to higher central government funding in the early 1980s—shown above. In the next subsection we provide direct evidence on intergenerational transmission of human capital by focusing on the education levels of ENEM and Prova Brasil test-takers' parents.

6.5 Impact on parental education of ENEM high school graduating cohorts

Table 7 gives estimates of the jump in education levels of the parents of ENEM high school graduating cohorts from 2007 to 2011. The estimates suggest that the proportion of parents with no more than a 4th grade education decreased by about 3 percentage points, and that corresponding increases in parents' education are about equally distributed in the proportions with some middle school (up to grade 8) and some high school (up to grade 12). The proportion of parents with college education is no different between treatment and comparison communities. Statistical significance reaches 5 percent in the 3 percent and 4 percent samples and is weaker in the 2 percent and 5 percent samples. Figure 9 shows clear evidence of the reduction in the proportion of parents with no more than primary education and corresponding increases in proportions of parents with some middle or high school education.

In Table 8 we present estimates of discontinuities in education levels of the parents of Prova Brasil test-takers in 8th or 9th grade in 2007, 2009, and 2011. The estimates suggest again that the proportion of parents with no more than a 4th grade education decreased by about 3 percentage points. The corresponding increase in parents' education is observed mostly in the proportion of parents who completed at least high school. The parental education coding for Prova Brasil differs from the coding for ENEM because of differences in possible responses across the two socioeconomic surveys. Statistical significance reaches 1 percent in the 4 percent and 5 percent samples and is weaker in the 2 percent sample. Figure 10 shows clear evidence of the reduction in the proportion of parents with no more than primary education and a corresponding increase in the proportion of parents who completed at least high school.

Table 9 shows estimates of discontinuities in education levels of the parents of Prova Brasil test-takers in 4th or 5th grade in 2007, 2009, and 2011. These estimates also suggest that the proportion of parents with no more than a 4th grade education decreased by about 3 percentage points. The corresponding increase in parents' education is again observed mostly in the proportion of parents who completed at least high school. Statistical significance reaches 1 percent in the 4 percent and 5 percent samples for the discontinuity estimate in the lowest education category and is weaker for other parental education categories. Figure 11 shows clear evidence of the reduction in the proportion of parents with no more than primary education and a somewhat less clear increase in the proportion of parents who completed at least high school.

7 Conclusion

This paper builds on the findings by Litschig and Morrison (2013) showing that communities that received extra financing from the central government in Brazil in the early 1980s benefited in terms of education outcomes (completed grades and literacy) and poverty reduction, measured in 1991. We show that these education and income gains are attenuated but persist in the census data of 2000 and 2010. More strikingly, we uncover evidence of learning gains for the next generation, i.e. those who attended school during the early 1990s and the decade of the 2000s, some twenty years after the positive shock to federal funding had ended.

More specifically, we find that students from municipalities that benefited from extra federal transfers twenty years earlier show a 0.08 standard deviation gain across the entire score distribution of two nationwide exams (Prova Brasil and ENEM) at the end of the 2000s. Younger children who started school in the mid-2000s do not show any test score gains at the end of the 2000s. While we find no evidence of persistent public service improvements, we document discontinuities in education levels, literacy rates and incomes of test-takers' parents, consistent with intergenerational human capital spillovers.

To conclude, our results suggest that even in the absence of reforms that strengthen local accountability or top-down monitoring, and despite well founded worries about corruption, other leakages, and local capture, temporary transfers to local governments in Brazil had long-lasting impacts on education outcomes and incomes of directly affected cohorts. Perhaps even more important from a policy perspective is the finding that those impacts were not only persistent, but that education gains also spilled over to the next generation.

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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: Impact on years of schooling for two directly exposed cohorts in 1991, 2000 and 2010

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
Avg. schooling (19- to 28-year-olds in 1991)	4.26								
I[X > 0]		0.322	0.225	0.516***	0.301***	0.528***	0.275***	0.456***	0.187**
		(0.260)	(0.151)	(0.198)	(0.114)	(0.171)	(0.102)	(0.155)	(0.093)
R-squared		0.71	0.89	0.71	0.89	0.69	0.88	0.68	0.88
Avg. schooling (28- to 37-year-olds in 2000)	4.88								
I[X > 0]		0.143	0.062	0.381**	0.188	0.430***	0.193*	0.368**	0.119
		(0.225)	(0.163)	(0.182)	(0.128)	(0.161)	(0.116)	(0.149)	(0.103)
R-squared		0.73	0.86	0.70	0.86	0.69	0.86	0.68	0.87
Avg. schooling (38- to 47-year-olds in 2010)	6.10								
I[X > 0]		0.062	-0.041	0.283	0.062	0.345**	0.106	0.279*	0.033
		(0.227)	(0.177)	(0.189)	(0.142)	(0.163)	(0.121)	(0.150)	(0.106)
R-squared		0.64	0.81	0.60	0.80	0.60	0.81	0.59	0.81
Avg. schooling (9- to 18-year-olds in 1991)	2.61								
I[X > 0]		0.207	0.155	0.287**	0.166**	0.288***	0.136**	0.230**	0.079
		(0.157)	(0.095)	(0.117)	(0.071)	(0.099)	(0.062)	(0.090)	(0.055)
R-squared		0.84	0.94	0.83	0.93	0.81	0.93	0.81	0.92
Avg. schooling (18- to 27-year-olds in 2000)	5.78								
I[X > 0]		0.174	0.108	0.385**	0.200	0.436***	0.229*	0.304**	0.093
		(0.245)	(0.182)	(0.186)	(0.139)	(0.158)	(0.118)	(0.145)	(0.104)
R-squared		0.74	0.86	0.73	0.86	0.73	0.86	0.72	0.86
Avg. schooling (28- to 37-year-olds in 2010)	7.36	0.042	-0.038	0.287	0.096	0.410***	0.193	0.311**	0.093
I[X > 0]		(0.210)	(0.185)	(0.178)	(0.145)	(0.158)	(0.127)	(0.148)	(0.115)
		0.65	0.80	0.61	0.78	0.61	0.79	0.60	0.79
R-squared									
Observations		202	199	297	294	391	387	479	473

Notes: Census 1991, 2000 and 2010 samples. Census 1991 results from Litschig and Morrison (2013), Tables 5 and 6. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 3: Impact on literacy for two directly exposed cohorts in 1991, 2000 and 2010

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
Literacy rate (19- to 28-year-olds in 1991)	0.76								
I[X > 0]		0.057**	0.047***	0.062***	0.049***	0.059***	0.041***	0.051***	0.032***
		(0.027)	(0.016)	(0.019)	(0.012)	(0.016)	(0.011)	(0.014)	(0.010)
R-squared		0.78	0.91	0.80	0.91	0.80	0.91	0.80	0.91
Literacy rate (28- to 37-year-olds in 2000)	0.80								
I[X > 0]		0.023	0.013	0.041**	0.029**	0.039***	0.022**	0.033***	0.016*
		(0.022)	(0.014)	(0.016)	(0.011)	(0.014)	(0.010)	(0.012)	(0.009)
R-squared		0.78	0.91	0.79	0.90	0.79	0.90	0.78	0.90
Literacy rate (38- to 47-year-olds in 2010)	0.80								
I[X > 0]		0.023	0.015	0.032**	0.021**	0.034***	0.019**	0.028**	0.012
		(0.019)	(0.012)	(0.014)	(0.009)	(0.012)	(0.008)	(0.011)	(0.007)
R-squared		0.81	0.92	0.81	0.91	0.81	0.92	0.81	0.92
Literacy rate (9- to 18-year-olds in 1991)	0.73								
I[X > 0]		0.037	0.028	0.043**	0.027*	0.046***	0.024**	0.040***	0.019*
		(0.028)	(0.019)	(0.020)	(0.014)	(0.017)	(0.012)	(0.015)	(0.011)
R-squared		0.82	0.93	0.82	0.91	0.82	0.91	0.82	0.91
Literacy rate (18- to 27-year-olds in 2000)	0.88								
I[X > 0]		0.021	0.015	0.026**	0.018**	0.025**	0.014*	0.017**	0.007
		(0.016)	(0.012)	(0.011)	(0.009)	(0.010)	(0.008)	(0.009)	(0.007)
R-squared		0.76	0.88	0.76	0.86	0.76	0.86	0.76	0.86
Literacy rate (28- to 37-year-olds in 2010)	0.87								
I[X > 0]		0.012	0.004	0.023*	0.014	0.025**	0.013	0.021**	0.010
		(0.016)	(0.012)	(0.012)	(0.009)	(0.010)	(0.008)	(0.009)	(0.007)
R-squared		0.78	0.88	0.78	0.87	0.79	0.87	0.78	0.87
Observations		202	199	297	294	391	387	479	473

Notes: Census 1991, 2000 and 2010 samples. Census 1991 results from Litschig and Morrison (2013), Tables 7 and 7.2. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 4: Impact on the distribution of ENEM test scores, 2007-2011, high school graduating cohorts

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
Average test score	-0.47								
I[X > 0]		0.082	0.082*	0.108**	0.078**	0.075*	0.033	0.081**	0.035
		(0.057)	(0.045)	(0.045)	(0.036)	(0.041)	(0.033)	(0.036)	(0.029)
R-squared		0.76	0.84	0.75	0.83	0.73	0.83	0.73	0.83
10th percentile	-1.44								
I[X > 0]		0.094**	0.102***	0.089***	0.073***	0.062**	0.038	0.060**	0.031
		(0.042)	(0.034)	(0.033)	(0.027)	(0.029)	(0.025)	(0.026)	(0.022)
R-squared		0.73	0.81	0.73	0.80	0.69	0.78	0.69	0.78
25th percentile	-1.05								
I[X > 0]		0.080	0.081*	0.092**	0.070**	0.057	0.027	0.063**	0.029
		(0.052)	(0.041)	(0.040)	(0.033)	(0.036)	(0.029)	(0.031)	(0.026)
R-squared		0.74	0.82	0.74	0.81	0.72	0.81	0.72	0.80
Median test score	-0.55								
I[X > 0]		0.080	0.080	0.109**	0.080**	0.066	0.026	0.069*	0.024
		(0.061)	(0.050)	(0.049)	(0.040)	(0.043)	(0.036)	(0.038)	(0.032)
R-squared		0.75	0.83	0.75	0.82	0.72	0.81	0.72	0.82
75th percentile	0.04								
I[X > 0]		0.077	0.079	0.121**	0.085*	0.089*	0.036	0.098**	0.041
		(0.068)	(0.056)	(0.055)	(0.045)	(0.050)	(0.042)	(0.044)	(0.037)
R-squared		0.76	0.83	0.74	0.82	0.72	0.82	0.72	0.82
90th percentile	0.62								
I[X > 0]		0.098	0.088	0.128**	0.080	0.090	0.026	0.111**	0.041
		(0.080)	(0.066)	(0.064)	(0.052)	(0.058)	(0.048)	(0.051)	(0.042)
R-squared		0.73	0.81	0.72	0.80	0.70	0.81	0.70	0.80
Observations		202	199	297	294	391	387	479	473

Notes: ENEM 2007-2011 test-taker samples, high school (12th grade) graduating cohorts. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 5: Impact on the distribution of Prova Brasil test scores, 2007, 2009, and 2011, 8th or 9th graders

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
Average test score	-0.14								
I[X > 0]		0.032	0.048	0.105*	0.089*	0.113**	0.078*	0.120***	0.089**
		(0.068)	(0.063)	(0.056)	(0.050)	(0.048)	(0.043)	(0.043)	(0.039)
R-squared		0.72	0.78	0.72	0.77	0.70	0.76	0.70	0.76
10^{th} percentile	-1.28								
I[X > 0]		0.039	0.065	0.103*	0.090*	0.112**	0.084*	0.110**	0.085**
		(0.074)	(0.068)	(0.059)	(0.054)	(0.050)	(0.045)	(0.044)	(0.040)
R-squared		0.61	0.68	0.61	0.67	0.60	0.67	0.61	0.67
25^{th} percentile	-0.78								
I[X > 0]		0.060	0.086	0.136**	0.125**	0.140***	0.110**	0.136***	0.108***
		(0.074)	(0.069)	(0.060)	(0.055)	(0.052)	(0.047)	(0.046)	(0.042)
R-squared		0.67	0.73	0.68	0.74	0.66	0.72	0.66	0.72
Median test score	-0.17								
I[X > 0]		0.023	0.039	0.111*	0.093*	0.125**	0.086*	0.132***	0.096**
		(0.076)	(0.070)	(0.061)	(0.055)	(0.053)	(0.047)	(0.047)	(0.042)
R-squared		0.70	0.76	0.70	0.76	0.68	0.75	0.69	0.75
75^{th} percentile	0.47								
I[X > 0]		0.015	0.025	0.087	0.069	0.100*	0.060	0.113**	0.078*
		(0.074)	(0.067)	(0.059)	(0.053)	(0.052)	(0.047)	(0.046)	(0.041)
R-squared		0.73	0.79	0.73	0.79	0.72	0.78	0.72	0.78
90^{th} percentile	1.06								
I[X > 0]		0.011	0.013	0.092	0.070	0.090*	0.049	0.108**	0.074*
		(0.070)	(0.067)	(0.057)	(0.053)	(0.050)	(0.047)	(0.045)	(0.041)
R-squared		0.76	0.80	0.75	0.79	0.74	0.79	0.74	0.79
Observations		202	199	297	294	391	387	479	473

Notes: PB 2007 8^{th} grade, 2009 9^{th} grade, and 2011 9^{th} grade test-taker samples. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 6: Impact on the distribution of Prova Brasil test scores, 2007, 2009, and 2011, 4th or 5th graders

Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
	Comparison mean:								
Average test score	-0.11								
I[X > 0]		-0.023	-0.030	-0.003	-0.032	0.022	-0.018	0.034	-0.002
		(0.099)	(0.099)	(0.073)	(0.071)	(0.062)	(0.060)	(0.055)	(0.052)
R-squared		0.74	0.77	0.76	0.80	0.76	0.80	0.75	0.78
10^{th} percentile	-1.18								
I[X > 0]		0.027	0.025	0.041	0.021	0.055	0.028	0.048	0.022
		(0.085)	(0.088)	(0.063)	(0.064)	(0.053)	(0.053)	(0.047)	(0.046)
R-squared		0.62	0.66	0.65	0.69	0.63	0.68	0.62	0.67
25^{th} percentile	-0.74								
I[X > 0]		0.002	0.002	0.025	0.001	0.050	0.015	0.050	0.015
		(0.098)	(0.100)	(0.072)	(0.072)	(0.061)	(0.060)	(0.054)	(0.052)
R-squared		0.67	0.71	0.70	0.74	0.68	0.73	0.67	0.72
Median test score	-0.18								
I[X > 0]		-0.029	-0.035	-0.008	-0.038	0.021	-0.023	0.035	-0.005
		(0.108)	(0.109)	(0.080)	(0.078)	(0.067)	(0.065)	(0.059)	(0.057)
R-squared		0.72	0.76	0.75	0.78	0.74	0.79	0.74	0.77
75^{th} percentile	0.45								
I[X > 0]		-0.036	-0.046	-0.024	-0.058	0.002	-0.044	0.025	-0.016
		(0.113)	(0.111)	(0.085)	(0.082)	(0.072)	(0.069)	(0.063)	(0.060)
R-squared		0.75	0.79	0.78	0.81	0.78	0.81	0.77	0.80
90^{th} percentile	1.06								
I[X > 0]		-0.051	-0.067	-0.038	-0.071	-0.011	-0.055	0.019	-0.018
		(0.116)	(0.111)	(0.087)	(0.083)	(0.073)	(0.070)	(0.064)	(0.061)
R-squared		0.76	0.80	0.79	0.82	0.79	0.82	0.78	0.81
Observations		202	199	297	294	391	387	479	473

Notes: PB 2007 4^{th} grade, 2009 5^{th} grade, and 2011 5^{th} grade test-taker samples. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 7: Impact on parents' education levels, ENEM 2007-2011, high school graduating cohorts

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
No more than primary school	0.40								
I[X > 0]		-0.025	-0.027	-0.045**	-0.031*	-0.041**	-0.024	-0.030*	-0.011
		(0.026)	(0.021)	(0.022)	(0.017)	(0.019)	(0.015)	(0.017)	(0.013)
R-squared		0.61	0.77	0.55	0.75	0.52	0.75	0.53	0.74
Some middle school	0.19								
I[X > 0]		0.011	0.015	0.018*	0.019**	0.015*	0.012	0.012*	0.009
		(0.012)	(0.012)	(0.009)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
R-squared		0.51	0.59	0.51	0.58	0.45	0.53	0.46	0.53
Some high school	0.26								
I[X > 0]		0.013	0.015	0.023*	0.014	0.024*	0.015	0.016	0.007
		(0.016)	(0.014)	(0.013)	(0.011)	(0.013)	(0.011)	(0.011)	(0.009)
R-squared		0.42	0.60	0.37	0.59	0.33	0.56	0.33	0.55
Some college	0.15								
I[X > 0]		0.001	-0.002	0.004	-0.002	0.003	-0.003	0.002	-0.006
		(0.011)	(0.011)	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)
R-squared		0.61	0.70	0.54	0.62	0.53	0.62	0.52	0.62
Observations		202	199	297	294	391	387	479	473

Notes: ENEM 2007-2011 test-taker samples, high school (12^{th} grade) graduating cohorts. The four categories correspond to the highest education level of the most educated parent. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 8: Impact on parents' education levels, Prova Brasil 2007, 2009, and 2011, 8th or 9th graders

	Comparison mean:								
Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
No more than primary school	0.30								
I[X > 0]		-0.018	-0.022*	-0.030**	-0.024**	-0.040***	-0.030***	-0.028***	-0.016**
		(0.016)	(0.012)	(0.012)	(0.009)	(0.011)	(0.008)	(0.010)	(0.008)
R-squared		0.72	0.85	0.73	0.84	0.70	0.83	0.68	0.81
Some middle school	0.28								
I[X > 0]		-0.002	-0.003	-0.013	-0.008	-0.015	-0.011	-0.013	-0.008
		(0.015)	(0.015)	(0.011)	(0.011)	(0.010)	(0.010)	(0.009)	(0.009)
R-squared		0.45	0.51	0.39	0.49	0.33	0.46	0.33	0.45
Some high school	0.15								
I[X > 0]		0.005	0.010	0.009	0.008	0.011	0.007	0.009	0.004
		(0.010)	(0.009)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)
R-squared		0.53	0.69	0.49	0.65	0.46	0.62	0.44	0.62
Completed at least high school	0.27								
I[X > 0]		0.016	0.016	0.034**	0.024*	0.044***	0.033***	0.032***	0.020*
		(0.017)	(0.018)	(0.014)	(0.014)	(0.013)	(0.012)	(0.012)	(0.011)
R-squared		0.57	0.66	0.55	0.65	0.51	0.63	0.49	0.60
Observations		202	199	297	294	391	387	479	473

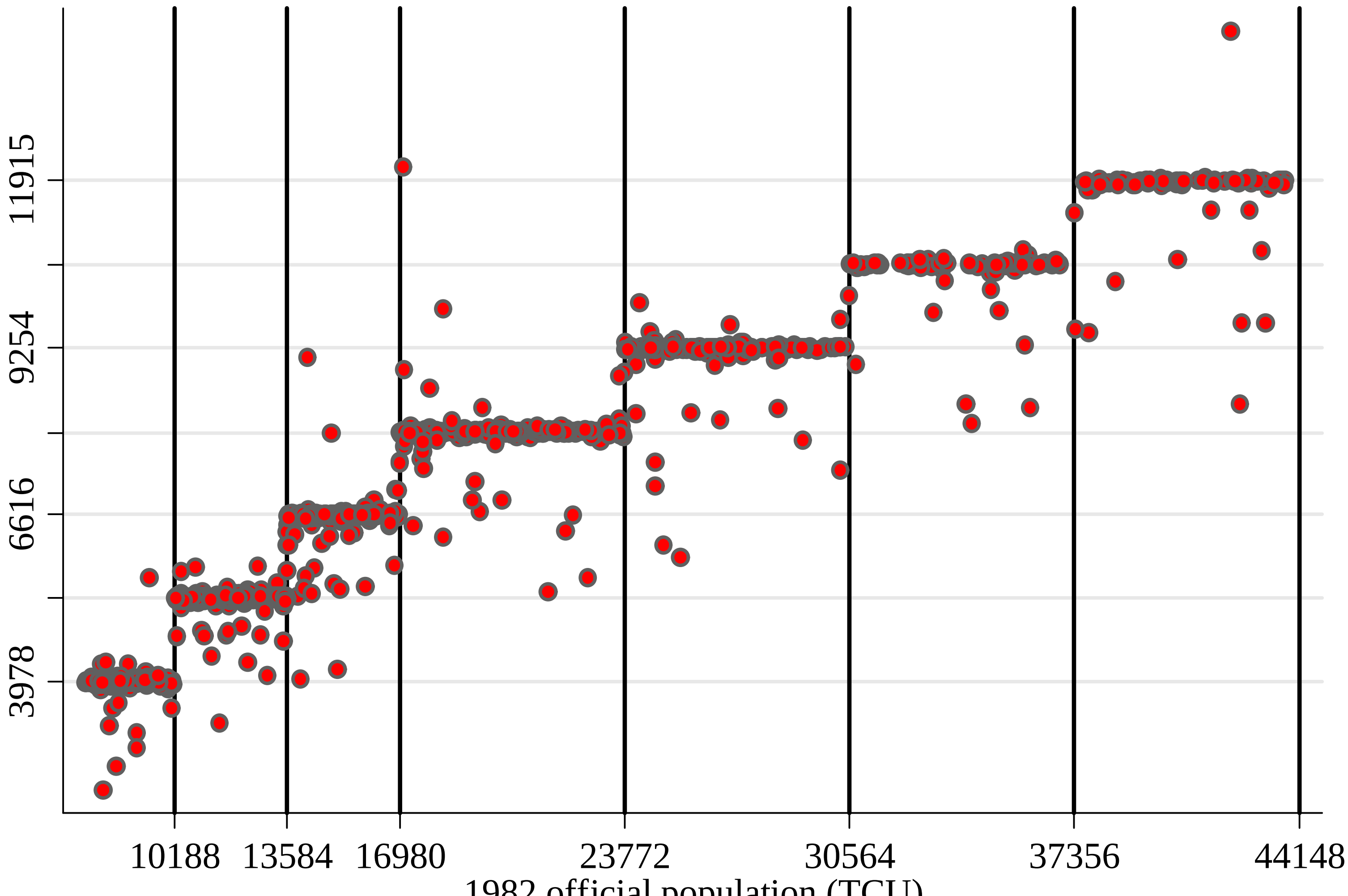
Notes: PB 2007 8th grade, 2009 9th grade, and 2011 9th grade test-taker samples. The four categories correspond to the highest education level of the most educated parent. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 9: Impact on parents' education levels, Prova Brasil 2007, 2009, and 2011, 4th or 5th graders

Neighborhood (percent):		2	2	3	3	4	4	5	5
Pretreatment covariates:		N	Y	N	Y	N	Y	N	Y
	Comparison mean:								
No more than primary school	0.42								
I[X > 0]		-0.028**	-0.027**	-0.025**	-0.018*	-0.032***	-0.026***	-0.026***	-0.021**
		(0.014)	(0.013)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.008)
R-squared		0.60	0.66	0.59	0.65	0.58	0.62	0.55	0.59
Some middle school	0.21								
I[X > 0]		0.010	0.006	-0.002	-0.001	-0.001	-0.000	0.007	0.009
		(0.011)	(0.011)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
R-squared		0.37	0.44	0.35	0.42	0.30	0.39	0.28	0.38
Some high school	0.15								
I[X > 0]		0.008	0.008	0.013**	0.011*	0.008	0.006	0.005	0.003
		(0.008)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
R-squared		0.28	0.40	0.24	0.35	0.21	0.30	0.21	0.28
Completed at least high school	0.22								
I[X > 0]		0.011	0.013	0.014	0.008	0.025***	0.020**	0.014*	0.009
		(0.012)	(0.012)	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)
R-squared		0.53	0.58	0.53	0.58	0.51	0.57	0.48	0.53
Observations		202	199	297	294	391	387	479	473

Notes: PB 2007 4th grade, 2009 5th grade, and 2011 5th grade test-taker samples. The four categories correspond to the highest education level of the most educated parent. Table entries are OLS discontinuity estimates using the pooled linear specification across the first three cutoffs. Heteroskedasticity-robust standard errors in parentheses. Neighborhood (percent) is percent distance from respective cutoff. All specifications include state fixed effects and segment dummies. Pretreatment covariates (1980 census) include county income per capita, average years of schooling for individuals 25 years and older, poverty headcount ratio, illiterate percentage of people over 14 years old, infant mortality, enrollment of 7- to 14-year-olds and percent of population living in urban areas. All specifications allow for differential slopes or curvature by segment and on each side of the cutoff. (***, **, and *) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

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



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

Figure 2: Municipalities with 1982 official population within 5% distance from cutoffs 10’188, 13’564, and 16’980

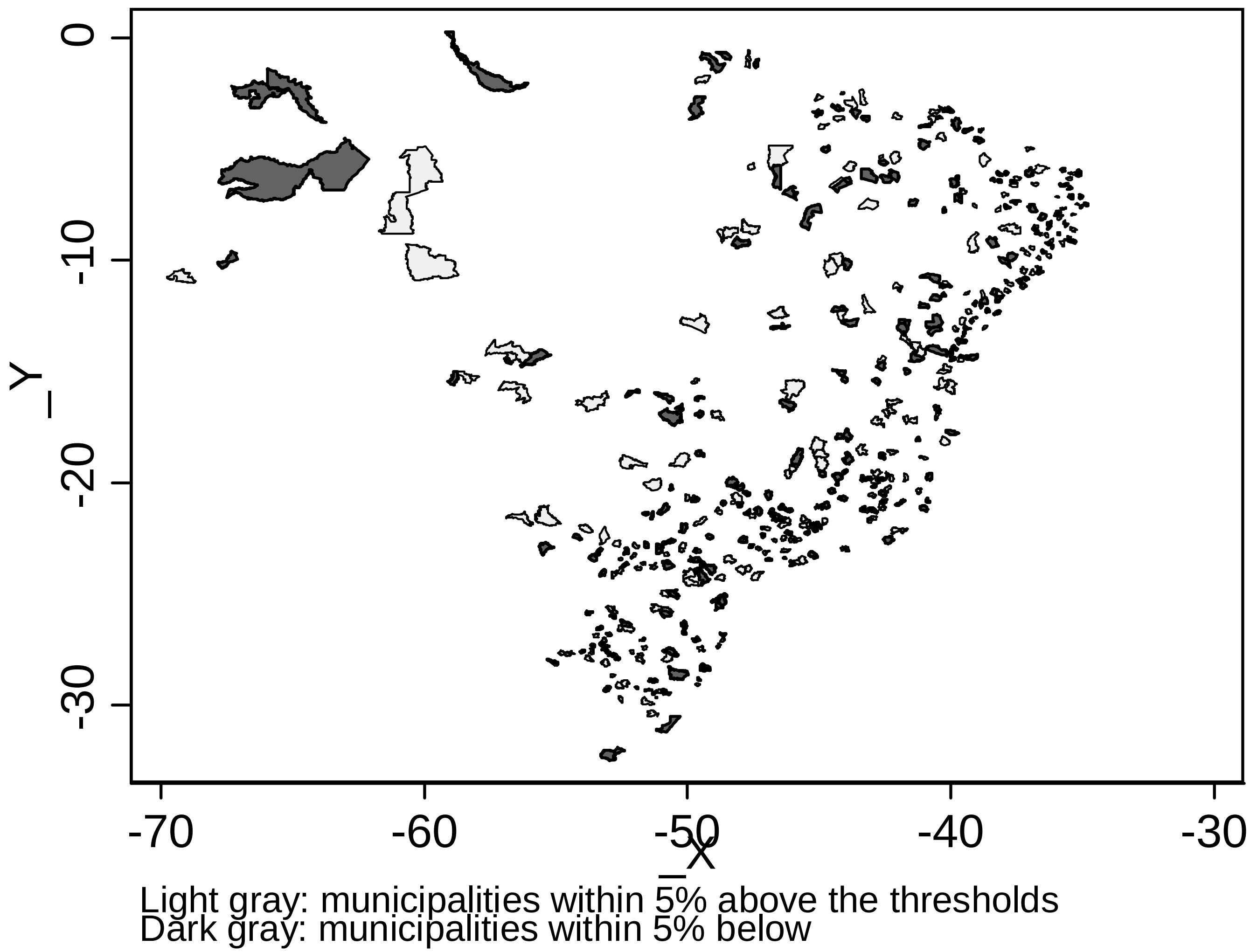
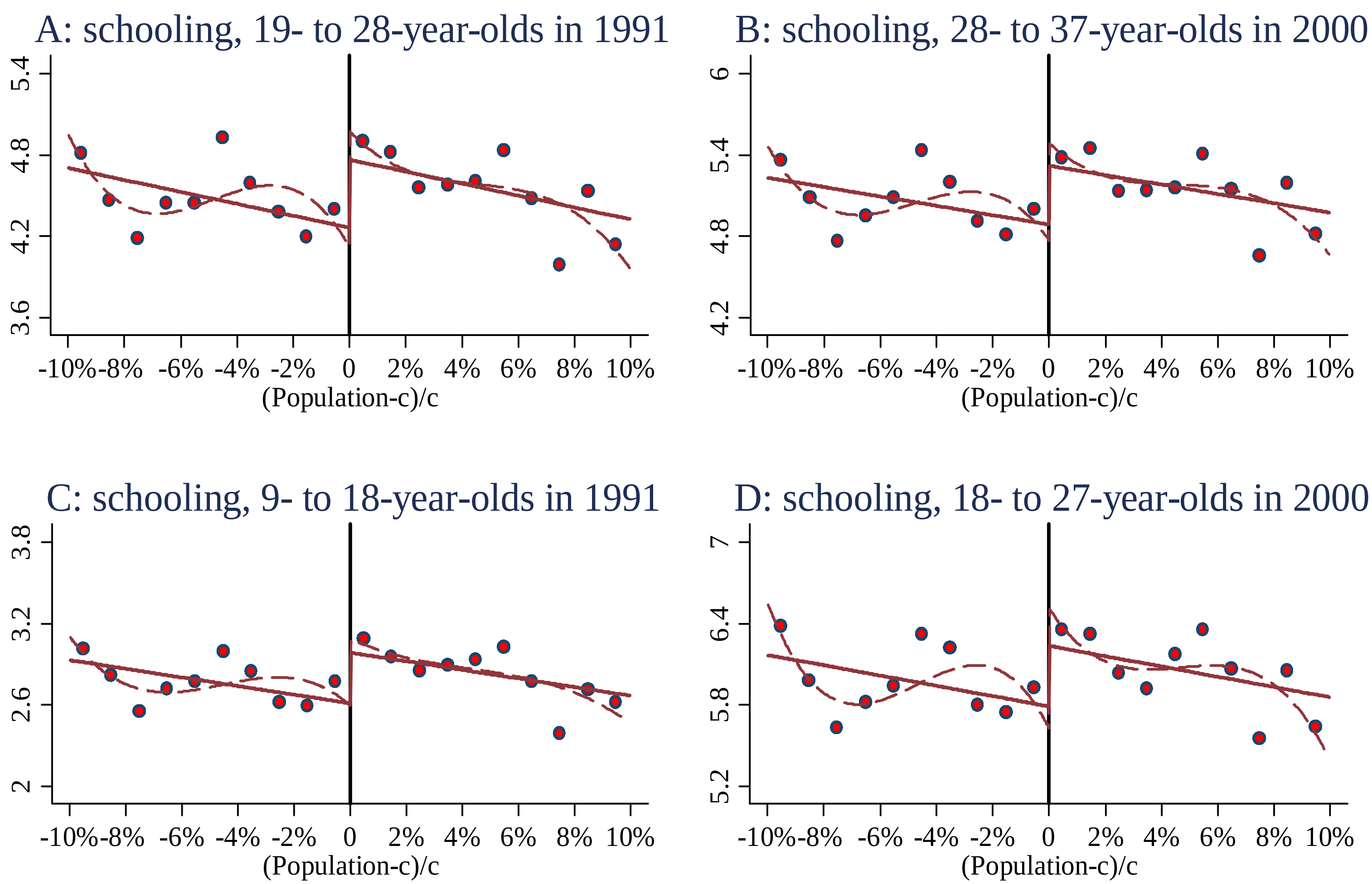
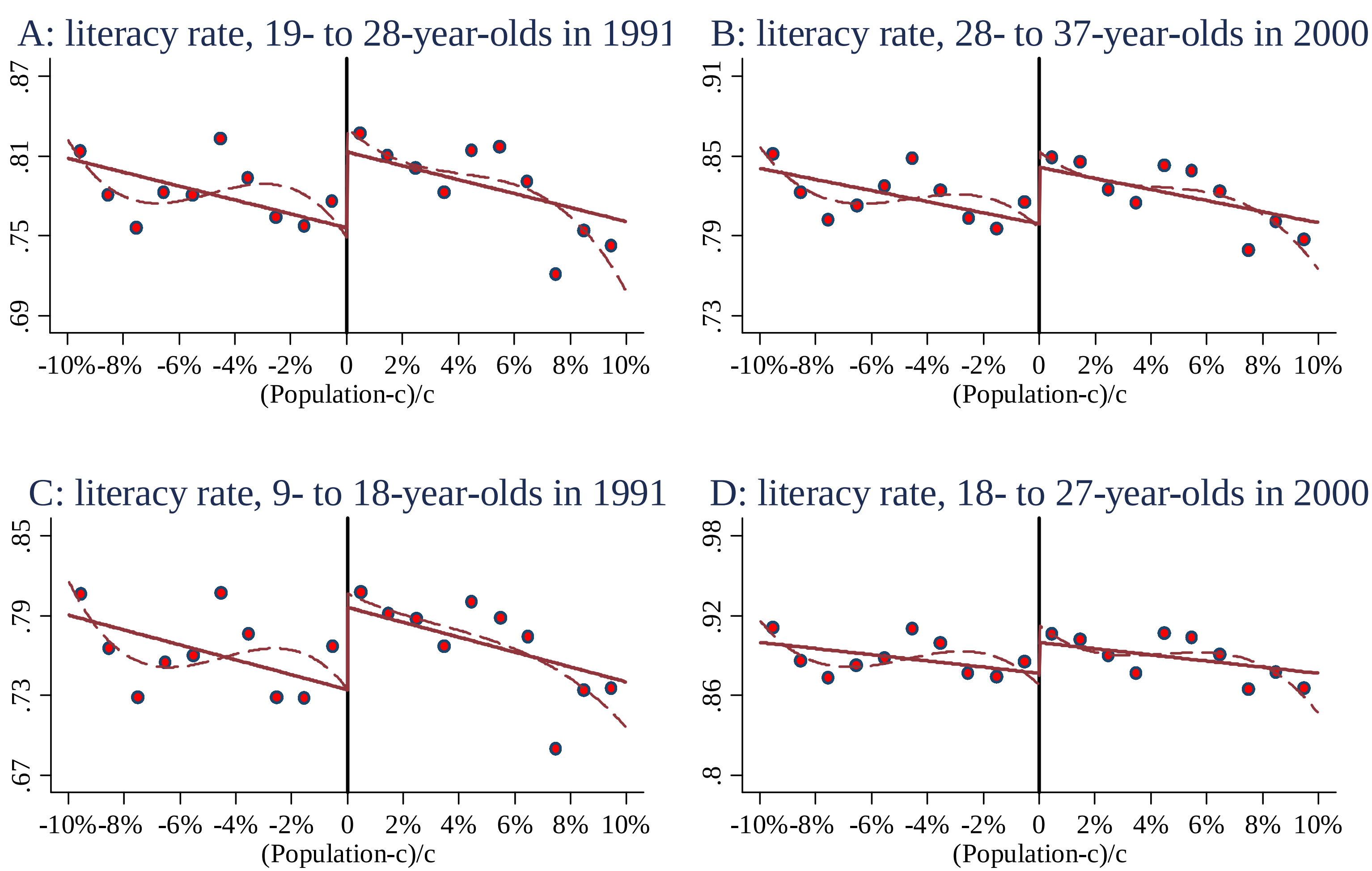


Figure 3: Impact on years of schooling for two directly exposed cohorts, 1991 and 2000



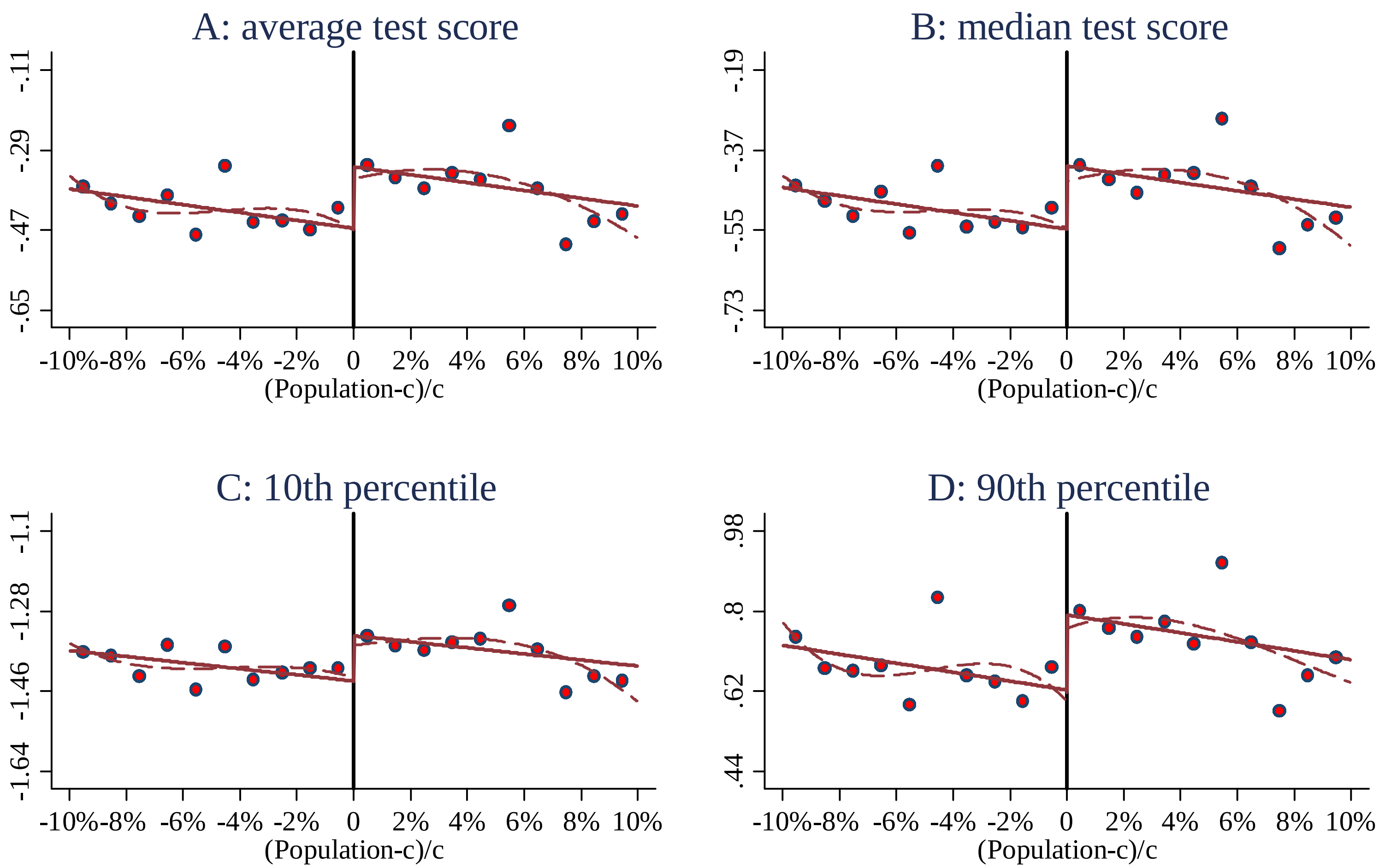
Notes: Census 1991 and 2000 samples. Census 1991 results from Litschig and Morrison (2013). Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 4: Impact on literacy for two directly exposed cohorts, 1991 and 2000



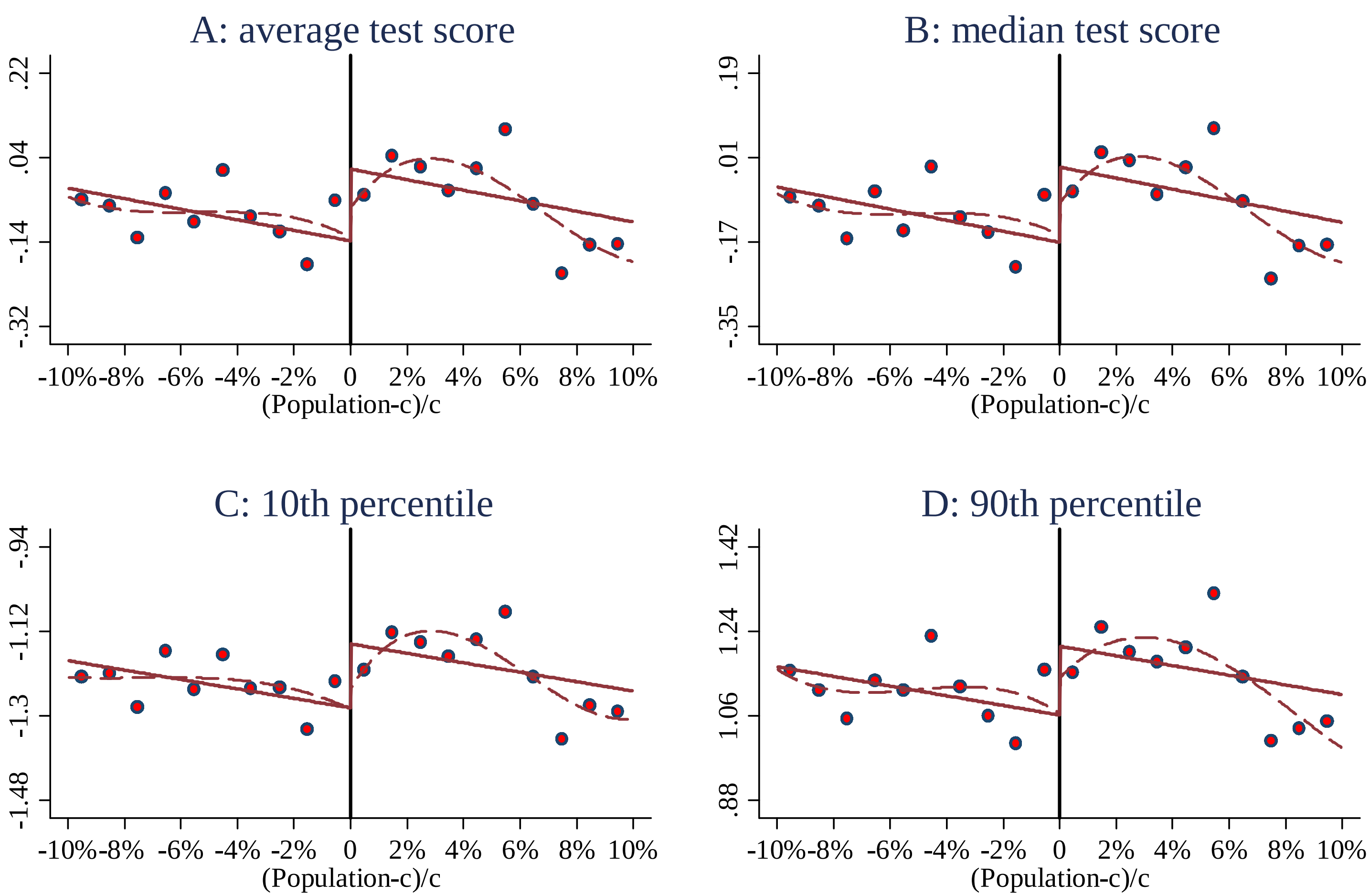
Notes: Census 1991 and 2000 samples. Census 1991 results from Litschig and Morrison (2013). Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 5: Impact on the distribution of ENEM test scores, 2007-2011, graduating cohorts



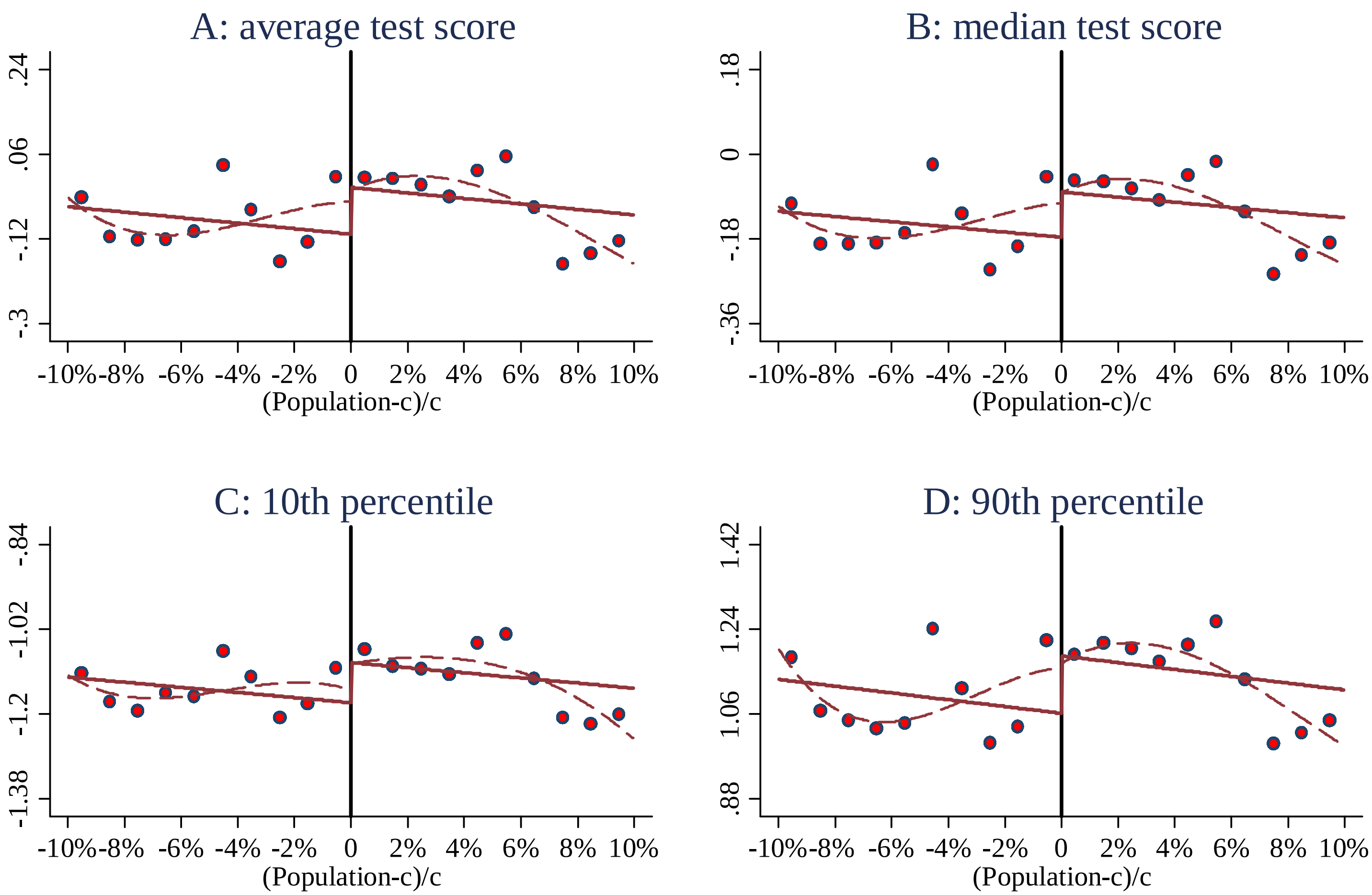
Notes: ENEM 2007-2011 test-taker samples, high school (12th grade) graduating cohorts. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 6: Impact on the distribution of Prova Brasil test scores, 2007, 2009, and 2011, 8th or 9th graders



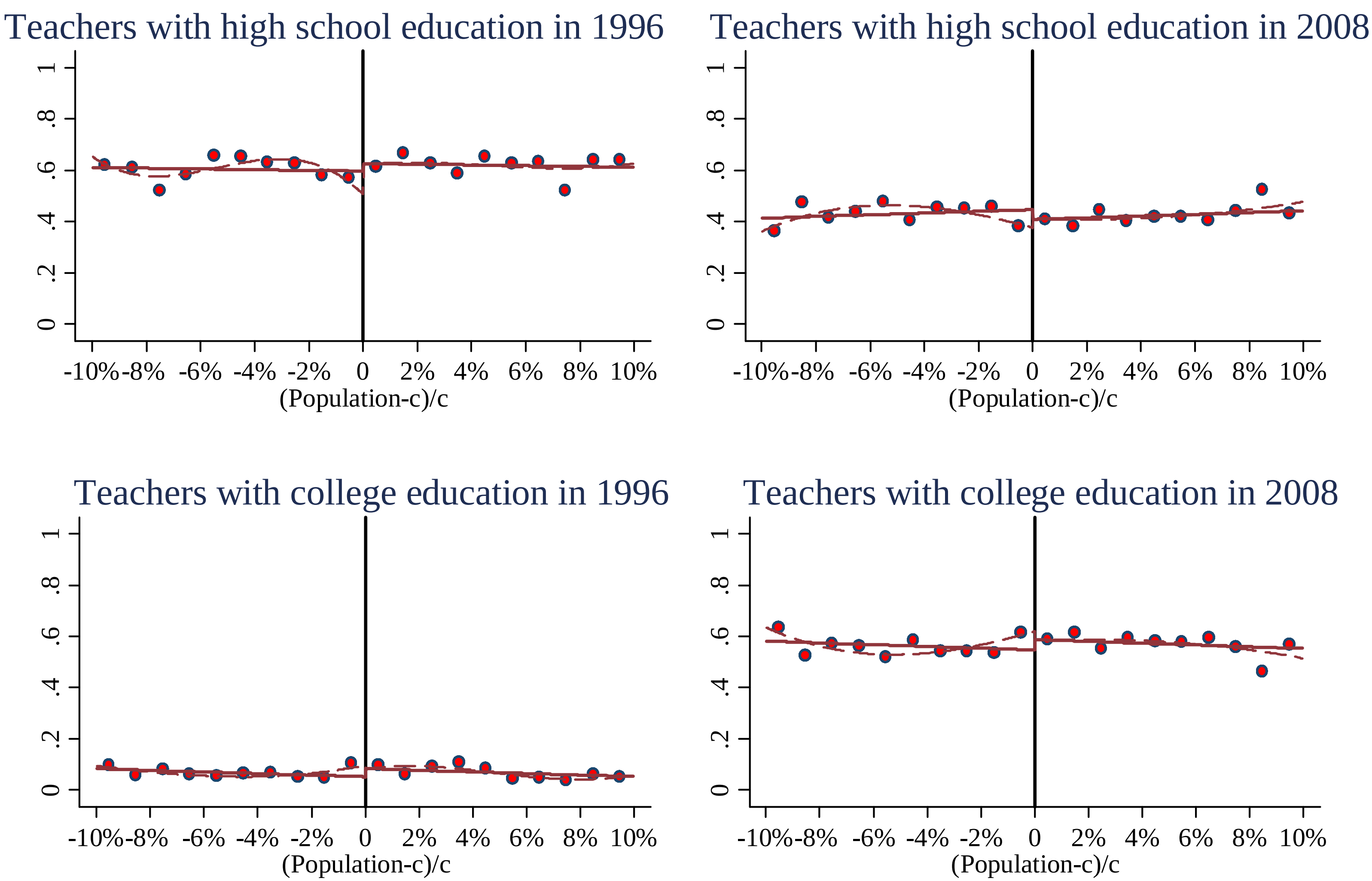
Notes: PB 2007 8th grade, 2009 9th grade, and 2011 9th grade test-taker samples. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 7: Impact on the distribution of Prova Brasil test scores, 2007, 2009, and 2011, 4th or 5th graders



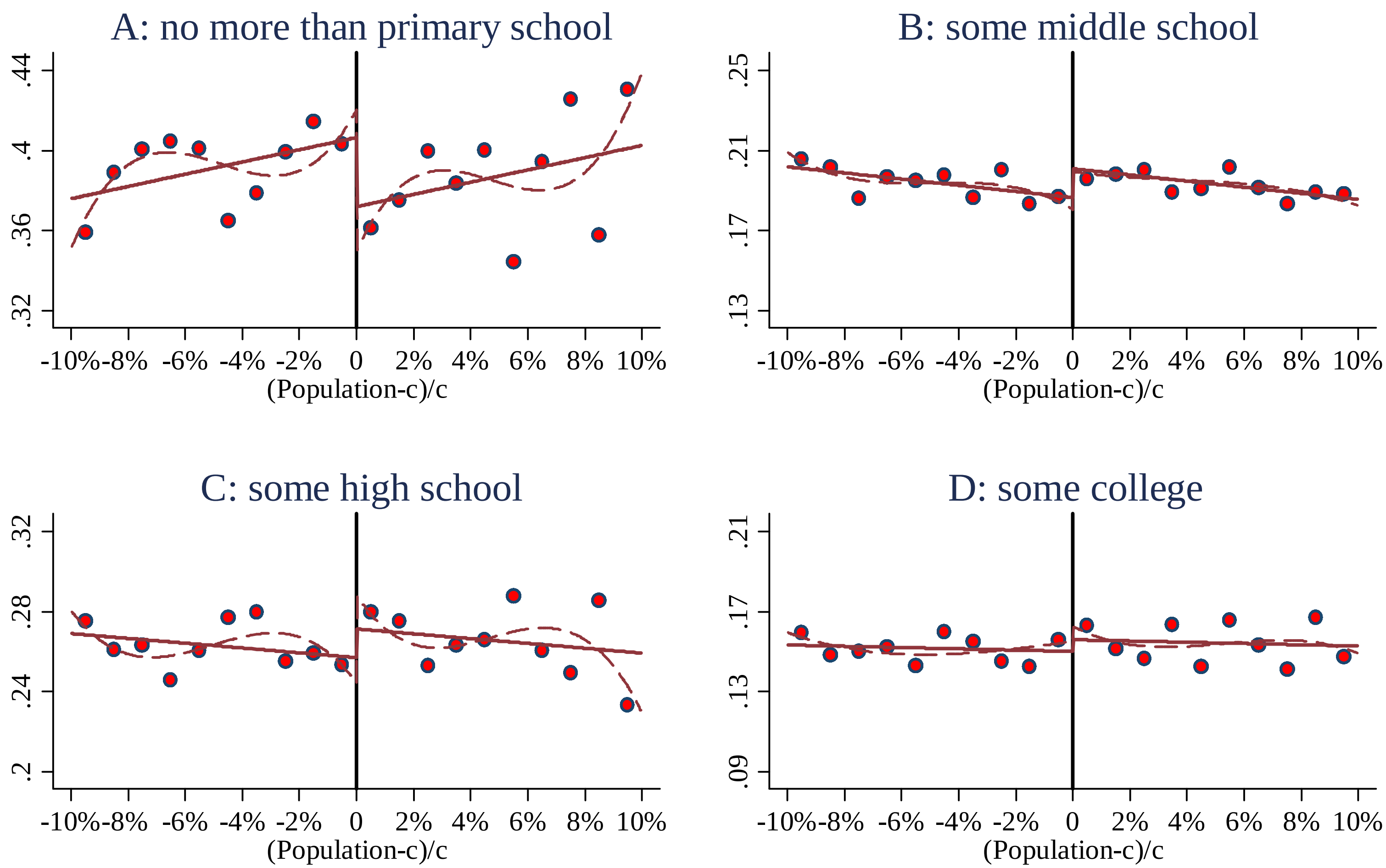
Notes: PB 2007 4th grade, 2009 5th grade, and 2011 5th grade test-taker samples. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 8: Municipal teachers' education levels, 1996 and 2008



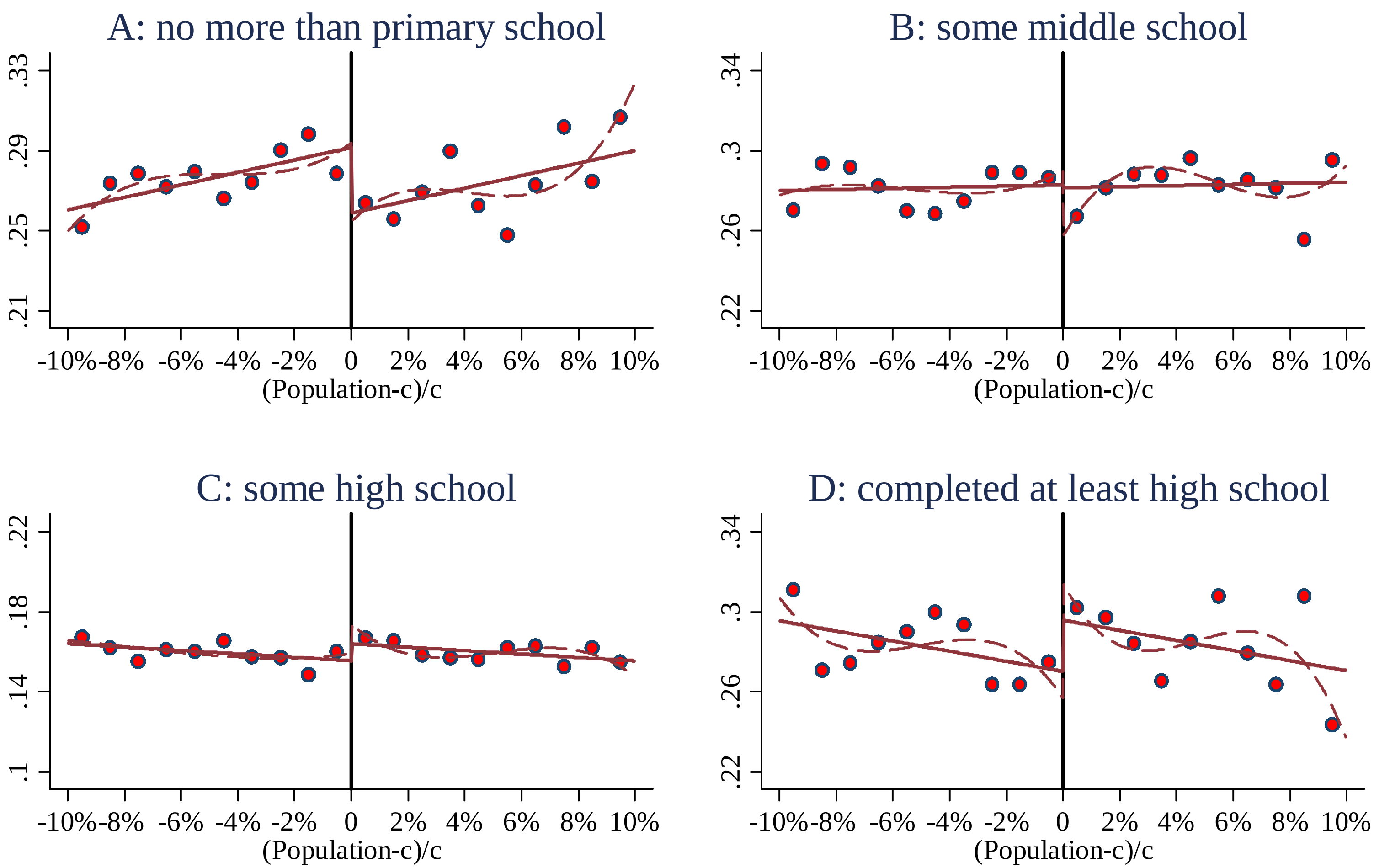
Notes: School census 1996 and 2008. Teachers from the municipal school system only.

Figure 9: Impact on parents' education levels, ENEM 2007-2011, high school graduating cohorts



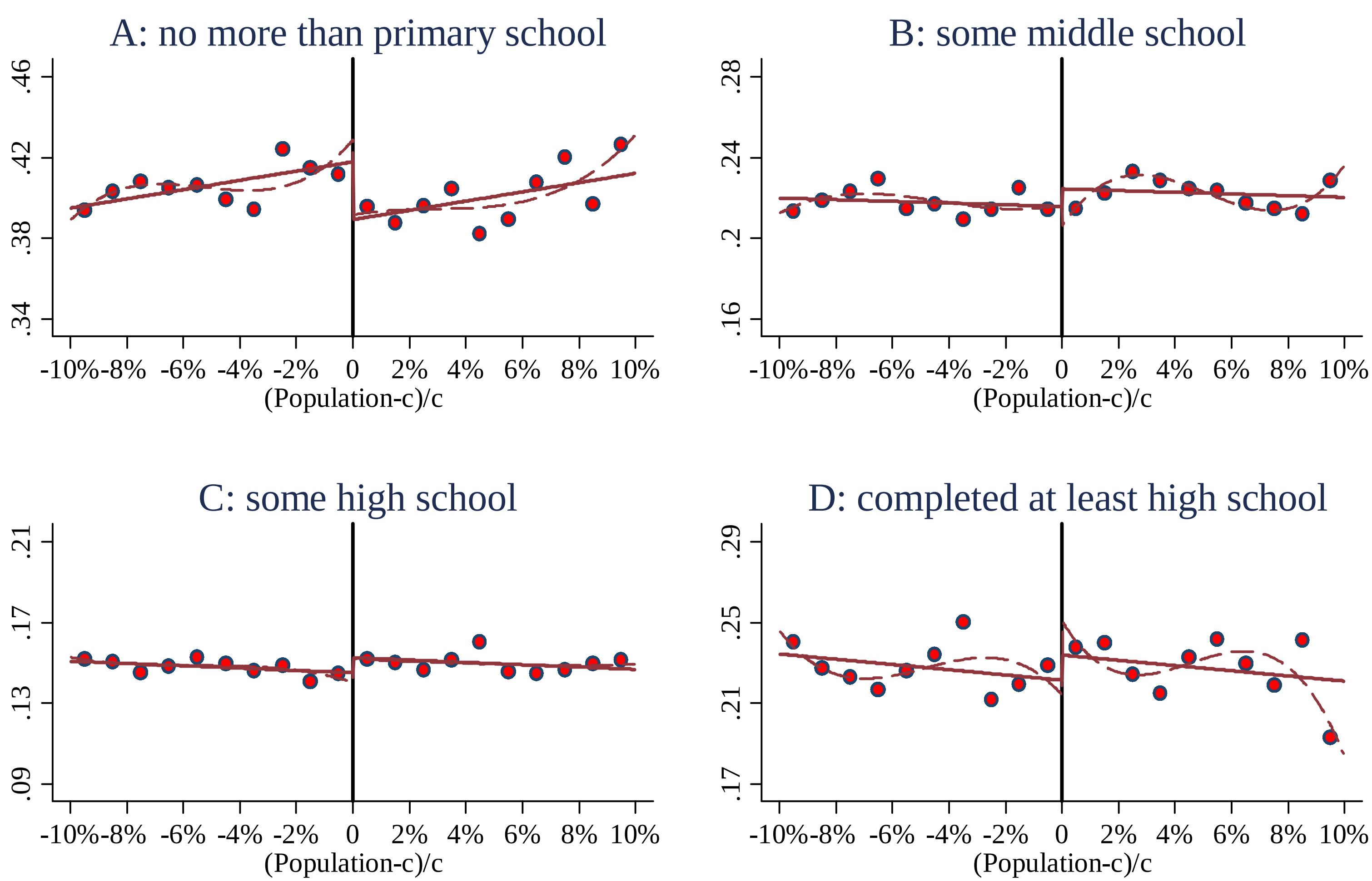
Notes: ENEM 2007-2011 test-taker samples, high school (12th grade) graduating cohorts. The four categories correspond to the highest education level of the most educated parent. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 10: Impact on parents' education levels, Prova Brasil 2007, 2009, and 2011, 8th or 9th graders



Notes: PB 2007 8th grade, 2009 9th grade, and 2011 9th grade test-taker samples. The four categories correspond to the highest education level of the most educated parent. Each dot represents the sample average of the dependent variable in a given bin. The bin-width is 1 percentage point of the respective threshold, c=10'188,13'584,16'980.

Figure 11: Impact on parents' education levels, Prova Brasil 2007, 2009, and 2011, 4th or 5th graders



Notes: PB 2007 4th grade, 2009 5th grade, and 2011 5th grade test-taker samples. The four categories correspond to the highest education level of the most educated parent. 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.