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Essays on the Economics of Education

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Abstract

Education is a powerful tool to improve lives and enhance the development prospects of nations. While primary and secondary school enrollment have increased considerably over the past decades in Brazil, completion rates and learning have remained low. This dissertation consists of three empirical essays, the first two focused on factors that may be holding back completion rates and the last one tackling the unsatisfactory learning levels.

The first essay analyses the impact of cumulative heat exposure on educational outcomes in the Brazilian Northeast. Using data from over 78 thousand schools, I find that a one-degree Celsius increase in the annual mean maximum temperature during the school year reduces grade progression by 0.35 percentage points and increases dropout rates by 0.26 percentage points. Results are consistent with established links between temperature and cognitive performance and have important implications for evaluating the welfare-burden of climate change.

The second essay, written jointly with Emily Hanno, examines the consequences of chronic exposure to community violence for children's academic progression in Rio de Janeiro, relying on longitudinal data for five cohorts of children attending municipal schools in the city. We estimate that a single year of exposure to high violence during the first four years of elementary school leads to a 6.9 percentage points reduction in the probability of reaching fifth grade on time, with effects being more than twice as large for children facing four years of high violence.

The third essay evaluates a peer mentoring program for school principals implemented in Ceará in 2008. Exploiting a regression discontinuity design for the school rank defining program participation, I find that low performing schools participating in the program subsequently achieve education quality indexes that are 0.18 standard deviations higher. Similarity in number of enrolled students and proximity between paired schools are associated with stronger outcomes for low performing participants, indicating that matching of mentee-mentors deserves careful consideration. These findings provide an example of how school turnaround may be achieved systemically without leadership replacement.

Resumo

Educação é um poderoso instrumento para a mobilidade social de indivíduos e o desenvolvimento de nações. No Brasil, apesar de melhoras consideráveis das taxas de matrícula na educação primária e secundária, as taxas de conclusão e de aprendizado continuam baixas. Esta tese de doutorado consiste em três estudos empíricos, os dois primeiros focados em possíveis entraves às taxas de conclusão, e o último abordando os níveis insatisfatórios de aprendizado.

O primeiro estudo analisa o impacto cumulativo de exposição ao calor nos resultados educacionais no Nordeste. Usando dados de mais de 78 mil escolas, encontro que um aumento de um grau Celsius na temperatura máxima média anual durante o ano letivo reduz a progressão escolar em 0,35 pontos percentuais e aumenta a taxas de abandono em 0,26 pontos percentuais. Esses resultados são consistentes com vínculos estabelecidos entre temperatura e desempenho cognitivo e têm implicações importantes para avaliar o impacto social das mudanças climáticas.

O segundo estudo, escrito conjuntamente com Emily Hanno, examina as consequências da exposição crônica à violência urbana para a progressão acadêmica de crianças no Rio de Janeiro, baseando-se em dados longitudinais para cinco coortes de alunos de escolas municipais. Estimamos que um único ano de exposição à alta violência durante os anos iniciais do ensino fundamental leva a uma redução de 6,9 pontos percentuais na probabilidade de atingir o quinto ano no tempo ideal, sendo os efeitos duas vezes maiores para crianças expostas por quatro anos à alta violência.

O terceiro estudo avalia um programa de parceria entre diretores de escolas, implementado no Ceará em 2008. Exploro uma descontinuidade na classificação escolar que define a participação no programa, e estimo que as escolas de baixo desempenho que participam do programa melhoram em 0,18 desviões padrões seus índices de qualidade de educação. A similaridade no número de alunos e a proximidade entre as escolas pareadas estão associadas a melhores resultados, indicando que a atribuição de mentorados e mentores merece consideração cuidadosa. Essas descobertas fornecem um exemplo de como a melhora de escolas com baixo desempenho pode ser alcançada sistemicamente sem a substituição de diretores.

Too hot to learn?
Evidence from the Brazilian Northeast

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Abstract

Using data from over 78 thousand schools in the Brazilian Northeast between 2007 and 2016, this study analyzes the impact of cumulative heat exposure on educational outcomes. Hotter school days during the academic year reduces grade progression, with extreme heat being particularly detrimental. In our preferred specification – which includes spatial, temporal and school-grade fixed effects –, a one-degree Celsius increase in the annual mean maximum temperature during the school year reduces grade progression by 0.35 percentage points (0.43%) and increases dropout rates by 0.26 percentage points (3.86%). Results are consistent with established links between temperature and cognitive performance and have important implications for evaluating the welfare-burden of climate change in developing countries.

Keywords: education, temperature, climate change impacts, economics.

1. Introduction

Despite much recent progress in primary education enrollment, which reached 91% worldwide, completion rates still lag far behind in developing countries. In Latin America, only 48% of those aged 20-24 have completed secondary education, due to high dropout and repetition rates[1]. Given the importance of human capital accumulation for economic growth (Romer, 1986; Barro, 1991; Card, 2001) and concerns that climate change will disproportionally affect least developed countries, we study the links between temperature and educational attainment.

Weather shocks are known to impact schooling decisions in developing countries, especially in rural areas, through agricultural income and health channels (Jensen, 2000; Maccini and Yang, 2009; Björkman-Nyqvist, 2013; Groppo and Kraehnert, 2017; Shah and Steinberg, 2017). Temperature is also known to have a direct negative effect on cognition: a meta-analysis from laboratory studies estimates that human cognition is the highest at 21.75°C (71.5°F) with a 9% decrement of performance when temperature is 30°C (86.1°F) (Seppanen, Fisk, and Lei, 2006).

Using data from over 78 thousand schools and 114 million K-12 enrollments in Northeast Brazil from 2007-2016, we analyze the impact of cumulative heat exposure on educational outcomes. This region is the largest pocket of poverty in Brazil, characterized by a hot and semi-arid climate. We find strong evidence for negative impacts of heat on schooling: a one-degree Celsius increase in the annual mean of maximum temperature during school days reduces grade progression by 0.35 percentage points (0.43%) and increases dropouts by 0.26 percentage points

[1] Worldwide primary school enrollment and Latin American secondary education completion figures from UNICEF Global databases updated in Dec 2017 and available at http://data.unicef.org/topic/education/. Regional averages consider countries with available data from 2010–2016, based on national household surveys.

(3.86%). We cannot untangle the mechanisms through which heat is affecting academic progress, with both income and cognition effects being plausible components. A suggestive evidence for cognition effects is that our estimates for urban areas are significant and of similar magnitude to the estimates for rural areas, despite a higher sensitivity to weather income effects in rural areas through agriculture. Those results are also interpreted as estimated net effects, including direct effects on children and indirect effects through teachers and principals. Using estimators by temperature bins and considering the Intergovernmental Panel on Climate Change predicted 2-degree Celsius warming by 2050 (IPCC, 2014), this would mean a 0.67 percentage points decrease in grade promotion and a 0.34 increase in dropouts.

We build on the bourgeoning literature on the impacts of weather and climate on human-capital accumulation. Prior work has focused on standardized assessments, that is, the *intensive margins* of education and fall into two categories. They analyze either the contemporaneous effects of high-temperatures on performance, or the effects of long-run exposure to heat on learning. In the first group, estimates range from a decline of 1.3% to 2.9% standard deviations in high-stakes exams' performance per a one-degree Celsius temperature increase, respectively in New York high schools (Park, 2018)[2] and in China's college entrance exams (Graff Zivin et al., 2018)[3]. Estimated effects on low-stakes and voluntary survey assessments fall within the same rage in the U.S. (Graff Zivin et al., 2017) and in India (Garg et al., 2017). The second strand, concerned with the effects

[2] Reports an approximately linear decline of -0.2 percentiles per °F above 70°F (22°C), equivalent to a performance reduction of 0.14 standard deviations for an exam day at 90°F, using exam data from 1 million public NY high school students from 1999-2011.

[3] Reports that a one-standard-deviation increase in temperature (3.29°C) decreases the total test score by 1.12% (9.62% of a standard deviation), using 14 million observations of the National College Entrance Examination (NCEE) from 2005-2011.

of long-run exposure to heat, is closest in spirit to this analysis. Goodman et al. (2018) use data from 10 million U.S. high school students and find that a 1°F (0.56°C) hotter school year prior to the exam lowers PSAT scores by 0.2% standard deviations[4], with access to air conditioning completely offsetting this effect. We are similarly interested in the effects of long-run exposure to heat but focus on whether students remain enrolled and progress through K-12, offering insights on how weather directly affects the *extensive margins* of education, a salient and first order issue in developing countries.

The rest of the paper unfolds as follows. Section 2 provides contextual information on Northeast Brazil. Section 3 details data sources and methods. Section 4 presents this study's empirical strategy and its results, including a series of robustness checks. Section 5 concludes.

2. Context

Brazil is divided into five administrative regions, of which the Northeast (*Nordeste*) is the poorest. Northeast Brazil encompasses 18% of the country's land area and about 28% of the population, including 15 million primary and secondary school students[5]. In 2017, the mean per capita income in the Northeast was 64% of the national average and its adult illiteracy rate, at 14.8%, was more than twice the Brazilian mean[6].

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[4] Regression includes student-fixed effects, thus concerns only students from classes of 2001-2014 who took the PSAT exam multiple times. The effect size is relative to a school year with an average of 60°F (15.6°C).

[5] According to Brazil's 2010 census, from the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE).

[6] Brazil's average monthly household per capita income (*Rendimento nominal mensal domiciliar per capita*, RDPC) in 2017 was R$1,268 (~US$316) and R$806 in the Northeast region (~US$201), the lowest of the five regions. Adult illiteracy applies to Brazilians aged 15 and above. Data from IBGE household survey (*Pesquisa Nacional por Amostra de Domicilios*, Pnad 2017)

Grade retention and dropout rates have recurrently been highest in the Northeast (versus the national average) at each grade level, particularly in rural areas[7]. However, current preschool enrollment rates are relatively high in the Northeast (94.1% for children aged 4 and 5, versus the national figure of 90.2%[8]), and primary school enrollment is close to universal. This suggests that ensuring regular grade advancement is a binding constraint to human capital accumulation in the Northeast.

Finally, the Northeast is the warmest region in Brazil, and one that experiences recurring draughts. When droughts hit, thousands of agricultural workers abandon their homes for the cities in search of work. Draughts may also affect human capital investment — with more students dropping out — helping to perpetuate economic deprivation (Shah and Steinberg, 2017).

[Figure 1 – Brazil mean temperature]

3. Data

Our analysis examines grade advancement in all primary and secondary schools in Brazil's Northeast region in relation to temperatures at the county level during the school year. We first describe each of the data sources used.

3.1. Weather Data

[7] See Figures A.1, A.2, A.3 for a comparison of grade promotion, grade retention and dropout rates in the Northeast and national averages in recent years. See Gomes-Neto and Hanushek (1994) for early statistics and an exploration of the causes and consequences of high retention rates in the Brazilian Northeast.

[8] Pnad IBGE 2017

We construct historical information on temperature by county using the ERA-Interim dataset produced by the European Centre for Medium-Range Weather Forecasts (ECMWF, Dee et al. 2011). This is a simulation of past worldwide weather conditions from 1979 onwards, based on the application of a four-dimensional variational analysis using satellite radiance data from observations every 12 h at a raw grid spacing of 0.75°, further interpolated for a 0.125° or 14 kilometer resolution. The dataset provides simulated maximum temperature at three-hourly intervals, and we calculate daily maximum temperature by taking the maximum of all eight intervals in a day for the 2007-2017 period. We performed a standard bias-adjustment[9] procedure based on observational data, as proposed by Jones et al. (2017).

Following Goodman et al. (2018), we focus on daily maximum temperatures instead of mean daily temperatures, because schooling occurs during the daytime, when maximum temperatures usually are recorded. We allocate this information across counties by assigning the weather information of the pixel closest to the administrative headquarters coordinates of each county, according to the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE)[10].

We construct two county-by-year measures of cumulative heat exposure: the average daily maximum temperature during school days and the share of school days with maximum temperature within various temperature bins (below 27°C, 27-31°C, 31-35°C, 35-39°C and above 39°C). The distribution of the first measure is presented in Figure 2, a box plot of the mean of the

[9] Bias correction consisted on a lump-sum adjustment per month (unique value for the whole dataset), to match ERA-Interim monthly means to the National Meteorological Institute (*Instituto Nacional de Metereologia*, INMET) monthly means, when averaging the 95 pixels corresponding to all INMET's weather stations in the Brazilian Northeast.

[10] More precisely, IBGE assigns as administrative headquarters the centroid of the urban zone within a county. Data available in: https://ww2.ibge.gov.br/home/geociencias/cartografia/territ_localidades.shtm

daily maximum temperature by county-year, weighted by number of students. The boxes include the counties in the 25th to 74th percentiles, and the horizontal line in each box represents the median county-year. The second measure is shown in Figure 3, in which the y-axis represents the average number of days in which maximum temperature falls into one of five bins in a year, weighted for the number of students. Both figures show that the most commonly occurring maximum temperatures, in Celsius, were in the low 30s, and daily temperatures above 39 were rare during this period.

[Figure 2 – Boxplot of county annual mean temperature during the school year]

[Figure 3 – Share of school days by bins of maximum temperature]

Compared to settings of previous work examining the impacts of temperature in educational outcomes, Northeast Brazil is significantly warmer. The mean of maximum daily temperatures during the school year of 31.9°C (89.5°F) is not attained in any US county (the setting of Goodman et al. 2018).

3.2. Education data

The education data comes from the School Census (*Censo Escolar*), an annual survey of every school in Brazil made available by the National Institute for Research on Education (*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira*, INEP)[11]. We use information on *all* the K1-K12 enrollments reported in the School Census from 2007 to 2016 to

[11] This is a reliable source of information because the federal government frequently checks and audits the information reported in the School Census, for a large share of the public educational budget is based on its enrollment figures.

build a panel of over 3.4 million school-grade-year observations across 78 thousand schools in all 1,794 counties in the Brazilian Northeast[12].

Our panel includes the following information: characteristics of the school (such as the quality of its infrastructure, whether the school is located in an urban or rural area and whether it is a public or private school), school-level teacher characteristics (such as share by gender, average age, and share by education attainment), school-grade-level enrollment figures and student characteristics (such as share by gender and whether they are older than the ideal age for their grade) and end-of-year grade progression results. This last measure is our main outcome variable, and is computed as share of students in a given school-grade-year that were promoted, retained or that dropout – thus, always adding to 100%.

$$Pro_{sgy} + Ret_{sgy} + Drop_{sgy} = 100\% \quad \text{(Equation 1)}$$

We also used the first and last day of classes declared by each school in the INEP Census. Since weather is observed at the county level, we averaged the valid dates of all schools in a county to construct a mask of school and non-school days for each year[13].

Table 1 describes the dataset and Table 2 shows some descriptive statistics of the education data. The unit of observation in this table is a school-grade-year, unless otherwise noted. Most observations correspond to public schools in rural areas (57%), though enrollment is concentrated

[12] Though we started with information on *all* enrollments, roughly 0.3% of enrollments occurred in singleton observations in our panel, that is, in a school-grade that appeared only once in our ten-year window. Given that our identification strategy depends on temporal variation within a school-grade, those observations were discarded.

[13] The Census does not contain any information on chosen dates for the Winter break. As a proxy, we considered the full month of July as non-school days.

in public urban schools (65%), which have on average more students per grade. Private schools are overwhelmingly urban and represent 12% of enrollments and 14% of observations.

[Table 1 – Dataset description]

[Table 2 – Summary Statistics]

4. Empirical specification and results

To estimate the effect of cumulative heat exposure on student grade progression, we exploit both spatial and temporal variations in temperature for ten years. The main identification assumption is that unobserved determinants of shares of enrolled students by end-of-year grade progression results are uncorrelated with year-to-year variation in county weather. We implement this identification strategy with school-grade fixed effects regressions of the following form:

$$Flow_{sgy} = \beta Temp_{c_s y} + \eta_{sg} + \gamma_{n_s gy} + \delta X_{sgy} + \epsilon_{sgy} \qquad \text{(Equation 2)}$$

where $Flow$ denotes the outcome variable of interest (share of grade promotion, grade retention or dropout) for students enrolled in school *s*, grade *g*, in year *y*. Inclusion of school-grade fixed effects η_{sg} implies that identification comes from within-school-grade comparisons of heat exposure and enrollment outcomes over multiple years. Grade-year by school-network[14] fixed effects $\gamma_{n_s gy}$ flexibly control for a variety of potential confounds, including time-evolving state educational policy and differential standards for grade promotion between public and private schools in any given state and year. We also include a set of school-grade-level controls X, including student demographics (average number of students per classroom, share of males, share of students who

[14] School-network is defined as the pair state (9 states in the Brazilian Northeast) and school type (public or private)

are older than the ideal age for their grade, whether the classroom is a mixed-level grade) and school-level characteristics (infrastructure score[15], average age of teachers, share of male teachers and share of teachers with tertiary education). Standard errors are clustered by county, the level of variation in our treatment variable, as usual in the literature (Bertrand, Duflo, and Mullainathan 2004; Abadie et al. 2017).

Our treatment variable $Temp$, is the average of maximum temperature experienced in the county of school s during school days in year y, in which case the coefficient of interest β can be interpreted as the impact on grade progression of students experiencing a one-degree Celsius hotter school year on average. Absent an analysis of the mechanisms through which heat may affect academic progress, β is interpreted as both cognition and income net effects, including direct effects on children and indirect effects through teachers and principals.

In a more flexible specification, we replace average maximum temperature with a vector of shares of school days with maximum temperature falling into various bins (below 27°C, 27-31°C, 31-35°C, 35-39°C and above 39°C). In this specification, the vector of coefficients β can be interpreted as the impact of experiencing 1% more days in each temperature bin, relative to a 1% less days below 27°C (the omitted temperature bin).

As noted, we include a rich set of fixed effects. In particular, we are identifying off within-school-grade, within-network temporal trends variation. Our identifying assumption is that once school-grade and network time trends are controlled for, the realization of temperature on any particular year is exogenous to unobserved determinants of students' grade progression results.

[15] Infrastructure score refers to the average of five indicator variables: whether a school has a library, a sports court, a teachers' room, a science laboratory and a computer laboratory.

One potential concern with this strategy is that grade promotion standards vary by year within a school, in a way not captured by network time trends. We believe this to be unlikely, for education policies education set at the state level are already factored in our school-network fixed-effects[16].

4.1. Mean impacts

The base results are summarized in Tables 3 and 4. In both, Panel A shows the impact of the mean of daily maximum temperature and Panel B displays the impact of share of days in each temperature bin. The first column contains results for grade promotion (Table 3) or dropouts (Table 4), measured in percentage points, using our preferred specification (described in Equation 2), incorporating cohort demographic controls and analytical weights corresponding to the average number of enrolled students in a school-grade for ten years. Columns 2 to 4 present alternative specifications by omitting the cohort controls and/or weights.

[Table 3 – Grade promotion regression results]

[Table 4 – Drop-out rate regression results]

On average in the Brazilian Northeast, enrolled K1-K12 students experiencing a 1°C hotter school year achieve 0.36 percentage points lower grade promotion and 0.26 higher dropout rates, results that are robust statistically significant. Given an average grade promotion of 81.08% in the sample, this implies a 0.43% decrease in grade promotion. Similarly, the effect on dropouts

[16] The provision of public education in Brazil is the responsibility of both the municipality and state. Whereas the municipality is in charge of the elementary and middle school grades (i.e., K1-K9), the state is responsible for high school (i.e., K10-K12). However, states have strong influence over K1-K9 policies, coordinating programs and overseeing the delivery by municipalities.

represent a 3.86% increase from the average rate of 6.73%. The impact on grade retention is not statistically significant (mean +0.09, s.d. 0.16)[17].

Very hot days are particularly damaging to end-of-year grade promotion: replacing one school day (roughly 0.5% of 200 annual school days) below 27°C with a day above 39°C lowers grade promotion by 0.106 percentage points and increases dropouts by 0.081 percentage points. Panel B of Tables 3 and 4 shows the specification in which we consider the impacts of cumulative heat exposure as share of school days per temperature bins, with days below 27°C as the omitted category. Those coefficients suggest that the magnitude of the impact increases roughly linearly with the temperature, as represented in Figure 4 and 5, which exbibit graphically the coefficients in columns (1) of Tables 3 and 4.

[Figure 4 – Coefficients from 4°C temperature bins on grade promotion]

[Figure 5 – Coefficients from 4°C temperature bins on drop-out rates]

Using the coefficients by temperature bins to calculate the impacts of a 2°C degree increase in every school day, in line with IPCC predictions (IPCC, 2014), yields an estimated 0.671 percentage points decrease in grade promotion and a 0.338 increase in dropouts.

4.2. Heterogeneity by school type

We now turn our attention to school characteristics that may justify heterogeneous effects of heat exposure. Though the physical mechanisms, should not differ between schools, private institutions have greater resources available to mitigate the effect of heat, e.g., with fans and air conditioning. In our setting, air conditioning prevalence is believed to be very small, but

[17] See Table A.1 for the full results table on grade retention

unfortunately, we do not observe these mitigation technologies directly. Similarly, schools in urban areas tend to have better infrastructure than their rural counterparts. In section 4.4. we use existing data on rural schools' water supply to further investigate this heterogeneity.

For this section we re-estimate the preferred specification (including a comprehensive set of controls and weighting by number of students) splitting the sample by school type: rural/urban and public/private. Results are presented graphically[18] in figures 6 through 9.

[Figure 6 – Coefficients from 4°C temperature bins on grade promotion: heterogeneity rural/urban schools]

[Figure 7 – Coefficients from 4°C temperature bins on drop-out rates: heterogeneity rural/urban schools]

[Figure 8 – Coefficients from 4°C temperature bins on grade promotion: heterogeneity public/private schools]

[Figure 9 – Coefficients from 4°C temperature bins on drop-out rates: heterogeneity public/private schools]

Though the point estimates of the effects of heat on grade progression and drop-out rates are larger for rural schools, they are not statistically different from each other. There's a noticeable contrast between private and public schools: they both exhibit statistically significant negative effects of heat on grade progression, however, dropouts only increase in public schools. While the "leakage" on regular progression caused by increased retention is significant both on private and

[18] See Table A.2 for detailed estimates

public schools, students who attend private schools are generally from higher-income families and less likely to drop-out.

4.3. Robustness checks

We repeat the estimation for narrower temperature bins, in 2°C increments, as a robustness check. Figure 10 shows results for grade progression: standard errors increase but the estimated coefficient on temperature retains sign for all and significance for most bins.

[Figure 10 – Coefficients from 2°C temperature bins on grade promotion: robustness check]

As falsification tests we perform two placebo exercises. First, we replace school days with non-school days when calculating the temperature bins for each county-year. Second, we replace the temperature of a given year with data from the following year in the same county. The results of these exercises on grade progression are reported in Figures 11 and 12. In each case the point estimate of the coefficients of interest are smaller and in no case these estimates are statistically significant. This makes it less likely that our results are driven by spurious correlations between temperature and schooling and is consistent with our interpretation of these impacts as causal[19].

[Figure 11 – Coefficients from 2°C temperature bins on grade promotion: falsification test using non-school days]

[Figure 12 – Coefficients from 2°C temperature bins on grade promotion: falsification test using following year]

[19] See Figures A.4 to A.6 for equivalent results for dropout rates.

4.4. Water cisterns in schools

Constructing residential water cisterns in drought-prone areas of the Brazilian Northeast has been shown to reduce political clientelism, through a reduction in economic vulnerability (Bobonis et al. 2017). Likewise, we investigate whether school water cisterns could influence educational outcomes, particularly in rural areas, in which most schools are not connected to the public water network. Cisterns would attenuate the detrimental effects of droughts created by abnormal warm years. Weighted by enrollment, only 45% of rural public schools receive water through the public network, an additional 23% are not linked but have cisterns to store rain water, 27% access water through spring and wells and 6% don't have a reliable water source.

[Table 5 – Water supply statistics]

We investigate whether the rural public schools that are most vulnerable to droughts – without access to the public water network nor a cistern – experience more intense effects from high temperatures. As Figures 13 and 14 show, we did not find any statistically significant difference between those subgroups. This suggests that the effects we capture above do not concern to access to water (droughts) caused by the heat, but from exposure to high temperatures.

5. Conclusion

In this paper, we estimate the effects of high temperature on human capital accumulation, using enrollment outcomes from the Brazilian Northeast. We build on the existing research of impacts from long-run exposure to heat (e.g., Goodman et al., 2018) but unlike previous studies, we focused on *extensive margins*: whether students remain enrolled and progressing through K-12 grades, a salient issue in developing countries.

We show that a one-degree Celsius increase in the daily temperature during the school year leads to an estimated 0.35 percentage points (0.43%) decrease in grade promotion and a 0.26 percentage points (3.86%) increase in dropouts. Using estimators by temperature bins and considering the predicted 2-degree Celsius warming by 2050 (IPCC, 2014), this would mean a 0.67 percentage points decrease in grade promotion and a 0.34 increase in dropouts. We also examined heterogeneity of effects across school characteristics, finding that rural and public schools seem to be more affected by heat. Water vulnerability (e.g., lack of cisterns and connections to public water supply) does not explain the effects of heat exposure on rural schools' grade progression. Additional research is needed to disentangle the precise mechanisms that underlie the observed differences, for both income and cognition channels are plausible.

Understanding the relationship between cumulative heat exposure and grade progression is of policy relevance considering the accelerating warming in most parts of the world. Though our empirical setting is Brazil, our results have implications for other developing countries that, similarly, have a significant gap between enrollment and conclusion rates and a low access to air conditioning. Our findings suggest that heat exposure can further aggravate this gap, hindering skill formation and therefore potentially perpetuating poverty and reducing economic growth.

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Figures

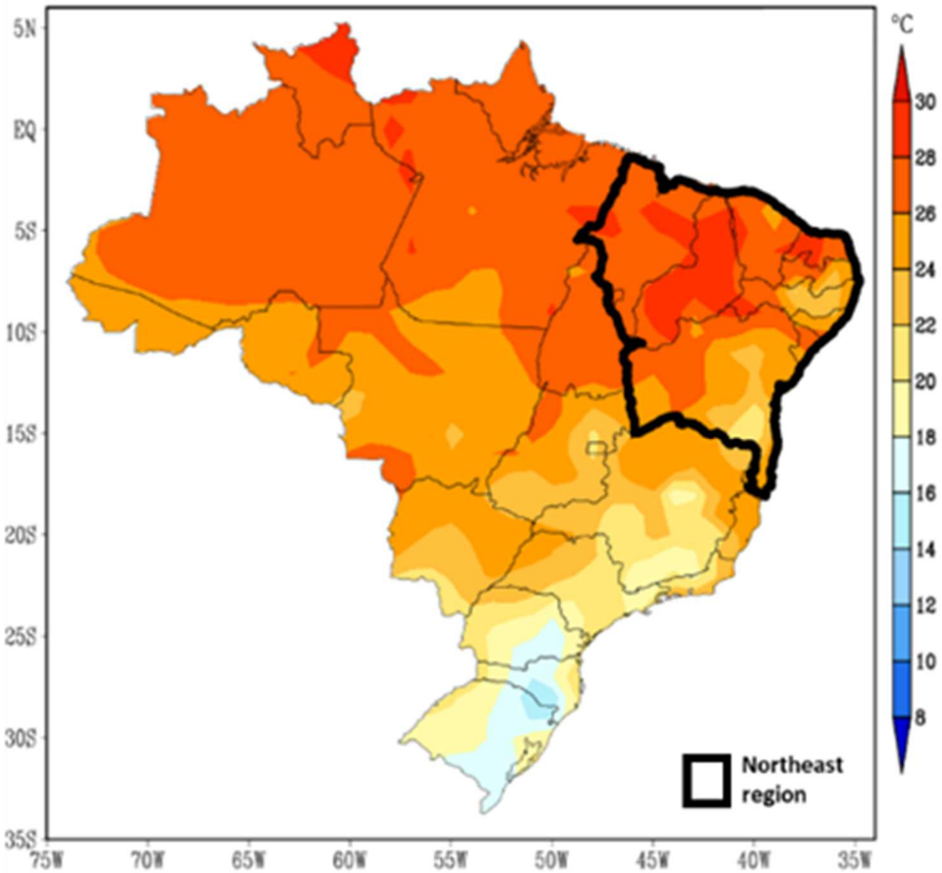

Figure 1 - Brazil mean temperature, 2007-2017 (Source: INMET)

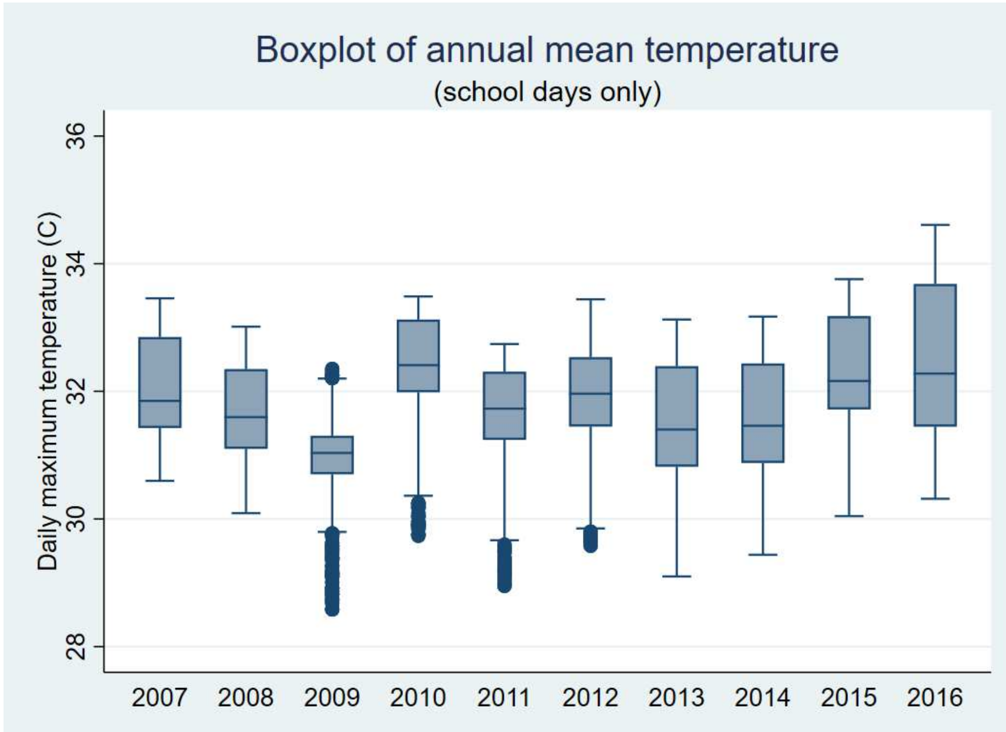

Figure 2 - Boxplot of county annual mean temperature during the school year

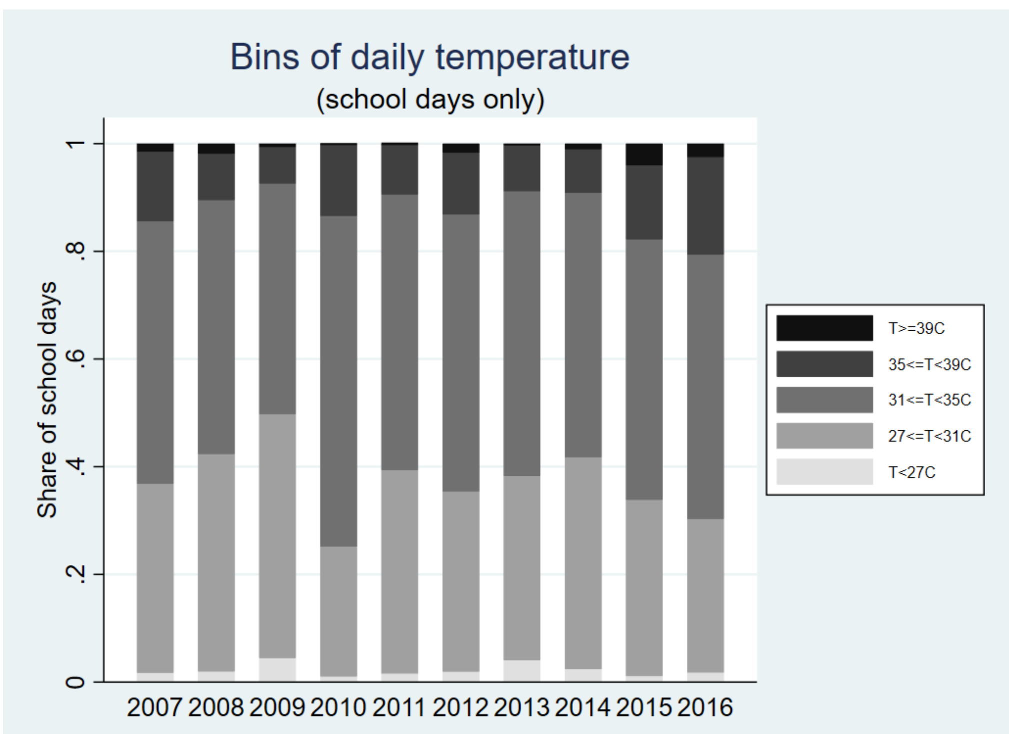

Figure 3 – Share of school days by bins of maximum temperature

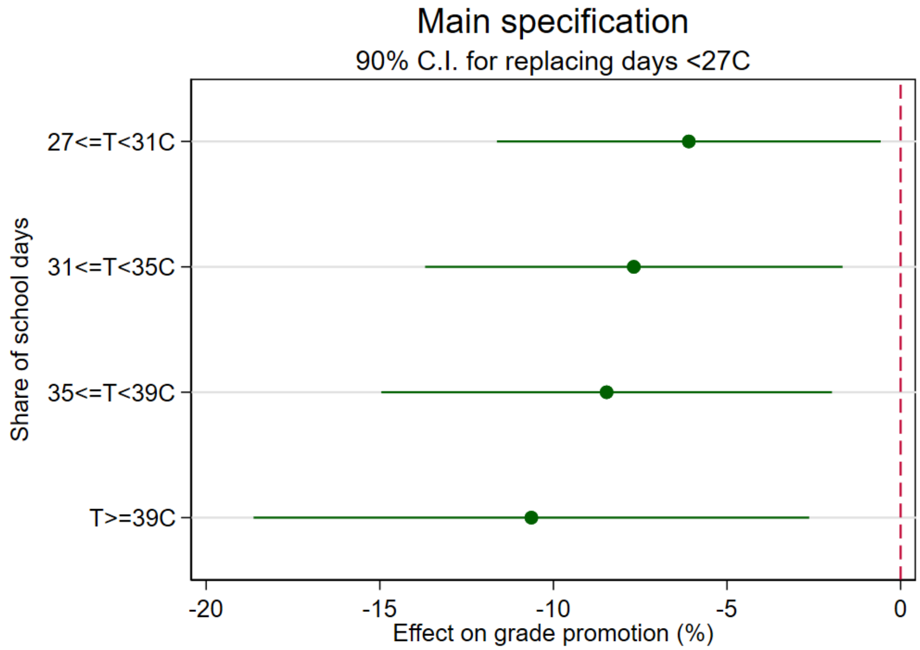

Figure 4 - Coefficients from 4°C temperature bins on grade promotion (Column 1 in Panel B of Table 3)

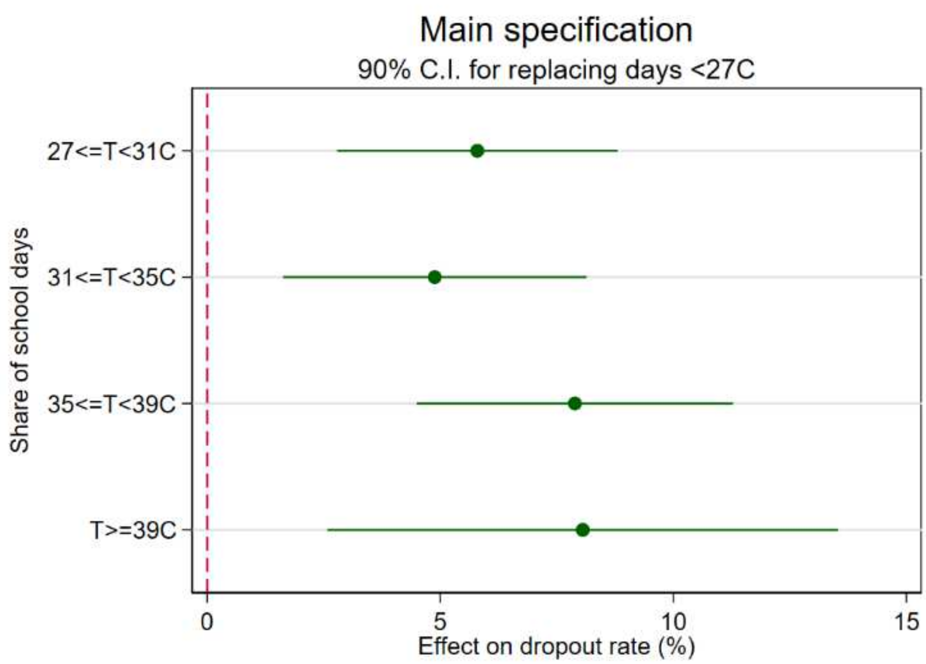

Figure 5 - Coefficients from 4°C temperature bins on dropout rates (Column 1 in Panel B of Table 4)

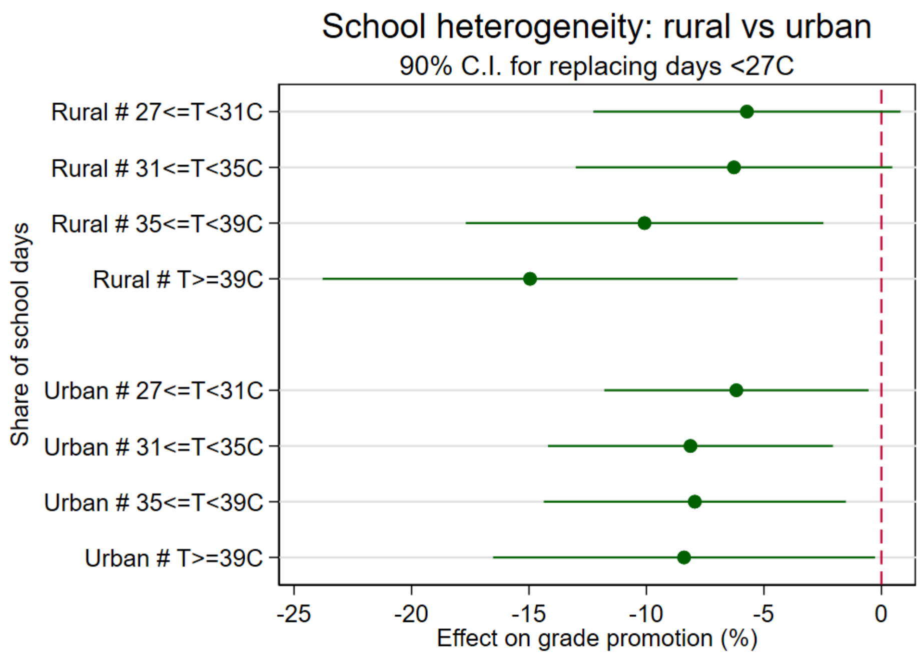

Figure 6 - Coefficients from 4°C temperature bins on grade promotion: heterogeneity rural/urban schools

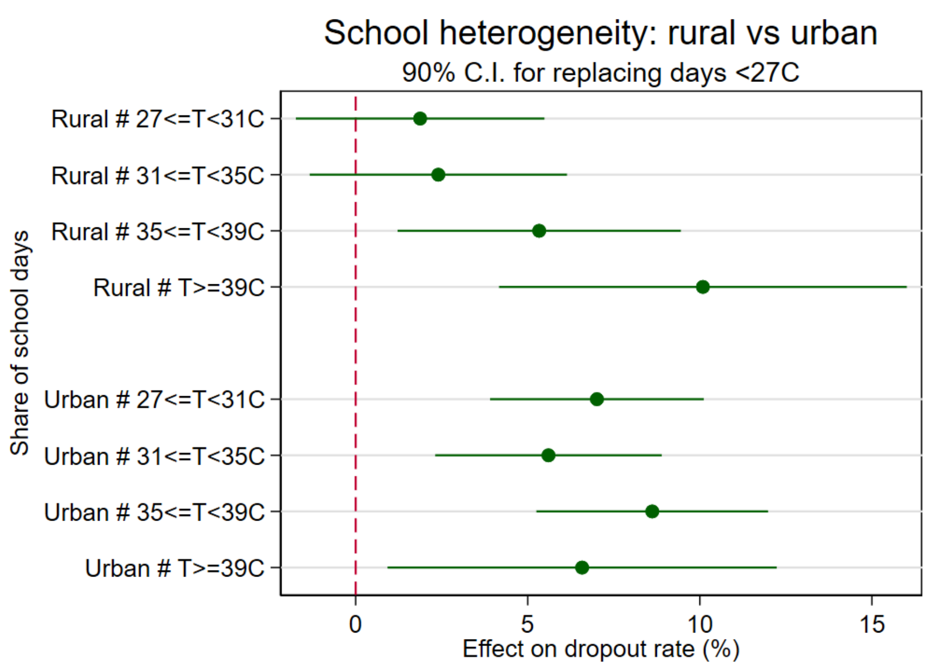

Figure 7 - Coefficients from 4°C temperature bins on dropout rates: heterogeneity rural/urban schools

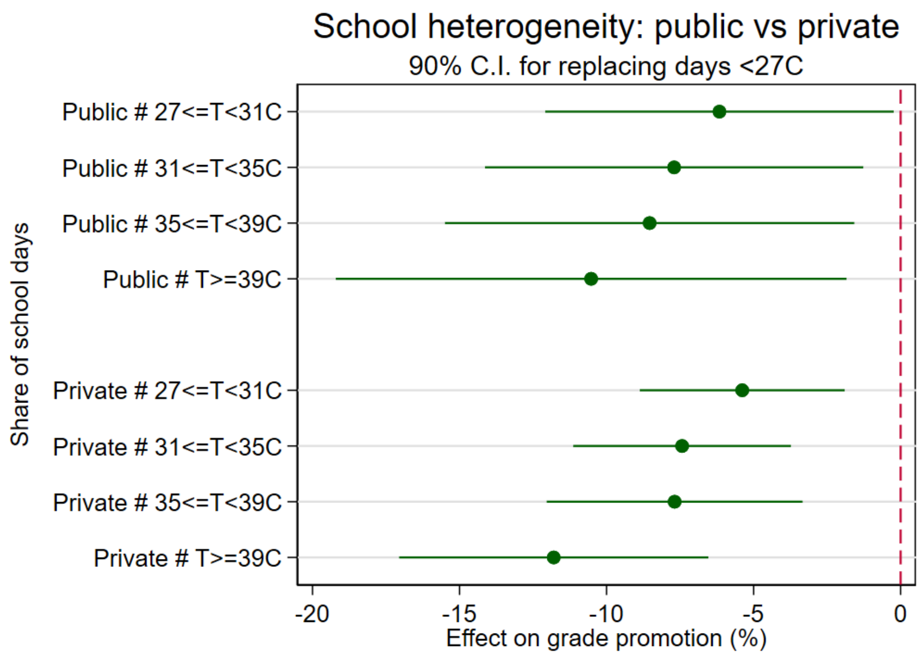

Figure 8 - Coefficients from 4°C temperature bins on grade promotion: heterogeneity public/private schools

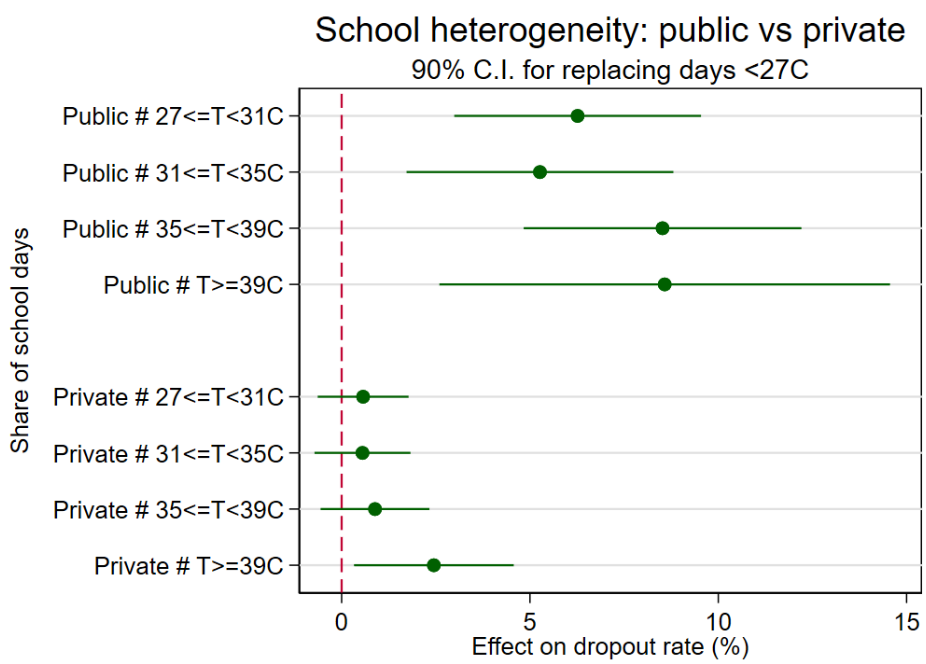

Figure 9 - Coefficients from 4°C temperature bins on dropout rates: heterogeneity public/private schools

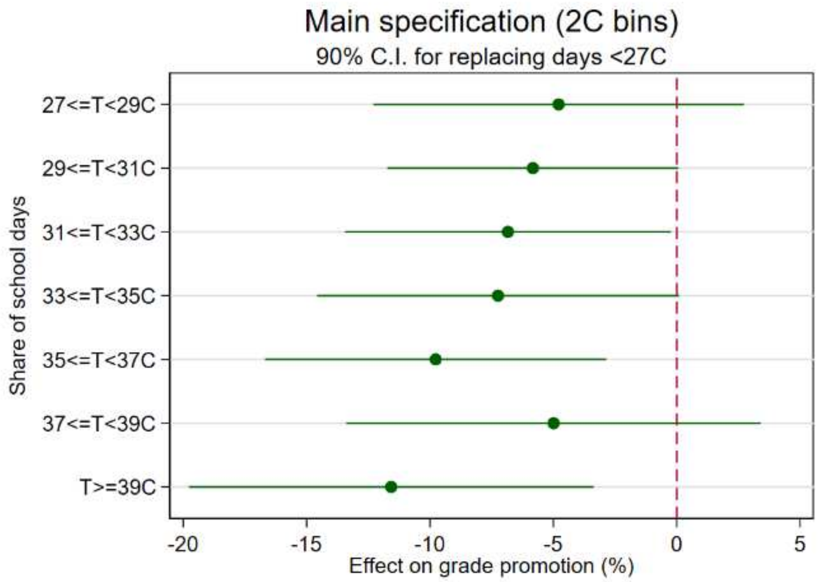

Figure 10 - Coefficients from 2°C temperature bins on grade promotion: robustness check

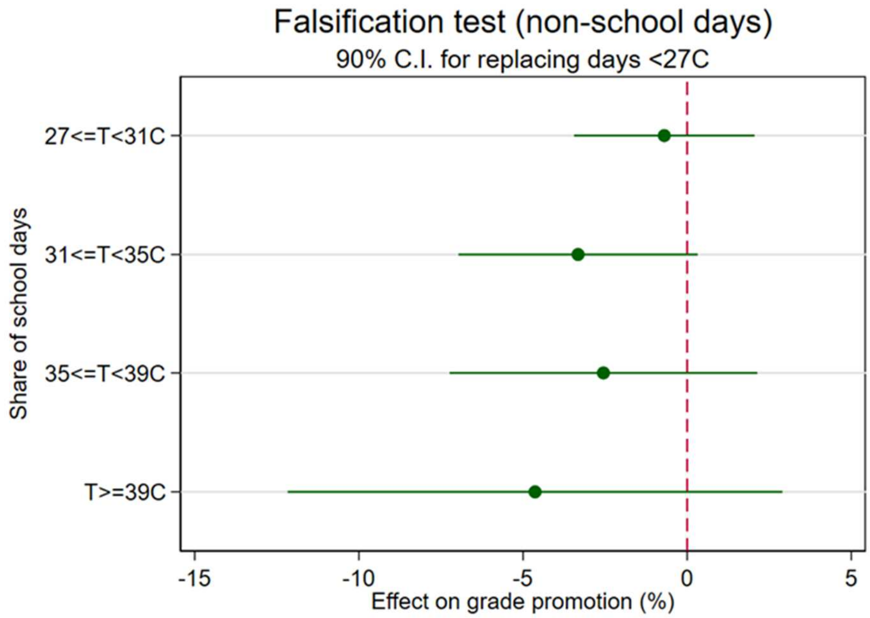

Figure 11 - Coefficients from 4°C temperature bins on grade promotion: falsification test using non-school days

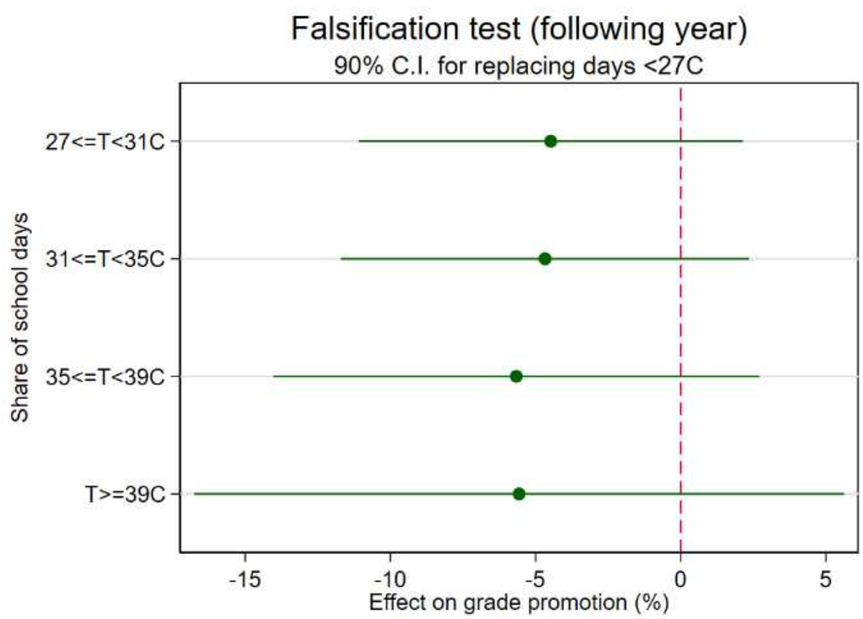

Figure 12 - Coefficients from 4°C temperature bins on grade promotion: falsification test using following year

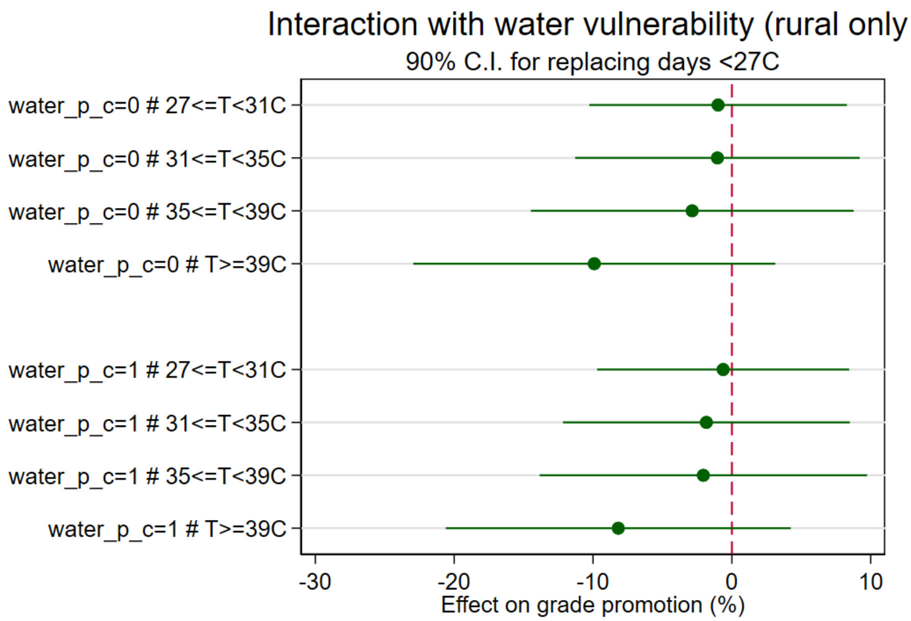

Figure 13 - Coefficients from 4°C temperature bins on grade promotion: by water vulnerability (rural schools only)

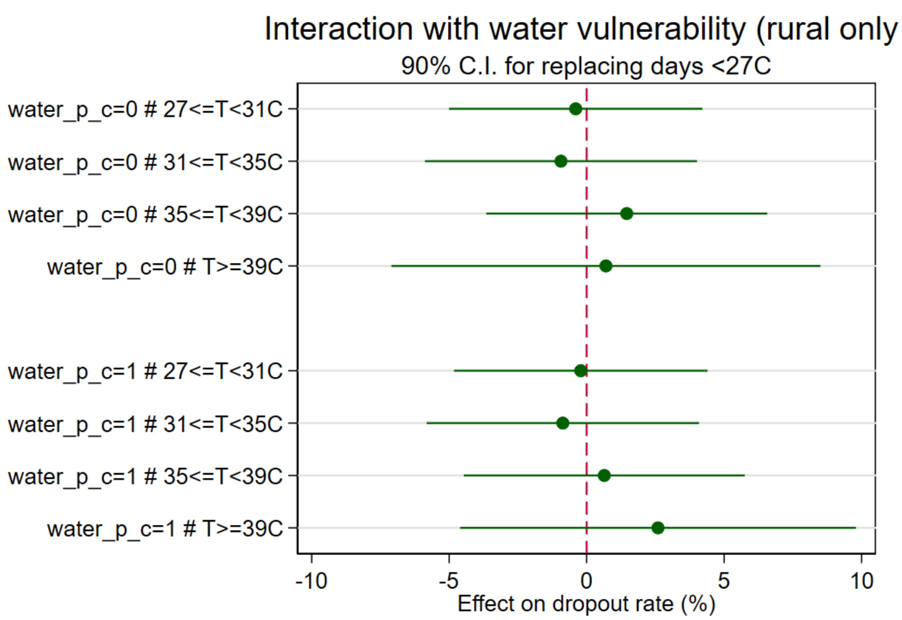

Figure 14 - Coefficients from 4°C temperature bins on dropout rates: by water vulnerability (rural schools only)

Tables

Table 1 - Dataset description (K1-K12, Brazilian Northeast, 2007-2016)

	All schools	By school type			
		Public Rural	Public Urban	Private Rural	Private Urban
	(1)	(2)	(3)	(4)	(5)
Absolute numbers and relative shares					
N observations (school-grade-year)	3,405,517	1,936,043	984,008	8,712	476,754
	100%	57%	29%	0%	14%
N panel units (school-grade)	454,230	262,784	119,988	1,489	69,969
	100%	58%	26%	0%	15%
N unique schools	78,573	48,565	19,349	289	10,370
	100%	62%	25%	0%	13%
N enrollements (millions)	114.2	25.9	74.1	0.2	14.0
	100%	23%	65%	0%	12%

Notes: Absolute numbers and relative shares of dataset descriptive variables are shown. Column 1 comprises all K1-K12 students in the Brazilian northeast from 2007-2016 for which a school-grade appeared at least twice in the 10 year interval (that is, it excludes the roughly 0.3% of enrollments that occurred in singleton observations). Columns 2-5 disaggregates the dataset into four mutually exclusive and collectively exhaustive subgroups of school type.

Table 2 - Summary Statistics (K1-K12, Brazilian Northeast, 2007-2016)

	All schools	By school type			
		Public Rural	Public Urban	Private Rural	Private Urban
	(1)	(2)	(3)	(4)	(5)
Student-weighted statistics *(mean / sd)*					
Enrollment outcomes					
Grade progression (%)	81.1	81.8	78.1	91.6	95.4
	16.6	18.1	15.9	13.0	7.1
Grade retention (%)	12.2	12.9	13.5	6.3	4.1
	12.7	15.3	12.0	11.0	6.6
Drop-outs (%)	6.7	5.3	8.4	2.1	0.5
	9.4	9.0	9.8	6.5	2.3
Enrollment characteristics					
# students/grade	33.5	13.4	75.3	21.1	29.5
	51.7	17.9	74.6	20.5	32.0
# students/class	28.5	22.6	31.1	23.9	26.3
	9.6	8.9	8.2	10.2	11.8
Student gender: male	0.50	0.53	0.49	0.54	0.50
	0.11	0.15	0.09	0.14	0.10
Behind ideal grade-age	0.38	0.41	0.43	0.26	0.11
	0.23	0.24	0.20	0.25	0.10
In a mixed-level grade	0.11	0.26	0.08	0.08	0.05
	0.32	0.44	0.26	0.26	0.23
Teacher characteristics					
Teacher gender: male	0.24	0.22	0.23	0.26	0.26
	0.19	0.23	0.17	0.23	0.19
Teacher age (years)	38.24	35.98	39.54	34.79	35.51
	5.15	5.86	4.47	5.03	4.56
Has tertiary education	0.64	0.43	0.72	0.45	0.60
	0.32	0.36	0.27	0.33	0.30
School characteristics					
Infrastructure score	0.50	0.21	0.57	0.53	0.70
	0.33	0.25	0.30	0.31	0.30

Notes: Mean values and standard deviations of key variables are shown. Infrastructure score refers to the average of five indicators: whether a school has a library, a sports court, a teachers meeting room, a science laboratory and a computer laboratory. Column 1 comprises all K1-K12 students in the Brazilian northeast from 2007-2016 for which a school-grade appeared at least twice in the 10 year interval (that is, it excludes the roughly 0.3% of enrollments that occurred in singleton observations). Columns 2-5 disaggregates the dataset into school types.

Table 3 - Flow Regression Results (K1-K12, Brazilian Northeast, 2007-2016)

dependent variable:	**Grade Promotion**			
	(1)	**(2)**	**(3)**	**(4)**
(A) Avg of max temperature				
Avg of tmax during school year (°C)	-0.36	-0.40	-0.31	-0.31
	(0.18)**	(0.19)**	(0.20)	(0.21)
(B) Bins of max temperature				
Share of days with 27°C ≤ t < 31°C	-6.10	-5.44	-3.04	-3.22
	(3.36)*	(3.48)	(3.94)	(3.94)
Share of days with 31°C ≤ t < 35°C	-7.68	-6.89	-3.46	-3.39
	(3.65)**	(3.89)*	(4.30)	(4.32)
Share of days with 35°C ≤ t < 39°C	-8.47	-8.46	-5.01	-5.36
	(3.94)**	(4.24)**	(4.70)	(4.78)
Share of days with t ≥ 39°C	-10.63	-9.28	-9.42	-8.88
	(4.86)**	(4.89)*	(5.50)*	(5.51)
Specification				
Cohort demographic controls	Yes	No	Yes	No
Analytical weights	Yes	Yes	No	No
N obs (school-grade-year)	3,405,517			

Notes: Heteroskedasticity robust standard errors clustered by municipality are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients in each column come from a regression of hundredths of share of enrolled students on the weather measure(s) shown. Controls include: student demographics (average number of students per classroom, share of males, share of students who are older than the ideal age for their grade, whether the classroom is a mixed-level grade) and school-level characteristics (infrastructure score , average age of teachers, share of male teachers and share of teachers with tertiary education). Analytical weights consider observations according to the average number of enrolled students in a school-grade. Temperature refers to the daily maximum observed in school days in that school year. All regressions include fixed effects for each combination of school network, grade and year and school-grade fixed effects.*

Table 4 - Flow Regression Results (K1-K12, Brazilian Northeast, 2007-2016)

dependent variable:	**Drop-out**			
	(1)	**(2)**	**(3)**	**(4)**
(A) Avg of max temperature				
Avg of tmax during school year (°C)	0.26	0.29	0.18	0.17
	(0.10)**	(0.10)***	(0.09)**	(0.09)*
(B) Bins of max temperature				
Share of days with 27°C ≤ t < 31°C	5.79	5.42	0.53	0.33
	(1.83)***	(1.96)***	(2.21)	(2.21)
Share of days with 31°C ≤ t < 35°C	4.88	4.40	-0.18	-0.48
	(1.98)**	(2.15)**	(2.26)	(2.26)
Share of days with 35°C ≤ t < 39°C	7.89	7.94	1.93	1.79
	(2.06)***	(2.34)***	(2.33)	(2.33)
Share of days with t ≥ 39°C	8.06	7.14	2.53	1.72
	(3.33)**	(3.48)**	(3.54)	(3.65)
Specification				
Cohort demographic controls	Yes	No	Yes	No
Analytical weights	Yes	Yes	No	No
N obs (school-grade-year)	3,405,517			

Notes: Heteroskedasticity robust standard errors clustered by municipality are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients in each column come from a regression of hundredths of share of enrolled students on the weather measure(s) shown. Controls include: student demographics (average number of students per classroom, share of males, share of students who are older than the ideal age for their grade, whether the classroom is a mixed-level grade) and school-level characteristics (infrastructure score , average age of teachers, share of male teachers and share of teachers with tertiary education). Analytical weights consider observations according to the average number of enrolled students in a school-grade. Temperature refers to the daily maximum observed in school days in that school year. All regressions include fixed effects for each combination of school network, grade and year and school-grade fixed effects.*

Table 5 - Water Supply Statistics (K1-K12, Brazilian Northeast, 2007-2016)

	All schools (1)	By school type: Public Rural (2)	Public Urban (3)	Private Rural (4)	Private Urban (5)
Student-weighted statistics *(mean / sd)*					
Water supply: public network	0.83	0.45	0.94	0.43	0.92
	0.38	0.50	0.24	0.49	0.26
Water supply: cistern	0.06	0.23	0.01	0.15	0.01
	0.24	0.42	0.12	0.36	0.12
Water supply: spring / well	0.10	0.27	0.04	0.43	0.06
	0.29	0.44	0.20	0.49	0.24
Water supply: none	0.01	0.06	0.00	0.00	0.00
	0.12	0.23	0.06	0.04	0.02

Note: A few schools report relying in multiple water sources. In those cases, we considered them in the least vulnerable to drought category (public network > cistern > spring/well > none).

Appendix

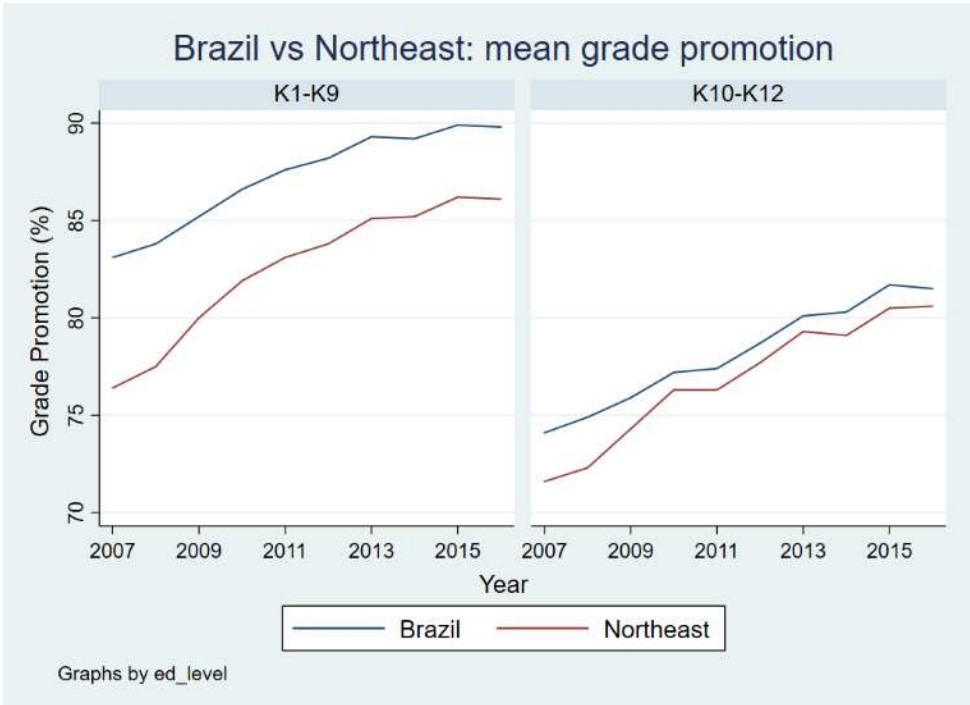

Figure A.1 – Grade promotion in Brazil and Northeast Brazil (2007-2016). Source: INEP

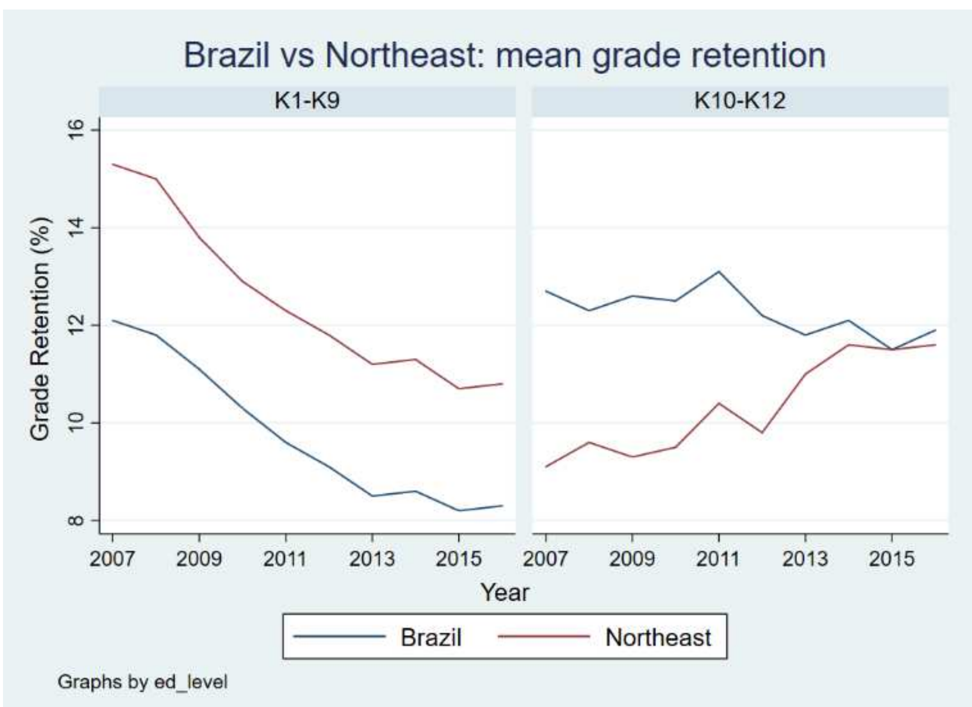

Figure A.2 – Grade retention in Brazil and Northeast Brazil (2007-2016). Source: INEP

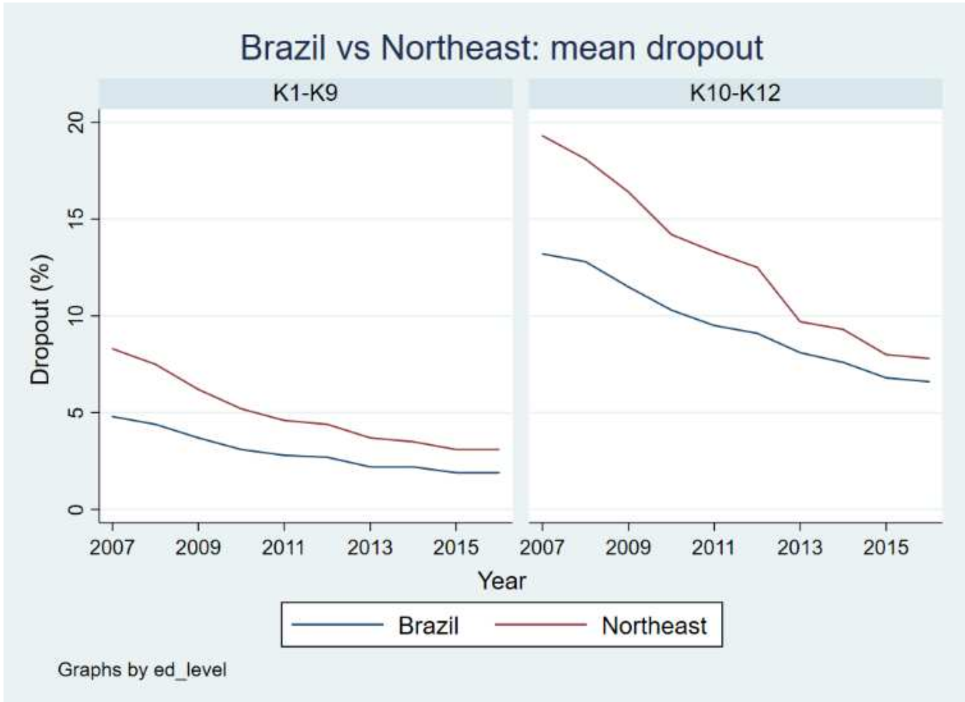

Figure A.3 – Dropout rates in Brazil and Northeast Brazil (2007-2016). Source: INEP

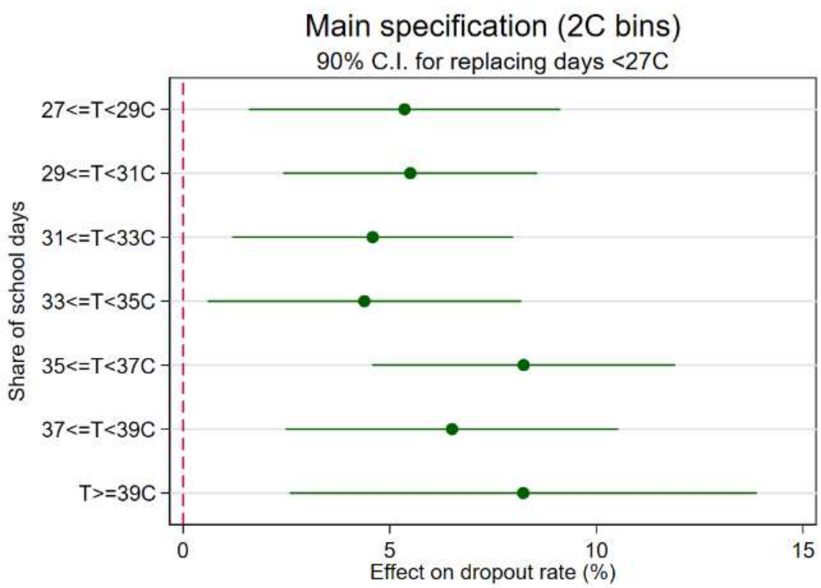

Figure A.4 - Coefficients from 2°C temperature bins on dropout rates: robustness check

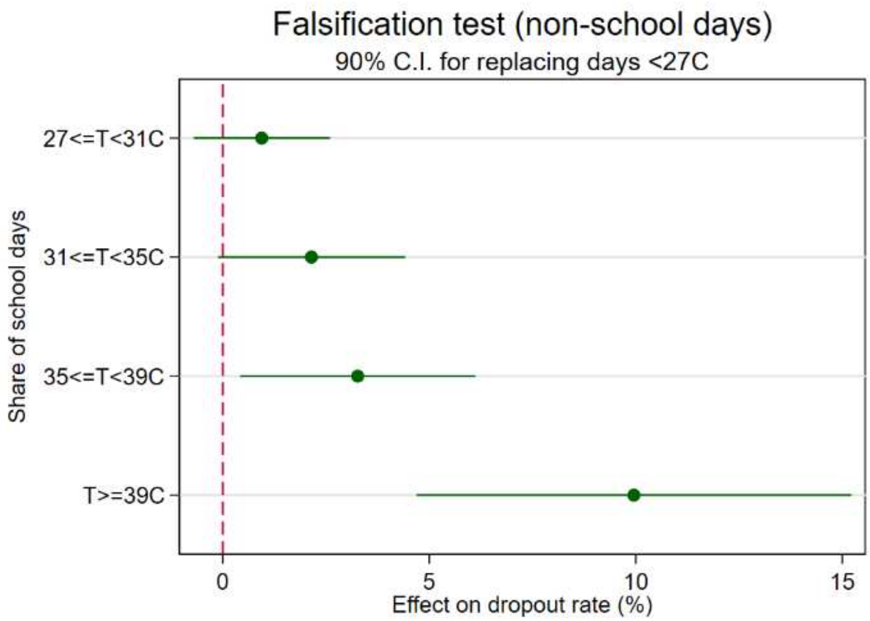

Figure A.5 - Coefficients from 4°C temperature bins on grade promotion: falsification test using non-school days

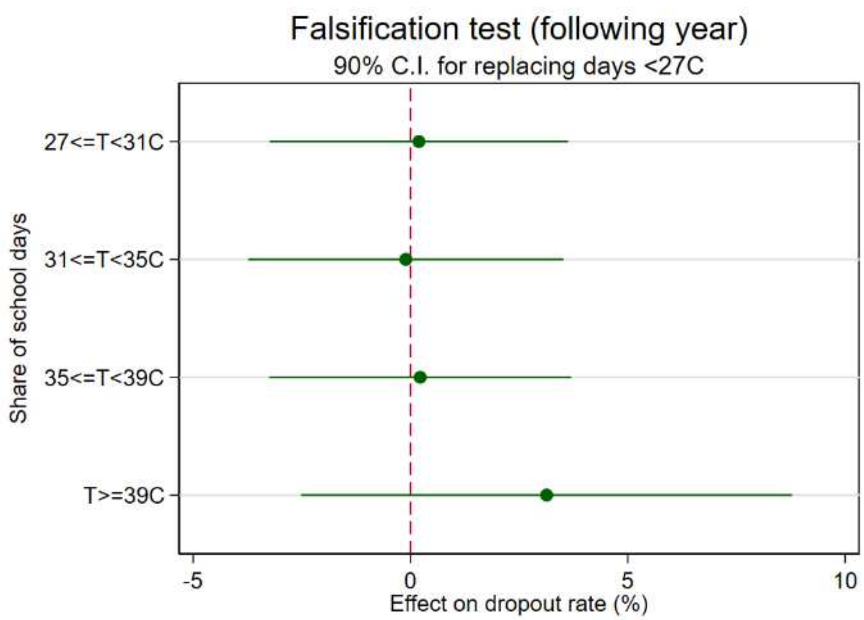

Figure A.6 - Coefficients from 4°C temperature bins on grade promotion: falsification test using following year

Table A.1 - Flow Regression Results (K1-K12, Brazilian Northeast, 2007-2016)

dependent variable:	Grade Retention			
	(1)	**(2)**	**(3)**	**(4)**
(A) Avg of max temperature				
Avg of tmax during school year (°C)	0.10	0.11	0.13	0.14
	(0.16)	(0.17)	(0.17)	(0.18)
(B) Bins of max temperature				
Share of days with 27°C ≤ t < 31°C	0.31	0.02	2.51	2.89
	(2.56)	(2.54)	(2.91)	(2.95)
Share of days with 31°C ≤ t < 35°C	2.80	2.48	3.64	3.88
	(2.75)	(2.78)	(3.20)	(3.25)
Share of days with 35°C ≤ t < 39°C	0.58	0.52	3.08	3.57
	(3.27)	(3.30)	(3.59)	(3.69)
Share of days with t ≥ 39°C	2.58	2.15	6.88	7.16
	(4.00)	(3.95)	(4.34)	(4.43)
Specification				
Cohort demographic controls	Yes	No	Yes	No
Analytical weights	Yes	Yes	No	No
N obs (school-grade-year)	3,405,517			

Notes: Heteroskedasticity robust standard errors clustered by municipality are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients in each column come from a regression of hundredths of share of enrolled students on the weather measure(s) shown. Controls include: student demographics (average number of students per classroom, share of males, share of students who are older than the ideal age for their grade, whether the classroom is a mixed-level grade) and school-level characteristics (infrastructure score , average age of teachers, share of male teachers and share of teachers with tertiary education). Analytical weights consider observations according to the average number of enrolled students in a school-grade. Temperature refers to the daily maximum observed in school days in that school year. All regressions include fixed effects for each combination of school network, grade and year and school-grade fixed effects.*

Table A.2 - Heterogeneity (K1-K12, Brazilian Northeast, 2007-2016)

	Coefficients for bins of max temperature				
interaction terms:	**None**	**Rural**	**Urban**	**Public**	**Private**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
(A) Grade Progression					
Share of days with 27°C ≤ t < 31°C	-6.10	-5.72	-6.17	-6.16	-5.39
	(3.36)*	-3.97	(3.42)*	(3.60)*	(2.12)**
Share of days with 31°C ≤ t < 35°C	-7.68	-6.27	-8.13	-7.7	-7.43
	(3.65)**	-4.09	(3.69)**	(3.91)**	(2.25)***
Share of days with 35°C ≤ t < 39°C	-8.47	-10.08	-7.94	-8.53	-7.69
	(3.94)**	(4.63)**	(3.91)**	(4.23)**	(2.64)***
Share of days with t ≥ 39°C	-10.63	-14.95	-8.4	-10.52	-11.8
	(4.86)**	(5.37)***	(4.94)*	(5.28)**	(3.19)***
(A) Dropout Rate					
Share of days with 27°C ≤ t < 31°C	5.79	1.87	7.01	6.27	0.57
	(1.83)***	(2.19)	(1.89)***	(1.99)***	(0.73)
Share of days with 31°C ≤ t < 35°C	4.88	2.40	5.60	5.27	0.55
	(1.98)**	(2.27)	(2.00)***	(2.15)**	(0.77)
Share of days with 35°C ≤ t < 39°C	7.89	5.33	8.62	8.52	0.89
	(2.06)***	(2.50)**	(2.05)***	(2.24)***	(0.88)
Share of days with t ≥ 39°C	8.06	10.09	6.58	8.58	2.45
	(3.33)**	(3.60)***	(3.44)*	(3.63)**	(1.29)*
Specification					
Cohort demographic controls	Yes	Yes	Yes	Yes	Yes
Analytical weights	Yes	Yes	Yes	Yes	Yes
N obs (school-grade-year)			3,405,517		

Notes: Heteroskedasticity robust standard errors clustered by municipality are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients in each column come from a regression of hundredths of share of enrolled students on the weather measure(s) shown. Controls include: student demographics (average number of students per classroom, share of males, share of students who are older than the ideal age for their grade, whether the classroom is a mixed-level grade) and school-level characteristics (infrastructure score , average age of teachers, share of male teachers and share of teachers with tertiary education). Analytical weights consider observations according to the average number of enrolled students in a school-grade. Temperature refers to the daily maximum observed in school days in that school year. All regressions include fixed effects for each combination of school network, grade and year and school-grade fixed effects.*

The consequences of chronic exposure to community violence for children's academic progression in Rio de Janeiro, Brazil

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Abstract

This paper explores the effects of cumulative exposure to urban violence on grade retention and on-time grade completion of children attending elementary schools in Rio de Janeiro, Brazil. Relying on longitudinal data on crime and educational outcomes for five cohorts of children attending municipal schools in the city (n = 256,961), we find that a one standard deviation increase in homicides rate in the school neighborhood raises the likelihood that a child will be retained in grades K1-K4 by 0.42 percentage points any given year. We further explore the consequences of continued exposure, over multiple years, to high violence rates. We estimate that a single year of exposure to high violence during the initial four years of elementary school leads to a 6.9 percentage points reduction in the probability of reaching fifth grade on time. The effect is roughly twice as large for children facing four continuous years of high violence, suggesting that the deleterious impacts of prolonged exposure to violence do not accrue linearly.

Keywords: education, violence, on-time grade progression, economics.

1. Introduction

While violence is a threat facing many communities across the globe, certain regions are disproportionately affected. In 2017, Latin America was home to 41 of the 50 most violent cities in the world and Brazil, in particular, was home to 17 of these cities (Consejo Ciudadano para la Seguridad Pública y Justica Penal A.C., 2018). In one of Brazil's most well-known metropolises, Rio de Janeiro[20], rising violence levels motivated a rare takeover by the federal military in early 2018 (Lopes, 2018; BBC, 2018). Rio's uptick in violence over the last several years has also resulted in frequent closures of schools due to shootouts in their surrounding communities (Londoño, 2017). It therefore stands to reason that children's learning is likely an additional causality of Rio's violence.

Indeed, a growing body of research conducted in Rio and elsewhere shows that exposure to community violence has adverse consequences for a host of child outcomes, including those related to schooling and academics (Molano, Harker, & Cristancho, 2018; Monteiro & Rocha, 2017; Sharkey, Schwartz, Ellen, & Lacoe, 2014). Beyond the effects of violence on the availability of education, insights from the biological sciences suggest that traumatic events activate the brain's stress response system, and that repeat or sustained activation of that system negatively affects children's cognitive and emotional functioning (Foster & Brooks-Gunn, 2009; National Scientific Council on the Developing Child, 2014; Sharkey, 2018).

Existing studies of the effect of violence on student outcomes fall into two categories. They analyze either the acute effects (i.e., direct exposure to singular violent events), or the chronic

[20] From now on this paper will refer as Rio de Janeiro or Rio to the area comprised by the municipality of Rio de Janeiro. When it talks about the State it will refers as the State of Rio de Janeiro.

effects of violence occurring within a set time span (i.e., one academic year). The potential impacts of prolonged exposure to violence (i.e., over multiple years) remains a less explored topic, which is the gap this paper aims to address. Descriptive evidence shows that children exposed to multiple risk factors or that endure prolonged socioeconomic adversity have lower academic outcomes, suggesting that prolonged exposure to violence might also be a relevant concern (Edwards, Holden, Felitti, & Anda, 2003; Jimenez, Wade, Lin, Morrow, & Reichman, 2016; Michelmore & Dynarski, 2017). However, two competing hypotheses are plausible: (i) continued exposure to community violence might compound to an even greater impact on children's functioning than short-term exposure, or (ii) children may develop adaptive responses to chronic violence in such a way that continued exposure to violence causes diminishing marginal damage. We are particularly interested on the effects of violence on grade retention, for this is a salient issue in Brazil, where over 35% of 15-year old students assessed in PISA reported repeating at least one grade, the second highest rate of retention among the 72 participating countries (OECD, 2016).

To investigate the cumulative effects of exposure to violence, we rely on longitudinal data on crime and educational outcomes for five cohorts of children attending municipal schools in Rio de Janeiro. We construct a panel with students' grade progress during their first four years in elementary school. From the 256,961 children in our sample, over 30% fail to reach fifth grade on time. We observe violence at the school-neighborhood level and use school- and flexible cohort-fixed effects to compare the academic progress of children attending the same schools but exposed to distinct amounts of violence over time. Our identification strategy relies on the assumption that unobserved determinants of grade progression in any specific school are uncorrelated with year-to-year variation in school-neighborhood violence.

During 2007-2016, the average violent crime rate in Rio was 33.6 deaths per 100,000 inhabitants[21], with substantial variation across its neighborhoods (standard deviation of 33.7). We measure how many years a student attended school in high-violence neighborhoods, in which high-violence refers to standardized violence rates above certain thresholds (0.5, 1.0 or 1.5 SD above the sample mean). To put those numbers in perspective, in 2017, only 4 US cities with population above 250 thousand recorded homicide rates above Rio's average, just 2 reached our lowest threshold of 0.5 SD above the mean, or 51.6 violent deaths per 100,000 inhabitants, and none reached the higher thresholds (FBI, 2018). In contrast, 47% of students in our sample have been exposed to high-violence at least one year, and 8% during all their four initial years of elementary school.

We find that a one standard deviation increase in violence raises the likelihood that a child will be retained in elementary grades (i.e., grades K1-K4) by 0.42 percentage points. We additionally explore whether violence seems to matter more in earlier years when children might be more sensitive to violence exposure. While we find that the point estimates of the impacts of violence across the elementary grades are similar, these estimates are less precisely estimated in the later grades. We also find evidence of a negative relationship between mean school-neighborhood violence rates and drop-outs in elementary school. Finally, we explore the consequences of continued exposure, over multiple years, to high violence rates for grade retention and the likelihood that children complete elementary school on time. Considering the threshold

[21] Deaths from aggression. This indicator is composed of the following offenses: murder and nonnegligent manslaughter, homicides resulting from police action, robbery or aggravated assault followed by death. Transportation fatalities are not included. Throughout this paper, the terms homicides, violent deaths and violent crimes, refer interchangeably to those same categories.

for high violence as 1 standard deviation above Rio de Janeiro neighborhood mean, we find that a single year of exposure to high violence during the initial four years of elementary school leads to a 6.9 percentage points reduction in the probability of reaching fifth grade on time. The effect is approximately twice as large for children facing four years of high violence (i.e., these students were 14.9 percentage points more likely to be behind ideal grade than students never exposed to high violence). We cannot separate the mechanisms through which violence is affecting academic progress, and those results are interpreted as estimated net effects, including direct effects on children and indirect effects through teachers, principals and school climate. Our findings suggest that negative impacts from continued exposure to violence exceeds the effects from short-term exposure, while allowing us to reject the hypothesis of a linear relationship between violence exposure and educational outcomes.

The rest of the paper unfolds as follows. Section 2 presents the related literature. Section 3 provides contextual information and describes the data and sources. Section 4 presents this study's empirical strategy and its results. Section 5 discusses implications and concludes.

2. Related literature

2.1. Acute and chronic exposure to violence

The negative effects of acute violent incidences on children's academic and cognitive outcomes is well documented (McCoy, Raver, & Sharkey, 2015; Sharkey, 2010; Sharkey et al., 2014). In particular, these studies compare the outcomes of children in the same or similar communities, some of whom were exposed to a violent incident immediately before the assessment (the "treated") and some of whom were exposed to a violent incident immediately after or long

before the assessment (the “control”). This approach assumes that the timing of a crime relative to when the assessment took place is exogenous in order to make causal claims about the effect of individual incidents. Sharkey (2010) exploits the random timing of assessments and violent crimes to compare children living in the same communities in Chicago with different exposure to homicide right before the assessment. He finds that in the immediate aftermath of a violent crime (e.g., within a week) there is an adverse but temporal shock to the vocabulary and reading achievement of African American children of approximately 0.5 and 0.7 SD, respectively.

Another strand of literature has focused on the chronic effects of violence occurring within a set time span, though establishing a causal relation between community violence and academic outcomes poses important challenges. Work that explores the observed association between violence levels and academic achievement across different communities risks reporting spurious correlations, for violent incidents tend to occur in areas marked by poverty and other risk factors that could also drive the negative academic achievement. However, recent work exploiting data collected in the same communities over time allows researchers to circumvent this challenge, and has proven that exposure to violent incidents has adverse causal effects on academic outcomes (Monteiro & Rocha, 2017; Sharkey, 2010; Sharkey et al., 2014; Burdick-Will, 2016). Instead of studying the short-term effect of individual violent occurrences, these studies seek to understand the consequences of exposure to violence across a given time period. Burdick-Will (2016), for instance, uses student fixed-effects to estimate the medium-term effects of exposure to violent crimes (i.e., the number of violent crimes in the student’s census block group during a school year) on statewide exams conducted between third and eleventh grade in Chicago. She finds that early exposure has negative effects on math and reading achievement across grades, with these negative

impacts building on each other over time. Also using a fixed effects approach, Monteiro & Rocha (2017) exploit variation across three years in gang conflict in the neighborhoods surrounding schools in Rio de Janeiro. They find that in the years in which the school was exposed to at least two days of conflict in a favela within at least 250 meters of the school, children's math and Portuguese achievement was 0.05 and 0.03 SD lower on average, respectively. Significantly negative results are also found on studies that analyze the long-term effects on human capital accumulation from civil wars (León, 2012; Chamarbagwala and Morán, 2011).

2.2. Mechanisms

Community violence may negatively impact children's academic outcomes through a variety of mechanisms (Burdick-Will, 2016; Leventhal & Brooks-Gunn, 2000). In extreme situations like Rio, violence may have practical implications, preventing children or teachers from being able to attend school. Violence may also affect children's academic functioning through its effect on children's physiological and mental wellbeing (Burdick-Will, 2016; Shonkoff et al., 2012). Typically, the body's stress response is a healthy protective reaction to danger but exposure to intense or multiple stressors elevates stress hormones to levels that can weather the body's organs, including the brain (Gould, McEwen, Tanapat, Galea, & Fuchs, 1997; Shonkoff et al., 2012). These neurobiological changes are thought to then influence how children understand and interact with their surrounding environments (McCoy, 2013). While little work explicitly connects these biological processes to observable child behaviors, some work hypothesizes that such intensified stress responses affect children's self-regulation and mental health (McCoy, Roy, & Raver, 2016; Molano et al., 2018). Regulatory-related skills are in turn thought to undergird children's ability to engage successfully in school (Blair & Razza, 2007; McClelland & Cameron, 2011).

Violence may also affect the schools and adults responsible for delivering education to children. School climate, or the nature of the school environment, may be one mechanism through which violence adversely affects student achievement (McCoy, Roy, & Sirkman, 2013). Additionally, educators themselves may experience adverse physiological and mental consequences of violence exposure, which in turn could have consequences for their work with students. Whereas less research has studied the consequences of violence on teachers, more recent work documenting the adverse consequences of community violence on parental wellbeing and behaviors suggest that violence could also affect teachers' wellbeing and behaviors with students (Cuartas, McCoy, & Molano, 2018).

2.3. On-time grade progression

Most work estimating the effects of violence on academic outcomes has focused exclusively on student achievement (e.g., math or reading achievement). Less work has documented the consequences of violence for other academic outcomes like grade retention or on-time grade completion, additional indicators of academic success.

Retaining students in the same grade is costly in terms of additional per pupil spending and foregone earnings if, as a result of being held back, students spend an additional year in full-time education. Moreover, retention is found to increase the probability that children will drop out of school (Jacob & Lefgren, 2009; Manacorda, 2012). In Uruguay, Manacorda (2012) estimates that grade failure leads to dropout and lower educational attainment on the order of -0.8 school years. Several papers also suggest that grade repetition has adverse consequences for children's long-term academic performance (Eide & Showalter, 2001; Gomes-Neto & Hanushek, 1994). Specifically, in a sample of U.S. elementary school students, those that were retained learned 7%

less by age 11 than their peers who had not been retained (Fruehwirth, Navarro, & Takahashi, 2016). Violence could plausibly affect retention and on-time grade progression through two primary mechanisms. First, violence could affect children academic achievement, making them academically unprepared for the next grade. Second, violence could prevent students from attending a sufficient amount of school required for promotion. These consequences on attendance could also exacerbate the effects on achievement.

3. Context and Data

Our analysis examines children's grade progression in municipal schools in Rio de Janeiro in relation to community violence around their school. We first describe the context and each of the data sources, sample and procedures used in more detail.

3.1. Violence in Rio de Janeiro: context

Rio de Janeiro is Brazil's second largest city and considered the cultural hub of the country. Violent crime has been a problem in Rio for many decades (Couto, Ruediger, & Novis, 2017). Lethal violence disproportionately affects young black men living in slums (Soares & Ribeiro, 2018), and the victims of gun fights among the city's warring gangs and police are often caught in the crossfire. Though the introduction of *Pacifying Police Units* was followed by a temporary reduction in violence, particularly of police violence (Magaloni, Franco, & Melo, 2015), the situation deteriorated in recent years, consistent with studies linking negative economic shocks with increased criminal activity (Dix-Carneiro, Soares, & Ulyssea, 2018).

The number of violent homicides in Rio de Janeiro rose by 22% between 2015 and 2016 and by 12% between 2016 and 2017 (ISP, 2018). In 2017, 2,131 individuals were violently killed in the city, 134 of whom were children under the age of 18 (ISP, 2018). To put this figure in perspective, the city of Rio de Janeiro has approximately 2.2 million or 25% fewer residents than New York City but experienced over seven times the number of homicides that took place in New York City in 2017 (Honan, 2018).

For those who live in the city, this violence has resulted in constant fear and anxiety for physical safety (Pearson & Magalhaes, 2018; Reuters, 2018). This recent rise in crime rates has motivated a federal military takeover of security in the city (Lopes, 2018). It has also affected schools, with nearly 200,000 of the city's students missing at least some days of school in 2017 due to violence-induced closures (Londoño, 2017).

3.2. Violence Data

We construct a ten-year longitudinal dataset on neighborhood violence using information on all murders in Rio de Janeiro made available by the state agency for public safety (*Instituto de Segurança Pública*, ISP). The municipality is currently divided in 162 neighborhoods and 42 police service areas (PSA, *Circunscrições Integradas de Segurança Pública*). We consolidated the 162 official neighborhoods into 137 neighborhoods by merging some[22], to accurately attribute the crime microdata received from ISP at the least aggregated possible level[23]. Each of those 137

[22] The merging criteria and the resulting map of neighborhood is described in Appendix Figure A1.

[23] For approximately 14% of the cases, the neighborhood information was not observed (9%) or could not be matched to a valid neighborhood (5%), their PSA being the lowest-level geographic information available. We distributed those cases proportionally to all the neighborhoods contained in each PSA, preserving the share of cases each neighborhood represented within a PSA over the whole period.

neighborhoods has, on average, 45.7 thousand inhabitants and an area of 8.8 square kilometers[24]. Then, we used annual population estimates[25] to create a panel of neighborhood-level violent crime rates, the number of violent deaths per 100,000 inhabitants, observed yearly for 2007-2016.

Figure 1 presents the distribution of violent crime rates by neighborhood-year: the boxes include the neighborhoods in the 25th to 75th percentiles, the horizontal line in each box represents the median neighborhood-year and the circle represents the municipal average, weighted by population. This figure shows that across the included years, average violence rates were between approximately 24.3 and 53.8 deaths per 100,000 inhabitants. The number of outliers depicted in the box plots by dots above the 75th percentile also shows that there are many neighborhoods with high violence rates. Figure 2 displays Rio de Janeiro neighborhoods according to their average violent crime rate in the period.

In our analyses, our primary predictors of interest are the school-neighborhood violent crime rates, standardized for the average of 33.6 and standard deviation of 33.7 deaths per 100,000 inhabitants exhibited in 2007-2016. Alternatively, we measure how many years a student attended school in high-violence neighborhoods, in which high-violence refers to standardized violence rates above certain thresholds in each given year (0.5, 1.0 or 1.5 SD above the sample mean). Those thresholds correspond to rates of above 51.6, 69.8 and 88.0 violent deaths per 100,000 inhabitants.

[Figure 1: boxplot]

[Figure 2: violence map]

[24] Comparable to Chicago's official 77 community areas, which average 37.6 thousand inhabitants and 7.9 km^2.

[25] Interpolating and extrapolating the 2000 and 2010 population census by the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, IBGE).

3.3. Education in Rio de Janeiro: context

The provision of public education in Rio de Janeiro is the responsibility of both the municipality and state. Whereas the municipality is in charge of the elementary and middle school grades (i.e., K1-K9), the state is responsible for high school (i.e., K10-K12). Of elementary school aged children in Rio de Janeiro in 2017, 64% attend public municipal-funded schools (INEP, 2018)[26]. Public school students' performance in national standardized assessments is, on average, 84% that of children in private schools for fifth graders (INEP, 2018). Though municipal school students in Rio de Janeiro perform slightly better than the national average, only 60% of fifth-graders exhibit satisfactory Portuguese levels and 49% reach satisfactory mathematics levels (INEP, 2018)[27].

A large proportion of children in Brazil are retained. According to figures from the 2015 Program for International Student Assessment (PISA), more than 35% of students in Brazil reported repeating at least one grade, the second highest rate of retention among the 72 participating countries (OECD, 2016). Specifically, in elementary school, 15.9% of students reported repeating one grade and 4.5% reported repeating two or more grades. For comparison, 8.6% of students in the United States were retained in elementary school and only 0.3% had been retained twice or more. In most countries participating in the PISA, children from low socio-economic status households were more likely to be retained, however, this was not true of children in Brazil, as socio-economic status was not a significant predictor of being retained (OECD, 2016).

[26] According to the 2017 School Census, of the 406,000 enrollees in K1-K5 in Rio de Janeiro city 64% were in municipal schools, 34% were in private schools, and 2% were in state or federal schools (INEP, 2018).

[27] According to Prova Brasil 2017 results. Private school results are not nationally representative, for only public schools are obliged to take part in the bi-annual assessment.

As in the rest of the country, retention rates in Rio de Janeiro are high. In 2016, the average retention rate in municipal schools was 6.3% in first through fifth grades (i.e., K1-K5), reaching 15.9% in third grade (i.e., K3) (INEP, 2018). As a consequence, only 58% of children reach sixth grade at the ideal age of eleven years old or younger (INEP, 2018). Children can be retained due to significant absences in any grade, but failure to meet academic standards does not lead to retention in first or second grades, because Rio de Janeiro adopts a cycle-based promotion policy for K1-K3[28]. The decision to retain a child is made by the school's principal in consultation with the child's classroom teacher, and parents have little say in the decision. In Rio de Janeiro, community violence could therefore potentially affect both the attendance and academic performance pathways to retention. In the former case, children could miss more school if parents are hesitant to send their children to school in violent contexts. In the latter case, community violence could affect violence either through additional absences (i.e., fewer learning opportunities) or through psychological consequences that inhibit student learning.

3.4. Education Data

The education data comes from the School Census (*Censo Escolar*)[29], an annual survey of every school in Brazil made available by the National Institute for Research on Education (*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira*, INEP). We follow the

[28] In Brazil, schools may adopt grade-based promotion (i.e., retention is possible in any grade) or cycle-based promotion (i.e., students should only be retained at end of cycles). States and municipalities have leeway to decide on their promotion policy, as well as to establish cycles in case they opt for cycle-base promotion. In Rio de Janeiro, the K1-K3 cycle was implemented in 2000 and remains currently in place. In 2007, two additional cycles were created, for K4-K6 and K7-K9, both eliminated in 2009, reverting K4-K9 in Rio de Janeiro to grade-based promotion (Alves, 2012).

[29] This is a reliable source of information because the federal government frequently checks and audits the information reported in the School Census, for a large share of the public educational budget is based on its enrollment figures.

academic progression of the 256,951 students that were first enrolled in elementary education between the ages of 5 and 7 in a municipal school in Rio de Janeiro between 2008 and 2012. Using information on *all* the K-7 enrollments reported in the School Census from 2007 to 2017 (public and private; in the whole country), we verify whether those students had previously enrolled in preschool and how they progressed through elementary school, correcting for missing data[30].

[Table 1: describing cohorts timeline]

We construct a panel with students' grade progress during their first four years in elementary school. Ideally, this period should correspond to being incrementally promoted from K1 to K5, which is the case for only 69.7% of the children in our sample (65.9% enroll in K5 on time in Rio and 3.8% enroll in K5 on time but moved to other municipalities). The remaining proportion of students are enrolled in lower grades (26.9%), suggesting they have been retained or abandoned school at least once, or are not enrolled in any Brazilian school in what should be their fifth year of elementary school (3.4%), being considered a drop-out[31]. Table 2 presents descriptive statistics of our sample. Table 3 combines violence data to present summary statistics of violence exposure for the students in our samples and the schools in which they enrolled.

[Table 2: summary stats: education]

[Table 3: summary stats: violence exposure]

[30] In the few cases in which we observe no enrollment information for a student in a particular year but then observe them a year later in their expected grade we impute their expected grade in the prior year. Similarly, if a student is not observed for two years but then observed in their expected grade after three years, we impute the two missing years. Thus, we use sixth and seventh grade enrollments only to correct for those cases.

[31] If a child moves abroad, we may wrongly classify him as a drop out. However, migration from Brazil is not large enough for this to represent a significant bias. In 2017, 450 thousand Brazilians lived abroad (IBGE, 2017), with 8 to 21 thousand taxpayers each year reporting definitive emigration to the Brazilian Internal Revenue Service from 2011 to 2017 (Receita Federal, 2018).

4. Empirical specification and results

This section presents the main results of the paper. In each of our specifications, our outcomes of interest are indicator variables for student progress. First, we disregard the panel structure and consider year-by-year grade promotion relative to violence in the school-neighborhood. Then, we focus on cumulative outcomes from our student panel, such as the number of grades progressed in four years, an indicator for whether the student is behind the ideal grade but still enrolled and an indicator variable for drop-outs. We discuss and interpret the results in the next section.

4.1. Year-by-year impacts

We estimate the effects of violence within a given year on the likelihood that a child is retained in that year, for all children attending K1-K4 in municipal schools in Rio de Janeiro. Each individual student is observed for four years, which under ideal grade progression should correspond to being incrementally promoted from first to fifth grade[32]. We implement this identification strategy with a regression of the following form:

$$Y_{icsgy} = \beta V_{n_s y} + \eta_s + \gamma_{cgy} + X_i' \delta + \epsilon_{icsgy} \tag{1}$$

where Y is one of three indicators denoting end-of-year enrollment result (promoted, retained or dropout) for student i from cohort c, enrolled in school s and grade g in year y. Inclusion of school fixed effects η_s implies that identification comes from within-school comparisons of violence

[32] Given that students' cohort is simply the year in which they first enrolled in first grade, if they progress as expected, they will enroll in second grade in year $y=c+1$ and fifth grade in year $y=c+4$, that is, under ideal grade progression we should have $g=1+(y-c)$ in every observation.

exposure and enrollment outcomes over multiple years. Grade-cohort-year fixed effects γ_{cgy} flexibly control for a variety of potential confounds, including time-evolving municipal policy on grade retention per grade and differential outcomes for students who are already behind their cohort in any given year. We also include a set of individual demographic controls X (i.e., the student's gender, race, first-grade enrollment age, kindergarten enrollment, and whether the child was born in Rio). Standard errors are clustered by neighborhood (137 clusters), the level of variation in our treatment variable, as usual in the literature (Bertrand, Duflo, and Mullainathan 2004; Abadie et al. 2017).

Our treatment variable $V_{n_s y}$, is the standardized violent crime rate recorded in the neighborhood of school s during the year y, relative to Rio's average and standard deviation for all neighborhoods in 2007-2016. The coefficient of interest β can be interpreted as the average impact on grade progression for children attending school in a one-standard deviation more violent neighborhood, that is, with additional 33.7 violent deaths per 100,000 inhabitants. In an alternative specification, we replace this single coefficient with a vector of coefficients β_g, one for each grade, to explore whether children might be more sensitive to violence exposure in different grades. Absent an analysis of the mechanisms through which violence may affect academic progress, β is interpreted as net effects, including direct effects on children and indirect effects through teachers, principals and school climate.

Our identification strategy relies on the assumption that unobserved determinants of grade progression in any specific school are uncorrelated with year-to-year variation in school-neighborhood violence. That is, any endogeneity needs to be conditional on all these flexible fixed effects used. One threat to this assumption is that socioeconomic status of students enrolled over

time in a school have fluctuated significantly, according to the neighborhood-level violence. This could be the case, for instance, if the installation on a *Pacifying Police Units* (UPP) dramatically altered student demand for enrollment in a school. However, previous work on the educational impacts of the UPPs have not found any significant changes in student body composition – in terms of race, safety-net eligibility nor parents' educational attainment (Ribeiro da Silva, 2015).

The base results are summarized in Table 4. The first column shows the impact of violence on grade retention, measured in percentage points, for pooled grades estimated from equation (1) and the second displays coefficients estimated separately for each grade. On average in Rio de Janeiro, elementary schoolers experiencing a one standard deviation increase in violence are 0.42 percentage points more likely to be retained. Point estimates for each individual grade are very similar, but standard errors increase for later grades. One possible explanation for this increased imprecision when estimating impacts on older children is that cumulative impacts may be interfering with the results, as we explore in the next section. Figure 3 depicts these grade-specific coefficients graphically.

[Table 4: Y2Y regression]

[Figure 3: Y2Y regression coefficients by grade]

4.2. Cumulative impacts

We now explore the consequences of continued exposure to high violence rates for grade retention and the likelihood of on-time elementary school progress for each child. That is, we extract cumulative variables for each student from our panel. We model grade progression of

individual children as a regression of their cumulative violence exposure, using the following equation:

$$Y_{ics} = \sum_{k=1}^{4} \beta_k \, 1\{CV_{cs} = k\} + \eta_s + \gamma_c + X_i'\delta + \epsilon_{ics} \tag{2}$$

where Y denotes academic outcome for child i from cohort c, first enrolled in school s, within neighborhood n, when we observe the child four years after their initial enrollment (in year $y=c+4$). Ideally, children should have progressed four grades, reaching fifth grade, the last grade in elementary school in Brazil. We construct three outcome measures: (i) an indicator variable for whether the student is behind the ideal grade but still enrolled in school; (ii) an integer variable indicating the number of grades progressed in four years; (iii) an indicator variable for drop-outs, not being enrolled in any school in Brazil, public or private.

As in the year-by-year specification, we include school fixed effects η_s and cohort fixed effects γ_c, implying that identification comes from within-school and within-cohort comparisons of cumulative violence exposure. We also include the same set of individual demographic controls X (gender, race, first-grade enrollment age, kindergarten enrollment, whether the child was born in Rio). Standard errors are clustered by first-school neighborhood, in which the child started elementary school.

We use as violence measure the number of years in which a child's school-neighborhood violent crime rate was above a certain threshold in each of the four observed years for each cohort, given by CV_{cs}. We use a discrete variable for cumulative violence, β is a vector of coefficients for each possible value of such measure, integers 0 to 4. In our preferred specification, the threshold that determines whether a school-year is high violence is a crime rate above 69.8 murders per 100,000 inhabitants, one standard deviation above Rio de Janeiro neighborhood mean in the

period. Alternatively, we use thresholds of 0.5 and 1.5 standard deviations above the sample mean, corresponding to 51.6 and 88.0 violent deaths per 100,000 inhabitants.

[Table 5: Cumulative regression]

The base results for the cumulative impacts of violence on academic progression are summarized in Table 5. Conditional on remaining enrolled in Rio de Janeiro after four years, students that were exposed to a single year of high violence (standardized rates above 1), are 6.9 percentage points more likely to be behind the ideal grade and not reach fifth grade, compared to those never exposed to high violence. Children facing four years of high violence exposure exhibit effects almost twice as large (14.9 percentage points more likely to be late), a result that holds for alternative thresholds of high violence. These impacts translate in lower number of grades progressed, from 0.11 for one year of high violence to 0.21 for continued exposure over four years.

To examine the impact of violence exposure on the likelihood that children will drop out of elementary school, we use mean violence rates instead of number of years of exposure, because children may drop at different grades. That is, we repeat Equation 2 replacing the measure of cumulative exposure for the average standardized school-neighborhood violence rate only in the years that a child remained enrolled. Table 6 reports those results. A one standard deviation increase in the average violence augments by 2.2% the odds of a child dropping out after four years, impacts that are statistically significant.

[Table 6: Dropout regression]

5. Discussion and Conclusion

This study used longitudinal data on five cohorts of elementary school students in Rio de Janeiro municipal schools (n = 256,961) to show that students' grade progression in elementary school is negatively affected by community violence. Violence is a widespread phenomenon in Rio de Janeiro and it has been shown that this pervasive violence has negative consequences for children's schooling outcomes (e.g., Monteiro & Rocha, 2017). We build on the existing research by considering how year-to-year variation in the violence levels faced by cohorts of children affects those children's academic outcomes, namely on-time grade progression, and, particularly, the effect of cumulative exposure to violence. Such a methodological approach allows us to understand whether children's outcomes are adversely impacted in years with relatively high violence, as compared to other years in our data.

We find that a one standard deviation increase in violence during the school year increased the likelihood that a child would be retained in K1-K4 by 0.42 percentage points. This apparently small magnitude may have substantial impacts given the high costs of grade retention (Manacorda, 2012). Coefficients across the considered grades are similar but point estimates for the later grades (i.e., K3 and K4) are less precisely estimated and not statistically significant at conventional levels of significance. This finding suggests that the impact of violence on retention may be most salient for children in younger grades.

We also find that the effects of violence are cumulative. By comparing the exposure levels of cohorts of students within the same school, we are able to isolate the effects of the number of years of exposure to high levels of violence. This relies on the assumption that subsequent cohorts of children in the same school are similar. We find that a single year of exposure to high levels of

violence during the four years of elementary school led to a 6.9 percentage points reduction in the probability of reaching fifth grade on time. Persistent exposure to violence was even more damaging for on-time grade completion, with more than twice the effect observed on children exposed to four years of high violence. Our results suggest that negative impacts from continued exposure to violence exceeds the effects from short-term exposure, while allowing us to reject the hypothesis of a linear relationship between violence exposure and educational outcomes. Even if the marginal damage caused by children being first-exposed to violence exceeds the marginal damage of prolonging exposure to violence in communities that already face high crime rates, the policy implications from these findings cannot be disentangled from a moral discussion or they may compound pre-existing inequalities.

5.1. Limitations

We recognize that our research design has limitations that future research on the topic should seek to address. First, we do not know whether children in our sample were actually exposed to the violent incidences in our data on community violence. It may be that some students are better insulated from violent incidences if they, for example, have parents who shield them from the news, are not allowed to walk on the streets in their neighborhoods, or have never personally known a victim of violence. Thus, our exposure to violence measure captures an intent to treat effect, and our work does not estimate the average treatment effects of directly experiencing, witnessing, or being affected by a violent incident and likely represents a lower bound for the consequences of community violence when one is more proximally exposed.

Second, our work does not disentangle the potential mechanisms through which community violence may affect grade progression. We hypothesized that violence could have an impact on

grade progression through two principal child-level pathways: attendance and achievement. Children may be retained if they miss a substantial number of school days or if they do not meet standards for academic achievement. Academic achievement could be affected by violence either through increased absences or through decreased psychological functioning. Unfortunately, our data does not allow us to test either the attendance or achievement pathways. In addition, it may be that community violence has impacts on teacher- and school-level factors that trickle down to student outcomes. For example, violence may result in increased teacher turnover, which could in turn interrupt children's learning. Future work should thus consider impacts on student achievement and attendance, as well as on teacher- and school-level factors, in order to unpack the potential mechanisms through which community violence has impacts on retention. An understanding of these mechanisms could inform the development of policies and practices that seek to ameliorate the consequence of violence on student outcomes.

5.2. Conclusion

This study highlights the potential for policies aimed at reducing urban violence to have important educational consequences that have lasting impacts in society. In particular, we find that children's on-time grade progression in Rio de Janeiro is affected by community violence and that the consequences are even greater when children are repeatedly exposed to violence. These findings motivate further research into the mechanisms through which violence affects student outcomes and on potential policies that may serve to reduce violence and its consequences.

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Figures

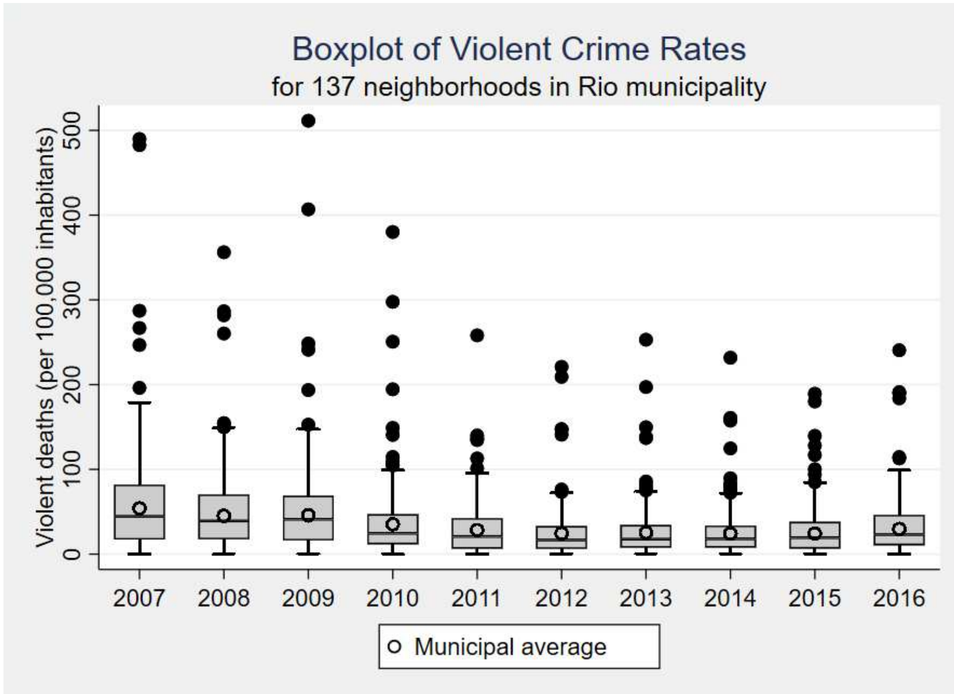


Figure 13 - Boxplot of violent crime rates by neighborhood

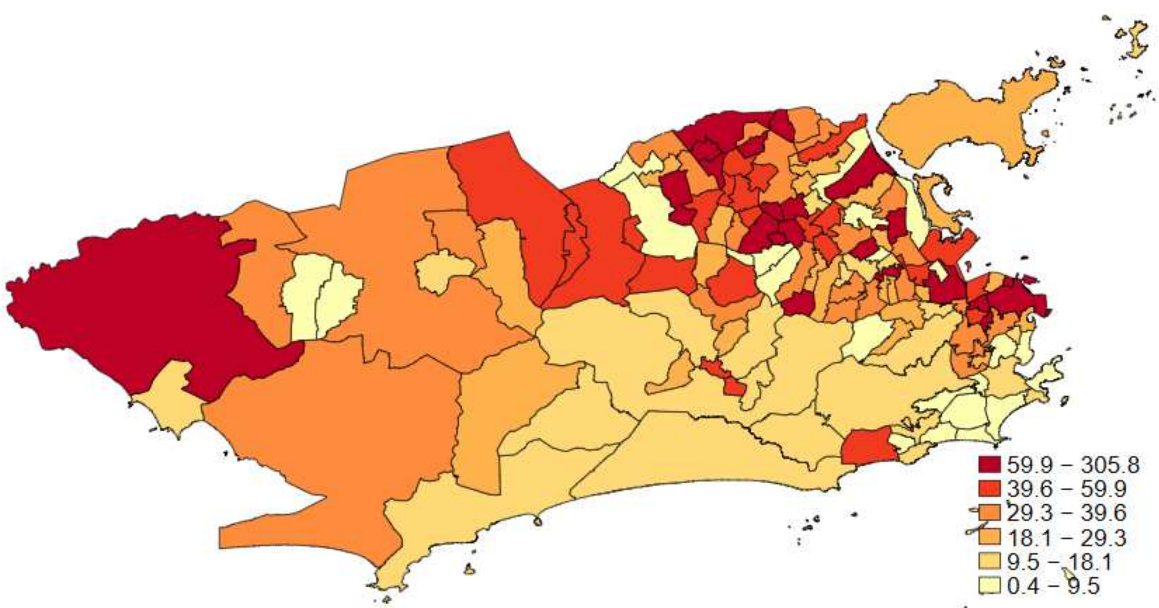
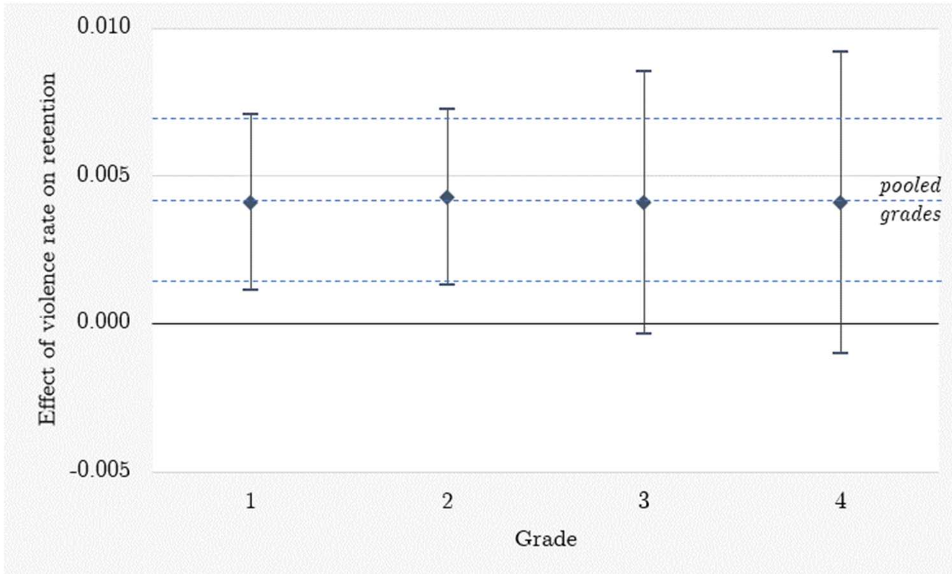
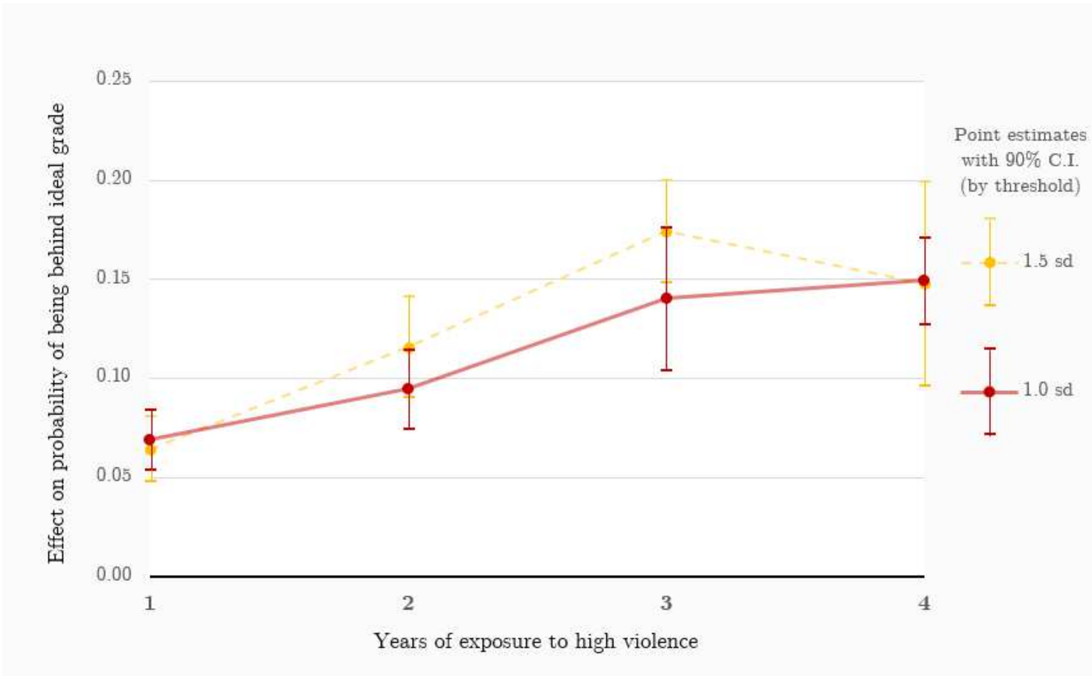


Figure 14 - Rio de Janeiro 137 neighborhoods according to their average annual violent crime rate (deaths per 100,000 inhabitants), in 2007-2017



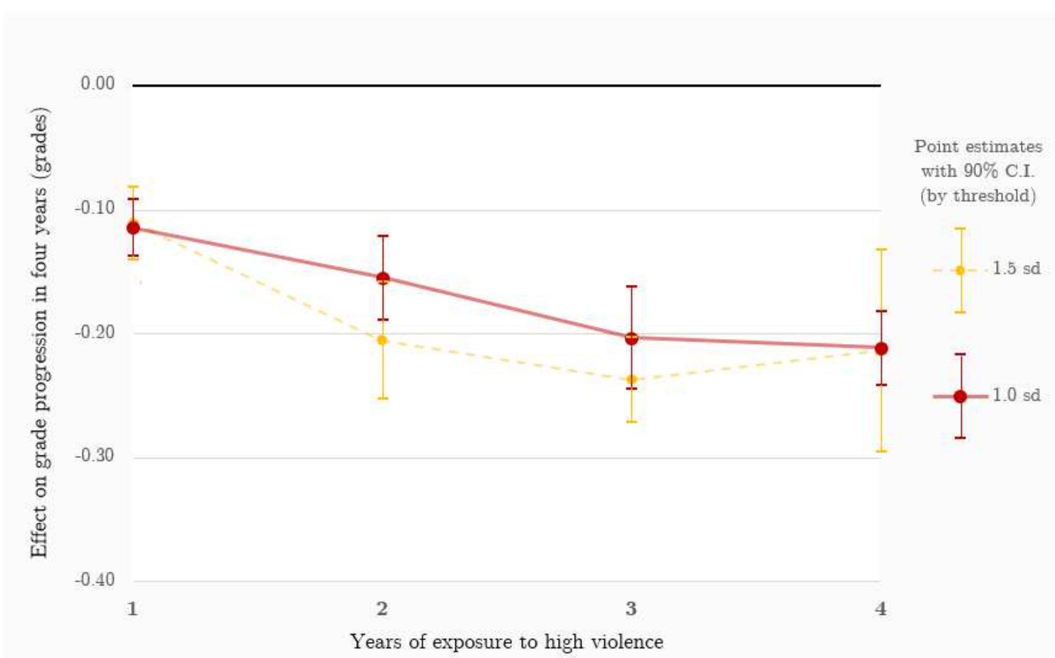
Notes: Coefficients of a regression of grade retention on school-neighborhood violence rates, as described in Equation 1, with interaction terms by grade. Coefficients are depicted with 90% confidence intervals.

Figure 15 - Coefficients from Y2Y regression



Notes: Effects on the likelihood of being behind ideal grade, per years of exposure to high violence, as described in Equation 2. Coefficients are depicted with 90% confidence intervals, for two high violence thresholds.

Figure 16 - Effect of cumulative violence on likelihood of being behind ideal grade



Notes: Effects on number of grades progressed, per years of exposure to high violence, as described in Equation 2. Coefficients are depicted with 90% confidence intervals, for two high violence thresholds.

Figure 17 - Effect of cumulative violence on likelihood of being behind ideal grade

Tables

Table 1 - Base sample (1st graders in Rio de Janeiro municipal schools in 2008-12)

Ideal Grade	N	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Cohort 2008	51,830	K	1	2	3	4	5	6	7			
Cohort 2009	55,251		K	1	2	3	4	5	6	7		
Cohort 2010	57,372			K	1	2	3	4	5	6	7	
Cohort 2011	48,393				K	1	2	3	4	5	6	7
Cohort 2012	44,115					K	1	2	3	4	5	6

Notes: represents the ideal grade progression for each cohort and their corresponding observations. Each child is classified into a cohort which corresponds to their first enrollment in first grade, and observed in the prior year (to assess kindergarten enrollment) and up to 7 years forward (to assess grade progression).

Table 2 - Summary statistics

Cohort (by first grade enrollment)	**2008**	**2009**	**2010**	**2011**	**2012**	**Total**
N Students	51,830	55,251	57,372	48,393	44,115	256,961
Student demographics						
Born in Rio city	87.8%	88.8%	90.4%	89.6%	89.0%	89.2%
Gender: Masculine	51.6%	51.5%	51.1%	51.4%	51.1%	51.4%
Race: White	39.3%	38.8%	38.9%	38.6%	38.2%	38.8%
Race: Non-white	60.7%	61.2%	61.1%	61.4%	61.8%	61.2%
Preschool enrollment	85.0%	86.7%	84.6%	81.2%	82.5%	84.1%
K1 enrollment age (Mar 31)						
5 years old	0.0%	8.1%	20.2%	24.4%	19.4%	14.2%
6 years old	88.3%	81.7%	73.5%	72.2%	77.5%	78.7%
7 years old	11.7%	10.1%	6.3%	3.4%	3.2%	7.1%
Status 4 years later	**2012**	**2013**	**2014**	**2015**	**2016**	**Total**
Enrolled in Rio de Janeiro	91.2%	91.1%	90.9%	89.5%	87.6%	90.2%
in K1-K4 (late)	25.7%	24.0%	23.2%	25.4%	22.9%	24.3%
in K5+ (on time)	65.5%	67.1%	67.7%	64.1%	64.7%	65.9%
Enrolled in other cities	6.3%	6.4%	6.2%	6.4%	6.6%	6.4%
in K1-K4 (late)	2.3%	2.6%	2.6%	2.7%	2.6%	2.6%
in K5+ (on time)	4.0%	3.9%	3.7%	3.7%	4.0%	3.8%
Not enrolled in school	2.5%	2.5%	2.9%	4.1%	5.7%	3.4%

Table 3 - Exposure to high violence (above selected thresholds)

Years of exposure	0	1	2	3	4	Total (N)
Threshold of 0.5 (crime rate above 51.6 per 100,000 inhabitants)						
% Students	53%	15%	12%	12%	8%	256,961
% Schools	65%	12%	10%	7%	6%	816
Threshold of 1.0 (crime rate above 69.8 per 100,000 inhabitants)						
% Students	70%	10%	8%	9%	3%	256,961
% Schools	82%	7%	5%	4%	2%	816
Threshold of 1.5 (crime rate above 88.0 per 100,000 inhabitants)						
% Students	79%	9%	5%	6%	1%	256,961
% Schools	91%	6%	2%	1%	1%	816

Notes: considering only students that were first enrolled in primary education between the ages of 5 and 7 in a municipal school in Rio de Janeiro between 2008 and 2012, and the municipal primary schools in which those students enrolled.

Table 4 - Y2Y regression results

dependent variable	**Grade Retention**	
	(1)	**(2)**
Violence rate (sd)	0.0042	
	(0.0017)**	
Violence * grade 1		0.0041
		(0.0018)**
Violence * grade 2		0.0043
		(0.0018)**
Violence * grade 3		0.0041
		(0.0027)
Violence * grade 4		0.0041
		(0.0031)
N obs (student-year)	931,910	931,910

Notes: Heteroskedasticity robust standard errors clustered by neighborhood are in parentheses (p<.10 ** p<.05*** p<.01). Coefficients in each column come from a regression of grade retention of individual students on standardized school-neighborhood violence rate. All regressions include the following demographic controls: gender, race, first-grade enrollment age, kindergarten enrollment, born in Rio.*

Table 5 - Cumulative impacts (conditional on remaining enrolled in Rio after 4 years)

outcome	Behind ideal grade			Grade progress		
threshold	**0.5 sd**	**1.0 sd**	**1.5 sd**	**0.5 sd**	**1.0 sd**	**1.5 sd**
Number of years of exposure to high violence						
n=1	0.0480	0.0691	0.0644	-0.0784	-0.1143	-0.1103
	(0.0077)***	(0.0091)***	(0.0100)***	(0.0124)***	(0.0139)***	(0.0177)***
n=2	0.0737	0.0945	0.1160	-0.1231	-0.1545	-0.2050
	(0.0115)***	(0.0123)***	(0.0156)***	(0.0180)***	(0.0203)***	(0.0286)***
n=3	0.1073	0.1402	0.1743	-0.1566	-0.2028	-0.2366
	(0.0183)***	(0.0218)***	(0.0158)***	(0.0254)***	(0.0252)***	(0.0207)***
n=4	0.1074	0.1494	0.1479	-0.1565	-0.2110	-0.2134
	(0.0316)***	(0.0133)***	(0.0314)***	(0.0423)***	(0.0180)***	(0.0494)***
N students	231,749	231,749	231,749	231,749	231,749	231,749

Notes: Heteroskedasticity robust standard errors clustered by first-school neighborhood are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients in each column come from a regression of individual student outcomes on the violence measure shown, with high violence refering to years with school-neighborhood violence above the threshold. Outcomes of being behind ideal grade and total grade progress are conditional to being enrolled in Rio de Janeiro in the fifth year. All regressions include first-school fixed effects, cohort fixed-effects and individual controls (gender, race, born in Rio de Janeiro, attended pre-school, age of first K1 enrollment).*

Table 6 - Cumulative impacts on drop out

	Drop out
Mean violence rate	0.0219
while enrolled	(0.0037)***
N students	256,961

Notes: Heteroskedasticity robust standard errors clustered by neighborhood are in parentheses (p<.10; ** p<.05; *** p<.01). Coefficients from a regression of an indicator variable reporting individual non-enrollment after four years (drop-out) on mean school-neighborhood violence rate during years of enrollment. Includes first-school fixed effects, cohort fixed-effects and individual controls (gender, race, born in Rio de Janeiro, attended pre-school, age of first K1 enrollment).*

Appendix

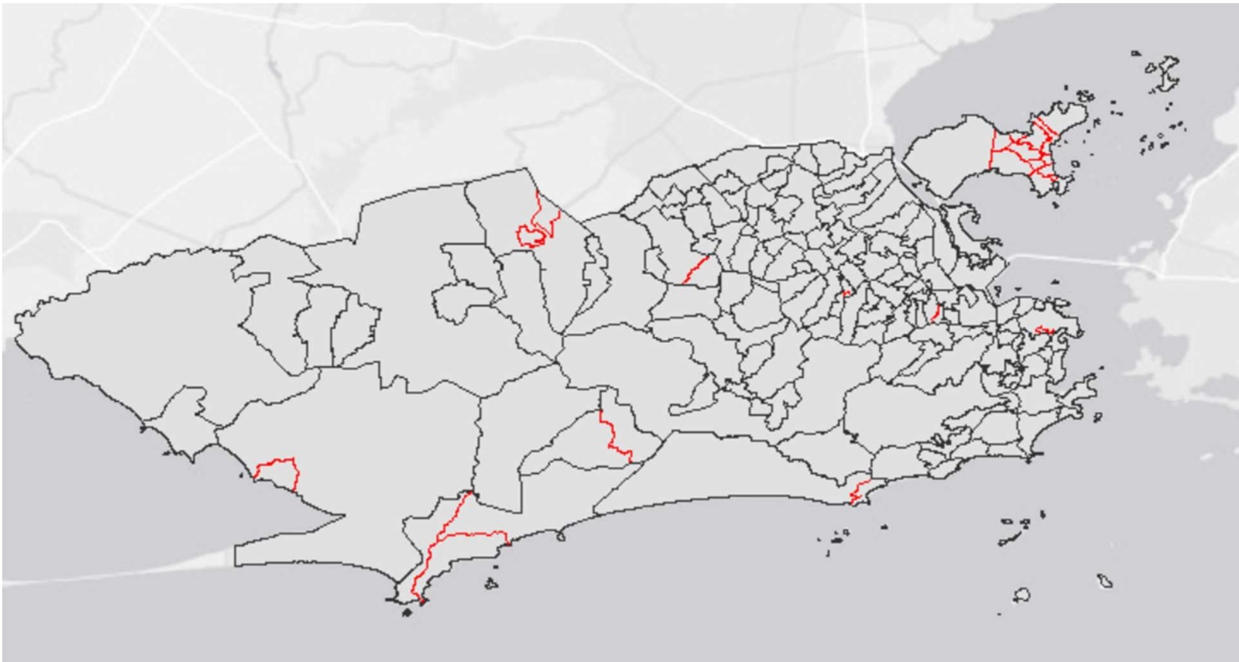


Figure A.1 – Rio de Janeiro neighborhoods: highlighted red lines were dissolved, to more accurately depict the violence microdata, considering 137 neighborhoods, down from the 162 recognized by the municipality

Note: The violence microdata provided by ISP contained a string field corresponding to the neighborhood, as it appeared in the first filling up of the case by a police officer. Given that agents freely type this field, they cannot always be directly linked to the official neighborhood divisions. Using contextual knowledge of Rio de Janeiro, we can link several cases (for instance: *Morro da Providência* to *Gambôa*). However, in some instances the use of labels is inconsistent and we chose to aggregate several neighborhoods into one, most notably, the 15 neighborhoods under the responsibility of the 37$^{\text{th}}$ police station (DP 37), which spans *Ilha do Governador* and *Ilha do Fundão* (the 15 neighborhoods merged into a single one are: *Ribeira, Zumbi, Cacuia, Pitangueiras, Praia da Bandeira, Cocotá, Bancários, Freguesia, Jardim Guanabara, Jardim Carioca, Tauá, Moneró, Portuguesa, Galeão, Cidade Universitária*). The remaining consolidations followed under two cases, according to the last Census: (i) neighborhoods that had less than 10,000 inhabitants (*Pedra de Guaratiba, Joá, Grumari, Barra de Guaratiba, Campo dos Afonsos, São Francisco Xavier, Camorim, Abolição*), and (ii) neighborhoods that were not yet dismembered by 2010 (*Lapa* from *Centro, Gericinó* and *Vila Kennedy* from *Bangu*). Those changes correspond to dissolving the red lines in the above map, which depicts the boundaries of the 162 neighborhoods officially recognized by the municipality as of July 2018, resulting in the 137 neighborhoods considered for this study. Each of those 137 neighborhoods has, on average, 45.7 thousand inhabitants in 8.8 square kilometers. A valid comparison could be Chicago's official 77 community areas, which average 37.6 thousand inhabitants and 7.9 square kilometers.

Peer mentoring for principals: a systemic approach to school turnaround

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Abstract

This paper evaluates a program of peer-mentoring for principals in Brazil, in which the principals of schools with poor academic performance are paired with the principals from top performance schools and both receive a small grant. Exploiting a regression discontinuity design for the school rank defining program participation, we find that low-performing schools participating in the program subsequently achieve education quality indexes that are 0.18 standard deviations higher. Participation does not significantly impact subsequent performance of top-performing schools. Similarity in number of enrolled students and proximity between paired schools are associated with stronger outcomes for low-performing participants. These findings provide an example of how school turnaround may be achieved systemically and at scale without leadership replacement.

Keywords: education, school turnaround, peer-mentoring, economics.

1. Introduction

Turning around low-performing schools is a perennial challenge for districts, in developed and developing countries alike. Despite numerous well-documented cases of successful school turnarounds, there is a knowledge gap on systemic and scalable approaches to school improvement (Herman 2012; Baroody 2011; Herman et al. 2008; Sternberg et al. 2006).

The literature on school turnaround in the US and Europe considers the school principal as the main change lever (Meyers and Sadler 2018, Wikeley et al. 2005). Nevertheless, most professional development and mentoring programs for school leaders are restricted to newly appointed or aspiring principals (Spiro, Mattis, and Mitgang 2007; Service, Dalgic, and Thornton 2018). Principals from the worst-performing schools are systematically, and often mandatory replaced in turnaround policies and programs, such as the federally funded School Improvement Grants in the US (Trujillo and Renée 2015).

In this paper, we provide evidence that low-performing schools can make dramatic gains in short periods, even when their leadership remains unchanged. We evaluate a state-wide program of peer-mentoring for principals in Ceará, Brazil, in which the principals of schools with poor academic performance are paired with the principals from top performance schools and both receive a small grant. The program was part of a broad initiative to improve early-grade literacy in the state, which implemented an annual state-wide literacy assessment of second graders and publicized its results through an Education Quality Index (EQI), calculated for every public school and every municipality. Each year from 2009 to 2015, the program selected the 150 top and bottom schools from the EQI ranking, provided they had at least 20 enrolled students in K2. Participants were matched in pairs by the state education board without a formal algorithm, but

roughly considering their location and ranking. The low-performing participant schools receive the first half of the grant unconditionally, an average of US$ 7,200 (less than 5% of the school's annual budget), plus an equal disbursement in the following year, conditional on participating on peer-mentorship sessions and on improving their performance. The top-performing participant schools receive 75% of the award in the first disbursement and the remaining 25% conditional on having successfully helped their mentee to improve, while sustaining their own high performance.

We combine administrative data on the peer-mentoring initiative with publicly available education data to build a panel at the school-level for all eligible schools. Pooling the seven years of the program, we explore the discontinuity at the participation cutoff, the EQI rank 150, both for top and bottom-performing schools. This identification strategy is superior than a comparison of outcomes in schools that participated in the program with schools that did not, which may give biased estimates of the program participation effects. The assumption is that, in schools that were very close to the participation cutoff, participation in the program is essentially as good as randomly assigned and indeed we find evidence in support of this identification assumption.

Student outcomes improve in low-performing schools that receive mentoring through this program, without negative consequences for schools whose principals served as mentors. We find that low-performing schools participating in the program subsequently achieve an EQI that is 0.18 standard deviations higher, and no significant EQI impact for the top-performing schools. Similarity in number of enrolled students and proximity between paired schools are associated with stronger outcomes for low-performing participants. This heterogeneity has important policy implications for other education districts considering replicating this program.

Peer-assistance and peer-mentoring are a common component of teachers' professional development programs, praised by their cost-effectiveness (Goldstein 2003; Papay and Johnson 2012). Mentoring programs for school principals are also not new, being the most cited priority among U.S. states education board leaders, according to a 2017 survey (Riley and Meredith, 2017). However, impact evaluations of such programs are still rare. Herman et al (2017) present a comprehensive review of school leadership interventions under the *Every Student Succeeds Act* and find that only two US support programs for sitting principals having been subject of experimental or quasi-experimental impact evaluation studies. The outcomes of those two programs are mixed. The *McREL's Balanced Leadership Program* was found to increase staff stability in treatment schools but had no effect on student achievement (Jacob et al., 2015). The *National Institute for School Leadership Executive Development Program* showed positive effects on reading and math achievement, using propensity score matching to form a control group (Nunnery et al., 2011). Results were also mixed on training programs for new principals: while the *Texas Principal Excellence Program* shows no statistically significant effects on student achievement outcomes for program participants after one year (Fouche, 2012), the *New Leaders Program*, exhibited larger student-achievement gains in math and reading (Gates, Hamilton, et al., 2014). We add to this literature by providing another evaluation of a peer-mentoring intervention for school principals, to our best knowledge, the first in a developing context. Our findings suggest that school turnaround may be achieved systemically and at scale without leadership replacement.

The rest of the paper unfolds as follows. Section 2 provides contextual information on the peer-mentorship initiative and our data sources. Section 3 details this study's empirical strategy. Section 4 presents its results, and Section 5 discusses the mechanisms. Section 5 concludes.

2. Context and data

2.1. Education in Ceará

The provision of public education in Brazil is the responsibility of both municipalities and states. Whereas the municipality oversees the primary and middle school grades (i.e., K1-K9), the state is responsible for high school (i.e., K10-K12). In Ceará, 76% of primary school students attend public municipal-funded schools[33].

Ceará is a relatively poor state in Northeast Brazil[34], famously portrayed as a 'good government in the tropics' by Judith Tendler (1997). Her homonymous book highlights the role played by the state government in improving service delivery by municipal governments, through active collaboration, monitoring and civil society engagement[35]. Those pillars formed a state-wide initiative to improve early-grade literacy, launched in 2007 (*Pacto pela Alfabetização na Idade*

[33] In 2017, out of 673,124 enrollments in K1-K5 in Ceará, 76.0% were in municipal schools, 23.5% in private schools, and the remaining 0.5% in federal and state schools (INEP School Census 2017).

[34] In 2017, monthly per capita income in Ceará was R$ 824, roughly US$ 200, only 65% of the Brazilian national average (IBGE, Pnad 2017)

[35] The book covers four program case studies implemented by Ceará in the mid-1980s: rural preventive healthcare, employment-creating work programs, agricultural extension, and assistance to small enterprises.

Certa, PAIC[36]), when only 7% of Ceará's municipalities scored above Brazil's mean in the national quality index for primary education[37]. PAIC successfully increased this share to 71% by 2017.

PAIC includes the distribution of teaching material, face-to-face training to early-grade teachers, a state-wide annual standardized assessment and a peer-mentoring program to award the top-performing schools while supporting the low performers. The main object of this paper is this peer-mentoring initiative, *Prêmio Escola Nota 10* (PEN10), which represented R$ 43 million or 52% of PAIC's budget in 2017[38]. Costa and Carnoy (2015) estimated that PAIC was responsible for gains of 0.10 standard deviation in Portuguese and 0.18 standard deviation in Math in 5th graders in the state. We build on their work by investigating one specific component of the program, the peer-mentoring initiative for school principals.

2.2. Peer-mentoring initiative

The assignment of schools into the peer-mentoring initiative between the best and worst-performing primary schools, PEN10, is based on the results from the state-wide literacy assessment of second graders (*Sistema Permanente de Avaliação da Educação Básica do Ceará*, SPAECE Alfa). Its structure is:

[36] PAIC was a scale-up of a successful early-grade literacy initiative led by Sobral municipality in 2001, by the mayor that later became Ceará's governor and created the state-wide program. In 2012, the Brazilian Federal government attempted to replicate it at a national scale, under the name PNAIC.

[37] Considering the Index of Basic Education Quality (*Índice de Desenvolvimento da Educação Básica*, IDEB) of elementary municipal schools, composed by the performance of students in national assessments at the end of fifth grade with flow rates.

[38] PAIC was complemented by fiscal incentives to municipalities, tied to their education performance, which are not included in program's budget, and represented roughly R$ 200 million in 2009 and R$ 513 million in 2017.

(1) The state government rank schools by their education quality index (EQI), an indicator composed by participation and performance, adjusted for equity, in the Portuguese of second graders in SPAECE, presented in a 0 to 10 scale.

(2) The 150 top- and bottom-performing schools with at least 20 enrolled students in K2 are selected to participate[39]. Participants are matched in pairs by the state department of education team and are invited to a one-day orientation and award ceremony, in which they sign a letter of intentions. The state education board did not have any formal algorithm in place to match schools, but reports using both the classification (i.e., match the best school with the worse school, the second best with the second worse) and attempting to assign schools within the same education district[40].

(3) All participant schools receive grants, with a first disbursement immediate and unconditional. The grant is proportional to the number of enrolled second graders[41], and the first disbursement represented, on average, R$ 27,543 (~US$ 7,200) for the low performers and R$ 64,529 (~US$ 16,800) for the top performers. To put those numbers on perspective, they represent between 2-4% of the school annual budget for the low-

[39] Defined by state legislation, the eligibility and selection criteria evolved over time. See Appendix for details. Take-up of the program is a 100% to this date, though selected principals could in theory decide not to participate.

[40] Ceará state education board subdivides its 184 municipalities into 21 education districts (*Coordenadoria Regionais de Desenvolvimento da Educação*, CREDES)

[41] The total grant value changed slightly over time. During the first two editions of the program, the grants were R$ 2,500 for top performers and R$ 1,250 for low performers, per enrolled second graded student. From 2011, the grants decreased to R$ 2,000 and R$ 1,000 respectively. The share of each disbursement remained unchanged throughout, with 75% for top performers and 50% for low performers being paid at the first unconditional disbursement, and the remaining reserved for the second disbursement, provided the conditions are met.

performing schools, and 7-10% for the top performers[42]. One year later, participant schools may receive a second disbursement, provided the following conditions are jointly met:

a. At least six peer-mentoring sessions took place between the matched principals and their teams[43]
b. The low-performing school improved its performance in the subsequent state-assessment to an EQI above 5 points
c. The top-performing school EQI performance was maintained (not applicable to the low performer, conditioning the second disbursement of the top performer only)

School principals must submit their spending plan for approval by the state team, which suggests reserving 10-20% of the first disbursement for peer-mentoring travel and logistic expenses. Top-performing schools may distribute up to 20% of both disbursements as performance bonus to its staff. Low-performing schools are not allowed to distribute any amount of the first disbursement to its employees, but may do so if they turnaround successfully, receiving the second disbursement.

2.3. Data

We combine administrative data on the peer-mentoring initiative with publicly available education data to build a panel at the school-level.

[42] The federal government, through its Fund for the Development and Maintenance of Basic Education (FUNDEB), redistributes education resources across the country to complement state taxes and ensure that all elementary school student is matched to at least R$ 2,875 of funds per year (2017 values). Ceará is one of the nine Brazilian states which receives those funds. The lower bound estimate considers that 100% of the FUNDEB value, considering enrollment in all grades, is passed on to the participant schools, while the upper bound considers overhead expenses in the state/municipal education boards to retain a third of those funds.

[43] Participant schools have, on average, 13 teachers and 360 students enrolled across all grades. Therefore, school leadership and management is very centralized in the figure of the school principal. Some schools have a deputy principal responsible for pedagogical practices (*coordenador pedagógico*).

Ceará Administrative Data. The state education board provided administrative data reporting which partnerships were formed each year and how much was disbursed to each participant school. Between 2009 and 2015[44], the seven PEN10 editions established a total of 1,048 partnerships[45] among 1,429 unique schools based on their EQI, all over the state[46]. It is worth highlighting that 62 schools that had participated as low performers, after a successful turnaround, were selected to participate as top performers offering mentorship.

SPAECE Data. School-level results from the annual state-wide assessment which determines the school ranking over which PEN10 participants are selected are public. We use both the EQI and its three components (participation rates, performance, and equity adjustment of performance) for the literacy assessment of second graders (SPAECE-Alfa) from 2008 to 2017. Thus, we can observe up to three years of lagged performance from program participation.

School Census Data. We use the School Census (*Censo Escolar)*, an annual survey of every school in Brazil conducted by the National Institute for Research on Education (*Instituto Nacional de Estudos e Pesquisas Educacionais Anisio Teixeira, INEP*) to enrich our school panel with other relevant school characteristics. We include a score that captures the quality of its

[44] The program was not terminated in 2015, rather, it changed focus to promote educational improvement in later grades. *Escola Nota Dez* selected up to 150 pairs of schools based on 2nd grade assessment from 2008 to 2014, plus up to another 150 pairs based on 5th grade assessment since 2011 and up to 150 pairs based on 9th grade assessment since 2015. Despite discontinuing the mentorship component based on 2nd grade results and the financial incentives for low-performing schools, the program continues to award the 150 top performers in this grade-level.

[45] In the first year, only 148 partnerships were established. The remaining six years had 150 partnerships each.

[46] See Figure A.1 for a map of participant schools

infrastructure[47], basic teacher characteristics (average age of teachers, share of male teachers and share of teachers with tertiary education) and enrollment figures.

Program eligibility is conditional on having at least 20 students enrolled in second grade. Thus, only 44% of Ceará's public elementary schools were ever eligible to participate in the program, but those schools are significantly larger, representing 70% of the enrollments, and more urban. Table 1 presents descriptive statistics for the all public elementary schools in Ceará (column 1) and two subsamples of interest: low-performing schools that are just below and just above the participation cutoff of rank 150 (columns 3 and 4). By design, the baseline EQI of those barely included to participate is significantly lower, though it is interesting to note that those two groups do not differ in participation rates in the state-wide assessment. Column 5 reports the p-value for equality of means between the groups. Column 6 reports the p-value for a discontinuity in the baseline characteristics at the participation cutoff for low-performing schools.

[Table 1: Descriptive Statistics]

3. Empirical strategy

We rely on a regression discontinuity design to estimate the effects of the peer-mentorship program for school principals in Ceará. This section describes the details of our identification strategy and provides evidence in support of the identification assumptions.

[47] Infrastructure score refers to the average of five indicator variables: whether a school has a library, a sports court, a teachers' room, a science laboratory and a computer laboratory.

3.1. Program participation effects

To quantify the effects of participation in the incentivized mentorship program, we use a sharp regression discontinuity design[48], comparing schools that *barely* made it into the program with schools that *barely* didn't. Participation in the program was granted based on the school rank at the EQI educational quality index, according to a set of eligibility and tie breaker rules which evolved slightly over years[49]. Even while having some influence over their school performance, principals are unable to *precisely* manipulate their rank, which makes the variation in treatment near the threshold similar to a randomized assignment. Therefore, our estimated treatment effects have a causal interpretation.

Our main specification is a linear regression for eligible schools ranked *around* 150$^{\text{th}}$, where *around* is defined according to the optimal bandwidth selection of Calonico et al. (2017). We estimate the effects of participating in the program at both ends – separately for the top and low performer schools – by estimating the following equation:

$$Y_{sc(y+1)} = \alpha_c + \beta_c \mathbb{1}\left\{r_{scy} \leq 150\right\} + \sigma_c r_{scy} + \gamma_c \mathbb{1}\left\{r_{scy} \leq 150\right\} r_{scy} + X_{sy}'\delta + \varepsilon_{scy} \qquad (1)$$

The running variable is the rank of a given school amongst all eligible schools in the low/top category of participation in a year (r_{scy}). The treatment variable is $\mathbb{1}\left\{r_{scy} \leq 150\right\}$, an indicator variable equal to one if the school was ranked below 150 amongst eligible low/top performers and, hence, selected to participate in the mentorship program. Our main parameter of interest is β_c, the effect of participation in the program for schools in the cutoff, estimated for the

[48] The RDD method was first applied in a very similar context, evaluating the impact on students from receiving a merit award allocated on the basis of an observed test score (Thistlethwaite and Campbell, 1960).

[49] See the Appendix for a comprehensive description of the rules evolution and their corresponding legislation.

top and low categories of participation. The outcome variable $Y_{sc(y+1)}$ reflects the temporal evolution of the school's EQI, either as the observed change relative to the baseline ($EQI_{sc(y+1)} - EQI_{scy}$) or as an indicator variable reflecting the achievement of the target performance of at 5 for the low-performing schools $\mathbb{1}\{EQI_{sc(y+1)} \geq 5\}$ and the maintenance of good results for the top-performing schools $\mathbb{1}\{EQI_{sc(y+1)} \geq EQI_{scy}\}$. We include as controls X_{sy} the baseline EQI and a year dummy to control for a general time trend between editions of the program. Standard errors are clustered at the school level.

3.2. Identification Assumptions

For Equation (1) to estimate the causal effects of program participation, the key identification assumption is that any potential factors that influences the outcomes are continuous around the cutoff $r_{scy} = 150$, and, thus, any discontinuity in outcomes at the cutoff is the result of the program[50]. By design, our running variable has a uniform distribution, precluding the need of any formal test for manipulation of the running variable[51]. Further evidence that lends support to our identification assumption is that we do not find evidence of discontinuity in covariates at the program cutoff, as reported in Table 1, column 6 for the low-performing schools.

4. Results

This section presents the program participation effects on the Education Quality Index (EQI) – a summary measure of student achievement outcomes – for both the low- and top-

[50] That is, for any potential factor X that influences the outcome Y, such as Y=f(r,X), f(X) needs to be continuous, with only r being discontinuous at the threshold.

[51] We present in the Appendix, Figure A.2, the graph of baseline EQI versus the running variable, EQI rank, which, as expected, exhibits no discontinuity around the cutoff.

performing schools. First, we present the results obtained by pooling all the seven years of the program, then, we exhibit results for subsets of years as a robustness check.

4.1. Program participation effects

We estimate equation (1) separately for low-performing schools receiving mentorship and top-performing schools providing mentorship. We show the RD plots using the optimal bandwidth for each outcome, and the corresponding regression tables. In the tables, we include alternative bandwidths to show that our point estimates are not sensitive to the using bandwidth. All the estimates presented in this subsection consider the pooled data from the seven years of the program, 2009-2015.

Low-performing schools. We consider two outcomes of interest for low-performing schools, after one year of mentorship: (i) the change in EQI, relative to the baseline, and (ii) an indicator variable for achieving an EQI above 5, considered a successful turnaround of previous poor performance and a condition for receiving the second grant disbursement.

The first outcome is reflected in Figure 1a, the EQI change one year after the mentorship program began in schools that received mentoring. The point estimates appear in Table 2, Panel A. In our preferred specification, reported in the first column, the program gains are estimated as 0.484 points in EQI, significant at 5 percent.

Figure 1b shows likelihood that previously low-performing schools succeeded in turning around, reaching an EQI of 5 or above. Panel B of Table 2 reports the point estimates: including controls, the program increases turnaround by 15.2 percentage points, significant at 1 percent.

[Figure 1: Program participation effects for low-performing schools]

[Table 2 – Regression Discontinuity Design (low performers, 2009-2015)]

Top-performing schools. The program could potentially impact top-performing schools in different directions. On one hand, the immediate cash grant and the public recognition entailed by participation could boost staff motivation and increase availability of educational inputs. On the other hand, the extra work taken by the top principal, serving as a mentor, could harm student outcomes – despite the financial incentives that conditional the second disbursement to both own EQI maintenance and mentee school improvement.

We find that the effects of program participation for top-performing schools, which receive a grant and provide mentorship, are not statistically significant. This is shown by Figure 2a, depicting the change in EQI after a year, and Figure 2b, showing whether those schools suffer a decrease in performance. Point estimates are presented on Table 3, respectively on Panel A and B. This suggest that the extra workload on principals in top schools did not affected their excellence performance.

[Figure 2: Program participation effects for top-performing schools]

[Table 3 – Regression Discontinuity Design (top performers, 2009-2015)]

Thus, our main results show that program participation improved the education quality index of the low-performing schools and did not significantly affect the top-performing schools. Put together, the program helped lifting low-performance schools without harming the performance of the top schools.

One important caveat is that, each year, the program selected only 150 pairs of schools from approximately 3,000 schools that satisfy the minimum enrollment criteria for participation.

The skill gap between the top- and bottom-performing principals, which possibly explains the productive outcomes of the mentoring relationship, is likely much larger at the 150 cutoff than it would be if the program was significantly expanded.

4.2. Robustness check

We now repeat our estimates dropping one program year at a time, as a robustness check. As Table 4 reports, excluding any given year from our sample changes does not alter our results substantially. The estimated EQI gains remain between 0.311 and 0.581, significant at conventional levels for most subsamples, while the estimated effects on achieving an EQI above 5 remain between 0.107 and 0.196, significant for all subsamples.

[Table 4: Robustness check]

5. Mechanisms

We now turn our attention to all the 1,048 partnerships created by the program. We are particularly interested in the results of the mentorship for the low-performing schools. At the discontinuity, we estimated the local average treatment effect of participating in the program is positive, an improvement of 0.484 (0.18 SD) in the EQI after one year. Considering all the participants, thus abandoning causality claims, we observe an average change of 2.38 points in EQI after a year, departing from a baseline of 2.77 (in a 0 to 10 scale). However, this average change mask significant heterogeneity of performance across schools. Figure 3 presents the histogram of EQI improvement (i.e., level changes) after one year of program participation for all treated schools.

[Figure 3 - Histogram of low-performing participant schools' improvement]

The results of the mentorship relationship for each established pair and their varying degree of success in improving student achievement outcomes, may be driven by various mechanisms. One key factor is mentorship implementation – whether the assigned principals did meet at least six times, and the quality of those mentoring sessions (i.e., whether they were live or online, their duration, possible exchange between other members of the school leadership or teaching teams). The other mechanism is the fit between mentor and mentee (i.e., whether the mentor is able to understand the context of the mentee and provide efficient strategies, whether they have positive personal interactions). We do not directly observe variables that indicate mentorship implementation nor match quality. However, we observe several characteristics of program participants, which we use to construct *geographical* and *contextual distance* measures between matched pairs, which we will use as proxies for the above mechanisms.

Table 5 presents the descriptive statistics of eligible schools in Ceará (column 1) and the selected participants: top performers serving as mentors (column 2) and low performers receiving mentorship for turnaround (column 3). By design, the baseline Education Quality Index (EQI) of top-performing participants (9.67 in a 0 to 10 scale) is expressively higher than observed for the low-performing participants (2.77). Those schools differ in other significant ways: the top performers are smaller (38.8 versus 52.1 enrolled second graders), have a larger share of students using free school transport (0.28 versus 0.18) and have a lower share of students behind their ideal grade (0.09 versus 0.23). Surprisingly, teacher qualification, measured by share of teachers with a bachelor's degree, and school infrastructure[52] are significantly worse in the top-performing schools.

[52] Infrastructure score refers to the average of five indicator variables: whether a school has a library, a sports court, a teachers' room, a science laboratory and a computer laboratory.

Though counterintuitive, the fact that low-performing schools are not worse endowed in terms of teachers nor physical infrastructure may be a necessary condition for the program positive outcomes. That is, the performance difference between top and bottom schools may arise partially due some unobserved principal ability gap, such as managing skills, which may be transferable through mentoring.

[Table 5 – Descriptive statistics of program participants (pooled 2009-2015)]

To investigate the drivers of turnaround success, we document the outcomes of low performers participants in relationship to characteristics of both the low performers and their top performer counterparts in the peer-mentorship relationship. Though the state education board did not have any formal algorithm in place to match schools, we cannot consider the assignment random, because they report loosely attempting to use the classification (i.e., match the best school with the worse school, the second best with the second worse) and the school education districts as guidelines. We capture those two metrics by observing the absolute rank difference in each the matched pair and a dummy variable indicating whether the schools belong to the same district.

Furthermore, we examine additional relational variables that attempt to capture both the *contextual* and *geographical* distance between matched pairs, which may impact the mentorship's potential outcomes. On contextual differences, we consider an indicator variable for whether both schools are in urban or rural areas and whether they are located in the same municipality. We also factor in the dissimilarity of school size, given by the ratio of the absolute difference between second grade enrollment and the maximum of both enrollment figures. For the geographical distance, we rely on driving times and distances between two schools, corresponding to the Google

Maps API estimates given the school coordinates obtained by geocoding the addresses in the School Census[53]. Table 6 report summary statistics of all the partnerships according to the above detailed metrics.

[Table 6 – Descriptive statistics of partnerships (pooled 2009-2015)]

5.1. Improvement drivers

To investigate the drivers of turnover success, we regress the observed improvement of the low performer participants in aspects of those schools in relationship with their top performer counterparts. The coefficients estimated in this section do not allow for any causal interpretation and should be interpreted as correlations. Nevertheless, this exercise is useful to provide indicators of what may constitute a successful mentorship between two school leaderships. We use the following equation:

$$Y_{s_t s_l y} = \alpha_y + X_{s_t s_l}{}'\beta + \varepsilon_{s_t s_l y} \qquad (2)$$

where $Y_{s_t s_l y}$ is the outcome obtained by the mentee low-performing school s_l, from partnering with the mentor top-performing school s_t, after participating in the program in year y. We focus on the evolution of the EQI of a school relative to the baseline ($EQI_{sc(y+1)} - EQI_{scy}$). We model this outcome as a linear regression with constants by year of partnership α_y and an explanatory variable $X_{s_t s_l}$ capturing some aspect of interest in the partnership between the two schools. Our parameter of interest β captures the relationship between the explanatory variable and the obtained outcome.

[53] We were unable to precisely identify coordinates for 5% of the participant schools and inputted their municipality centroid in those few cases.

We use as $X_{s_t s_l}$ the variables depicted in Table 5, first individually, then collectively: (i) whether both schools are located in rural or in urban zone; (ii) whether both schools belong to the same municipality; (iii) whether both schools belong to the same education district; (iv) the absolute rank difference between schools (v) the dissimilarity of school sizes, which takes the value 0 if both mentee and mentor schools have the same number of enrolled second graders, according to $abs(n_{s_l} - n_{s_t}) \,/\, max(n_{s_l}, n_{s_t})$; and (vi) the driving time in hours between the schools. When considering all variables simultaneously, we omit the dummy for same education district (iii) due to collinearity with the dummy for same municipality (vi).

Table 7 reports the estimation results from equation (2). Though not causal, they suggest a strong relationship between proximity of matched schools and successful turnover. Belonging to the same municipality is associated with a 0.583 increase in the EQI delta, while belonging to the same district – which nests on average 9 neighboring municipalities – is associated with a 0.506 increase on the outcome of interest. Consistently, we observe a negative relationship between driving time and EQI delta: a one hour increase in the travel time between schools is associated with a 0.068 lower delta. Those results suggest that being paired with a school located at a short distance is more effective – perhaps because those pairs succeed at implementing a more intense and better-quality mentorship relationship. Another important factor in determining partnership success is the similarity in school size, which is the only factor to retain its significance when all variables are simultaneously considered.

[Table 7 – Performance improvement regressions on mentorship characteristics]

6. Conclusion

In this paper, we evaluate a mentoring program for school principals in Ceará. Using regression discontinuity design around the participation cutoff, we estimate a causal improvement of 0.18 SD in the subsequent Education Quality Index of low-performing schools, and no significant impact for top performers. This evidence suggests that low-performing schools can make dramatic gains in student achievement outcomes in short periods, even when their leadership remains unchanged.

The analysis of the mechanisms suggests that *contextual* and *geographical* distance between matched pairs may impact the mentorship's potential outcomes. Though we are not able to make causal claims regarding this heterogeneity, we document that similarity in number of enrolled students and geographical proximity between paired schools are associated with stronger outcomes for low-performing participants.

A limitation of this study is being unable to untangle the impacts of the peer-mentorship from the cash grant received, a natural next step for future research. Nevertheless, this study suggests that peer-mentorship between school principals can offer a systemic approach to school turnaround.

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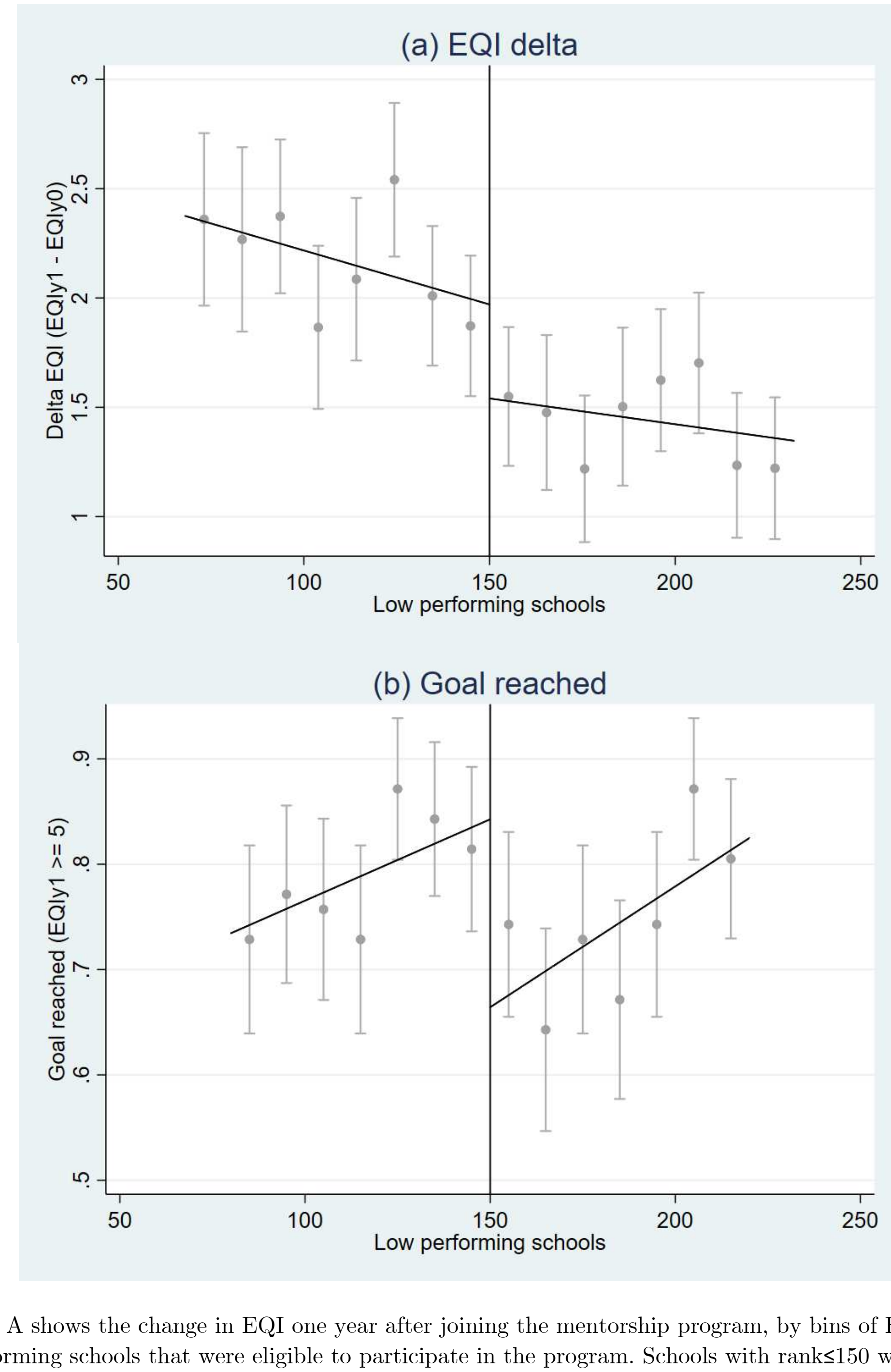
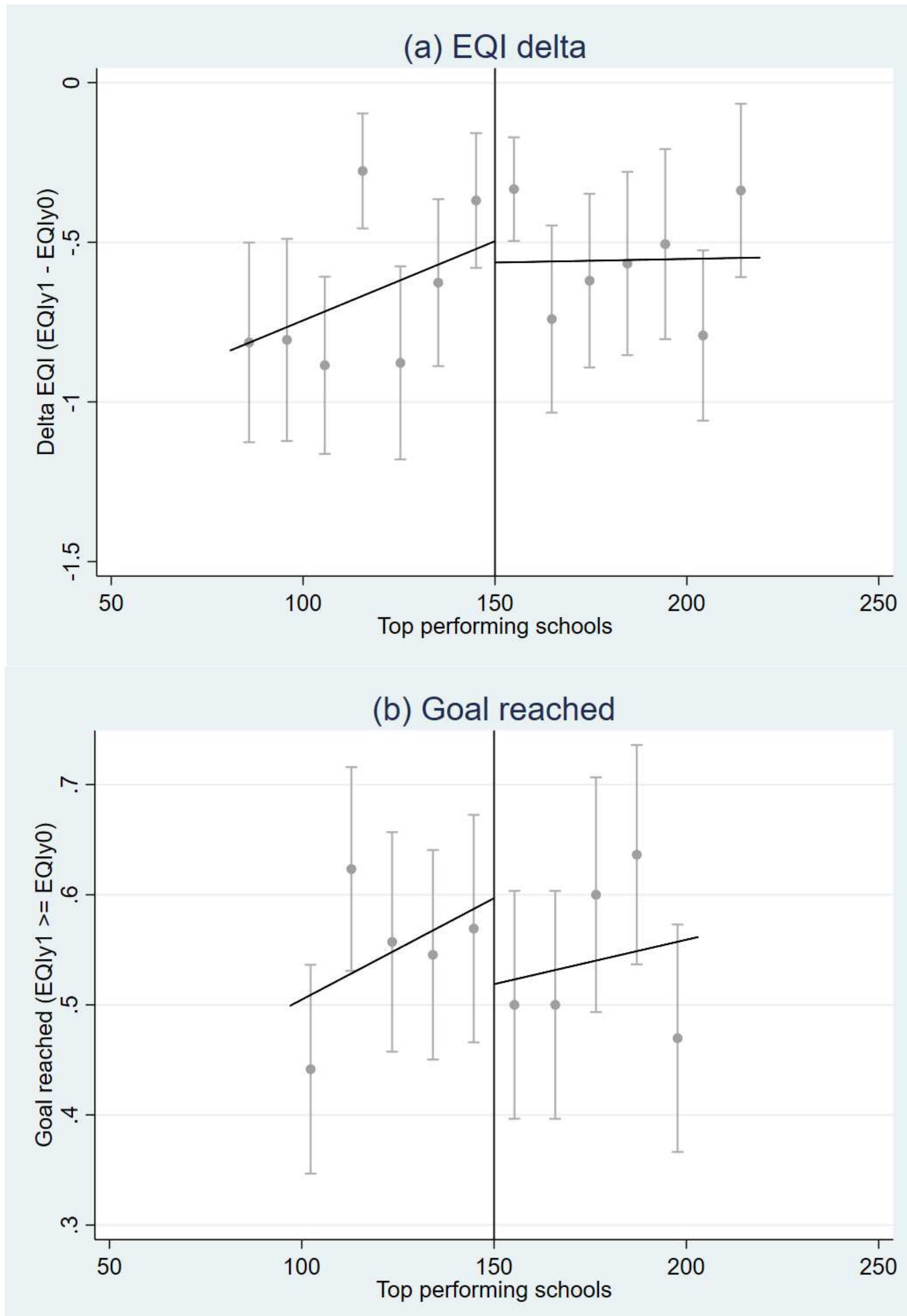
Figures

Figure 1 - Program participation effects for low-performing schools



Notes: Panel A shows the change in EQI one year after joining the mentorship program, by bins of EQI ranking of the low-performing schools that were eligible to participate in the program. Schools with rank≤150 were selected to join the program, while those with rank>150 were not. Panel B shows the proportion of low-performing schools that reached an EQI≥5, a precondition to receiving the second disbursement of the grant, one year later.

Figure 2 - Program participation effects for top-performing schools



Notes: Panel A shows the change in EQI one year after joining the mentorship program, by bins of EQI ranking of the top-performing schools that were eligible to participate in the program. Schools with rank≤150 were selected to join the program, while those with rank>150 were not. Panel B shows the proportion of top-performing schools that sustained their EQI one year later, a precondition to receiving the second disbursement of the grant.

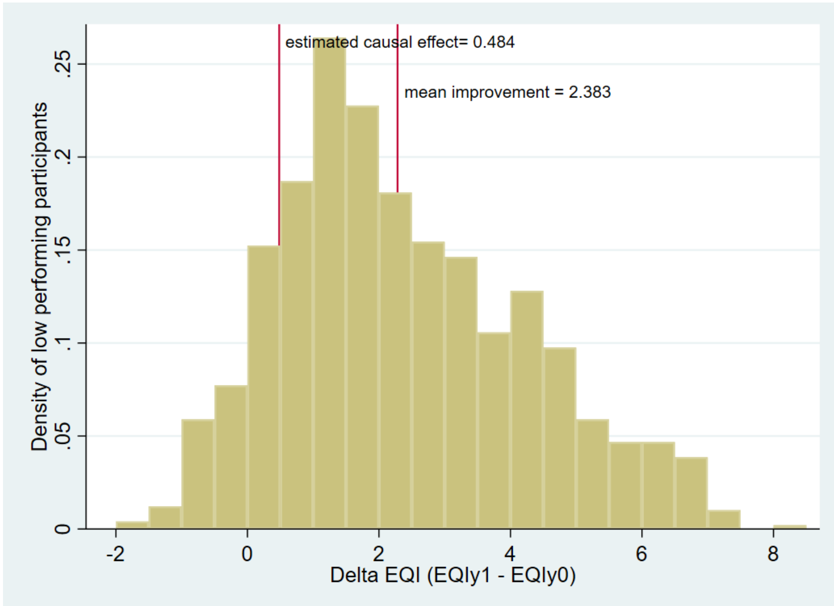


Figure 3 - Histogram of low-performing participant schools' improvement

Tables

Table 1 - Descriptive Statistics (pooled 2009-2015)

	(1) All public elementary schools	(2) All elegible schools in Ceará	(3) Low performing "barely" included (101th-150th)	(4) Low performing "barely" excluded (151st-200th)	(5) P-value equal means col. 3 & 4	(6) P-value RDD col. 3 & 4
Sample size						
Number of Unique Schools	6,676	2,934	317	313	-	-
Number of Observations (school-year)	33,476	12,223	350	350	-	-
School Characteristics						
Share urban	0.33	0.68	0.72	0.74	0.58	0.32
Infrastructure score	1.62	2.68	2.97	2.99	0.89	0.67
Students						
Number of 2nd graders per school	24.7	47.6	54.8	56.1	0.68	1.00
Share of students using school transport	0.27	0.24	0.18	0.19	0.67	0.97
Share of students behind ideal grade	0.16	0.17	0.23	0.22	0.53	0.05
Teachers						
Number of teachers per school	7.9	12.7	13.1	14.5	0.03	0.59
Teacher average age	36.6	37.7	39.0	39.0	0.98	0.95
Share of female teachers	0.85	0.86	0.84	0.84	0.67	0.92
Share of teachers with B.A.	0.60	0.73	0.77	0.75	0.27	0.98
Education Quality Index						
Participation rate on assessment	0.95	0.96	0.92	0.93	0.95	0.91
Literacy proficiency	7.52	7.83	5.95	6.10	0.04	0.94
Equity adjust factor	0.73	0.77	0.55	0.57	0.05	0.92
Baseline Education Quality Index	5.72	6.14	3.21	3.43	0.04	0.94

This table shows descriptive statistics for all public schools in Ceará which offer second grade (1), schools large enough to participate in the program (2), the subsample of elegible low performing school just below the participation cutoff (3) and the subsample of elegible low performing schools just above the participation cutoff (4). Column 5 tests for equality of means and Column 6 for a discontinuity in baseline characteristics at the participation cutoff for low performers (using Equation 1), and both report the corresponding p-values. Note that the running variable is the rank of EQI, thus, one should expect that EQI and its components would have significantly different means between (3) and (4), as shown in Column 5, but no discontinuity, as captured by Column 6.

Table 2 - Regression Discontinuity Design (low performers, 2009-2015)

	Low Performers						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
(A) Delta EQI							
Participation effect	0.484	0.433	0.595	0.265	0.466	0.452	0.445
	(0.218)**	(0.197)**	(0.229)***	(0.311)	(0.251)*	(0.196)**	(0.181)**
Using bandwidth	83	90	81	41	62	104	124
(B) Goal reached *(EQI>=5)*							
Participation effect	0.154	0.152	0.153	0.094	0.123	0.161	0.147
	(0.061)**	(0.057)***	(0.086)*	(0.091)	(0.073)*	(0.054)***	(0.049)***
Using bandwidth	69	70	67	34	52	86	103
Specification							
Bandwidth method	Opt	Opt	Opt	.5*Opt	.75*Opt	1.25*Opt	1.5*Opt
Year dummies	No	Yes	No	No	No	No	No
Analytical weights	No	No	Yes	No	No	No	No
Observations	12,205	12,205	12,205	12,205	12,205	12,205	12,205

Notes: Coefficients on program participation for low performing schools from regressing each of the outcomes on the running variable of the RDD (rank), program participation (rank<150) and the interaction of these two variables for the set of schools with |rank-150|<Using bandwidth. Outcomes are the change in EQI after one year (Panel A), or an indicator variable for whether the school reached the EQI goal of 5 (Panel B). Heteroskedasticity robust standard errors clustered at the school-level are in parentheses (p<.10 ** p<.05*** p<.01).*

Table 3 - Regression Discontinuity Design (top performers, 2009-2015)

	Top Performers					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
(A) Delta EQI						
Participation effect	0.016	0.051	-0.022	-0.022	0.039	0.012
	(0.158)	(0.143)	(0.199)	(0.174)	(0.146)	(0.137)
Using bandwidth	70	69	35	53	88	105
(B) Goal reached *(sustained EQI)*						
Participation effect	0.100	0.040	0.153	0.113	0.078	0.070
	(0.083)	(0.105)	(0.118)	(0.096)	(0.075)	(0.069)
Using bandwidth	54	50	27	40	67	81
Specification						
Bandwidth method	Opt	Opt	.5*Opt	.75*Opt	1.25*Opt	1.5*Opt
Analytical weights	No	Yes	No	No	No	No
Observations	3,173	3,173	3,173	3,173	3,173	3,173

Notes: Coefficients on program participation for top performing schools from regressing each of the outcomes on the running variable of the RDD (rank), program participation (rank<150) and the interaction of these two variables for the set of schools with |rank-150|<Using bandwidth. Outcomes are the change in EQI after one year (Panel A), or an indicator variable for whether the school sustained its EQI (Panel B). Heteroskedasticity robust standard errors clustered at the school-level are in parentheses (p<.10 ** p<.05*** p<.01).*

Table 4 - Robustness Check: RDD (low performers, 2009-2015 subsamples)

	Low Performers							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
(A) Delta EQI								
Participation effect	0.484	0.416	0.479	0.466	0.311	0.604	0.528	0.581
	(0.218)**	(0.229)*	(0.233)**	(0.238)*	(0.235)	(0.234)***	(0.237)**	(0.238)**
Using bandwidth	83	83	83	83	83	83	83	83
(B) Goal reached *(EQI>=5)*								
Participation effect	0.154	0.123	0.158	0.182	0.107	0.196	0.156	0.158
	(0.061)**	(0.065)*	(0.069)**	(0.069)***	(0.063)*	(0.066)***	(0.064)**	(0.065)**
Using bandwidth	69	69	69	69	69	69	69	69
Specification								
Year dropped	None	2009	2010	2011	2012	2013	2014	2015
Observations	12,205	10,237	10,152	10,113	10,388	10,906	10,718	10,716

Notes: Coefficients on program participation for low performing schools from regressing each of the outcomes on the running variable of the RDD (rank), program participation (rank<150) and the interaction of these two variables for the set of schools with |rank-150|<Optimal bandwidth when full sample. Column 1 contains the full sample, while columns 2 to 8 drop one year at a time. Outcomes are the change in EQI after one year (Panel A), or an indicator variable for whether the school reached the EQI goal of 5 (Panel B). Heteroskedasticity robust standard errors clustered at the school-level are in parentheses (p<.10 ** p<.05*** p<.01).*

Table 5 - Descriptive Statistics of program participants (pooled 2009-2015)

	(1) All elegible schools in Ceará	(2) Top performing participants	(3) Low performing participants
Sample size			
Number of Unique Schools	2,934	704	787
Number of Observations (school-year)	12,223	1,048	1,048
School Characteristics			
Share urban	0.68	0.64	0.71
Infrastructure score	2.68	2.51	2.88
Students			
Number of 2nd graders per school	47.6	38.8	52.1
Share of students using school transport	0.24	0.28	0.18
Share of students behind ideal grade	0.17	0.09	0.23
Teachers			
Number of teachers per school	12.7	11.6	13.3
Teacher average age	37.7	36.1	39.1
Share of female teachers	0.86	0.86	0.84
Share of teachers with B.A.	0.73	0.67	0.76
Education Quality Index			
Participation rate on assessment	0.96	0.99	0.91
Literacy proficiency	7.83	9.94	5.63
Equity adjust factor	0.77	0.98	0.49
Baseline Education Quality Index	6.14	9.67	2.77

This table shows descriptive statistics for all public schools in Ceará which offer second grade and were large enough to participate in the program (1), those that actually participated as mentors (2) or mentees (3).

Table 6 - Descriptive Statistics of partnerships (pooled 2009-2015)

	Mean	SD
Relational dummy variables		
X1: Same type (urban or rural)	0.56	0.50
X2: Same municipality	0.05	0.22
X3: Same education district	0.26	0.44
Relational continuous variables		
X4: Absolute rank difference (top/low)	47.7	34.5
X5: Dissimilarity of school size	0.40	0.25
X6: Driving time (hours)	3.05	1.94
X7: Driving distance (km)	209	142
N (partnerships) = 1,048		

Note: characteristic of the matched pairs of participant schools. Dissimilarity of school size is given by the ratio of the absolute difference between enrolled second graders and the maximum of both enrollment figures.

Table 7 - Performance improvement regressions on mentorship characteristics

	Low Performers' EQI delta							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(7)**
X1: Same type (urban or rural)	-0.144							-0.181
	(0.117)							(0.114)
X2: Same municipality		0.583						0.339
		(0.249)**						(0.257)
X3: Same education district			0.506					
			(0.131)***					
X4: Absolute rank difference (top/low)				0.002				0.001
				(0.002)				(0.002)
X5: Dissimilarity of school size					-1.262			-1.253
					(0.206)***			(0.215)***
X6: Driving time (hours)						-0.068		-0.029
						(0.027)**		(0.028)
N (partnerships) = 1,048								

Note: regressions on EQI delta by low performers that participated in the program on the selected characteristic of the matched school pairs. All regressions include year fixed-effects. Heteroskedasticity robust standard errors clustered at the school-level are in parentheses (p<.10 ** p<.05*** p<.01).*

Appendix

This is an exhaustive list of the state legislation creating and regimenting PEN10:

- **Law nº 14.371 / June 19, 2009**: creates PEN10. Defines number of participants as 150 schools in each category (*premiada & apoiada*). Eligibility conditioned on at least 20 enrolled second graders and participation rate above 50% on SPAECE. Additionally, top performers must score above 8.5 in the education quality index (*Índice de Desempenho Escolar na Alfabetização*, IDE-Alfa). Grants for top performers are set to R$2,500 per enrolled K2 student, payed 75% in the first disbursement. Low performers receive R$1,250 per enrolled K2 students, payed 50% in the first disbursement
- **Decree nº 29.896 / September 16, 2009**: defines rules for the second, conditional disbursement, and provide guidelines for the grant use
- **Law nº 14.580 / December 21, 2009**: specifies that the minimum enrollment of 20 students condition has SPAECE application date as reference point
- **Law nº 14.949 / June 27, 2011**: expands the program to the K5 assessment
- **Law nº 15.052 / December 6, 2011**: increases minimum participation rate of students in SPAECE to 90%, establishes tie breaking rules and a new eligibility condition (only schools in municipalities with above 70% of students with satisfactory achievement are eligible as top performers), limits repeated participation
- **Decree nº 30.797 / December 29, 2011**: reflects a change in the calculation method of the education quality index (IDE-Alfa) and regiments the K5 program (based on IDE-5)
- **Law nº 15.246 / December 6, 2012**: modifies tie breaking rules and imposes additional eligibility condition (only schools in districts above 70% of students with satisfactory achievement are eligible as top performers)
- **Law nº 15.923 / December 15, 2015**: expands the program to the K9 assessment and modifies the K2 version to discontinue the peer-mentoring, preserving only the prize for top performers. The selection of low performers to be matched with top performers remains for K5 and K9 levels. Grants values for top performers changed to R$2,000 per enrolled student and R$1,000 for low performers
- **Decree nº 32.079 / November 9, 2016**: regiments the K9 program (based on IDE-9) and revises guidelines for grant disbursement and use

Figure A.1 - Map of participant schools

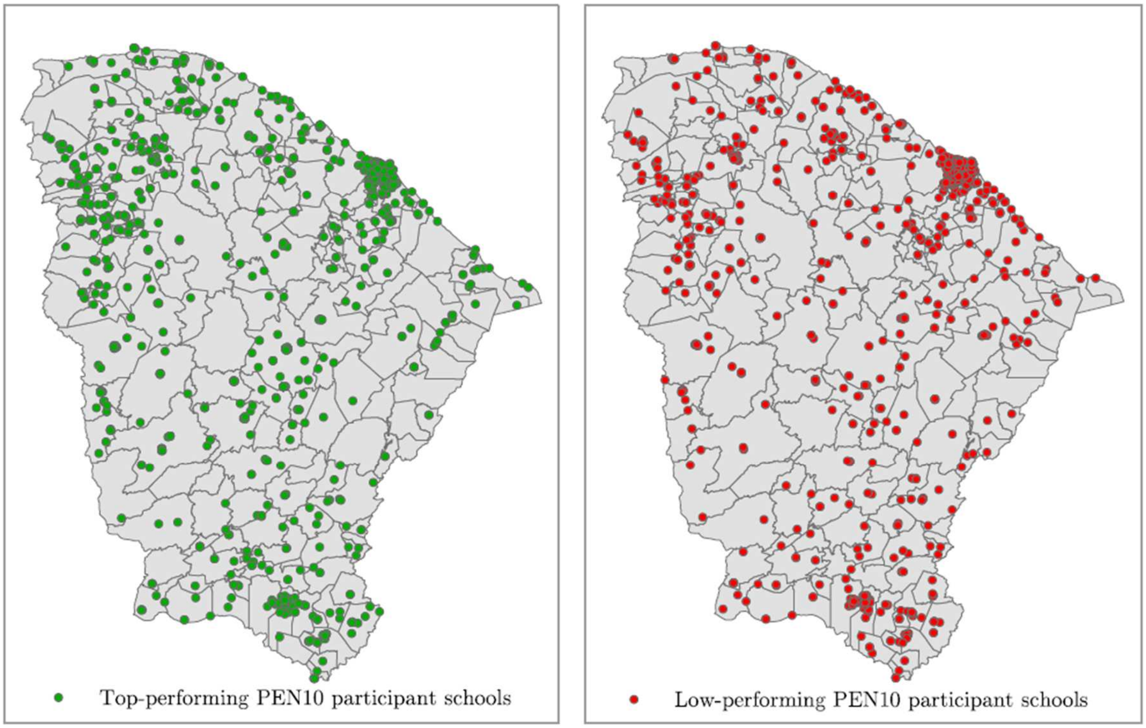
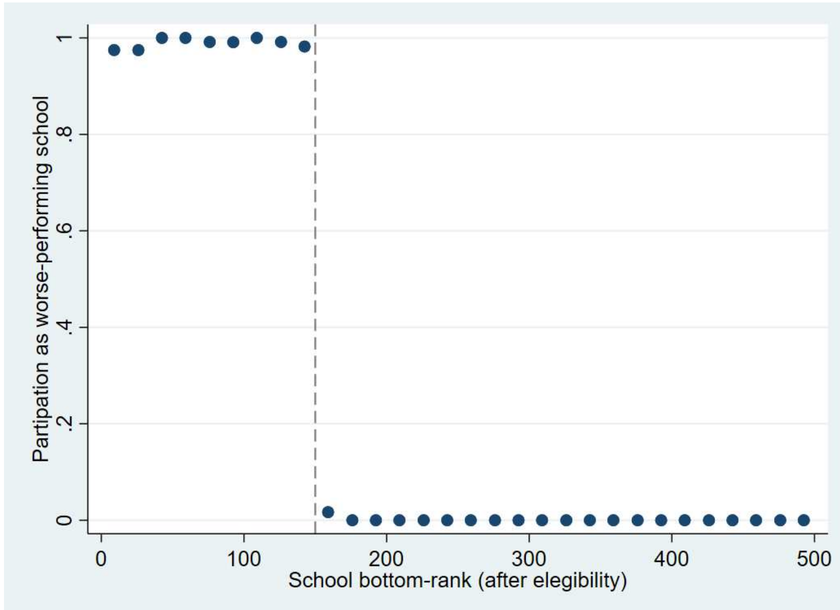


Figure A.2 – No evidence of EQI discontinuity at the participation cutoff for low-performing schools



The figure above reports the proportion of schools participating in the program receiving mentoring, according to their EQI rank (running variable). The EQI rank is directly determined by EQI at the baseline. The figure below shows no evidence of discontinuity of EQI at the baseline.